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Essays in Empirical Political Economy

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Abstract

This thesis is composed of three chapters. In the first chapter, joint work with Federico Masera (UNSW), we study how the capacity of the state in providing similar services influences the support for the non-state organizations. We do so in the context of Pakistan and study the competition between the Pakistani state and the Taliban in the provision of natural disaster relief. We first look at the floods of 2010 that received inadequate response from the government due to poor Pakistan-U.S. relations at that time. We show that support for the Taliban increased in the areas affected by the flood. We then study the 2005 earthquake that instead received a swift government response and show that the Taliban lost support in the areas affected by the earthquake. Alternate mechanisms such as anger against the incumbent, political competition and substitution, and religiosity do not account for these results.

In the second chapter, I study the indirect impact of international terrorism on politics. Using the September 11 attacks as an exogenous shock to the salience of terrorism and employing a Difference-in-Differences strategy, I compare changes in political participation in areas with higher risk of terrorism to areas with lower risk. I measure the risk of terrorism for each county in the U.S. based on three different measures: Department of Homeland Security funding, presence of critical infrastructure, and distance from the state capitol. I find that areas with higher risk of terrorism increased political participation and campaign contribution in the subsequent elections. Using instrumental variable strategy based on the distance of each county from the state centroid yield similar results. The results highlight how unfortunate national shocks such as international terrorism can increase the political engagement among citizens.

In the third chapter, I study the political impacts of mass shootings in the United States. Mass shootings are unfortunately frequent events which keep drawing public attention towards gun policy. The divide on gun policy among Republicans and Democrats has increased both among voters and politicians. However, we know very little about mass shootings and its effects. In this paper, I construct a list of mass shootings in the U.S.

from 2001-12 and analyze their impact on electoral outcomes, voter preferences, and gun policy. Using a Difference-in-Difference strategy, I find that Republicans lose significant votes in all federal (Presidential, Gubernatorial, Senatorial, and House) elections after mass shootings. Variations of identification strategy, placebo and falsification exercises suggest that this decline reflects a causal impact of mass shootings. While mass shootings result in lower individual campaign contributions for the Republicans, the NRA increases its contributions to Republican candidates. I then show that mass shootings do not change the average preferred gun policy among the electorate, but rather impact the electoral outcomes through an increase in the importance of gun policy among voters. The lack of change in the average preferred policy masks the increase in the polarization between Republicans and Democrats. Mass shootings lead to *an even greater* disagreement on gun policy among voters: while Democrats demand greater gun control after mass shootings, Republicans shift towards *lower* gun control. Likewise, politicians from both parties shift to more diverging stances on gun policy.

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The Charitable Terrorist: State Capacity and the Support for the Pakistani Taliban *

Federico Masera Hasin Yousaf

Abstract

Violent, criminal or terrorist organizations often provide many social services. In this paper we show how the capacity of the state in providing similar services influences the support for these groups. We do so by studying the competition between the Pakistani state and the Taliban in the provision of natural disaster relief. We first look at the floods of 2010 that received inadequate response from the government due to poor Pakistan-U.S. relations at that time. We show that support for the Taliban increased in the areas affected by the flood. We then study the 2005 earthquake that instead received a swift government response and show that the Taliban lost support in the areas affected by the earthquake. Alternate mechanisms such as anger against the incumbent, political competition and substitution, and religiosity do not account for these results.

Keywords: Taliban; State Capacity; Terrorist support; natural disaster; aid
JEL Classification: D74, H12, H40, O20, P16, Q54

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1 Introduction

The Pakistani Taliban provide many goods and services similar to ones by the formal Pakistani state. Taliban, for instance, provide services like education, hospitals, a legal system and a parallel police system to enforce these rules.[1] Several other violent, criminal and terrorist organizations around the world operate in a similar fashion by competing with the state in the provision of social services. For example, criminal organizations in Latin America provide social services, especially to the very poor. They maintain the public goods infrastructure of the most disadvantaged parts of the cities by building roads, maintaining the water distribution system and trash disposal (Solis and Rojas, 2009). Similarly, mafias provide security and a resolution mechanism especially in areas where the state is not strong (Gambetta, 1996). In several Muslim majority countries, violent religious groups such as Hamas in Palestine, the Muslim Brotherhood in Egypt and Hezbollah in Lebanon provide social services (Berman and Laitin, 2008). More recently, violent groups, like Boko Haram in Nigeria and ISIS in Syria and Iraq, recreated numerous social services institutions (Isaac, 2015; Khalaf, 2015).

Does state capacity determine the presence and popular support of these organizations? In this paper we test how the state capacity can affect the rise and fall in support of these extreme groups. In particular, we test whether in a context of lack of state capacity i.e. where the needs of people are not taken care of by the state leads to an increase in the popular support for the non-state actors who compete to provide for these needs.

This hypothesis is generally difficult to test for at least two main reasons. First, several factors jointly determine the state capacity and the existence and capacity of these non-state groups. It is difficult to find clean exogenous variation in state capacity. Second, the popular support of these groups is difficult to observe at geographically fine level.

We overcome these two problems by focusing on the competition between the Pakistani state and the Taliban in the provision of natural disaster relief after the 2010 floods that

[1] See: Telesetsky (1998); Mohammad and Conway (2003); Rashid (2010); Giustozzi (2012)

covered one-quarter of the country under water. In Pakistan, both the state and Taliban provide immediate relief such as food, water and medicine after natural disasters. In the long-run, they are both involved in the reconstruction and provide a legal system to resolve the disputes, often land-related, that arise after a natural disaster. Unlike other places, the support for Taliban can be measured accurately because one of the extreme Islamist political parties: Muttahida Majlis-e-Amal (MMA) are directly related to the Taliban (Norell, 2007). Voting is a precise and geographically fine measure of support for ideologies. Hence, the vote share of MMA provides us an accurate measure of the support for the Taliban at a small geographical unit for every election year.

The 2010 floods were the deadliest floods in the history of Pakistan. The natural disaster directly impacted more than quarter of the total land area and 20 million people. The floods occurred in a period of deteriorated relationship between the US and Pakistan which resulted in very little international aid.[2] In the first three weeks, the international agencies only gave 15 percent of the $10 million emergency appeal. The average gap in funding received relative to what was required was 60% six months after the disaster (NDMA GOP, 2010a). The Taliban jumped to provide relief and rehabilitation aid to the affected areas (Masood, 2010; CBSNews, 2010).

We combine political data with the data on floods and use a Difference-in-Differences (DiD) strategy to measure the impact of floods on the Taliban support. Specifically, we compare changes in the support for MMA in areas affected by the floods relative to the areas unaffected by the floods. We find that MMA vote share increased by 5.4 p.p. in areas affected by the flood relative to unaffected areas. These are big changes for the MMA given average vote share of MMA was 7.4% in the 2008 elections. This result highlights that state capacity can move millions of people away or towards the support of a terrorist organization like the Taliban.

To highlight the importance of the proposed mechanism, we then use data on the

[2]This may be due to many factors that include a change in the presidency of the US and Pakistan, the use of unauthorized drone attacks in the Pakistani territory and the presence of many high-caliber terrorist (most famously Bin Laden) in Pakistani territory.

percentage of funding which was unmet five months after the onset of the 2010 floods. We employ similar a DiD approach to identify the effect the funding gap on the support for the MMA. To alleviate endogeneity concerns, we instrument the funding gap in an electoral district with the intensity of exposure to the flood. We show that the areas with higher funding gap had a higher increase in the vote share of Taliban parties. A 10 p.p. increase in the funding gap leads to a 0.84 percentage points increase in the vote share of the Taliban parties.

We then provide evidence that these results are produced by the lack of state capacity by studying the 2005 earthquake that instead happened in a period where Pakistan was an essential partner in the war of terror. Because of this, Pakistan received high levels of international aid and the government was widely praised for the good management of this disaster (Wilder, 2010). Applying the same difference in difference strategy we find that instead in this natural disaster the vote share of MMA decreased by 18.7 percentage points in the areas affected by the 2005 earthquake compared to the areas which were not affected. This analysis shows how that changes in the support for the Taliban were driven by state capacity and not due to the natural disaster itself.

An alternative explanation for our results could be a standard political model in which voters punish the incumbent for bad management of the natural disaster making political competitors gain votes. We show that the results are not in line with this explanation. We show that the incumbent party indeed lost votes after the 2010 floods but not significantly more in the affected areas. The main competitor to the incumbent party also received no particular change in their political outcomes in places affected by the flood. The only competitor political party that showed significant changes specifically in places affected by the natural disasters was the MMA. This indicates that these effects are due to the fact that the MMA through the Taliban are the only party that was directly providing goods and services in competition with the state.

In all our preferred specifications we control for three important factors that may influence the changes we observe in the MMA vote share and are correlated with being affected by natural disaster. First, we allow for differential trends among Pashtun majority electoral districts relative to other districts.[3] Second, we allow for differential trends with respect to the ex-ante propensity of floods in an electoral district.[4] Third, we allow for differential trend with respect to rural and urban areas.[5] This implies, that for our identification we compare changes in the MMA vote share in flooded areas with areas not affected by flood which had an ex-ante similar propensity of flooding, similar share of Pashtun and urbanization.

Finally, for we provide evidence in favor of our identification strategy by exploiting the availability of two pre-treament elections. We show that the electoral districts which were affected by the 2010 flood had similar changes in the support for the Taliban between 2002 and 2008 elections compared to the areas which were not affected by the 2010 flood. This strengthens the interpretation that our estimates reflect a causal effect of flood on the support for Taliban.

Furthermore, the heterogeneous impact of natural disaster on the MMA vote share is in line with our mechanism. We find stronger impact of state capacity on the MMA vote share in areas closer to the Afghanistan border and with lower MMA shares. In addition, the results are concentrated in rural and areas with low literacy rate, which are precisely the areas where government lacks the state capacity the most and are more vulnerable to extreme groups.

This paper is broadly related to the literature that studies the causes of civil conflict, war and terrorism.[6] These causes may be due to economic shocks (Bruckner and Ciccone, 2011; Chaney, 2013), ethnic differences (Esteban and Ray, 2011; Esteban et al., 2012),

[3]Pasthun ethnicity is historically closely connected with the Taliban. Because of this trends in the MMA vote share can be very different in Pashtun majority areas with respect to other electoral districts. At the same time majority Pashtun areas have been especially affected both by both the floods.

[4]Floods are very common in Pakistan. There have been more than 70 floods since 1900. The 2010 flood is the largest flood in the modern history of Pakistan. The places which are regularly affected by flood may have very different political preferences due to different economic structure and may already have pre-existing informal institutions compared to an average electoral district.

[5]More urban areas are less likely to be affected by the floods and may have a different trend in the MMA vote share.

[6]For a review of the literature refer to Blattman and Miguel (2010)

extreme climate conditions (Hsiang et al., 2013), political instability (Fearon and Laitin, 2003), price shocks (Besley and Persson, 2008; Dube and Vargas, 2013) among many others.

More closely related to our paper are recent studies exploring the effect of aid on conflict and violence. For example Berman et al. (2011b) study a model of competition between a government providing a reconstruction program and violent rebels. They then test the model using panel data from Iraq and find that reconstruction spending reduces insurgent violence. Other papers have tried to identify, causally, how international aid affects conflict (Beath et al., 2012; Crost et al., 2014; Nunn and Qian, 2014). Most of the papers have identified that development aid has either no or a detrimental effect on civil conflict. We contribute to this literature in two ways. We are the first to study the determinants of the support for a violent or terrorist group, which sheds light on how aid determines actual conflict and violence. Second, we are the first to causally identify how state capacity is the driver of these results. We exploit the unique setting in which there is both lack and sufficient state capacity in the same geography over different points in time.

Additionally, we contribute to the literature that tries to understand the support for terrorist or rebel groups. A detailed review is provided by de Mesquita (2008). More recently, Jaeger et al. (2012) show how radicalization of the Palestinian population is influenced by Israeli violence and major political events like the Oslo negotiations or the first Intifada. Berman et al. (2011a) find no evidence of the opportunity-cost theory i.e. only individuals with a low opportunity-cost (poor and unemployed individuals) use violence. Similarly, Blair et al. (2013) find that there is no link between income and personal support for militant and terrorist organizations. In a study in Iraq instead Iyengar et al. (2011) find a positive correlation, at the district level, between spending in labor-creating projects by the US military and violence reduction. In this paper we causally identify a mechanism similar in spirit to Berman and Laitin (2008) who observe the support for terrorist groups as a way of receiving local public goods when neither the government nor the markets can deliver these goods.

Finally, our paper also contributes to the literature that studies the effects of natural disasters and aid relief. A comprehensive discussion on the topic can be found in Stromberg (2007). For example it has been shown that international aid delivery (or the lack of thereof) may have economic and political consequences (Alesina and Dollar, 2000). In particular, Drury et al. (2005) show that large disasters, if not handled properly by the international community, may destabilize local governments. More closely related to the effects of natural disasters on terrorism Berrebi and Ostwald (2011) show in a cross-country comparison that natural disasters are positively associated with terrorist attacks. Looking specifically at Pakistan, Fair et al. (2017) show how people more harshly affected by the 2010 flood in Pakistan increased their turnout to elections. Andrabi and Das (2010) demonstrate a positive effect of the 2005 earthquake on trust towards foreigners caused by a prompt delivery of foreign aid. We contribute to the literature by testing a particular mechanism as to how natural disasters may lead to increase or decrease in support for terrorist organizations based on aid relief.

In the following section, we present the detailed background of the context with focus on the electoral system of Pakistan, a description of the floods and the relief provided by the state and the data sources. In Section 3 we outline our empirical methodology. In section 4, we provide the baseline results, along with the mechanisms that could account for the results and demonstrate the heterogeneity of the results. We carry out robustness checks in the Section 5, and discuss and conclude in Section 6.

2 Context

In this section we briefly discuss the context of our setting and the data sources. Specifically, in the first subsections, we give an overview of the political system of Pakistan, providing summary of the elections, major political and the Islamic parties. Then, we present summary of the flood of 2010, followed by the data sources.

2.1 Political System of Pakistan

The governing structure of Pakistan is a parliamentary system. The National Assembly has 342 members out of which 272 members are elected for a 5 year tenure.[7] In our analysis, the elections were held in 2002, 2008 and 2013 respectively.[8] The voting structure is first-pass-the-post system. Each candidate can belong to at most a single political party or decide to run independently without any party affiliation.

Historically, the two biggest parties in the political system of Pakistan are the Pakistan Muslim League Nawaz, PML (N) and the Pakistan's People Party (PPP).[9] There are several political parties with extreme Islamic ideology. The major Islamic parties include: Jamiat-e-Ulema-e-Islam (JUI-F), Jamiat Ulema-e-Pakistan (JUP), Jamaat-e-Islami Pakistan (JI), Jamiat-e-Ahle Hadith, and Pakistan Isami Tehrik (ITP) (formerly Tehriq-e-Jafaria (TeJ)). In 2002, these five parties formed a political alliance, Muttahida Majlis-e-Amal (MMA), as a direct opposition to U.S. war in Afghanistan (Adel et al., 2012).[10]

2.2 Islamic Parties and connections with Taliban

Since the inception of war-on-terror, the Islamist political parties have voiced their disapproval of the Pakistan's support to the United States unequivocally (Pike, 2012). The parties merged together in 2002 as a result of common opinion and to provide strong opposition to the President Musharraf's unconditional support to the United States for

[7] The 70 non-elected members are seats reserved for women (60) and minorities (10). These seats are elected through an indirect proportional representation based on share of parties in the other 272 elected seats.

[8] The parliament of Pakistan has bicameral structure composed of the Senate and the National Assembly (NA). In this paper we focus on the National Assembly Results for two reasons. First, the Senate elections only take place every 6 years in a staggered manner. There are only fourteen senators for each province, which yields a total of 66 seats. Second, the senators are usually the political elites and the MMA is not represented in the senate. Third, the electoral data on the senate elections is unavailable.

[9] Between 1989 and 1999, both PML (N) and PPP were incumbent three times. The PML (Q), PPP and PML(N) won the most seats in 2002, 2008 and 2013 respectively.

[10] The political alliance did not last more than a single election. In 2008, JI wanted to boycott the elections, while the other parties wanted to run for the elections. The parties, however, remained close in their ideology and continued to support each other (Hussain, 2006)

the war-on-terror (Norell, 2007).[11]

These parties have long standing relations with the Talibans in Afghanistan and share the same ideology. "All the individual parties in the MMA have links to militant groups, hence this coalition is of great interest when examining Pakistani links to the Taliban." (Norell, 2007, pg. 69). Moreover, "MMA ⋯ maintains close ties to its leadership (Johnson and Mason, 2008, pg. 58). For instance, JUI and JI were in the center of forming Taliban during the 1980s (International Crisis Group, 2011). The leader of the JUI, Sami ul Haq, is regarded as the "Father of Taliban" because many Taliban leaders, including Mullah Omar, graduated from his *Madrasah* (International Crisis Group, 2011). As many as $30,000$ Afghan refugee students from his *madrasahs* in Pakistan went to join the Taliban cause in Afghanistan (Abbas, 2014)

These parties have remained cordial with the Taliban even after the Afghanistan war. The parties not only openly voice their support for the Taliban, but also provide political cover to them (Johnson and Mason, 2008). For instance, Sami ul Haq, openly advocates Taliban to take back power of Afghanistan (Golovnina and Sardar, 2013). Similarly, the JI leader, Fazlur Rahman has recursively asked the Pakistan and U.S. government to negotiate with the Talibans and offered to act as a moderator between the two groups (Zaman, 2012). More recently, the JUI-F general secretary, Abdul Ghafoor Haideri, invited the Taliban to join the JUI-F directly (Express Tribune, 2017). These relations are mutual as Taliban also support these Islamist parties. For example, when there was an unsuccessful suicide bomb attack to kill Fazlur Rahman in 2014, the Taliban condemned that attack and called it "disgraceful" (Sherazi, 2014).

2.3 Taliban and public good provision

Taliban have a long history of providing public goods at the local level (Berman and Laitin, 2008). For instance, Berman (2003) notes: "In October 1994 the ISI sent a trial

[11]This pro-Taliban stance was very popular among the province of Khyber Pakhtunkhwa in which the party gained majority. The MMA was able to form a coalition government in the province of Balochistan.

convoy loaded with medicine from Quetta to Ashkabad, in Turkmenistan. When the convoy was held up by warlords south of Kandahar, a small, largely unknown group of radical Islamists, the Taliban, conveniently emerged to free it." (Berman, 2003, page 6).

The Taliban jumped to provide relief and rehabilitation aid to the affected areas. After the floods, the leader of Tehreek-e-Taliban Pakistan (Pakistan version of Taliban), Mullah Fazlullah, announced that "his men are returning to the area" (AsiaNews.it, 2010). They urged the government not to accept any foreign aid (CBSNews, 2010) and threatened to kidnap foreign workers providing relief efforts (Masood, 2010). The Taliban claimed that they would themselves provide money if the government ceases obtaining foreign aid (CBSNews, 2010).

The government was largely absent from the flood affected areas. The Taliban used the floods as an opportunity to re-assert their influence. One of the flood affectees remarked: "With the exception of a few Islamic organizations, nobody has been here," (Kazim, 2010). The "Islamists made sure that their presence was felt" (Kazim, 2010).

2.4 The 2010 Flood

The 2010 flood was caused by abnormal monsoon rains in late July which resulted in floods across all the provinces of Pakistan. It affected more than one-fifth of the land area and 20 million individuals directly. The flood resulted in more than $9.7 billion in economic damages (Dorosh et al., 2010).[12]

Pakistan is a country that is frequently flooded due to heavy rains in the summers and inefficient inundation network. However, the flood of 2010 was not like any other flood witnessed by Pakistan. The 2010 flood affected more than four times more individuals than the second largest flood in the history of Pakistan that took place in 1992 (Dorosh

[12]The cost of re-constructing the infrastructure was estimated to be around $ 8 billion. The flood caused large-scale damage not only to the houses and infrastructure of the area, but also resulted in wide-scale agricultural damages. More than $700,000$ acres ($3,000km^2$) of cotton, $200,000$ acres ($800km^2$) acres each of rice and cane, $500,000$ tonne of wheat and $300,000$ acres ($1,000km^2$) of animal fodder were destroyed by the flood (NDMA GOP, 2010b).

et al., 2010). According to UN, it was the greatest humanitarian crisis in recent history, with more people affected than were affected by the South-East Asian tsunami and the 2010 earthquake in Haiti combined Ferris (2011).

Figure 1 shows the extent of the flood in September 2010. As it is apparent from the figure, the flood affected significant part of Pakistan. The areas from all the major provinces were affected by the disaster. Many areas which never received any flood in the last 100 years were affected by the flood.

The government failed to respond to the flood promptly. The National Disaster Management Authority (NDMA) was completely ill-equipped and unprepared to deal with a natural disaster of such an extent. The NDMA was in complete disarray (Ahmed, 2013). To add to the unorganized response, the government miscalculated the gravity of the situation and did not act promptly. Although the floods started July $20th$, the first Flash Appeal for relief and recovery was not sent out until August $9th$. The poor rehabilitation and relief efforts were also due to lack of international support following the disaster. In the first three weeks after the flood, the international agencies only committed $ 45 million in donations.[13]

The poor rehabilitation was visible among the disaster affected regions. Doocy et al. (2013) surveyed households in the affected areas six months after the 2010 flood and found that there was still need for humanitarian aid in all the affected areas. Only two-third of the affected areas reported that they received some food aid after the floods. In addition, more than 60% of the food needs was unmet across the flood affected areas even six months after the flood.

These lack of adequate relief efforts resulted in people getting angry at the government.

[13]As a comparison, in the first ten days after the earthquake in 2005, the international governments had already committed $247 million (6 times more). This difference is particularly stark because the floods caused greater damage (1.25 times in economic damages and 5 times in people affected) than the earthquake. Other natural disasters around the world also receive significantly higher immediate amount of aid. For instance, in the first ten days of Cyclone Nargis, which hit Myanmar in May 2008, the international governments committed $110 million. Similarly, the earthquake in January 2010 in Haiti saw a commitment of $742 million during the first ten days of the disaster.

The areas affected by the floods were baffled by the governments response (Shah, 2010). The citizens felt helpless, as one of the affected individual lamented: "We are being treated like orphans, animals." (Independent, 2010). The response to the flood reinforced the perception of individuals about lack of state capacity (Independent, 2010).

2.5 Data sources

We use data sets from several different sources. We geo-locate the electoral districts using data from the Election Commission of Pakistan (ECP). In addition, we collect the electoral data from the ECP. For each electoral district, we have the number of votes won by each candidate and his/her political affiliation. In the 2002 elections, the Muttahida Majlis-e-Amal (MMA) participated as a single political party composed of the coalition of the following five parties: Jamiat-e-Ulema-e-Islam (JUI), Jamiat Ulema-e-Pakistan (JUP), Jamaat-e-Islami Pakistan (JI), Jamiat-e-Ahle Hadith, and Pakistan Islami Tehrik (ITP). The number of votes secured by the sum of these five parties is measured as the votes received by MMA.[14] In 2002, the MMA is represented in 171 out of 272 electoral districts. The MMA is widely represented in the provinces of Balochistan and Khyber Pakhtunkhwa.[15]

The data on the flood of 2010 is gathered from two different sources: for the areas which were flooded, we use United Nation Office for the Coordination of Humanitarian Affairs (UNOCHA) and for the funding gap, we utilize the National Disaster Management Authority (NDMA). We digitize the maps published by UNOCHA. The NDMA published

[14] We compute this share if two of the three major parties of the previous alliance MMA participate from that particular district. That is, if in any district less than the two major parties participate, we label it as if the MMA did not participate in that particular electoral district and do not consider it in the main specification. Results are robust to constructing MMA vote share in alternate ways. Table A2 shows the results in which MMA vote share is constructed in two different ways. First, we only consider MMA vote share if all the three major parties participated. Second, we consider MMA vote share as zero if they did not participate from an electoral district (instead of missing).

[15] Their representation is spread over all the provinces. They appeared in 13 out of the 14 electoral districts in Balochistan, and 32 out of the 35 electoral districts in Khyber Pakhtunkhwa. In the 150 electoral seats of Punjab and 61 electoral seats of Sindh, the MMA appeared in 90 and 36 electoral districts respectively.

the gap in funding faced by every district in form of categorical variable (five categories) six months after the floods. In order to assess the ex-ante risk of flooding and earthquake, we use the UNISDR (2016), which records the risk of several natural disasters around the world.

We use the GeoEPR 2014 dataset to determine whether an electoral district is ethnically composed of the Pashtun ethnicity (Vogt et al., 2015). The data on the socio-demographic characteristics of an electoral district are collected from the 1998 Census (Pakistan Bureau of Statistics, 1998).[16] We collect data on: population density, average household size, literacy rate, proportion of housing units with access to electricity, piped water, proportion of households working in agriculture, proportion of children under 5 years immunized and the proportion of population living in urban area. Since, all these variables measure the development of an area and tend to be highly correlated; we use the Principal Component Analysis (PCA) to compute the development index, which we use in our analysis.

3 Empirical Methodology

In this section, we provide brief overview of the empirical framework for testing our mechanism. We compare the MMA results before and after 2010 floods between places that were affected compared to unaffected areas. Specifically, we estimate:

$$MMA_{it} = \alpha_i + \delta Post_t + \beta(affected_i * Post_t) + u_{it}, \tag{1}$$

where MMA_{it} is the proportion of votes secured by the Muttahida Majlis-e-Amal in electoral district, i, at elections, t.

The variable $affected_i$ denotes whether electoral district, i, was affected by the natural disaster. We use the definition directly provided by the UNOCHA. $Post_t$ is a time dummy

[16]The Pakistan Bureau of Statistics (1998) reports the socio-demographic characteristics at a higher level (administrative district). The electoral districts are perfectly contained within an administrative district, so we assign the electoral district the value of socio-economic variables corresponding to the administrative district.

that indicates the election-year after the natural disaster. α_i are the electoral districts fixed effects. The change in MMA vote share common to all electoral districts is captured by the term δ.

This methodology has several advantages. First, it controls for the pre-existing differences among the electoral districts through the electoral district fixed effects. Second, the specification allows for differences in the result of election result for the MMA between elections through the term δ.

In order to obtain causal effect of the flood on the proportion of votes secured by the MMA in the election, the districts that receive treatment (flood) and do not receive flood should have a common trend. That is in the absence of the flood, the MMA vote share would have evolved in the same way in the districts which were flooded relative to districts which do not receive the flood.

The above is likely to not hold without proper controls. A first source of concern involves the risk of flooding. Some areas experience higher number of floods than the others due to pre-existing differences in the risk of flooding.[17] The trend in political outcomes may be different in the areas with high risk of flood, as the population there may be systematically different from average population. For instance, due to frequent natural disasters, these places may already have in place informal mechanisms to cope with the disaster and may rely less on the outside support (be that of government or the Taliban). This would violate the identification assumption and generate a bias in the obtained estimates. In order to account for this potential concern, we control for the ex-ante frequency of flood for each given electoral district multiplied by the time dummy ($frequency_i * Post_t$). This allows differential trends in MMA votes in areas with different ex-ante risk of flooding. Hence, our identification of the parameter β comes only from comparing the changes in MMA vote share in places which were affected by the 2010 floods to changes in MMA vote share in places with similar ex-ante risk of natural disaster which were not affected

[17] Due to heavy monsoon rains and melting of snow in the northern mountains in the summer, several areas around the main river, Indus, are flooded frequently (almost every other year).

by the 2010 floods.

One may be also concerned that the level of urbanization of an electoral district may explain the differences in MMA vote share. The urbanized areas may have pre-existing differences in trends in the MMA vote share. At the same time, the urbanized areas are less likely to be affected by the floods due to better intra-structure and protection against floods. We address this potential concern by allowing urban and rural electoral districts to have a differential trend in MMA vote share ($Urban_i * Post_t$).

Finally, one additional concern could be that the ethnic affinity of an electoral district to the ethnicity of the Taliban may explain the differences in MMA vote share. The areas with predominant Pashtun ethnicity may have pre-existing differences in trends in the MMA vote share. If these areas receive treatment disproportionately more (or less) than the other areas, we might expect to find average treatment effect due to these pre-existing differences. This may cause them having very different voting patterns towards the MMA independently of the natural disasters. We address this potential problem by allowing majority Pashtun electoral districts to have differential trend in MMA vote share ($Pashtun_i * Post_t$).

Hence, our preferred specification is:

$$\begin{aligned} MMA_{it} = \quad & \alpha_i + \delta Post_t + \beta Affected_i * Post_t + \gamma_1 frequency_i * Post_t + \\ & \gamma_2 Urban_i * Post_t + \gamma_3 Pashtun_i * Post_t + + u_{it}, \end{aligned} \tag{2}$$

where $frequency_i$ denotes ex-ante propensity of flooding for each electoral district, $Urban_i$ is a dummy equal to one for majority urban areas, and $Pashtun_i$ is a dummy that takes value 1 if the electoral district is composed of majority of Pashtun ethnicity. In all our specifications, we cluster the standard errors at the electoral district level.[18]

[18] In addition, the development level of a place may also be a cause of concern for the estimation of the causal impact of natural disaster on MMA vote share. The level of development of an electoral district may determine whether it is affected by the natural disaster, and the people in highly developed areas may vote systematically differently relative to individuals in low developed areas. Hence, we also allow for differential trend in the vote share of MMA w.r.t. the level of development of the area (measured in 1998).

4 Results

In this section we present the main results. First, we present the results of effect of flood on MMA share. Then, we use the data on funding gap to show evidence in favor of our mechanism. We show that alternate explanations are not consistent with the observed results.

4.1 Baseline Results

In this section, we discuss the effects of the flood on the vote share of the MMA. For all the estimations we use data from the 2008 and the 2013 national elections.

Table 2 shows the results. The estimate of β shows that the MMA won disproportionately more in the areas affected by the flood. Our preferred specification shows that the electoral districts affected by the flood experienced an extra 5.4 percentage points increase in the MMA vote share. This corresponds to a 40% (.0302/.079) increase in the votes for the MMA in the affected areas in 2013 compared to the MMA votes in affected areas in 2008. This translates to an extra $8,921$ voters for the MMA between the 2008 and 2013 in an average area affected by the flood. Overall, this constitutes an additional 2.37 million (of the total 44.2 million living in the affected areas) votes for the MMA in the affected areas relative to the unaffected areas by the flood. These results show that the natural disaster can lead to increase in the support for the Taliban.

4.2 Funding Gap

In order to shed more light on the mechanism underpinning these changes, we utilize the funding gap data (the difference between required and received aid). Following the previous sections, we estimate similar difference-in-difference specification:

$$MMA_{it} = \alpha_i + \delta Post_t + \beta FundingGap_{it} + X_{it}'\gamma + u_{it}, \tag{3}$$

where $FundingGap_{it}$ represents the funding gap in district i. It is equal to zero for all areas in 2008 (before the flood) and equal to the proportion of required aid that was not received for the year 2013.

One potential problem with the specification above is that funding gap may not be exogenous. The reason of this is that both the national government and international donors may decide to strategically provide aid in places that are either gaining or losing support for the Taliban. Additionally, is possible that even if the donors are not acting strategically they maybe just can't deliver aid in places where the Taliban had a lot of support.

The anecdotal evidence suggests that due to lack of international aid, the government was unable to assign aid systematically to areas. Nevertheless, we employ an Instrumental Variable (IV) strategy to address potential endogeneity of the funding gap. We use whether an area is affected by the flood and the severity of the flood to instrument the funding gap.[19]

The exclusion restriction assumes that the flood affects the electoral outcomes only through funding gap. The exclusion restriction will not hold if floods directly change the religiosity or political preferences, independent of the funding gap, which in turn impact electoral outcomes. Some recent evidence suggests that natural disasters affect time preferences (Callen, 2015) and trust and risk preferences (Cassar et al., 2017). However, it is not clear how these preferences are related to the preferences for extremist parties. It is difficult to imagine how the natural disaster changes directly the preferences for extreme political parties independent of the funding gap. Instead, we argue that the interaction between natural disaster and the funding gap is important, previously missing, part in order to understand how the people change their preferences. Moreover, we carry out over-identification test to show evidence in favor of validity of our instruments.

Table 3 shows how a higher level of funding gap in the 2010 flood resulted in an increase

[19]The places that are affected by the flood are mechanically likely to have higher funding gap. Within the affected areas, areas that were severely affected are likely to have higher funding gap.

in the votes for the MMA in the 2013 election. The OLS estimates in Column 1 show that 1 percentage points increase in the funding gap increased the MMA vote share in the 2013 by 0.093 percentage points. In column 3, we instrument the funding gap with whether the electoral district was affected by the 2010 flood. Column 2 shows the first stage. We see a clear positive relation between being affected by the flood and the funding gap. The F-statistic for the first stage is 59.86 suggesting strong relation between being affected by the flood and the funding gap. The IV estimates suggest that 1 percentage points increase in the funding gap increases the MMA vote share by 0.097 percentage points.

We then estimate the non-parametric relation between the change in MMA vote share and the funding gap. Similar to the DiD, we are comparing high funding gap areas to the low funding gap areas and analyzing the gain in votes by MMA in the high funding gap areas relative to the low funding gap areas. However, we are not imposing a linear relation between funding gap and change in MMA vote share. Instead, we are calculating the effect on MMA vote share for each local value of the funding gap.[20]

Figure 2 plots the non-parametric relation between the change in MMA vote share and the funding gap along with the 95% confidence interval. The figure shows that the relation between change in MMA vote share and funding gap is concave with very close to being linear. The relation is strongest in the interval where the funding gap is between 20 and 50%. Areas with funding gap of 40% experienced a three-fold increase in MMA vote share, while areas with funding gap 80% experienced a four-fold increase in the MMA vote share compared to the areas with very little funding gap.

The results clearly indicate that funding gap is an important determinant of increase in the vote share of MMA. These results highlight the importance of funding gap as one of the mechanisms through which the change in support for MMA is operating. The areas which had higher funding gap witnessed greater increase in the vote share of the MMA in

[20]Non-parametric estimation has several advantages over the parametric one. The estimation does not impose any functional form on the relation as in parametric estimation. Instead, it fits the best polynomial which explains the relation. Moreover, it finds relation at every point in the distribution of the dependent variable i.e. local regressions throughout the distribution. This is more informative than the average effects.

the entire range of funding gap.

4.3 Identification

In the sections above, we employ a difference-in-difference estimation strategy to establish the causal effect of being affected by flood on the increased support for Taliban. In this section we show several evidence in favor of our identification strategy.

4.4 Parallel Trends Assumption

Since we employ a difference-in-differences strategy, our identification relies on the assumption that the affected and unaffected areas would have had similar trends in the absence of the floods. This assumption is directly untestable. However, since we have electoral data for two elections before the floods, we can provide evidence in favor of our identification assumption. We do that by showing that the vote share of MMA did not change differently between the 2002 and 2008 elections in the affected areas relative to the unaffected areas.

Table 1 shows the results of the baseline regression for the flood using data before the flood occurred (2002 and 2008 election years). The results are illustrated graphically in the Figure A1. The results show that for our preferred specification (column 1) there were no significant differences in trends in the affected areas compared to the unaffected areas before the flood. In addition, there are no differences in trend in the MMA vote share in affected and unaffected areas prior to the flood even after controlling for political competition and development index. Hence, the results suggest that the identification of our parameter of interest does not come from pre-existing differences but rather only from the fact that some areas were affected by the flood while others were not.

4.5 Falsifications Tests

In order to test if there are any unobserved trends in the data driving our results, we carry out a falsification test. The falsification exercise randomly assigns the status of affected by the natural disaster to electoral districts with the same proportion as the actual natural disasters. The 2010 floods affected 41% of the electoral districts. We estimate:

$$MMA_{it} = \alpha_i + \delta POST_t + \beta(FAKEaffected_i * POST_t) + X'_{it}\gamma + u_{it}, \qquad (4)$$

where as in the previous estimation $t = 2008, 2013$ for the analysis. X_{it} includes $Pashtun_i * POST_t$ and $frequency_i * POST_t$ in the analysis of the flood. We repeat 1000 times.

The distribution of β coefficients obtained from the falsification exercises are illustrated in the Figure 5. The red line indicates the results obtained using the actual affected status. The placebo estimates lie between -0.04 and 0.04 . Only three out the 1000 combinations of placebo assignments of being affected by the flood has an effect larger than the actual treatment effect. These results are encouraging as it shows that there is something specific to the places affected by the natural disasters that creates this big loss in votes, in the case of the earthquake, and a big gain in the case of the flood.

4.6 Additional Robustness

In the Appendix, we present additional estimates which demonstrate that the results are robust to alternate estimations. In Table A1 we test whether there were any significant spillovers of the natural disasters on the neighboring electoral districts. Columns 3 and 4 of Table A1 show that there are no significant spillovers to the nearby places. Column 5 shows that the results remain the same if we estimate the long-run impact of earthquake and the effect of flood together using the data from all the elections: 2002, 2008 and 2013.

In the main specification, we only consider the vote share of MMA if two out of the three major Islamist parties are running from an election district. In Table A2, we define the

MMA vote share in alternate ways. In columns 1 and 2, we consider the MMA vote share only when all the three major Islamist parties are present. The results, if anything, are stronger than the one in the main analysis. In Columns 3 and 4, we consider the MMA vote share as zero (instead of missing) if the MMA did not run from a particular electoral district. This implies that these estimations use the entire set of electoral districts for the estimation. The results remain practically unchanged. The results are slightly weaker than the ones in the main section because we are replacing the non-MMA participation with zeros.

In order to show that the results are not driven only from a particular province, we add province-year fixed effects. The results for the earthquake are slightly weaker (though still statistically significant) and results for the flood are slightly stronger (Columns 5 and 6), suggesting that the effect is not concentrated in a particular province. In addition, in Columns 7 to 10, we estimate weighted regressions instead of unweighted ones. The results are unchanged if we weight the regressions by turnout (Columns 7 and 8) or by $ln(turnout)$ (Columns 9 and 10).

We replicate the results of 2010 floods using the data from the Provincial Assembly. Table A3 shows the results. The MMA won 2.3 percentage points more in the areas affected by the flood relative to the other areas (Column 1). In columns 2 and 4, we show that the funding gap is the main mechanism explaining the results. A 1 percentage point increase in the funding gap increases the MMA vote share by 0.04 percentage points (OLS; Column 2) or by 0.03 percentage points (IV; Column 6).

5 Mechanisms and alternative explanations

In this section we provide evidence that our mechanisms is the one at play. Specifically, we show that sufficient state capacity may deter support for the Taliban by exploiting the 2005 earthquake. In addition, we show that the results are stronger in areas where the Taliban are more likely to have resources to compete with the government and where the damage from the floods is high.

5.1 The state capacity mechanism

5.1.1 The 2005 Earthquake

The previous section showed how lack of state capacity impacts support for NSO. In this section, we show that the results also operate in the opposite direction. That is, adequate state capacity can crowd out support for the NSO. In order to show that is the case, we use the 2005 earthquake. The government, with the help of international community, was quick to respond to the calamity. Within three days of the disaster, the government, through national and international donations, secured \$312 million, which was enough to coordinate and provide the emergency response for the first three months (Wilder, 2008). Similarly, within the first week 24 U.S. helicopters and 1,200 military personnel came to Islamabad to assist in the relief efforts (Wilder, 2008). The response was so well coordinated that all the relief efforts were completed by the March 2006 (Andrabi and Das, 2010). These efforts are discussed among the natural disaster response as a specimen for an effective response to any natural disaster: " the earthquake was perceived by many to be one of the largest and most effective responses to a natural disaster to date." (Wilder, 2008, pg. 8)

In this section, we discuss the effects of the earthquake on the vote share of the MMA. For all the estimations we use data from the 2002 and the 2008 national elections. We define an electoral district as affected if it is within 200 km from the earthquake epicenter.[21]

As displayed in the Table 4 the MMA lost disproportionally more in the places affected by the 2005 earthquake

Our preferred specification is the one in column (4) which controls for the differential trends w.r.t. risk of earthquake, Pasthun ethnicity and urbanization. The results show

[21] In the Appendix, we show that the results are robust to considering affected as: 150 km radius, 300 km radius or the continuous distance.

that places affected by the earthquake experienced an extra 18.7 percentage points drop in the MMA vote share. The effect is economically large in magnitude, as it corresponds to around half of the MMA vote share in the affected areas in 2002 and is twice the average vote share lost in the unaffected areas. This translates into $23,779$ less voters for the MMA between the 2002 and 2008 in an average electoral district affected by the earthquake. Overall, this translates into 3.12 million lower votes for the MMA in the areas affected by the earthquake.

The results show that the Taliban lost in the areas affected by the earthquake. These results together with the results from the impact of 2010 floods and the impact of funding gap on Taliban support paint a consistent picture in favor of our proposed hypothesis.

5.1.2 Distance from Afghanistan

The mechanism we propose has a testable implication. Since the extent of lack of state capacity depends on how much the Taliban feel the void, we should see that the Taliban gain more votes in areas where they help the most. The capacity of the Taliban to provide support in a region greatly depends on their presence on the territory before the natural disaster occurs. One proxy for the ex-ante presence of Taliban is distance of the electoral district from the Afghanistan border.[22] Areas closer to the Afghanistan border are more likely to receive greater Taliban support, everything else equal.

This generates testable implication according to our mechanism. Places close to the Afghan border usually receive many social services from the Taliban. In the case of the 2010 flood, places close to the Afghan border managed to receive more aid from Taliban with respect to other places, which should lead to a disproportional increase

[22]This is mainly because the Taliban originate from the area close to the Afghanistan border and have many tribal connections with this area. The Taliban have their headquarters in the North Waziristan and Mohamand agency in FATA, next to the Afghanistan border.

in MMA votes in places close to Afghanistan.[23] This can be also thought as a triple difference-in-difference specification were we compare places affected or not by the natural disaster, before and after and observe how the results differ between places close and far away from the Afghan border.

Column 3 in Table 6 shows the estimates for a linear $f_1(.)$. We see that the increase in MMA votes is concentrated around the Afghan border. As the distance increases, and presumably the capacity of the Taliban to help decreases, the gains for the MMA party also decreases. Figure 4 illustrates the results graphically. We see that after the flood most of the gains are concentrated in the area 100 km around the border.

5.1.3 Intensity of the floods

Another testable implication of our mechanism is that, independent of the relief efforts, the lack of state capacity should be more prevalent in the areas which suffered more due to the floods. Specifically, we analyze how the effect of the floods changes with its intensity. We proxy the intensity of the severity of the floods by the distance from the main river Indus.

In column (5) of Table 6 we allow for MMA to gain differential votes based on the severity of the floods. The results show that severely affected areas display a slightly higher increase in the MMA vote relative to moderately affected areas, though the difference is not statistically significant. Another proxy for the severity of the flood is the distance from the river Indus.[24]

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[23] The following equation captures the mechanism formally:

$$\begin{aligned} MMA_{it} = \quad & \alpha_i + \delta POST_t + \beta_1 Affected_i * Post_t + f_1(dist\_afgh_i) * Post_t \\ & f_2(dist\_afgh_i) * Post_t * Affected_i + X_{i,t}^{'}\gamma + u_{it}, \end{aligned} \tag{5}$$

where $dist\_afgh_i$ is the distance of the electoral region, i, from the Afghanistan border.

[24] We estimate equation similar to the Equation 25. Notice that the distance from the river Indus is an imprecise proxy for the severity of the flood because it severity depends on many other geographical factors such as the morphology of the terrain around the river.

Figure 3 on the left panel illustrates the results graphically.[25]. Figure 3 on the right panel illustrate the gain in MMA vote share w.r.t. distance from river Indus. The MMA gained around 5 percentage points in electoral districts at less than 200 km from the river Indus. The effect decreases from 200 to 350 km until it reaches zero at 350 km.[26] The graph in-line with the fact that the flood affected a large proportion of the Pakistani population and consequently letting the MMA gain votes in many electoral districts.

5.2 Alternate Explanations

In this sub-section, we provide evidence against three alternate explanations. Specifically, we show that the results are not driven by anti-incumbent motivations, political competition, increase in religiosity, and changes in the political participation of voters and parties.

5.2.1 Punishing or Rewarding the Incumbent

An alternative explanation that could generate the results found in the previous section is the punishing the incumbent after a natural disaster (Cole et al., 2012). If this is true, we should see a systematic decrease in incumbent vote share in the areas affected by the natural disaster.

Another alternative explanation could be that the voters reward (punish) the incumbent based on response to the natural disaster (HEALY and MALHOTRA, 2009). If this is true, we should see an increase in the vote share of the incumbent party in the affected areas in 2008 elections and a decrease in the 2013 elections.

Results in Table 5 show that the incumbent vote share is unaffected by the natural

[25]Specifically, we estimate the following equation using function $f(.)$ as a 4th degree polynomial:

$$MMA_{it} = \alpha_i + \delta Post_t + Post_t * f(dist\, epi_i) + X^{'}_{i,t}\gamma + U_{it}, \tag{6}$$

[26]The median distance from river Indus is 112 km with only 15% of the electoral districts being more than 300 km away from the river

disasters as described by this theory. In the earthquake scenario, as shown in column (1), the incumbent party, PML(Q), did not won or lost votes systematically more in the areas affected by the earthquake. Similarly, in the flood scenario, PPP (the incumbent in 2008), did not systematically gain or lose votes in the areas affected by the flood.

5.2.2 Political Competition

Another natural explanation for the observed change in MMA vote share can be political competition. This explanation is closely related with the previous one. In this framework not only the votes for the incumbent could be affected by the natural disaster but also its main competitors. In the case of the 2005 earthquake given the good performance of the government, the main political competitor could observe a decrease in votes due to political competition. Instead in the badly managed 2010 flood, we could see an increase of votes for the main political competitors.

Results in Table 5 show that the results for the two main political competitors did not change. In the earthquake scenario, the main competitors PML(N) and PPP did not lost (or gained) systematically in the affected areas (Columns 2 and 3). Similarly, in the flood scenario, as shown in column (5) and (6), the two main competitors: PML(N) and PML(Q) did not won (or lost) specifically in places affected by the flood.

MMA was the only political party that systematically lost in the affected areas after the 2005 earthquake and systematically gained in the affected areas after the 2010 elections. The main feature that differentiates the MMA from other political parties is their connection to the NSO (Taliban) and their ability of providing goods that compete with the formal state.

5.2.3 Political Participation

There could be two alternate explanations which would make political participation the center of the argument instead of state capacity. On one hand, we can argue that people increase political participation after the natural disaster and this leads to the observed change in the vote share of the MMA. On the other hand, the natural disaster can displace people away from the affected areas leading to lower political participation and an effect on the MMA vote share.

Table A1 shows estimates the impact of natural disaster on the turnout. Column 3 shows that there is no significant increase in the turnout in the affected areas relative to the other areas. Similarly, we do not see any systematic increase in the number of political parties running for elections from areas affected by the earthquake relative to the unaffected areas (Column 4). One consequence of the floods could be that it induces the MMA to participate more from these areas relative to the unaffected areas. We do not see any systematic increase in MMA being represented from areas affected by the floods compared to unaffected areas.

5.2.4 Long-run Effects

What is the long-run impact of state capacity on the support for NSO? We can answer this question by assessing the impact of earthquake in the subsequent elections after the natural disaster. In particular we estimate this, by running the baseline analysis with the sample that include all elections and allow the earthquake to have potentially different effects in the short-run (2008 election) and the long-run (2013 election) .[27]

Column 4 in Table 6 shows the results. The effect of the earthquake decreases in time but is still statistically significant also in 2013, eight years after the earthquake. This is further evidence of the power of state capacity for eradicating terrorist groups.

[27] Specifically, we estimate the following:

$$\begin{aligned} MMA_{it} = \quad & \alpha_i + \delta_1 1(Year = 2008) + \delta_2 1(Year = 2013) + \beta_1 1(Year = 2008) * Affected_i \\ & + \beta_2 1(Year = 2013) * Affected_i + X^{'}_{i,t}\gamma + U_{i,t} \end{aligned} \tag{7}$$

6 Discussion & Conclusion

In this paper we provide first evidence that the state capacity can directly determine the support for the extremist groups. We use the unique context of Pakistan in which the Taliban are politically represented by the MMA. The two natural disasters in the near history provide exogenous variation in the needs of the citizens. An efficient handling of the 2005 earthquake (adequate state capacity) resulted in crowding out of the support for Taliban from these areas, while lack of state capacity (2010 floods) result in the opposite scenario.

The results shown above highlight an important determinant of extremist ideology and support for extremist groups. Individuals respond to the way non-state actors and the state provides for them. In particular we show that the efficiency of the state in the post-natural disaster period can move individuals away or to a terrorist organization. Future public policy and research should take into account the complementarity between government relief efforts and rise of extremist groups in areas with weak institutions and extremism.

The estimates provided in our study show how extremely reactive support for violent groups is with respect to changes in state capacity. In particular, our results shed a light how powerful international aid can act as anti-terrorist tool. From back of the envelope calculations, the effect of lack of state we can study the difference in funding. In the 2005 earthquake around 45% of the aid was already delivered after two months. In contrast in the 2010 flood only 25% was delivered. This 20 percentage points difference is equivalent to around $2 billion. Given our estimates, the $2 billion lower international aid moved around 1.3 million voters to vote for the MMA. For a comparison, in 2010 the US spent $181 billion for the war on terror in Iraq and Afghanistan (Belasco, 2009). These $2 billion are equivalent to only 4 days of war on terror while at the same time being extremely efficient in reducing radicalization of citizens in the region.

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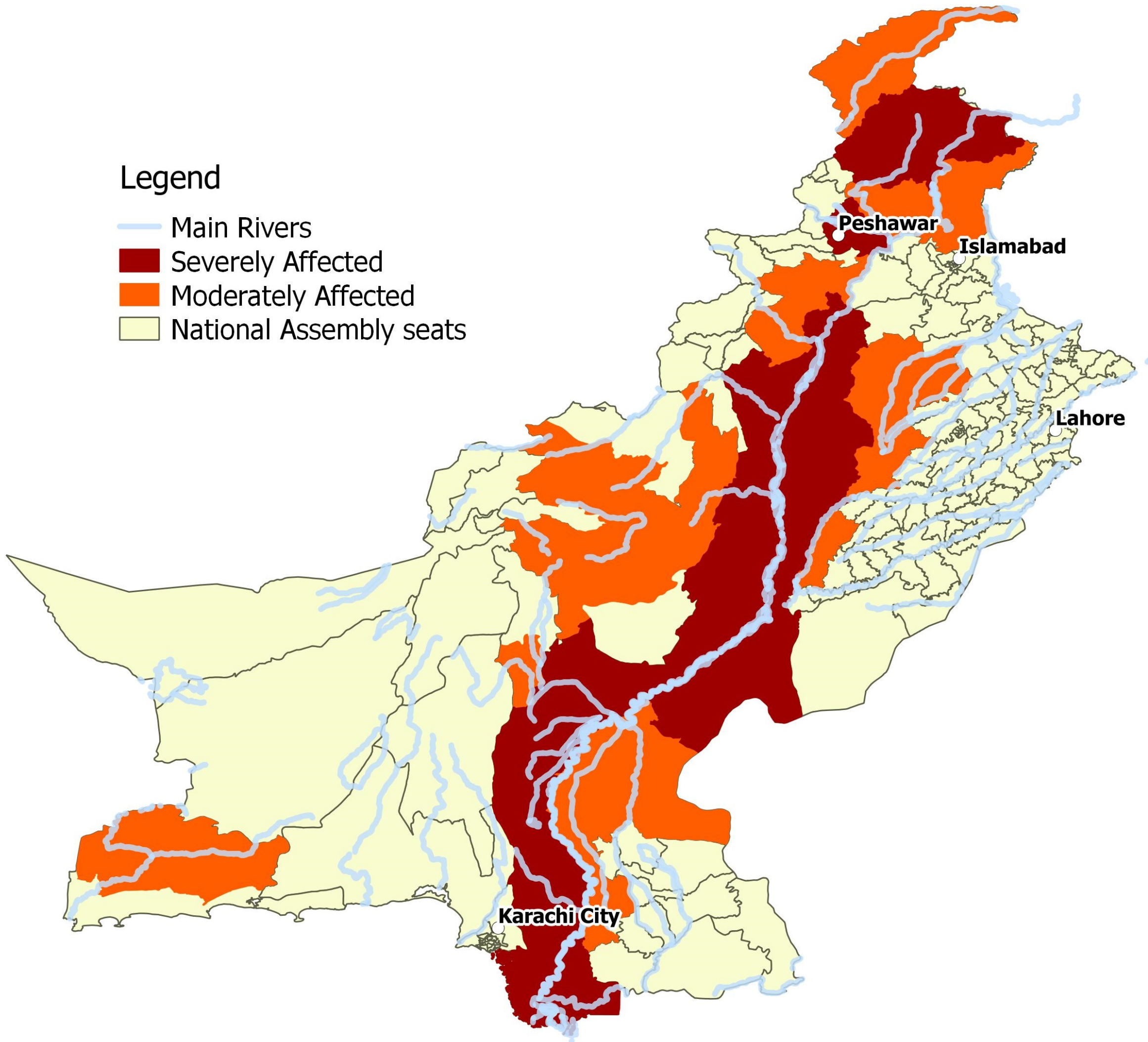
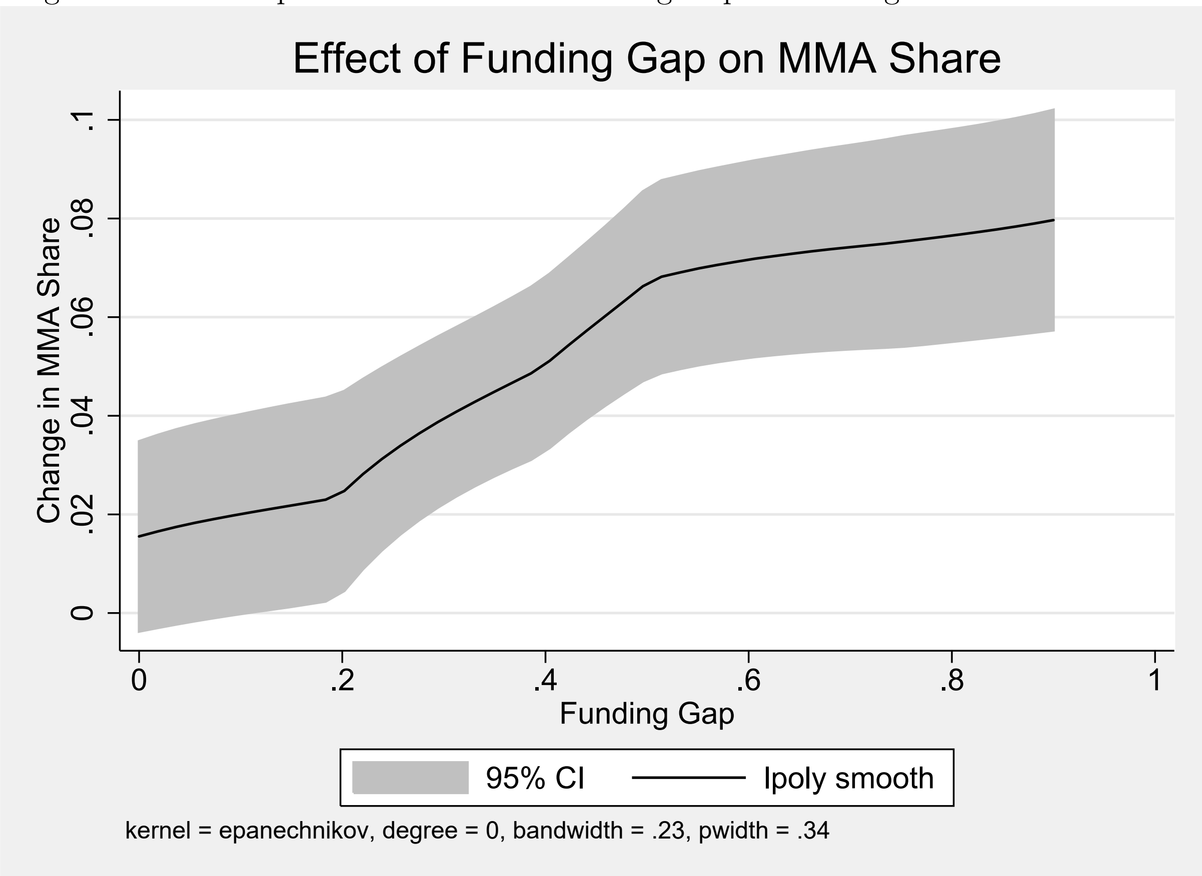
Figure 1: The extent of the 2010 floods



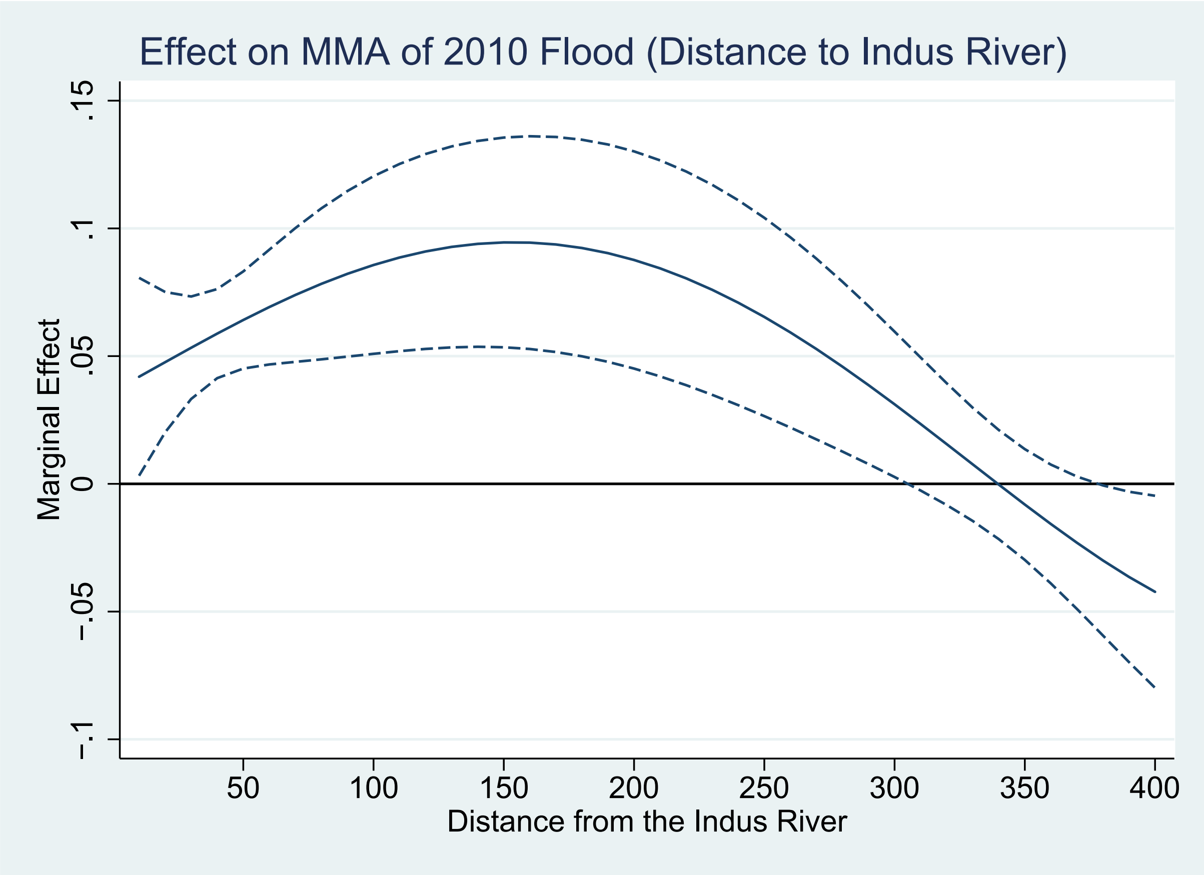
Notes: The figure shows the map of Pakistan overlaid with the extent of the 2010 floods. The severely affected areas are shown in red, moderately affected areas in orange, while the unaffected areas are shown in yellow.

Figure 2: The non-parametric relation: Funding Gap and Change in MMA vote share



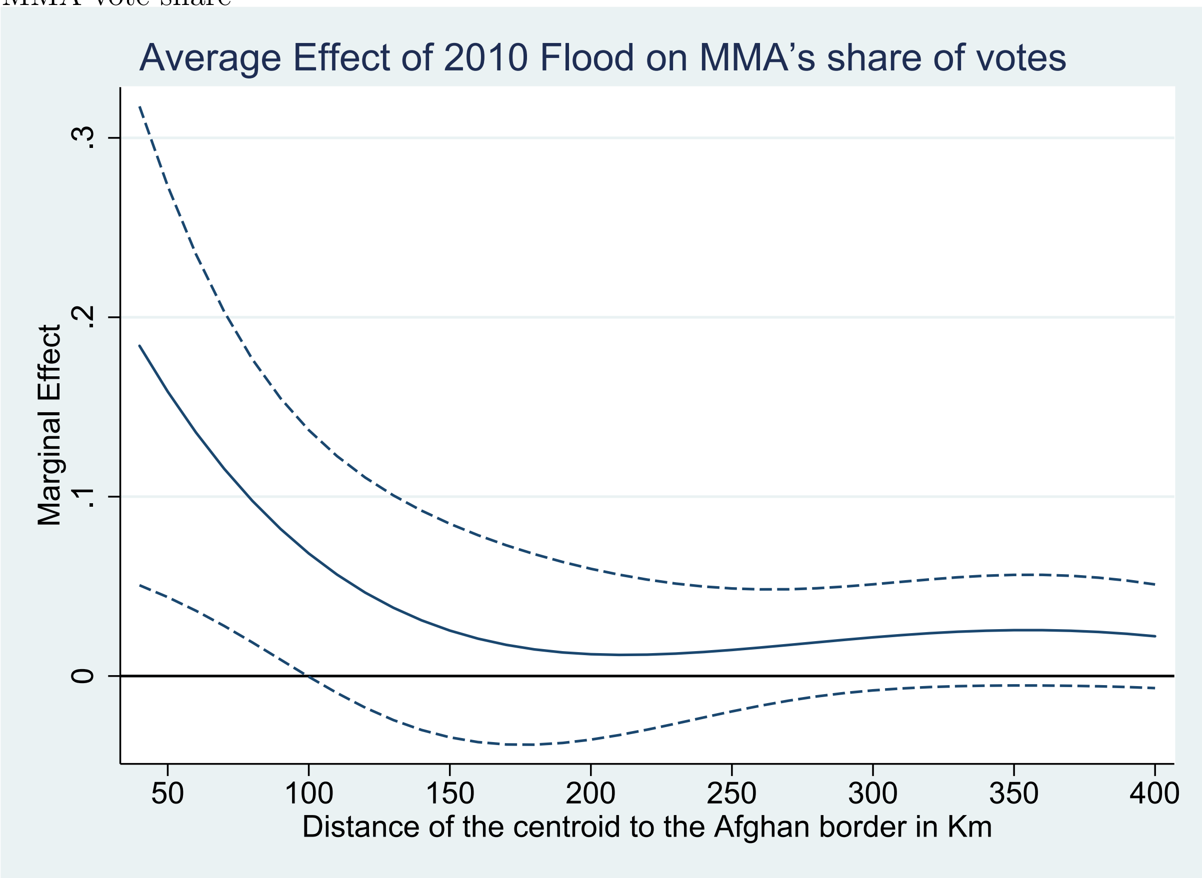
Notes: The figure shows the non-parametric relation between the funding gap in an electoral district in the 2010 floods and the change in the MMA vote share from 2008 to 2013. The estimates are plotted with the black line, while the grey shaded area represents the 95% confidence interval.

Figure 3: Relation between floods intensity and the effect of 2010 Floods on MMA vote share



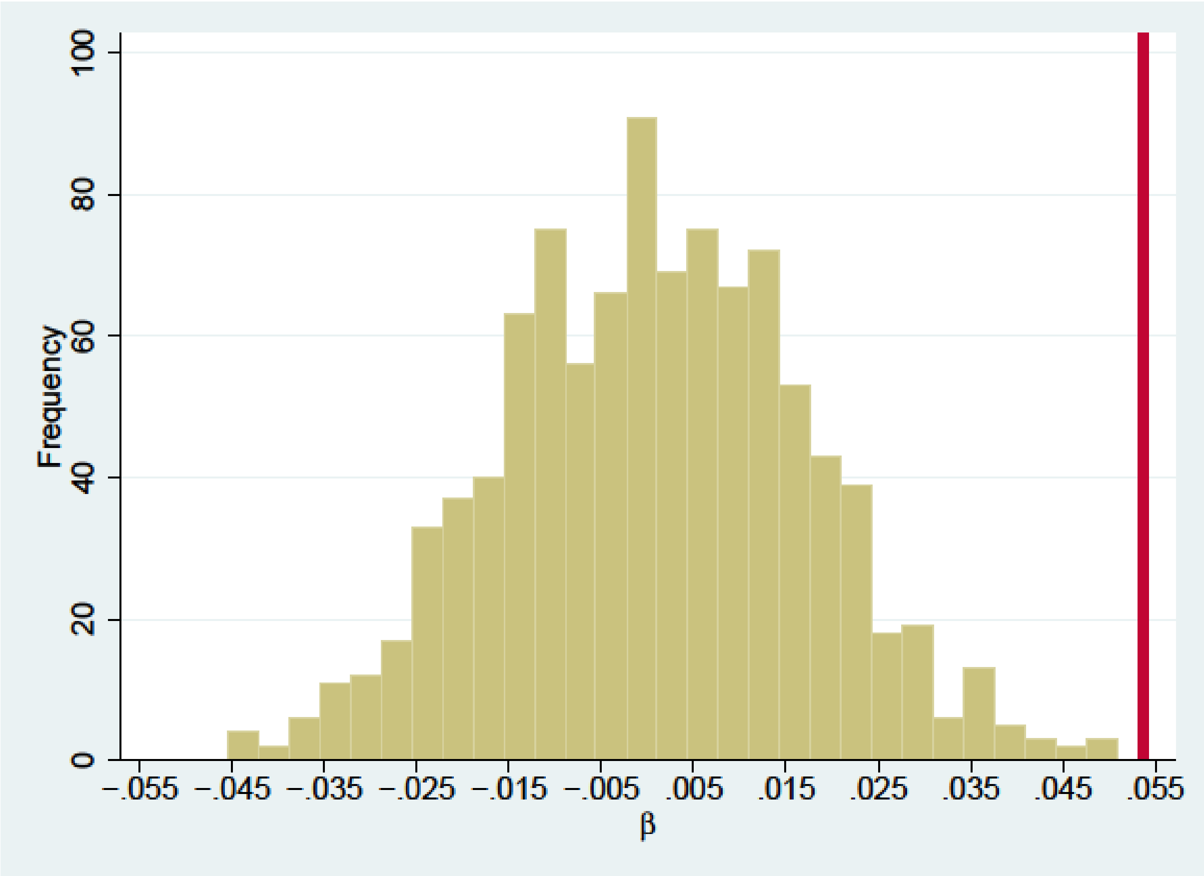
Notes: The figure plots the marginal impact of distance of an electoral district from the Indus river on the MMA vote share. The results are estimated using fourth order polynomial in the distance. The estimates are represented by the solid line, while the dashed lines represent the bounds for the 95% confidence interval.

Figure 4: Relation between distance from Afghanistan and the effect of 2010 Floods on MMA vote share



Notes: The figure plots the marginal impact of distance of an electoral district from the Pakistan-Afghanistan border on the MMA vote share. The results are estimated using fourth order polynomial in the distance. The estimates are represented by the solid line, while the dashed lines represent the bounds for the 95% confidence interval.

Figure 5: The distribution of the fake natural disaster effect on MMA vote share



Notes: The figure plots the distribution of estimates of β from the falsification exercise carried out by randomly assigning whether an area is affected by the flood or not to different electoral districts while keeping the proportion of districts that are affected by the flood same.

Table 1: Testing Parallel Trends

	(1) Share MMA	(2) Share MMA	(3) Share MMA	(4) Share MMA
Affected * Y2008	0.00362	-0.0169	0.00644	-0.0645
	(0.0349)	(0.0359)	(0.0396)	(0.0413)
Observations	276	276	276	274
Freq. Flood * Y2008	NO	YES	YES	YES
Pashtun * Y2008	NO	NO	YES	YES
Urban * Y2008	NO	NO	NO	YES

Notes: The table illustrates the difference in trend in the vote share of MMA between 2002 and 2008 using OLS estimation. The main dependent variable is the vote share of MMA in 2002 and 2008, and the main independent variable is interaction of whether an area was affected by the flood in 2010 and the time indicator equal to one for the year 2008. Column 1 shows the unconditional estimates. Column 2 includes differential trend across areas with different ex-ante propensity of flooding. Column 3 adds differential trend based on whether the dominant ethnicity of the district is Pasthun or not, and Column 4 further allows for differential trend for rural and urban areas. In all the estimations the standard errors are clustered at the electoral district level. * indicates significance at 10% significance level, ** indicates significance at 5% significance level, while *** indicates significance at 1% significance level.

Table 2: Effect of 2010 Floods on MMA Vote Share

	(1)	(2)	(3)	(4)
	Share MMA	Share MMA	Share MMA	Share MMA
Affected * Y2013	0.0416***	0.0478***	0.0422***	0.0537***
	(0.0148)	(0.0157)	(0.0155)	(0.0153)
Observations	331	331	331	318
Freq. Flood * Y2013	NO	YES	YES	YES
Pashtun * Y2013	NO	NO	YES	YES
Urban * Y2013	NO	NO	NO	YES

Notes: The table illustrates the impact of the 2010 floods on the MMA vote share using OLS estimation. The main dependent variable is the vote share of MMA, and the main independent variable is interaction of whether an area was affected by the flood in 2010 and the time indicator equal to one for the year 2013. Column 1 shows the unconditional estimates. Column 2 includes differential trend across areas with different ex-ante propensity of flooding. Column 3 adds differential trend based on whether the dominant ethnicity of the district is Pasthun or not, and Column 4 further allows for differential trend for rural and urban areas. In all the estimations the standard errors are clustered at the electoral district level. * indicates significance at 10% significance level, ** indicates significance at 5% significance level, while *** indicates significance at 1% significance level.

Table 3: Effect of Funding Gap in 2010 Floods on MMA Vote Share

	(1) Share MMA	(2) Funding Gap * Y2013	(3) Share MMA	(4) Funding Gap * Y2013	(5) Share MMA
Funding Gap * Y2013	0.0931***		0.0971***		0.105***
	(0.0164)		(0.0255)		(0.0254)
Affected * Y2013		0.554***			
		(0.0643)			
Moderate Aff * Y2013				0.532***	
				(0.0682)	
Severe Aff * Y2013				0.609***	
				(0.0605)	
Observations	318	318	318	318	318
Adjusted R^2	0.456	0.803	0.133	0.804	0.136
Result	OLS	FS	IV	FS	IV
Freq. Flood * Y2013	YES	YES	YES	YES	YES
Pashtun * Y2013	YES	YES	YES	YES	YES
Urban * Y2013	YES	YES	YES	YES	YES
F-stat	.	.	59.68	.	30.33
Pvalue Overind.	.	.	.	.	0.163

Notes: The table illustrates the impact of funding gap during the 2010 floods on the vote share of MMA using both OLS and IV estimations. In Columns 1, 3 and 5, the main dependent variable is the vote share of MMA, while in Columns 2 and 4 the main dependent variable is the interaction between funding gap and time indicator equal to one for the year 2013 (first stage). The main independent variable in Columns 1, 3 and 5 is the interaction of funding gap with time indicator equal to one for the year 2013. In Columns 2 and 4, the main independent variables are interaction of whether a district is affected by flood with time indicator equal to one for the year 2013 and interaction of intensity of how much an area is affected by flood with time indicator equal to one for the year 2013 respectively. In all the estimations, we allow for differential trend across areas with different ex-ante propensity of natural disaster, whether the dominant ethnicity of the district is Pasthun or not, and for rural and urban areas. In all the estimations the standard errors are clustered at the electoral district level. * indicates significance at 10% significance level, ** indicates significance at 5% significance level, while *** indicates significance at 1% significance level.

Table 4: Effect of 2005 Earthquake on MMA Vote Share

	(1)	(2)	(3)	(4)
	Share MMA	Share MMA	Share MMA	Share MMA
200Km * Y2008	-0.154***	-0.165***	-0.162***	-0.187***
	(0.0452)	(0.0470)	(0.0454)	(0.0459)
Observations	276	276	276	274
Freq. Earth * Y2008	NO	YES	YES	YES
Pashtun * Y2008	NO	NO	YES	YES
Urban * Y2008	NO	NO	NO	YES

Notes: The table illustrates the impact of the 2005 earthquake on the MMA vote share using OLS estimation. The main dependent variable is the vote share of MMA, and the main independent variable is interaction of whether an area was affected by the earthquake in 2005 and the time indicator equal to one for the year 2008. Column 1 shows the unconditional estimates. Column 2 includes differential trend across areas with different ex-ante propensity of earthquake. Column 3 adds differential trend based on whether the dominant ethnicity of the district is Pasthun or not, and Column 4 further allows for differential trend for rural and urban areas. In all the estimations the standard errors are clustered at the electoral district level. * indicates significance at 10% significance level, ** indicates significance at 5% significance level, while *** indicates significance at 1% significance level.

Table 5: The Effect of Natural Disasters on Incumbents and Competitors

	Earthquake			Flood		
	(1)	(2)	(3)	(4)	(5)	(6)
	Incumbent	Competitor 1	Competitor 2	Incumbent	Competitor 1	Competitor 2
POST * Affected	0.0290	-0.00995	0.0863**	0.0410	-0.00857	-0.0535
	(0.0270)	(0.0235)	(0.0384)	(0.0301)	(0.0353)	(0.0596)
Observations	358	466	353	460	409	238
Freq. Disaster * Post	YES	YES	YES	YES	YES	YES
Pashtun * Post	YES	YES	YES	YES	YES	YES
Urban * Post	YES	YES	YES	YES	YES	YES

Notes: The table illustrates the impact of the 2005 earthquake and the 2010 floods on the vote share of other political parties using OLS estimation. The main independent variable in Columns 1 to 3 (Columns 4 to 6) is interaction of whether an area was affected by the earthquake in 2005 (floods in 2010) and the time indicator equal to one for the year 2008 (2013). The main dependent variables are the incumbent vote share in Columns 1 and 4, vote share of the major political party in Columns 2 and 5, and vote share of second major political party in Columns 3 and 6. The equation is estimated using the years 2002 and 2008 for Columns 1 to 3 and 2008 and 2013 for Columns 4 to 6. In all the estimations, we allow for differential trend across areas with different ex-ante propensity of natural disaster, whether the dominant ethnicity of the district is Pasthun or not, and for rural and urban areas. In all the estimations the standard errors are clustered at the electoral district level. * indicates significance at 10% significance level, ** indicates significance at 5% significance level, while *** indicates significance at 1% significance level.

Table 6: Heterogeneity and Robustness of Results

	Earthquake			Flood	
	(1)	(2)	(3)	(4)	(5)
	ln(Dist Epi)	Long-Run	Dist Afgh	Severity	Dist Afgh
ln(dist. epi) * Y2008	0.0589***				
	(0.0202)				
Affected * Y2008		-0.178***	-0.492***		
		(0.0388)	(0.0660)		
Affected * Y2013		-0.120***			0.138***
		(0.0315)			(0.0259)
Affected * Dist Afgh * Y2008			0.218***		
			(0.0358)		
Affected * Dist Afgh * Y2013					-0.0287***
					(0.00701)
Moderate * Y2013				0.0418**	
				(0.0166)	
Severe * Y2013				0.0847***	
				(0.0241)	
Observations	274	488	274	318	318
Pashtun * Y2008	YES	YES	YES	NO	NO
Pashtun * Y2013	NO	YES	NO	YES	YES
Freq. Distater * Y2008	YES	YES	YES	NO	NO
Freq. Distater * Y2013	NO	YES	NO	YES	YES

Notes: The table illustrates the heterogeneous impact of the 2010 floods on the MMA vote share using OLS estimation. The main dependent variable is the vote share of MMA. Columns 1 and 3 use the years 2002 and 2008, Columns 4 and 5 use the years 2008 and 2013, and Column 2 uses years 2002, 2008 and 2013 for the estimation. In Column 1, the main independent variable is the interaction between distance from the earthquake epicenter and the time indicator equal to one for the year 2008. In Column 2, the main independent variables are the interaction between whether an area is affected by the earthquake and the time indicator equal to one for the year 2008 and 2013. In Column 3, the main independent variables are the interaction between whether an area is affected by the earthquake and the time indicator equal to one for the year 2008 and its interaction with distance from the Afghanistan border. In Column 4, the main independent variables are the interaction between whether an area is severely or moderately affected by the floods and the time indicator equal to one for the year 2013. Finally in Column 5, the main independent variables are the interaction between whether an area is affected by the floods and the time indicator equal to one for the year 2013 and its interaction with distance from the Afghanistan border. In all the estimations, we allow for differential trend across areas with different ex-ante propensity of natural disaster, whether the dominant ethnicity of the district is Pasthun or not, and for rural and urban areas. In all the estimations the standard errors are clustered at the electoral district level. * indicates significance at 10% significance level, ** indicates significance at 5% significance level, while *** indicates significance at 1% significance level.

APPENDIX

Figure A1: Testing Parallel Trends

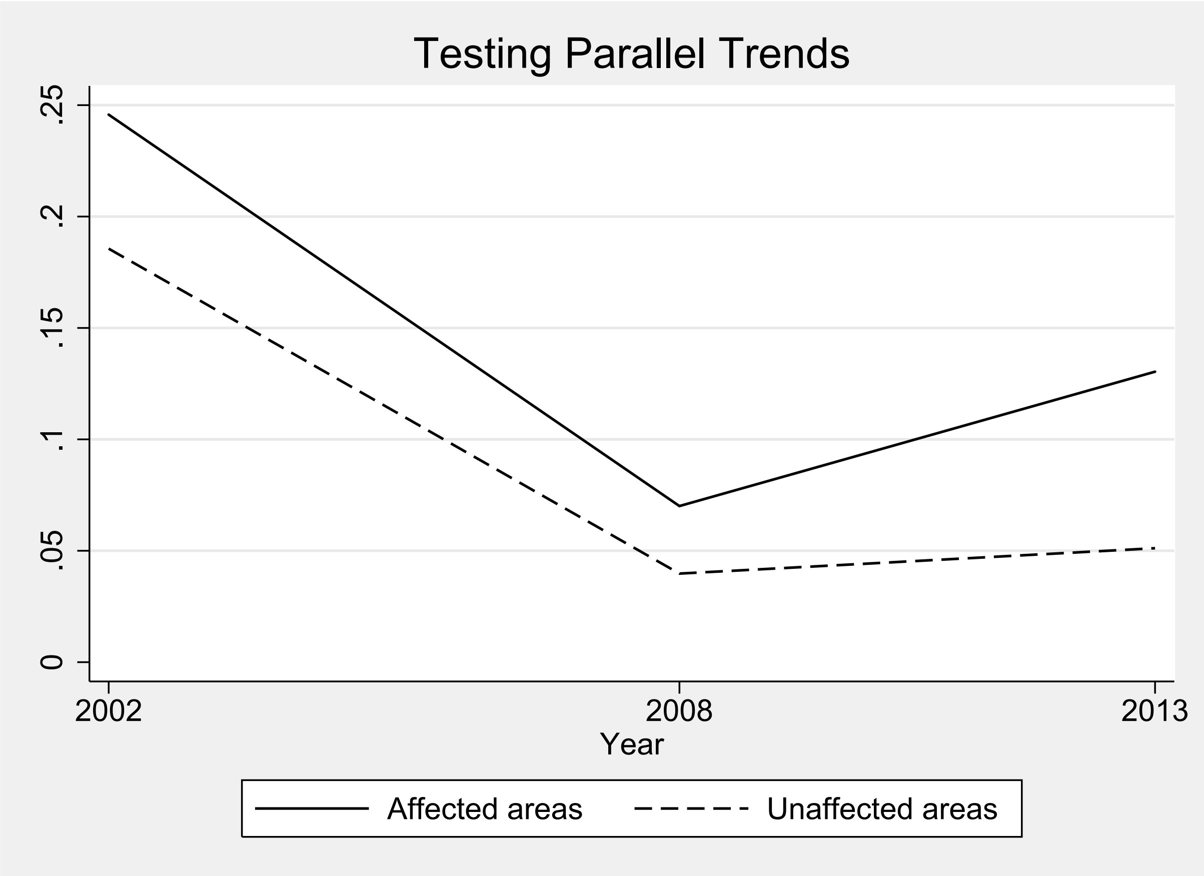


Table A1: The Effect of 2010 Floods on Other Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Share MMA	Share MMA	Turnout	Parties	P(MMA present)	P(MMA present)
Flood * Y2013	0.0374**	0.0663***	0.0115	0.0911	-0.147	-0.0527
	(0.0179)	(0.0178)	(0.0382)	(0.990)	(0.113)	(0.0891)
Earthquake * Y2008	-0.166***					
	(0.0374)					
Earhquake * Y2013	-0.111***					
	(0.0298)					
Neighbor Flooded * Y2013		0.0297				
		(0.0188)				
Observations	488	318	514	518	208	310
Adjusted R^2	0.648	0.401	0.636	0.639	0.200	0.743
Result						
Freq. Flood * Y2013						
Pashtun * Y2013						
Urban * Y2013						

Table A2: Additional Robustness Results

	Earthquake		Flood	
	(1)	(2)	(3)	(4)
	Share MMA	Share MMA	Share MMA	Share MMA
Affected * Post	-0.179***	-0.189***	0.0465***	0.0479***
	(0.0479)	(0.0483)	(0.0138)	(0.0142)
Observations	162	274	184	315
Weights	Voters	Voters 2002	Voters	Voters 2002
Freq. Disaster * Post	YES	YES	YES	YES
Pashtun * Post	YES	YES	YES	YES
Urban * Post	YES	YES	YES	YES

Table A3: Additional Robustness Results - Provincial Assembly

	(1)	(2)	(3)	(4)	(5)	(6)
	Share MMA	Share MMA	Funding Gap * Y2013	Share MMA	Funding Gap * Y2013	Share MMA
Affected * Y2013	0.0228**		0.404***		0.456***	
	(0.0115)		(0.0304)		(0.0317)	
Funding Gap * Y2013		0.0409***		0.0575**		0.0303**
		(0.0114)		(0.0251)		(0.0151)
Severe Aff * Y2013			0.0805***		0.0781***	
			(0.0183)		(0.0278)	
Dist. Capital * Y2013					0.0423***	
					(0.00478)	
Observations	970	970	970	970	970	970
Adjusted R^2	0.131	0.138	0.736	-0.734	0.813	-0.732
Result	OLS	OLS	FS	IV	FS	IV
F-stat	.	.	.	135.15	.	193.95
Pvalue Overind.	.	.	.	0.148	.	0.046

Table A4: Pre-Level Demographic Variables

Variable	**Earthquake 2005**			**Flood 2010**		
	Not Affected	Affected	Difference	Not Affected	Affected	Difference
Literacy Male	51.58	63.15	11.57	53.81	54.19	0.38
Literacy Female	28.89	37.91	9.02*	33.43	28.78	-4.65
Agricultural Emp.	35.01	27.21	-7.80	33.47	33.27	-0.20
Household Size	7.11	6.95	-0.16	6.91	7.21	0.30
Water Access	35.55	37.76	2.21	37.62	34.79	-2.83
Electricity Acess	72.74	75.90	3.17	72.16	74.36	2.20
<5 Years Immunized	66.79	73.74	6.94	67.03	69.21	2.18
Population Density	1825.44	1898.15	72.72	1681.24	1962.99	281.75

Risk of International terrorism and Political Participation: Evidence from September 11 attacks

Hasin Yousaf *

Abstract

In this paper, I study how the risk of international terrorism impacts political participation. I measure the risk of terrorism for each county in the U.S. based on three different measures: Department of Homeland Security funding, presence of critical infrastructure, and distance from the state capitol. Using the September 11 attacks as an exogenous shock to the salience of terrorism and employing a Difference-in-Differences strategy, I compare changes in political participation in areas with higher risk of terrorism to areas with lower risk. I find that areas with higher risk of terrorism increased political participation and campaign contribution in the subsequent elections. Using instrumental variable strategy based on the distance of each county from the state centroid yield similar results. The results highlight how unfortunate national shocks such as international terrorism can unite citizens and increase the political participation.

Keywords: Terrorism; Risk of Terrorism; Political Participation; Civic engagement

JEL Classification: D72, D74, F52, P16

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1 Introduction

International terrorism attacks in the developed countries have become more common than twenty years ago. In 2017 alone there were 731 terrorist attacks in OECD countries leading to death of more than 500 individuals and attacks in the major metropolitan cities around the world including London, Paris, and Barcelona. International terrorism triggers fear and creates insecurity among the citizen (Hirst et al., 2009). International terrorism can have a direct impact on the area targeted or may have an indirect effect on other areas within the same country due to increased salience of terrorism. Most of the literature focuses on the direct impact of terrorism ignoring its indirect impact.[1]

In this paper, I analyze the indirect impact of terrorism on political outcomes. Specifically, I investigate how areas with different level of ex-ante risk of terrorism react in wake of a terrorist attack. I do so in the context of the September 11 attacks which led to an increase in the salience of terrorism in the United States. According to the Gallup Poll, the percentage of Americans citing fear of a terrorist attack in near future on American soil increased from 24% in April 2000 to 58% right after the September 11 attacks. This number has not dropped even once below 40% since the attacks.[2] Similarly, the number of articles in the New York Times mentioning terrorism increased more than 13 folds to 455 in the decade following the September 11 attacks (Lexis Nexis).

I use three different measures of risk of international terrorism to capture different aspects of risk. First, I use the data from the distribution of Urban Area Security Initiative (UASI) funding by the Department of Homeland Security (DHS), which was started in 2003 in order to prepare and strengthen areas which were considered likely targets of

[1] Most of the literature uses multiple terrorist events to compare changes in economic and political outcomes in areas with attacks relative to other areas (Abadie and Gardeazabal, 2003; Abadie and Dermisi, 2008; Berrebi and Klor, 2008; Gould and Klor, 2010; Rehman and Vanin, 2017; Brodeur, forthcoming). The only paper that traces the impact of a single terrorist event is Montalvo (2011), who studies the impact of Madrid train attacks on the subsequent election. My paper studies how increased salience of terrorism may impact areas with different risk of terrorism differently, instead of assuming a uniform impact of attack across the country.

[2] http://news.gallup.com/poll/4909/terrorism-united-states.aspx

future terrorist attacks. This measure of risk reflects whether the government considers an area has a high risk of terrorism or not based on their intelligence assessment. Second, I use the stock of buildings that could be the potential target of international terrorism ("critical infrastructure) and the population to construct a risk of terrorism index for each area. This measure captures the objective level of terrorism risk associated with each area. Third, I use the distance from the state capitol as another measure of risk of terrorism. This measure captures the risk of terrorism associated with being in proximity to an important city.

I then use a difference-in-differences identification strategy to compare changes in counties with different level of risk of terrorism risk before and after the September 11 attacks. In all my empirical specifications I include county fixed effects and allow for differential trends in political outcomes across states and urban areas. Thus, essentially I am comparing changes in political outcomes in counties with different risk of terrorism located within the same state and urban-rural classification.

I find that the political participation increases significantly in areas with higher risk of terrorism relative to areas with lower risk post-September 11 attacks. Specifically, I find that voter turnout increased by 1.2 percentage points in areas that received the UASI funding compared to areas that did not receive it within the same state before and after the September 11 attacks. Similarly, counties located around 120 km further away from the state capitol (e.g. Dallas versus Houston in Texas) witnessed a 0.76 percentage points lower increase in voter turnout after the September 11 attacks. The counties with a higher risk of terrorism also witness an increase in the political campaign contributions due to an increase in the number of individuals who contribute. These increases in political participation are not driven by voters of a particular political party, as the vote share of major parties does not vary with different levels of risk. The finding can be broadly seen in line with evidence that violence leads individuals to become more pro-social and increase political and civic engagement (Bellows and Miguel, 2006, 2009; Blattman, 2009; Bateson, 2012; Voors et al., 2012).

I carry out several tests in order to show that these results reflect a causal effect of risk of terrorism on political participation. First, I execute an event study analysis by allowing for counties with different level of risk of terrorism to have a different trend in turnout in each election. I find that the counties with a higher level of risk had a similar trend in turnout prior to the September 11 attacks compared to counties with lower risk, and they only start to differ after the attacks. Second, in order to show that the results are driven by the risk of terrorism, I construct placebo risk indices based on buildings that are less likely to be the target of international terrorism. I find that the counties with critical infrastructure saw an increase in turnout, while there was no systematic change in counties with a stock of placebo buildings. Third, falsification exercise by randomly assigning the risk of terrorism to different counties yields insignificant estimates centered around zero showing that the main results are not driven by unobserved systematic differences across counties around the timing of the attacks.

In order to address any potential remaining endogeneity concerns, I use an instrumental variable identification strategy based on the idiosyncrasies of the shape and size of different states. In particular, I use the distance of the county from the geographic centroid of the state as an instrument for the distance of the county from the state capitol. This instrument is similar in methodology to Campante and Do (2014), who instrument the distance of the largest city in the state to the state capitol with the distance of the largest city from the geographic centroid of the state. The instrument is relevant because the state capital city tends to be located near the geographic center of the state due to equity concerns. Moreover, a state's geographic center depends on the geographic shape of the state and is an arbitrary location that is unlikely to have any direct role in of itself on the political and other outcomes. The results obtained using this strategy are very similar to the ones in the main analysis, which further strengthens the assertion that these results reflect a causal impact of risk of terrorism.

The paper contributes to the literature on the impact of terrorism on economic and political outcomes. Most of the literature has focused on the effects of terrorism, by exploiting repeated terrorist attacks, rather than tracing the differential impact of a single terrorist attack across the nation (Abadie and Gardeazabal, 2003; Abadie and Dermisi, 2008; Berrebi and Klor, 2008; Gould and Klor, 2010; Rehman and Vanin, 2017; Brodeur, forthcoming). Two important exceptions are Montalvo (2011) and Kaplan and Mukand (2011), who show that the Madrid bombings just before the 2004 Spanish elections lead to a loss in votes for the Socialist party and the voter registrations increased for Republican party post-September 11 attacks, respectively. My paper differs from theirs in two important ways. First, I analyze how areas with different level of risk of terrorism change political participation differentially in response to the terrorist incident instead of assuming a uniform impact of attack across the country. Second, my main variables of interest are measures of political participation and not the vote share of different political parties. I contribute to this literature by proposing and showing that terrorist attacks can have an indirect impact on political outcomes due to areas with different risk of terrorism responding differently in response to increased salience of terrorism.

The paper also contributes to the literature on determinants of political participation. The literature has shown that, among others, media (Gentzkow, 2006; Gentzkow et al., 2011; Falck et al., 2014), rain shocks (Madestam et al., 2013) and habit formation (Fujiwara et al., 2016) impact turnout. I contribute to this literature by showing that risk of terrorism in an episode of increased salience of terrorism due to high profile terrorist attack can also impact political participation.

Finally, the paper is also related to the literature on the endogenous formation of preferences. Several recent papers have demonstrated how different events and shock alter preferences. For instance, Voigtlander and Voth (2012) how the black death led to the formation of anti-Semitic attitudes, Giuliano and Spilimbergo (2013) show that growing up in recession impacts the preferences for redistribution, weather shocks impact political preferences for revolution (Chaney, 2013), and weather shocks variability impact social trust (Buggle and Durante, 2017). I contribute to this literature by demonstrating that September 11 attacks were such an event that shaped and have a long-lasting effect on the political preferences of individuals through its indirect impact on individuals around the nation.

The remaining paper is divided into five sections. In Section 2, I present the data sources and the descriptive statistics of the main variables employed in the paper. The empirical strategy and main results are outlined in Section 3 and Section 4 respectively. I provide further evidence in favor of the identification, carry out robustness checks and falsification tests in section 5. Finally, I conclude in section 6.

2 Data

In this section, I discuss the data sources and present the summary statistics of the main variables used in the paper.

2.1 Data Sources

The paper combines data from various sources. The county-level Presidential election data from 1984 to 2012 is collected from the Dave Leip's Atlas of U.S. Elections database (Leip, 2012). The data contains information on the total number of votes, and votes for each political party. The campaign contributions data from 1992 to 2012 is obtained from the Federal Election Commission (FEC). The data, available at fec.gov, contains all the individual contributions, geo-located, given to a political party, candidate or committee.[3]

The data on the risk of terrorism is collected from four main sources. First, I collect the data on the funding given by the Department of Homeland Security (DHS) directly from their website.[4] After the September 11 attacks, Congress gave DHS an annual budget to allocate funds across states and local areas to prevent future terrorist attacks. The

[3]In order to reduce noise, I restrict attention to zip codes with more than 20 or more contributions (Gentzkow and Shapiro, 2010).

[4]The data is available for download at dhs.gov

DHS gave funding to different areas under three different programs of which Urban Area Security Initiative (UASI) was the biggest program and the only program which gave funding to the local areas instead of states.[5] In 2006 alone, DHS gave around $1 billion in funding to local areas as part of the UASI funding compared to a total of $1.6 billion. The DHS allocated the UASI funding solely based on the population, presence of critical infrastructure, and the likelihood of terrorist attack (Reese, 2006), and not distributed along political lines (Prante and Bohara, 2008). Since there is practically no difference in the amount of UASI funding allocated across local areas, I use whether a county received UASI funding in 2006 as a measure of risk of terrorism.

Second, in order to construct a risk of terrorism index, I extract data on the presence of critical infrastructure for each county using the Geographic Names Information System (GNIS) from the U.S. Geological Survey (2015).[6] THE GNIS contains information on major man-made and natural items across the country. It was first collected in the 1980s and is updated every decade for new buildings. The data contains more than 2.2 million buildings distributed across 65 different categories. From the data, I obtain the number of government buildings for each county as a component for the measure of critical infrastructure.[7] In addition, I also record information on the number of other buildings (airports, bridges, churches, military bases and post offices) in order to construct placebo measures of risk of terrorism.

Third, I obtain the stock of tall buildings from the Global Tall Building Database provided by Council of Tall Buildings and Urban Habitat (CTBUH) (2015). The database

[5]The other two programs were State Homeland Security Grant Program (SHSGP) and the Law Enforcement Terrorism Prevention Program (LETPP). Both were allocated at the state level based on a base amount of 0.75% of the total appropriations and the remaining funds distributed according to the relative state population.

[6]The data is available for download at services.nationalmap.gov

[7]The government buildings are not labeled as a separate category. I identify the government buildings using their name in two steps. First, I eliminate all the physical items and clearly labeled man-made items such as church, school, etc. Second, among the remaining buildings, I use a set of words to identify whether it is a government building. The set of words include: "Federal", "State", "Capitol", "County", "Justice", and "Court". In practice, however, many buildings, for instance, correctional facilities, police stations, and trooper forces, usually contain one of these words in their name. I exhaustively enumerate all these cases and ensure that these buildings are excluded from the stock of government buildings.

contains an exhaustive list of worldwide buildings taller than 150 meters along with the location (city), building date and the purpose of use.[8] From the database, I obtain the number of tall buildings for each county.[9] I then use the population, stock of government, and tall buildings to construct a risk of terrorism index. The methodology of constructing the index is similar to Willis et al. (2006). However, my approach differs slightly as I incorporate population as an additional component of the index and construct the index for the entire U.S. rather than for some specific areas.

Fourth, in order to construct a measure of risk of terrorism based on the distance of the county from important locations, I obtain the centroid for each county and state from the U.S. Census Bureau (2011). I then measure the distance of each county from the state capital city, largest city, city that received UASI funding, and state geographic centroid.[10]

Finally, the data on control variables is collected from the U.S. Census Bureau (2011). The Census contains detailed information about the demographic, social and economic indicators. The variables collected include the demographics such as the proportion of males, whites, blacks, Hispanics, the educational attainment (the individuals with high school and a college degree), unemployment rate, median income and indicator for whether it is an urban area.

2.2 Descriptive Statistics

Table 1 shows the summary statistics for the main variables employed in the paper. An average county has a voter turnout of 56.5% with a standard deviation of 10.7 percentage points. We see that 36% of the counties are urbanized. The measures of risk of terrorism

[8]The CTBUH defines any building over 150 m (492 ft.) as a “tall” building, while it defines any building with more than 300 m (984 ft.) height as a “super-tall” building.

[9]There are few cities which are located in multiple counties e.g. Dallas, Houston, and New York. For these cities, I manually verify in which county the building is located by searching for the exact address of the building using its name.

[10]In order to make sure that the distances are calculated correctly, I cross-validate the distance of each county from the state capitol using the data on distances obtained by Campante and Do (2014).

show that 12.5% of the counties (387) received the UASI funding. In addition, the average (median) county has 7 (4) critical infrastructure buildings. The middle 50% of the counties (25th to 75th percentile) have between 2 to 7 critical infrastructure buildings. Los Angeles County has the highest number (376) of these buildings. Moreover, we see that the average county has a risk of terrorism index of 0.261 with a standard deviation of 0.149. The middle 50% of the counties have a terrorism risk index between 0.134 and 0.384. Los Angeles County and New York County have the highest value of risk of terrorism index. Finally, we see that the average county is located around 196 km and 188 km away from the state capital city and state geographic centroid respectively. The middle 50% of the counties are located within 100 to 260 km away and 110 to 240 km away from the state capitol and state geographic centroid respectively.

We can see the geographic distribution of different measures of risk of terrorism in Figure 1. Specifically, Figure 1a shows the location of counties that received UASI funding. We see that most major metropolitan areas received the UASI funding. Figure 1b shows the distribution of risk of terrorism index across the country. We see that the pattern is very similar to the one in Figure 1a. The areas that received UASI funding have systematically higher risk of terrorism according to the index. This pattern becomes clearer if we focus on the top 10 percentile of the counties (Figure 1c), in which we observe a major overlap between the areas that received the UASI funding and areas in the top 10 percentile according to the risk of terrorism index.

3 Empirical Strategy

In this section, I describe the two empirical strategies employed in the paper. I first illustrate in detail the Difference-in-Differences identification strategy and then discuss the instrumental variables strategy.

3.1 Difference-in-Difference Strategy

In this paper, I want to assess how the risk of terrorism impacts political outcomes. In order to do that, I exploit the September 11 attacks as a shock that makes terrorism more salient in conjunction with the pre-existing differences in the level of risk of terrorism across counties. Thus, I employ the following Difference-in-Difference strategy:

$$\Delta turnout_{cst} = \alpha_t + \alpha_s + \sum_{t=1}^{T} \rho_t Urban_{cs} + \beta\Delta(Risk_c * Post_t) + \Delta X'_{cst}\gamma + \Delta u_{cst}, \quad (1)$$

where $\Delta turnout_{cst}$ is the change in turnout in Presidential elections in county c located within state s in the election period t with respect to $t-1$. $Post_t$ is dummy equal to 1 for elections after 2001 and 0 otherwise. $Risk_c$ is one of the three measures of the risk of terrorism. Specifically, $Risk_c$ is (i) a binary indicator whether the county received the UASI funding; (ii) the risk of terrorism index based on population and critical infrastructure in the county c. (iii) the logarithmic distance of the county c from the state capitol. X_{cst} are the set of controls variables discussed in the Section 2. $Urban_{cs}$ is a dummy equal to one if the county belongs to an MSA and zero otherwise. $\rho_t Urban_{cs}$ allows for a differential trend in turnout across areas within and outside an MSA.

The county fixed effects are already factored out because the specification is written in first-differences. Thus, all county specific factors that do not vary over time such as the geography, set of institutions and laws, among other factors are already accounted for by taking the first-difference. In addition, I include election fixed effects (α_t) and state fixed effects (α_s) in all specifications. α_t controls for the election specific factors that are common to all counties. The term captures, for instance, whether the Presidential candidate is running for re-election. Finally, α_s are the state-specific fixed effects, which capture the variation in the dynamics of political variables across different states and also absorb differences in measures of risk of terrorism across states. Factors such as whether a state is a swing state are absorbed by these fixed effects.

β estimates the impact of risk of terrorism on turnout. Specifically, β measures the change in voter turnout in counties with a higher risk of terrorism relative to counties with a lower risk of terrorism located inside an urban area within the same state before and after the September 11 attacks.

The identification assumption is that in the absence of the September 11 attacks, the counties with a higher risk of terrorism and counties with a lower risk of terrorism would have evolved in the same way. This assumption is directly untestable. However, I provide evidence in favor of this assumption by carrying out an event study and testing for the differences in voter turnout in areas with high and low risk of terrorism for each election prior to and post-September 11 attacks. If the areas with high and low risk of terrorism had a similar trend in turnout several periods prior to the September 11 attacks, it is reasonable to assume that in absence of any shock to these areas, they would have maintained similar trends later as well. Thus, I estimate the following specification:

$$\Delta turnout_{cst} = \alpha_t + \alpha_s + \sum_{t=1}^{T} \rho_t Urban_{cs} + \sum_{t=-n}^{m} \beta_t (Risk_c * \mathbb{1}(t = j)) + \Delta X'_{cst}\gamma + \Delta u_{cst}, \quad (2)$$

where β_t measures the difference in trend in the counties with high and low risk of terrorism located within the same urban characterization and the same state for each election period.

Figure 2 illustrates the results obtained from the event study using different measures of risk of terrorism. We clearly see that the counties with different level of risk of terrorism had a similar trend in the turnout for several election periods prior to the September 11 attacks. The estimates are both economically small in magnitude (more than three times smaller than the effect) and statistically insignificant. These results provide strong evidence in favor of the identification assumption by demonstrating that the areas with different level of risk of terrorism had similar trend in turnout prior to the attacks, and that they only diverge after the attacks.

We also see that the impact of risk of terrorism on turnout is similar across elections after the September 11 attacks. The impact is slightly stronger for the 2008 elections, although it statistically similar to the effect on 2004 elections. We see that the impact does not disappear even 10 years after the attacks. These results suggest that the September 11 attacks caused a long-term difference in turnout among areas with different level of terrorism risk.[11]

3.2 Instrumental Variable Strategy

One remaining concern could be that the results obtained from the Difference-in-Differences identification strategy discussed above may reflect some systematic differences among counties with different level of risk of terrorism rather than the risk of terrorism itself. For instance, say that the counties with a higher risk of terrorism are more likely to have highly educated individuals and say they react differently to the salience of risk of terrorism relative to less educated individuals. Then, my estimates may reflect the differential effect of terrorism on high and low educated individuals. This concern would not invalidate the estimates but would imply that part of the estimates captures the effect that is not due to the risk of terrorism but is due to pre-existing differences among high and low-risk counties.

In order to address this concern, I use an instrumental variable strategy. Specifically, I instrument the distance of a county from the state capitol with its distance from the geographic centroid of the state. This instrument is similar in methodology to the one employed by Campante and Do (2014), who study the impact of accountability in explaining corruption across states. However, they only rely on cross-sectional variation across states, while I exploit the cross-sectional variation in conjunction with the September 11 attacks to explain changes in voter turnout across areas. Thus, one advantage that my estimation strategy has is that I can control for county fixed effects and rely on the changes in outcomes over time.

The instrument is likely to be valid because the geographic centroid of the state is an arbitrary point that simply depends on the shape and size of the state, and should not have

[11] We get a similar picture if we combine event study analysis with IV estimation. The results are shown in Figure A1.

any direct effect in of itself on the political, demographic, social and economic outcomes. In order to provide evidence in favor of the validity of the instrument, I compare how differences in both the levels and trends of the political and other variables vary with the distance from the state centroid prior to the September 11 attacks.

In order to provide evidence in favor of the validity of the instrument, I compare how differences in both the levels and trends of the political and other variables vary with the distance from the state centroid prior to the September 11 attacks.[12] Table 2 shows the results. We can see that counties located closer to the state centroid are very similar, both in levels and trends, on political (turnout and Republican vote share), demographic, and economic variables compared to counties located further away. Thus, the counties that are located close to the state centroid appear to be ex-ante very similar to the counties that are located further away, which adds confidence to the validity of the instrument. Interestingly, the same can not be said about the counties located closer to and further away from the state capitol. In Table A1 we see that the counties located close to state capital city tend to have a lower Republican vote share, a higher proportion of blacks, college educated, high earning and middle age individuals. Apart from these differences in levels of variables, which are absorbed by the inclusion of county fixed effects, we see little differences in the trend in these variables.

Moreover, the distance from the state geographic centroid should be correlated with the distance from the state capitol in order for it to be a good instrument. This correlation is present because states tend to place their capital city close to the geographic center due to equity of citizen accessibility concerns (Campante and Do, 2014). The relevance of the instrument can also be quickly ballparked from the Figure 3, which maps the location of state capitols, the largest city and geographic centroid for each state. We can clearly

[12] In particular, I estimate the following specification:

$$X_{cs} = \alpha_s + \rho(Log.DistCentroid_{cs}) + \epsilon_{cs}, \tag{3}$$

where X_{cs} represents the level (trend) of variable X in county c located in state s in the year 2000 (between 1996 to 2000). ρ is the main variable of interest which measures the differences in levels and trends in variable X prior to the September 11 attacks.

observe that the state capital city (also largest city) tends to be close to the geographic centroid of the state.

4 Main Results

In this section, I first discuss the results obtained using the Difference-in-Difference strategy and then discuss the results obtained using the instrumental variables strategy.

4.1 OLS Results

The main results on how the risk of terrorism impacts the voter turnout after September 11 attacks are shown in Table 3. In all the estimations the standard errors are clustered at the state level to allow for arbitrary correlation in the unobservable factors affecting voter turnout among counties within the same state. In Column 1, I only include year fixed effects. In Columns 2 and 3, I successively add state fixed effects and allow for a differential trend in turnout among urban and rural counties respectively. Finally, I include the control variables in Column 4 to account for demographic, educational, and economic differences among counties.

In Columns 1 to 4, I measure the risk of terrorism using whether the county received the UASI funding from the DHS. The results remain stable and both highly economically and statistically significant despite the inclusion of a substantial number of fixed effects. We see that the counties that received the UASI funding increased voter turnout by 1.20 percentage points more relative to counties located in the same state that did not receive the UASI funding after the September 11 attacks.

In Columns 5 to 8, I measure the risk of terrorism using the index constructed based on population and stock of critical infrastructure buildings. We see a similar pattern to the one observed in Columns 1 to 4. We notice that the counties with one standard deviation higher risk of terrorism experienced an increase in voter turnout of 0.94 ($= 0.149 * 0.063$)

percentage points more compared to the counties with a standard deviation lower risk of terrorism located in an urban area within the same state after the September 11 attacks. To put it differently, the voter turnout increased by 1.57 ($= (0.384 - .135) * 0.063$) percentage points more in counties at the 75th percentile according to the risk of terrorism index relative to the counties at the 25th percentile according to the risk of terrorism index.

I measure the risk of terrorism using the logarithmic distance of the county from the state capitol in Columns 9 to 12. The results paint a similar picture to the one obtained using other two measures of risk. The results imply that doubling the distance of the county from the state capitol leads to a decrease of 1.2 percentage points more in the voter turnout after the September 11. In other words, counties that are located roughly 120 km further away from the state capitol (e.g. Dallas relative to Houston in Texas) witnessed a 0.76 percentage points lower increase in voter turnout after the September 11 attacks relative to a county located at the average distance from the state capitol (196 km).

4.2 Instrumental Variable Results

In this section, I discuss the results obtained using the distance of a county from the state centroid as an instrument for its distance from the state capitol. Table 4 shows the results using the instrumental variable strategy. Panel A shows the estimates from the first stage. We see a clear positive and statistically strong relationship between the distance of a county from the geographic centroid of the state and its distance from the state capitol. Counties that are located 1% further away from the states geographic centroid also tend to be located 0.55% further away from the state capitol. The instrument is quite strong: the Kleibergen-Paap F Statistic for the weak instrument test is 61.38, which is more than four times larger than the Stock-Yogo weak identification critical values (Stock and Yogo, 2005).

Panel B of Table 4 shows the results from the second stage. We see a negative, stable and statistically significant effect of the distance of a county from the state capitol on the voter turnout. The results imply that doubling the distance of the county from the state capitol leads to a decrease of 1.1 percentage points more in the voter turnout after the September 11. In other words, counties that are located roughly 120 km further away from the state capitol (e.g. Dallas relative to Houston in Texas) witnessed a 0.68 percentage points lower increase in voter turnout relative to a county located at the average distance from the state capitol (196 km) after the September 11 attacks. The results are remarkably similar and statistically indistinguishable to the ones obtained using the Difference-in-Differences strategy.

5 Robustness, Heterogeneity and Other Results

In this section, I present further evidence in favor of the identification strategy and mechanism. In addition, I present results using alternate definitions and present how the risk of terrorism impacts other outcomes.

5.1 Placebo Estimates

In this section, I provide evidence that further strengthens the claim that the previous results are driven by the risk of terrorism and not due to some other systematic differences. Specifically, I carry out two difference placebo exercises. First, if the effect is really through the risk of terrorism, we should see an increase in turnout only in the areas with the critical infrastructure and not in areas with large or other infrastructure in general. Thus, I include a measure of other buildings in my estimation to see how turnout changes post-September 11 attacks in these areas.

Table 5 shows the results. Column 1 illustrates the effect of each component of the risk of terrorism index separately. We see that both critical infrastructure buildings and population contribute towards an increase in voter turnout. In particular, we see that turnout increased by 1.58 percentage points (0.026*(.159/.263)) in counties with one standard deviation higher number of buildings relative to the county with an average number of buildings. On the other hand, the turnout increased by 1 percentage points (0.074*(1.39/10.20)) in counties with one standard deviation higher population relative to the county with average population.

Columns 2 to 6 of Table 5 add different buildings to the specification. We see that the coefficient on the critical infrastructure buildings and the population remains stable and statistically significant throughout. In addition, the presence of other buildings such as airports (Column 2), bridges (Column 3), churches (Column 4), military bases (Column 5) and post offices (Column 6) in a county does not explain the change in turnout in these counties post-September 11 attacks. These results strengthen that the increase in turnout in these counties is really due to having a particular type of infrastructure that is critical with respect to terrorism.

Second, in order to show that the results are not driven by systematic differences among counties with different risk of terrorism, I carry out a falsification exercise by randomly assigning the risk of terrorism to each county. Specifically, I randomly assign the value of risk of terrorism of a county to another. The appealing point of this method is that it keeps the mean, standard deviation and the distribution of each of the three measures of risk of terrorism same. I then run my main specification with this fake risk of terrorism measure and carry out this exercise $1,000$ times to obtain a distribution of the β estimates.

Figure 4 shows the distribution of β from the falsification exercise overlaid with the actual estimates. It is reassuring to see that the distribution of the estimates obtained from the falsification exercise is centered around 0, with a mean of 0.000064 and a standard deviation of 0.00248. This provides evidence that the results obtained in the main analysis are likely not driven by the systematic differences among counties around the September 11 attacks. The minimum and maximum estimates obtained from the falsification exercise are -0.0091 and 0.0078 respectively. It is further convincing to see that the maximum estimates from the falsification exercise are much lower (more than half) than the original estimates. Thus, we can conclude with at more than 1% significance level that the main results are not due to some systematic differences among counties around the September 11 attacks. I obtain similar conclusion by carrying out falsification exercise based on other measures of risk of terrorism. The results for them are shown in Figure A2.

5.2 Other Results

In this section, I discuss the impact of risk of terrorism on Republican vote share and campaign contributions.

5.2.1 Republican Vote Share

In order to see whether the increase in turnout is driven by voters of a particular political party, I study how the Republican vote share changes in counties with different risk of terrorism. Table 6 shows the results. We see that the Republican vote share did not change differentially in areas with a higher risk of terrorism relative to a lower risk of terrorism post-September 11 attacks. The results are similar across the different measures of the risk. These results imply that the increase in voter turnout is not driven solely by an increase in the voters of a particular political party.

5.2.2 Campaign Contributions

In order to see whether the risk of terrorism impacted other measures of political participation as well, I study how the campaign contributions change in counties with a higher risk of terrorism relative to those with a lower risk of terrorism. Table 7 shows the results. Columns 1 to 4 study the impact on the total $ amount of campaign contributions. We notice that the political campaign contributions increased in counties with a higher risk of terrorism relative to a lower risk of terrorism post-September 11 attacks. More concretely, we see that the counties that received UASI funding saw an increase of 9.5% (equivalent to around $ 4,600 per year for a county with the median amount of contributions) post-September 11 attacks relative to counties that did not receive the funding. The results obtained from using the risk of terrorism index, and the distance from the state capitol paint a similar picture.

In Columns 5 to 8, I study the impact on the total number of contributions. We see that the total number of contributions increased in counties with a higher risk of terrorism. Specifically, we see that the counties which received the UASI funding saw an increase of 12.9% (equivalent to around 31 more contributors per year) in the number of individuals giving campaign contributions after the September 11 attacks relative to the counties that did not receive the UASI funding. The results obtained from using the risk of terrorism index and the distance from the state capitol paint a similar picture.

These results together show that the total $ amount of campaign contributions increased in counties with a higher risk of terrorism relative to a lower risk of terrorism after September 11 attacks. Moreover, we find that this change is not driven by existing donors contributing more to the political parties and candidates, but rather by individuals who did not contribute previously.

5.3 Robustness

In this section, I show that the results are robust to alternate specifications and measuring variables. First, we may be concerned that the results are driven by some particular states. In particular, we may be concerned that the results are driven by New York or the states close to New York. In order to address this concern, I estimate the main equation by dropping each state at a time and plot the estimates in Figure A3. We see that the estimates remain very similar if we drop each state at a time, indicating that the results are not driven by a particular state. In fact, no state has a large impact on the estimates as the coefficient remains between 0.010 and 0.014 during this exercise.

Second, I show that the results are robust to different ways of defining distance. In this section, I consider several different distances such as the distance from the largest city in the state, distance from the closest city that received UASI funding, distance from the closest city with more than 0.5 million population and distance from the closest city with more than 0.1 million population. The results are shown in Table A2. We see that the results remain robust across different ways of defining the risk of terrorism. We see that the OLS estimates vary between 0.0026 and 0.0067, which are slightly smaller than the estimates obtained using the distance from the state capitol. On the other hand, the estimates obtained from IV range between 0.0163 and 0.324, which are slightly larger than the results obtained in the main analysis.

Third, I relax the assumption that the effect of risk of terrorism, as measured by distance from the state capitol, is linear. In particular, I allow the impact of risk of terrorism to be non-linear by estimating a fourth order polynomial of distance. The results are shown in Figure 5. We see that the counties closest to the state capitol saw the greatest increase in voter turnout. The impact decreases steadily until the distance of the county is around 200 km from the state capitol, before becoming statistically insignificant and similar to zero.

6 Conclusion

In this paper I provide evidence that the risk of terrorism threat may increase political participation. I use the September 11 attacks as a source of exogenous variation in salience of terrorism combined with pre-existing level of risk of different areas to measure the response of different areas within United States to the risk of terrorism.

Measuring risk of terrorism index using UASI funding, critical infrastructure and distance from the state capitol, I find that the areas with higher risk of terrorism witnessed an increase in political participation relative to areas with lower risk of terrorism. The estimates imply that counties that received UASI funding (had one more critical infrastructure building) increased voter turnout by 1.20 (0.65) percentage points more relative to counties located in the same state that did not receive the UASI funding (had average number of buildings) after the September 11 attacks. The results reflect a permanent increase in the voter turnout, as the effect is similar and does not disappear for subsequent elections (2008 and 2012). The areas with higher risk of terrorism also saw a higher increase in campaign contributions primarily due to new donors. Furthermore, these results do not reflect a partisan increase in turnout, as the vote share of the Republican party remains unchanged in these areas.

The paper contributes to our understanding of determinants of political participation. In particular, the paper hypothesizes and tests that trigger events such as trans-national terrorism may bring people closer together and may result in an increase in political participation among citizens.

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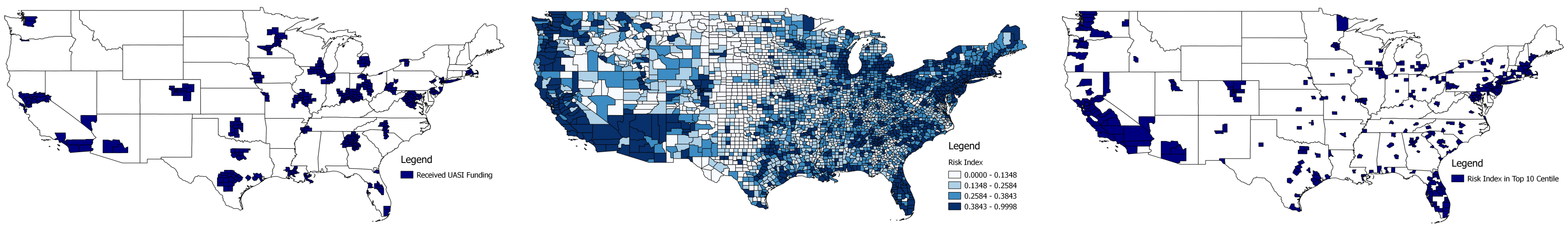
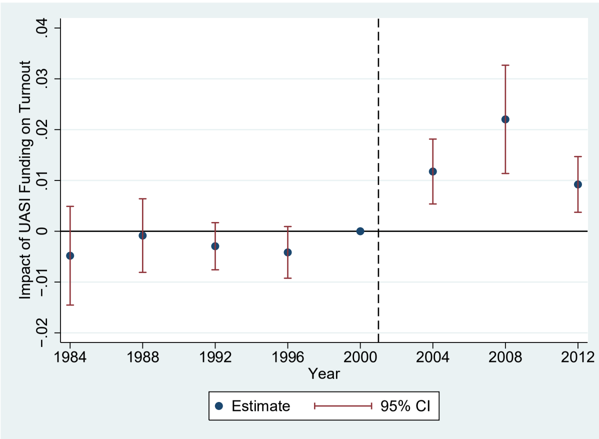
Figure 1: Distribution of UASI Funding and Risk of Terrorism Index



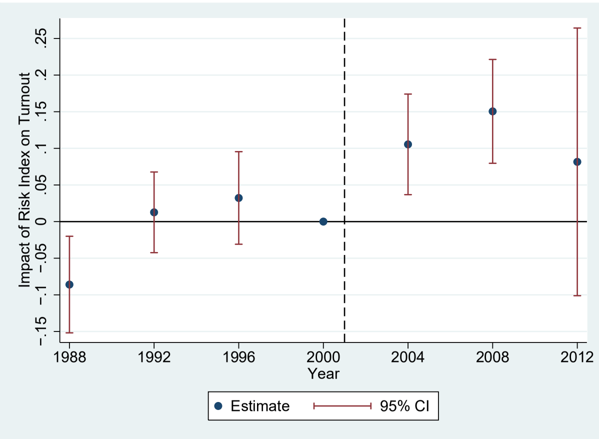
(a) Counties that received UASI Funding (b) Risk of Terrorism Index for Counties (c) Risk of Terrorism Index in top 10 percentile

Notes: The Figure shows the geographic distribution of different measures of risk. Panel (a) shows the location of counties that received UASI funding. Panel (b) shows the distribution of risk of terrorism index by quartile. Panel (c) shows the location of counties that were in the top 10 percentile according to the risk of terrorism index.

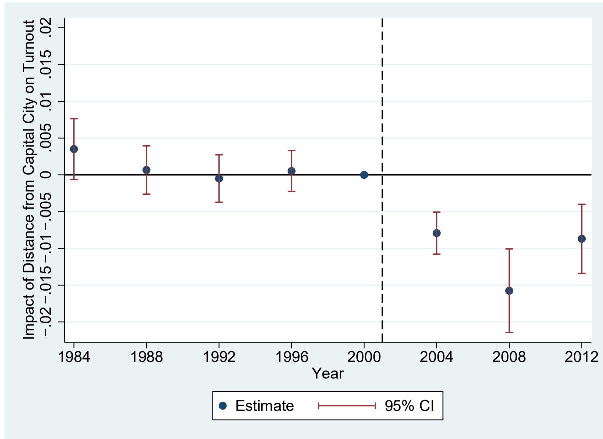
Figure 2: Event Study for Presidential Elections



(a) UASI Funding



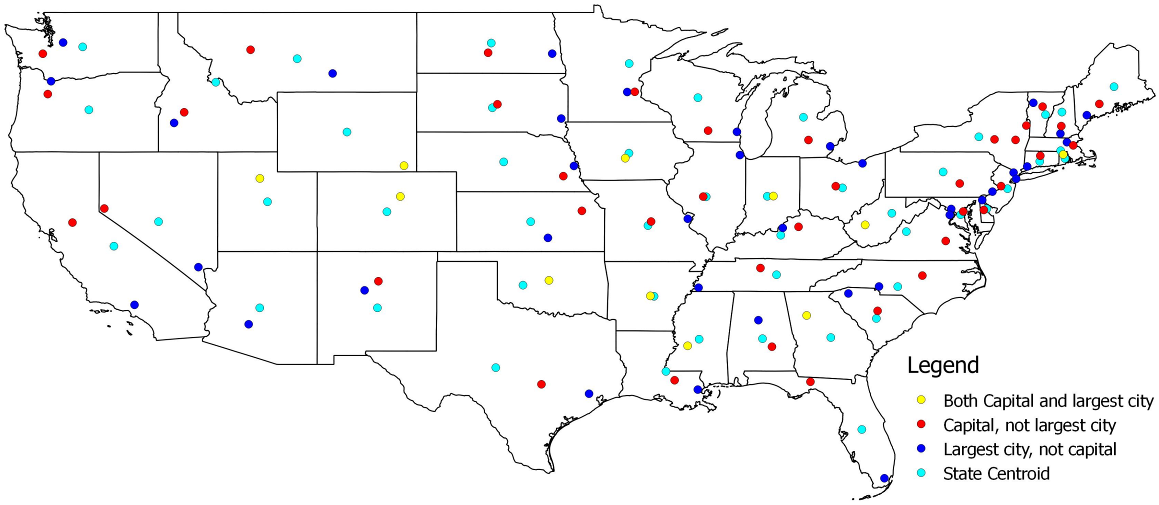
(b) Risk of Terrorism Index



(c) Distance from State Capitol

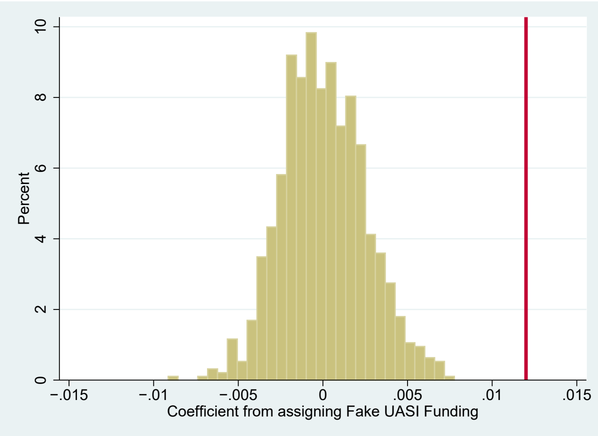
Notes: The figure plots the estimated coefficients and 95% confidence intervals obtained from event study exercise given by Equation 2 using three measures of risk of terrorism. Panel (a) shows the estimates using UASI funding, Panel (b) shows the estimates using the risk of terrorism index and Panel (c) uses the distance from the state capitol as a measure of risk of terrorism.

Figure 3: Location of State Capitol, Largest City and Geographic Centroids

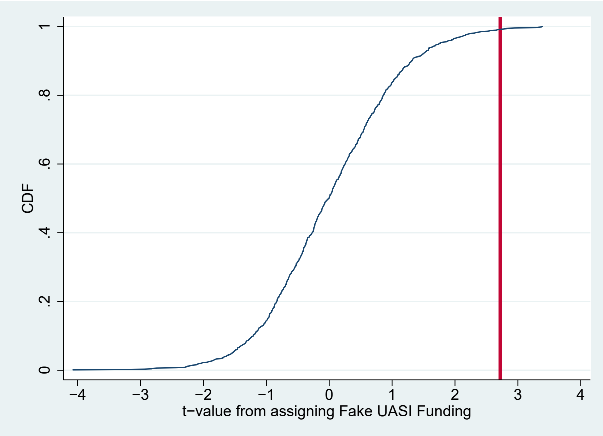


Notes: The figure maps the location of the state capitol, state's largest city and the geographic centroid of the state. The location of state capitol which is not the largest city in the state appear in red; the location of state capitol that is also the largest city appear in yellow; and the largest city that is not state capitol appear in blue. The geographic centroid of each state is labeled in light blue.

Figure 4: Falsification Exercise using UASI



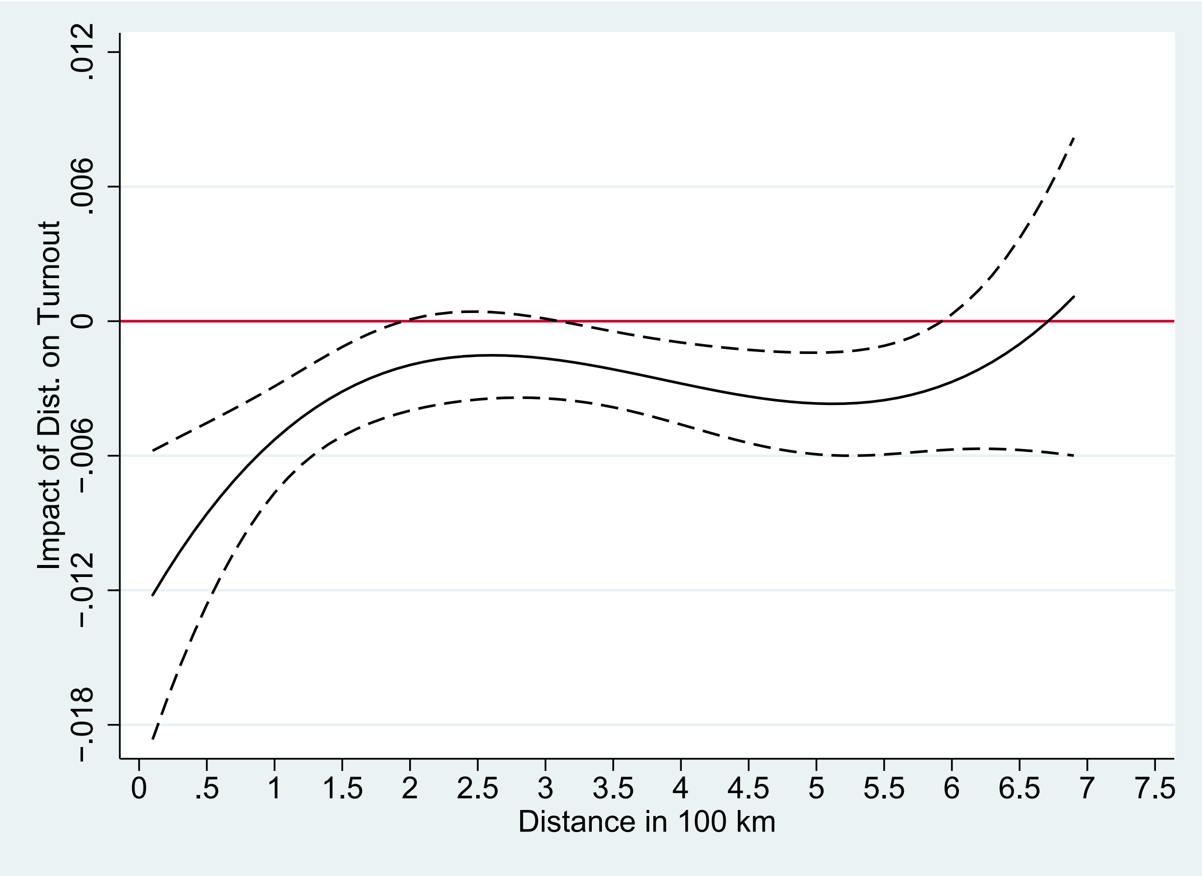
(a) Distribution of Point Estimates



(b) Distribution of t-values

Notes: The figure plots the results from the falsification exercise carried out by randomly assigning UASI funding to different counties while keeping the proportion of counties that receive UASI funding same. Panel (a) shows the distribution of estimates of β obtained from this falsification exercise, while Panel (b) shows the Empirical Cumulative Distribution Function (CDF) of the t-values from the falsification exercise.

Figure 5: Non-Linear Impact of Distance on Turnout



Notes: The figure plots the marginal impact of distance of a county from the state capitol on the voter turnout. The results are estimated using fourth order polynomial in the distance. The estimates are represented by the solid black lines, while the dashed black lines represent the bounds for the 95% confidence interval.

Table 1: Descriptive Statistics

	Mean	Median	SD	Min	Max
Panel A: Political Variables					
Turnout	0.565	0.565	0.107	0.002	0.997
Republican Vote Share	0.542	0.548	0.143	0.000	0.959
$ Campaign Contributions	11.179	10.801	1.607	8.062	18.910
# Campaign Contributions	3.244	3.045	1.955	0.000	11.428
Panel B: Risk of Terrorism Variables					
UASI Funding	0.125	0.000	0.330	0.000	1.000
lnpop	10.202	10.086	1.395	3.912	16.102
Critical Infrastructure Buildings	6.928	4.000	14.635	0.000	376.000
Risk Index	0.261	0.258	0.149	0.001	1.000
Log. Distance from Large City	5.038	5.126	0.776	-0.154	6.715
Log Dist. from State Centroid	5.059	5.142	0.644	0.541	6.475
Panel C: Socio-economic Variables					
Male	0.497	0.493	0.024	0.319	0.925
White	0.861	0.926	0.158	0.033	1.000
Black	0.088	0.018	0.145	0.000	0.865
Hispanic	0.061	0.017	0.122	0.000	1.000
High School educated	0.348	0.348	0.064	0.091	0.707
College educated	0.159	0.140	0.076	0.000	0.683
Married	0.767	0.779	0.059	0.351	0.966
Urban	0.361	0.000	0.480	0.000	1.000
Log. Median Income	10.318	10.341	0.378	8.960	11.719
Unemployment	0.066	0.062	0.030	0.000	0.417

Notes: The table shows the summary statistics of the main variables used in the paper. All the data is at the county level.

Table 2: Differences w.r.t distance from the state centroid

	Levels		Trends	
	Difference	SE	Difference	SE
Turnout	-0.006	(0.004)	0.001	(0.002)
Republican Vote Share	-0.011	(0.007)	0.000	(0.003)
Male	-0.001	(0.001)	0.000	(0.000)
White	-0.001	(0.010)	-0.001	(0.001)
Black	-0.002	(0.010)	0.000	(0.000)
Hispanic	0.001	(0.003)	0.000	(0.001)
High School educated	-0.004	(0.003)	0.000	(0.000)
College educated	-0.005*	(0.003)	0.000	(0.000)
Married	0.002	(0.003)	0.000	(0.000)
Urban	0.004	(0.016)	0.000	(0.000)
Log. Median Income	-0.014	(0.008)	0.000	(0.000)
Unemployment	0.002	(0.002)	-0.002	(0.002)
Pop 18-65 years	-0.001	(0.002)	-0.002	(0.002)
Pop $>$ 65 years	0.000	(0.003)	-0.001	(0.000)

Notes: The table illustrates how the levels and trends of main variables vary with respect to the distance from the state's geographic centroid before the September 11 attacks. The table reports the estimates and standard error of ρ from Equation 2. The standard errors are clustered at the state level. * indicates significance at 10% significance level, ** indicates significance at 5% significance level, while *** indicates significance at 1% significance level.

Table 3: Impact of Risk of Terrorism on Turnout (OLS)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Δ turnout	Δ turnout	Δ turnout	Δ turnout	Δ turnout	Δ turnout	Δ turnout	Δ turnout	Δ turnout	Δ turnout	Δ turnout	Δ turnout
Δ UASI Fund * Post,	0.022***	0.021***	0.017***	0.012***								
	(0.005)	(0.005)	(0.005)	(0.005)								
Δ Risk Index * Post,					0.101***	0.089***	0.076***	0.063***				
					(0.024)	(0.021)	(0.021)	(0.021)				
Δ Log Dist. from Large City * Post,									-0.012***	-0.014***	-0.013***	-0.012***
									(0.003)	(0.002)	(0.003)	(0.003)
Observations	21,736	21,736	21,736	21,736	21,736	21,736	21,736	21,736	21,729	21,729	21,729	21,729
R-squared	0.411	0.451	0.466	0.496	0.412	0.452	0.467	0.496	0.412	0.453	0.468	0.498
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Urban-Year FE	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Controls	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes

Notes: The table illustrates how the risk of terrorism impacts voter turnout using OLS estimation described in Equation 1. Columns 1 to 4 use UASI funding as a measure of the risk of terrorism. Columns 5 to 8 use the terrorism risk index, while Columns 9 to 12 use distance from the state capitol as a measure or risk of terrorism. Columns 1, 5 and 9 only include year fixed effects in the estimation. Columns 2, 6 and 10 include the year and state fixed effects in the estimation. Columns 3, 7 and 11 include the year, state and urban-year fixed effects in the estimation, while Columns 4, 8 and 12 include the year, state and urban-year fixed effects and county level controls. The control variables include: the proportion of the population that is male, white, black, Hispanic, has high school degree, is college educated and is married; log. median income and unemployment rate. In all the estimations the standard errors are clustered at the state level. * indicates significance at 10% significance level, ** indicates significance at 5% significance level, while *** indicates significance at 1% significance level.

Table 4: Impact of Risk of Terrorism on Turnout (IV)

	(1)	(2)	(3)	(4)
First Stage	Δ Log Distance from State Capitol * Post			
Δ Log Distance from State Centroid * Post	0.5498***	0.5501***	0.5492***	0.5456***
	(0.070)	(0.070)	(0.070)	(0.070)
Second Stage	Δ Turnout			
Δ Log Distance from State Capitol * Post	-0.0106**	-0.0108**	-0.0107**	-0.0112**
	(0.0045)	(0.0044)	(0.0044)	(0.0050)
Observations	21,729	21,729	21,729	21,729
R-squared	0.412	0.4526	0.4678	0.4981
Kleibergen-Paap F Statistic	62.28	61.63	61.57	61.38
Year FE	Yes	Yes	Yes	Yes
State FE	No	Yes	Yes	Yes
Urban-Year FE	No	No	Yes	Yes
Controls	No	No	No	Yes

Notes: The table illustrates how the risk of terrorism impacts voter turnout using IV estimation. Panel A shows the estimates of the coefficient on the instrument (log. distance from the state's geographic centroid) from the first stage. Panel B shows the estimates of the coefficient on the instrumented variable (log. distance from the state capitol) from the second stage. Column 1 only includes year fixed effects in the estimation; Column 2 includes the year and state fixed effects; Column 3 includes the year, state and urban-year fixed effects, while Column 4 includes the year, state and urban-year fixed effects and county level controls. The control variables include: the proportion of the population that is male, white, black, Hispanic, has high school degree, is college educated and is married; log. median income and unemployment rate. In all the estimations the standard errors are clustered at the state level. * indicates significance at 10% significance level, ** indicates significance at 5% significance level, while *** indicates significance at 1% significance level.

Table 5: Placebo Estimates using non-critical infrastructure

VARIABLES	(1) Δ Turnout	(2) Δ Turnout	(3) Δ Turnout	(4) Δ Turnout	(5) Δ Turnout	(6) Δ Turnout
Δ Log. Buildings * Post,	0.026**	0.029**	0.027**	0.027**	0.028**	0.028**
	(0.012)	(0.012)	(0.012)	(0.012)	(0.013)	(0.013)
Δ Log. Pop. * Post,	0.074*	0.120***	0.092**	0.076*	0.098**	0.098**
	(0.037)	(0.042)	(0.040)	(0.039)	(0.040)	(0.038)
Δ Log. Placebo Buildings * Post,		-0.004	0.002	0.003	-0.039	-0.000
		(0.003)	(0.002)	(0.002)	(0.024)	(0.003)
Observations	21,736	21,729	21,729	21,729	21,729	21,729
R-squared	0.451	0.458	0.458	0.458	0.458	0.458
Placebo Buildings	None	Airports	Bridges	Churches	Military Bases	Post Offices

Notes: The table illustrates shows how the how the risk of terrorism impacts Republican vote share using OLS estimation.
Column 1 shows the estimates on each component of the risk of terrorism index individually. Column 2 includes the airports as a placebo measure of buildings. Column 3, 4, 5 and 6 include bridges, churches, military bases and post offices as measures of placebo buildings respectively. All estimations include year, state and urban-year fixed effects and county level controls. In all the estimations the standard errors are clustered at the state level. The control variables include: the proportion of the population that is male, white, black, Hispanic, has high school degree, is college educated and is married; log. median income and unemployment rate. * indicates significance at 10% significance level, ** indicates significance at 5% significance level, while *** indicates significance at 1% significance level.

Table 6: Impact of Risk of Terrorism on Republican Vote Share

VARIABLES	(1) Δ REP	(2) Δ REP	(3) Δ REP	(4) Δ REP	(5) Δ REP	(6) Δ REP
Δ UASI Fund * Post,	0.000	0.002				
	(0.005)	(0.005)				
Δ Risk Index * Post,			0.012	0.018		
			(0.026)	(0.026)		
Δ Log Dist. from Large City * Post,					-0.000	-0.001
					(0.002)	(0.002)
Observations	21,736	21,736	21,736	21,736	21,729	21,729
R-squared	0.607	0.610	0.607	0.610	0.607	0.610
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Urban-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes

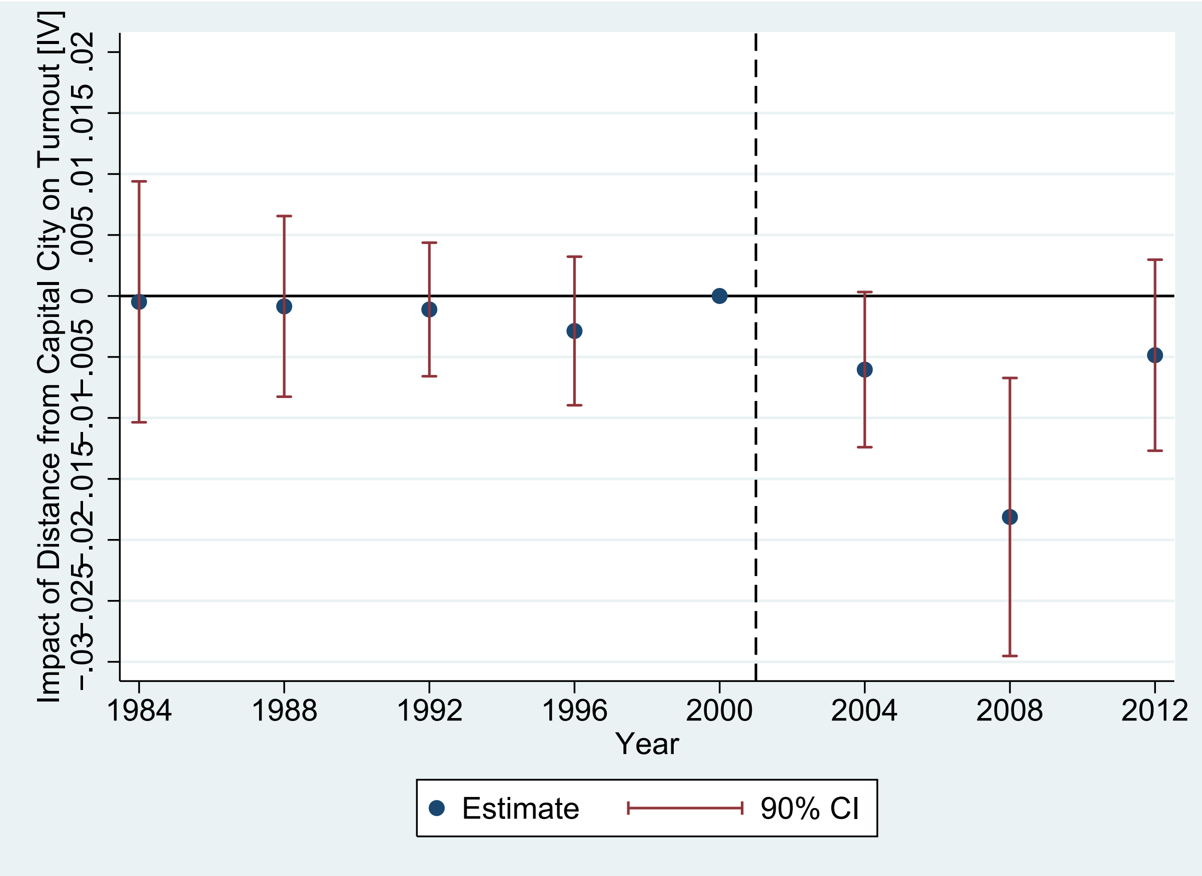
Notes: The table illustrates shows how the risk of terrorism impacts Republican vote share using OLS estimation. Columns 1 and 2 use UASI funding as a measure of the risk of terrorism. Columns 3 and 4 use the terrorism risk index, while Columns 5 and 6 use distance from the state capitol as a measure or risk of terrorism. Columns 1, 3 and 5 include the year, state and urban-year fixed effects in the estimation, while Columns 2, 4 and 6 include the year, state and urban-year fixed effects and county level controls. The control variables include: the proportion of the population that is male, white, black, Hispanic, has high school degree, is college educated and is married; log. median income and unemployment rate. In all the estimations the standard errors are clustered at the state level. * indicates significance at 10% significance level, ** indicates significance at 5% significance level, while *** indicates significance at 1% significance level.

Table 7: Impact of Risk of Terrorism on Campaign Contributions

VARIABLES	(1) \$	(2) \$	(3) \$	(4) \$	(5) #	(6) #	(7) #	(8) #
UASI Fund * Post	0.095***				0.129***			
	(0.031)				(0.036)			
Risk Index * Post		0.547***				0.688***		
		(0.164)				(0.179)		
Log Dist. from Large City * Post			-0.030**	-0.059*			-0.058***	-0.094***
			(0.014)	(0.035)			(0.014)	(0.031)
Observations	30,987	30,987	30,987	30,987	30,987	30,987	30,987	30,987
R-squared	0.937	0.937	0.937	0.937	0.955	0.955	0.955	0.955
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Estimation	OLS	OLS	OLS	IV	OLS	OLS	OLS	IV

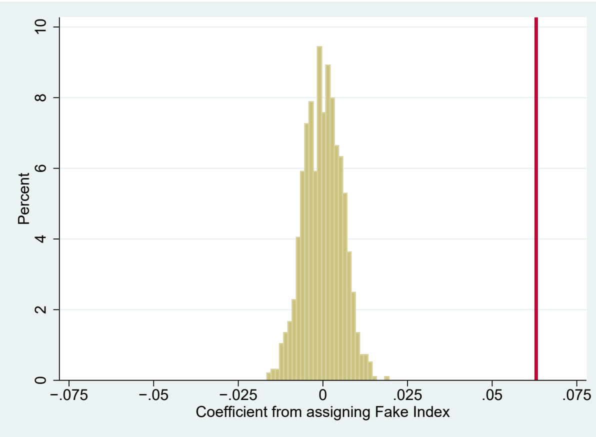
Notes: The table illustrates shows how the risk of terrorism impacts political campaign contributions using both the OLS and IV estimation. Columns 1 to 4 use the total \$ amount of campaign contributions as the dependent variables, while Columns 5 to 8 use the total number of campaign contributions as the dependent variable. Columns 1 to 3 and 5 to 7 show the estimates from OLS, while Columns 4 and 8 show the results from IV estimation. Columns 1 and 5 use UASI funding as a measure of the risk of terrorism. Columns 2 and 6 use the terrorism risk index, while Columns 3 and 7 use distance from the state capitol as a measure or risk of terrorism. All the estimations include the year, state and urban-year fixed effects and county level controls. In all the estimations the standard errors are clustered at the state level. The control variables include: the proportion of the population that is male, white, black, Hispanic, has high school degree, is college educated and is married; log. median income and unemployment rate. * indicates significance at 10% significance level, ** indicates significance at 5% significance level, while *** indicates significance at 1% significance level.

Figure A1: Event Study using Distance (IV)

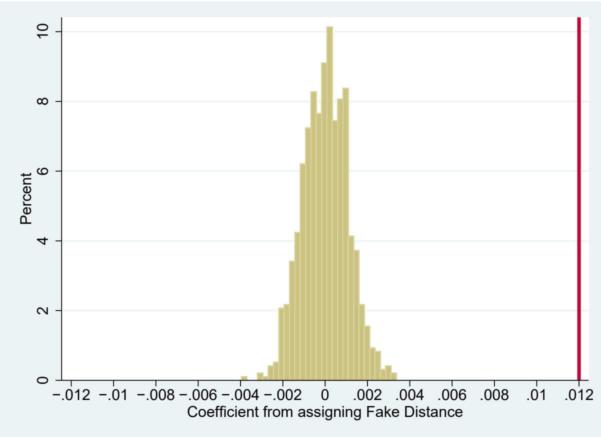


Notes: The figure plots the estimated coefficients and 90% confidence intervals obtained from event study exercise given by Equation 2 using IV estimation.

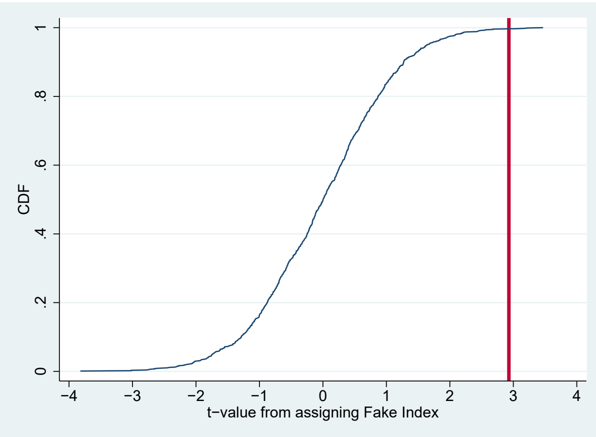
Figure A2: Falsification Exercise using Other measures of Risk



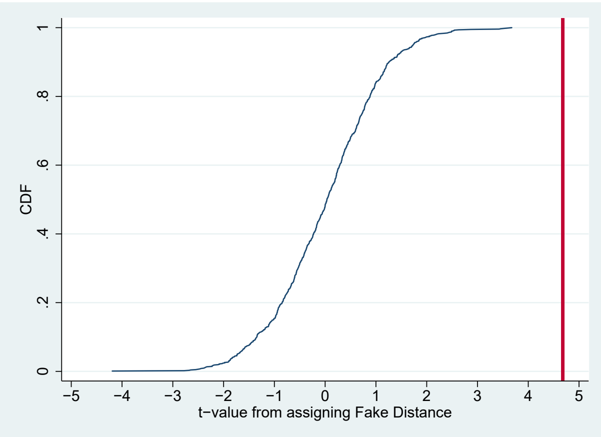
(a) Estimates using Index



(b) Estimates using Distance



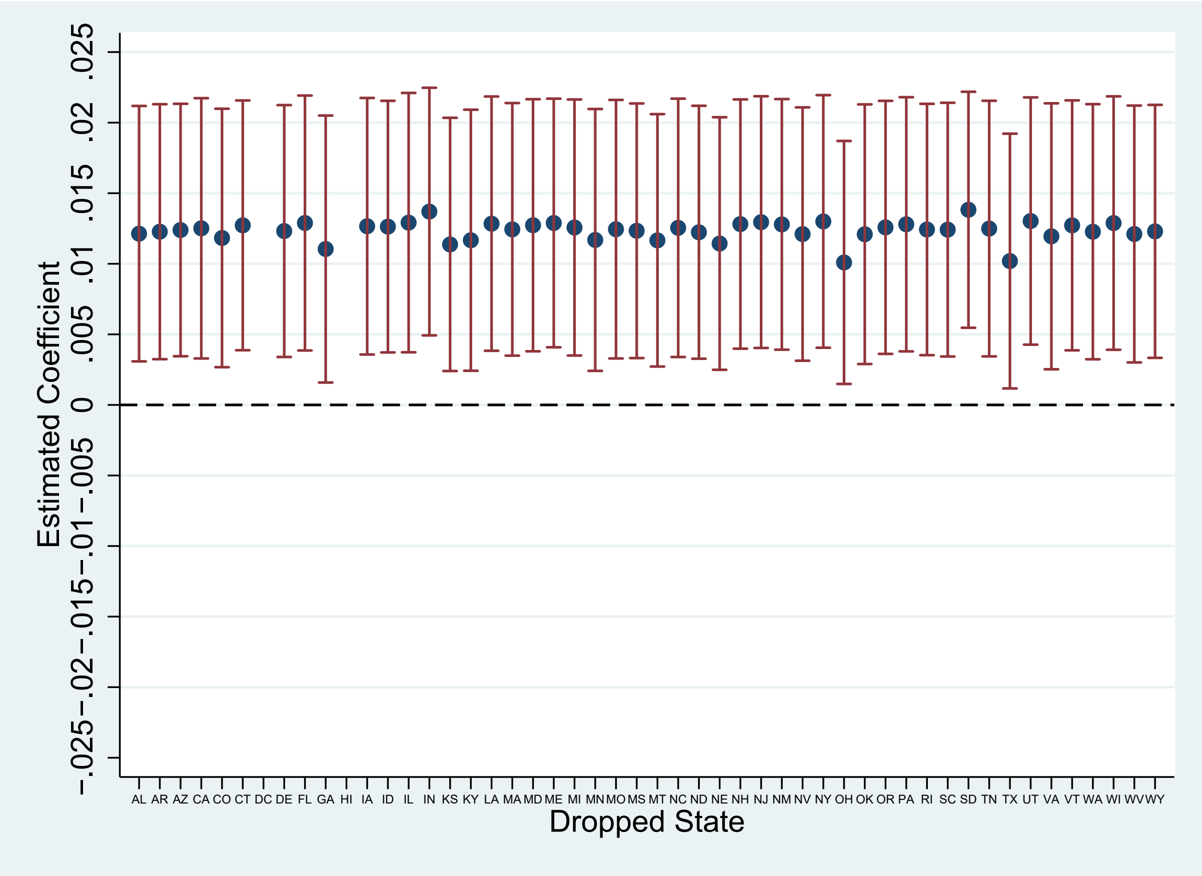
(c) t-values using Index



(d) t-values using Distance

Notes: The figure plots the results from the falsification exercise carried out by randomly assigning the risk of terrorism index and the distance from the state capitol while keeping their distribution the same. Panels (a) and (b) show the distribution of estimates of β obtained from falsification exercise using the risk of terrorism index and distance from state capitol respectively. Panels (c) and (d) show the Empirical Cumulative Distribution Function (CDF) of the t-values from the falsification exercise using the risk of terrorism index and distance from state capitol respectively.

Figure A3: Estimates dropping a state at a time



Notes: The figure plots the estimates and 95% confidence interval of β obtained by dropping all counties located in a given state. The measure used for risk of terrorism is whether the county received UASI Funding.

Table A1: Differences w.r.t distance from the state capitol

	Levels		Trends	
	Estimate	SE	Estimate	SE
Turnout	-0.003	(0.003)	-0.001	(0.001)
REP	0.019***	(0.006)	0.009	(0.005)
Male	-0.001	(0.001)	-0.000**	(0.000)
White	0.019*	(0.009)	0.000	(0.000)
Black	-0.022**	(0.009)	0.000	(0.000)
Hispanic	0.008**	(0.004)	0.001	(0.001)
High School educated	0.003	(0.002)	0.000	(0.001)
College educated	-0.015***	(0.002)	0.001	(0.000)
Married	0.009***	(0.002)	0.001**	(0.000)
Urban	-0.02	(0.017)	-0.001***	(0.000)
Log. Median Income	-0.050***	(0.010)	0.000	(0.000)
Unemployment	0.002**	(0.001)	-0.003*	(0.002)
Pop 18-65 years	-0.008***	(0.002)	-0.004	(0.002)
Pop > 65 years	0.007***	(0.002)	0.000	(0.000)

Notes: The table illustrates how the levels and trends of main variables vary with respect to the distance from the state capitol before the September 11 attacks. The table reports the estimates and standard error of ρ from Equation 2 using log. distance from state capitol instead of log. distance from state's geographic centroid. The standard errors are clustered at the state level. * indicates significance at 10% significance level, ** indicates significance at 5% significance level, while *** indicates significance at 1% significance level.

Table A2: Robustness to Definition of Distance

VARIABLES	(1) Δ Turnout	(2) Δ Turnout	(3) Δ Turnout	(4) Δ Turnout	(5) Δ Turnout	(6) Δ Turnout	(7) Δ Turnout	(8) Δ Turnout
Δ Log. Alternate Distance from Closest City * Post,	-0.0067***	-0.0163**	-0.0026*	-0.0275**	-0.0049**	-0.0324***	-0.0046***	-0.0265***
	(0.0024)	(0.0075)	(0.0014)	(0.0125)	(0.0022)	(0.0126)	(0.0015)	(0.0097)
Closest City Definition	State Largest		UASI Funding		500k Pop.		100k Pop.	
Observations	21,736	21,729	21,736	21,729	21,736	21,729	21,736	21,729
R-squared	0.4966	0.4945	0.4961	0.4474	0.4964	0.4717	0.4968	0.4689
Estimation	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Urban-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Kleibergen-Paap F Statistic		24.86		20.26		24.60		23.72

Notes: The table illustrates shows how the risk of terrorism impacts voter turnout using both the OLS and IV estimation using alternate measures of distance. Columns 1 and 2 measure the risk of terrorism as distance from the largest city in the state; Columns 3 and 4 measure the risk of terrorism as the distance from the closest city that received UASI funding; Columns 5 and 6 (7 and 8) measure the risk of terrorism as the distance from the closest city with more than 500,000 (100,000) population. Columns 1, 3, 5 and 7 show the estimates from OLS, while Columns 2, 4, 6 and 8 show the results from IV estimation. All the estimations include the year, state and urban-year fixed effects and county level controls. In all the estimations the standard errors are clustered at the state level. The control variables include: the proportion of the population that is male, white, black, Hispanic, has high school degree, is college educated and is married; log. median income and unemployment rate. * indicates significance at 10% significance level, ** indicates significance at 5% significance level, while *** indicates significance at 1% significance level.

Sticking to one's guns: Mass Shootings and the Political Economy of Gun Control in the U.S.

Hasin Yousaf *

Abstract

Mass shootings are unfortunately frequent events which keep drawing public attention towards gun policy. The divide on gun policy among Republicans and Democrats has increased both among voters and politicians. However, we know very little about mass shootings and its effects. In this paper, I construct a list of mass shootings in the U.S. from 2001-12 and analyze their impact on electoral outcomes, voter preferences, and gun policy. Using a Difference-in-Difference strategy, I find that Republicans lose significant votes in all federal (Presidential, Gubernatorial, Senatorial, and House) elections after mass shootings. Variations of identification strategy, placebo and falsification exercises suggest that this decline reflects a causal impact of mass shootings. While mass shootings result in lower individual campaign contributions for the Republicans, the NRA increases its contributions to Republican candidates. I then show that mass shootings do not change the average preferred gun policy among the electorate, but rather impact the electoral outcomes through an increase in the importance of gun policy among voters. The lack of change in the average preferred policy masks the increase in the polarization between Republicans and Democrats. Mass shootings lead to *an even greater* disagreement on gun policy among voters: while Democrats demand greater gun control after mass shootings, Republicans shift towards *lower* gun control. Likewise, politicians from both parties shift to more diverging stances on gun policy.

Keywords: Mass Shootings; Gun Policy; Salience; Polarization; Issue-voting; NRA

JEL Classification: D72, D83, H11, J18, P16

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1 Introduction

Mass shootings have unfortunately become a common part of American life. The number of mass shootings has doubled from around 10 per year in the first half of the 2000s to around 20 per year in the second half. In addition, the recent mass shootings are more deadly. More than 400 people have been killed and 1,500 have been injured in the mass shootings in 2017 alone. Being killed in a mass shooting is one of the top five fears among Americans: about one-in-ten individuals are scared of dying in a mass shooting (Bader, 2016). Mass shootings are salient events that draw public attention towards gun policy (Krouse and Richardson, 2015). Gun policy is one of the issues on which the American electorate is deeply divided. The proportion of electorate supporting gun control and gun rights stands at 51% and 47% respectively in 2015 (PEW U.S. Politics & Policy, 2016). Yet, we have little understanding of consequences of mass shootings on the political outcomes and preferences for gun control. Do events such as mass shootings impact political outcomes? If so, why do voters react to such events? Do these events bring together or divide the voters? Do these events influence gun policy?

In this paper, I analyze how mass shootings impact political outcomes, preferences on gun control and gun policymaking. Mass shootings are public events in which four or more people are killed. Using detailed data from the FBI and media sources, I construct a list of mass shootings from 2001-2012 at the county level and employ a difference-in-difference strategy (DiD) to compare changes in political outcomes in areas with mass shootings relative to changes in areas without mass shootings before and after the event. Several tests show that conditional on county population, mass shootings are events that can be considered to a large extent random. First, I show that there is no difference in the political outcomes among areas with and without mass shootings prior to the event and only emerges after it. That is, the areas with and without mass shootings have similar trends prior to the event. Second, I show that the probability of a mass shooting in an area is uncorrelated with a rich set of local demographic, economic, crime, gun, and health characteristics. Third, the trends in the past political and local characteristics are uncorrelated with the occurrence of mass shootings. In addition, mass shootings cannot be predicted by local characteristics and past political outcomes. Furthermore, the placebo estimates show that mass shootings in the future do not impact the current political outcomes. In addition, I perform a falsification exercise by randomly assigning mass shootings to different areas and obtain estimates with a zero mean.

Four different identification strategies, which are variants of DiD, yield similar results. First, in order to alleviate the concern that unobserved local geographic factors may be different across counties with mass shootings, I utilize only the sample of counties with mass shootings and its contiguous counties for my estimation. In my second strategy, I use propensity score matching on local characteristics to predict the probability of a mass shooting for each county and use counties that have the most similar predicted probability of mass shooting for my estimation. Third, in order to alleviate the concern that counties with mass shootings differ systematically on some underlying unobserved factors, I use only the counties with mass shootings and compare changes in the counties with mass shootings today (during my sample period) to the changes in the counties with mass shootings in the past (pre 1990s). Finally, I use set of counties with "successful" and "failed" mass shootings and exploit the inherent randomness in the success or failure of mass shootings to identify the impacts of mass shootings on political outcomes. This identification relieves any concern that the counties with mass shootings are selected according to some unobserved factors because it relies on a much weaker assumption i.e. conditional on being a location targeted by a mass shooting, the success or failure of the shootings may be considered as plausibly exogenous.

Mass shootings have a large impact on the electoral outcomes. I find that the Republicans lose 4 percentage points in the counties with mass shootings in the Presidential elections compared to counties without mass shootings. This reflects a loss of 1,900 (or 7%) votes for Republicans in an average county in the Presidential elections. Moreover, Republicans lose significant vote share in the other federal elections (House, Senatorial and Gubernatorial elections) as well. These results are not explained by an increase in the political participation (turnout) or do not reflect an anti-incumbent effect. The estimates are robust to different ways of measuring the key variables, different specifications, and alternate inferences. In addition, evidence from the individual campaign contributions reflects a similar picture: Republicans, relative to Democrats, lose on average 4.4 percentage points (or $17,000) in the individual campaign contributions in areas affected by mass shootings.

Some mass shootings lead to a stronger electoral impact than the others. Shootings that take place during the campaign period, in the swing counties and in schools result in a stronger electoral response among the voters. The media plays an important role in determining the electoral consequences of mass shootings. In order to measure the impact of media, I use intuition from Durante and Zhuravskaya (forthcoming) to compare changes in the electoral outcomes due to mass shootings that occur during big sports events such as Super Bowl, FIFA World Cup and Olympics with changes in the electoral outcomes of mass shootings during other times. Shootings that occur around during times of news competition from other events result in a slightly lower electoral impact (3 percentage points loss for Republicans) which is statistically indistinguishable from the average effect.

I then explore the channels driving the electoral outcomes. In a simple multi-dimensional voting setting (Riker and Ordeshook, 1973), mass shootings can increase the importance of gun control relative to the other issues (salience) or can change the preferred policies on gun control among voters. I use the American National Election Studies (ANES) survey to study the relative contribution of each factor. I find that mass shootings increase the importance of gun policy. However, these shootings do not change the average preferred gun policy among voters.[1] Previous empirical studies associate the impact of a shock on electoral outcomes to a change in preferences, without being able to identify which part of the preferences (salience or preferred policy) changes.[2] It is important to understand

[1]The impact of a pure increase in salience without any change in the preferred gun policy would depend on the distribution of preferred policy among voters on gun policy. The Republicans lose votes because the preferred policy for gun control is skewed in favor of the Democrats. The number of individuals supporting gun control and gun rights stands at 57% and 41% respectively (PEW Research Center, 2012). In addition, 55% voters think that Democrats are better at handling gun policy, compared to only 23% voters who think Republicans are better at handling gun policy (ANES 2016 Pilot Study).

[2]For instance, shale oil energy booms lead to an increase in the Republican vote share (Fedaseyeu et al., 2015). Does this reflect a shift in the preferred energy policy in favor of Republicans? Or are

which part of the preferences drives the electoral outcomes because they lead to different policy conclusions (Hatton, 2017). While a change in the preferred gun policy would imply that gun control should change, a simple change in the importance of gun control does not imply that the gun policy should change necessarily.

While there is no average change in the preferred gun policy, do mass shootings help the electorate reach consensus on gun policy? I find that mass shootings contribute towards the increasing political divide among Republican and Democrat voters. Mass shootings lead Republican and Democrat voters to update their preferred gun policy in the opposite direction. While after observing the shootings Republican voters in districts with mass shootings shift their preferred policy towards *lower* gun control, Democrat voters shift towards *higher* gun control. This result is consistent with the recent theoretical literature which rationalizes why individuals may diverge upon observing common signal (Dixit and Weibull, 2007; Andreoni and Mylovanov, 2012; Acemoglu et al., 2016).[3]

Mass shootings also impact the gun policymaking. Using roll-call voting of politicians on gun specific issues in the U.S. House, I find that mass shootings lead to further policy divergence among Republican and Democrat politicians. While Republicans from districts with mass shootings are more likely to vote in favor of laws *reducing* gun control after the event, Democrats are more likely to vote in favor of laws *tightening* gun control after the event.

My paper is related to the recent growing literature on causes and consequences of mass shootings. On the consequences of mass shootings, few recent papers use only the deadliest mass shootings for analysis. For instance, Koenig and Schindler (2016) and

these results due to the higher salience of the energy policy? Similarly, trade shocks from China impact the electoral outcomes (Autor et al., 2016). However, what is the relevant contribution of increased importance of trade policy among voters and changes in the preferred policy on trade?

[3]It is one of the first empirical findings in political economy providing empirical support to the recent theoretical literature. The only other paper in political economy that I am aware of that finds similar evidence is Autor et al. (2016) who show that the increased trade pressure from China contributes to political polarization. My paper differs from theirs in two important ways. First, I use a direct measure of preferences (survey data) to show polarization instead of using electoral data. Using survey data to measure preferences is important because, as shown in the mechanisms, changes in electoral outcomes do not trivially imply changes in preferred policy. Second, they show geographic polarization i.e. districts which were initially Republican became more conservative and vice versa. On the other hand, I show polarization among Republicans and Democrats *within* district.

Doyle and Stancanelli (2017) use the Sandy Hook school shootings to analyze how gun ownership impacts crime and how shootings impact emotions and time use respectively. I contribute to the literature by analyzing using data from several mass shootings and studying the impact of mass shootings on a wide range of political outcomes including electoral outcomes and preferences for gun control. The closest work to my paper is Luca et al. (2016) who study the impact of mass shootings on the state gun policy.[4] They find that the changes in state law depend on who is in power at the time of shootings: the states with Republican-majority legislators are more likely to decrease gun control and vice versa. My paper differs from theirs in three important aspects. First, I use a geographically finer variation (county level instead of state level) to estimate how the policy making changes among the Republican and Democrat politicians. Second, I study the impact of mass shootings on a wide range of political outcomes to paint the whole picture of the impact of shootings. Specifically, in addition to analyzing the gun policymaking, I analyze the impact of mass shootings on electoral outcomes and individual preferences for gun control. Third, I complement their work by providing possible mechanisms explaining their results. My results suggest that the gun policy making may become more polarized because the politicians become more convinced that their solution is the right one (Rabin and Schrag, 1999; Dixit and Weibull, 2007), due to the role of special interest groups (Grossman and Helpman, 1996) or due to preferences of "base" voters (Mian et al., 2010). In addition, we know very little about the causes of mass shootings. The limited literature in criminology argues (without proper systematic analysis) that local area characteristics play little or no role in determining mass shootings (Muschert, 2007; Duwe, 2013; Metzl and MacLeish, 2015). I expand on this literature by providing one of the first systematic evidence on when and where mass shootings take place and whether local characteristics are related to mass shootings.

Finally, I contribute to the growing literature that tries to understand the rising polarization among the U.S. voters and politicians. Most of the literature has established

[4] Apart from this paper, most of the literature on gun policy has focused on the impact, rather than determinants, of gun policy. For instance, Depetris-Chauvin (2015) and Knight (2013) study the impact of gun policy on the demand for guns and crime respectively.

correlations (Gentzkow, 2016). The limited literature trying to attribute causal factors to political polarization has focused on the role of economic shocks such as economic inequality (Voorheis et al., 2016), financial crisis (Mian et al., 2014) and trade shocks from China (Autor et al., 2016). I contribute to the literature by providing one of the first causal evidence that shocks to non-economic issues can cause an increase in political polarization.

The remaining paper is organized as follows. In section 2, I present an overview of mass shootings, the politics of gun control and the data sources. In section 3, I describe the identification strategy and present results supporting the empirical strategy. In section 4, I present the main results of the impact of mass shootings on the Republican vote share. In section 5, I present further evidence in favor of my identification and replicate my results using alternate identification strategies. In section 6, I present some additional results. I first present further results on other electoral outcomes, campaign contributions, and gun policymaking. Then, I discuss the main channels through which these results operate. I then study how the impact of mass shootings varies with local and shooting characteristics. Finally, I present the concluding remarks in section 7.

2 Context

In this section, I first present the background on mass shootings and then describe the various data sources.

2.1 Mass Shootings

The FBI defines mass shootings as: "the shooting of four or more victims at one location or crime scene" (Douglas et al., 2013, p. 437). Throughout the paper, I follow the FBI definition of mass shootings. There are three different types of mass shootings: public, familicide and other felony mass shootings (Krouse and Richardson, 2015). In this paper, I focus on the public mass shootings and loosely refer to them as mass shootings. There are several reasons for considering only public mass shootings. First, public mass shootings are plausibly exogenous compared to the familicide and other felony mass shootings. Second, public mass shootings may have a completely different meaning for the community: a community may generally not relate to the family and criminal shootings because they may feel that they would never be a part of these events. The shootings which take place in the malls, schools, religious places and other public places have a clear public domain and the wider community can relate to these events.[5] Third, public mass shootings are more likely to be attributable to the issue of gun control as these shootings are highly salient events that receive great public and media attention and renew an interest in the gun policy debate (Krouse and Richardson, 2015).

We know very little about the causes of public mass shootings (Fox and DeLateur, 2014).[6] According to the leading criminologist, Grant Duwe, "no one knows why mass public shootings take place $\cdots$ Those who blame these events on violent video games and availability of weapons are really missing the mark" (National Post, 2012). There is limited evidence that the local or community factors, such as demographics and economic conditions, are the main cause of these shootings (Muschert, 2007). Similarly, there is no systematic evidence that poor mental health is the primary cause of these shootings (Metzl and MacLeish, 2015). More than half of the shootings are carried out by individuals with no pre-diagnosed mental illness (Stanford Mass Shootings in America, 2016). Similarly, the hypothesis that lax gun laws are the primary catalyst for mass shootings has little statistical support, as the likelihood of mass shootings is similar across states with different set of gun laws.[7]

While the number of homicides has decreased steadily since the 1990s, the number of mass shootings has increased over the last two decades. Figure 1a shows that the annual number of mass shootings doubled from 10 in the first half of the 2000s to 20 in the

[5]The research on psychology refers to this phenomenon as the "identified" victims. See Cohen (2015) for a synthesis of the literature.

[6]We only know some basic facts about the characteristics of mass shooters. A typical mass shooting is carried out by a 20 to 40 years old, mentally healthy, white male.

[7]For instance, from 2001-12, Texas (the state with one of the weakest gun control) has had 13 mass shootings, while California (the state with one of the strongest gun control) has had 12 mass shootings.

second half. Not only have the number of mass shootings increased in the past decade, but the recent mass shootings are more deadly shootings. For instance, eight out of the ten deadliest mass shootings in the history of U.S. have occurred since the 2000s.[8]

2.2 Politics of Gun Control

Gun policy is one of the most partisan and debated topics in U.S. politics (Callaghan and Schnell, 2001). The Republicans tend to favor access to guns as a fundamental right for every American citizen, while the Democrats tend to make a case for restrictions on access to guns. Similarly, Republicans and Democrats disagree on the causes of mass shootings and propose different solutions to prevent future shootings. Republicans blame the shooters' individual conditions, particularly the mental health conditions, as the primary determinant of mass shootings. They propose that the law abiding citizens should have access to guns to protect themselves and as a consequence foil these shootings (gun rights). Democrats, on the other hand, blame the weak gun control laws as the main reason for these events. Therefore, they propose tightening the access to guns to prevent these events (gun control).

The electorate is deeply divided on whether to increase gun control or gun rights. While the majority of electorate inclined towards Republicans supports gun rights (71% to 26%), the majority of electorate inclined towards Democrats back gun control (72% to 21%). On the other hand, the independents are split between support for gun rights (39%) and gun control (54%) (PEW Research Center, 2012). Figure 2 shows the evolution of preferences for gun rights and gun control over time. We see that there has been a dramatic increase in polarization on preferences for gun control, as the individuals supporting gun control have steadily decreased from 64% in 1999 to 51% in 2015, while individuals supporting gun rights have increased from 32% in 1999 to 47% in 2015.

[8]These include: 58 people killed in the Las Vegas Strip shootings, Las Vegas (NV) in 2017; 49 people killed in the night club in Orlando (FL) in 2016; 32 people killed in the Virginia Tech University, Blacksburg (VA) in 2007; 27 people killed in the Sandy Hook Elementary School, Newtown (CT) in 2012; 14 people killed in the San Bernardino (TX) in 2015; 13 people killed in the Fort Hood (TX) in 2009; 13 people killed in the Binghamton (NY) in 2009.

Similarly, the electorate is divided on which party provides a better solution for gun policy. According to The National Election Studies (2016), the majority of voters (55%) think that Democrats are better at handling the gun policy, while 23% of the voters think that Republicans are better at handling the gun policy. 82% Democrat voters (66% Republican voters) think that the Democratic (Republican) party is better at handling the gun policy. On the other hand, 25% independents cite that the Democratic party is better at handling the gun policy, while only 19% independents think that Republican party is better at handling the gun policy.

2.3 Data

In order to compile a comprehensive list of mass shootings, I combine data from the official and media sources. The primary data source is the FBI Supplementary Homicide Reports (2016). The local enforcement agencies submit a detailed report (type, timing, location and probable motive) of all the violent crimes that took place in their jurisdiction to the FBI.[9] I complement the FBI data with the data from the media sources in two ways. First, I perform a validation exercise for all the incidents recorded in the FBI. That is, I search the media sources to find the reporting of these mass shootings recorded in the FBI. Second, I augment the FBI data with the list of mass shootings by USA TODAY reporting and analysis (2016). The USA TODAY reporting and analysis (2016) analyzed the list of mass shootings from FBI and compared them to mass shootings reported in the media. They found that around 10% of the mass shootings are not covered by the FBI. In my main analysis, I consider together the mass shootings from these two data sources.[10][11]

[9] In order to extract only the public mass shootings from the FBI SHR database, I keep the events in which: four or more people died, the main weapon used was some form of gun, the probable motive of the offender was unknown and the victims were unrelated with the offender.

[10] I obtain similar results if I only consider mass shootings obtained through the FBI sources. The results are shown in Table 8.

[11] Recently there are several different databases which claim to track the location of mass shootings. I rely on the official sources instead of these sources for three main reasons. First, most of these databases are new and track only the recent mass shootings. For instance, the Gun Violence Archive website only started tracking the mass shootings from 2014 onwards. Second, most of these databases use a different definition from the official definition to record mass shootings. Third, these databases may reflect a recording bias based on their political affiliation (Gentzkow and Shapiro, 2010).

In total, there were 143 mass shootings in 122 counties during 2001 to 2012.[12] Figure 3 shows the location of mass shootings from 2001 to 2012 along with the local (county) population. We see that mass shootings are not specific to a certain geography but occur in almost every state. We do not see any particular pattern in the location of mass shootings other than that the shootings are more likely in the counties with higher population. 77 (7) counties in the top (bottom) population quartile have a mass shooting. The counties with a quarter million higher population are twice as likely to have a mass shooting relative to a county with average population. Figure 1a shows the timing of mass shootings across the sample period. We see that mass shootings are relatively frequent events and seem largely unrelated to the timing of the elections. There are in total 68 mass shootings in the years in which there are congressional elections, while there are 70 mass shootings in the years without congressional elections.[13]

The electoral data is compiled from two main sources. First, I use the U.S. Election Atlas website (Leip, 2016) to obtain the total votes, votes for the Republican and Democrat candidates for each county for the Presidential, Senatorial and Gubernatorial elections from 1996 to 2012. Second, I collect the House of Representative election results directly from the Federal Election Commission (2016b) website. The data includes the total votes for each candidate along with the incumbent status of each candidate in a congressional election. I use the data to construct the total votes, the incumbency status and the vote share for each candidate.

The data on campaign contributions is obtained directly from the Federal Election Commission (2016a) database. The FEC requires all the individual and political action committee (PAC) donations of more than $200 to be reported to the FEC. In order to study how the individual campaign contributions change due to mass shootings, I construct the total individual contributions received by each political party at the county

[12] In total 104, 15 and 3 counties have only one, two and three mass shootings, respectively, during 2001 to 2012. The results are similar if I drop the counties with multiple mass shootings.

[13] Figure 1b shows the distribution of mass shootings across different months. We see that mass shootings are well distributed across months. There are 29, 36, 40 and 43 mass shootings in the first, second, third and fourth quarters respectively.

level from 1996 to 2012. To test whether the special interest groups respond to the shootings, I calculate the total contributions by the National Rifle Association (NRA) to the Republican and Democrat candidates.[14]

In order to understand the underlying mechanisms explaining the electoral outcomes, I use the American National Election Studies (ANES) survey. Specifically, I use 2000, 2008 and 2012 pre-election survey in which the respondents are asked about the importance of gun policy and their preferences on the gun policy.

In order to measure the policy response to the mass shootings, I analyze how the roll-call voting on the gun control changes in the House of Representatives. Using roll-call voting by the politicians on the gun control, I construct a gun specific DW-Nominate for each politician. The DW-Nominate characterizes each politician on an ideological scale from liberal to conservative (on a scale from -1 to 1) based on their voting pattern.[15] In order to construct a gun specific DW-Nominate, I extract the roll-call votes related to firearms (issue code = 82).[16]

The data on demographic and economic variables are obtained from the United States Bureau of the Census (2016). The crime data is obtained from the FBI Uniform Crime Reports (2016). The gun-related variables are constructed using the Vital Statistics from Centers for Disease Control and Prevention (2016).[17] Finally, the health-related variables are obtained from the University of Wisconsin Population Health Institute (2017).

[14]The NRA is by far the biggest special interest group in U.S. on the issue of gun control. It is consistently ranked in the top quintile of the PAC donors. For instance, in 2000 the NRA gave \$3.3 million in campaign contributions to the candidates and party committees (92% to the Republicans). Similarly, their contributions are wide-spread: on average, they donate to half of the total members of the House and Senate. On the other hand, there are no large organized gun-control PACs. All the gun control PACs collectively gave \$5,899 in contributions during the 2012 election cycle. Therefore, I focus on the political contributions behavior of NRA.

[15]The DW-Nominate score is widely used in the political science to study the ideologies of the legislators (Poole and Rosenthal, 1985, 1991).

[16]This yields a total of 71 roll-call votes from the 105th to the 113th House of Representatives. The complete list of issues are available at `http://www.voteview.com/isscodes.htm`.

[17]I use the Fatal Injury Reports which contains the cause of death. I use the data to extract all the non-natural deaths due to firearms. The gun homicides are given by the categories: W32, W33, W34, X93, X94, X95, Y22, Y23, Y24, Y35.0, while the gun suicides are given by the categories: X72, X73, and X74. The Vital Statistics data encompasses the universe of deaths and suicides and is the most reliable source of data on the deaths and suicides related to gun (Regoeczi et al., 2014)

Table 1 shows the summary statistics for the main variables. We see that an average county has a Republican vote share of 56.5% and a turnout of 53.2% in the Presidential elections. The mean value of mass shootings is 0.015 implying that 1.5% of the county-year observations have mass shootings. In a typical county, there are 2329 violent crimes, out of which 1454 are larceny. Moreover, a typical county has 1.76 (8.25) murders (suicides) per 100,000 individuals by guns. The accidental deaths by guns are 0.034 per 100,000 individuals.

3 Identification

In this section, I outline the identification strategy employed to estimate the causal impact of mass shootings on electoral outcomes. In addition, I discuss the main threats to the identification and present results in favor of the identification strategy.

3.1 Empirical Strategy

In order to identify the causal impact of mass shootings on electoral outcomes, I estimate the following specification:

$$repshare_{it} = \alpha_i + \alpha_t + \beta(MSE * Post)_{it} + X'_{it}\Gamma + u_{it}, \tag{1}$$

where $repshare_{it}$ represents the Republican vote share in the county i in the election t. MSE equals to one for counties which have a mass shooting and is zero otherwise. $Post$ is an indicator equal to one for counties with mass shootings after the event and is zero otherwise.

The term α_i controls for county fixed effects and absorbs all the time-invariant county-level factors and characteristics that are correlated with both mass shootings and political variables. The differences among counties in factors such as geography, institutions, and legislation which do not change over the sample period are accounted for by these fixed effects. Thus, the estimation relies only on changes in the political outcomes across counties.[18] α_t accounts for year fixed effects, which absorbs factors which are common for all counties within an election. Factors such as the federal gun laws, the valence of presidential candidates and national shocks are absorbed by this term. Thus, the estimation relies only on the changes in political outcomes relative to other counties within an election.[19]

X_{it} is a vector of time-varying local including population, demographic, socio-economic, gun, crime and health-related variables shown in Table 1 that may affect the electoral outcomes. In all the estimations, I cluster standard errors at the congressional district level to allow for arbitrary correlation among counties within a congressional district across elections.[20]

β is the main variable of interest. β compares changes in the Republican vote share in counties with mass shootings to changes in counties without mass shootings.[21] The identification assumption for estimating the causal impact of mass shootings on political outcomes is that in the absence of mass shootings, counties with and without mass shootings would have followed the same path. In the next section, I show evidence in favor of my identification assumption.[22]

[18] In particular, the inclusion of fixed effects accounts for the difference in population level among counties.

[19] The results are similar if I include state-year fixed effects or allow for congressional district specific time trends. The inclusion of state-year fixed effects yields two additional benefits over the baseline specification. First, it accounts for any differential trend in the political outcomes among counties across states. Second, since most gun laws vary at the state level and may change over time, the inclusion of state-year fixed effects accounts for these changes. The inclusion of congressional district specific time trends compares changes in the political outcomes in counties with mass shootings to counties without mass shootings within the same congressional district. The results are shown in Table 8.

[20] The results are robust to alternate ways of computing standard errors. In Table 8, I calculate standard errors in four different ways. I cluster the standard errors at the state level, calculate the standard errors using block bootstrap as proposed by Bertrand et al. (2004), allow for potential auto-correlation in the residuals and estimate the spatial robust standard errors as in Conley (1999). All these different methods yield virtually identical standard errors.

[21] β captures the average impact of mass shootings on changes in the political outcomes in counties with mass shootings relative to the other counties in all the election periods after the event. In Section 3.2.1, I study the dynamic impact of mass shootings. That is, I study how the impact of mass shootings varies in each election after the event.

[22] One may be concerned that county is not the actual level of treatment because there a mass shooting may affect a larger geography than a county. In A2, I study whether there are any geographic spillovers of mass shootings in other counties located near the shootings and whether there are any spillovers to other

3.2 Plausibility of Identification Assumption

In this section, I discuss possible threats to the identification of the causal impact of mass shootings on political outcomes and show results in favor of my identification assumption. The main identification assumption needed to identify the causal impact of mass shootings on political outcomes is that the areas with and without mass shootings would have evolved in the same way in the absence of mass shootings. I show evidence in favor of this assumption in the next section by carrying out an event study around the time of mass shootings. In addition, I show that the counties which have mass shootings are similar to other counties in both levels and trends. Finally, I show that local characteristics and past political variables cannot predict when and where mass shootings will take place.

3.2.1 Event Study Around Mass Shootings

Since I use Difference-in-Difference strategy, an important assumption for estimation of the causal impact of mass shootings in this setup is that the counties with and without mass shootings would have evolved in the same way in the absence of mass shootings (parallel trends assumption). This assumption is untestable. However, we can lend support in favor of this assumption by analyzing how counties with and without mass shootings were prior to the shootings. If counties with and without mass shootings have similar trends in Republican vote share prior to the shootings and nothing else systematically changes around the event,[23] then the difference that emerges after the event between counties with and without mass shootings can be attributed to mass shootings.

In particular, I compare changes in Republican vote share in counties with mass shootings to counties without mass shootings in a flexible way around the event. This allows us to study how does the Republican vote share changes in each election before and after mass

counties within the same geographic unit. The results show that there are limited geographic spillovers of mass shootings.

[23]In my case this is less of a concern because the timing of mass shootings varies across counties. Nonetheless, in Section 3.2.2, I will provide evidence that none of the local variables systematically vary around the event.

shootings. I estimate the following:

$$repshare_{it} = \alpha_i + \lambda_t + \sum_{t=-n}^{m} \beta_t (MSE_i * \mathbb{1}(t = j)) + X'_{it}\Gamma + u_{it}, \tag{2}$$

where $t = 0$ denotes election during which mass shootings take place. β_t measures the difference in changes in Republican vote share between counties with and without mass shootings in each period (prior to and after mass shootings). If we find that all β_t prior to mass shootings ($t < 0$) are economically and statistically insignificant, then this provides strong evidence in favor of our identification assumption. Since I am using county fixed effects in all the specifications, I have to use one period as a base comparison period ($t = -1$).

Figure 4 plots the coefficients β_t from Equation 2. We see that the counties with and without mass shootings have a similar trend in Republican vote share prior to the shootings in all the federal elections. The finding that areas with and without mass shootings have a similar trend in Republican vote share in all the federal elections and have a similar trend in up to eight election periods prior to the shootings (Senatorial elections) provides strong support in favor of the identification assumption. The results are not only statistically insignificant but are also economically insignificant i.e. the majority of the coefficients are lower than 0.5 percentage points.

Figure 4 also highlights the dynamic effect of mass shootings. We see that the impact of mass shootings on voting outcomes does not fade away after the immediate election. Instead, the mass shootings result in a long-term loss for the Republicans. The dynamics are slightly different across different elections. While, the effect of mass shootings on Presidential and House elections remains same across elections, the effect of mass shootings increases (decreases) over time for Senatorial (Gubernatorial) elections.[24]

[24] One should be careful in comparing the magnitude of β_t across time periods. Although the total number of counties in the sample always remains the same, the set of counties used for estimation of β_t varies across time periods. For instance, for Presidential elections, β_0 is estimated using all mass shootings (2000 to 2012), while β_1 (β_2) is estimated using only mass shootings that occur between 2000 and 2008 (2000 and 2004).

3.2.2 Comparing Counties with and without Mass Shootings

One concern could be that the counties with mass shootings are not comparable to counties without mass shootings due to pre-existing differences in local characteristics. For instance, we may be concerned that counties with mass shootings are, say, more violent (higher crime rate) than the other counties. Since, I include county fixed effects in all my specifications, the pre-existing differences in levels do not matter for my identification. In order to see whether counties with mass shootings are similar to other counties in levels, I estimate the following:

$$x_i^{2000} = \rho_0 + \rho_1 MSE_i + \rho_2 f(Pop_i^{2000}) + v_i, \tag{3}$$

where x_i^{2000} represents the value of a local characteristic (x) in the year 2000. MSE_i equals to one for all counties that have a mass shooting and zero otherwise. Since mass shootings are more likely in more populated places, I control flexibly for the local population. ρ_1 captures the difference in the level of local characteristic, x, in 2000 between counties with mass shootings and other counties.

Columns 1 and 2 of Table 2 report the estimates and standard errors of ρ_1 for a rich set of local characteristics. We see that there are no systematic differences among counties with and without mass shootings. Counties which have mass shootings are similar on political, socio-demographic, economic, gun, crime, and health variables to counties without mass shootings.[25] Although the pre-existing differences in levels do not matter for my identification due to the inclusion of county fixed effects, it is, however, reassuring to see that the counties with mass shootings are not different than counties without mass shootings in levels of pre-existing local characteristics.

One additional concern may be that the local characteristics may evolve differently in counties with mass shootings relative to the other counties. Say, we may be concerned that the counties that have mass shootings were, say, becoming more violent. In order to see

[25] The joint F-Test of significance of each of these categories is also statistically insignificant.

whether counties with mass shootings have similar dynamics in the local characteristics, I estimate:

$$x_i^{12-00} = \rho_0 + \rho_1 MSE_i + \rho_2 f(Pop_i^{2000}) + v_i, \tag{4}$$

where x_i^{12-00} represents the change in local characteristic x between 2000 and 2012. ρ_1 captures the difference in the trend of x between counties with mass shootings and other counties. If mass shootings only impact electoral outcomes and not change the local characteristics, we should expect no differential trend in these variables in counties with mass shootings relative to other counties.[26]

Columns 3 and 4 of Table 2 report the estimates and standard errors of ρ_1 for the local characteristics. We see that past political variables, socio-demographic, economic, crime, gun, and health variables were evolving in the same way in counties with mass shootings compared to other counties. Together, these results show that while mass shootings are more likely in more populated areas, once we compare counties with similar size, the assignment of mass shootings is essentially random. Since I include county fixed effects in all my specifications, the pre-existing differences in population level are absorbed by these fixed effects.

3.2.3 Predicting Mass Shootings

One further concern may be that the counties which have mass shootings may be similar on local characteristics, but these local characteristics may jointly explain which counties receive mass shootings. In order to study nature of the selection, I predict the probability of mass shootings using past political outcomes and local characteristics. In particular, I run the following linear probability model:

$$MS_{it} = \alpha_i + \alpha_t + \beta_1 REP_{it-1} + \beta_2 Turnout_{it-1} + X_{it}'\Pi + \epsilon_{it}, \tag{5}$$

[26]In addition, in order to check if the pre-existing trends in the local characteristics are similar among counties with and without mass shootings, I estimate Equation 4 using changes in the local characteristics before the sample period (between 1996 to 2000). The unreported results show that the counties with and without mass shootings do not have a pre-existing differential trend in the local characteristics.

Table 3 presents the coefficients β_1 and β_2 along with joint F-Test of significance on each group of variables. Column 1 shows the unconditional estimates. We see that the past political outcomes do not explain the incidence of mass shootings. Political variables together explain almost no variation in the likelihood of mass shootings. In the subsequent columns, I add the demographic, economic, gun, crime, and health variables successively. None of these local characteristics together predict location and timing of mass shootings. The coefficient on Republican vote share and turnout remains statistically and economically insignificant. All these variables together along with county fixed effects explain less than 10 percent of the variation in the mass shootings.[27]

Together, these results show that counties that have mass shootings are similar, both in levels and trends, to other counties in political, demographic, economic, gun, crime, and health characteristics. In other words, we cannot point which counties will have mass shootings and cannot distinguish counties with mass shootings from other counties based on a rich set of local characteristics.

In the next section, I analyze the impact of mass shootings on Republican vote share. In Section 5, I provide further results in favor of my identification assumption and use four different identification strategies, which are variants of DiD and rely on much weaker identification assumptions, to measure the impact of mass shootings.

4 Election Results

In this section, I study the impact of mass shootings on voting outcomes. First, I study the impact of mass shootings on Republican vote share in the Presidential elections and then in the other federal elections.

[27]One may be concerned that the extreme values of local characteristics may predict mass shootings. That is, local characteristics may non-linearly predict mass shootings. I address this concern by using deciles of local characteristics to test whether they predict mass shootings. The results are presented in Table A3.

4.1 Presidential Elections

In this section, I test if there are any electoral consequences of mass shootings on Republican vote share in the Presidential elections. Specifically, I estimate Equation 1 using voting in the Presidential elections. Table 4 shows the results. Column 1 shows the estimates without any controls. The Republicans lose, on average, 4.4 percentage points (significant at 1% level) in counties with mass shootings after the event relative to counties without mass shootings. In the following columns, I successively add demographic, economic, gun, crime, and health variables respectively. Column 7 shows the preferred specification with demographic and economic controls together. Finally, column 8 includes all the local characteristics together. The impact of mass shootings on Republican vote share remains unchanged upon addition of each of these variables. Republicans lose, on average, 4 percentage points in the Presidential elections in counties with mass shootings relative to counties without mass shootings.

The estimates suggest a significant number of lost votes for Republicans. The estimates imply that the Republicans lose $1,910$ (or 7%) votes after mass shootings in an average county. These results are large compared to the impact of other events and media persuasion. For instance, DellaVigna and Kaplan (2007) found that introduction of Fox News increased the Republican vote share in the areas by 1 percentage points. My results imply that the impact of mass shootings is four times as strong as the persuasion by the Fox News.

How large is the effect of mass shootings on aggregate election results? Assuming that mass shootings do not impact turnout substantially, this implies that mass shootings move $65,000$ votes in a Presidential election away from the Republicans. These are a substantial number of votes which can change the electoral outcome. For instance, in the 2000 election, George Bush won the pivotal state of Florida by a meager 537 votes. An additional mass shooting in Florida during the 2000 election cycle would have resulted in Al Gore winning Florida and securing the presidency. Similarly, Donald Trump won the three swing states: Michigan, Wisconsin, and Pennsylvania in the 2016 presidential elections by $10,704$, $22,748$ and $44,292$ votes respectively. According to my estimates, 6 additional mass shootings per year (1 in Michigan, 2 in Wisconsin and 3 in Pennsylvania) in these states would have changed the electoral outcome.[28]

4.2 Other Elections

In this section, I study the impact of mass shootings on Republican vote share in other elections. Specifically, I analyze whether Republican vote share changes in the Gubernatorial, Senatorial and House elections in areas with mass shootings relative to other areas.[29]

Table 5 reports the estimates of β using Gubernatorial, Senatorial and House elections. Columns 1 to 3, 4 to 6 and 7 to 9 show the impact of mass shootings on the Gubernatorial, Senatorial elections and House elections respectively. We see a precisely estimated negative impact of mass shootings on the Republican candidates in all the three elections. The Republicans lose 3.7, 2.7 and 2.7 percentage points in the Gubernatorial, Senatorial and House elections, respectively, in counties with mass shootings relative to other counties. Interestingly, the impact of mass shootings on Republican vote share in the elections for executive branch (Gubernatorial and Presidential) is similar to one another.[30]

5 Further Results in Favor of Identification and Robustness

In this section, I present further results supporting that the main results reflect a causal impact of mass shootings on voting outcomes. I start by showing that four different

[28]These are conservative estimates of the number of mass shootings needed to flip the elections. In Section 6.3, I show that certain mass shootings have greater impact on the political outcomes than others.

[29]The data for the House elections is at the congressional district level. If impact of mass shootings are localized (only impact county and not congressional district), then using election results at congressional district level would yield lower (less negative) and less precise estimates.

[30]The executive branch has the veto power to propose or oppose new laws discussed in the legislative branch.

identification strategies, which are variants of DiD, yield virtually identical results. Second, I perform falsification tests by randomly assigning "fake" mass shootings to counties. Third, I use intuition from Altonji et al. (2005) to get a sense of how much selection on unobservables has to be to explain away the obtained results. In the robustness part, I show that the main results are unchanged to alternate variable definitions, specifications, and inference. In addition, I show that the results are not driven by mass shootings in a particular state or year.

5.1 Four different Identification Strategies

In my main analysis, I use the universe of counties to estimate the impact of mass shootings on electoral outcomes. Despite strong evidence presented in Section 3.2, which shows that counties that have mass shootings are similar to an average county, if we believe that the counties with mass shootings are different on unobservables due to geography, political dynamics or other factors which I do not observe, the main results may not reflect causal impact of mass shootings. In this section, I address this issue in detail by using four different identification strategies.

If we believe that the local geographic factors of counties with mass shootings are different from other counties, then an average county may not be an appropriate "control" group. If there are geographic unobserved factors that are specific to counties with mass shootings, then the set of neighboring counties forms a natural comparison group. In my first alternate identification strategy, I compare changes in counties with mass shootings to changes in Republican vote share in their contiguous counties to estimate the impact of mass shootings on electoral outcomes.

Although on average counties with and without mass shootings are similar on local characteristics, some counties may be more similar on local characteristics to counties with mass shootings than the others. In my second alternate identification strategy, I employ propensity score matching to estimate the impact of mass shootings on electoral outcomes. I match counties with mass shootings to 10 counties which are most similar (based on the Mahalanobis distance) on the local population, demographics, population, crime, gun and health characteristics using matching estimator proposed by Abadie et al. (2004). In this strategy, I compare changes in Republican vote share in counties with mass shootings to counties that do not have mass shootings but have a similar predicted probability of mass shootings (as predicted by local characteristics).

Despite the counties with and without mass shootings are similar on local characteristics, one may be concerned that counties with mass shootings differ significantly on unobservables. We may be concerned that counties with mass shootings are "selected" according to some underlying unobserved factors. If we believe that some underlying time-invariant unobserved process explains which counties have mass shootings, the counties that have mass shootings today or in the past may share this process. In my third alternate identification strategy, I use only the set of counties that ever had mass shootings (today or in the past). That is, I compare changes in the Republican vote share in counties that have mass shootings to counties that had mass shootings in the past.[31] Thus, this identification relies on using the timing of mass shootings to estimate their electoral impact.

Finally, if we believe that the underlying unobserved process that leads to mass shootings is time-varying, the counties that receive mass shootings today may be very different from the set of counties that received mass shootings in the past and an average county. In order to address this concern, I compare the changes in the electoral outcomes in areas with "successful" mass shootings to changes in areas with "failed" mass shootings. Using FBI Active Shooter, I make a list of failed mass shootings. In these incidents, a shooter started shooting in the public but was unable to carry out a mass shooting. The shooter either fled or committed suicide (more than 60% times) or was apprehended by locals or captured by police (less than 40% times). Hence, in my fourth alternate identification, I use the inherent randomness in the success and failure of carrying out a mass shooting to estimate the impact of mass shootings on electoral outcomes. This identification relies

[31] Figure A1 shows the location of past and present mass shootings. In total, there were 401 mass shootings in 377 different counties in the past (1965 to 1990). We can clearly see that mass shootings in the past were also well spread across the U.S.

on a much weaker assumption i.e. conditional on being a location targeted by a mass shooting, the success or failure of the shootings may be considered as plausibly exogenous. This is especially true in my case because these shooters do not have any experience in carrying out these shootings.[32]

Table 6 shows the results obtained using these different identification strategies. We see that the estimates are similar to the ones obtained in the main section, remain stable across different empirical strategies and also remain highly statistically significant. Since all the results paint a similar picture, these results add confidence that the results obtained in the main section reflect a causal impact of mass shootings on voting outcomes.

5.2 Falsification Tests

In this section, I perform series of placebo estimates to show that the main results are driven by mass shootings. In particular, I study whether mass shootings have an impact on the Republican vote share in the previous elections. That is, I test whether mass shootings in the future impact the Republican vote share today. In addition, I study whether the placebo mass shootings events such as familicide and gang-related mass shootings, shootings in which three individuals are killed and "failed" mass shootings have any electoral impact. Finally, I carry out a falsification exercise by randomly assign "fake" mass shootings to the counties and estimate the main equation 1,000 times to test whether the main results are driven due to some unobserved systematic differences among counties.

Table 7 shows the results from the placebo estimates. Columns 1 and 2 show that the mass shootings today have no impact on the Republican vote share in one period and two periods before the event. Column 3 shows that the familicide and gang-related mass shootings do not impact the Republican vote share. Similarly, column 4 shows that the "failed" mass shootings do not impact the Republican vote share. Finally, shootings in

[32]Figure A2 shows the location of failed and successful mass shootings. In total, 67 mass shootings were prevented in 65 different counties during 2001 to 2012. We can clearly see that the failed mass shootings are also well spread across the U.S.

which three people are killed do not generate any electoral impacts.

Figure 5a plots the distribution of β parameter from estimating Equation 1 by randomly assigning "fake" mass shootings to counties (keeping proportion of counties that receive mass shootings same) $1,000$ times. We see that the estimates are centered around zero: the mean magnitude of β indicates an effect of lower than 0.025 percentage points ($-.0002448$), with a standard deviation of .0076. Most of the simulations (81% or 813 out of 1,000) result in an estimated magnitude between -0.01 and 0.01. Only 13 (1.3%) of the estimates result in an absolute magnitude of greater than an absolute value of 0.02 percentage points. None of the estimates yield a value greater than 3 percentage points. Figure 5b plots the Cumulative Density of t-values of β obtained from these estimates. We see that the estimates follow the t-distribution, with 10.6%, 5.9% and 1.4% of β being statistically significant at 10%, 5%, and 1% respectively. The t-value obtained in the main results is a clear outlier in the distribution.

5.3 Using Selection on Observables to Assess the Bias from Unobservables

In order to understand if the main results are driven by selection on unobservables, I use strategy proposed by Oster (2017) to gauge potential bias from unobservables using selection on observables.[33] Oster (2017) proposes that we can gauge the magnitude of selection on unobservables based on selection on observables by analyzing the change in coefficient, scaled by movements in R^2, after including representative set of controls to the coefficient obtained without any controls. That is, $(\dot{\beta} - \tilde{\beta})(\frac{R_{max}-\tilde{R}}{\tilde{R}-\dot{R}})$ gives an estimate of

[33] Even though I show that the counties with and without mass shootings are very similar on observables and control for them in my estimations, one concern may be that I fail to account for unobservables which are correlated with the likelihood of mass shootings and political outcomes, which leads to a biased estimate of the impact of mass shootings. The results from four alternate identification strategies which rely on much weaker identification assumptions yield similar results, adding confidence that what we are capturing reflects a causal impact of mass shootings and is unlikely to be driven by differences in unobservables among areas with and without mass shootings. However, if we are still not convinced that the estimations reflect a causal impact of mass shootings, here I explicitly study the likelihood that the results are driven by unobservables.

the omitted variable bias.[34] $\tilde{\beta}$ and $\tilde{R}$ are the coefficient on the main variable of interest and R^2 obtained using a set of representative controls and $\dot{\beta}$ and $\dot{R}$ are the coefficient on the main variable of interest and R^2 obtained without the controls. R_{max} is the hypothetical maximum possible R^2 obtained if we observe all unobservables and include them in our regression. The main intuition is that given a set of fully representative controls, closer is beta without any controls to the one with controls, greater the selection on unobservables needs to be to explain away the results. Similarly, higher is the beta with controls, the greater is the magnitude of the effect that needs to be explained away by the selection on unobservables. These coefficient movements need to be scaled by movements in R^2 to account for how important these controls are in explaining the outcome variable. Hence, a higher number implies it is less likely for selection on unobservables to explain the obtained results.

Table A1 shows the results obtained for all the federal elections using the method proposed byOster (2017). We see that the selection on unobservables as a proportion of selection on observables needed to obtain a β equal to zero is higher than 2 in all the cases.[35] That is, on average the selection on unobservables has to be twice the selection on observables for the true effect of mass shootings on electoral outcomes to be equal to zero. This is much higher than the bounding value of $\delta = 1$ proposed by Oster (2017). In addition, given the rich set of controls employed, it is less likely that the impact of mass shootings is purely driven by unobservables. Moreover, the "identified set", i.e. the bias-adjusted range of treatment effect which corrects for selection on observables, does not include zero which suggests that the true effect of mass shootings on electoral outcomes is different from zero. Finally, we can see that the "identified set" lies within the 95% confidence interval of the estimates suggesting that the estimates obtained in the main results suffer very little, if any, from bias due to unobservables.

[34] Oster (2017) builds on the work of Altonji et al. (2005) by making further assumptions to arrive at a simple formula for the omitted variable bias.

[35] For House elections, the negative value implies that we need the selection on unobservables to work in the opposite way as to selection on observables to obtain a bias-adjusted treatment effect equal to zero.

5.4 Robustness

In this section, I show that the main results are robust to different ways of defining variables, specifications, and inference. Specifically, I study the effect of mass shootings on electoral impact by using a different definition of Republican vote share and using mass shootings from FBI SHR only. I then study how the results change to difference specifications. Moreover, I calculate the standard errors allowing for different structure among the residuals. Finally, I study whether the impact of mass shootings is driven by shootings in a particular year or by a particular state.

Table 8 shows the results with different definitions and specifications. In Column 1, I use only the mass shootings recorded in the FBI SHR. In Column 2, instead of using overall Republican vote share, I use the two-party Republican vote share. We see that the results are robust to an alternate way of coding mass shootings (Column 1) and Republican vote share (Column 2).

In Column 3, I include state-year fixed effects. This implies that I am comparing changes in electoral outcomes in counties with mass shootings to changes in electoral outcomes in other counties within the same state. This accounts for any time-variant changes in political outcomes across states. Similarly, in Column 4 I include congressional district specific time trends to allow for differential trends in political variables across congressional districts. The results remain similar across inclusion of both state-year fixed effects and congressional district trends.

In Column 5, instead of using county fixed effects specification, I use a first difference specification. The magnitude is identical to the one obtained in the main results, while the estimates are more precise.[36] In Columns 6 and 7, I weight the observations by population and natural logarithm of population respectively and obtain very similar results.[37] Finally,

[36]Higher the precision of the first difference specification relative to the fixed effects specification suggests that there may be serial correlation in the errors in the first difference ($\Delta\epsilon_{it}$) in the electoral data. In Table 8, I estimate the standard errors allowing for potential correlation among the residuals.

[37]The standard errors in both columns 6 and 7 are similar to those obtained using unweighted estimates suggesting that there is limited heteroskedasticity present in the error term (Solon et al., 2015).

in column 8, I control for local characteristics in a flexible way by including deciles of each variable. This allows for arbitrary differences in Republican vote share across different deciles of local characteristics. The results are similar to the results in the main section.

In addition, we may be worried that the electoral impacts of mass shootings are due to some specific shootings or concentrated in some particular states. In Figure A3, I allow for mass shootings in each year to have a potentially differential impact on the electoral outcome. We see that the results are not driven by mass shootings that occur during a particular year. All of the individual years result in a loss for Republicans. Mass shootings in nine out of twelve years result in a loss for Republicans of more than 3 percentage points. Similarly, the results are not driven by mass shootings occurring in a particular state. In Figure A4, I re-estimate my main estimates by excluding one state at a time. The results show that the impact of mass shootings remains same if I drop any state, suggesting that the results are not concentrated due to mass shootings in a particular state.

Moreover, the results are robust to alternate methods of inference. Table 8 shows the estimates and standard errors constructed in several different ways. In the baseline, the standard errors are clustered at the congressional district level (Column 1).[38] In Columns 2, 3 and 4, I allow for the residuals to have an AR(1), AR(2) and arbitrary order serial correlation among the residuals respectively. We see that the standard errors are slightly lower than the one in the baseline, suggesting that there is some serial correlation among the residuals and accounting for it improves the precision. In Column 5, I cluster the standard errors at the state level and obtain standard errors twice as large as the baseline suggesting that there is a substantial correlation in unobservables among counties within a state. However, the results are still statistically significant at 5% significance level, with a p-value of 0.025. In column 6, I follow Bertrand et al. (2004) and estimate standard error using the Block Bootstrap method and obtain comparable standard errors. Finally, in column 7, I correct for potential spatial dependence among the residuals by using Spatial

[38] I obtain lower standard errors (0.0100) if I cluster the standard errors at the county level (county is geographically smaller than congressional district).

Robust standard errors as in Conley (1999) and obtain similar standard errors.

6 Mechanisms, Additional Results and Heterogeneity

In this section, I discuss the possible mechanisms driving the changes in electoral outcomes. Specifically, I study how mass shootings impact other electoral outcomes and campaign contributions. I then explore the relative contribution of a change in the importance of gun policy and a change in the preferred policy on gun control in explaining the electoral outcomes. In next section, I provide some additional results. Specifically, I provide evidence that the electorate and the policymaking are becoming more polarized on the gun policy. In the final section, I present heterogeneity of the electoral results.

6.1 Mechanisms

In this section, I study possible mechanisms explaining the main results. In particular, I first study the impact of mass shootings on voter turnout and incumbent vote share. I then study the impact of mass shootings on campaign contributions. Finally, I outline a canonical multi-dimensional Downsian model and use survey data to understand the relative contribution of change in the importance of gun policy and change in preferred gun policy in explaining the observed electoral change.

6.1.1 Turnout and Incumbent Share

In this section, I study the impact of mass shootings on the voter turnout and incumbent vote share. Studying turnout helps us understand whether the mass shootings mobilize new voters (turnout increases), dissuade the Republicans from voting (turnout decreases) or simply shift votes from Republicans to Democrats (turnout unchanged). Similarly, analyzing the incumbent vote share helps us understand whether the voters systematically punish the incumbents for mass shootings.

Table 9 shows the impact of mass shootings on the turnout and incumbent vote share in all the federal elections. We see that mass shootings have little or no impact on turnout in the Presidential (Column 1) and House (Column 7) elections. On the other, mass shootings lead to a small increase in turnout in the Gubernatorial (Column 3) and Senatorial (Column 5) elections. We do not see any impact of mass shootings on the incumbent vote share in the federal elections i.e. incumbents do not lose systematically more in the areas with mass shootings relative to other areas after the event. These results suggest that the changes in the electoral outcomes are not primarily driven by mobilization of new voters nor do they reflect an anti-incumbency effect.[39]

6.1.2 Campaign Contributions

In this section, I study the impact of mass shootings on the political campaign contributions. Analyzing the individual campaign contributions helps us understand whether the mass shootings impact the political arena beyond the voting outcomes. Moreover, campaign contributions can help us study whether mass shootings result in Republicans being disadvantaged in terms of political contributions, which may reflect in their vote share in the elections. In addition, studying campaign contributions by special interest groups helps us investigate how these groups respond to the mass shootings.

Table 10 shows the results using individual political campaign contributions and contributions by special interest groups. Columns 1 to 4 analyze the impact of mass shootings on the individual campaign contributions and paint a picture similar to the one obtained in the electoral outcomes. That is, we see that while the total individual contributions do not change (Column 1), individual campaign contributions going to Republicans fall by 15% (Column 2) and individual campaign contributions going to Democrats does not change (Column 3) in counties with mass shootings relative to other counties. Overall,

[39] In addition, if I analyze the incumbent vote share separately for Republicans and Democrats, I find that voters only punish the Republicans for these events. That is, the Republican incumbents lose votes after mass shootings, while Democrat incumbents *gain* votes after mass shootings.

the total contributions going to Republicans decrease by 4.4 percentage points (Column 4). This reflects a decrease of $17,000 during an election cycle in an average county.[40]

Columns 5 to 7 of Table 10 illustrate the change in political contributions by NRA to the areas with mass shootings. NRA increases its total contributions in areas with mass shootings by 18% relative to the other counties after the shootings (Column 5). This increase in the NRA contributions is driven entirely by an increase in the contributions to the Republicans (Column 6), while there is no change in the NRA contributions to the Democrats (Column 7). Columns 8 to 10 study whether the total contributions by other PACs changed in the areas with mass shootings relative to the other areas. We see that the total PAC contributions (Column 8), PAC contributions to Republicans (Column 9) and PAC contributions to Democrats (Column 10) do not change in the areas with mass shootings relative to other areas.[41]

6.1.3 Media

In this section, I access the role of media in determining the impact of mass shootings on political outcomes. Since I do not have actual media coverage data, I use the intuition from Durante and Zhuravskaya (forthcoming) to study the impact of media coverage of mass shootings on the electoral outcomes. Specifically, I compare the impact of mass shootings that occur during times with news pressure from other events with the impact of mass shootings that occur during other times. The intuition is that mass shootings that occur during times of news pressure from other events are less likely to be extensively covered relative to the other mass shootings that occur during times of little or no news pressure. In order to only consider news pressure around times of non-political events, I use the mass shootings during major sports events (Super Bowl, FIFA World Cup, and Olympics) to study the role of media in determining the impact of mass shootings.[42]

[40] The average individual campaign contribution to the Republicans is around 0.11 million.

[41] In addition, mass shootings do not change the probability whether NRA contributes to an area or not. This may be because the NRA is already present in several counties and congressional districts.

[42] The Super Bowl is the most watched sports event in the U.S., with 114.4 million or 50% of the adult U.S. population watching the event. Moreover, during the Super Bowl not only is the news under

Figure 6 shows the results. We see that the impact of mass shootings on Republican vote share is slightly weaker if it occurs around the major sports events compared to the shootings that occur at other times. We see that the Republicans lose 3 percentage points in counties with mass shootings around the major sports events, while Republicans lose around 3.9 percentage points in counties with mass shootings during other times relative to other counties. If we assume that mass shootings around the major sports events are not systematically different from other shootings, the difference, which is statistically insignificant, tells us that around one-quarter of the effect of mass shootings on Republican vote share is through media. This result can stem from both voters being more informed about gun policy as a consequence of mass shootings or left-leaning of popular media (Groseclose and Milyo, 2005). These results are reassuring as they suggest that the media coverage of mass shootings is not the only channel through which mass shootings impact political outcomes.

6.1.4 Issue importance and preferences

In a simple multi-dimensional voting setting (Riker and Ordeshook, 1973), the voting outcomes depend not only on the preferences on the individual issues but also on the relative weight of each issue in the voting utility function. Without loss of generality, we can focus on two dimensions: guns and other issues and express the utility function as:

$$U_i(j) = -\left(\alpha_G(x^*_G - x^j_G)^2 + \alpha_{-G}(x^*_{-G} - x^j_{-G})^2\right) \tag{6}$$

where $U_i(j)$ represents the utility of i from voting for party j. x^*_G represents the preferred policy of individual i on gun policy, while x^j_G represents the implemented gun policy if party j is elected. x^*_{-G} and x^j_{-G} are defined in an analogous way. α_G and $\alpha_{-G} = 1 - \alpha_G$ represent the relative importance of gun policy and other policy issues respectively.

Assuming that mass shootings change only the part of the utility function on gun policy, according to Equation 6, the utility (and hence decision) from voting for a party may

pressure, but also the voters may be distracted due to entertainment (Durante et al., 2017).

change either due to a change in the preferred gun policy (x_G^*) or due to a change in the importance (salience) of the gun policy (α_G), for fixed x_G^j.

In order to understand the relative contribution of each component of the utility function, I use the survey data. In particular, I use the question on the importance of gun policy and the preferred gun policy in the American National Election Studies to understand which factor drives the electoral outcomes. I measure the importance of gun policy as the answer to the question: "How important is guns issue to you personally?". The response lies on a five-point scale ranging from: "Not at all important", "not too important", "somewhat important", "very important", or "extremely important?". In addition, I measure the preferred gun policy as the answer to the question: "Do you think the federal government should make it more difficult for people to buy a gun than it is now, make it easier for people to buy a gun, or keep these rules about the same as they are now?".

I use a similar DiD setup to compare the changes in the importance of gun policy and preferred gun policy among individuals in districts with mass shootings relative to changes in other districts. In particular, I estimate:

$$Y_{idt} = \alpha_d + \alpha_t + \beta(MS * Post)_{dt} + X'_{idt}\Gamma + u_{idt}, \tag{7}$$

where Y_{idt} measures the importance of gun policy and the preferred gun policy. X_{idt} are individual level controls which include the race, income, age, education, marital status, political leaning and religiosity of the individual. In addition, I cluster the standard errors at the congressional district level. Since, I include congressional district fixed effects, β measures the changes in Y_{idt} in the districts with mass shootings relative to the changes in the other districts.

Table 11 shows the results. Column 1 measures the effect of mass shootings on the importance of the gun policy. We see that mass shootings increase the importance of gun policy by 0.085 (3% of the mean value). Moreover, we see that mass shootings do not move the average preferred policy on guns in either direction. That is, the average preferred gun policy among the electorate does not shift towards an increase in the gun control (Column 2) nor towards a decrease in the gun control (Column 3). Together, these results suggest that one of the main channels for the reduction in the Republican vote share in the federal elections is the increased importance of gun policy among the electorate.

Theoretically, a mere increase in the importance of gun policy can have three potential effects: an increase, a decrease or no change in the Republican vote share. The direction of the electoral impact depends on the distribution of the preferred gun policy among the electorate. The Republicans lose votes because the preferred gun policy among the electorate is skewed in favor of the Democrats. The number of individuals supporting gun control and gun rights stands at 57% and 41% respectively (PEW Research Center, 2012). In addition, 55% voters think that the Democratic party is better at handling the gun policy compared to only 23% voters who think that the Republican party is better at handling the gun policy (ANES 2016 Pilot Study).[43]

This result suggests that the electoral outcomes can change without a change in the preferred policy among the electorate. This calls into question the empirical literature in political economy that makes a one-to-one connection between the changes in electoral outcomes and changes in the preferred policy. For instance, recent working papers by Autor et al. (2016) and Halla et al. (forthcoming) show that the increased trade pressure from China and increased immigration leads to changes in the electoral outcomes. However, we would like to know the relative contribution of the change in the importance of trade policy and the change in the preferred trade policy among the electorate. Similarly, we would like to know how much of the changes in the electoral outcomes due to increased immigration are due to a change in the importance of immigration policy and how much

[43] An immediate question that arises from this result is that which is the issue that the gun policy replaces. Theoretically, if gun policy decreases the importance of an issue in which Republican party has an advantage over the Democratic party (or Democratic party has a lower advantage over the Republican party compared to the gun policy), we would see that the Republicans lose votes. Analyzing ANES 2016 Pilot Study, we see that there are only 7 (out of 21) issues (inequality, climate change, LGBT, women's rights, racism, poverty, and education) in which the Democratic party has a greater advantage over the Republican party relative to the gun policy. Thus, if an increase in the importance of gun policy decreases the importance of any other issues besides these seven issues, the Republican party's vote share would decrease. Unfortunately, I cannot assess which exact issue, if any, the gun policy replaces because the questions are asked in open-ended and not relative manner.

are due to changes in the preferred immigration policy. My results suggest that we cannot conclude changes in the preferred policy based on the changes in the electoral outcomes alone. This is because a shock to preferences on certain policy may not impact the preferred policy on that issue alone, but may also change the importance of that policy. Thus, any observed changes in the electoral outcomes would reflect a combination of these two changes.

6.2 Polarization and Policy Making

In this section, I study the impact of mass shootings on the preferred gun policy among the electorate in detail. Specifically, I analyze the impact of mass shootings on the partisan voters. I then study the impact of mass shootings on the voting behavior of the politicians on gun policy in the U.S. House of Representatives.

6.2.1 Political Polarization

The political polarization among the American voters has been systematically increasing since the 1990s (Gentzkow, 2016). In this section, I study whether mass shootings have contributed towards this increasing political divide among the electorate? Specifically, I analyze the changes in the preferred gun policy among voters who associate themselves with the Republican, Democratic and neither party in the areas with mass shootings relative to the other areas after the event.

Columns 4 to 12 of Table 11 illustrate how does the importance of gun policy and preferred gun policy changes among the Republicans, Democrats, and independents in areas with mass shootings relative to other areas. We see that mass shootings increase the importance of gun policy among all voters (Columns 4, 7, and 10), with a significantly higher increase in the importance of gun issue among the Democrat voters. On the other hand, we see that mass shootings lead the Republican and Democrat voters to update their preferred gun policy in the opposite direction. That is, Republicans (Democrats) in districts with mass shootings are 2.1 (4.2) percentage points more likely to say that the gun control should decrease (increase). On the other hand, mass shootings do not have an impact on the average preferred gun policy among the independent voters. One may be concerned that the current political affiliation may change itself as a result of mass shootings. In Table A4, I use the past political affiliation of the respondents and obtain similar results.

The result that the Republican and Democrat voters update their beliefs in the opposite direction as a result of mass shootings is inconsistent with a simple Bayesian updating (Andreoni and Mylovanov, 2012).[44] However, if Republican and Democrat voters update their beliefs in a Bayesian way, but have heterogeneous prior beliefs, then mass shootings may not result in the convergence in beliefs (Dixit and Weibull, 2007). To illustrate this further, suppose that both Republican and Democrat voters agree on minimizing the number of mass shootings. Suppose further that there are two states of the world: stronger gun control *cannot* reduce mass shootings (S1), and stronger gun control can reduce mass shootings (S2). Since Republicans (Democrats) are more likely to believe that the state of the world we live in is S1 (S2),[45] then upon observing mass shootings (common signal) the two groups will update their beliefs in the opposite direction. That is, mass shootings make Republicans more convinced that stronger gun control cannot prevent mass shootings, while Democrats become more persuaded that stronger gun control can prevent mass shootings, thus increasing the political divide between them. Even several mass shootings may not lead Republican and Democrat voters to converge if they are uncertain about the conditional distribution of mass shootings (Acemoglu et al., 2016).[46]

[44]Assuming that mass shootings provide some information about the current and optimal gun policy, if Republican and Democrat voters have common prior beliefs, then under Bayesian updating, Republican and Democrat voters will update their preferred gun policy in the same direction.

[45]Using PEW February 2013 survey, I find that only 29% Republicans believe that stronger gun laws can reduce mass shootings. On the other hand, 81% Democrats believe that stronger gun laws can reduce mass shootings.

[46]On the other hand, Republicans and Democrats may update their belief in the opposite direction because they may update their belief in a non-Bayesian way. For instance, the Republican and Democrat voters may suffer from a conformity bias (Rabin and Schrag, 1999). If Republican and Democrat voters suffer from a conformity bias, then mass shootings will lead them to confirm their initial hypothesis. That is, if Republicans (Democrats) believed that greater gun control would not (would) prevent mass shootings, then they will take the shootings as a confirmation of their initial belief and update their belief in the opposite direction.

6.2.2 Policy Making

Do mass shootings impact gun policy making? In this section, I study the impact of mass shootings on the voting pattern of Republican and Democrat politicians on the gun policy. Specifically, I use voting record of each politician in the House of Representatives on the gun policy to construct a DW-Nominate specific to gun issue for each politician. I then study how does this DW-Nominate score changes in the districts with mass shootings relative to the districts without mass shootings. I estimate the following:

$$Nominate_{dt} = \alpha_d + \alpha_t + \beta(MSE_d * Post_t) + X'_{dt}\Gamma + u_{dt}, \tag{8}$$

where $Nominate_{dt}$ represents the DW-Nominate score of the politician representing the congressional district d in the next congressional session. The $Nominate_{dt}$ ranges between -1 to 1, with a higher value implying more conservative voting (voting for decreasing gun control) and a lower value implying more liberal voting (voting for increasing gun control). MSE_d is equal to one for congressional districts that have a mass shooting, while $Post_t$ equals to one for all periods after the mass shootings for the congressional districts with mass shootings and is zero otherwise. α_d controls for the congressional district specific time-invariant factors and α_t are time fixed effects. X_{dt} are demographic and economic factors that impact how a politician votes in the Congress. I cluster the standard errors at the state level to allow for arbitrary correlation among congressional districts within the same state across years.

β is the main parameter of interest. β captures the average change in how politicians vote in districts with mass shootings after the event relative to the average change in politicians voting pattern in other districts. In order to see whether Republican and Democrat politicians react differently to mass shootings, I estimate:

$$\begin{aligned} Nominate_{dt} = \alpha_d + \alpha_t + \beta_1(MSE_d * Post_t) + \beta_2(MSE_d * Post_t) * DEM_{dt} + X'_{dt}\Gamma + \\ \pi_1 DEM_{dt} + \pi_2(MSE_d * DEM_{dt}) + \pi_3(DEM_{dt} * Post_t) + u_{dt}, \end{aligned} \tag{9}$$

where DEM_{dt} is equal to one for the congressional districts represented by Democrat politicians. Thus, β_1 measures the average change in how Republicans vote in the Congress on gun policy in the districts with mass shootings relative to the other districts. β_2 measures the changes in difference in the voting pattern between Republican and Democrat politicians on gun policy in districts with mass shootings compared to other districts. If $\beta_2 > 0 (< 0)$, then the difference in the voting pattern on the gun policy between Republicans and Democrats decreases (increases) in districts with mass shootings after the event compared to other districts.

Table 12 shows the impact of mass shootings on the voting pattern of politicians. Column 1 shows that there is no change in how politicians in districts with mass shootings vote on gun policy compared to other districts. Thus, on average, there does not seem to be strong evidence that mass shootings shift policymaking towards stronger gun control. However, this aggregate masks the way Republican and Democrat politicians react. Column 2 shows how Republicans and Democrats react differentially to mass shootings. We see that while Republicans become more conservative (vote for weaker gun control) in districts with mass shootings compared to other districts, Democrats become more liberal (vote for stronger gun control). The gap in Republicans and Democrats voting pattern on gun policy increases by 0.082 (or 0.16 standard deviations) in the districts with mass shootings relative to the other districts. The average difference between Republicans and Democrats in districts with mass shootings increases by 0.19 (from 0.94 in 2000 to 1.13 in 2012). This implies that mass shootings account for almost half of this increase in the difference between Republicans and Democrats on the gun policy.

Columns 3 and 4 of Table 12 study the impact on mass shootings on the probability that politicians change their voting towards lower gun control and higher gun control respectively. We see a clear pattern of Republicans and Democrats diverging in their voting on gun policy in the districts with mass shootings. We see that Republicans (Democrats) are 16.8 (9.5) percentage points more (less) likely to shift their voting towards decreasing gun control in districts with mass shootings relative to other districts. On the other hand, Republicans (Democrats) are 12.9 (3.4) percentage points less (more) likely to shift their voting towards increasing gun control in districts with mass shootings.

Does NRA play a role in this political divide between Republicans and Democrats? In Column 5, I study whether the presence of NRA matters for how Republicans and Democrats react to mass shootings. I measure the NRA presence as whether the NRA gave political contributions to candidates in the district. Since NRA can choose whether to give political contributions as a result of mass shootings, I use the past NRA contributions to measure whether a district has NRA presence or not. We see that the presence of NRA exacerbates the political divide between Republicans and Democrats. While the difference between Republicans and Democrats voting pattern on gun policy increases by 0.051 (0.10 standard deviations) in the districts with mass shootings without NRA presence compared to other districts, this difference increases by 0.254 (0.203 + 0.051) or 0.50 standard deviations in the districts with mass shootings where NRA is present. The greater increase in the political divide in the districts with NRA presence is mainly driven by Republicans voting more conservatively in those districts where NRA is present.

Do local political preferences play a role in alleviating or worsening this political divide? In Column 6, I study whether the political divide between Republicans and Democrats depends on whether the district is closely contested. I define a district as closely contested or swing district if the margin of victory of Republicans or Democrats in the past five elections is lower than 0.25. We see that though the political divide due to mass shootings increases in both swing and other districts, this increase is much lower in the closely contested districts. The difference between the voting pattern of Republicans and Democrats increases by 0.122 or 0.25 standard deviations (0.038 or 0.08 standard deviations) in non-swing (swing) districts with mass shootings relative to other districts.

How much of this increase in the political divide between Republicans and Democrats is due to politicians changing their voting (within politician) and how much is due to new politicians being elected (between politician)? Columns 7 and 8 show the relative contribution of within and between politicians on the political divide between Republicans and Democrats on the gun policy. We see that Republican incumbents become more conservative (imprecisely measured), while incumbent Democrats become more liberal (imprecisely measured) in districts with mass shootings. The difference between Republicans and Democrats increases by 0.063 or 0.13 standard deviations (precisely measured). This implies that three-fourths of the increase in the political divide is driven by within politician changes. Column 8 shows that Republicans and Democrats that are elected after mass shootings take a much more extreme stance on the gun policy. The newly elected Republicans vote 0.245 or 0.85 standard deviations more conservatively relative to their predecessor in districts with mass shootings, while newly elected Democrats vote 0.033 or 0.10 standard deviations more liberally relative to their predecessors.

6.3 Heterogeneity

In this section, I study some important heterogeneity in the effect of mass shootings on the electoral outcomes. Specifically, I analyze different characteristics of mass shootings and study whether they lead to a differential impact on the voting outcomes. Throughout this section, I focus on the Presidential elections. I study how the timing, local political preferences, intensity, location and local gun laws matter in determining the impact of mass shootings on the electoral outcomes.

6.3.1 Timing of Mass Shootings

In this section, I study the impact of timing of mass shooting on the electoral outcomes. Specifically, I study the relation between when (number of months before elections) mass shootings take place and its impact on Republican vote share. Figure 7a illustrates the results. We see that independent of the timing of mass shootings, Republicans lose statistically significant vote share in the counties with mass shootings relative to other counties. Mass shootings that take place just one month before the election have a 50% stronger impact on the Republican vote share than other mass shootings. We see that the impact of mass shootings during the electoral campaigning period (up to 9 months before elections) is much stronger than the impact during other periods. We see a jump in the impact of mass shootings on Republican vote share between the 9th and the 10th month which coincides perfectly with the start of the electoral campaign period.

6.3.2 Political orientation of county

In this section, I analyze whether the impact of mass shootings is different across counties with different political preferences. Specifically, I study how the impact varies with Republican margin of victory in the county in 2000 Presidential elections.[47] Figure 7b shows the results. We see that Republicans lose statistically significantly in all counties regardless of its political orientation. However, the impact of mass shootings is strongest in the counties which are closely contested by Republicans and Democrats. Furthermore, the impact of mass shootings is weakest in the Republican counties, with Republicans losing almost no votes in strongly Republican counties (margin of victory greater than 20%).

6.3.3 Number of deaths

In this section, I study whether mass shootings with more deaths result in a stronger impact on electoral outcomes. Figure 7c shows that the impact of number of deaths in shootings on Republican vote share. We see that mass shootings with a higher number of deaths do result in a higher lose for Republicans. However, the estimates are very imprecisely measured due to few mass shootings with a higher number of deaths.[48]

6.3.4 Location

In this section, I study whether the location of the shootings matters for electoral outcomes. Specifically, I study whether shootings that take place in schools, malls, workplaces,

[47]There are 208 counties in which Republicans lose by a margin greater than 20%, while there are 1239 counties in which Republicans win by a margin greater than 20%. The remaining 1690 (909) counties are ones with Republican margin of victory or loss is less than 20% (10%).

[48]Majority of the mass shootings (57%) are one in which exactly 4 people die. Only 11% of the mass shootings result in 10 or more deaths, with the deadliest mass shooting in my sample resulting in 32 deaths (Virginia Tech Mass Shooting, Blacksburg, VA, on April 16, 2007).

residential places and other places generate different impact on Republican vote share.[49] Figure 7d shows the results. We see that Republicans lose votes no matter where mass shootings take place. Mass shootings that take place in schools result in 8 percentage points decrease in Republican vote share relative to other counties. On the other hand, mass shootings that take place in residential locations generate the weakest impact on Republican vote share. The differences are economically significant. However, due to lack of power, the differences are not statistically significant across the different locations of mass shootings.

6.3.5 State Gun Law

In this section, I analyze whether there is a differential electoral impact of mass shootings in areas with different state gun laws. I obtain the data on state gun laws from https://www.statefirearmlaws.org. State Firearm Laws provides yearly state gun score by counting the number of 114 firearm restrictions that are upheld according to the state law. The score is scaled from 0 to 100, where higher score means a more restrictive gun law.[50] Figure 7e shows how the impact of mass shootings varies with the past state gun laws (laws before the mass shooting). We see that the impact of mass shootings is similar across states with different gun laws. The flat inverse U-shaped relation suggests that impact of mass shootings is slightly stronger (though not statistically different) in states with weakest and strongest gun laws.

[49] Around 40% of the mass shootings take place in a school (university or other teaching institutes), 21% mass shootings take place in a mall, 14% in the workplace, 15% in the residential area and the rest (10%) in other places.

[50] Vermont (3), Montana (5), South Dakota (5), Idaho (6) and Alaska (7) are the states with weakest gun control laws, while Massachusetts (100), California (85), Hawaii (67), Connecticut (65) and New York (60) are the states with strongest gun control laws in 2000. The middle 50% of the states (inter-quartile range) lies between 10 and 24.

6.3.6 Shooter Characteristics

In this section, I explore whether the shooter characteristics matter in determining the impact of mass shootings on the Republican vote share.[51] Figure 8 shows the effect of mass shootings with respect to different shooter characteristics on the Republican vote share.[52] We see that none of the shooter characteristics are important in determining the electoral consequences of mass shootings. The effect is slightly stronger in magnitude, though statistically insignificant, if shootings are carried out by old and white shooters. The impact of mass shootings on Republican vote share does not depend on the mental condition of the shooter, whether the shooter was killed, arrested or escaped and the type of gun used by the shooter.

7 Conclusion

In this paper, I studied the impact of mass shootings on the political outcomes in the United States. Using data from the official and media sources, I construct a list of mass shootings at the county level from 2001 to 2012 and analyze its impact on the federal elections. I find that Republicans lose votes in all the federal elections, with Republicans losing most in the Presidential and Gubernatorial elections. Data on campaign contributions reveals a similar picture i.e. individual campaign contributions shift from Republicans to Democrats. The NRA increases its contributions to Republicans in areas with mass shootings after the event.

Results suggest that mass shootings impact electoral outcomes mainly by changing the

[51] One should be careful in comparing differences along a shooter characteristic because these characteristics are not mutually exclusive. For instance, a shooter can be white and mentally ill. Moreover, shooter characteristics may be correlated with other shooting features. For instance, white shooters are likely to kill more people compared to black shooters.

[52] The median shooter age is 28 years, with half of the shootings carried out by shooters who are 20 to 42 years old. 47%, 28%, 13% and 11% of mass shootings are carried out by white, black, native American and other race shooters respectively. Similarly, 58% mass shootings are carried out by individuals who are not mentally ill. In addition, the probability that the shooter gets killed, arrested or escapes is 58%, 36% and 7% respectively. Finally, 63% mass shootings are carried out using handguns, 14% using rifles, while 23% are carried out using multiple weapons.

salience of gun policy. The districts with mass shootings are more likely to report gun policy as an important issue after the event. However, mass shootings do not change the average preferred gun policy in these districts. Republicans lose votes from higher salience of gun policy because Democrats have a relative advantage over Republicans on gun policy. This finding has an important policy implication. The popular media and politicians call for increasing gun control after every mass shooting. If mass shootings lead to a change in the preferred gun policy among the electorate, then we would need to update gun control laws in a way that reflects changes in the underlying preferences of the electorate. However, a mere change in the salience of gun policy may not imply that gun control laws should change. Policymakers should take these two factors (salience and preferred policy) into account and gauge the relative contribution of each channel before making policy decisions.

In addition, I find that mass shootings exacerbate the political divide among Republicans and Democrats. Mass shootings lead Republican voters and politicians to demand a decrease in the gun control, while these shootings lead Democrat voters and politicians to demand an increase in the gun control. Mass shootings account for half of the increase in the polarization among Republicans and Democrats since the last two decades. This result is consistent with Republicans and Democrats having heterogeneous prior belief leading them to diverge further due to mass shootings (Dixit and Weibull, 2007). This is first systematic evidence that events which make an issue more salient such as mass shootings can contribute towards polarization of politics. The increasing divide among both the electorate and the politicians on what is the correct policy solution makes changing the gun policy even more difficult.

More broadly, these results highlight how electorate makes political decisions in the wake of events that draw attention towards a particular issue. The paper highlights that an increased salience of an issue may generate changes in the electoral outcomes without a change in the preferred policy on that issue. For instance, if areas receive weather shocks or large waves of immigrants, significantly different from their average, these events may draw voters' attention towards climate change or immigration. This may lead voters to change votes in favor of the party that they think has a better policy on climate change or immigration. This change may reflect a pure salience effect, and may not lead to change in preferred policy on climate change or immigration among the electorate. However, the weather shocks and immigrant waves may divide the electorate and politicians further on what is the best policy solution and may make changing policy on these issues even more difficult.

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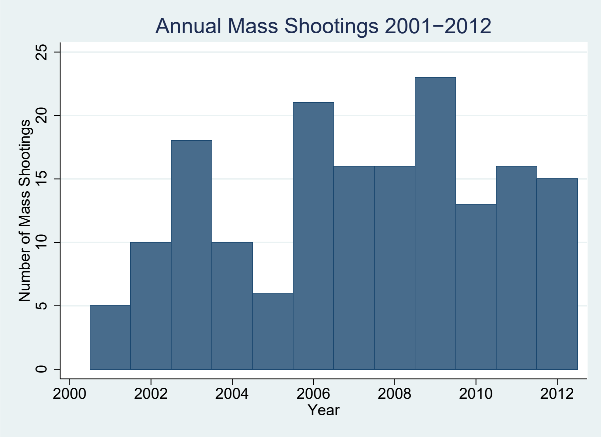
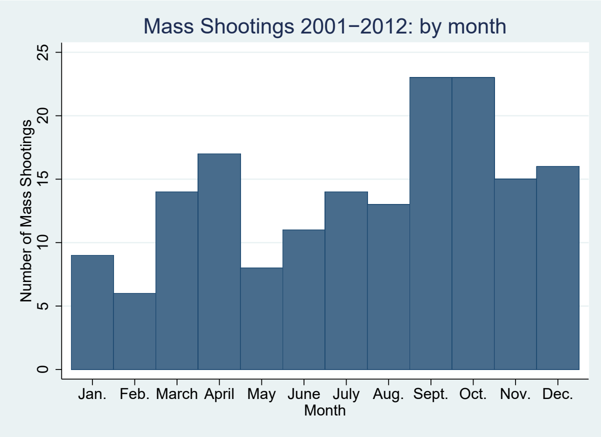
Figure 1: Mass Shootings by year and month



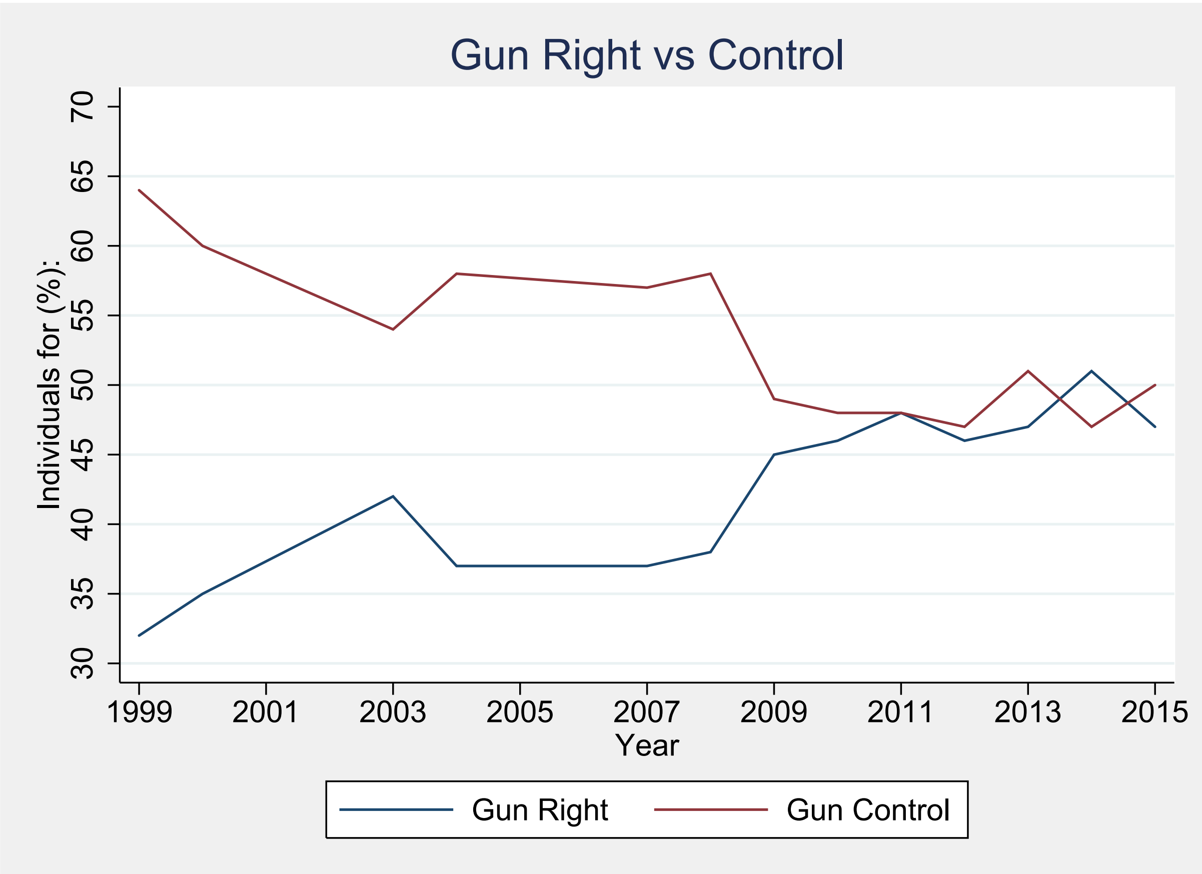
(a) Mass Shootings each year



(b) Mass Shootings each month

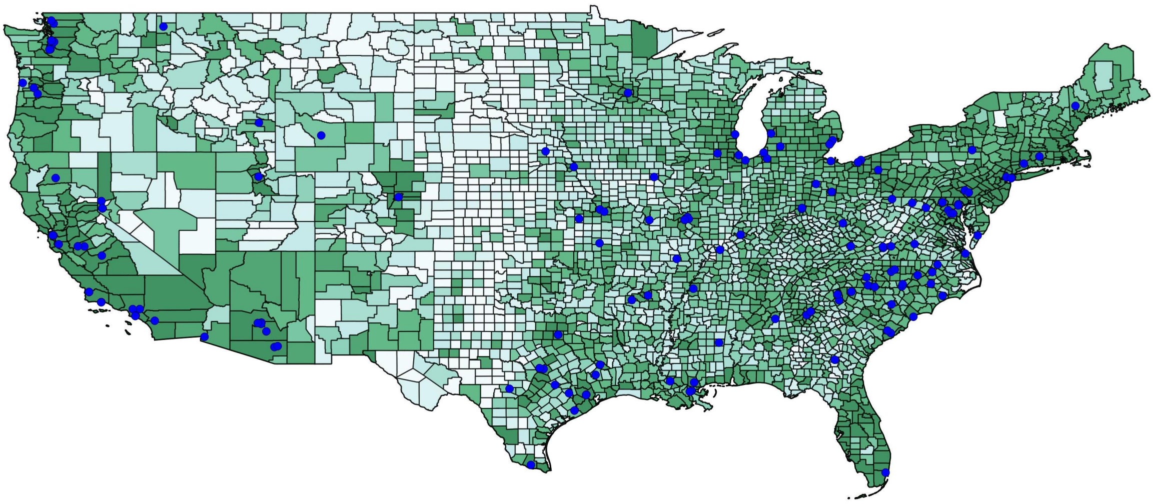
Notes: The Figure shows number of mass shootings for each year and month from 2001 to 2012.

Figure 2: Gun Right v.s. Gun Control over time



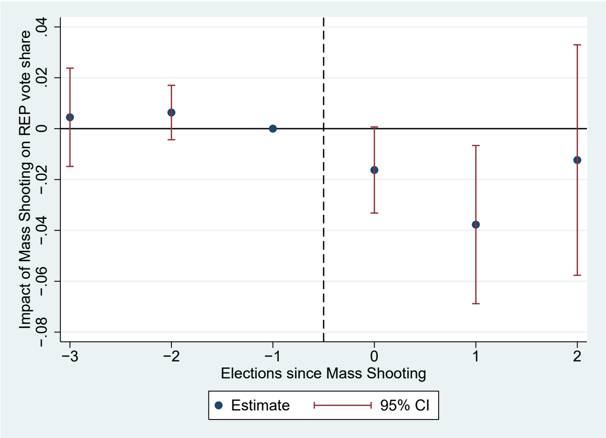
Notes: The Figure shows number of individuals supporting gun rights and gun control since 1999. These numbers are taken from PEW U.S. Politics & Policy (2016).

Figure 3: Location of Mass Shootings with County Population

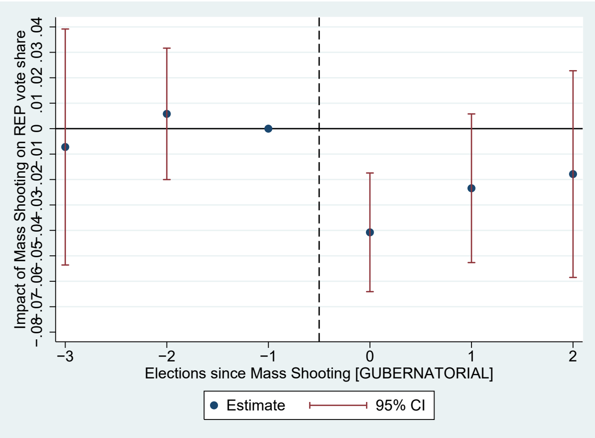


Notes: The Figure shows location of mass shootings from 2001 to 2012 along with county population. Each blue dot represents a mass shooting. The counties are shaded according to their population in 2000, with darker color signifying higher population.

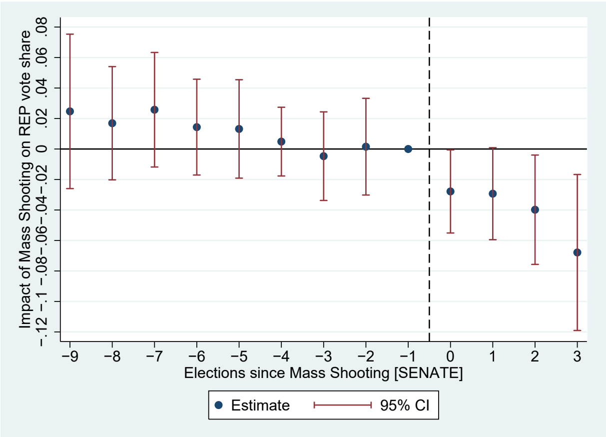
Figure 4: Testing Parallel Trends



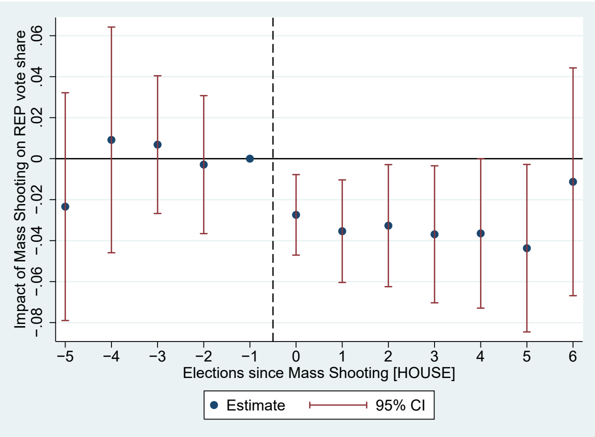
(a) Presidential Elections



(b) Gubernatorial Elections



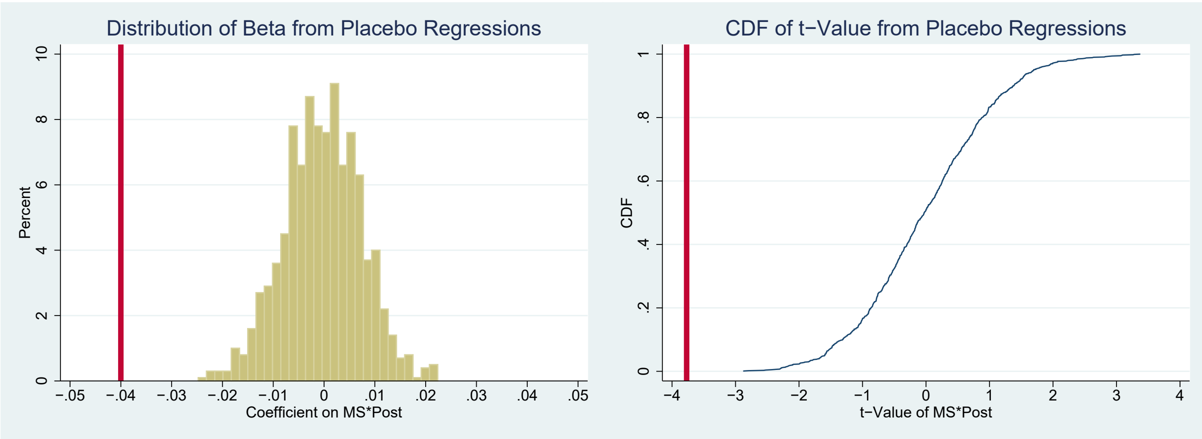
(c) Senatorial Elections



(d) House Elections

Notes: The figure plots the estimated coefficients and 95% confidence intervals from event study around mass shootings (Equation 2) for Presidential (Panel A), Gubernatorial (Panel B), Senatorial (Panel C) and House (Panel D) elections. $t = 0$ denotes the election immediately after mass shootings, while $t < 0 (t > 0)$ represent elections before (after) mass shootings.

Figure 5: Distribution from Falsification Estimates of:

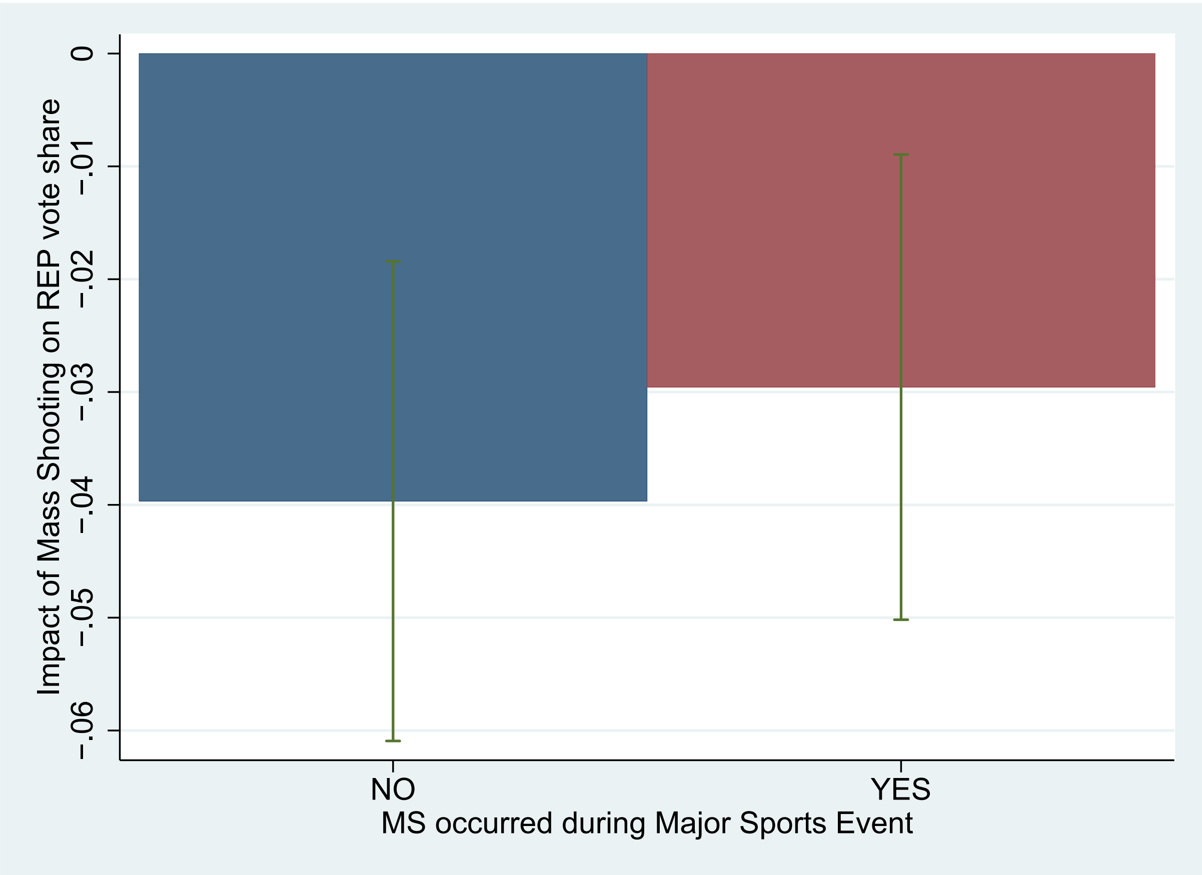


(a) Estimate

(b) t-value

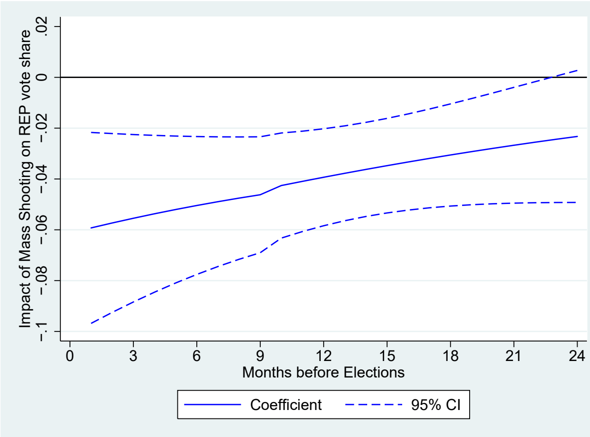
Notes: The figure plots the distribution of estimated coefficients and its corresponding t-value by randomly assigning "fake" mass shootings to counties.

Figure 6: Impact of Media Coverage of Mass Shootings

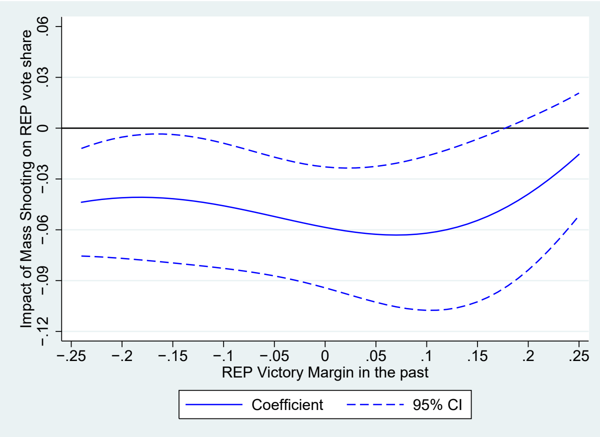


Notes: The figure plots the estimated coefficient and 95% confidence interval for the differential impact of mass shootings depending on the media coverage. I compare the effect of mass shootings during times with news pressure from other events (during Super Bowl, FIFA World Cup and Olympics) and during other times to quantify the role of media in determining the electoral outcomes.

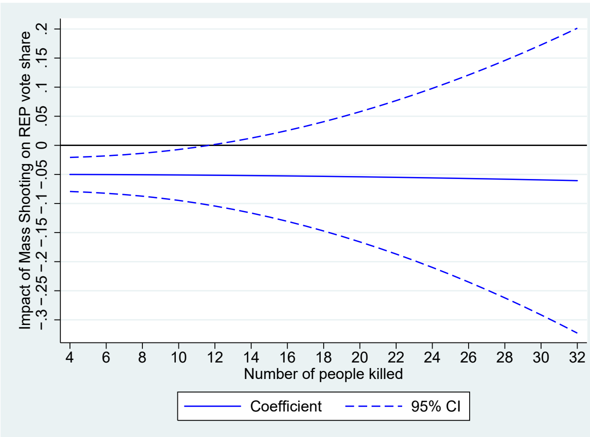
Figure 7: Heterogeneous Impact of Mass Shootings



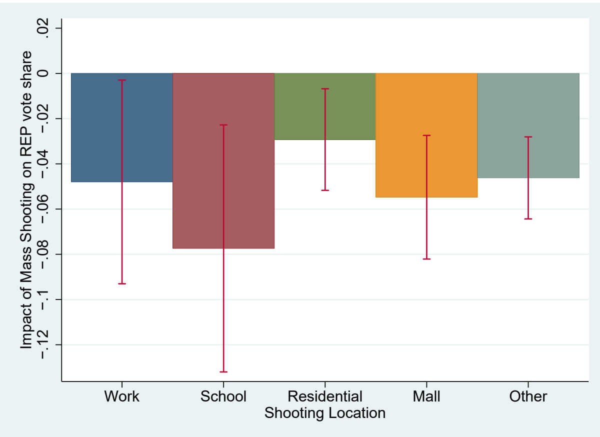
(a) Timing of Mass Shootings



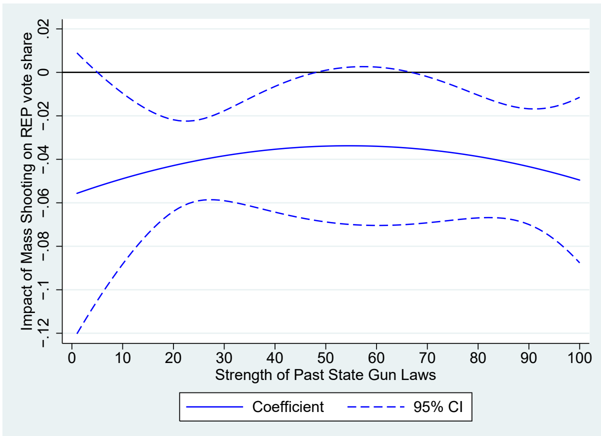
(b) County Political Preferences



(c) Number of deaths



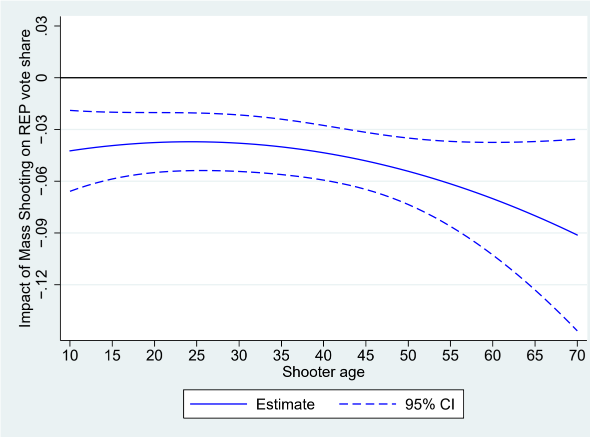
(d) Location of shootings



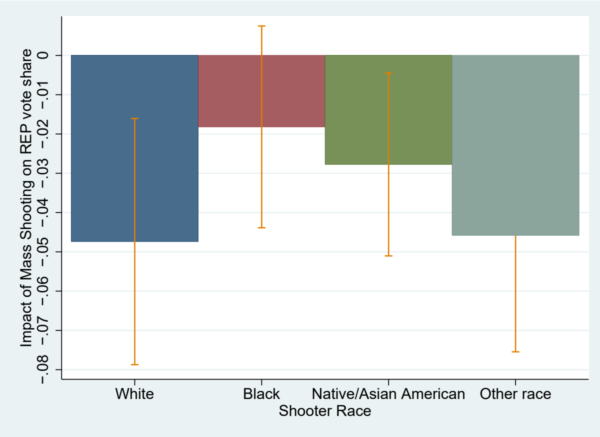
(e) State Gun Laws

Notes: The figure plots the estimated coefficient and 95% confidence interval for different shooting characteristics. Panel A plots the differential impact of mass shootings depending on how many months before elections they occur. Panel B plots how impact of mass shootings depends on local political preferences (Republican vote share in 2000). Panel C shows how number of deaths in the event influences Republican vote share differentially. Panel D plots the heterogeneous impact of shootings depending on where they occur. Finally, Panel E shows how the impact of mass shootings varies depending on local gun laws.

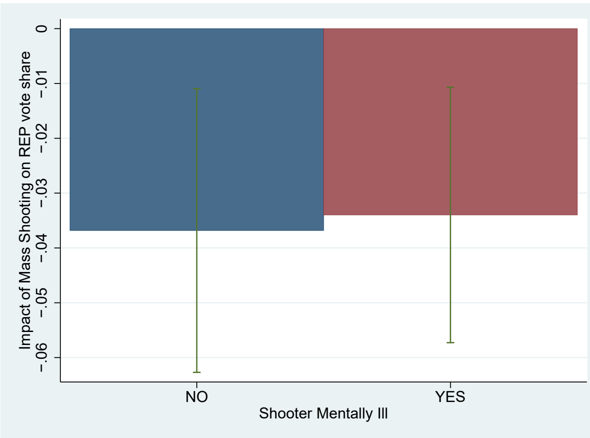
Figure 8: Heterogeneous Impact of Mass Shootings w.r.t. Shooter Characteristics



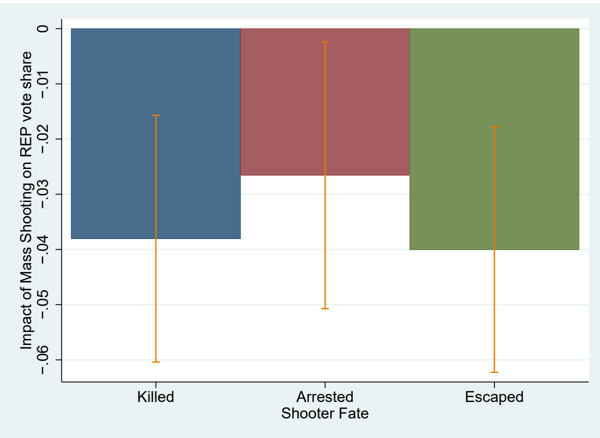
(a) Shooter's age



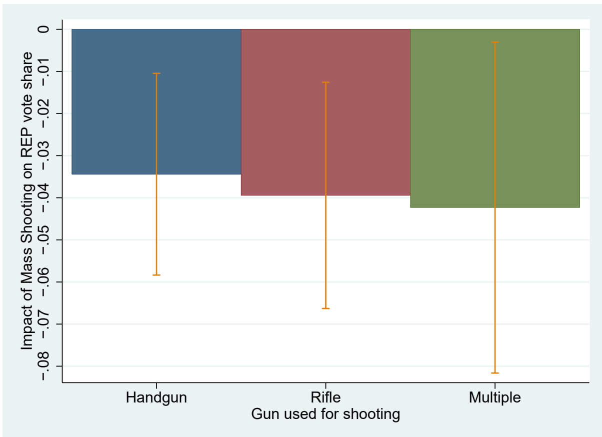
(b) Shooter's race



(c) Shooter's mental condition



(d) Shooter's fate



(e) Type of gun used by shooter

Notes: The figure plots the estimated coefficient and 95% confidence interval for different shooter characteristics. Panel A plots the differential impact of mass shootings depending on shooter's age. Panel B plots how impact of mass shootings depends on shooter's race. Panel C shows how shooter's mental condition influences Republican vote share differentially. Panel D plots the heterogeneous impact of shootings depending on shooter's fate. Finally, Panel E shows how the impact of mass shootings varies depending on type of gun used in the shootings.

Table 1: Descriptive Statistics

	Mean	SD	Min	Median	Max
Panel A: Political Variables					
Turnout	0.532	0.111	0	0.532	1
Republican Vote Share	0.565	0.129	0	0.569	0.925
Log(Campaign Contributions)	9.315	2.525	0	9.330	17.248
Republican Contributions Share	0.712	0.270	0	0.786	1.000
Contributions by NRA	1.001	3.630	0	0	10.873
Panel B: Demographic Variables					
Urban	0.496	0.500	0	0	1
Male	0.496	0.020	0.426	0.492	0.673
White	0.846	0.164	0.045	0.912	0.997
Black	0.086	0.143	0	0.017	0.865
Hispanic	0.062	0.120	0.001	0.018	0.975
Educ < High School	0.135	0.047	0.022	0.133	0.503
High School educated	0.347	0.065	0.109	0.348	0.532
College educated	0.110	0.049	0	0.100	0.400
Some college	0.205	0.044	0.087	0.204	0.373
High School and above	0.774	0.088	0.347	0.792	0.968
College and above	0.222	0.086	0.073	0.206	0.677
Never married	0.224	0.056	0.090	0.212	0.561
Separated	0.018	0.010	0	0.016	0.102
Divorced	0.095	0.019	0	0.095	0.191
Married	0.586	0.058	0.197	0.597	0.876
Veterans	0.064	0.030	0	0.058	0.397
Pop >18 years	0.745	0.033	0.534	0.747	0.980
Pop >65 years	0.148	0.042	0.018	0.144	0.347
Pop 5 to 14 years	0.145	0.019	0.020	0.144	0.287
Pop 15 to 19 years	0.076	0.013	0	0.074	0.245
Pop 20 to 24 years	0.060	0.025	0.014	0.056	0.292
Pop 25 to 34 years	0.121	0.022	0.030	0.122	0.257
Pop 35 to 44 years	0.153	0.016	0.068	0.152	0.256
Pop 45 to 54 years	0.136	0.015	0.035	0.136	0.276
Pop 55 to 64 years	0.052	0.008	0.010	0.052	0.129
Panel C: Economic Variables					
log(Median Income)	10.445	0.236	9.141	10.427	11.326
Labor Force	0.606	0.071	0.226	0.613	0.861
Employed	0.572	0.075	0.209	0.578	0.836
Unemployed	0.034	0.016	0	0.032	0.329
Poverty	0.137	0.063	0	0.126	0.567
Youth Poverty	0.073	0.033	0	0.067	0.280
Panel D: Crime Variables					
Violent Crimes	2329.767	1745.804	0	2128.709	22008
Rapes	20.733	23.002	0	15.598	284.360
Robberies	34.273	63.789	0	12.010	1383.929
Aggravated Assaults	189.758	224.923	0	126.858	4866.071
Burglary	485.524	384.694	0	431.259	3928.571
Larceny	1454.771	1128.034	0	1310.287	11428
Arson	15.240	22.132	0	8.623	349.803
Panel E: Gun Variables					
Homicides by Gun	1.765	2.775	0	0	25.279
Homicides by Other	0.843	1.433	0	0	18.299
Suicides by Gun	8.248	5.198	0	8.328	54.054
Suicides by Other	4.149	3.660	0	4.571	56.924
Accidental deaths by Gun	0.034	0.192	0	0	2.643
Accidental deaths by Other	8.540	4.819	0	8.600	34.499
Panel F: Health Variables					
Fair of poor health (2010-16)	0.170	0.050	0.070	0.160	0.420
Poor psychical health (2010-16)	0.038	0.007	0.022	0.037	0.065
Poor mental health (2010-16)	0.037	0.006	0.021	0.037	0.056
Heavy Drinkers (2010-16)	0.166	0.034	0.080	0.170	0.270

Notes: The table shows summary statistics for main variables used in the paper. All the data is at the county level. Panel A represents the summary statistics for main dependent variables. Panels B, C, D, E and F report the summary statistics for the demographic, economic, crime, gun and health variables. All variables are election cycle level from 2000 to 2012. The health variables used are from 2010 onwards.

Table 2: Comparing Variables

	Levels		Trends	
	Difference	SE	Difference	SE
Panel A: Political Variables				
Turnout	-0.008	(0.012)	-0.001	(0.005)
Republican Vote Share	-0.019	(0.012)	-0.004	(0.005)
Log(Campaign Contributions)	0.392	(0.248)	-0.035	(0.164)
Republican Contributions Share	-0.012	(0.027)	-0.022	(0.029)
Contributions by NRA	0.279	(0.338)	-0.167	(0.309)
Panel B: Demographic Variables				
Urban	0.079	(0.049)	-0.010	(0.022)
Male	-0.003	(0.002)	-0.002	(0.001)
White	-0.025	(0.017)	-0.001	(0.004)
Black	0.019	(0.015)	0.001	(0.002)
Hispanic	0.008	(0.012)	0.003	(0.002)
Educ < High School	0.002	(0.005)	-0.002	(0.002)
High School educated	-0.016**	(0.006)	-0.001	(0.003)
College educated	0.001	(0.005)	-0.001	(0.002)
Some college	0.007*	(0.004)	0.001	(0.002)
High School and above	0.002	(0.009)	-0.001	(0.003)
College and above	0.005	(0.008)	-0.001	(0.003)
Never married	0.009*	(0.005)	0.001	(0.003)
Separated	0.002	(0.001)	0.000	(0.001)
Divorced	0.003	(0.002)	-0.001	(0.002)
Married	-0.011*	(0.006)	0.000	(0.004)
Veterans	0.001	(0.003)	-0.001	(0.003)
Pop >18 years	0.001	(0.003)	-0.003*	(0.002)
Pop >65 years	-0.008*	(0.004)	-0.001	(0.002)
Pop 5 to 14 years	-0.001	(0.002)	0.001	(0.001)
Pop 15 to 19 years	0.000	(0.001)	0.001	(0.001)
Pop 20 to 24 years	0.004	(0.003)	0.000	(0.001)
Pop 25 to 34 years	0.003	(0.002)	0.002	(0.002)
Pop 35 to 44 years	-0.001	(0.002)	0.001	(0.001)
Pop 45 to 54 years	-0.002	(0.002)	-0.003**	(0.001)
Pop 55 to 64 years	-0.001	(0.001)	0.000	(0.001)
Panel C: Economic Variables				
log(Median Income)	-0.004	(0.023)	-0.014	(0.011)
Labor Force	0.004	(0.007)	0.001	(0.004)
Employed	0.001	(0.007)	0.000	(0.004)
Unemployed	0.002	(0.002)	0.000	(0.003)
Poverty	0.006	(0.006)	-0.007	(0.006)
Youth Poverty	0.004	(0.003)	-0.004	(0.003)
Panel D: Crime Variables				
Violent Crimes	299.534	(210.451)	-36.169	(151.083)
Rapes	5.016*	(2.875)	-0.962	(3.454)
Robberies	4.571	(6.935)	2.131	(4.89)
Aggravated Assaults	6.524	(28.076)	-17.333	(24.943)
Burglary	88.123*	(47.825)	7.113	(41.504)
Larceny	156.992	(137.242)	-30.516	(97.908)
Arson	4.128	(2.74)	-1.230	(3.082)
Panel E: Gun Variables				
Homicides by Gun	0.403	(0.307)	-0.036	(0.221)
Homicides by Other	0.176	(0.157)	0.086	(0.109)
Suicides by Gun	0.529	(0.578)	-0.450	(0.677)
Suicides by Other	0.318	(0.41)	0.052	(0.434)
Accidental deaths by Gun	0.032	(0.021)	0.006	(0.011)
Accidental deaths by Other	-0.417	(0.534)	0.229	(0.692)
Panel F: Health Variables				
Fair of poor health (2010-16)	0.006	(0.005)	0.002	(0.004)
Poor psychical health (2010-16)	0.001	(0.001)	0.000	(0.001)
Poor mental health (2010-16)	0.001	(0.001)	0.000	(0.001)
Heavy Drinkers (2010-16)	-0.004	(0.003)	-0.004	(0.004)

Notes: The table shows the estimates and standard errors of ρ_1 from OLS estimation of equations 3 (Columns 1 and 2) and 4 (Columns 3 and 4). The dependent variable is each variable mentioned in the first column. The main independent variable is mass shootings indicator MSE_i. All the results are conditional on county population. The standard errors are clustered at congressional district level. * indicates significance at 10% significance level, ** indicates significance at 5% significance level, while *** indicates significance at 1% significance level.

Table 3: Predicting Mass Shooting

VARIABLES	(1) Mass Shooting	(2) Mass Shooting	(3) Mass Shooting	(4) Mass Shooting	(5) Mass Shooting	(6) Mass Shooting	(7) Mass Shooting
REP Share t-1	0.001	0.030	0.004	-0.003	0.001	0.018	0.019
	(0.030)	(0.033)	(0.030)	(0.030)	(0.030)	(0.034)	(0.037)
Turnout t-1	0.025	0.024	0.018	0.024	0.020	0.029	0.021
	(0.038)	(0.037)	(0.038)	(0.037)	(0.039)	(0.038)	(0.039)
Observations	9,364	9,364	9,364	9,364	9,364	9,364	9,364
R-squared	0.000	0.003	0.002	0.003	0.002	0.001	0.008
Number of fips	3,135	3,135	3,135	3,135	3,135	3,135	3,135
Controls	None	Demographic	Economic	Gun	Crime	Health	All
F-Test: Electoral variables = 0	0.233	0.593	0.122	0.203	0.134	0.420	0.269
F-Test: Demographic variables = 0	–	1.109	–	–	–	–	1.062
F-Test: Economic variables = 0	–	–	1.560	–	–	–	1.173
F-Test: Gun variables = 0	–	–	–	0.586	–	–	0.503
F-Test: Crime variables = 0	–	–	–	–	0.648	–	0.661
F-Test: Health variables = 0	–	–	–	–	–	0.879	0.628

Notes: The table shows the results from OLS estimation of equation 5. The dependent variable is MS_{it}. The main independent variables are lagged Republican vote share and lagged voter turnout. The first column shows unconditional estimates. The local characteristics are added sequentially in the estimation. Each category of characteristics include variables as shown in Table 1. The standard errors are clustered at congressional district level. * indicates significance at 10% significance level, ** indicates significance at 5% significance level, while *** indicates significance at 1% significance level.

Table 4: Effect of Mass Shootings on Presidential REP Share

VARIABLES	(1) REP	(2) REP	(3) REP	(4) REP	(5) REP	(6) REP	(7) REP	(8) REP
MS * Post	-0.044***	-0.043***	-0.042***	-0.042***	-0.040***	-0.045***	-0.040***	-0.036***
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Observations	15,658	15,658	15,658	15,657	15,654	15,653	15,658	15,649
R-squared	0.378	0.406	0.383	0.380	0.382	0.388	0.410	0.424
Controls	None	Demographic	Economic	Gun	Crime	Health	Dmg & Ecn	All

Notes: The table shows the results from OLS estimation of equation 1. The dependent variable is county Republican vote share in Presidential elections. The main independent variable is indicator of mass shootings interacted with dummy equal to one for after mass shootings. In all the estimates, I include county and year fixed effects. The first column shows unconditional estimates. The local characteristics are added sequentially in the estimation. Demographic characteristics include: urban dummy, proportion of population that is male, white, black, Hispanic, has completed high school, has completed college, married, greater than 18 years old and greater than 65 years old. Economic characteristics include: natural log of income, unemployment rate and poverty rate. Gun variables include: homicide rate by guns, homicide rate by other means, suicides by gun and suicides by other means. Crime and health variables include violent crime rate and mental health measure and excessive drinking measures respectively. The standard errors are clustered at congressional district level. * indicates significance at 10% significance level, ** indicates significance at 5% significance level, while *** indicates significance at 1% significance level.

Table 5: Effect of Mass Shootings on Other Federal Elections REP Share

VARIABLES	(1) REP	(2) REP	(3) REP	(4) REP	(5) REP	(6) REP	(7) REP	(8) REP	(9) REP
MS * Post	-0.039***	-0.037***	-0.040***	-0.033***	-0.027***	-0.026***	-0.024**	-0.027***	-0.028***
	(0.010)	(0.009)	(0.010)	(0.010)	(0.009)	(0.009)	(0.010)	(0.010)	(0.011)
Observations	12,729	12,729	12,729	18,993	18,993	18,993	2,737	2,737	2,347
R-squared	0.143	0.153	0.162	0.131	0.143	0.145	0.084	0.177	0.171
Election	GOV	GOV	GOV	SEN	SEN	SEN	HOUSE	HOUSE	HOUSE
Controls	None	Pref	All	None	Pref	All	None	Pref	All

Notes: The table shows the results from OLS estimation of equation 1 using Gubernatorial, Senatorial and House elections data. The dependent variable is county Republican vote share in each federal election. The main independent variable is indicator of mass shootings interacted with dummy equal to one for after mass shootings. In all the estimates, I include area and year fixed effects. Columns 1, 4 and 7 show unconditional estimates. Columns 2, 5 and 8 include preferred set of controls in the estimation, while Columns 3, 6 and 9 include all the local characteristics as controls. The preferred controls contain demographic and economic characteristics. Demographic characteristics include: urban dummy, proportion of population that is male, white, black, Hispanic, has completed high school, has completed college, married, greater than 18 years old and greater than 65 years old. Economic characteristics include: natural log of income, unemployment rate and poverty rate. Gun variables include: homicide rate by guns, homicide rate by other means, suicides by gun and suicides by other means. Crime and health variables include violent crime rate and mental health measure and excessive drinking measures respectively. The standard errors are clustered at congressional district level. * indicates significance at 10% significance level, ** indicates significance at 5% significance level, while *** indicates significance at 1% significance level.

Table 6: Results from Alternate Identification

VARIABLES	(1) REP	(2) REP	(3) REP	(4) REP	(5) REP	(6) REP	(7) REP	(8) REP	(9) REP	(10) REP	(11) REP	(12) REP
MS * Post	-0.035***	-0.027***	-0.030***	-0.046***	-0.026***	-0.027***	-0.036***	-0.024***	-0.020**	-0.033***	-0.023**	-0.026***
	(0.009)	(0.009)	(0.008)	(0.010)	(0.008)	(0.008)	(0.010)	(0.008)	(0.009)	(0.010)	(0.012)	(0.009)
Observations	3,255	2,655	3,940	5,622	4,570	6,793	2,494	2,030	2,987	935	747	1,093
R-squared	0.317	0.179	0.147	0.176	0.169	0.138	0.130	0.173	0.037	0.270	0.209	0.143
Identification	Contiguous	Contiguous	Contiguous	Matching	Matching	Matching	Past MS	Past MS	Past MS	Fail MS	Fail MS	Fail MS
Election	PRES	GOV	SEN	PRES	GOV	SEN	PRES	GOV	SEN	PRES	GOV	SEN

Notes: The table shows the results from OLS estimation of equation 1 using four different identification strategies. The dependent variable is county Republican vote share in each federal election. The main independent variable is indicator of mass shootings interacted with dummy equal to one for after mass shootings. In all the estimates, I include county and year fixed effects. Columns 1 to 3 use sample of counties with mass shootings along with its contiguous counties to estimate the impact of mass shootings. Columns 4 to 6 use sample of counties with mass shootings matched with 5 most similar counties on population and all local characteristics. Columns 7 to 9 use sample of counties with mass shootings today and mass shootings in the past. Finally, Columns 10 to 12 use sample of counties with "successfull" and "failed" mass shootings. All estimations control for demographic and economic characteristics. Demographic characteristics include: urban dummy, proportion of population that is male, white, black, Hispanic, has completed high school, has completed college, married, greater than 18 years old and greater than 65 years old. Economic characteristics include: natural log of income, unemployment rate and poverty rate. The standard errors are clustered at congressional district level. * indicates significance at 10% significance level, ** indicates significance at 5% significance level, while *** indicates significance at 1% significance level.

Table 7: Placebo Estimates

VARIABLES	(1) REP t-1	(2) REP t-2	(3) REP	(4) REP	(5) REP
MS * Post	-0.006	0.015			
	(0.007)	(0.022)			
Family MS * Post			-0.010		
			(0.019)		
Failed MS * Post				0.005	
				(0.013)	
Shooting with 3 deaths * Post					-0.009
					(0.006)
Observations	15,010	9,384	15,655	15,655	15,635
R-squared	0.784	0.109	0.409	0.408	0.419

Notes: The table shows the results from OLS estimation of equation 1 using placebo outcomes. The dependent variable is: one period lagged Republican vote share in Column 1, two period lagged Republican vote share in Column 2, and Republican vote share in Columns 3 and 4. The independent variable is indicator of mass shootings interacted with dummy equal to one for after mass shootings in Columns 1 and 2, one period forward value of interaction of mass shootings and post (Column 3) and two period forward value of interaction of mass shootings and post (Column 4). All estimations control for demographic and economic characteristics. Demographic characteristics include: urban dummy, proportion of population that is male, white, black, Hispanic, has completed high school, has completed college, married, greater than 18 years old and greater than 65 years old. Economic characteristics include: natural log of income, unemployment rate and poverty rate. The standard errors are clustered at congressional district level. * indicates significance at 10% significance level, ** indicates significance at 5% significance level, while *** indicates significance at 1% significance level.

Table 8: Robustness Checks

Panel A: Definitions and Specifications

VARIABLES	(1) REP	(2) REP	(3) REP	(4) REP	(5) REP	(6) REP	(7) REP
MS * Post	-0.044***	-0.040***	-0.029***	-0.041***	-0.033***	-0.041***	-0.031***
	(0.011)	(0.011)	(0.007)	(0.008)	(0.008)	(0.010)	(0.010)
Observations	15,658	15,658	15,653	12,521	15,658	15,658	15,658
R-squared	0.410	0.410	0.917	0.064	0.270	0.402	0.412
Robust	MS in FBI	2 Party REP	State*Year FE	FD	Weight Pop	Weight ln Pop	Flexible controls

Panel B: Inference

VARIABLES	(1) REP	(2) REP	(3) REP	(4) REP	(5) REP	(6) REP	(7) REP
MS * Post	-0.040***	-0.041***	-0.041***	-0.040***	-0.040**	-0.040***	-0.040***
	(0.0107)	(0.0078)	(0.0077)	(0.0092)	(0.0175)	(0.0099)	(0.0114)
Observations	15,658	15,658	15,658	15,658	15,658	15,658	15,658
R-squared	0.410	0.410	0.410	0.410	0.410	0.410	0.410
SE	Baseline	AR(1)	AR(2)	HAC	Cluster at State	Block Bootstrap	Spatial SE

Notes: The table shows the results from OLS estimation of equation 1. The dependent variable is Republican vote share in Presidential elections. The independent variable is indicator of mass shootings interacted with dummy equal to one for after mass shootings. Panel A studies whether results are robust to alternate definitions and specifications. Column 1 measures the incidence of mass shootings using only the FBI data. Column 2 constructs Republican vote share as a fraction of total votes for Republicans over sum of Republican and Democrat votes only. Column 3 includes state times year fixed effects in the estimation. Column 4 estimates the equation using first difference instead of fixed effects. Columns 5 and 6 weight the regressions by population and natural log of population respectively. Column 7 includes the deciles of demographic and economic characteristics. The standard errors are clustered at congressional district level. Panel B studies whether results are robust to alternate inference methods. Column 1 shows the baseline estimates clustered at congressional district level. Columns 2 and 3 allow for residuals to have Auto-Regressive structure of order 1 and 2 respectively. Column 4 allows for arbitrary order Auto-Regressive structure among the residuals. Column 5 clusters standard errors at state level. Column 6 estimates standard error using Block Bootstrap as in Bertrand et al. (2004). Finally, Column 7 estimates standard error allowing for arbitrary spatial correlation among the residuals as in Conley (1999). All estimations control for demographic and economic characteristics. Demographic characteristics include: urban dummy, proportion of population that is male, white, black, Hispanic, has completed high school, has completed college, married, greater than 18 years old and greater than 65 years old. Economic characteristics include: natural log of income, unemployment rate and poverty rate. * indicates significance at 10% significance level, ** indicates significance at 5% significance level, while *** indicates significance at 1% significance level.

Table 9: Effect of Mass Shooting on Other Electoral Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Turnout	Incumbent	Turnout	Incumbent	Turnout	Incumbent	Turnout	Incumbent
MS * Post	0.002	-0.021	0.011***	0.043	0.013***	-0.014	0.029	-0.010
	(0.007)	(0.017)	(0.004)	(0.050)	(0.004)	(0.013)	(0.020)	(0.009)
Observations	15,658	15,658	12,747	11,792	18,993	18,986	2,737	2,406
R-squared	0.415	0.331	0.375	0.117	0.725	0.059	0.920	0.060
Election	PRES	PRES	GOV	GOV	SEN	SEN	HOUSE	HOUSE

Notes: The table shows the results from OLS estimation of equation 1. The dependent variable is voter turnout (Columns 1, 3, 5 and 7) and incumbent vote share (Columns 2, 4, 6 and 8) in federal elections. The independent variable is indicator of mass shootings interacted with dummy equal to one for after mass shootings. In all the estimates, I include county and year fixed effects. Columns 1 and 2 use data from Presidential elections, Columns 3 and 4 use data from Gubernatorial elections, Columns 5 and 6 use data from Senatorial elections and Columns 7 and 8 use data from House elections. Columns 1 to 6 include data at county level, while columns 7 and 8 use data at congressional district level. All estimations control for demographic and economic characteristics. Demographic characteristics include: urban dummy, proportion of population that is male, white, black, Hispanic, has completed high school, has completed college, married, greater than 18 years old and greater than 65 years old. Economic characteristics include: natural log of income, unemployment rate and poverty rate. The standard errors are clustered at congressional district level. * indicates significance at 10% significance level, ** indicates significance at 5% significance level, while *** indicates significance at 1% significance level.

Table 10: Effect of Mass Shooting on Campaign Contributions

VARIABLES	(1) Total	(2) To REP	(3) To DEM	(4) Share to REP	(5) By NRA	(6) By NRA to REP	(7) By NRA to DEM	(8) By Other	(9) By Other to REP	(10) By Other to DEM
MS * Post	0.008	-0.151***	-0.071	-0.044***	0.177**	0.159**	-0.038	0.391	0.272	0.481
	(0.045)	(0.048)	(0.088)	(0.013)	(0.086)	(0.075)	(0.276)	(0.347)	(0.414)	(0.376)
Observations	27,753	27,753	27,753	27,479	27,957	27,957	27,957	27,957	27,957	27,957
R-squared	0.285	0.206	0.161	0.026	0.887	0.912	0.380	0.004	0.010	0.003
Controls	All	All	All	All	All	All	All	All	All	All

Notes: The table shows the results from OLS estimation of equation 1 using campaign contributions data. In all the estimates, I include county and year fixed effects. The independent variable is indicator of mass shootings interacted with dummy equal to one for after mass shootings. The dependent variables are natural log of total individual contributions (Column 1), natural log of total individual contributions to Republicans (Column 2), natural log of total individual contributions to Democrats (Column 3), proportion of total individual contributions to Republican candidates (Column 4), natural log of total contributions by NRA (Column 5), natural log of total contributions by NRA to Republicans (Column 6), natural log of total contributions by NRA to Democrats (Column 7), natural log of total contributions by other PACs (Column 8), natural log of total contributions by other PACs to Republicans (Column 9) and natural log of total contributions by other PACs to Democrats (Column 10). All estimations control for demographic and economic characteristics. Demographic characteristics include: urban dummy, proportion of population that is male, white, black, Hispanic, has completed high school, has completed college, married, greater than 18 years old and greater than 65 years old. Economic characteristics include: natural log of income, unemployment rate and poverty rate. The standard errors are clustered at congressional district level. * indicates significance at 10% significance level, ** indicates significance at 5% significance level, while *** indicates significance at 1% significance level.

Table 11: Effect of Mass Shooting on Voter Preferences

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Imp. Gun	GC Inc.	GC Dec.	Imp. Gun	GC Inc.	GC Dec.	Imp. Gun	GC Inc.	GC Dec.	Imp. Gun	GC Inc.	GC Dec.
MS * Post	0.085***	0.005	0.000	0.077**	-0.038	0.021**	0.154***	0.042**	-0.011	0.055**	0.002	0.002
	(0.019)	(0.014)	(0.005)	(0.034)	(0.038)	(0.010)	(0.029)	(0.017)	(0.013)	(0.025)	(0.019)	(0.005)
Observations	9,681	9,705	9,705	2,184	2,188	2,188	3,822	3,829	3,829	3,675	3,688	3,688
R-squared	0.193	0.183	0.101	0.340	0.274	0.295	0.307	0.245	0.214	0.256	0.227	0.238
Voters	All	All	All	REP	REP	REP	DEM	DEM	DEM	IND	IND	IND
Mean	2.743	0.515	0.0468	2.912	0.317	0.0738	2.672	0.656	0.0204	2.715	0.486	0.0580

Notes: The table shows the results from OLS estimation of equation 7 using data from American National Election Studies. In all the estimates, I include congressional district and year fixed effects. The independent variable: $MS * Post$ is indicator of mass shootings interacted with dummy equal to one for after mass shootings. The dependent variables are measure of importance of gun control (Columns 1, 4, 7 and 10), indicator equal to one if the respondent says gun control should increase (Columns 2, 5, 8 and 11) and indicator equal to one if the respondent says gun control should decrease (Column 3, 6, 9 and 12). Columns 1 to 3 use data from all respondents. Columns 4 to 6 use data on respondents who identify themselves as Republicans, Columns 7 to 9 use data on respondents who identify themselves as Democrats and Columns 10 to 12 uses data on respondents who identify themselves as Independents. All estimations include congressional district fixed effects and individual characteristics: race, income, age, education, marital status, political leaning and religiosity. The standard errors are clustered at congressional district level. * indicates significance at 10% significance level, ** indicates significance at 5% significance level, while *** indicates significance at 1% significance level.

Table 12: Effect of Mass Shooting on Policy Making

VARIABLES	(1) Gun Ideo	(2) Gun Ideo	(3) Less Gun Cntrl	(4) More Gun Cntrl	(5) Gun Ideo	(6) Gun Ideo	(7) Gun Ideo	(8) Gun Ideo
MS * Post	0.030	0.070**	0.168***	-0.129***	0.049**	0.098**	0.052	0.245**
	(0.023)	(0.035)	(0.046)	(0.034)	(0.022)	(0.049)	(0.034)	(0.119)
MS * Post * DEM		-0.082**	-0.263***	0.163***	-0.051*	-0.122**	-0.063*	-0.277**
		(0.033)	(0.056)	(0.057)	(0.031)	(0.050)	(0.033)	(0.112)
MS * Post * NRA					0.158**			
					(0.072)			
MS * Post * DEM * NRA					-0.203**			
					(0.087)			
MS * Post * Swing						-0.060		
						(0.072)		
MS * Post * DEM * Swing						0.084		
						(0.075)		
Observations	2,396	2,396	2,396	2,396	2,310	2,396	2,075	321
R-squared	0.453	0.455	0.288	0.136	0.452	0.456	0.447	0.446

Notes: The table shows the results from OLS estimation of equation 8 (Column 1) and 9 (remaining columns) using roll-call voting data. In all the estimates, I include congressional district and year fixed effects. The independent variable: $MS * Post$ is indicator of mass shootings interacted with dummy equal to one for after mass shootings. The variable DEM equals one for districts represented by Democrats. The variable NRA equals to one if NRA gave political contributions to candidates in the district in previous election. The variable $Swing$ equals to one for closely contested districts. The dependent variables are DW-Nominate score of politician in Columns 1, 2, 5, 6, 7 and 8. The dependent variable is an indicator equal to one if roll-call voting moves towards less gun control (Column 3) and is equal to one if roll-call voting moves towards more gun control (Column 4). Column 7 uses data on districts in which there is no change in incumbent during the sample period, while Column 8 uses data on districts in which there is change in incumbent during the sample period. All estimations control for demographic and economic characteristics. Demographic characteristics include: urban dummy, proportion of population that is male, white, black, Hispanic, has completed high school, has completed college, married, greater than 18 years old and greater than 65 years old. Economic characteristics include: natural log of income, unemployment rate and poverty rate. The standard errors are clustered at congressional district level. * indicates significance at 10% significance level, ** indicates significance at 5% significance level, while *** indicates significance at 1% significance level.

Sticking to one's gun: Mass Shootings and the Political Economy of Gun Control in the U.S.

Online Appendix

7.1 Predicting Mass Shootings flexibly

In my main analysis, I study whether linear functional form of local characteristics can predict mass shootings. One may be concerned that local characteristics may predict mass shootings non-linearly. For instance, we may think that counties with and without mass shootings are similar on mean level of crime, but counties which have extreme crime rate (highest decile) are much more likely to have mass shootings. In order to test if there is non-linear relation between local characteristics and mass shootings, I estimate Equation 5 using deciles of variables and test if they jointly predict mass shootings. In particular, I estimate the following:

$$MS_{it} = \alpha_i + \alpha_t + \beta_1 REP_{it-1} + \beta_2 Turnout_{it-1} + \sum_{d=1}^{9} X_{it}^{\prime d} \Pi_d + \epsilon_{it}, \tag{10}$$

where X_{it} are deciles of each of the local characteristics. x_{it}^d represents the *d-th* decile of variable x_{it}. The F-Test on these deciles would tell us whether counties that have different deciles of demographic, population, crime, health and gun related variables have systematically different likelihood of mass shootings.

Table A3 shows the results. As in the main section, I include each group of variables individually before adding them together in the last column. We see that none of the joint F-Test is statistically significant at even 10% significance level. The magnitude of the joint F-Tests for each group are very similar to the ones obtained in the main section. This suggests that extreme values of local characteristics do not systematically predict when and where mass shootings occur.

7.2 Spillovers

An interesting question that arises from analysis in the main section is whether there are any spillover effects of mass shootings on electoral outcomes. That is, are counties whose neighbor has mass shootings affected? If so, then what is the extent of spillover?. In this section, I study whether there are any spillovers of mass shootings on voting outcomes.

Specifically, I study if there are mass shootings within certain distance from the county (outside the county), does it have any impact on Republican vote share. Since, we do not know the extent of spillovers, I try different bandwidths to access the scope of spillovers. In particular, I estimate the following:

$$repshare_{it} = \alpha_i + \alpha_t + \beta(MSE_i * Post_t) + \beta(MSE_i^n * Post_t^n) + X_{it}'\Gamma + u_{it}, \qquad (11)$$

where MSE_i^n equals to one if there is mass shootings within different geographic definitions around county i. $Post_t^n$ is indicator equal to one for counties whose "neighbor" county has mass shootings after it has the shootings and is zero otherwise.

Table A2 shows the results. We see that there is very little evidence of spillovers. The spillover if mass shootings takes place within 25 km from county results in 2.3 percentage points loss in Republican vote share (statistically insignificant) relative to other counties.[53] However, if we consider any distance greater than 25km, there is limited evidence of any spillovers. In addition, mass shootings within a congressional district, city (MSA), commuting zone or state do not yield economically or statistically significant spillovers to other counties within the same geography. Finally, distance from closest mass shootings does not matter for electoral impacts (Column 9).

The limited spillovers of impact of mass shootings on electoral outcomes may be due to coverage of mass shootings. Anecdotal evidence suggests that most mass shootings receive either local attention (local newspapers) or deadliest mass shootings receive wide spread attention (national newspapers). Then, mass shootings that receive attention in local newspaper will have little spillovers because they did not get extensive coverage outside the county where they took place. Counties are reasonable approximation of news market because median newspaper sells more than 80% of its copies in the county where it is headquartered (Gentzkow and Shapiro, 2010). This will explain why we find limited evidence of spillovers. On the other hand, mass shootings that receive national attention

[53]The average distance of county to its closest county is 38.3 km. There are only 512 counties with another county within 25 km.

implies that counties that are located far away also receive news about these shootings. This potentially makes spillovers less correlated on geographic distance.

7.3 Further Robustness

Does the impact of mass shootings on Republican vote share depends on when it took place? I allow for different impact of mass shootings on Republican vote share depending on the year it took place. Figure A3 plots the impact of each individual year's mass shootings on Republican vote share in the Presidential elections. We see that regardless of the year, mass shootings result in loss for Republicans. 9 out of 12 individual year's mass shootings result in loss greater than 3 percentage points in Republican vote share. We see that the mass shootings closer to or during election year produce stronger impact on Republican vote share. Moreover, mass shootings in 2007, 2009 and 2011 produce greatest loss in Republican vote share because these were the years when some of the deadliest mass shootings took place (Virginia Tech (VA); Fort Hood (TX) and Seal Beach (CA) respectively).

Are the results driven by mass shootings in some particular state? In order to see how results depend on each state, I estimate my Equation 1 dropping each state at a time. Figure A4 plots the estimates of β dropping one state at a time. We see that the results are unchanged if we drop any state, suggesting that the results are not driven by a particular state. The results are centered around 4 percentage points (average effect). The magnitude decreases the most, to 3.87 percentage points if we drop Georgia, while the magnitude increases the most 4.5 percentage points if we drop Virginia from our estimation.

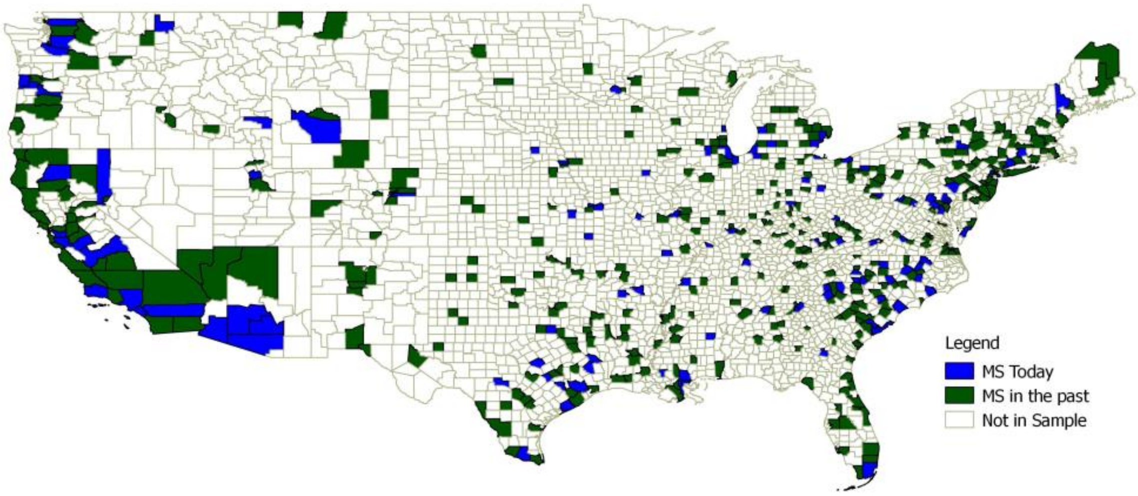
7.4 Robustness of Mechanisms

In Section 6.2.1, I use current political affiliation to estimate how the Republicans and Democrats behave after mass shootings. These results may reflect composition effect.

That is, affiliation for a political party may change as a result of mass shootings itself. In practice, this may not be a big concern because party affiliation is very persistent and stable (Kaplan and Mukand, 2011). In order to address this concern, I use past voting behavior as measure of Republican or Democrat. This measure is a noisier measure of party affiliation because independents who lean towards one political party are included in this definition. One short coming of using past voting behavior is that we cannot study how independent voters react to mass shootings.

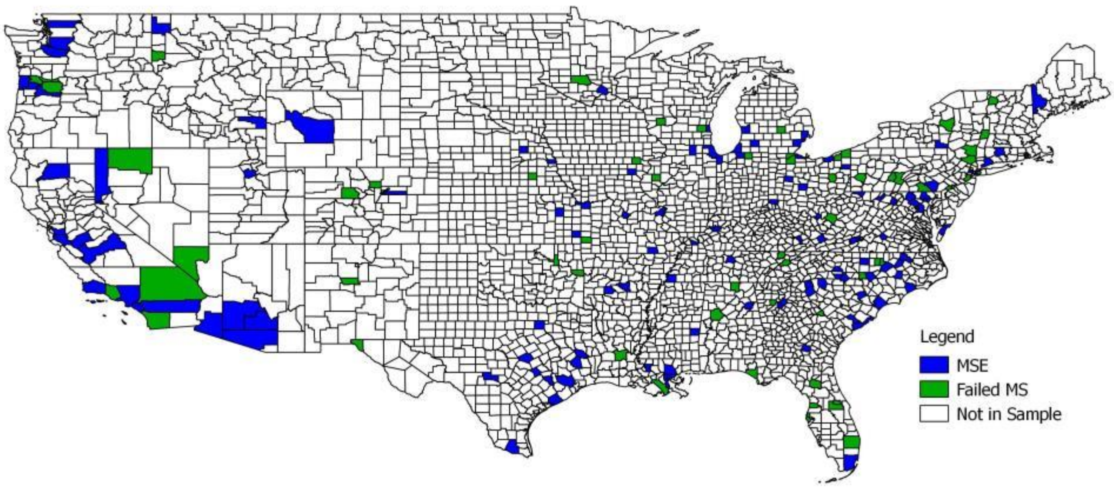
Table A4 shows the results. We see that the results are similar to the ones obtained using current political affiliation. We see a clear increase in importance of gun policy among both Republicans and Democrats (Columns 1 and 4). Column 3 shows that respondents who voted for Republicans in the past are much more likely to ask for decrease in gun control in districts with mass shootings relative to other districts. Similarly, Column 5 shows that respondents who voted for Democrats in the past are much more likely to ask for increase in gun control in districts with mass shootings relative to other districts.

Figure A1: Past and Current Mass Shootings



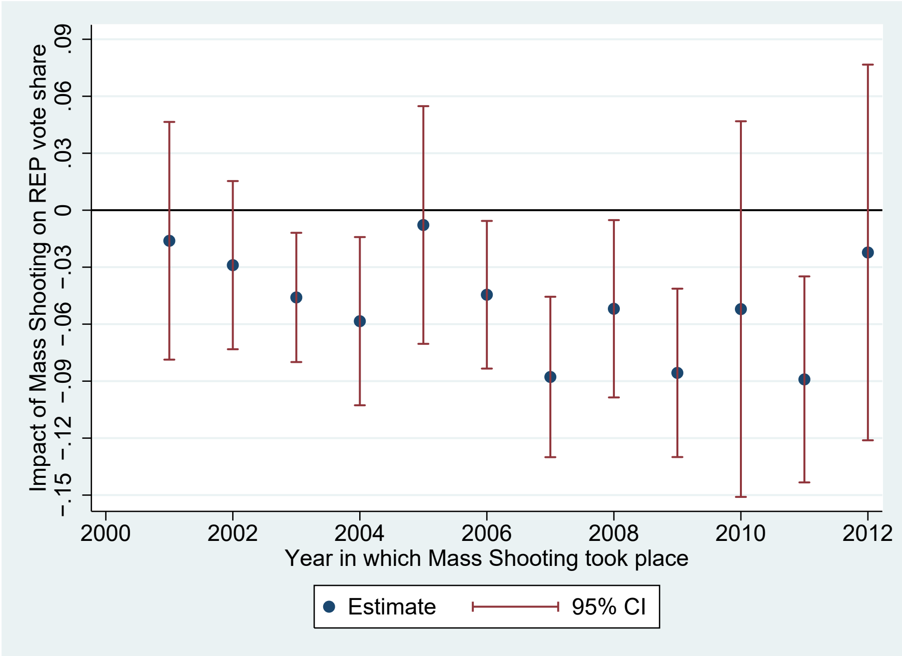
Notes: The Figure shows the location of current and past mass shootings. The counties with mass shootings during my sample period are highlighted in blue, while the counties with mass shootings in the past are colored in green.

Figure A2: Failed and Successful Mass Shootings



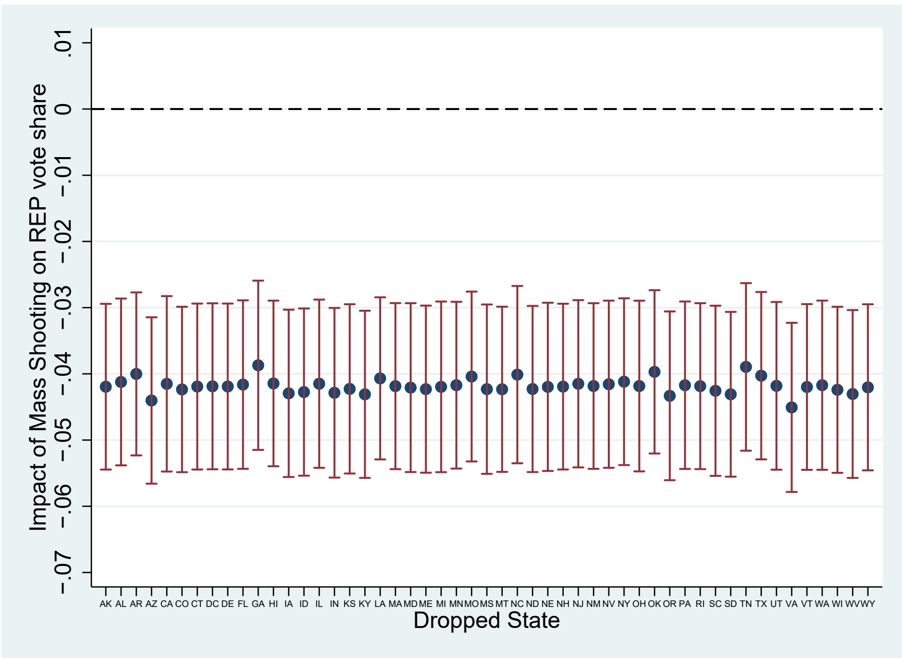
Notes: The Figure shows the location of "failed" and "successful" mass shootings. The counties with "successful" mass shootings are highlighted in blue, while the counties with "failed" mass shootings are colored in green.

Figure A3: Effect of Mass Shootings by year it occurred



Notes: The figure shows the point estimate and the 95% confidence interval for the impact of mass shootings on the Republican vote share in the Presidential election depending on the year it occurred.

Figure A4: Effect of Mass Shootings by dropping one state at a time



Notes: The figure shows the point estimate and the 95% confidence interval for the impact of mass shootings on the Republican vote share in the Presidential election dropping one state at a time. The x-axis labels the alpha code of the state which is dropped from the estimates.

Table A1: Measuring Bias from Unobservables using Selection on Observables

	Baseline Effect	Controlled Effect	"Identified Set"	Exclude Zero	Within Coef Interval	δ for $\beta = 0$
Presidential	-0.044	-0.040	[-0.026, -0.040]	Yes	Yes	2.58
Gubernatorial	-0.039	-0.037	[-0.024, -0.037]	Yes	Yes	2.84
Senatorial	-0.033	-0.027	[-0.011, -0.027]	Yes	Yes	1.89
House	-0.024	-0.027	[-0.028, -0.041]	Yes	Yes	-15.44

Notes: The tables shows the results from applying method proposed by Oster (2017) to assess bias from selection on unobservables from observables. The column titled "Baseline Effect" shows the estimates without any controls, while the column titles "Controlled Effect" shows the estimates with the preferred set of controls included in the regression. The "Identified Set" calculates the bounds on treatment effect. That is, it is the set bounded by $\tilde{\beta}$ and $\beta^*(\overline{R_{max}}, 1)]$, where $\tilde{\beta}$ is the estimate of β obtained using preferred set of controls and β^* is the bias-adjusted treatment effect under maximum possible R^2: $\overline{R_{max}}$ and an equal selection on unobservables as observables ($\delta = 1$). The column titled "Exclude Zero" notes whether zero lies within the bound on treatment effect i.e. the "Identified Set". The column titled "Within Coef Interval" notes whether the bound on treatment effect i.e. the "Identified Set" lies within the 95% confidence interval of $\tilde{\beta}$. Finally, Column titled "δ for $\beta = 0$" estimates the degree of selection on unobservables as a proportion of selection on observables is needed to obtain a bias-adjusted treatment effect of zero.

Table A2: Detecting Spillovers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	25km	50km	100km	200km	CD	MSA	C Zone	State	Closest
MS * Post	-0.041***	-0.041***	-0.040***	-0.034***	-0.041***	-0.039***	-0.040***	-0.043***	-0.043***
	(0.011)	(0.011)	(0.012)	(0.012)	(0.012)	(0.011)	(0.012)	(0.014)	(0.012)
MS within:	-0.023	-0.004	0.001	0.008	-0.003	0.013	0.001	-0.004	
	(0.015)	(0.007)	(0.006)	(0.006)	(0.014)	(0.010)	(0.007)	(0.010)	
Log(Dist. from MS):									-0.002
									(0.003)
Observations	15,658	15,658	15,658	15,658	15,658	15,658	15,658	15,658	15,658
R-squared	0.410	0.409	0.409	0.410	0.409	0.410	0.409	0.410	0.410

Notes: The tables shows the results from OLS estimation of Equation 11. The dependent variable is county Republican vote share in Presidential elections. The main independent variable is indicator of mass shootings interacted with dummy equal to one for after mass shootings. In Columns 1 to 4, $MSwithin$ is an indicator equal to one for all periods after there is a mass shooting within x km from the county and zero otherwise. In Columns 5 to 8, $MSwithin$ is an indicator equal to one for all periods after there is a mass shooting inside the x geographic entity in which the county is located and zero otherwise. $Log(Dist.fromMS)$ is natural logarithm of the distance from the closest mass shooting that occurred in a given election cycle. In all the estimates, I include county and year fixed effects and preferred set of controls. The preferred controls contain demographic and economic characteristics. Demographic characteristics include: urban dummy, proportion of population that is male, white, black, Hispanic, has completed high school, has completed college, married, greater than 18 years old and greater than 65 years old. Economic characteristics include: natural log of income, unemployment rate and poverty rate. The standard errors are clustered at congressional district level. * indicates significance at 10% significance level, ** indicates significance at 5% significance level, while *** indicates significance at 1% significance level.

Table A3: Predicting Mass Shootings Flexibly

VARIABLES	(1) Mass Shooting	(2) Mass Shooting	(3) Mass Shooting	(4) Mass Shooting	(5) Mass Shooting	(6) Mass Shooting	(7) Mass Shooting
REP Share t-1	0.001	0.057	0.009	0.001	0.003	0.017	0.056
	(0.030)	(0.039)	(0.031)	(0.032)	(0.031)	(0.036)	(0.043)
Turnout t-1	0.025	0.028	0.004	0.019	0.034	0.019	0.025
	(0.038)	(0.040)	(0.039)	(0.037)	(0.037)	(0.039)	(0.039)
Observations	9,364	9,364	9,364	9,364	9,364	9,364	9,364
R-squared	0.000	0.027	0.008	0.007	0.011	0.005	0.057
Number of fips	3,135	3,135	3,135	3,135	3,135	3,135	3,135
Controls	None	Demographic	Economic	Gun	Crime	Health	All
F-Test: Electoral variables = 0	0.233	1.249	0.0435	0.141	0.447	0.234	0.961
F-Test: Demographic variables = 0	–	1.522	–	–	–	–	1.475
F-Test: Economic variables = 0	–	–	0.829	–	–	–	0.756
F-Test: Gun variables = 0	–	–	–	0.637	–	–	0.661
F-Test: Crime variables = 0	–	–	–	–	0.748	–	0.779
F-Test: Health variables = 0	–	–	–	–	–	1.395	1.342

Notes: The table shows the results from OLS estimation of equation 10. The dependent variable is MS_{it}. The main independent variables are deciles of the lagged Republican vote share and lagged voter turnout. The first column shows unconditional estimates. The deciles of local characteristics are added sequentially in the estimation. Each category of characteristics include variables as shown in Table 1. The standard errors are clustered at congressional district level. * indicates significance at 10% significance level, ** indicates significance at 5% significance level, while *** indicates significance at 1% significance level.

Table A4: Robustness of effect of Mass Shootings on Voter Preferences

VARIABLES	(1) Imp. Gun	(2) GC Inc.	(3) GC Dec.	(4) Imp. Gun	(5) GC Inc.	(6) GC Dec.
MS * Post	0.094**	-0.038	0.017***	0.105***	0.069***	-0.003
	(0.038)	(0.029)	(0.006)	(0.025)	(0.018)	(0.010)
Observations	2,604	2,607	2,607	3,965	3,970	3,970
R-squared	0.325	0.248	0.310	0.295	0.257	0.196
Voters	REP	REP	REP	DEM	DEM	DEM
Mean	2.935	0.314	0.0739	2.762	0.647	0.0199

Notes: The table shows the results from OLS estimation of equation 7 using data from American National Election Studies. I define REP and DEM based on their past voting behavior. That is, a respondent is REP (DEM) if s/he voted for Republican (Democratic) Presidential candidate in the previous election. In all the estimates, I include congressional district and year fixed effects. The independent variable: $MS * Post$ is indicator of mass shootings interacted with dummy equal to one for after mass shootings. The dependent variables are measure of importance of gun control (Columns 1 and 4), indicator equal to one if the respondent says gun control should increase (Columns 2 and 5) and indicator equal to one if the respondent says gun control should decrease (Columns 3 and 6). Columns 1 to 3 use data on respondents who voted for Republican Presidential candidate in the previous election, and Columns 4 to 6 use data on respondents who voted for Democratic Presidential candidate in the previous election. All estimations include congressional district fixed effects and individual characteristics: race, income, age, education, marital status, political leaning and religiosity. The standard errors are clustered at congressional district level. * indicates significance at 10% significance level, ** indicates significance at 5% significance level, while *** indicates significance at 1% significance level.