Bullets to Ballots:
Proximity to Crime and Political Behavior

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Abstract

This paper analyzes the association of experiencing a nearby homicide on individual voter turnout in the 2016 U.S. general election in Houston, Texas. We assemble a novel individual-level dataset by merging geocoded murder incident records with the state voter file. Our empirical strategy exploits the as-if random timing of murders around Election Day in a difference-in-differences design: individuals living within one mile of a murder in the 180 days *before* the election serve as the treatment group, and those living near murders that occurred in the 180 days *after* the election form a control group. We find that pre-election local homicides substantially depress turnout: treated individuals are about 7.6 percentage points less likely to vote in 2016 relative to the control group. Younger voters (Millennials) and politically unaffiliated (non-partisan) voters exhibit especially large turnout declines when exposed to local violence, with Millennials experiencing a 12 percentage point reduction and non-partisans experiencing an 8 percentage point reduction. By contrast, we find that all racial groups experience similar demobilization effects. Men and women also show comparable turnout declines. We discuss mechanisms for the depressive effect of community violence on voting, and consider the implications for democratic representation and public safety.

1 Introduction

Crime is a persistent and visible feature of urban life. Its presence can have far-reaching consequences not only for individuals' well-being and economic opportunities but also for their political behavior. While much research has explored the impact of direct victimization or highly publicized events like police killings, less is known about how proximity to everyday violent crime affects whether individuals participate in democratic processes such as voting. For residents of a community, a violent incident on their own block or street could alter their sense of security and engagement with local government in ways that more distant events might not. Research has shown that individuals residing in areas with heightened crime rates often favor policies and candidates emphasizing a dedication to public safety (Philipson and Posner, 1996). However, the study of the relationship between crime and voting behavior encounters challenges due to endogeneity in occurrence, posing threats to

any causal identification. The existing literature on this topic is limited, even though a significant portion of voters consider violent crime a pivotal factor in their electoral decisions (Gramlich, 2022). Understanding how being exposed to crime in one's neighborhood will affect political participation and party affiliation holds considerable public significance.

This paper examines how spatial proximity to violent crime incidents in the city of Houston, Texas affects individual-level voter turnout. We focus on a panel of registered voters and match their residential addresses to nearby criminal events with precise geographic and temporal identifiers. By implementing a quasi-experimental design that leverages the timing of crime events relative to the November 2016 general election, we test whether individuals exposed to a violent crime incident in the six months leading up to the election exhibit different voting behavior compared to those who experienced a similar incident in the six months following the election. Our design follows and adapts the empirical strategy introduced by Markarian (2022), who studies the effect of police killings on voter turnout using a matched difference-in-differences framework. However, unlike police shootings or politically motivated violence, everyday criminal homicides often lack an explicit political message or organized protest response. We extend this methodology to study community violent crime, which excludes state violence, in one of the most populous and demographically diverse cities in the United States. Because Houston's crime and voter data are rich and geocoded, the setting allows for a high-resolution analysis of spatial exposure and temporal variation in incidents.

2 Literature Review

2.1 Crime and Political Behavior

The literature on the political consequences of crime dates back decades but has recently gained traction with the availability of high-quality administrative data. Bateson (2012) argues that victimization can serve as a catalyst for political engagement, motivating individuals to seek redress through political channels. Her evidence from Latin America suggests that crime can increase participation, particularly when institutions are perceived as responsive. Other studies, however, highlight the demobilizing effects of violence. Markarian (2022) finds that individuals living near police killings in the United States are less likely to vote in presidential elections, particularly among Black voters. His study leverages the quasi-random timing of police killings within neighborhoods and shows that such exposure depresses turnout. In Latin America, Berens and Dallendorfer (2019) demonstrate that crime and insecurity shift electoral preferences toward more punitive, authoritarian-leaning candidates. Sønderskov et al. (2022) find that violent victimization in Denmark increases political participation, while property crime does not. These diverging findings suggest that context, crime type, and institutional trust mediate the political consequences of crime.

2.2 Neighborhood Spillovers

Beyond direct victims, violence can have spillover effects on those living nearby. Most research on spillovers focuses on children and students. For instance, exposure to shootings

near schools negatively impacts students' educational outcomes. Studies such as Beland and Kim (2016) and Ang (2021) found that police violence in neighborhoods detrimentally affects the educational achievement of students, especially minority students. Additionally, exposure to more affluent neighborhoods tends to improve children's long-term outcomes (Chetty et al., 2016). With respect to voting behavior, some evidence indicates that areas experiencing higher crime rates exhibit greater voter turnout (Berens and Dallendorfer, 2019; Caiazza and Putnam, 2014). This counterintuitive pattern could reflect heightened political salience of public safety in high-crime areas or community mobilization in response to disorder.

2.3 Spatial Proximity and Quasi-Experimental Designs

Recent work in political science and criminology has employed spatial proximity to define treatment in studying violence. Ang (2021) uses proximity to police killings as a treatment to examine differential effects on students' educational outcomes. Markarian (2022) advances this design by holding location constant and using the timing of incidents relative to elections to define treatment and control groups. This approach allows for credible causal identification, assuming that the timing of incidents is exogenous to the electoral calendar. We adopt a similar approach by identifying individuals who live in neighborhoods where a violent crime occurred and comparing those who were exposed before the election to those who were exposed after. Because crime-prone neighborhoods differ systematically from low-crime areas, restricting attention to voters living within a given radius of any violent crime ensures that treated and control groups are drawn from the same general risk environment. In our study, we consider voters within one mile of a crime to capture broad neighborhood exposure. This allows us to examine the effects of local violent crime on political behavior.

3 Theoretical Framework

We model the decision to vote as a utility-maximizing choice. Individual i votes if the perceived benefit from voting exceeds the associated cost:

$$V_i = \begin{cases} 1 & \text{if } U_i^v - C_i > 0, \\ 0 & \text{otherwise.} \end{cases}$$

Here, U_i^v is the utility from voting and C_i is the perceived cost of voting. Both are functions of local crime exposure and individual characteristics. Specifically, we define:

$$U_i^v = \alpha + \delta \cdot \mathit{Crime}_i + \mathbf{X}_i'\beta + \varepsilon_i,$$

$$C_i = \kappa + \theta \cdot \mathit{Crime}_i + \mathbf{Z}_i'\gamma.$$

In these expressions, Crime_i is an indicator (or intensity measure) of whether individual i was recently exposed to a violent crime near their residence (e.g., within one mile of home and within six months of the election). $\mathbf{X}_i$ and $\mathbf{Z}_i$ are vectors of covariates (e.g., age, race, education, partisanship), and ε_i captures unobserved heterogeneity in voting utility.

This formulation allows local crime exposure to affect both the benefits and costs of voting. The coefficient δ captures a *mobilization effect*: individuals may perceive higher political stakes when crime looms large in their immediate environment, motivating them to vote. This channel is supported by studies such as Bateson (2012), who finds that crime victimization increases political participation in Latin America, and some evidence in Markarian (2022), which suggests proximity to police killings can increase turnout in certain subgroups. The coefficient θ captures a *demobilization effect*: crime may induce fear, psychological distress, or political disillusionment that raises the cost of participation. Gilliam Jr. and Iyengar (2000) argue that media amplification of local crime can generate cynicism and disengagement, while Enos (2016) finds that perceived threat from neighborhood violence can reduce turnout among some groups.

Empirical Implication. The reduced-form net effect identified by our research design is

$$\beta = \delta - \theta.$$

A positive β indicates that the mobilizing effect of crime outweighs the deterrent effect, while a negative β suggests demobilization prevails. The ambiguity of this net effect is consistent with mixed findings in the literature: for example, Sønderskov et al. (2022) show that violent victimization in Denmark increases turnout, whereas Markarian (2022) reports turnout suppression near police use-of-force incidents in the U.S. This theoretical framework also allows for testable heterogeneity. For instance, the mobilizing effect δ may be stronger among voters with high institutional trust or strong partisan alignment, while the demobilizing effect θ may be greater for historically marginalized populations or those with weaker baseline civic engagement.

4 Empirical Strategy

4.1 Overview

We employ a difference-in-differences (DiD) framework to estimate the causal impact of local violent crime exposure on changes in voter turnout between the 2012 and 2016 presidential elections. Following Markarian (2022), our design compares voters in neighborhoods where a violent crime occurred shortly *before* the 2016 election (treatment group) to voters in similar neighborhoods where a violent crime occurred shortly *after* the election (control group). By differencing turnout between 2016 and the pre-treatment election (2012) for each individual, and comparing treated versus control individuals, this approach controls for time-invariant characteristics of voters and common election shocks. In effect, we exploit the quasi-random timing of violent incidents around the election to identify the causal effect, under the assumption that the exact date of a crime relative to Election Day is unrelated to potential turnout.

4.2 Treatment and Control Definition

We define the treatment and control groups based on spatial proximity to violent crime incidents and the timing of those incidents relative to the election:

- **Treatment Group:** Individuals living within one mile of a homicide that occurred in the six months *prior* to the November 8, 2016 general election.
- **Control Group:** Individuals living within the same radius of a homicide that occurred in the six months *after* the 2016 election.

By focusing on voters residing in neighborhoods where at least one homicide occurred within a narrow temporal window around the election, we compare individuals who live in similar social and spatial contexts. This ensures the treated and control voters face comparable baseline neighborhood conditions (e.g., in terms of crime propensity, socioeconomic factors, etc.). In other words, we hold constant the *location* of crime risk and only vary the timing of exposure relative to the election. The identifying assumption is that, absent the timing of the crime incident, the two groups would have had similar trends in turnout from 2012 to 2016. We bolster this assumption by verifying that pre-election characteristics (including prior turnout) are balanced or controlled for between the groups (see Table 1).

We implement this treatment assignment using crimes within a one-mile radius of the voter's residence. If multiple crimes occurred in a given area, we consider the incident nearest in time to Election Day (within the window) as the focal crime event for treatment assignment.

4.3 Identification: Temporal and Geographic Distribution of Homicides

A critical assumption underlying our difference-in-differences design is that the timing of homicides relative to Election Day is quasi-random—that is, conditional on a homicide occurring in a given neighborhood, whether it happens before or after the election is unrelated to potential voter turnout. To assess the plausibility of this assumption, we examine the temporal and geographic distribution of homicides in Houston during our study period.

Figure 1 presents the monthly distribution of homicides in Houston for 2016 and 2017. The left panel shows the raw counts of homicides by month, while the right panel displays the percentage distribution across months for each year. Several patterns emerge from this analysis. First, we observe some seasonal variation in homicide rates, with certain months showing higher concentrations of incidents. However, importantly, there is no evident spike or anomaly in homicide occurrences specifically around the November 2016 election period. The months immediately before and after the election (September through January) show relatively stable rates without systematic differences that would threaten our identification strategy. Second, the patterns are broadly similar across 2016 and 2017, suggesting that the seasonal variation we observe represents typical fluctuations in Houston's homicide rates rather than year-specific shocks.

The lack of a pronounced discontinuity in homicide rates around Election Day supports our identifying assumption that the precise timing of homicides relative to the election is plausibly exogenous. If there were systematic factors causing homicides to cluster either just before or just after the election, we would expect to see clear breaks in the time series around November. The relatively smooth temporal distribution gives us confidence that treated and control groups are comparable along unobserved dimensions.

Figure 2 complements this temporal analysis by showing the geographic distribution of homicides within 180 days of the 2016 election. Red circles indicate homicides that occurred before the election (our treatment exposure), while black triangles indicate homicides that occurred after the election (our control exposure). The map reveals that both pre-election and post-election homicides are spatially interspersed throughout Houston, with substantial overlap in the neighborhoods affected by treatment versus control incidents. This geographic intermixing is crucial for our identification strategy: it means that treated and control voters live in the same general areas of the city and face similar baseline neighborhood characteristics. Rather than comparing voters in systematically different parts of Houston (which might differ in unobserved ways), we are comparing nearby residents who happened to experience a homicide at different times relative to the election.

The spatial overlap also addresses concerns about geographic confounding. If pre-election homicides were concentrated in particular neighborhoods with distinct political or socioeconomic characteristics, our estimates might reflect these neighborhood differences rather than the causal effect of crime timing. However, the intermixed pattern in Figure 2 demonstrates that our treatment and control groups are drawn from largely overlapping spatial distributions, strengthening the internal validity of our design.

Together, these temporal and geographic patterns provide strong support for the key identifying assumption of our analysis: that the timing of homicides around Election Day is quasi-random, allowing us to interpret differences in turnout between treated and control groups as causal effects of pre-election crime exposure.

4.4 Data Sources

We combine data from multiple sources:

- **Crime Data:** Incident-level records from the Houston Police Department for homicides occurring between May 2016 and May 2017. Each record includes the offense type, date and time, and latitude/longitude of the incident.
- **Voter Data:** Individual-level voter file data from L2, which provides voter registration information, party affiliation as of 2016, voting history (including whether the person voted in 2012 and 2016), and geocoded residential addresses as well as other demographic information from the voter.

For this study, we restrict the sample to registered voters who resided at the same address from at least 2014 through 2017, to avoid issues of people moving (and thereby potentially changing their exposure status or voting jurisdiction). This yields a panel of voters for whom we observe turnout in 2012 and 2016, and who could be matched to any nearby crime events in the 2016 period of interest. Our crime data includes all recorded homicides in Houston over the year spanning mid-2016 to mid-2017, which we use to identify treated and control exposures for each voter as described above. Table 1 provides summary statistics for the treated and control groups in our baseline (1-mile radius) sample.

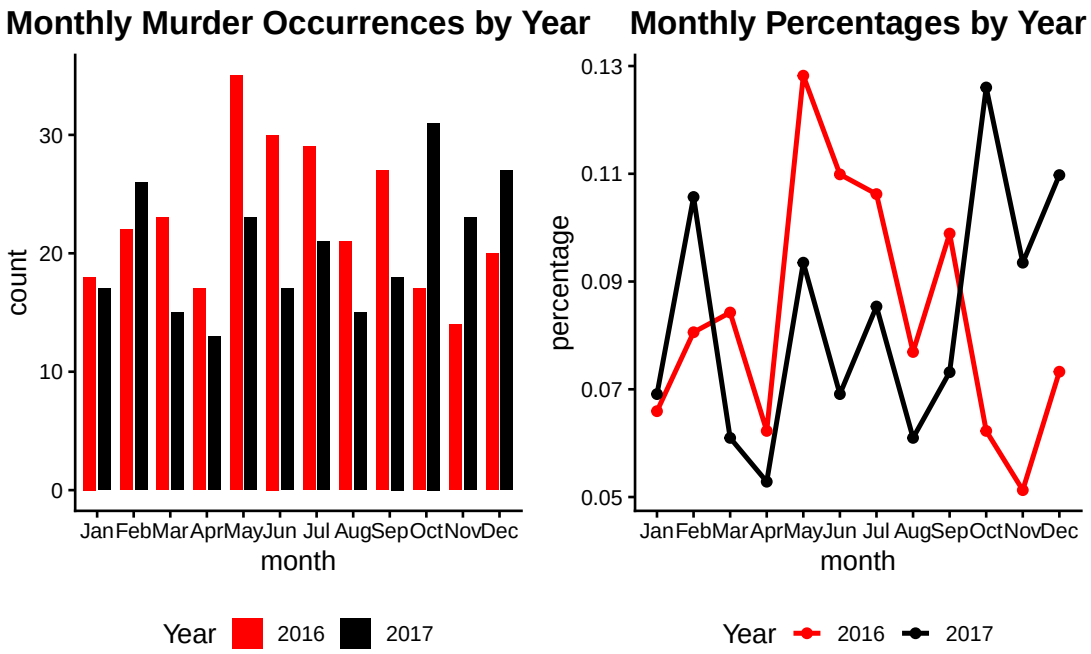

Figure 1: Monthly Distribution of Homicides in Houston, 2016-2017

Note: Left panel shows raw counts of homicides by month for 2016 (red) and 2017 (black). Right panel shows the percentage distribution of homicides across months for each year. The November 2016 election period shows no anomalous spike or discontinuity in homicide rates.

4.5 Model Specification

We estimate the following linear probability model in a difference-in-differences framework:

$$Y_{it} = \gamma_t + \beta_1 \text{Treated}_i + \beta_2 \text{Post}_t + \delta \left(\text{Treated}_i \times \text{Post}_t\right) + u_{it}, \tag{1}$$

where Y_{it} is an indicator for whether individual i voted in election year t. In practice $t \in \{2012, 2016\}$. Treated_i is a binary variable equal to 1 for individuals in the treatment group (i.e., those living near a crime that occurred pre-election), and Post_t is an indicator for the post-treatment period ($t = 2016$). The coefficient δ on the interaction $\text{Treated}_i \times \text{Post}_t$ is the difference-in-differences estimator of the treatment effect. Standard errors are clustered at the murder event level to account for multiple individuals exposed to the same murder.

5 Results

5.1 Baseline Results

Our baseline estimates indicate a significant *demobilizing* effect of local homicides on voter turnout. The results in Table 2 show that for voters living within one mile of a homicide, exposure to that homicide in the six months before the 2016 election reduced their probability of voting in 2016 by approximately 7.6 percentage points compared to similar voters

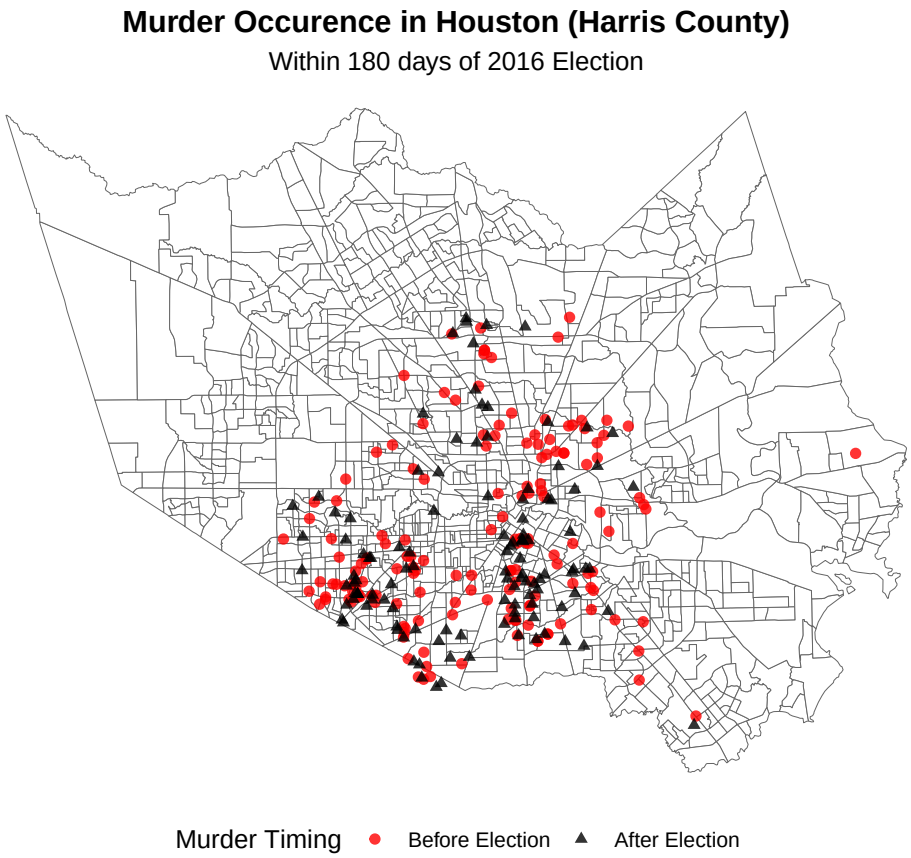

Figure 2: Geographic Distribution of Homicides Within 180 Days of 2016 Election

Note: Red circles indicate homicides occurring in the 180 days before the November 8, 2016 election (treatment group exposure). Black triangles indicate homicides in the 180 days after the election (control group exposure). The spatial intermixing of pre- and post-election homicides demonstrates that treated and control voters reside in overlapping neighborhoods.

whose nearest homicide occurred after the election. This estimate is obtained from the DiD specification, and it is statistically significant ($p < 0.01$). In substantive terms, an incident of lethal violence in one's vicinity shortly before an election appears to substantially depress electoral participation.

5.2 Heterogeneity by Generation

We examine whether the effect of local homicides on turnout varies across different age cohorts. Figure 3 shows the estimated treatment effects for Baby Boomers (born 1946-1963), Generation X (born 1964-1979), and Millennials (born 1980-1995) separately, using the one-mile pre-election exposure definition for treatment.

The results reveal striking generational differences in response to nearby homicides. Millennials exhibit by far the largest turnout decline, with an estimated decrease of approximately 11.7 percentage points associated with pre-election homicide exposure. This effect is highly statistically significant ($p < 0.01$). Generation X voters show a moderate turnout

Table 1: Summary Statistics for Voters Within 1 Mile of a Homicide

Variable	**Treated**	**Control**
Male (%)	46.47	46.47
Female (%)	53.53	53.53
White (%)	31.27	35.34
Black (%)	33.36	28.10
Hispanic (%)	27.41	27.54
Asian (%)	5.71	6.16
Mean distance to crime (miles)	0.578	0.627
2012 turnout (%)	51.96	49.57
2016 turnout (%)	58.10	63.26
Number of voters	435,310	127,935
Number of homicide incidents	268	

Note: Treated group voters lived within 1 mile of a homicide in the 6 months before the 2016 election; Control group voters lived within 1 mile of a homicide in the 6 months after the election. Percentages are of voters in each group with the given characteristic; distance is the average distance from the voter's residence to the relevant crime incident.

reduction of about 6.4 percentage points, also statistically significant. Baby Boomers experience the smallest effect, with a turnout decrease of approximately 5.2 percentage points.

These patterns suggest that younger voters are substantially more sensitive to nearby violence when making voting decisions. This finding is consistent with the literature on voting habit formation, which shows that younger voters have weaker voting habits and are still in the process of developing stable patterns of electoral participation (Plutzer, 2002; Franklin, 2004). Younger voters may be more easily deterred from participation because they have not yet formed the automatic, habitual response to elections that older voters possess (Gerber et al., 2003). Additionally, younger voters may have higher geographic mobility and feel less rooted in their communities, or they may experience greater psychological disruption from traumatic neighborhood events. The larger effect on Millennials is particularly concerning from a democratic representation perspective, as it suggests that violence may disproportionately silence the political voices of younger citizens.

5.3 Heterogeneity by Gender

Figure 4 presents the treatment effects on turnout for male and female voters separately. The results show that both genders experience substantial and statistically significant turnout declines in response to nearby homicides, with remarkably similar magnitudes. Male voters exposed to a pre-election homicide show a turnout decrease of around 8.1 percentage points relative to male voters without such exposure. Female voters exhibit a very similar effect, with an estimated turnout reduction of approximately 7.1 percentage points.

The lack of meaningful gender differences in the demobilizing effect of homicides is noteworthy. While women and men may respond to violence and threats differently in many contexts, our results suggest that when it comes to electoral participation, both genders are

Table 2: Difference-in-Differences Estimates of Homicide Proximity on Turnout

	Within 1 Mile
Treated × Post (pp)	-0.076***
(Std. Error)	(0.013)
Observations (voter-years)	1,126,490
Unique voters	563,245
Homicide incidents	268

Note: The table reports the coefficient on the interaction term from a DiD regression (Equation 1) where the treatment is defined by a homicide within one mile and in the 180 days before versus after the election. Each individual's treatment is assigned using the chronologically nearest homicide event relative to Election Day. The specification controls for voter demographics. Standard errors (in parentheses) are clustered by homicide event. *** $p < 0.01$.

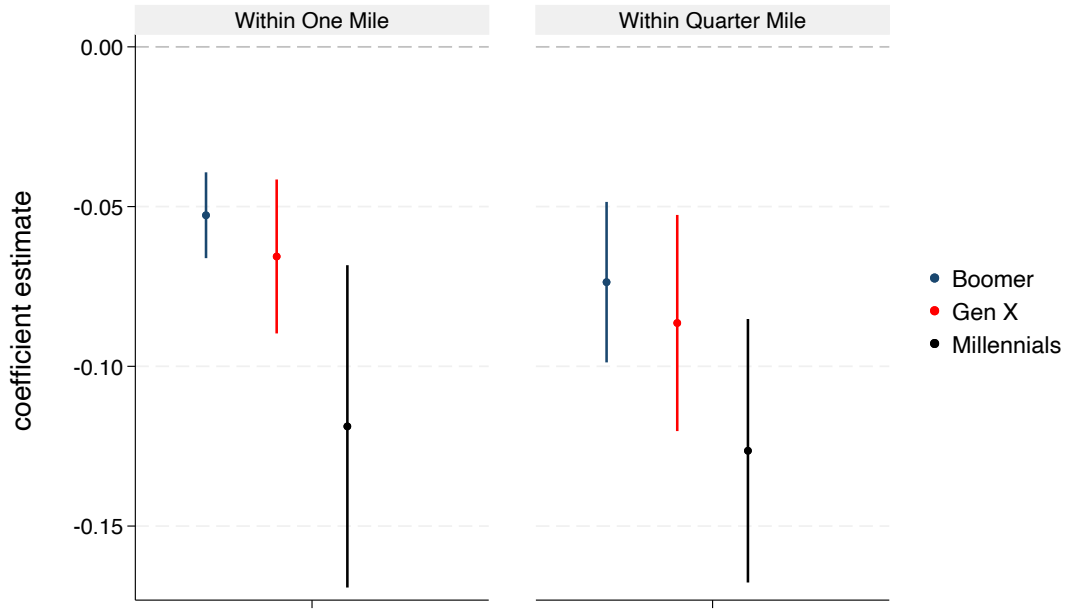

Figure 3: Treatment Effects on Turnout by Generation

Note: Each bar shows coefficient estimates and 95% confidence intervals from separate DiD regressions for voters in different generational cohorts within one mile of a homicide. Millennials (born 1980-1995) show the largest demobilization effect, consistent with research on weaker voting habits among younger cohorts (Plutzer, 2002).

similarly deterred by nearby lethal violence. This contrasts with some prior research suggesting gender-differentiated responses to crime and insecurity. The comparable magnitudes imply that the mechanisms driving demobilization—whether fear, trauma, logistical disruption, or political disillusionment—operate with similar force across genders in the context of neighborhood homicides.

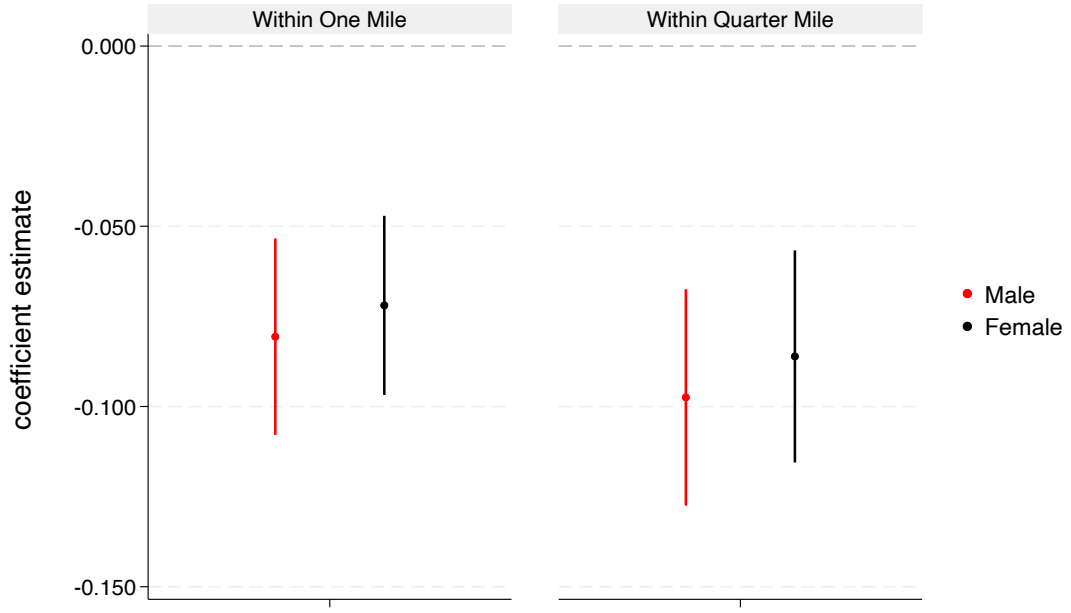

Figure 4: Treatment Effects on Turnout by Gender

Note: Each bar shows coefficient estimates and 95% confidence intervals from separate DiD regressions for male and female voters within one mile of a homicide. Both genders show similar turnout declines.

5.4 Heterogeneity by Race

We next examine whether the effect of local homicides on turnout varies across racial groups. Figure 5 shows the estimated treatment effects for White, Black, Hispanic, and Asian voters separately, all using the one-mile pre-election exposure definition.

The results indicate that all racial groups experience significant turnout declines in response to nearby homicides, with effect sizes that are relatively comparable across groups. White voters show an estimated decrease of approximately 7.7 percentage points. Black voters exhibit a turnout reduction of about 6.9 percentage points. Hispanic voters experience a decline of approximately 5.9 percentage points. Asian voters show a reduction of roughly 7.0 percentage points, though this estimate has wider confidence intervals due to the smaller sample size of Asian voters in the data.

While there are some modest differences in point estimates across racial groups, these differences are not statistically distinguishable from one another given the overlapping confidence intervals. This finding contrasts somewhat with prior research on police violence, which has documented particularly strong demobilizing effects on Black voters (Markarian, 2022). Our results suggest that when the violence is non-state perpetrated homicide rather than police killings, the demobilizing effect is more uniformly distributed across racial groups. All communities, regardless of racial composition, appear to experience significant political withdrawal in the face of nearby lethal violence. This pattern may reflect the fact that homicides, unlike police violence, do not carry the same racialized political messaging, and thus evoke a more uniform response of fear and trauma across different racial communities.

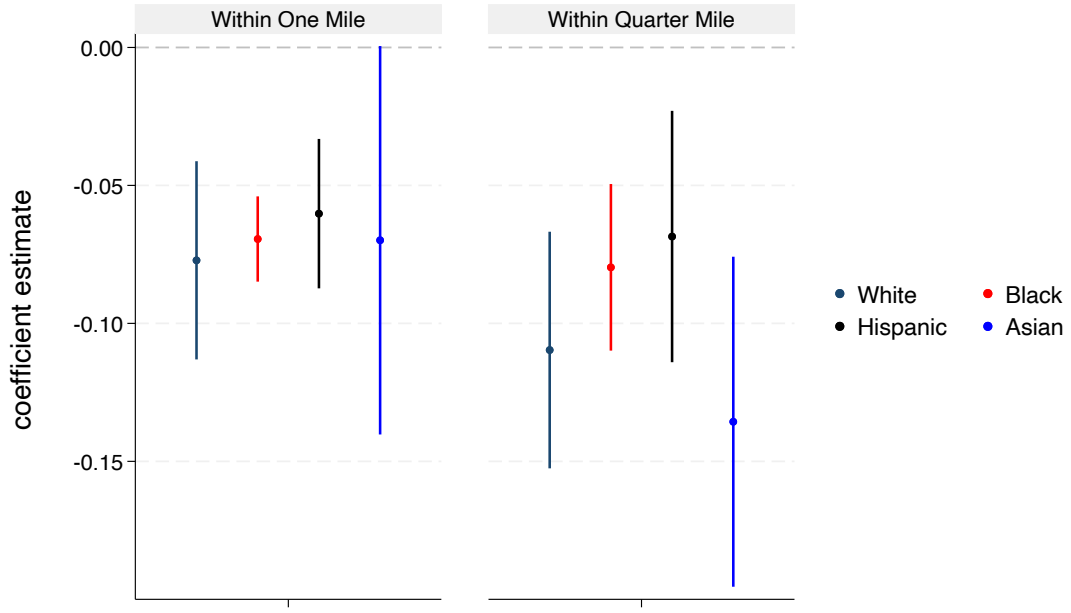

Figure 5: Treatment Effects on Turnout by Race

Note: Each bar shows coefficient estimates and 95% confidence intervals from separate DiD regressions for voters of different racial groups within one mile of a homicide. All racial groups show similar turnout declines.

5.5 Heterogeneity by Party Affiliation

We also explore heterogeneity by partisanship, leveraging the party registration information in our voter file. Figure 6 presents the treatment effects on turnout for voters of different party affiliations. We find substantial variation across partisan groups in their response to nearby homicides.

Non-partisan voters (those with no party affiliation or registered as independents) show the largest turnout decline, with an estimated decrease of approximately 8.2 percentage points. This effect is highly statistically significant. Democratic voters exposed to a pre-election homicide exhibit a turnout reduction of about 5.3 percentage points. Republican voters show a somewhat smaller decrease of approximately 4.2 percentage points. Both the Democratic and Republican effects are statistically significant, though smaller in magnitude than the non-partisan effect.

These divergent patterns by party suggest that political affiliation and ideology shape how voters interpret and respond to neighborhood violence. Non-partisan voters, who by definition have weaker attachments to the political system and lower baseline civic engagement, appear most susceptible to demobilization when confronted with local violence. This finding is consistent with research showing that non-partisans have weaker political habits and lower baseline turnout rates (Rodon, 2017). Non-partisans may be most easily discouraged from voting when faced with traumatic local events because they lack the strong partisan identity or organizational ties to political parties that can sustain participation even

in adverse circumstances.

The smaller effects on partisan voters, particularly Republicans, may indicate that strong partisan identity provides some resilience against demobilization. However, even among strong partisans, we observe significant turnout declines, suggesting that the shock of nearby lethal violence can overcome partisan motivations to participate. The somewhat larger effect on Democrats compared to Republicans could reflect different ideological frames for interpreting violence, or different levels of baseline trust in government's ability to address crime.

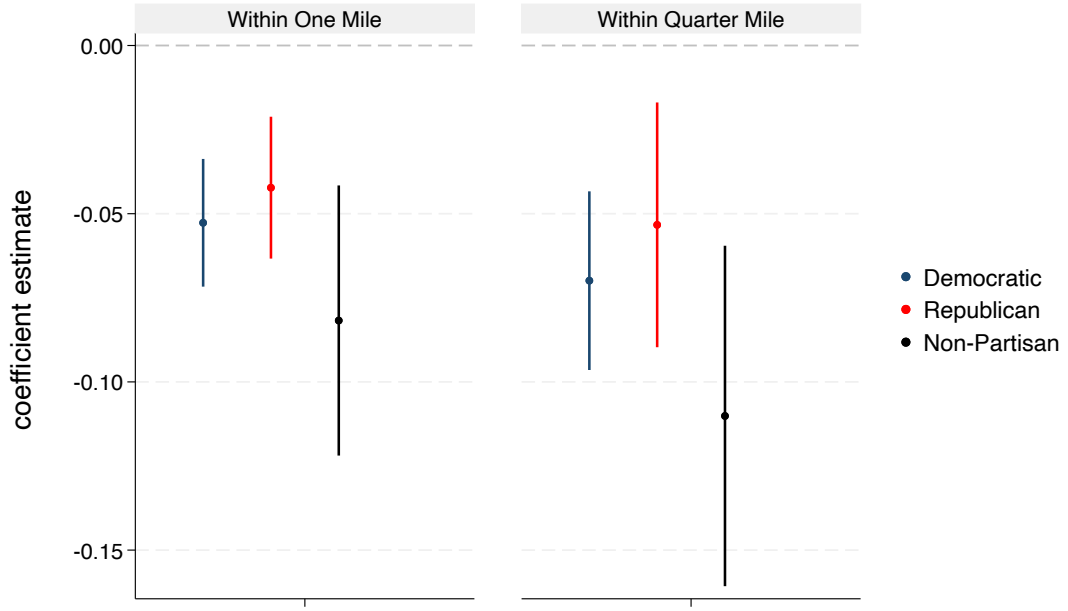

Figure 6: Treatment Effects on Turnout by Party Affiliation

Note: Each bar shows coefficient estimates and 95% confidence intervals from separate DiD regressions for voters with different party affiliations within one mile of a homicide. Non-partisan voters show the largest demobilization effect, consistent with their weaker baseline political attachments (Rodon, 2017).

5.6 Heterogeneity by Murder Location

Figure 7 examines whether the location where the homicide occurred affects its impact on voter turnout. We distinguish between homicides that occurred at residential locations (homes, apartments, etc.) versus non-residential locations (streets, businesses, parks, etc.).

The results reveal that homicides at both types of locations produce substantial turnout declines, but with some notable differences in magnitude. Residential homicides are associated with a turnout decrease of approximately 7.5 percentage points, while non-residential homicides produce a slightly smaller decline of about 7.0 percentage points.

These findings suggest that homicides occurring in residential settings may be particularly salient and threatening to nearby voters. A murder in someone's home or apartment complex may feel more invasive and create a greater sense of vulnerability than a street crime, as it

violates the sanctity of residential space. Nonetheless, the substantial effects for both types of locations indicate that any nearby homicide, regardless of where it occurs, can significantly depress political participation.

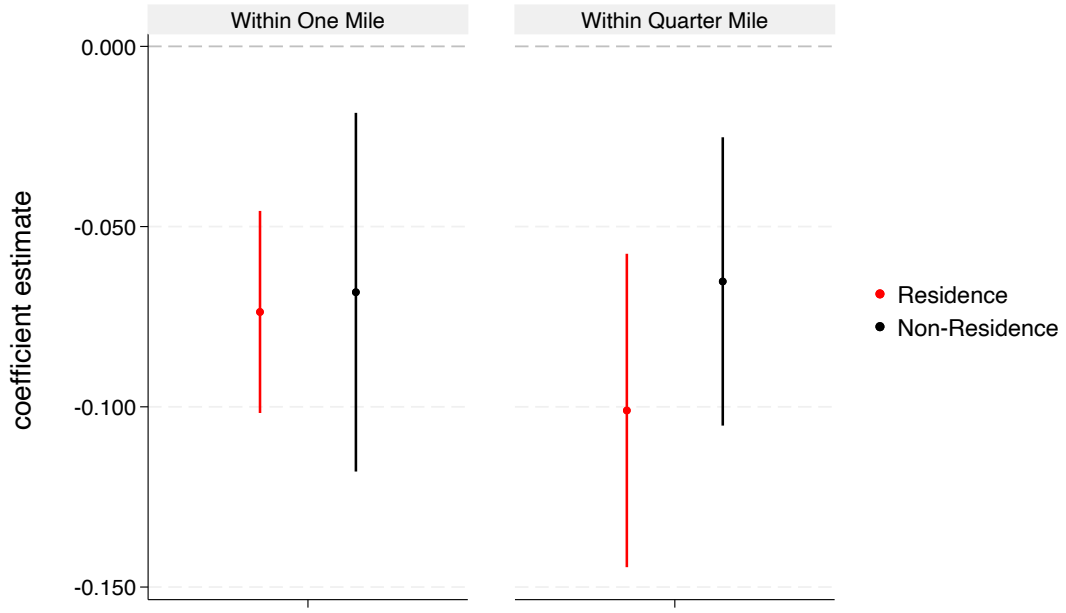

Figure 7: Treatment Effects on Turnout by Murder Location

Note: Each bar shows coefficient estimates and 95% confidence intervals from separate DiD regressions based on where the homicide occurred within one mile of voters. Residential homicides show somewhat larger effects.

5.7 Heterogeneity by Time of Day

Figure 8 explores whether homicides occurring during different times of day have differential effects on voter turnout. We categorize homicides as occurring during daytime (6 AM to 6 PM) or nighttime (6 PM to 6 AM) hours.

The results show that nighttime homicides have a considerably larger demobilizing effect than daytime homicides. Daytime homicides are associated with a turnout decrease of approximately 5.7 percentage points, while nighttime homicides produce a substantially larger decline of about 8.0 percentage points.

These findings are consistent with nighttime violence being particularly frightening and disruptive to residents. Homicides that occur at night may heighten fears about neighborhood safety during hours when people are typically at home, potentially making residents feel unsafe even in their own residences. Nighttime violence may also be more likely to disrupt sleep and create sustained anxiety. Additionally, nighttime homicides might be more likely to involve certain types of crime (such as gang violence or robbery) that are particularly threatening to residents. The larger effect of nighttime violence on turnout suggests that

not just the occurrence of violence, but its timing and the specific fears it evokes, matter for how voters respond politically.

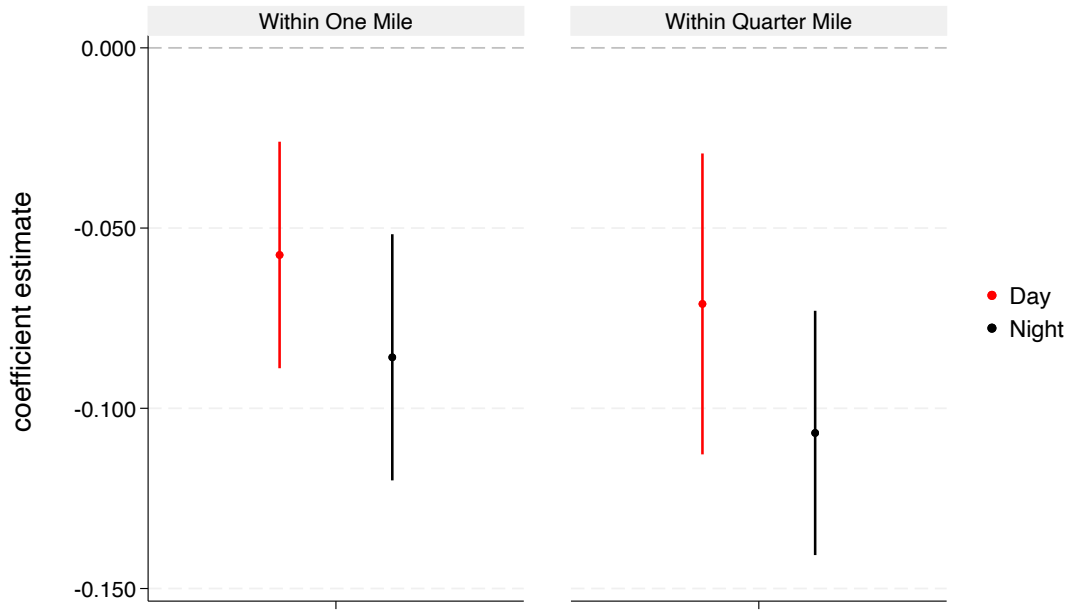

Figure 8: Treatment Effects on Turnout by Time of Day

Note: Each bar shows coefficient estimates and 95% confidence intervals from separate DiD regressions based on the time of day the homicide occurred within one mile of voters. Nighttime homicides show substantially larger demobilization effects.

6 Discussion: Potential Mechanisms

The evidence indicates that geographically proximate homicides can shape political participation, primarily through a demobilizing effect in our setting. What mechanisms might drive this result? Our theoretical framework posited two channels—mobilization via heightened issue salience (δ) and demobilization via increased costs or fear (θ). The pattern of findings suggests the cost/fear mechanism dominated in Houston's 2016 context.

The substantial negative effects we observe point to psychological shock and logistical disruption as key factors. A homicide close to one's home can create a climate of fear, grief, or insecurity that lowers the perceived benefits of voting or raises its costs (for example, someone might avoid public places, like polling stations, due to fear, or be too distraught to engage in politics). These reactions are especially plausible among communities that may already feel marginalized or cynical about political efficacy. Our finding that younger voters and non-partisan voters were especially demobilized is consistent with this interpretation—both groups have weaker baseline attachments to the political process and may be more easily deterred by traumatic neighborhood events.

The uniformity of effects across racial groups (in contrast to findings on police violence) suggests that everyday criminal homicides create a more universal experience of fear and trauma, without the racialized political overtones that characterize state violence. Similarly, the comparable effects across genders indicate that both men and women experience similar barriers to participation in the wake of nearby lethal violence.

The heterogeneity by location and timing of homicides provides additional insight into mechanisms. The larger effects of residential and nighttime homicides suggest that violations of home safety and disruptions during vulnerable hours are particularly psychologically impactful. These patterns point to fear and perceived threat as central to the demobilization process.

Notably, we find little evidence of a countervailing mobilization mechanism in our context. Unlike some studies that document increased political engagement following violence when it is politicized or tied to a social movement, the everyday criminal homicides in our Houston sample lack such political framing. Without a clear narrative linking violence to electoral politics or a rallying message for community action, the default response appears to be withdrawal rather than mobilization.

In sum, our findings underscore that local context and framing matter for how violence translates into political action. The mechanisms at play likely involve immediate emotional and practical responses to danger (which discourage voting), and only under certain circumstances (perhaps when violence is interpreted through a political lens or when community networks respond collectively) do we see countervailing mobilization.

7 Conclusion

This study provides evidence that geographically proximate homicides can causally influence voter turnout. Using a spatially refined difference-in-differences design around the 2016 election in Houston, we find that individuals who experienced a homicide in their immediate vicinity shortly before Election Day were significantly less likely to vote than those who experienced a similar incident after the election. The estimated turnout suppression of roughly 7.6 percentage points at the one-mile radius is substantial, suggesting that community violence can create a meaningful barrier to political participation.

Our heterogeneity analyses reveal important nuances in how different groups respond to nearby violence. The effects are especially pronounced among younger voters (Millennials showing an 11.7 percentage point decline) and non-partisan voters (8.2 percentage point decline), highlighting how violence may disproportionately silence those with weaker baseline civic engagement. By contrast, we find relatively uniform effects across racial groups and genders, suggesting that the psychological and logistical disruptions caused by homicides affect communities broadly. The larger impacts of residential and nighttime homicides point to the salience of home safety violations and vulnerabilities during non-daylight hours.

Our findings carry important implications. They suggest that high-crime communities may face a vicious cycle: violent disturbances not only harm public safety and quality of life, but also reduce political engagement, potentially leaving these communities with less voice in the democratic process and less influence over policies that could address crime. If local violence suppresses turnout, it could lead to under-representation of affected neighbor-

hoods in election outcomes, dampening the political incentive for officials to respond. This underscores the need for policymakers and community leaders to consider strategies to bolster electoral participation in areas struggling with violence—such as providing additional resources for voter outreach, trauma-informed civic engagement programs, or emphasizing public safety solutions during campaigns to mobilize affected residents.

Looking forward, this research opens several avenues for further inquiry. First, while we examined one election in one city, future studies could apply similar designs to other elections (e.g., midterms, local elections) and other urban areas to test the generalizability of these results. It may be that the crime-turnout relationship differs in municipal or congressional elections, or in cities with different socio-political dynamics. Second, extending the analysis to other forms of community violence or trauma could be illuminating. For example, do non-violent crimes (burglary, drug activity) or incidents of police violence, mass shootings, or civil unrest have analogous effects on voting behavior? Each type of incident could evoke a distinct mix of fear, outrage, or mobilization. Third, future work could explore longer-term impacts and cumulative exposure: does repeated exposure to crime across multiple election cycles compound the demobilization effect, or can communities become resilient over time? Finally, an important extension would be to consider political responses beyond turnout—such as changes in vote choice, support for law-and-order candidates or policies, or participation in protests and community meetings—to develop a more comprehensive picture of how geographically proximal violence shapes civic and political life. By continuing to investigate these questions, we can better understand the complex interplay between local environments and democratic participation, and inform interventions to ensure that all communities are able to make their voices heard even in the face of adversity.

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