Enclaves of Isolation: Violence and Political Participation in U.S. Cities

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Abstract

Does spatial proximity to violence mobilize or depress political participation? While research across the social sciences finds evidence of social isolation in high-violence neighborhoods, the democratic consequences of proximate exposure to violence remain underexplored. Merging voter files in U.S. cities with geocoded crime data, I test whether living near recent homicides affects federal election turnout. Using a regression discontinuity in time (RDiT) design, I find that close residential proximity to homicide significantly reduces turnout, with the strongest effects in plurality-Black block groups and those involving a Black victim. Mechanism tests using (1) foot-traffic data and (2) crime-linked survey responses suggest that violence exposure reduces movement and heightens perceived victimization risk, contributing to civic withdrawal and potentially reinforcing patterns of unequal political participation in race-class subjugated communities. More broadly, the study considers how persistent violence can undermine efforts towards political incorporation and democratic responsiveness in contexts of concentrated insecurity.

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Introduction

In 2022, the United States witnessed 20,138 firearm-related deaths, along with approximately 1,200 fatalities resulting from police shootings (The Trace 2022; Police Violence Report 2022).[1] American rates of state-sanctioned violence, such as police killings, vastly exceed those in other affluent democracies (Hirschfield 2023) and have appropriately drawn substantial scholarly and public attention (Morris and Shoub 2023; Ang and Tebes 2024; Markarian 2023; Clark, Glynn, and Owens 2025). In contrast, the homicide rate among civilians in the U.S., which is four to eight times higher than in peer nations, has received comparatively less focus. What are the democratic consequences of such high levels of fatal violence in the American context?

Previous research has identified two links that may inform our expectations. First, scholars of comparative politics have argued that violence can undermine the perceived legitimacy of political institutions. Democratic ideals rest on the mutual recognition of the state-citizen social contract. The state asserts its authority and legitimacy through territorial control and policy implementation, most notably through its "monopoly on violence" (M. Weber 2004; Tilly 1993; Soifer 2015; Skocpol 1979). Security provision, a core function of statehood, underpins this authority: individuals are motivated to cede certain rights in exchange for protection and public goods (Rotberg 2004). When the state fails to provide security, persistently high levels of violence may erode its perceived legitimacy.

Second, individuals living in high-violence contexts may simply lack the physical security necessary to engage in routine political, economic, and social activities essential to citizenship (O'Donnell 1999). Although research across the social sciences has shown that proximate exposure to violence may reduce individual incentives to engage in a variety of behavioral activities critical to upward mobility, social cohesion, and physical health (Sharkey 2018; Heissel et al. 2018; Sharkey and Torrats-Espinosa 2017), its effects on formal political participation remain underexamined. Violence may impose a psychological burden by heightening individuals' perceived risk of victimization, further discouraging social and political engagement (Arias and Goldstein 2010). In high-violence states marked by deficient and unequal service provision, participation in the social processes necessary for developing a citizen identity may become uneven–a limitation that Gonzalez (2017) refers to as "constrained" citizenship.

Yet, while comparative scholars have developed rich theoretical accounts of the democratically subversive consequences of violence (Bates 2001), its effects in the context of American politics remain underexamined. This is concerning given that, while the overall crime rate peaked in the early 1990s and has dropped since, the U.S. remains the most violent democracy in the Western world.

[1] This number excludes suicides.

Understanding these consequences in the American context is especially complicated in part because violence is unevenly distributed by race and space. At the height of homicide victimization in the 1970s, Black men were killed at a rate ten times higher than White men—72.9 versus 7.2 deaths per 100,000 (Fox and Zawitz 1999). In 2019, the Black–White homicide victimization ratio in the U.S. was roughly 5:1 (BJS 2019). Given that approximately 70 percent of violent encounters involve victims and offenders of the same race (Morgan and Thompson 2022), stark disparities in individuals' risk of violent victimization carry profound implications for the ability of Black Americans, in particular, to fully realize their rights as active participants in state functioning (Miller 2014).

Violence is also unevenly distributed across space. Not only are homicides more prevalent in urban areas, but also a small proportion of locations—such as specific street segments or intersections—account for a disproportionate share of overall crime in cities globally. Criminologists have developed the concept of the "Law of Crime Concentration" (LCC) to explain this phenomenon (Weisburd et al. 2004; Braga, Papachristos, and Hureau 2010; Levin, Rosenfeld, and Deckard 2017; Weisburd 2015). Work in American politics has begun to question how individuals' geographic context may shape inter-group sentiments and engagement with the democratic process (Enos 2017; A. P. Anoll, Davenport, and Lienesch 2024). Yet little attention has been paid to how intensely concentrated violence within communities may influence the political behavior of individuals residing within them.

To address this gap, I propose a theory of violence in spatial contexts to explain patterns of participation. Specifically, I argue that the concentration of violence exposure in enclaves of U.S. cities may depress turnout through two distinct mechanisms: 1) the delegitimization of the state and 2) a burden of fear, which leads to social isolation and disproportionately reduces democratic engagement among nearby residents. To test this theory, I employ a regression discontinuity in time (RDiT) design. Specifically, I examine whether living in close proximity to sites of homicide in U.S. cities has any effect on individual likelihood to formally engage with the political system via voting in federal elections. I provide evidence that residential proximity to homicide within roughly three-fourths of a mile leads to a decrease in turnout. This effect ranges from approximately 2 to 6 percentage points, the strongest of which are observed in predominantly Black census block groups and those with Black victims. Given that violence is disproportionately concentrated in Black neighborhoods, these results support my theory that the impact of violence on behavior differs depending on spatial context, with certain communities experiencing greater disruptions than others.

To substantiate the proposed mechanism of violence-induced fear leading to social isolation within neighborhoods, I analyze foot-traffic data at the census block group-level to assess how nearby violent events influence population movement. Additionally, I examine crime-linked survey data to offer evidence for the relative strength of my proposed mechanisms. Together, the analyses provide evidence that the unequal psychological burden of fear, shaped by an individual's perception of their risk of victimization, may drive observed the acute negative effects on turnout. However, my findings cannot rule out the possibility of state delegitimization as a plausible mechanism.

In addition to analyzing the impact of violence in spatial contexts and contributing to the broader study of how political geography shapes political behavior, this research makes several other notable contributions. First, from a methodological perspective, it provides causal evidence for a depressive effects of violent exposure on democratic engagement, which has previously been explored only through descriptive correlations and ethnographic research. Second, while the persistence of non-state sanctioned violence has spurred interesting lines of inquiry on the links between crime, citizenship, and democracy in developing contexts, the concentration of such analyses within urban areas of the global south has led to underestimates of the consequences of unabated violence within relatively "developed" democracies. While recent work has examined the participatory effects of mass shootings (Garca-Montoya, Arjona, and Lacombe 2022), these comparatively rare events account for a small proportion of American homicides, leaving the effects of the modal experience of violent exposure in the U.S. largely underexamined.[2] This research offers analyses to fill this critical gap.

Finally, this paper also offers valuable insights for the racialized policy feedback literature in American politics. While this work has understandably focused on the electoral effects of exposure to carceral institutions (e.g., police killings, incarceration), it has largely overlooked the broader contextual effects of civilian violence, which is often prevalent in these same communities. I conclude by considering how the spatial concentration of violence in urban enclaves may undermine efforts toward political equality in democratic participation more broadly.

Violence and Political Participation in a Comparative Context

Scholars of comparative politics have long sought to understand how exposure to violence shapes political participation, particularly in contexts marked by weak institutions and chronic insecurity. For the purposes of this study, I differentiate this body of work along two key dimensions. The first concerns an individual's positionality—whether they are directly victimized or indirectly exposed by residing in areas marked by

[2] In 2021, the Gun Violence Archive reports an estimate of 706 individuals murdered in instances of mass shootings. This number is less than 2 percent of the 48,830 deaths the CDC reports by firearms overall, and less than 3 percent of the 20,958 homicides specifically.

sustained or episodic violence. The second dimension concerns the nature of the violence itself. Much of the existing literature focuses on overtly political violence perpetrated by the state or organized armed groups, often in the context of conflict, rather than the everyday, "non-political" violence committed by civilians (Ley 2018; Garca-Sánchez 2010; Gallego 2018).

Focusing on the first dimension, work estimating effects of crime victimization in war and post-conflict settings have consistently identified positive impacts on political participation, most likely explained by the emotional or expressive factors related to victimization (Bellows and Miguel 2009; Bateson 2012; Voors et al. 2012; Blattman 2009). As Bateson (2012) notes, victims may seek engagement for expressive reasons related to the desire to "defining or reaffirming their identities," or to moderate the emotional responses of victimization such as anger (572). However, while emotional and expressive factors, such as the long-term psychological harm of victimization, have been emphasized (Macmillan 2001), the extent to which these mechanisms generalize to other forms of violent exposure remains uncertain. For instance, while the emotional response of "anger" is strongly linked to both victimization and political activism, "fear"—which may dominate among those living in highly violent contexts—has been more commonly associated with behaviors leading to political and social isolation (C. Weber 2013).

As Ley (2018) notes, while research on victimization has yielded valuable insights, important gaps remain in our understanding of the civic consequences of simply residing in environments subject to persistent or episodic violence (Barclay Child and Nikolova 2020). Studies from Mexico offer mixed evidence: Regidor and Hernandez (2012) find a weakly significant relationship between homicide rates and turnout, moderated by local development levels, while Trelles and Carreras (2012) present stronger evidence of demobilization in the most violent regions. During wartime, studies of proximate exposure finds that violence can suppress participation (Condra et al. 2018). However, while such studies point to broader mechanisms, they often focus narrowly on political violence or fail to differentiate its forms.

Turning to the second dimension, the question remains whether these findings extend to "non-political" civilian violence. For example, political violence such as insurgent attacks or assassinations near elections may explicitly signal electoral interference, undermining confidence in the voting process in ways that everyday civilian violence may not (Ley 2018). Additionally, researchers have identified conditional effects of proximate political violence expsure, often depending on who or what is targeted,[3] as well as whether the violence is ongoing or has ceased. These factors may help differentiate the experience of residing in a

[3] Following Arjona, Chacón, and Garca-Montoya (2025), targeted violence is aimed at specific individuals, such as activists or political leaders; indiscriminate violence affects bystanders or civilians regardless of identity or behavior; selective violence targets individuals based on their perceived affiliations or actions (e.g., known collaborators). These distinctions can shape how people assess personal risk, assign blame to institutions, and decide whether to participate politically.

high-violence neighborhood from post-conflict contexts, where studies more often find positive effects of previous violent exposure on political engagement (De Luca and Verpoorten 2015). The relative neglect of high levels of everyday, "non-political" civilian violence marks a critical gap in our understanding of the most common forms of violent exposure in urban settings, both in the U.S. and globally. By examining proximate exposure to homicide in the U.S., this study contributes to ongoing debates about when and how violence mobilizes or suppresses political participation.

Why might we expect proximate exposure to civilian violence to decrease one's likelihood of engaging politically? Previous literature identifies two individual-level mechanisms that may help explain this effect: 1) instances of violence evidencing a weak state capacity, breeding distrust and cynicism of the state, and 2) the fear induced by threat to one's physical insecurity directly hindering engagement with political processes (i.e., voting). This section reviews key findings from these literatures and considers their limitations when applied to non-political forms of violence.

Mechanism 1: Violence and State Legitimacy

There are two dominant theoretical approaches to explaining the relationship between exposure to violence and state legitimacy. The first, focused primarily on political conflict in wartime, argues that strategic violence can undermine trust in electoral processes and reduce incentives to vote. For example, Coupé and Obrizan (2016) find that during the conflict in Eastern Ukraine, indirect exposure to violence through property damage was associated with lower turnout, a greater likelihood of viewing elections as irrelevant, and weaker knowledge of local representatives. Similarly, Arjona, Chacón, and Garca-Montoya (2025) show that political assassinations in Colombia—especially those targeting candidates—likely undermine trust in the democratic process by disrupting candidate selection. However, as noted, these explicitly political cues may not translate as clearly in the context of non-political forms of violence.

Beyond studies employing formal causal inference, a substantial body of research has linked pervasive violence and broader challenges in state-building, democratization, and fostering political cooperation among citizens. Krasner (2001) defines the consolidation of power—what he calls "domestic sovereignty"—as the formal organization of political authority and the state's ability to exert effective control within its borders. This foundation underpins both economic and political development, reinforcing the perceived legitimacy of political institutions. By contrast, ongoing exposure to violence signals a breakdown in the state's capacity to provide security, undermining its role as guarantor of public safety (Malone 2010). As Rosenberg argues, when security consistently fails, "the very nature of the particular nation-state itself becomes illegitimate in the eyes and the hearts of a growing plurality of its citizens" (Rotberg 2004, 1). In turn, political institutions that fail to ensure basic safety lose public trust, diminishing incentives for political engagement.

A growing body of empirical research supports these claims. Survey-based studies have found a negative relationship between perceptions of crime and trust in political institutions (Cruz 2003; Carreras 2013). Consequently, low levels of trust and legitimacy are associated with reduced political engagement, as disillusioned citizens withdraw from participation and lose faith in democratic performance (Cox 2003; Trelles and Carreras 2012). Conversely, Grönlund and Setälä (2007) find that in 22 European countries, higher perceived legitimacy and trust in state institutions are positively linked to voter turnout, noting a "clear and linear relationship between trust in parliament and turnout as well as satisfaction with democracy and turnout" (418).

Mechanism 2: Fear and Participation

A second plausible mechanism is fear of violent victimization, which may hinder political engagement by fostering social isolation. This pathway aligns with political psychology research showing that anxiety often produces risk-averse and avoidant behaviors (Lerner and Keltner 2000). For example, in the aftermath of the 2004 Madrid bombings, Conejero and Etxebarria (2007) find that heightened fear and anxiety were linked to behaviors such as staying home, avoiding air travel, and limiting contact with Muslims. Other work has examined how geographic and emotional proximity to violent events shapes behavioral responses. Analyzing the emotional effects of the 9/11 attacks, Huddy and Feldman (2011) find that individuals directly affected—physically or emotionally—were more anxious about terrorism and less supportive of foreign interventions. In contrast, more distant individuals, who felt abstractly threatened but not anxious, were more likely to support aggressive anti-terror policies. While this paper does not focus on disaggregating emotional responses, these findings underscore how proximity to violence may elicit different reactions, potentially dampening or amplifying demand for government action. However, much of the literature centers on episodic violence. It remains unclear how fear operates in contexts marked by persistent threats.

In such environments, there is reason to suspect that enduring insecurity erodes individuals' willingness to invest in civic or economic activities essential to social well-being and stable governance (Bates 2001). Writing in 1957, political theorist Franz Neumann traced this political alienation to a psychological response of anxiety that develops over time in reaction to concrete danger situations. The intensification of this anxiety, he argued, can develop alongside a confirmation of group marginalization in social life, leading ultimately political alienation, a “conscious rejection of the rules of the game of a political system” (Neumann 1957).

Recent qualitative work supports a connection between persistent exposure to violence and political withdrawal. In interviews with Black women living in Chicago public housing, Moffett-Bateau (2023) finds that “residential violence”—whether from neighbors or the state—produced widespread isolation and disengagement. Many of the women described strategies to avoid interaction with neighbors in order to reduce their exposure to potential harm. They write, “. . . silence, and invisibility were their protective mechanisms of choice” (21). In relation to the plight of a specific isolated woman in the study, they explain that “. . . .heightened anxiety about whether her neighbors would target her or her children meant Laura lived in constant fear of residential violence” (21). The notable insight from this work is the centralization of fear and isolation as mechanisms of political depression.

Synthesizing findings from previous research in comparative and American politics, I advance a theory of the depressive effect of violent exposures on political participation in U.S. cities. Specifically, *I expect that as residential proximity to sites of homicide increases, voter turnout will decrease (H1).* The geographic concentration of violence in U.S. cities may create a unique spatial context characterized by an elevated threat of violence. Specifically, I claim that these highly violent *contexts* play a crucial role in shaping how individuals perceive and experience these events, distinguishing my theory from recent studies on the electoral effects of relatively rare and geographically sporadic instances of mass shootings (Garca-Montoya, Arjona, and Lacombe 2022), which find no effects on turnout. In alignment with previous work, I argue that proximate violence exposure in violent contexts may depress turnout by *H2) undermining perceptions of state legitimacy and H3) elevating individuals' perceptions of their risk of victimization, leading to social and political isolation.* As detailed in later sections, I perform additional analyses to test my expectations that both mechanisms drive observed depressive turnout effects.

Feedback Effects: Violent Incidents as Policy Signals

A key insight from prior research is that violent events can signal state failure to ensure public safety, undermining trust and reducing incentives for political engagement. This causal logic connects to the “feedback effects” literature in American politics. Here, institutions and policies are not only products of politics; they also reshape it by influencing how individuals and groups perceive the state, form political identities, and build or withdraw from civic life (Michener 2018; Mettler and Soss 2004; Pierson 1993; Skocpol 1995).

This project builds on work examining the democratic consequences of interactions with the “second face” of the state—institutions that govern through social control and coercion like policing and incarceration (Soss and Weaver 2017). Scholars here study the effects of *direct*, *proximate*, or *community contact* with carceral institutions. Research on direct contact—such as arrest or incarceration—generally finds mixed effects on trust in government and voter turnout (Gerber et al. 2017; Lerman and Weaver 2014; White 2019b; Peyton, Sierra-Arévalo, and Rand 2019). "Proximate contact" (Walker 2014)—via friends, family, or neighbors—may also shape civic engagement by increasing perceived surveillance and weakening social capital (Lerman 2013; Burch 2013; White 2019a).

Here, I use the term proximate to refer specifically to *geographic proximity* to homicide, rather than relational contact, following Ley (2018)'s call to examine how violent contexts shape behavior. My framework aligns most closely with community contact models, where people encounter state power through diffuse exposure to incidents in their environment (Morris and Shoub 2023). Using geographically proximity as a proxy, Morris and Shoub find that proximity to police killings can increase turnout within narrow spatial thresholds, theorizing that these deaths act as catalytic "policy" events, politicized by social movement actors. However, such mechanisms likely do not apply to homicide victimization, which has historically failed to generate national mobilization on the scale of the Black Lives Matter movement (Goss 2010).

Although this research has illuminated how carceral contact influences participation, it often overlooks a critical feature of these same communities: persistent exposure to civilian violence. This omission has important implications. If, as comparative politics suggests, violence signals state weakness or failure, ignoring its prevalence limits our understanding of how political orientations are shaped—not only by state intervention, but also by its absence. This study aims to address that gap.

Policy Feedback in a Racialized Polity

Thus far, I've argued that the spatial concentration of violence can create enclaves where fear and perceptions of state neglect are heightened. While these dynamics could affect any group living in such contexts, in the U.S., these areas are disproportionately Black. Policy feedback research shows that interactions with the state—or its absence—shape political behavior, often in racially contingent ways (Lerman and Weaver 2014; Mettler 2007). As Michener (2019) explains, "Disproportionality. . . funnels policy resources unevenly, serves some interests better than others and influences *interpretations of policies*" [428, emphasis added]. Although White Americans experience more violence than peers in other democracies, the U.S. Black–White homicide victimization ratio is nearly 5:1 (BJS 2019), reflecting starkly unequal experiences with the state as a security provider. I argue that racial differences in both the frequency and context of exposure—both historically and in the present—shape how violence is interpreted and how individuals respond.

The racially disparate frequency of homicides may produce different electoral responses via both of my proposed mechanisms. First, repeated exposure in some communities may disproportionately heighten perceived risk and fear. Research has documented cycles of retaliatory violence—“ping-pong murders”—within affected neighborhoods (Kubrin and Weitzer 2003). Given the racial concentration of these incidents, Black Americans may bear a greater psychological burden and fear of recurrence than others.

Second, the enduring concentration of violence in Black communities may uniquely erode perceptions of state legitimacy. In Chicago, violence has remained concentrated in predominantly Black neighborhoods despite overall crime declines (Sharkey and Marsteller 2022). This persistent exposure can foster deep distrust in the state and reinforce dynamics of delegitimation, aligning with sociological research on the prevalence of “legal cynicism”—a cultural frame in which the law is seen as illegitimate, unresponsive, and incapable of ensuring public safety. Notably, Kirk and Papachristos (2011) find that legal cynicism distinctly marks persistently violent, low-income neighborhoods in Chicago, setting them apart from similarly disadvantaged but nonviolent areas. As Neumann (1957) argues, increased anxiety about victimization—combined with a sense of group marginalization—can fuel political alienation. This is especially relevant for Black Americans, given a longstanding history of perceived state failure and abuse in the criminal justice system (Beckett and Francis 2020).

Situational context also matters. Black victims are more likely involved in drug-related homicides, while White victims are overrepresented in workplace and sex-related killings (BJS 2007). These distinctions shape public interpretation: drug-related deaths may raise fears of recurrence and signal the state’s failure to address illicit markets, while other forms may not elicit the same concerns. Thus, *I expect the demobilizing effects of proximate homicides to be strongest in predominantly Black neighborhoods (H4a), particularly when the victim is also Black (H4b).*

Empirical Strategy

Study 1: Estimating Effects of Violence on Voter Turnout

[Figure 1 about here]

The core objective of this paper is to empirically test the effect of violent exposure on an individual's likelihood to engage in civic processes, such as voting. Building on previous research, I examine whether residential proximity to a homicide has a causal effect on electoral participation, exploring how violence may contribute to isolation from formal political institutions. I begin by describing the three core datasets I use for estimation.

First, I draw upon geocoded homicide data made publicly available from the Washington Post's data collection initiative. They collected data on a handful of cities from 2007 to 2016, including data from the 50 most populous cities over this time period. Closely aligned with the FBI's Uniform Crime Reporting Program's definition of homicide and FBI records, the data include murder and non-negligent manslaughter but exclude suicides, accidents, justifiable homicides, deaths caused by negligence, mass shootings, and terrorist attacks.[4] They map over 52,000 homicides during this period, with data that includes geocoded incident locations and the race of the victim.

I link these homicide data with snapshots of the U.S. voter file from L2 political within the range of data available via the Washington Post's data (2014 to 2016). These registered voter files are geocoded, and indicate whether an individual voted in the particular election. While it is theoretically conceivable that any politically depressive effects of proximate exposure to homicide may linger for a significant period post-exposure, testing the long-term effects of residing in high-violence communities is beyond the scope of this paper. The evidence I present only sheds insight on proximate exposure within the weeks before and after the 2014 and 2016 national elections. Figure 1 maps the Washington Posts' homicide data within the full range of election years used in these analyses. I use aggregated individual-level data to calculate the number of ballots cast in each block group for the corresponding election. I then perform spatial mapping to measure the distance of the homicide to potential voters' block groups using the population centroid shape files provided by the U.S. Census Bureau.[5] I performed spatial mapping of census block group centroids to the geocoded homicide data in R markdown using the *sp* and *search trees* packages (Pebesma and Bivand 2005).[6]

[4]A homicide is considered "justifiable" when permitted by law, such as in cases of self-defense or executions for capital crimes. In Virginia, for example, a killing in self-defense is justifiable if the person acted without fault and under reasonable fear of death or serious harm.

[5]Following Morris and Shoub (2023), I rely upon block group centroids, which indicate the point to which the population has the smallest possible sum of squared distances (center of population), overcoming the limitations of relying on geographic center.

[6]I conduct my analysis at the census block group level because it enables the use of precise Census microdata to assess potential differences between groups. Although the L2 voter file provides modeled estimates for characteristics like race and income, these estimates are often inaccurate and vary significantly across states. Additionally, the L2 data lack key demographic variables such as education levels and population density at the block group level.

[Table 1 about here]

Table 1 reports the racial composition of block group homicide exposures within one mile and two months of the 2014 and 2016 elections. Reflecting broader patterns of American urban violence, nearly half of these exposures occurred in predominantly Black neighborhoods, followed by 27% in plurality-Latino areas, 20% in predominantly white areas, and 3% in neighborhoods with a plurality of other racial or ethnic groups. Given the unequal distribution of violence, mechanisms of fear and perceived state neglect may be especially salient in Black communities. In the appendix, violin plots illustrate that block groups unexposed to violence generally exhibited higher turnout, especially when disaggregated by racial plurality (Figure A8, 26). However, because exposed communities also disproportionately experience poverty and carceral contact, a causal design isolating the effect of violence is especially valuable. I now turn to my empirical strategy, which leverages the timing of exposure to strengthen causal identification.

Effects of Residential Proximity to Homicide on Voting

To estimate the causal effect of residential proximity to homicide on voter turnout, I utilize a regression discontinuity in time (RDiT) design. Leveraging homicide timing relative to election dates as a quasi-random assignment mechanism, I compare block groups exposed just before the election (treated) to those exposed just after (control). This approach may provide an unbiased estimate of the effect, provided that the precise timing of the homicide is not systematically related to other factors highly associated with turnout. Given that the analysis relies on the regression of a discrete running variable (days to/from election) on turnout, I follow Cattaneo, Idrobo, and Titiunik (2024)'s recommendation to analyze the RDD under local randomization assumptions rather than a the continuity-based framework.[7]

Figure 2 presents a regression discontinuity plot of homicide exposure and voter turnout, with each dot representing a block group within 0.25 miles of a homicide in the two months surrounding the 2014 and 2016 elections. The fitted polynomial lines show a discernible discontinuity at the election date cutoff, suggesting turnout declines in affected areas. Of course, it's important to note that the observed discontinuity

[7] There are two main reasons for adopting the local randomization approach over continuity-based estimation. First, because the running variable is discrete, local randomization avoids the shortcomings of relying on local polynomial methods, which treat each discrete value as a unique observation. When mass points are limited or unevenly distributed near the cutoff, continuity-based methods become less reliable without restrictive assumptions. Since my goal is to estimate hyper-local effects on turnout, focusing on the smallest windows allows for more precise estimation in the most comparable block groups. Second, local randomization requires formal validation of the randomization assumption via window selection, which improves transparency around covariate balance and sample composition across bandwidths.

may not necessarily be statistically meaningful due to the arbitrary bandwidth selection and potential demographic differences. The RDiT advantage lies in leveraging precise exposure timing for causal inference, resting on the assumption of comparable groups on either side of the cutoff.

[Figure 2 about here]

To strengthen covariate balance on key variables across treatment groups—including racial composition, income, age, education, population density, and prior homicide exposure—I use optimal propensity score matching via the *MatchIt* package in R. Optimal matching improves overall balance by minimizing the total distance across all matched pairs while retaining one-to-one matching. While I do observe natural parity in the most narrow geographic thresholds, longer distances are vulnerable to more demographic differences across treatment groups, likely due to the seasonal increases of homicides during warmer seasons.[8] By accounting for key demographic and socioeconomic covariates, this approach helps reduce confounding and better isolate the treatment effect. Alternative matching approaches—including nearest neighbor matching with a caliper of 0.2 and no replacement, as well as models estimated without matching—are presented in the Appendix as robustness checks (Tables A17-A21 (11-12)).

[Table 2 about here]

To increase power and precision, the primary models presented in this manuscript use turnout data from registered voters in the 2016 presidential election and include 2014 midterm turnout as a lagged predictor to account for prior voting behavior (see Figure A10 (28) for 2014 election results). Table 2 presents a diagnostic assessment of covariate balance, comparing treatment and control groups located within 0.25 miles of a homicide around the 2016 election. Estimates are based on the largest window of [-15,15] at this threshold,

[8]Supplementary Information (SI) Figure A1 (15) supports this point, aligning with prior research by showing a slight increase in homicide exposures in the two months before the election. One potential source of imbalance is that blocks experiencing seasonal upticksmaking them more likely to be exposed pre-election may, on average, have a higher Black population than those solely exposed post-election. While this does not appear to pose a significant threat to randomization at the narrowest thresholds, the concern may grow as the radius around the homicide expands.

using Fisherian p-values.[9] This means the treated group includes homicides occurring up to 15 days before the election, while the control group includes those occurring up to 15 days after (including election day). The local randomization framework assumes that, within this narrow window, the timing of homicide is effectively random, ensuring that the treated and control groups are statistically comparable. While this approach substantially reduces bias in the treatment estimates—with no covariate conventionally statistically different across treatment and control groups—Table 2 reveals minor residual imbalances. For instance, the treatment group has a slightly lower median income and population density. To address these residual differences, I include these covariates in the difference-in-means estimates, along with a lagged outcome variable for the block group's turnout in the previous 2014 election. (Table A2 (4) presents similar results without covariate adjustment).

Treatment Effects

I estimate the local randomization RD treatment effect of proximate homicide exposure on voter turnout using the *rdrandinf* function developed by Cattaneo, Titiunik and Vazquez-Bare (2016). A common critique of manually selecting windows (days) around the cutoff in RD analyses is the lack of transparency and objectivity. To address this, I follow Cattaneo, Idrobo, and Titiunik (2024) and use the `rdwinselect` function in R, which implements a data-driven approach to identify the "data-optimal" estimation window. This method begins with the smallest possible window and iteratively expands until covariate balance is achieved (defined as $p > 0.10$ for all covariates after matching). I begin by reporting results for these optimally selected windows.

Table 3 displays difference-in-means estimates with Fisherian p-values and 95% confidence intervals for block groups across treatment status, using varying distance thresholds based on the optimal window bandwidths, ranging from 0.25 miles to 2 miles.[10] As a reminder, the control group consists of block groups exposed in *t2* (i.e., after the election), while block groups exposed both before and after within the same threshold are excluded to maintain comparable exposure levels.[11] Additionally, for block groups exposed multiple times within the pre or post-election period, I select the most recent exposure-observation on both sides of the cutoff. The tables also include the number of block groups within each threshold, on either side of the cutoff.

[9] In the local randomization framework, Fisherian p-values and confidence intervals are employed for finite samples. These CIs are derived by inverting randomization-based hypothesis tests, rather than relying on asymptotic approximations typically used for large samples (Cattaneo, Idrobo, and Titiunik 2024).

[10] Optimal-window covariate balance is for each threshold is presented in Tables A5-A10 (7-8).

[11] For exposure thresholds above 0.25, I use a modestly asymmetric window of [-5,10] to improve covariate balance and retain sufficient observations in both groups. This choice is guided by empirical balance checks and the need to preserve internal validity while maintaining power.

[Table 3 about here]

As shown, I consistently find depressive effects of homicide exposure on voter turnout, which begin to weaken at a one-mile radius. Within the narrowest threshold of .25 miles, the treatment effect roughly 6 percentage points is significant at the ($p < .05$) level.[12] At the broader 0.50-mile threshold, the local average treatment effect (LATE) is estimated at a 2.4 percentage point decline. At 0.75 miles, the effect remains statistically significant, estimated at approximately 2 percentage points. Although each of these estimates reaches conventional levels of statistical significance ($p < .05$), the effects begin to attenuate around the one-mile threshold, where they are only marginally significant ($p < .08$). No discernible effect is detected beyond one mile.

Robustness Checks and Sensitivity to Window Selection

Next, I present results across different estimation windows. Similar to how researchers using a continuity-based regression discontinuity approach assess sensitivity to bandwidth choice, evaluating the sensitivity of effects to window selection serves as a natural robustness check in a local randomization framework. I select windows that maintain balance on predetermined covariates (p < 0.05), ensuring that treatment and control groups remain statistically comparable within each window. Covariate balance tests for each of these windows can be found in SI Tables A11-A16 (9-10).

[Figure 3 about here]

Figure 3 presents difference-in-means estimates for block groups exposed to homicide within varying distance thresholds, ranging from 0.25 miles to 2 miles. Within the narrowest threshold (0.25 miles), I observe a significant turnout decline of 2 to 6 percentage points. The trade-off with small windows is that they may contain fewer observations, potentially reducing power and leading to marginally significant estimates (grey dots). To address this, I extend the window from 5 to 15 days before and after the election and match for stronger covariate balance, thereby increasing the number of observations in both the treatment

[12] Across thresholds, I define the optimal window as the smallest bandwidth that achieves covariate balance while excluding the [1, 0] interval. This exclusion helps avoid potential spillover from homicides occurring earlier on election day or from treatment effects that may begin to manifest the day after the homicide (i.e., on day 1) rather than precisely on day 0.

and control groups within this threshold. At wider, covariate-balanced windows, I observe more consistent depressive effects on turnout.

[Table 4 about here]

Within 0.50 miles, homicide exposure is associated with a 2 to 3 percentage point decline in turnout, with six out of seven windows producing statistically significant estimates ($p < 0.05$). The farthest distance at which consistent effects are detected is 0.75 miles, where turnout declines by approximately 2 percentage points ($p < 0.05$). Beyond this threshold, the strength of the effects begins to dissipate, reinforcing prior findings that the behavioral impact of proximate homicide exposure is geographically concentrated. This spatially localized pattern is consistent with prior research on non-state sanctioned violence. For instance, Sharkey (2010) finds that acute exposure to homicide significantly lowers children's test scores, with the strongest effects observed at close geographic proximity. Similarly, my results show that effects weaken with distance: while the one-mile threshold yields one estimate significant at the 0.05 level and another marginally significant ($p < 0.08$), falsification checks at 1.5 and 2 miles show no discernible effects, reinforcing the conclusion that the impact of homicide exposure is highly localized.

Additionally, while the *rdlocrand* package does not natively support clustering in permutation tests, a single homicide event may affect multiple proximate block groups. As a robustness check, I estimate OLS models with standard errors clustered at the individual homicide level to account for potential within-cluster dependence. These models rely on standard asymptotic inference to test for differences in means between treated and control groups. This complements the randomization-based inference used in the primary analyses (via rdlocrand), which tests the sharp null hypothesis of no treatment effect for any unit using permutation methods. Table 4 presents the resulting treatment effects across the largest distance thresholds shown in Figure 3. These models include the full set of covariates listed in Table 2, as well as a lagged outcome variable for each block group's turnout in the 2014 election (see Table 22 (13) for results without covariate adjustment). In line with the local randomization results, there is no evidence of consistent effects beyond 0.75 miles.

While the appendix of this manuscript provides a more thorough discussion of my robustness checks, I note here that my results are robust to several other statistical specifications. Table A25 (17) includes a series of placebo regressions, where I shift the cut-point from five days before to five days after the election, to test whether the election day cut-point is meaningful. In Tables A5-A10 (7-8), I conduct falsification tests of theoretically relevant covariates on treatment assignment, as well as density tests of block group exposures near the cutoff to rule out potential manipulation of block groups into the treatment or control groups (Table A23, 14).

Estimating Heterogenous Effects by Race

Figure 4 presents estimates by the racial composition of the block groups and the race of the victim. Recall from hypotheses 4a and 4b that I anticipated disparate effects by race. In an earlier section, I outlined three potential reasons for these expectations, which I won't regurgitate in full here. However, I've argued that the racial and geographic concentration of violence in certain block groups may disproportionately drive effects in communities most likely to re-experience these events. As highlighted in Table 1, roughly half of the block groups exposed to homicide within a mile in 2014 and 2016 were plurality Black.[13]

While one might be inclined to theorize that high levels of violence nationally may provoke strong feelings of insecurity across the non-Black population, I find weak evidence in support of this proposition. As shown in Figure 4a, the depressive effect appears to be concentrated in block groups where Black residents represent the largest plurality. Among those exposed within 0.25 miles, estimated effects range from 3 to nearly *10* percentage points.

Figure 4b presents effects by victim race, similarly showing declines in turnout of three to ten percentage points for block groups exposed to Black victims. Exposures involving other groups yield null results. Two explanations are plausible. First, a victim's race may signal recurrence risk, given differences in homicide context—for instance, Black victims may more often be involved in drug-related or retaliatory killings, where one event raises the chance of another. Second, as shown in SI Figure A6 (22), Black victims are disproportionately represented across all block group types, increasing power to detect effects. I find no evidence of turnout decline in plurality-Latino or White block groups; however, wide confidence intervals around the White estimates suggest imprecision, cautioning against definitive conclusions.

Finally, to account for within-cluster dependence by homicide event, I re-estimate the models using OLS with standard errors clustered at the homicide level (Table A26, 18). Results remain significant and consistent with the heterogeneous patterns reported here.

[13]See SI for a discussion of the saturation of block group exposure by racial composition (Table A27, 25).

[Figure 4 about here]

These findings support hypotheses *H4a* and *H4b*: if fear drives political withdrawal, the recurrence of violence in Black communities and the context of Black victimization may intensify perceived risk. Consistent with this theory, Appendix Figure A5 (21) shows that effects are most consistent in block groups with higher prior-year exposure, where recurrence is more likely. At the same time, smaller effects in other areas may reflect limited power due to smaller sample sizes. I now turn to additional analyses that help adjudicate between fear and delegitimacy as driving mechanisms.

Mechanism Analyses

Study 2: Effects of Homicide Exposure on Foot-Traffic

Previous work has highlighted two potential mechanisms that may help explain what drives the negative effect of localized homicide exposure on turnout: 1) violence causing a psychological burden of fear, leading to isolation and 2) violence evidencing state failure to perform a core function, leading to delegitimacy and withdrawal from political engagement. I begin by exploring the mechanism of fear specifically, using data on foot traffic. I then offer analyses of survey data that test the relative strength of these two potential drivers.

[Table 5 about here]

To proxy for fear that might be induced by violence, I employ the Neighborhood Patterns dataset collected by Advan Research. The dataset includes estimates of daily footfall data aggregated by census block group across the U.S. Specifically, this dataset aggregates raw counts (measured as a stay of at least one minute) of visits to block groups from a panel of mobile devices, offering insights into visitation frequency, duration of stays, origins, subsequent destinations, and more.

[Figure 5 about here]

In alignment with the sentiments of fear and isolation highlighted by Moffett-Bateau (2023), I examine whether proximate exposure to homicide reduces foot traffic for affected census block groups. If fear drives social withdrawal, such exposure should lead to measurable declines in local movement. An advantage of analyzing foot traffic data is that it allows me to extend my examination of homicide's effects beyond purely electoral contexts, potentially strengthening the generalizability of the findings. Thus, I link the Advan block group data with homicide data for geo-spatial analysis. Unfortunately, the historical block group data only go back to 2019, preventing a simple mapping using the WP database (which ends in 2017). To overcome this limitation, I manually collected data from a handful of the largest U.S. cities that make their *geo-coded* homicide data publicly available.[14] To avoid population movement issues related to the COVID-19 pandemic, I focus my analysis on 2022 and 2023, two years well clear of regional variation in stay-at-home mandates. Table 4 gives an overview of the city locations, homicide counts, and the demographic composition of linked block groups for the crime data collected.

Motivated by the localized effects observed in the RDiT analysis, I present block group exposure within a narrow bandwidth of 5 days. Using fixed effects models, I compare visit counts before and after exposure, *within census block groups* (see online appendix for model equations (2) and visit count visualizations (30)). To account for potential spill-overs from multiple block group exposures, I remove any block group-homicide exposures within 30 days from the dataset, leaving roughly 32,600 unique block group-exposures over the course of 2022 and 2023. Figure 5 illustrates the total stop visit counts for the subset of these block groups exposed within .75 miles of a homicide during 2022 to 2023, by city.

[Table 6 about here]

Table 6 presents results from fixed-effects regressions estimating the impact of proximate homicide exposure on daily foot traffic across varying distance thresholds. All models include block group fixed effects and are weighted by the square root of average pre-treatment visits, with standard errors clustered at both the individual homicide and block group levels to account for within-event and within-unit correlation.

[14] See Tables B1 and B2 (31-32) in online appendix for details on city selection and crime report validation using FBI records.

Model 1, limited to block groups exposed within 0.3 miles, finds a reduction of roughly 16 visits in the five days following exposure—a decline of roughly 3.4 percentage points relative to the average of 472 visits. Model 2 estimates a reduction of 15 visits for block groups exposed within a 0.5-mile radius. Expanding the threshold to all block groups exposed within 0.75 miles, Model 3 estimates a smaller decline of 11.7 visits.

[Table 7 about here]

Turning to Table 7, I also restrict the sample to busier block groups, defined as those with an average daily visit count over 100 during the pre-treatment period (5 days prior to exposure). Across distance-exposure thresholds, I observe a strong negative effect on visits. In the case of those exposed within .3 miles, I observe a large decrease of 21.26 visits, a roughly 3.5 percentage point decrease in visits (average pre-treatment visits for these models is 610). Those exposed within .5 miles had an average depressive effect of 19.8 visits, respectively. For all of the block groups exposed (Model 3), the effect is estimated at 15 fewer visits.[15] In alignment with expectations, the results of my models suggest a strong behavioral response to proximate exposures to homicide as individuals avoid these block groups. These results offer support for the mechanism of fear driving observed effects on turnout.

Study 3: Adjudicating Between Mechanisms in Survey Data

This paper has presented causal evidence that proximate homicide exposure depresses voter turnout in federal elections. To probe the mechanism of fear, I also analyze foot traffic data to examine shifts in population movement following nearby acts of violence. While these patterns offer suggestive evidence that fear may partly drive the observed effects, it is not the only explanation emphasized in prior theoretical accounts. Another important mechanism is the perceived delegitimization of political institutions. As Rotberg (2004) argues, violence may signal the state's failure to fulfill a core function: maintaining order through its exclusive authority over the use of force. Because prior work suggests this mechanism may unfold over longer time horizons, I complement the causal analysis with descriptive evidence from survey data relevant to the mechanisms in question.

[15] While this analysis cannot rule out the influence of police activity, such as cordoning off areas, the persistence of social isolation beyond the temporary presence of police suggests other factors, like trauma or fear, are more influential. In the apppendix, I replicate the analysis with a 10-day threshold (Table B3, 34), and the results remain consistent.

Specifically, I rely upon the Race and Carceral State Survey (RCSS), collected in the summer of 2017 by A. Anoll and Israel-Trummel (2017). The survey includes a nationally representative sample of White and Black Americans, with a majority of questions focusing specifically on measures of carceral state contact, perceptions of state institutions, and social contexts. I link each respondent's zip code with their respective county-level crime data for the preceding year (2016), compiled by the FBI Uniform Crime Reporting Program. Together, the linked data include responses for roughly 10,130 individuals.[16]

While the FBI data offer valuable coverage, a key limitation is the use of counties—relatively large geographic units that can obscure important within-county variation. Because violence is often hyper-concentrated in specific neighborhoods or block groups, individuals' exposure may differ widely within the same county, potentially masking effects among those most directly impacted. Nevertheless, the breadth of the county-level sample enables a more precise analysis of how fear and trust vary by racial group across the spectrum from the least to the most violent counties (see Figures C1-C2 (39-40) for illustrations on this point). Still, to further investigate this relationship at a more granular geographic level, I reverse geocoded homicides from the WP database to their corresponding zip codes, linked aggregate homicide counts to RCS survey responses, and replicated the county-level analyses at the zip-code level (Table C4, p. 38), yielding similar results, albeit with a smaller sample.

[Table 8 about here]

I focus on two questions that may shed light on the role of the mechanisms of both fear and state delegitimacy. Speaking to the mechanism of fear, I rely on the question asked on a five-point scale (0 - "None of the time"; 4 - "Very worried"), "How worried are you that you or a member of your family might be a victim of a serious crime?" For the mechanism of state delegitimacy, the RCSS survey asks respondents on a four-point scale (0 - None of the time; 3 - Just about always), "How much of the time do you think you can trust the government in Washington to do what is right?" All questions are scaled from 0-1 for interpretability.

Table 8 examines how local violence relates to fear of victimization using a series of OLS models, with standard errors clustered at the county level. Models 1–3 use the standardized county-level violent crime rate (VCR) as the independent variable. As shown, the VCR is positively and significantly associated with

[16] Appendix Table C1 (35) shows the demographic distribution of respondents in the dataset.

fear, even after adjusting for covariates such as race, income and age. Model 3 introduces a race interaction, revealing that white respondents exhibit greater shifts in reported fear as violent crime rises, while Black respondents—who consistently report higher baseline levels of fear—are less responsive to county-level variation. This suggests that changes in the surrounding environment may more strongly influence white residents' perceptions, whereas Black residents may experience fear more as a constant, shaped by persistently high exposure.

Models 4–6 turn to the standardized county-level homicide rate. In Model 5, the homicide rate is not significantly associated with fear once covariates are included. However, Model 6 includes a race interaction and finds that higher homicide rates are significantly associated with increased fear among white respondents but not Black respondents. Again, Black respondents report consistently higher levels of fear across all models. This pattern may reflect the racialized geography of violence: if homicides are disproportionately concentrated in Black neighborhoods, county-level variation may have less salience for Black respondents. In contrast, rising violence may signal a broader diffusion into white or mixed areas, increasing fear among white residents.

[Table 9 about here]

Table 9 turns to the second hypothesized mechanism linking violence to political behavior: trust in government. Models 1 and 2 show that county-level violent crime is negatively and significantly associated with trust, suggesting that residents in higher-crime areas report less institutional trust. However, this relationship does not differ significantly by race, as the interaction term in Model 3 is not significant. Models 4 through 6 shift to the homicide rate and find no meaningful relationship—neither the main effect nor the interaction with race is statistically significant. Notably, white respondents report higher trust in government across all models. These results suggest that, unlike fear of victimization, trust in government is less responsive to local violence and exhibits more stable racial patterns.

This pattern is especially striking given that the outcome references trust in the federal government, paralleling the national elections at the heart of my turnout analysis. While Models 1 and 2 show a small but significant negative association between violent crime and trust, the overall evidence for trust as a mechanism is limited. Taken as a whole, the results point to fear as a potentially more immediate and potent mechanism linking proximate violence to political disengagement. This dynamic appears strongest in communities with persistently high levels of violence, where the burden disproportionately falls on Black Americans.

Discussion

In comparative politics, persistently high levels of violence are often interpreted as indicators of state weakness or instability, potentially eroding individual incentives for civic engagement. Yet in the U.S., the democratically subversive nature of high homicide rates has been largely overlooked. Mapping potential voters to homicide sites in U.S. cities, I examine whether residential proximity to violence depresses formal political participation, specifically voting.

Using a RDiT design, I find that exposure to homicides, within roughly three-fourths of a mile reduces turnout. This decline ranges from approximately two to six percentage points, with the strongest effects observed in predominantly Black block groups and those involving a Black victim. To test fear as a mechanism, I analyze foot-traffic data and find significant declines in movement following exposure, consistent with theories of fear-induced social withdrawal. Finally, to adjudicate between the mechanism of fear and political delegitimacy, I also link survey data with local violence rates. Across models, I find the strongest evidence for a positive relationship between rates of violence and fear of victimization.

While I find weaker evidence for state delegitimacy as a mechanism, the limitations of inference should be noted. For one, I'm constrained by data availability, limiting my ability to test acute changes in sentiments of state legitimacy. Given that prior research has suggested longer-term effects of residing in violent contexts, I analyze my survey data with a one-year lag in local violence rates and still find no support. Nonetheless, more research is needed to understand the potential longer-term effects of proximate violent exposure on participation, as well as the relative strength of its potential drivers.

Normative democratic theory asserts that individuals consent to the socio-political order in exchange for basic public services and goods, with a foundational tenet being political equality. This principle guarantees meaningful and equitable opportunities for participation, most clearly represented by equal access to voting (Dahl 1998). However, the unequal provision of public safety may signal a breakdown in these principles, as violence remains geographically concentrated and racially uneven. This paper presents evidence that disproportionate exposure to violence in predominantly Black urban enclaves may impose significant costs—particularly the risk of victimization—on affected communities, leading to a substantial depressive effect on electoral participation. These findings have important implications for urban politics and democratic governance more broadly.

For one, the field of comparative politics has rightfully shed light on the challenges of implementing democratic norms in developing contexts characterized by high levels of violence and weak state capacity (Arias and Goldstein 2010). However, while this work has sought to isolate the effects of various forms of violence facilitated by the state and organized criminal groups, it has largely overlooked the "non-political" forms of civilian violence that are pervasive in many urban contexts globally. Research across the social sciences has demonstrated the adverse effects of geographically-proximate exposure to violence on a wide range of outcomes (Heissel et al. 2018; Sharkey and Torrats-Espinosa 2017). However, this study is the first to provide causal estimates of the depressive impact of civilian violence on civic engagement specifically.

Additionally, as mentioned, there is a growing body of research in American politics concerned with understanding how policy interventions shape political and social orientations within a broader racialized polity. A strain of such work has focused on estimating participatory effects for the significant number of Americans who have had various forms of contact with criminal justice actors or institutions. Recent work, for example, has offered some evidence that proximity to state sanctioned violence via police killings has a mobilizing effect on community members (Ang and Tebes 2024; Morris and Shoub 2023). Still, while this research has undoubtedly illuminated key processes of political socialization in impacted areas, it has often overlooked the high levels of "non-political" violence present in these communities. This is a notable gap, limiting our ability to contextualize the relative strength and durability of the effects observed in existing work.

Lastly, while democratic theory suggests that high rates of violence should spur collective efforts to demand effective public safety reforms, disparate exposure—resulting in social isolation and muted mobilization—may help explain the relatively "missing movement" for government accountability around crime and violence in the U.S. (Goss 2010). It therefore may also shed light on a common critique of the American criminal justice system: that it has historically operated incongruently—and often counterproductively—relative to the needs of Black Americans, who arguably have had the greatest stake in its effective functioning.

The analyses presented here highlight the persistent depressive effects of non-state-sanctioned violence. A substantial body of work in American politics has systematically examined racial differences in electoral participation, identifying a range of factors that shape political engagement and efficacy (Fraga 2018; A. P. Anoll 2022). This study aligns with research in political science that emphasizes the role of spatial context and socialization in shaping political behavior. The negative participatory effects of geographically concentrated homicide exposure in the U.S may lead us to reconsider its broader democratic implications and challenge our frameworks for defining which forms of violence are inherently "political."

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Figures and Tables for Manuscript

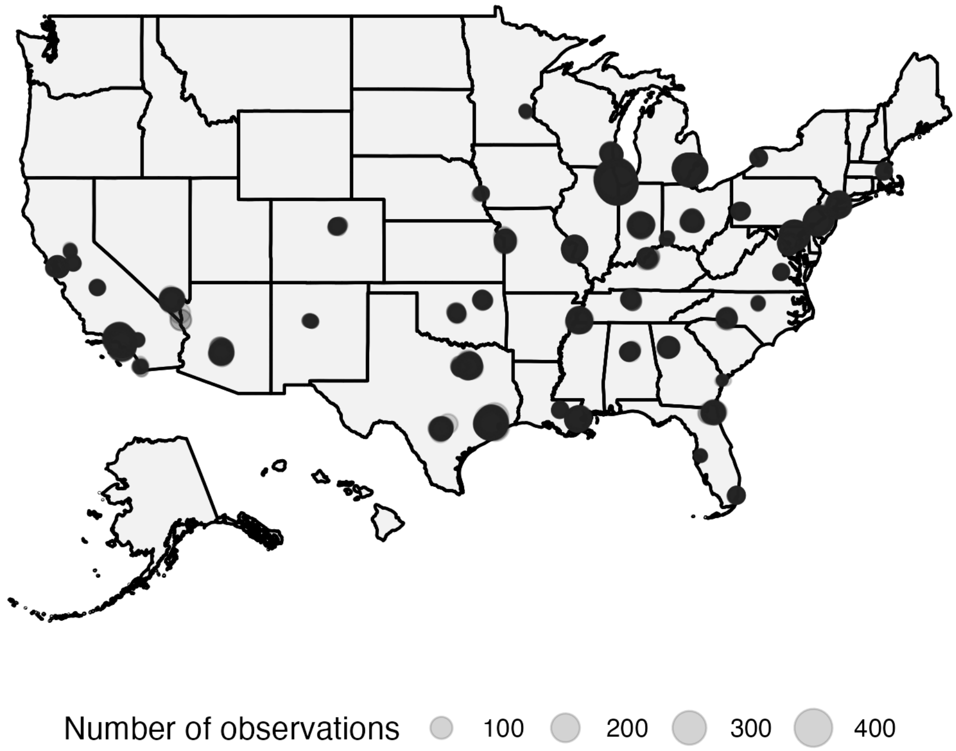


Figure 1: Washington Post Homicides within 2 Months of 2014 and 2016 Elections

Table 1: Racial Demographics of Homicide Exposures Within 1 Mile

Race	Block group exposures	Proportion of all exposures
Plurality black	40217	0.515
Plurality latino	20825	0.267
Plurality other	2166	0.028
Plurality white	14861	0.190

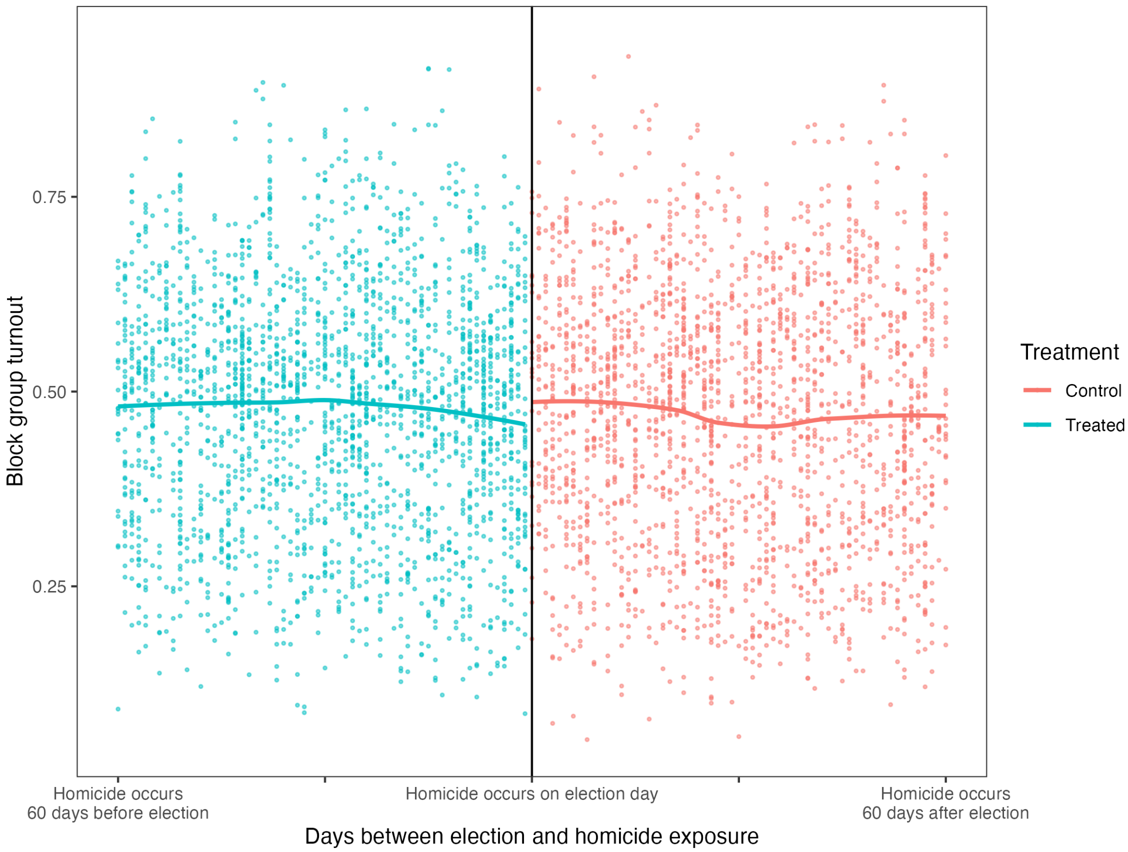


Figure 2: Regression Discontinuity Plot of Voter Turnout in Block Groups (dots) within 0.25 Miles of Homicide. Each dot represents a block group in the raw (unmatched) data. Block groups exposed in both the pre and post-election periods are excluded.

Table 2: Window Selection [-15,15] | Exposed within .25

Covariate	Difference	Fisherian p-value	Treated	Control	Treated n	Control n
% Latino	-0.001	**0.963**	0.263	0.264	321	321
% White	0.005	**0.785**	0.150	0.146	321	321
% Black	-0.002	**0.938**	0.524	0.526	321	321
Median income	-887.670	**0.620**	34816.984	35704.654	321	321
Median age	-0.107	**0.881**	34.538	34.645	321	321
Population density	-455.889	**0.826**	22985.360	23441.249	321	321
Some college	0.008	**0.594**	0.419	0.411	321	321
Previous year exposures	-0.826	**0.344**	11.125	11.950	321	321

Table 3: Optimal Window Selection by Distance

Model	Treated	Control	Difference	Fisherian p-value	95% confidence interval	Treated n	Control n
Within .25 miles [-2,1]	0.539	0.598	-0.059	**0.020**	[-0.100, -0.010]	38	32
Within .50 miles [-5,5]	0.557	0.581	-0.024	**0.010**	[-0.040, -0.010]	318	181
Within .75 miles [-5,7]	0.567	0.585	-0.018	**0.008**	[-0.030, -0.010]	559	436
Within 1 mile [-5,10]	0.577	0.589	-0.012	**0.078**	[-0.025, -0.000]	504	504
Within 1.5 miles [-5,9]	0.613	0.622	-0.009	**0.192**	[-0.020, -0.000]	674	633
Within 2 miles [-5,10]	0.643	0.650	-0.007	**0.297**	[-0.015, 0.005]	785	785

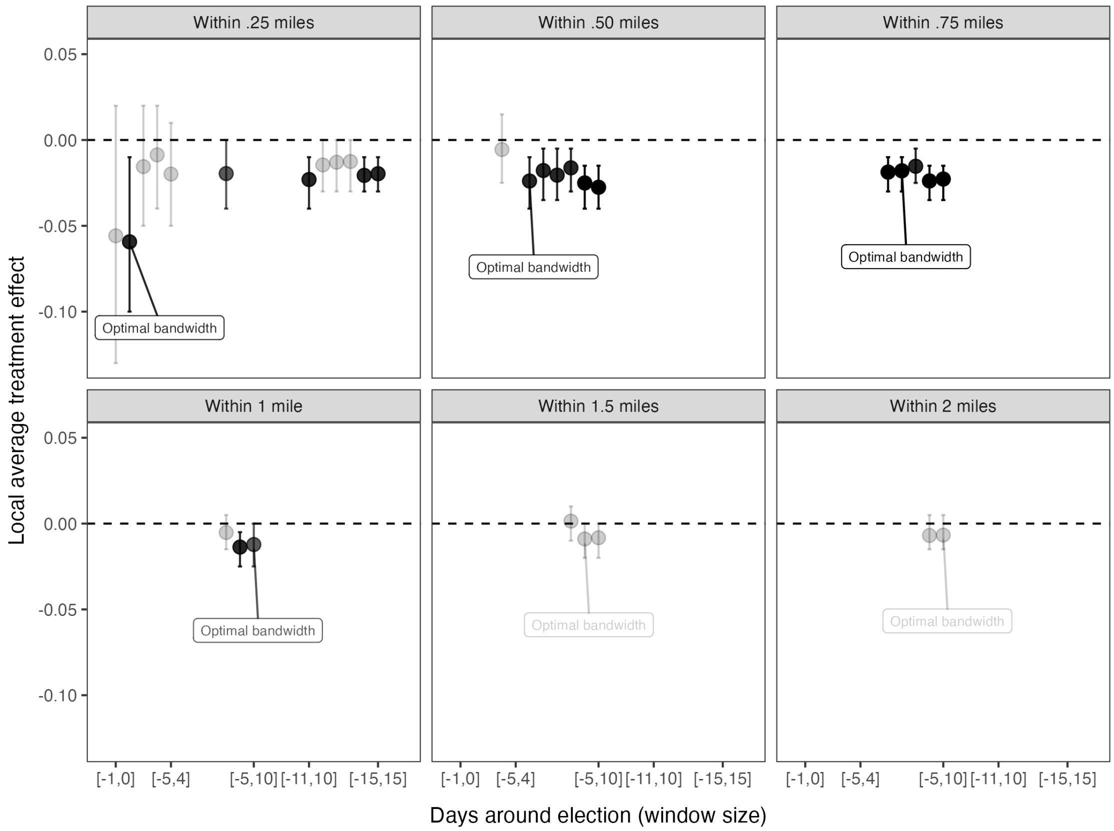


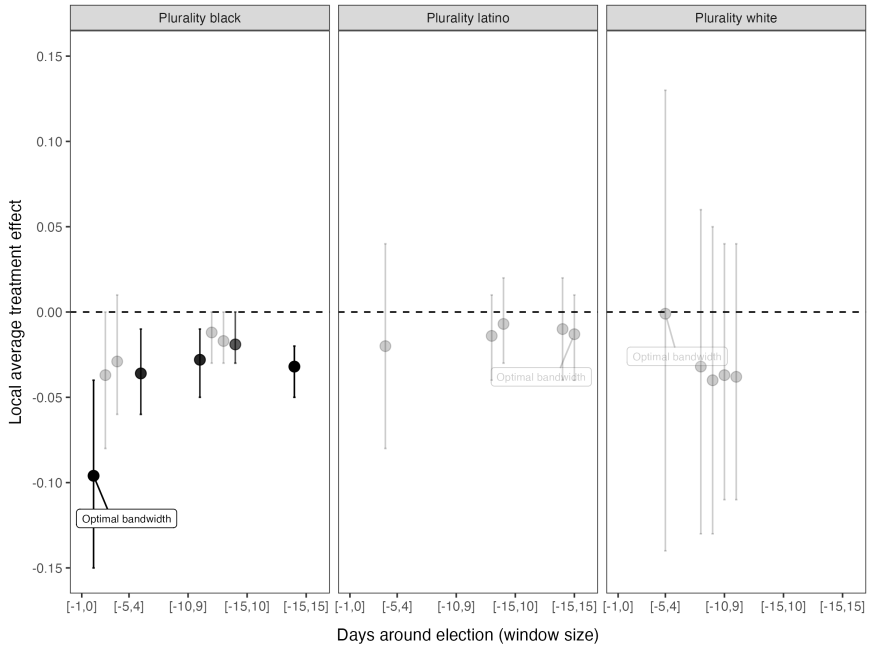
Figure 3: Local Average Treatment Effect by Distance of Homicide. Note: Fisherian p-values are shown by significance levels: black for p < .05, dark grey for p < .10, and light grey for non-significant estimates. The number of estimates per threshold varies, as only covariate-balanced windows are included.

Table 4: Cluster-Robust OLS Estimates of Homicide Exposure on Turnout by Distance

	OLS Results in Balanced Windows					
Variable	< 0.25 miles	< 0.50 miles	< 0.75 miles	< 1 mile	< 1.5 miles	< 2 miles
Treated	-0.021*	-0.018*	-0.017+	-0.006	-0.008	-0.005
	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)	(0.010)
(Intercept)	0.518**	0.296**	0.418**	0.377**	0.427**	0.493**
N (Obs)	537	636	1120	1008	1349	1570
Controls	Yes	Yes	Yes	Yes	Yes	Yes

[*] Control variables include those in Table 2 and a lagged outcome for 2014 block group turnout.
[†] Models estimated at widest threshold windows ([-15,15] - .25 miles; [-5,10] > .25 miles)
[‡] Standard errors clustered at the individual homicide level.
[§] Significance levels: +p < .10; *p < .05; **p < .01.

a) Proximate Homicide Exposure and Turnout by Block Group Racial Composition



b) Proximate Homicide Exposure and Turnout by Victim Race

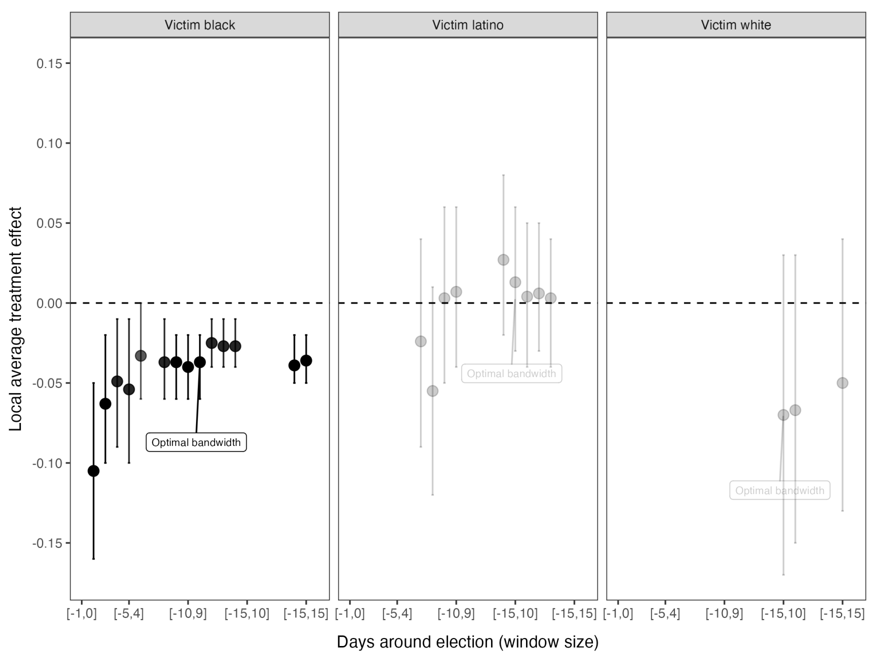


Figure 4: LATE by Distance from Homicide by Race. Note: Fisherian p-values are shown by significance levels: black for $p < .05$, dark grey for $p < .10$, and light grey for non-significant estimates. The number of estimates per threshold varies, as only covariate-balanced windows are included.

Table 5: Manually Collected Homicide Data

City	Number of homicides in data	Unique block group exposures
Atlanta, GA	230	750
Baltimore, MD	539	4502
Boston, MA	67	1242
Charlotte, NC	123	333
Chicago, IL	1255	12719
Detroit, MI	494	2849
Los Angeles, CA	325	3456
Miami, FL	81	752
Milwaukee, WI	342	3241
Washington, DC	420	2775

Note: Data compiled from city records and cross-referenced with UCR crime reports (Appendix Table B2 (32)).

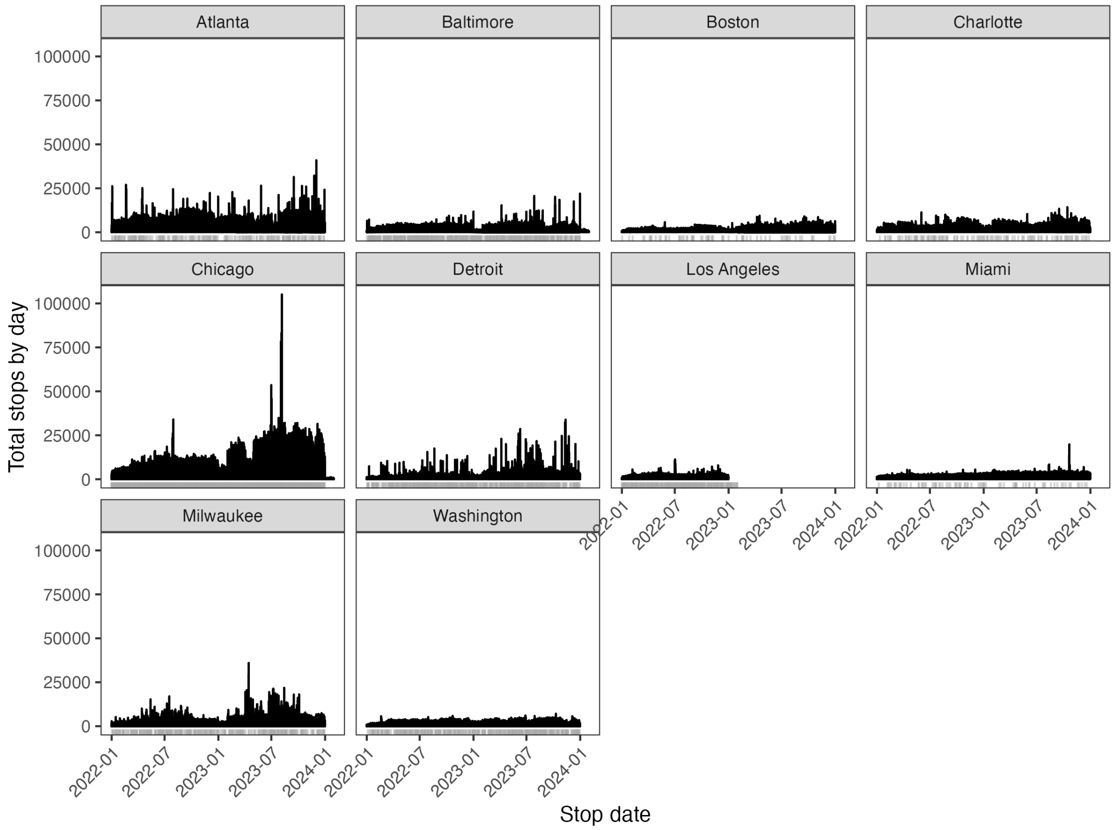


Figure 5: Monthly Foot Traffic Stops by City, 2022 and 2023. Rug marks at the bottom indicate the timing of homicides in the dataset.

Table 6: Effect of Proximate Homicide Exposure on Block Group Visits

	< 0.3 mi	< 0.5 mi	< 0.75 mi
Estimate	-15.977+	-15.076*	-11.697*
	(9.343)	(6.179)	(5.236)
Observations	80531	199529	358600
Block group exposures	7321	18139	32600
Block group fixed effects	Yes	Yes	Yes
Weighted by visits	Yes	Yes	Yes

* Standard errors are clustered at both the individual homicide and block group levels to account for within-event and within-unit correlation.

† Significance levels: +p < .10; *p < .05; **p < .01.

Table 7: Effect of Proximate Homicide Exposure on Populated Block Group Visits

	< 0.3 mi (100+ visits)	< 0.5 mi (100+ visits)	< 0.75 mi (100+ visits)
Estimate	-21.256+	-19.781*	-15.013*
	(12.499)	(8.14)	(6.738)
Observations	46530	117711	218735
Block group exposures	4230	10701	19885
Block group fixed effects	Yes	Yes	Yes
Weighted by visits	Yes	Yes	Yes

* Standard errors are clustered at both the individual homicide and block group levels to account for within-event and within-unit correlation.

† Significance levels: +p < .10; *p < .05; **p < .01.

Table 8: Reported Fear of Victimization and Local Rates of Violence

	Fear of victimization					
	(1)	(2)	(3)	(4)	(5)	(6)
Violent crime rate	0.019**	0.010*	0.001			
	(0.005)	(0.004)	(0.006)			
Violent crime rate * race (white)			0.016*			
			(0.007)			
Homicide rate				0.017**	0.007	0.001
				(0.006)	(0.005)	(0.006)
Homicide rate * race (white)						0.011*
						(0.006)
Race (white)		-0.133**	-0.135**		-0.134**	-0.135**
		(0.009)	(0.009)		(0.009)	(0.009)
Income		-0.024*	-0.023+		-0.023+	-0.023+
		(0.012)	(0.012)		(0.012)	(0.012)
Age		0.002**	0.002**		0.002**	0.002**
		(0.000)	(0.000)		(0.000)	(0.000)
(Intercept)	0.373**	0.408**	0.410**	0.373**	0.408**	0.409**
Number of Observations	10131	10131	10131	10131	10131	10131

[*] Standard errors clustered at the county level.

[†] Significance levels: +p < .10; *p < .05; **p < .01.

Table 9: Reported Trust in Government and Local Rates of Violence

	Trust in government					
	(1)	(2)	(3)	(4)	(5)	(6)
Violent crime rate	-0.006**	-0.005*	-0.004			
	(0.002)	(0.002)	(0.004)			
Violent crime rate * race (white)			-0.002			
			(0.005)			
Homicide rate				-0.002	-0.001	-0.003
				(0.002)	(0.002)	(0.005)
Homicide rate * race (white)						0.004
						(0.006)
Race (white)		0.018**	0.019**		0.020**	0.019**
		(0.006)	(0.007)		(0.006)	(0.006)
Income		-0.000	-0.000		-0.000	-0.000
		(0.009)	(0.009)		(0.009)	(0.009)
Age		-0.001**	-0.001**		-0.001**	-0.001**
		(0.000)	(0.000)		(0.000)	(0.000)
(Intercept)	0.311**	0.330**	0.330**	0.311**	0.329**	0.330**
Number of Observations	10131	10131	10131	10131	10131	10131

[*] Standard errors clustered at the county level.

[†] Significance levels: +p < .10; *p < .05; **p < .01.

Online Supplementary Information

Enclaves of Isolation: Violence and Political Participation

Table of Contents

Section A: Estimating Effects of Violence on Voter Turnout

Section B: Estimating Effects of Homicide Exposure on Foot Traffic

Section C: RCSS Survey Analysis

Section A: Estimating Effects of Violence Exposure on Voter Turnout

Model Equation for Regression Discontinuity in Time

I estimate a regression model linking census block group–level voter turnout to recent exposure to homicide during the 2016 federal election, using a regression discontinuity-in-time (RDiT) design within the local randomization framework. The model is specified as:

$$Y_{it} = B_1 \mathrm{Exposed}_{it} + \mathrm{X}_{it} + e_{it} \tag{1}$$

Where Y_{it} denotes voter turnout in block group i during election t, and $\mathrm{Exposed}_{it}$ is a binary indicator equal to 1 if the block group was exposed to a homicide (based on centroid proximity) in the pre-election period, and 0 if the exposure occurred after the election. The variable of interest, B_1, represents the effect of the treatment condition, $\mathrm{Exposed}_{it}$, on Y_{it}. Exposure is defined using time windows around the election cutoff: for the 0.25-mile threshold, I use an extended [-15,15] day window to increase power within this narrow distance band; for all other thresholds, I use a [-5,10] day window based on covariate balance and matching quality. X_{it} is a vector of observed covariates for block group i at time t (as listed in Table 2, along with lagged turnout from 2014), and e_{it} is a random error term. This approach estimates local average treatment effects among units plausibly randomized within each time window.

Primary Results for Bandwidth Windows (Figure 3)

In Figure 3 of this manuscript, I present the difference-in-means estimator for block groups exposed at various exposure windows across thresholds. Table A1 reports the corresponding local average treatment effects (LATEs), sample sizes, p-values, and confidence intervals for each estimate shown in the manuscript. I include only those windows that pass a covariate balance test, defined as no statistically significant differences between treatment and control groups at the p > 0.05 level. As discussed in the paper, consistent effects beginning to decay at roughly 1 mile.

Table A1: Window Selection Results Across Bandwidths

Model	Threshold	Treated	Control	Difference	Fisherian p-value	Confidence interval	Treated n	Control n
[-1,0]	Within .25 miles	0.518	0.574	-0.056	**0.149**	[-0.130, 0.020]	16	17
[-2,1]	Within .25 miles	0.539	0.598	-0.059	**0.020**	[-0.100, -0.010]	38	32
[-3,2]	Within .25 miles	0.556	0.572	-0.016	**0.437**	[-0.050, 0.020]	60	45
[-4,3]	Within .25 miles	0.556	0.564	-0.009	**0.603**	[-0.040, 0.020]	79	65
[-5,4]	Within .25 miles	0.548	0.568	-0.020	**0.226**	[-0.050, 0.010]	96	79
[-9,8]	Within .25 miles	0.540	0.560	-0.020	**0.076**	[-0.040, -0.000]	204	162
[-11,10]	Within .25 miles	0.545	0.568	-0.023	**0.024**	[-0.040, -0.010]	246	199
[-12,11]	Within .25 miles	0.550	0.565	-0.015	**0.147**	[-0.030, -0.000]	264	226
[-13,12]	Within .25 miles	0.552	0.565	-0.013	**0.155**	[-0.030, -0.000]	270	262
[-14,13]	Within .25 miles	0.553	0.566	-0.013	**0.162**	[-0.030, -0.000]	293	278
[-15,14]	Within .25 miles	0.548	0.568	-0.021	**0.025**	[-0.030, -0.010]	321	295
[-15,15]	Within .25 miles	0.548	0.567	-0.020	**0.021**	[-0.030, -0.010]	321	321
[-4,3]	Within .50 miles	0.565	0.570	-0.006	**0.626**	[-0.025, 0.015]	258	116
[-5,5]	Within .50 miles	0.557	0.581	-0.024	**0.010**	[-0.040, -0.010]	318	181
[-5,6]	Within .50 miles	0.557	0.575	-0.018	**0.047**	[-0.035, -0.005]	318	225
[-5,7]	Within .50 miles	0.557	0.577	-0.020	**0.014**	[-0.035, -0.005]	318	245
[-5,8]	Within .50 miles	0.557	0.573	-0.016	**0.044**	[-0.030, -0.005]	318	266
[-5,9]	Within .50 miles	0.557	0.582	-0.025	**0.000**	[-0.040, -0.015]	318	295
[-5,10]	Within .50 miles	0.557	0.584	-0.028	**0.000**	[-0.040, -0.015]	318	318
[-5,6]	Within .75 miles	0.567	0.585	-0.019	**0.006**	[-0.030, -0.010]	559	401
[-5,7]	Within .75 miles	0.567	0.585	-0.018	**0.008**	[-0.030, -0.010]	559	436
[-5,8]	Within .75 miles	0.567	0.582	-0.015	**0.027**	[-0.025, -0.005]	559	471
[-5,9]	Within .75 miles	0.567	0.591	-0.024	**0.000**	[-0.035, -0.015]	559	517
[-5,10]	Within .75 miles	0.567	0.589	-0.023	**0.000**	[-0.035, -0.015]	559	561
[-5,8]	Within 1 mile	0.577	0.582	-0.005	**0.482**	[-0.015, 0.005]	504	438
[-5,9]	Within 1 mile	0.577	0.590	-0.014	**0.049**	[-0.025, -0.005]	504	471
[-5,10]	Within 1 mile	0.577	0.589	-0.012	**0.078**	[-0.025, -0.000]	504	504
[-5,8]	Within 1.5 miles	0.613	0.612	0.001	**0.845**	[-0.010, 0.010]	674	574
[-5,9]	Within 1.5 miles	0.613	0.622	-0.009	**0.192**	[-0.020, -0.000]	674	633
[-5,10]	Within 1.5 miles	0.613	0.621	-0.008	**0.222**	[-0.020, -0.000]	674	675
[-5,9]	Within 2 miles	0.643	0.650	-0.007	**0.305**	[-0.015, 0.005]	785	726
[-5,10]	Within 2 miles	0.643	0.650	-0.007	**0.297**	[-0.015, 0.005]	785	785

Primary Results for Bandwidth Windows without Covariate Adjustment

Table A2 presents the same set of estimates shown in Figure 3, but without covariate adjustment. The results change only minimally—likely because matching improves balance prior to estimating the difference in means.

Table A2: Window Selection Results Across Bandwidths | No Covariate Adjustment

Model	Threshold	Treated	Control	Difference	Fisherian p-value	Confidence interval	Treated n	Control n
[-1,0]	Within .25 miles	0.518	0.574	-0.056	**0.149**	[0.135,-0.02]	16	17
[-2,1]	Within .25 miles	0.539	0.598	-0.059	**0.020**	[0.105,0.01]	38	32
[-3,2]	Within .25 miles	0.556	0.572	-0.016	**0.437**	[0.055,-0.025]	60	45
[-4,3]	Within .25 miles	0.556	0.564	-0.009	**0.603**	[0.04,-0.02]	79	65
[-5,4]	Within .25 miles	0.548	0.568	-0.020	**0.226**	[0.05,-0.01]	96	79
[-9,8]	Within .25 miles	0.540	0.560	-0.020	**0.076**	[0.04,0]	204	162
[-11,10]	Within .25 miles	0.545	0.568	-0.023	**0.024**	[0.04,0.005]	246	199
[-12,11]	Within .25 miles	0.550	0.565	-0.015	**0.147**	[0.03,0]	264	226
[-13,12]	Within .25 miles	0.552	0.565	-0.013	**0.155**	[0.03,-0.005]	270	262
[-14,13]	Within .25 miles	0.553	0.566	-0.013	**0.162**	[0.03,-0.005]	293	278
[-15,14]	Within .25 miles	0.548	0.568	-0.021	**0.025**	[0.035,0.005]	321	295
[-15,15]	Within .25 miles	0.548	0.567	-0.020	**0.021**	[0.035,0.005]	321	321
[-4,3]	Within .50 miles	0.565	0.570	-0.006	**0.626**	[0.025,-0.015]	258	116
[-5,5]	Within .50 miles	0.557	0.581	-0.024	**0.010**	[0.04,0.01]	318	181
[-5,6]	Within .50 miles	0.557	0.575	-0.018	**0.047**	[0.035,0.005]	318	225
[-5,7]	Within .50 miles	0.557	0.577	-0.020	**0.014**	[0.035,0.005]	318	245
[-5,8]	Within .50 miles	0.557	0.573	-0.016	**0.044**	[0.03,0.005]	318	266
[-5,9]	Within .50 miles	0.557	0.582	-0.025	**0.000**	[0.04,0.015]	318	295
[-5,10]	Within .50 miles	0.557	0.584	-0.028	**0.000**	[0.04,0.015]	318	318
[-5,6]	Within .75 miles	0.567	0.585	-0.019	**0.006**	[0.03,0.01]	559	401
[-5,7]	Within .75 miles	0.567	0.585	-0.018	**0.008**	[0.03,0.01]	559	436
[-5,8]	Within .75 miles	0.567	0.582	-0.015	**0.027**	[0.025,0.005]	559	471
[-5,9]	Within .75 miles	0.567	0.591	-0.024	**0.000**	[0.035,0.015]	559	517
[-5,10]	Within .75 miles	0.567	0.589	-0.023	**0.000**	[0.035,0.015]	559	561
[-5,8]	Within 1 mile	0.577	0.582	-0.005	**0.482**	[0.015,-0.005]	504	438
[-5,9]	Within 1 mile	0.577	0.590	-0.014	**0.049**	[0.025,0.005]	504	471
[-5,10]	Within 1 mile	0.577	0.589	-0.012	**0.078**	[0.025,0]	504	504
[-5,8]	Within 1.5 miles	0.613	0.612	0.001	**0.845**	[0.01,-0.015]	674	574
[-5,9]	Within 1.5 miles	0.613	0.622	-0.009	**0.192**	[0.02,0]	674	633
[-5,10]	Within 1.5 miles	0.613	0.621	-0.008	**0.222**	[0.02,-0.005]	674	675
[-5,8]	Within 2 miles	0.643	0.643	0.001	**0.938**	[0.01,-0.01]	785	677
[-5,9]	Within 2 miles	0.643	0.650	-0.007	**0.305**	[0.015,-0.005]	785	726
[-5,10]	Within 2 miles	0.643	0.650	-0.007	**0.297**	[0.015,-0.005]	785	785

Estimating Heterogeneous Effects by Race

In Figure 4 of this manuscript, I present the difference-in-means estimator for block groups by block group demographic composition and victim race. Here I present the LATE, sample sizes, p-values, and confidence intervals for each of the estimates presented in the manuscript. Table A3 shows results by the demographic composition of block groups. Table A4 shows results by victim race. Again, here I only report results for windows passing the balance test of a treatment group difference estimated at ($p > .05$) or larger. Within each data-optimal window, I maintain a minimum p value for estimated differences across treatment groups at ($p > .10$).

Table A3: Window Selection Results by BG Racial Composition

Model	Plurality	Treated	Control	Difference	Fisherian p-value	Confidence interval	Treated n	Control n
[-2,1]	Plurality black	0.513	0.609	-0.096	**0.006**	[-0.150, -0.040]	19	12
[-3,2]	Plurality black	0.540	0.577	-0.037	**0.113**	[-0.080, -0.000]	29	20
[-4,3]	Plurality black	0.536	0.565	-0.029	**0.133**	[-0.060, 0.010]	37	35
[-6,5]	Plurality black	0.532	0.568	-0.036	**0.025**	[-0.060, -0.010]	58	54
[-11,10]	Plurality black	0.538	0.566	-0.028	**0.017**	[-0.050, -0.010]	125	103
[-12,11]	Plurality black	0.544	0.556	-0.012	**0.267**	[-0.030, -0.000]	137	120
[-13,12]	Plurality black	0.542	0.559	-0.017	**0.107**	[-0.030, -0.000]	142	134
[-14,13]	Plurality black	0.540	0.559	-0.019	**0.064**	[-0.030, -0.000]	150	141
[-15,14]	Plurality black	0.530	0.561	-0.032	**0.000**	[-0.050, -0.020]	165	148
[-5,4]	Plurality white	0.599	0.599	-0.001	**0.986**	[-0.140, 0.130]	11	10
[-8,5]	Plurality white	0.604	0.636	-0.032	**0.502**	[-0.130, 0.060]	13	19
[-9,6]	Plurality white	0.603	0.643	-0.040	**0.359**	[-0.130, 0.050]	20	21
[-10,7]	Plurality white	0.612	0.648	-0.037	**0.394**	[-0.110, 0.040]	22	23
[-11,9]	Plurality white	0.628	0.666	-0.038	**0.368**	[-0.110, 0.040]	25	25
[-4,3]	Plurality latino	0.498	0.518	-0.020	**0.532**	[-0.080, 0.040]	14	17
[-13,12]	Plurality latino	0.508	0.522	-0.014	**0.413**	[-0.040, 0.010]	55	59
[-14,13]	Plurality latino	0.515	0.522	-0.007	**0.692**	[-0.030, 0.020]	62	60
[-15,14]	Plurality latino	0.515	0.525	-0.010	**0.538**	[-0.040, 0.020]	66	63
[-15,15]	Plurality latino	0.515	0.529	-0.013	**0.409**	[-0.040, 0.010]	66	66

Table A4: Window Selection Results by Victim Race

Model	Victim Race	Treated	Control	Difference	Fisherian p-value	Confidence interval	Treated n	Control n
[-2,1]	Victim black	0.524	0.630	-0.105	**0.001**	[-0.160, -0.050]	19	15
[-3,2]	Victim black	0.541	0.604	-0.063	**0.009**	[-0.100, -0.020]	27	22
[-4,3]	Victim black	0.542	0.591	-0.049	**0.034**	[-0.090, -0.010]	36	32
[-5,4]	Victim black	0.539	0.593	-0.054	**0.021**	[-0.100, -0.010]	40	37
[-6,5]	Victim black	0.541	0.574	-0.033	**0.056**	[-0.060, -0.000]	55	48
[-8,7]	Victim black	0.543	0.580	-0.037	**0.012**	[-0.060, -0.010]	90	72
[-9,8]	Victim black	0.540	0.577	-0.037	**0.003**	[-0.060, -0.020]	99	80
[-10,9]	Victim black	0.540	0.580	-0.040	**0.001**	[-0.060, -0.020]	113	91
[-11,10]	Victim black	0.542	0.579	-0.037	**0.005**	[-0.060, -0.020]	121	102
[-12,11]	Victim black	0.546	0.571	-0.025	**0.031**	[-0.040, -0.010]	130	114
[-13,12]	Victim black	0.545	0.572	-0.027	**0.020**	[-0.040, -0.010]	136	129
[-14,13]	Victim black	0.545	0.573	-0.027	**0.010**	[-0.040, -0.010]	147	137
[-15,14]	Victim black	0.535	0.574	-0.039	**0.000**	[-0.050, -0.020]	162	143
[-15,15]	Victim black	0.535	0.571	-0.036	**0.000**	[-0.050, -0.020]	162	162
[-15,10]	Victim white	0.535	0.605	-0.070	**0.202**	[-0.170, 0.030]	16	10
[-15,11]	Victim white	0.535	0.602	-0.067	**0.156**	[-0.150, 0.030]	16	14
[-15,15]	Victim white	0.535	0.585	-0.050	**0.256**	[-0.130, 0.040]	16	16
[-7,4]	Victim latino	0.510	0.534	-0.024	**0.483**	[-0.090, 0.040]	23	11
[-8,5]	Victim latino	0.502	0.557	-0.055	**0.127**	[-0.120, 0.010]	28	13
[-9,6]	Victim latino	0.520	0.518	0.003	**0.923**	[-0.050, 0.060]	33	21
[-10,7]	Victim latino	0.519	0.513	0.007	**0.818**	[-0.040, 0.060]	35	23
[-14,8]	Victim latino	0.526	0.499	0.027	**0.307**	[-0.020, 0.080]	39	26
[-15,10]	Victim latino	0.523	0.510	0.013	**0.605**	[-0.030, 0.060]	42	28
[-15,11]	Victim latino	0.523	0.519	0.004	**0.877**	[-0.040, 0.050]	42	33
[-15,12]	Victim latino	0.523	0.517	0.006	**0.772**	[-0.030, 0.050]	42	40
[-15,13]	Victim latino	0.523	0.521	0.003	**0.914**	[-0.040, 0.040]	42	42

Falsification Tests for Primary Results: Testing Predetermined Covariates

In Table 2 of the manuscript, I present the covariate balance analysis for the widest window within the .25 threshold [-15,15]. Serving as a falsification test, the analysis serves to validate the assumption that treated and control block groups at the cutoff are similar in terms of observable characteristics. Similar to the table presented in the manuscript, I use *rdrandinf* to estimate the RD effect of proximate homicide exposure on the predetermined covariates for the .50-2 mile thresholds (A5-A10). Generally, I observe parity at each optimal window threshold. Overall, I observe covariate balance at each optimal window selected by the balance procedure. The only marginally significant imbalance appears at the 1.5-mile (A9) threshold, where the treatment group experienced 0.64 fewer exposures in the previous year (p = 0.07) and the treatment group was roughly 1 year younger (p = .058). As noted in the manuscript, I present covariate-adjusted models to account for such minor discrepancies.

Table A5: Window Selection [-2,1] | Exposed within .25

Covariate	Difference	Fisherian p-value	Treated	Control	Treated n	Control n
% Latino	-0.024	**0.761**	0.264	0.288	38	32
% White	-0.031	**0.522**	0.132	0.163	38	32
% Black	0.089	**0.337**	0.549	0.460	38	32
Median income	798.253	**0.863**	37607.816	36809.562	38	32
Median age	-0.740	**0.694**	33.929	34.669	38	32
Population density	342.306	**0.943**	22039.889	21697.583	38	32
Some college	0.013	**0.805**	0.446	0.434	38	32
Previous year exposures	0.301	**0.918**	11.895	11.594	38	32

Table A6: Window Selection [-5,5] | Exposed within .50

Covariate	Difference	Fisherian p-value	Treated	Control	Treated n	Control n
% Latino	-0.031	**0.262**	0.290	0.321	318	181
% White	-0.017	**0.320**	0.119	0.136	318	181
% Black	0.026	**0.423**	0.506	0.480	318	181
Median income	-1378.785	**0.495**	37658.840	39037.624	318	181
Median age	1.028	**0.189**	35.064	34.035	318	181
Population density	-101.435	**0.982**	26902.112	27003.547	318	181
Some college	0.006	**0.731**	0.449	0.444	318	181
Previous year exposures	0.224	**0.798**	8.821	8.597	318	181

Table A7: Window Selection [-5,7] | Exposed within .75

Covariate	Difference	Fisherian p-value	Treated	Control	Treated n	Control n
% Latino	-0.030	**0.116**	0.301	0.331	561	436
% White	-0.003	**0.857**	0.153	0.155	561	436
% Black	0.025	**0.283**	0.464	0.440	561	436
Median income	-1339.800	**0.480**	42717.398	44057.197	561	436
Median age	-0.137	**0.818**	34.809	34.946	561	436
Population density	-2936.579	**0.305**	32306.627	35243.206	561	436
Some college	0.007	**0.600**	0.473	0.466	561	436
Previous year exposures	0.395	**0.381**	6.781	6.385	561	436

Table A8: Window Selection [-5,10] | Exposed within 1 Mile

Covariate	Difference	Fisherian p-value	Treated	Control	Treated n	Control n
% Latino	0.012	**0.492**	0.311	0.299	506	506
% White	-0.012	**0.488**	0.191	0.203	506	506
% Black	-0.015	**0.517**	0.416	0.431	506	506
Median income	-741.186	**0.743**	47048.208	47789.393	506	506
Median age	-0.780	**0.135**	35.355	36.135	506	506
Population density	-1493.235	**0.653**	42254.607	43747.841	506	506
Some college	0.002	**0.858**	0.505	0.503	506	506
Previous year exposures	-0.379	**0.385**	5.352	5.731	506	506

Table A9: Window Selection [-5,9] | Exposed within 1.5 Miles

Covariate	Difference	Fisherian p-value	Treated	Control	Treated n	Control n
% Latino	0.010	**0.518**	0.275	0.264	676	634
% White	-0.015	**0.431**	0.316	0.331	676	634
% Black	-0.003	**0.867**	0.323	0.326	676	634
Median income	-1733.265	**0.473**	56386.432	58119.697	676	634
Median age	-0.906	**0.058**	36.198	37.105	676	634
Population density	-2606.554	**0.411**	39034.492	41641.046	676	634
Some college	0.003	**0.819**	0.567	0.564	676	634
Previous year exposures	-0.620	**0.072**	3.544	4.164	676	634

Table A10: Window Selection [-5,10] | Exposed within 2 Miles

Covariate	Difference	Fisherian p-value	Treated	Control	Treated n	Control n
% Latino	0.015	**0.311**	0.245	0.230	786	786
% White	-0.003	**0.893**	0.393	0.396	786	786
% Black	-0.012	**0.542**	0.273	0.284	786	786
Median income	106.943	**0.960**	61137.756	61030.813	786	786
Median age	-0.642	**0.142**	36.594	37.236	786	786
Population density	-2649.793	**0.253**	25765.037	28414.829	786	786
Some college	0.008	**0.547**	0.594	0.586	786	786
Previous year exposures	-0.319	**0.155**	2.627	2.947	786	786

Covariate Balance in Remaining Windows - Distance Thresholds

I use the *rdwinselect* function to assess covariate balance for all windows analyzed in Figure 3. This procedure identifies the covariate with the lowest p-value for each window, providing a conservative test of balance between treatment and control groups. For example, in the window of [-15,15] (Table 2), prior-year exposure is the least balanced covariate, with a p-value of 0.37. The primary results in Table 3 are restricted to optimal windows where all covariates meet a balance threshold of p > 0.10. Additional estimates in Figure 3 are presented as robustness checks and limited to windows with minimum balance of p > 0.05. Tables A11–A16 report the *rdwinselect* balance diagnostics for all distance thresholds used in Figure 3, including both optimal and non-optimal windows. The covariates assessed are the same as those listed in Table 2. All reported windows meet the minimum balance threshold of p > 0.05, with p > 0.10 achieved in the optimal windows.

Table A11: rdwinselect Balance Table | Exposed within .25

Fisherian p-value	Variable	Obs < c	Obs >= c	Window left	Window right
0.152	Some college	16	17	-1	0
0.299	% Black	38	32	-2	1
0.353	Previous year exposures	60	45	-3	2
0.262	Some college	79	65	-4	3
0.119	Population density	96	79	-5	4
0.070	Median income	204	162	-9	8
0.138	Median income	246	199	-11	10
0.099	Previous year exposures	264	226	-12	11
0.109	Previous year exposures	270	262	-13	12
0.110	Previous year exposures	293	278	-14	13
0.293	Previous year exposures	321	295	-15	14
0.370	Previous year exposures	321	321	-15	15

Table A12: rdwinselect Balance Table | Exposed within .50

Fisherian p-value	Variable	Obs < c	Obs >= c	Window left	Window right
0.060	Some college	258	116	-4	3
0.155	Median age	318	181	-5	5
0.158	Median age	318	225	-5	6
0.378	Median income	318	245	-5	7
0.302	Median age	318	266	-5	8
0.287	Population density	318	295	-5	9
0.456	Previous year exposures	318	318	-5	10

Table A13: rdwinselect Balance Table | Exposed within .75

Fisherian p-value	Variable	Obs < c	Obs >= c	Window left	Window right
0.089	Population density	559	401	-5	6
0.259	Population density	559	436	-5	7
0.388	% Black	559	471	-5	8
0.537	Median age	559	517	-5	9
0.448	Median age	559	561	-5	10

Table A14: rdwinselect Balance Table | Exposed within 1 Mile

Fisherian p-value	Variable	Obs < c	Obs >= c	Window left	Window right
0.079	Population density	504	438	-5	8
0.061	Median age	504	471	-5	9
0.143	Median age	504	504	-5	10

Table A15: rdwinselect Balance Table | Exposed within 1.5 Miles

Fisherian p-value	Variable	Obs < c	Obs >= c	Window left	Window right
0.059	Population density	674	574	-5	8
0.063	Previous year exposures	674	633	-5	9
0.062	Previous year exposures	674	675	-5	10

Table A16: rdwinselect Balance Table | Exposed within 2 Miles

Fisherian p-value	Variable	Obs < c	Obs >= c	Window left	Window right
0.078	Median age	785	726	-5	9
0.135	Median age	785	785	-5	10

Primary Results for .25 Threshold Windows without Matching

Table A17 presents the difference-in-means estimator for windows maintaining natural balance (without matching) at the .25 mile threshold. As noted in the manuscript, within the relatively small number of observations within the .25 mile threshold and windows narrower than [-5,5], Figure 3 presents marginally significant and null results. At the wider windows, the results offer evidence of a depressive effect on turnout. Table A19 presents the covariate balance for the optimal-window [-2,1]. As shown, I observe parity across covariates.

Table A17: .25 Bandwidth Window Results - No Matching

Model	Difference	Treated	Control	Fisherian p-value	Confidence interval	Treated n	Control n
[-2,1]	-0.044	0.548	0.592	**0.076**	[-0.090, -0.000]	46	35
[-3,2]	-0.011	0.558	0.569	**0.579**	[-0.040, 0.020]	76	51
[-4,3]	-0.007	0.556	0.563	**0.618**	[-0.030, 0.020]	101	73
[-15,14]	-0.026	0.546	0.572	**0.000**	[-0.040, -0.010]	396	338
[-15,15]	-0.024	0.546	0.571	**0.002**	[-0.040, -0.010]	396	367

Table A18: rdwinselect Balance Table | Exposed within .25 - No Matching

Fisherian p-value	Variable	Obs < c	Obs >= c	Window left	Window right
0.268	% Black	46	35	-2	1
0.214	Some college	76	51	-3	2
0.078	Population density	101	73	-4	3
0.056	Median income	396	338	-15	14
0.085	Median income	396	367	-15	15

Table A19: Optimal Bandwidth Window Result (.25 Miles) - No Matching

Covariate	Difference	Fisherian p-value	Treated	Control	Treated n	Control n
% Latino	-0.036	**0.630**	0.269	0.305	46	35
% White	-0.035	**0.464**	0.132	0.167	46	35
% Black	0.095	**0.234**	0.541	0.446	46	35
Median income	2525.496	**0.624**	38285.239	35759.743	46	35
Median age	-1.232	**0.501**	33.639	34.871	46	35
Population density	560.992	**0.892**	21843.877	21282.885	46	35
Some college	0.038	**0.421**	0.459	0.421	46	35
Previous year exposures	-0.355	**0.887**	11.130	11.486	46	35

Primary Results for .25 Threshold Windows - Nearest Neighbor Matching

As mentioned, in the primary models presented in the manuscript, I use optimal propensity score matching to ensure covariate balance across the treatment groups. As a robustness check, here I present the results using nearest neighbor propensity score matching with the *MatchIt* package in R. Specifically, I apply nearest neighbor matching with a caliper of 0.2, restricting matches to those within 0.2 standard deviations of the propensity score. Each treated unit is matched to one control unit, without replacement, meaning each control unit is used at most once if it provides the best match. Generally, the results are in alignment with those presented in the manuscript, with the most significant estimates found in the widest thresholds.

Table A20: .25 Bandwidth Window Results - Nearest Neighbor

Model	Difference	Treated	Control	Fisherian p-value	Confidence interval	Treated n	Control n
[-1,0]	-0.050	0.524	0.574	**0.203**	[-0.120, 0.020]	20	11
[-2,1]	-0.041	0.548	0.589	**0.191**	[-0.100, 0.020]	46	17
[-3,2]	-0.002	0.558	0.560	**0.925**	[-0.040, 0.040]	76	31
[-4,3]	-0.001	0.556	0.556	**0.976**	[-0.030, 0.030]	101	41
[-5,4]	-0.013	0.548	0.561	**0.464**	[-0.040, 0.020]	120	49
[-9,8]	-0.018	0.540	0.558	**0.136**	[-0.040, -0.000]	260	99
[-11,10]	-0.027	0.544	0.572	**0.020**	[-0.040, -0.010]	309	124
[-12,11]	-0.018	0.549	0.568	**0.083**	[-0.030, -0.000]	330	143
[-13,12]	-0.017	0.550	0.566	**0.089**	[-0.030, -0.000]	340	170
[-14,13]	-0.016	0.551	0.567	**0.099**	[-0.030, -0.000]	364	180
[-15,14]	-0.024	0.546	0.570	**0.017**	[-0.040, -0.010]	395	193
[-15,15]	-0.022	0.546	0.568	**0.020**	[-0.040, -0.010]	395	205

Table A21: rdwinselect Balance Table (Nearest Neighbor) | Exposed within .25

Fisherian p-value	Variable	Obs < c	Obs >= c	Window left	Window right
0.209	Some college	20	11	-1	0
0.283	Population density	46	17	-2	1
0.135	Median age	76	31	-3	2
0.200	Median age	101	41	-4	3
0.231	Population density	120	49	-5	4
0.063	Median income	260	99	-9	8
0.110	Median income	309	124	-11	10
0.156	Median income	330	143	-12	11
0.195	Median income	340	170	-13	12
0.340	Median income	364	180	-14	13
0.352	% Black	395	193	-15	14
0.485	Median income	395	205	-15	15

Cluster-Robust OLS Estimates: Adjusted vs. Unadjusted Models

Table A22 compares cluster-Robust OLS estimates of the treatment effect across three distance thresholds, with and without covariate adjustment. Across all distances, models that include covariates yield more precise estimates, as reflected in smaller standard errors and generally higher statistical significance. For instance, at the < 0.25-mile threshold, the treatment effect is statistically significant when controls are included (p < .05), but loses significance without adjustment. This pattern holds across other windows as well, with the largest difference at the < 0.50-mile threshold, where the coefficient is marginally significant without covariates but reaches conventional significance levels when covariates are included. These results suggest that covariate adjustment meaningfully improves precision, consistent with the expectation that accounting for pre-treatment covariates reduces residual variance in voter turnout.

Table A22: Alternative Cluster-Robust OLS Estimates of Homicide Exposure on Turnout by Distance

	OLS Results in Balanced Windows					
Variable	< 0.25 miles	< 0.25 miles (2)	< 0.50 miles	< 0.50 miles (2)	< 0.75 miles	< 0.75 miles (2)
Treated	-0.021*	-0.020	-0.018*	-0.028+	-0.017+	-0.023
	(0.010)	(0.013)	(0.009)	(0.016)	(0.009)	(0.016)
(Intercept)	0.518**	0.567**	0.296**	0.584**	0.418**	0.589**
N (Obs)	642	642	636	636	1120	1120
Controls	Yes	No	Yes	No	Yes	No

* Control variables include those in Table 2 and a lagged outcome for 2014 block group turnout.
† Models estimated at widest threshold windows ([-15,15] - .25 miles; [-5,10] > .25 miles)
‡ Standard errors clustered at the individual homicide level.
§ Significance levels: +p < .10; *p < .05; **p < .01.

Density of Running Variable

An important falsification test examines whether the number of observations just above the cutoff is similar to the number just below it—an indicator of whether treatment assignment was plausibly as-if random. This is tested using a density test. The intuition is straightforward: if block groups cannot precisely control the timing of their homicide exposure, they should be equally likely to fall just before or after the election cutoff. I implement this test using the *rddensity* command on the full pre-processed dataset (i.e., before matching or removing block groups with multiple exposures) to assess potential manipulation around the cutoff. Table A23 reports the results for exposures within 2 miles, the broadest threshold examined in this manuscript.

Table A23: Density Test of Block Group Exposures

Left_Obs	Right_Obs	T_Stat	P_Value
79401	71157	1.094	**0.274**

Density of Running Variable

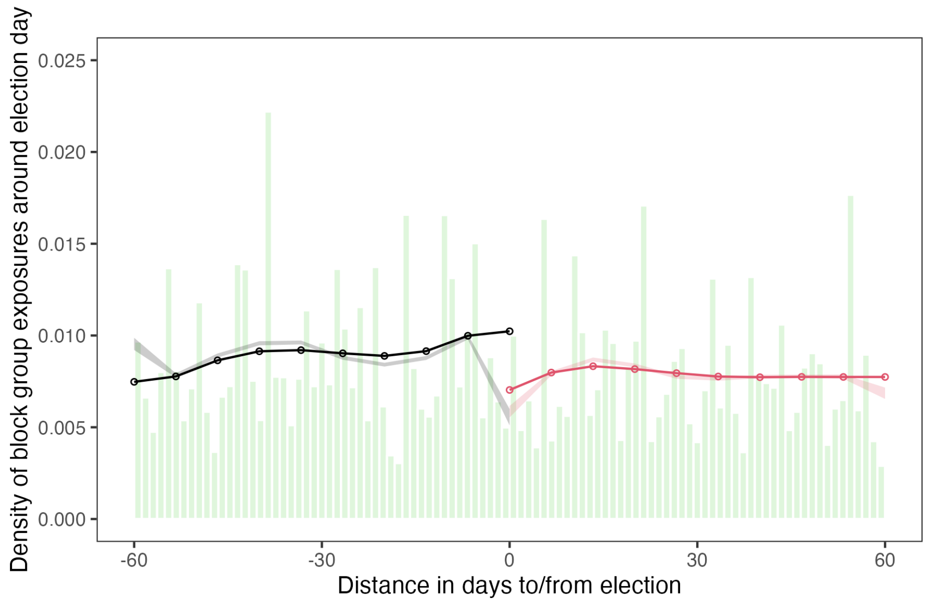


Figure A1: RDDensity Plot of Block Group Exposures Around Election Day

I also use the *rdplotdensity* function to visualize the distribution of block group exposures around the cutoff, shown in Figure A1. While the estimated discontinuity is not statistically significant, there are more observations in the treatment group. This pattern may reflect clustering of exposures, as multiple block groups may be exposed to the same homicide event, particularly when events occur close in time and space. Notably, a separate density test on the distribution of homicide events themselves (see Table A25) shows no discontinuity, supporting the assumption that the underlying treatment-generating process is not manipulated. Ultimately, the key identifying variation comes from the precise timing of exposure relative to the election, and the density tests provide minimal evidence of manipulation in treatment assignment across block groups.

Distribution of Homicides Around the Cutoff

To probe the validity of the local randomization assumption further, I also conduct a density test on the timing of homicide events themselves, treating homicides as the running variable rather than block group exposures. This serves as a robustness check to evaluate whether the underlying distribution of homicides—the basis of treatment assignment—is smooth across the election date. The results of this test show no evidence of a discontinuity in homicide timing, consistent with the assumption that homicides are not strategically timed around elections and are plausibly exogenous. Together, these tests suggest that while exposure assignments may cluster slightly due to the spatial structure of homicides, the underlying event distribution is balanced, supporting the design's identifying assumptions.

Table A24: Density Test of Unique Homicides Around the Cutoff

Left_Obs	Right_Obs	T_Stat	P_Value
1088	1023	0.449	**0.653**

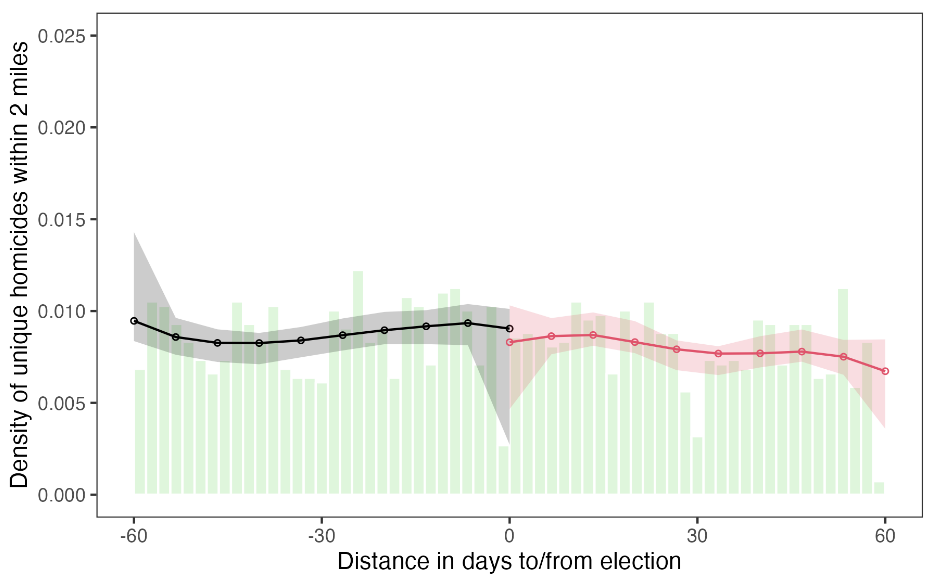


Figure A2: RDDensity Plot of Unique Homicides Around the Cutoff

Placebo Cutoffs

As a final robustness check, I replicate the primary analysis using placebo cutoffs—artificially chosen days near the election-day threshold—to assess whether the observed treatment effects might be artifacts of the design. If the identifying assumptions hold, no significant effects should be detected at these false thresholds. I define a set of artificial cutoffs ranging from five days before to five days after the election (excluding day 0 and day 1), and for each cutoff c, I construct a symmetric 21-day window [c - 10, c + 10], mirroring the bandwidth used in the largest specifications of the main analysis. Following guidance from Cattaneo, Idrobo, and Titiunik (2024), I re-center the analysis window at each artificial cutoff to maintain consistency in window length across placebo tests.

Table A25: Placebo Cutoffs for .25 Miles Unmatched Data

Cutoff	Estimate	p_value
Cutoff -5	0.003	**0.810**
Cutoff -4	0.006	**0.607**
Cutoff -3	0.011	**0.397**
Cutoff -2	-0.002	**0.890**
Cutoff -1	-0.022	**0.340**
Cutoff 2	-0.048	**0.089**
Cutoff 3	0.002	**0.914**
Cutoff 4	0.014	**0.401**
Cutoff 5	0.005	**0.725**

Table A25 presents the results using the raw (unmatched) data within the .25 mile threshold. Most placebo cutoffs yield null results, as expected under the assumption of no treatment. However, one specification produces a marginally statistically significant effect at $c = 2$ (p = 0.089). This isolated finding is not concerning on its own and is consistent with random chance, particularly given the number of placebo tests conducted. At the same time, it may point to a substantively meaningful pattern: if the effects of exposure to a homicide begin to materialize the day after the incident—rather than precisely on day 0—then the significant estimate at $c = 2$ may plausibly reflect early treatment effects. Rather than undermining the design, this likely supports the temporal sensitivity of the effects. Consistent with this interpretation, I exclude $c = 1$ from the placebo analysis, as it likely overlaps with the actual onset of treatment and may not constitute a true placebo.

OLS Results for Racially Heterogeneous Effects

In the manuscript, I present the main results estimated using the local randomization framework, along with OLS estimates that account for potential within-cluster dependence by clustering standard errors at the homicide ID level (Table 4). Here, I apply the same OLS specification to the heterogeneous effects models presented in Figure 4, providing a complementary robustness check using conventional regression methods.

Table A26: Effect of Proximate Homicide Exposure on Turnout by Race

	OLS results in balanced windows					
Variable	Plurality black	Plurality white	Plurality latino	Black victim	White victim	Latino victim
Treated	-0.027*	-0.042	-0.013	-0.035**	-0.009	-0.005
	(0.012)	(0.029)	(0.018)	(0.011)	(0.038)	(0.020)
(Intercept)	0.628**	0.510+	0.403+	0.712**	0.131	0.127
N (Obs)	330	78	132	324	32	84
Controls	Yes	Yes	Yes	Yes	Yes	Yes

* Control variables include those in Table 2 and a lagged outcome for 2014 block group turnout.
† Standard errors clustered at the homicide ID level.
‡ Significance levels: +p < .10; *p < .05; **p < .01.

Pre-Election Exposure Levels

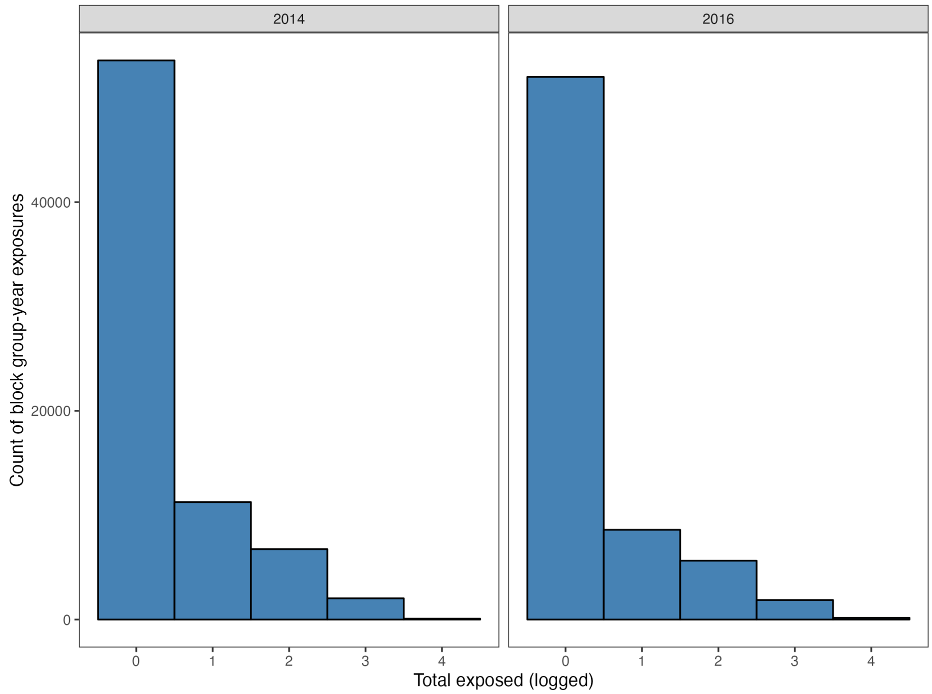


Figure A3: Previous Year Homicide Exposure (Logged)

In this section, I explore heterogeneous treatment effects by pre-treatment (homicides within 1 mile, in the year prior to the respective election) exposure levels. As shown in Figure A3, there does appear to be some heterogeneity, with moderate and high exposures producing the strongest depressive effects, reinforcing the expectations that the "frequency" of homicides should also inform our expectations for how the events are interpreted within a community context.[17] Of course, within the context of this analysis, this project focuses only on exposures in the months before and after federal elections, leaving the long term aggregate exposure beyond the scope of analysis. Future work should explore how longer-term exposure levels may mediate exposure effects.

[17]For this supplementary analysis, I present all the windows matched across the cutoff with covariate controls, regardless of their initial balance. While many of these windows do pass covariate balance (p > .10), this is definitely the case for each estimate labeled "optimal-bandwidth".

Distribution of Prior-Year Exposure Among Block Groups with at Least Two Homicides

Figure A4 displays the distribution of prior-year homicide exposure among block groups exposed to at least two homicides, shown separately for each election year. The long right tail highlights a small but persistent subset of block groups experiencing a high volume of homicides within one mile. Next, I turn to estimating heterogeneous effects by exposure levels, set by the interquartile range (IQR) of block group exposures. Those under the "low" category are those exposed to one or less homicides in the year prior to the election. Those under the "moderate" category are exposed between two to six times, while those exposed to seven or more homicides are in the "high" category.[18]

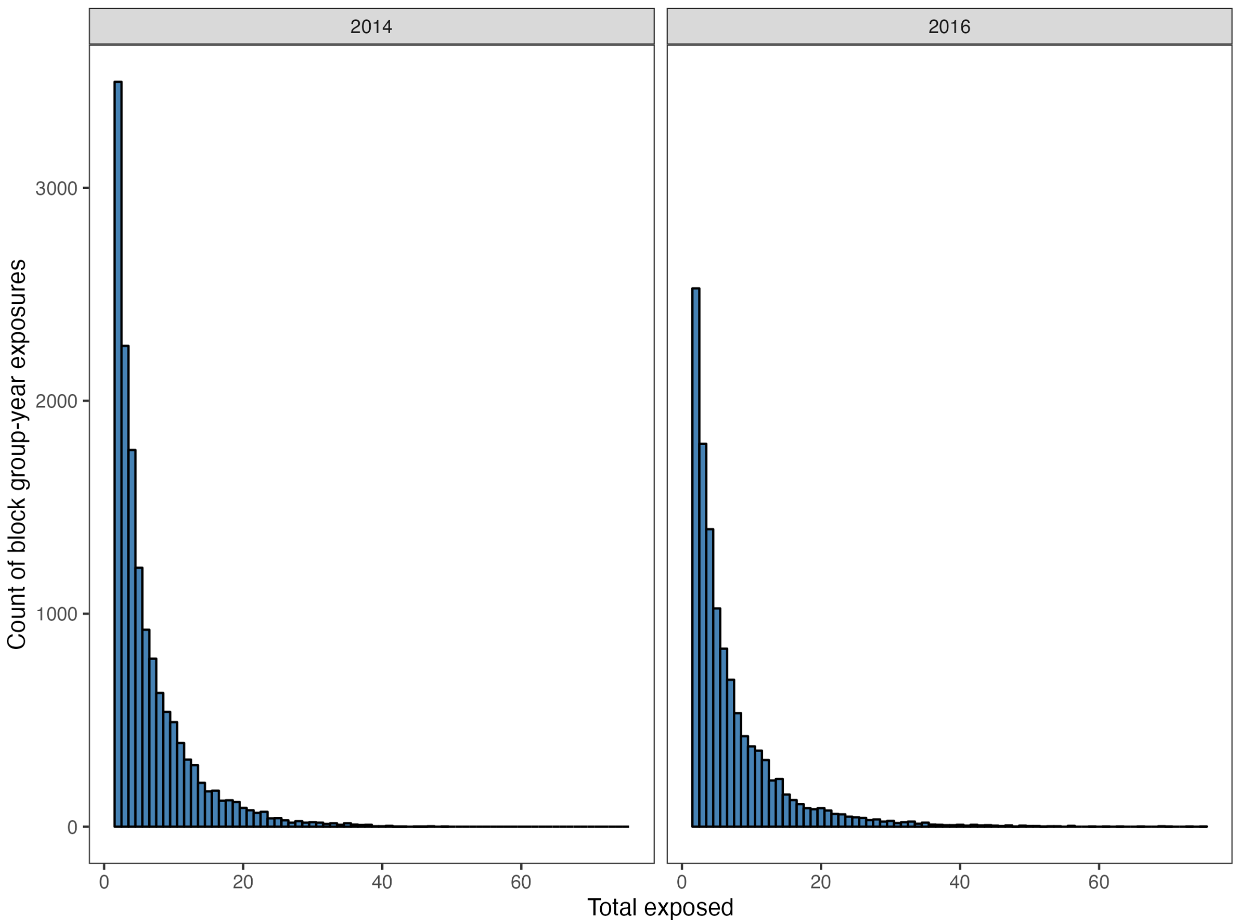


Figure A4: Previous Year Homicide Exposure (Logged)

[18]The average turnout in these pre-election year exposure groups vary by homicide exposure as well. For those exposed within 1 mile in an election year, "High" exposure block groups had an average turnout of 48.5 percent, while moderate and low exposure block groups had 48.8 and 53.5 percent respectively.

Treatement Effects by Previous Year Exposure Levels

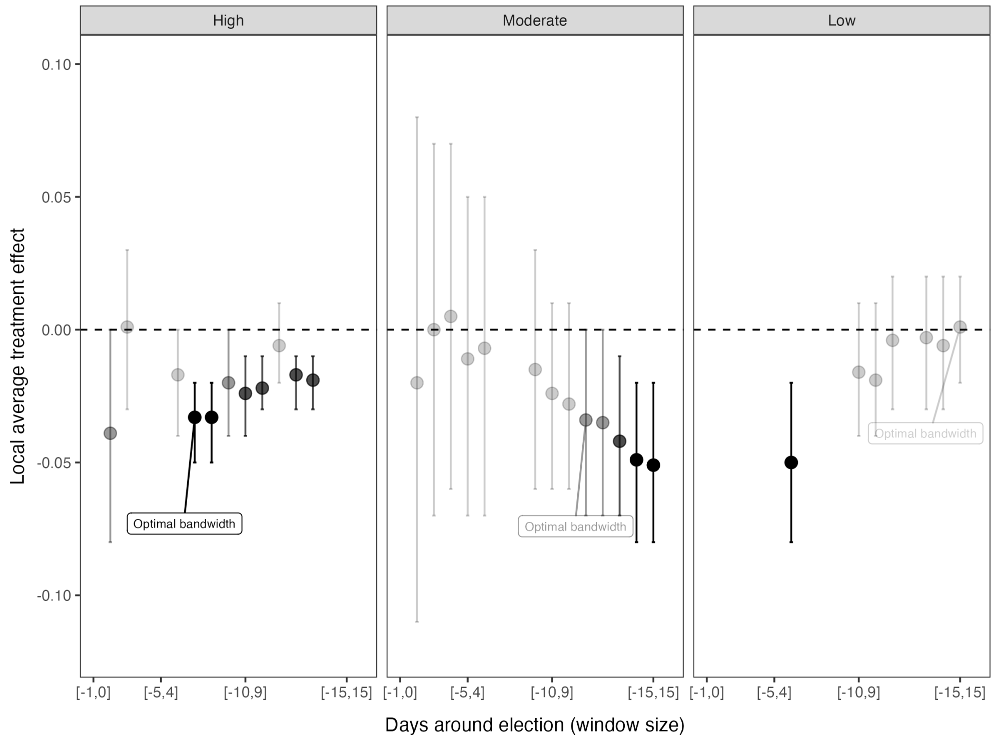


Figure A5: Local Average Treatment Effect by Previous Year Exposure Level

Figure A5 shows average treatment effects by prior-year exposure level (as defined on page 20). All windows meet a covariate balance threshold of $p > .05$, with optimal windows passing at $p > .10$. Highly exposed block groups consistently show depressive effects on turnout (2–5 percentage points). Effects for moderately exposed groups are less stable across windows, weakening their robustness. For low-exposure groups, results are also mixed—only one of seven balanced windows shows a depressive effect, casting doubt on consistency. These findings generally support the hypothesis that highly violent enclaves may be especially conducive to fear or perceptions of delegitimacy that could influence behavior, however I can't rule out the possibility of imprecision of estimation in the less violent block groups.

Victim Race and Racial Compostition of Block Groups

In the manuscript's theory and hypotheses sections, I motivate the analysis of heterogeneous effects of proximate homicide exposure based on both victim race and block group racial composition. Consistent with this framework, Figure A6 displays the racial distribution of homicide victims across block groups, using the largest window analyzed in the manuscript: 15 days before and after the election within a 2-mile radius. As shown in Table 1, block groups with a plurality Black population are generally overrepresented in the data. Notably, however, Black victims remain the most prevalent even within block groups where White residents make up the largest share of the population.

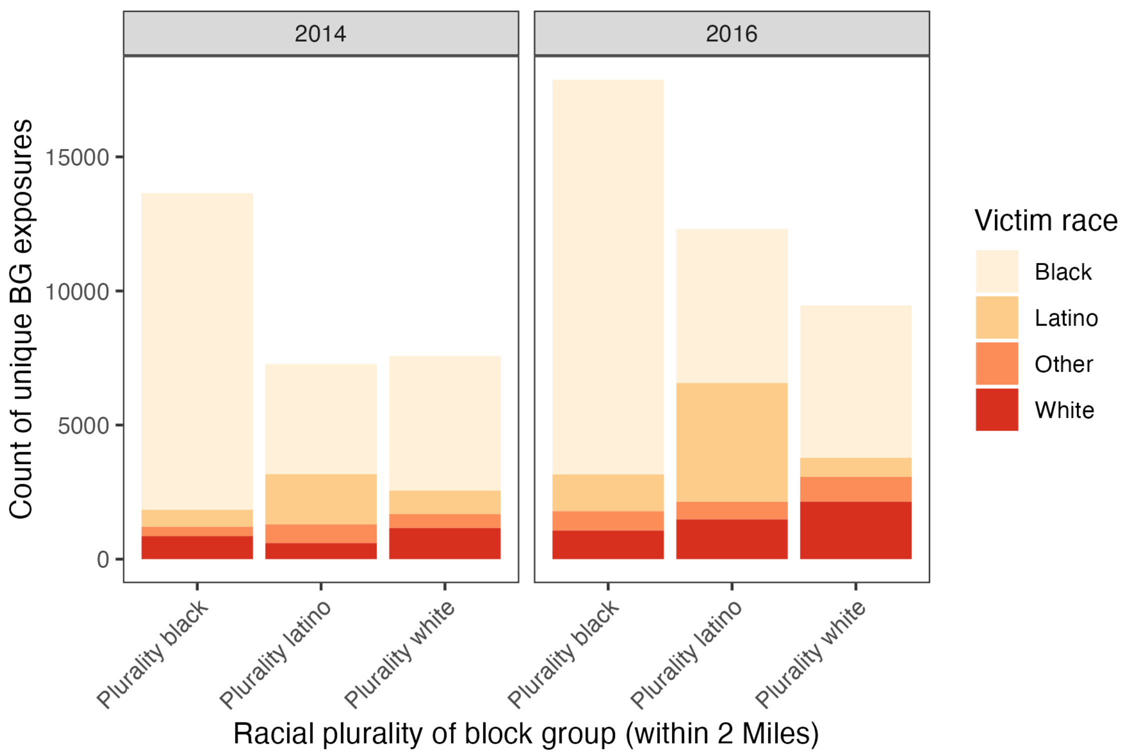


Figure A6: Race of Victims in Block Groups

Chicago Distribution

For descriptive clarity and further motivation for my empirical strategy, Figure A7 shows a city-level distribution of homicides in 2014 in the Chicago area. The white lines outline the boundaries of census tract block groups. Each dot represents a homicide victim in the Chicago area. In 2014, the Washington Post records 418 homicides, roughly in step with those reported in the FBI UCR (2015).[19] As illustrated, Black victims are the most dominant, indicated by white *x* indicated dots. Hispanic, White, and Asian victims are indicated by square, diamond, and point-place dots, respectively. The figure also includes a shaded gradient corresponding to the racial composition of residents in each census tract block group using 2020 Census data. The darkest shades of grey are those where the majority racial group is Black or African-American. The lighter grey tracts are majority Latino, and those in white are majority white. Across both years in this study (2014 and 2016), roughly 73 percent of homicides occurred within Chicago block groups that were majority Black. 19 percent of homicides occurred in majority Latino block groups, and roughly 7 percent occurred in majority white block groups. As highlighted, the majority Black block groups are significantly overrepresented among those experiencing homicides, informing my expectations for the contextual effects of violent exposures on political behavior.

[19]The FBI UCR records 411 homicides in 2014 for the city of Chicago.

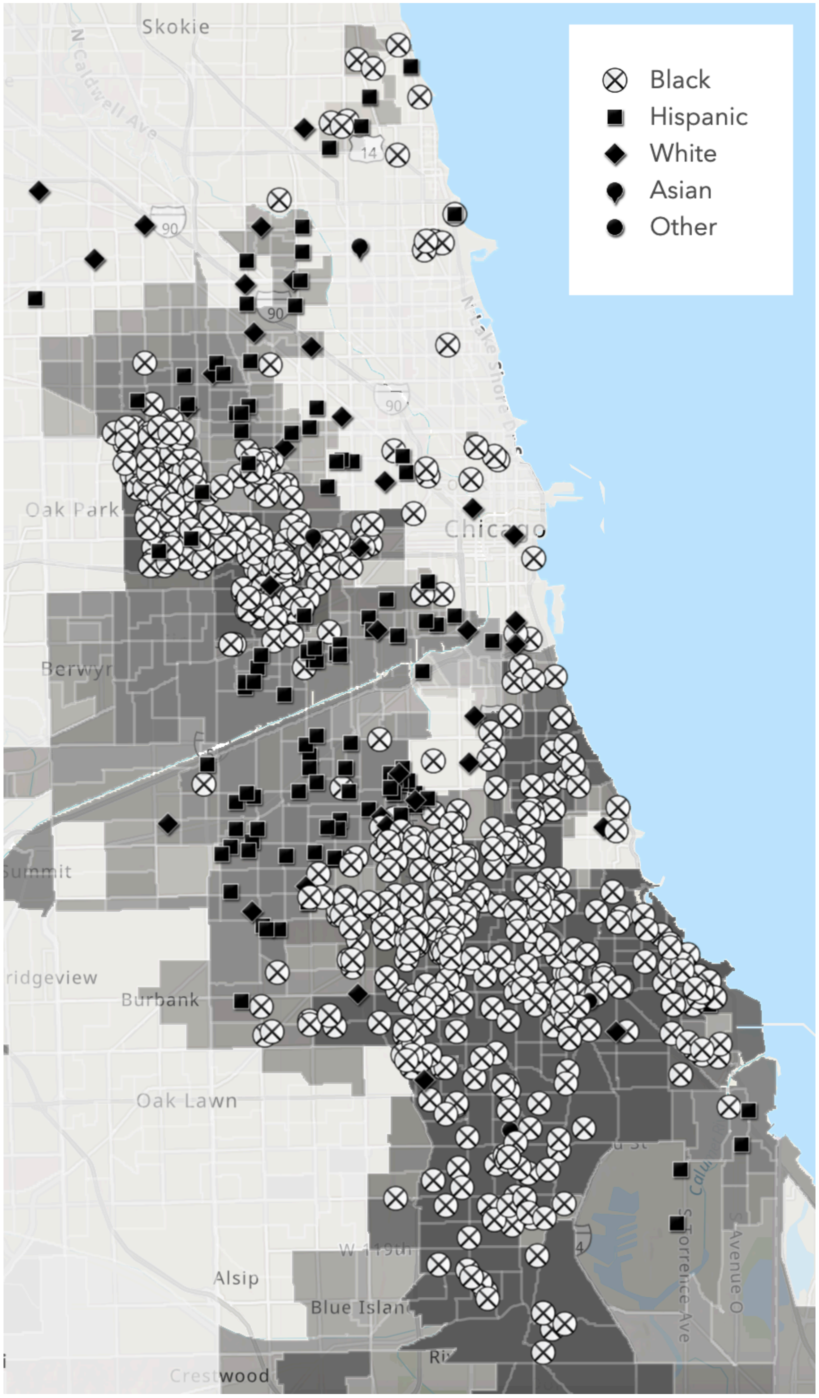


Figure A7: ArcGIS Mapping of 2014 Homicides in Chicago Area

Why the RDiT? Homicide Saturation of BG with Homicides

Table A27 presents the saturation of homicide exposures for block groups out to 2 miles. As highlighted in Table 1 of the manuscript, the majority black block groups are overrepresented in this sample. In alignment with my account of the over saturation of homicide exposures motivating expectations of heterogeneous effects, majority black block groups experience the highest saturation rate (exposures per unique block group ID) of racial groups across election years, further substantiating the expectation that these block groups may experience the greatest fear of recurrence when individual instances occur.

Table A27: Homicide Saturation by Block Group Racial Composition

Plurality	Year	Unique block groups	Exposure count	Saturation
Black	2016	6615	63058	9.533
Latino	2016	6309	42438	6.727
White	2016	9145	32992	3.608
Black	2014	7200	57521	7.989
Latino	2014	5461	24416	4.471
White	2014	8275	26642	3.220

Given the high saturation of homicides across block groups—particularly in majority Black communities—and my interest in estimating short-term causal effects of proximate exposures, I employ a Regression Discontinuity in Time (RDiT) design centered on the days surrounding a homicide event. This approach is well-suited to contexts where treatment is assigned at a discrete point in time and allows for a sharp comparison of outcomes immediately before and after exposure, while flexibly accounting for underlying time trends. Crucially, the RDiT framework relaxes the strong parallel trends assumption required by difference-in-differences models, which is especially important in this setting given the potential for differential pre-treatment trends across communities with varying baseline levels of violence. While RDiT strengthens internal validity by focusing on local contrasts, future work could leverage difference-in-differences models to explore how repeated exposures may accumulate over time and shape longer-term political behaviors and beliefs, particularly across racial or spatial lines.

Violin Plots of Treatment Status and Turnout

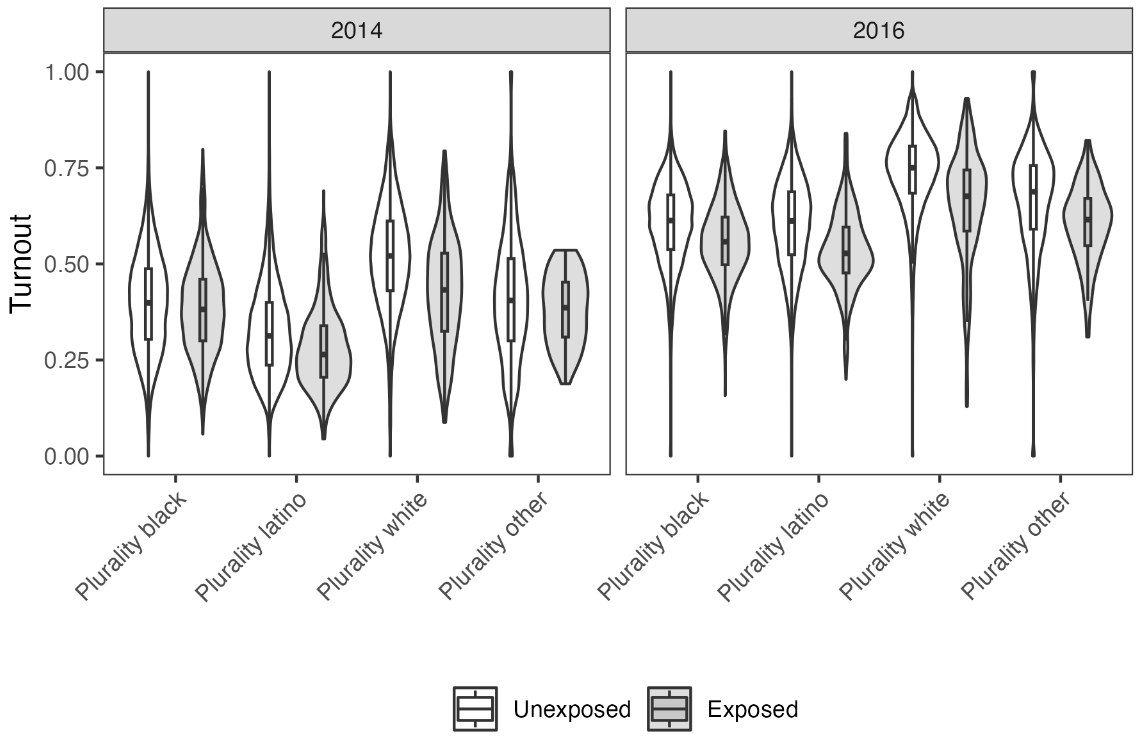


Figure A8: Distribution Plot of Block Group Turnout by Race and Exposure within .3 Miles of Homicide

In a similar vein, Figure A8 shows a violin plot of the distribution of turnout for all census block-groups in the 2014 and 2016 national elections, by exposure status and racial composition of the block group. In this context, block groups are considered exposed if they were ever (before or after the elections) within (arbitrarily set) .3 miles of a homicide during 2014 or 2016. As shown, across racial groups, there is a significant gap in turnout, conditional on a block group's proximity to homicide. Generally, the figure shows the racial disparity in turnout for exposed and unexposed groups alike. A more pronounced portion of majority Black and Latino block groups makes up the lower-turnout segments of the distributions in comparison to block groups that were majority White or "other." Noticeably, however, there also appear to be significant differences within racial groups, with exposed block groups on the lower end of the turnout distributions. Still, the many factors associated with propensity to live in close proximity to a site of homicide make identifying an effect of homicide particularly difficult.

Homicide and Turnout in U.S. Counties

Throughout the manuscript, I've highlighted that the geographic concentration of violence makes the investigation of causal estimates of the effect of exposures to homicide on turnout particularly well-suited at the micro-geographic unit. In this section, however, I zoom out to consider the broad association between homicide rates (UCR 2022) and turnout (Clary et. al 2024) in U.S. counties. Figure A9 presents this relationship using turnout for federal elections in midterm and presidential years (in 2-year cycles) between 2004 and 2020 and the logged county homicide rate within the same period, demonstrating a strong negative relationship.

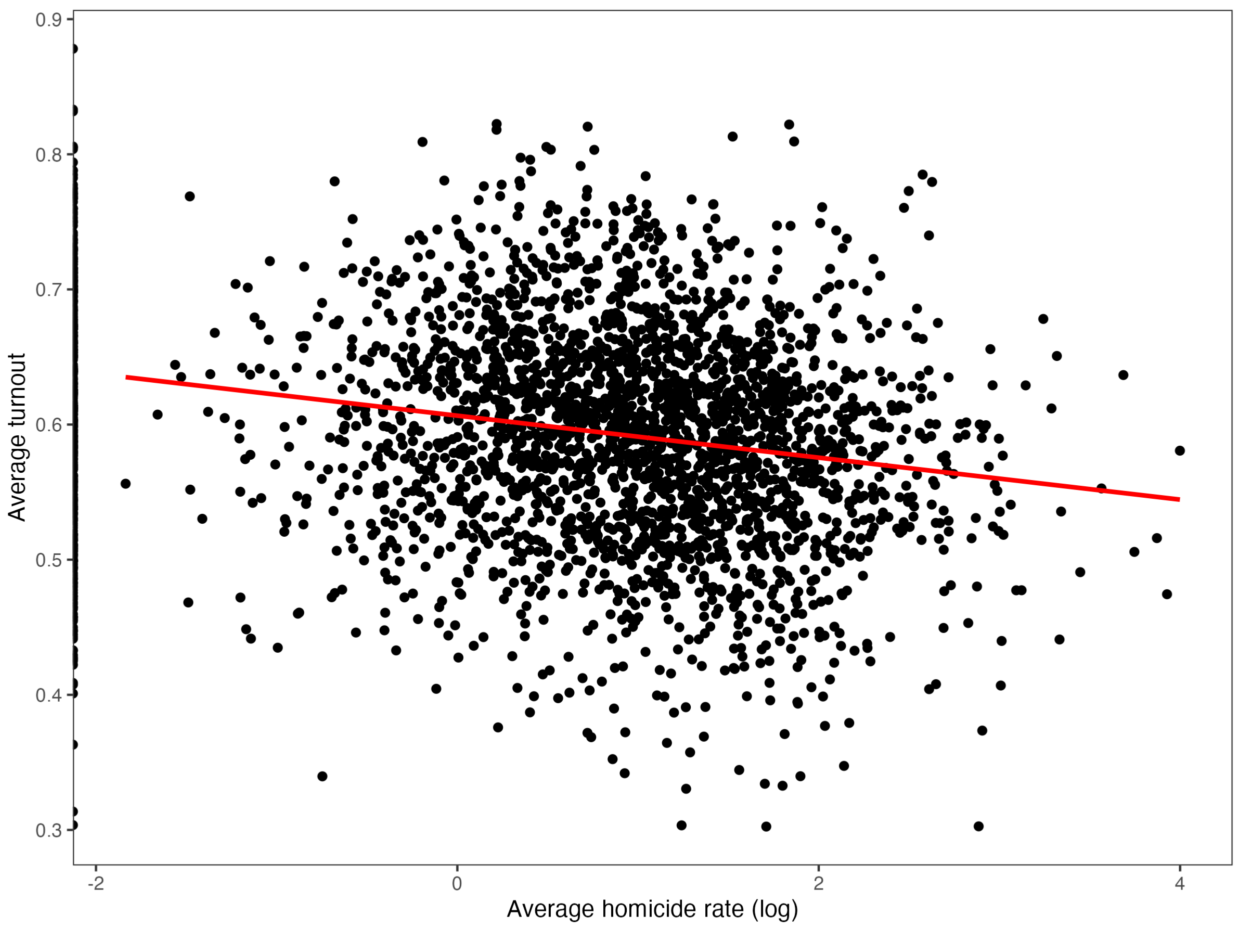


Figure A9: 2004-2020 Turnout and Homicide Rate in U.S. Counties

Treatment Effects for 2014 Federal Election

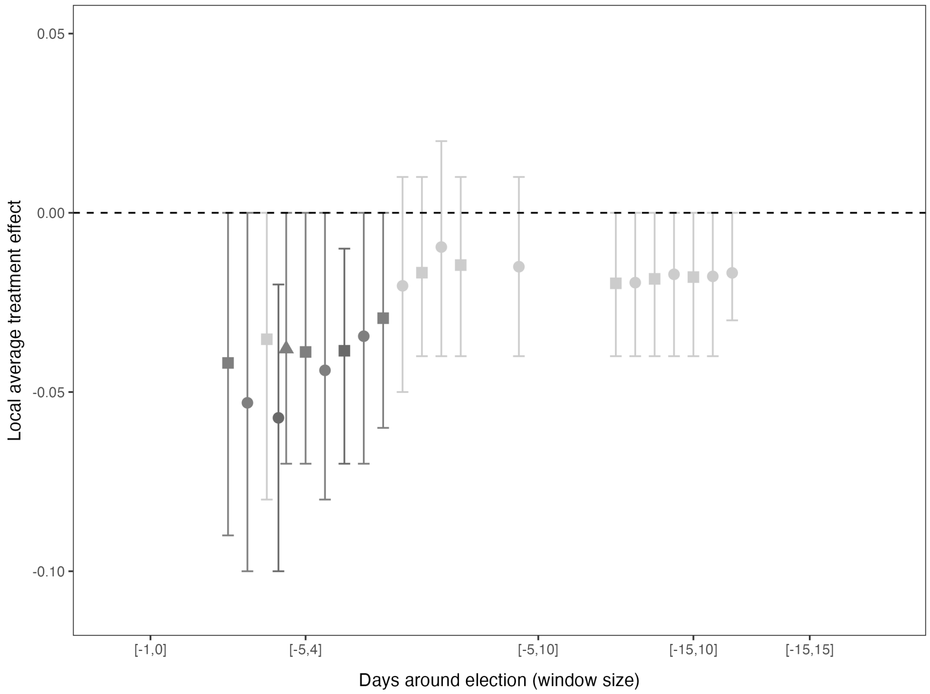


Figure A10: Local Average Treatment Effect for 2014 Federal Elections. Note: Coefficient shapes indicate the matching technique. Squares indicate nearest neighbor; Cirlce - Optimal; Trianlge - None

In this manuscript, I present the results for the 2016 elections, using 2014 turnout as a lagged dependent variable. However, Figure A10 reports the results separately for the 2014 election year.[20] While I do observe significant negative effects in some windows, the results do withstand clustering (see Table A28 (29)). This is not surprising, as midterm election voters tend to be systematically different—generally more engaged—compared to presidential election voters. The stronger results in 2016 may reflect the influence of a larger share of low-propensity voters during presidential elections, who may be particularly vulnerable to disengagement when exposed to violence. These results may be instructive for thinking about the implications of the findings for local elections, where low-propensity voters may be generally less active.

[20]L2 voterfile screenshots begin with in 2014, preventing the incorporation of earlier federal elections.

Table A28: Effect of Proximate Homicide Exposure within .25 Miles on Turnout - 2014 Election

Variable	< 0.25 miles (Nearest)	< 0.25 miles (Optimal)	< 0.25 miles (No Match)
Treated	-0.015	-0.008	-0.000
	(0.012)	(0.013)	(0.011)
(Intercept)	0.215+	0.144	0.144
N (Obs)	489	452	636
Controls	Yes	Yes	Yes

* Control variables include those in Table 2.
† Standard errors clustered at the individual homicide level.
‡ Significance levels: +p < .10; *p < .05; **p < .01.

Section B: Estimating Effects of Homicide Exposure on Foot Traffic

Model Equation for Fixed Effects Models

I estimate a fixed effect regression model that links census block group stop counts to a measure of proximity to homicide during 2022 and 2023. Specifically, I estimate

$$Y_{it} = B_1 \ \mathrm{Exposed}_{it} + \mathrm{X}_{it} + e_{it} \tag{2}$$

Where Y_{it} is the stop count for census block group i in month t. The variable of interest, B_1, represents the effect of the treatment condition, $\mathrm{Exposed}_{it}$, on Y_{it}. The variable $\mathrm{Exposed}_{it}$ takes the value 0 during the five days prior to exposure, as well as on the day of exposure itself.[21] It takes the value 1 for the five days following the exposure date. X_{it} represents block group fixed effects, allowing me to control for time-invariant characteristics of block groups that may influence population movement. In Equation (2), e_{it} is a random error term. Standard errors are clustered at both the individual homicide and block group levels to account for within-event and within-unit correlation.

[21] Given that violent crimes are more likely to occur at night (Tubbs et al. 2024), I code exposure as beginning the day after a homicide occurs.

City Data Collection and Selection

Table B1: City Source Data

Population ranking	City	State	Source data	Population (2023 estimates)
1	New York	NY	Link	8,258,035
2	Los Angeles	CA	Link	3,820,914
3	Chicago	IL	Link	2,664,452
4	Houston	TX	Link	2,314,157
6	Philadelphia	PA	Link	1,550,542
9	Dallas	TX	Link	1,302,868
12	Fort Worth	TX	Link	978,468
13	Austin	TX	Link	979,882
14	Charlotte	NC	Link	911,311
17	San Francisco	CA	Link	808,988
21	Nashville	TN	Link	687,788
22	Washington	DC	Link	678,972
24	Las Vegas	NV	Link	660,929
25	Boston	MA	Link	653,833
26	Detroit	MI	Link	633,218
30	Baltimore	MD	Link	577,193
31	Milwaukee	WI	Link	561,400
36	Atlanta	GA	Link	510,823
39	Raleigh	NC	Link	499,825
42	Miami	FL	Link	455,924
45	Oakland	CA	Link	436,504

Table B1 presents the U.S. cities included in the foot traffic analysis, selected based on the availability of publicly posted, geocoded crime data. I identified these cities by manually reviewing open data portals for the largest U.S. cities. The table includes each city's population rank, its 2023 population estimate, and a link to its crime data source.[22] These data are then spatially linked to foot-traffic block groups using the same geo-processing methods described in the RDiT section of the manuscript.[23]

[22]A handful of the largest U.S. cities do not publish their geocoded data publicly: AZ: Phoenix (5), Tuscon (33), Mesa (37); CA: San Diego (8), San Jose (12), Fresno (34), Bakersfield (47), Long Beach (44); FL: Jacksonville (10), Tampa (49); KY: Louisville (27); MO: Kansas City (38, restricted); IN: Indianapolis (16); NM: Albuquerque (32, unreliable); OH: Columbus (15); OK: Oaklahoma City (20), Tulsa (48); OR: Portland (28); TN: Memphis (29); TX: El Paso (23), Arlington, San Antonio (7); WA: Seattle (18, does not include geocodes for homicides).

[23]Block groups within .75 miles were not included in the foot-traffic data included in by Advan Research for the following cities: CA: Sacramento (35); CO: Denver (19), Colorado Springs (40); MN: Minneapolis (46); NE: Omaha (41); VA: Virginia Beach (43)

City Data Validation with FBI UCR Data

Table B2: UCR-City Data Validation

City	Year	City reported homicides	UCR homicides	Completeness
Miami, FL	2022	48	19	2.5263158
Chicago, IL	2022	684	604	1.1324503
Baltimore, MD	2022	314	287	1.0940767
Washington, DC	2022	198	197	1.0050761
Los Angeles, CA	2022	384	387	0.9922481
Atlanta, GA	2022	164	168	0.9761905
Charlotte, NC	2022	103	108	0.9537037
Detroit, MI	2022	293	308	0.9512987
Milwaukee, WI	2022	196	214	0.9158879
Boston, MA	2022	40	44	0.9090909
Raleigh, NC	2022	38	44	0.8636364
Austin, TX	2022	54	69	0.7826087
Philadelphia, PA	2022	374	514	0.7276265
Las Vegas, NV	2022	103	147	0.7006803
Nashville, TN	2022	57	83	0.6867470
New York, NY	2022	247	438	0.5639269
Oakland, CA	2022	64	121	0.5289256
Fort Worth, TX	2022	52	100	0.5200000
Dallas, TX	2022	77	157	0.4904459
San Francisco, CA	2022	14	55	0.2545455
Houston, TX	2022	43	433	0.0993072

Given the well-documented unreliability of crime data published on city open data portals, I cross-reference the number of homicides reported on these portals with those reported in the FBI's 2022 Uniform Crime Reports.[24] A key concern is that the subset of homicides published online may not be representative of the total number of homicides occurring in the city. Thus, as a rule of thumb, I include a city in the analysis only if its publicly reported homicide count reaches at least 90 percent of the corresponding UCR figure. All results presented in the manuscript hold if one includes Raleigh (at 86% reporting) and/or drops Miami.[25]

[24]Source: https://www.icpsr.umich.edu/web/NACJD/studies/39066/versions/V1/datadocumentation#.

[25]There are two agencies reporting for Miami, one reporting 98 homicides, and the one represented in Table B2, which reports 19. Given the number reported by local media outlets (reporting 47) are more in-line with the city's online data, I keep Miami in the analyses. Source: https://www.local10.com/news/local/2023/02/18/miami-police-reports-violent-crimes-continue-to-decrease/.

Foot Traffic Data Visualizations

Figure B1 illustrates the distribution of foot traffic stops for the block groups included in the models with the largest threshold (.75 miles). Note here that the identification strategy of these models isolates within unit change in stops, making it difficult to visualize the treatment effect where there is such wide variation in average stops. Nonetheless, in an effort to maintain transparency, Figure B1 presents a violin plot of block group stops in the pre and post exposure conditions.

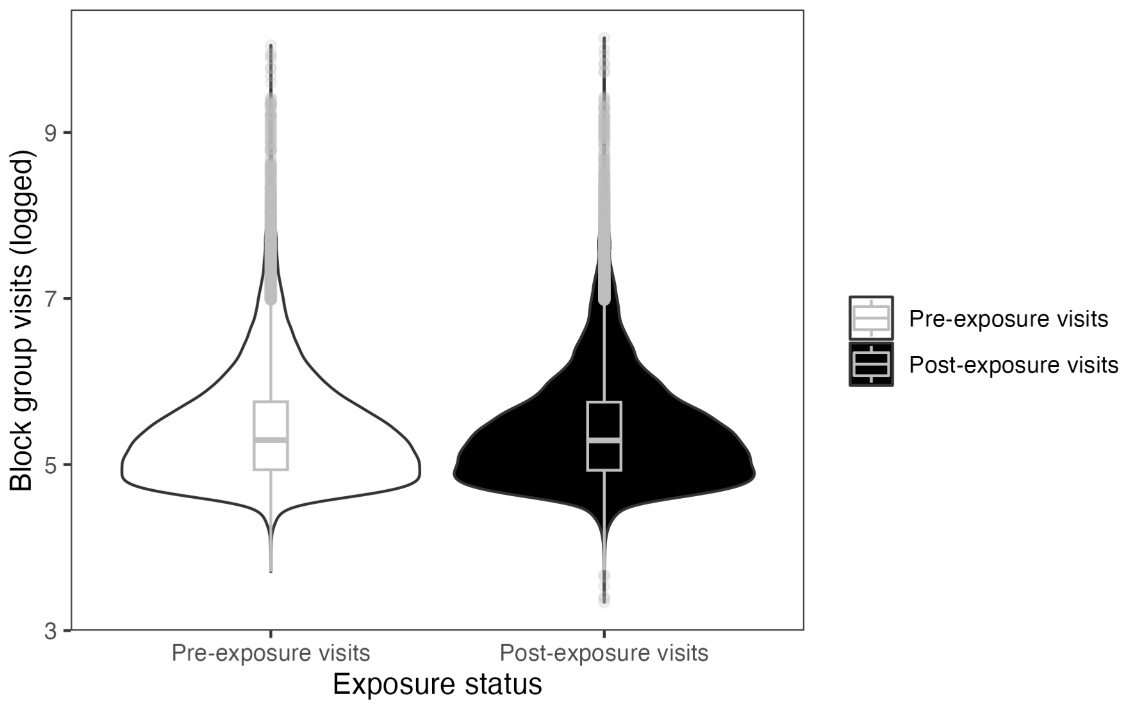


Figure B1: 2022 Foot Traffic Stops, by Treatment Status

Longer Time Bandwidths for Foot Traffic

The fixed effects models in the manuscript estimate the impact of homicide exposure at various thresholds, focusing on a five-day window before and after the event. This window is guided by the pattern observed in Figure 2, which shows a highly localized effect emerging within 5 to 15 days. While one plausible interpretation is that these patterns reflect avoidant behaviors linked to fear or anxiety about victimization, brief disruptions caused by police activity—such as taping off crime scenes—could also contribute. To address this possibility, I extend the analysis to a 10-day window, capturing the upper bound of the observed localized effect. Table B3 presents these estimates at the .5-mile threshold. Given that homicide investigations in the U.S. are often brief—particularly in firearm cases, where nearly half go unsolved—the persistence of the effect at 10 days offers additional support for the interpretation that behavioral changes are more likely driven by fear than by police presence.

Table B3: Effects for Longer Foot-Traffic Bandwidths

	< 0.5 mi	< 0.5 mi (100+ visits)
Estimate	-14.544*	-19.254*
	(6.293)	(8.297)
Observations	366072	216405
Block group exposures	17432	10305
Block group fixed effects	Yes	Yes
Weighted by visits	Yes	Yes

* Standard errors clustered at the homicide ID level.
† Significance levels: +p < .10; *p < .05; **p < .01.

Section C: Race and Carceral State Survey (RCSS) Analysis

Descriptive Data for Survey Respondents

Now turning to the analysis of the Race and Carceral State Survey, Table C1 displays the summary statistics for respondents included in the models presented in the manuscript. For the table presented in the manuscript, I used county-level crime data collected by the FBI (see Table C2 of this Appendix for handling missingness).[26] Below are the variable specific coding schemes used for this table.

Table C1: Summary Data on Survey Respondents

Statistic	Age	Race	Education	Income	Homicide rate	Violent crime rate	Population
Mean	44.809	0.720	2.247	0.378	0.354	13.821	1009978
Standard deviation	17.423	0.449	1.200	0.264	0.474	11.075	1675087
Median	42.000	1.000	2.000	0.364	0.254	11.801	408367
Minimum	19.000	0.000	0.000	0.000	0.000	0.000	1724
Maximum	89.000	1.000	5.000	1.000	6.517	114.938	9934597
Number of observations	10134.000	10134.000	10134.000	10134.000	10134.000	10134.000	10134

- Race: coded as binary (0 - Black; 1 - White). On average, roughly 72 percent of the sample was white.
- Education: coded on a scale of 0-5.
  - High school graduate (1)
  - Some college, but no degree (yet) (2)
  - 2-year college degree (3)
  - 4-year college degree (4)
  - Postgraduate degree (MA, MBA, MD, JD, PhD, etc.) (5)
- Income: rescaled to binary (0 - "Less than 10,000"; 1 - "More than 150,000") A mean value of 3.8 corresponds roughly to an income of 40,000-49,000 in USD.

[26] Source data: https://www.icpsr.umich.edu/web/ICPSR/studies/37059

Alternative County-Level OLS Results

Due to the small number of counties reporting zero homicides or violent crimes, it is challenging to determine whether this is due to missing data, underreporting, or a true reflection of their crime trends. Thus, Tables C2 and C3 present results excluding these cases and replicates the county-level estimates shown in Tables 8 and 9 of the manuscript. While removing these respondents reduces my sample size from 10,131 individuals to roughly 7,700, I still observe a positive relationship between the lagged county violent crime rate and the level of fear reported by survey respondents. Note, that I also observe a negative relationship between trust in government and the rate of violent crime within the county.

Table C2: Reported Fear of Victimization and Local Rates of Violence

	Fear of victimization					
	(1)	(2)	(3)	(4)	(5)	(6)
Violent crime rate	0.025**	0.013**	0.010			
	(0.005)	(0.005)	(0.007)			
Violent crime rate * race (white)			0.005			
			(0.007)			
Homicide rate				0.016*	0.007	0.004
				(0.007)	(0.005)	(0.007)
Homicide rate * race (white)						0.006
						(0.006)
Race (white)		-0.123**	-0.125**		-0.125**	-0.127**
		(0.009)	(0.009)		(0.009)	(0.009)
Income		-0.022	-0.022		-0.021	-0.021
		(0.014)	(0.014)		(0.014)	(0.014)
Age		0.002**	0.002**		0.002**	0.002**
		(0.000)	(0.000)		(0.000)	(0.000)
(Intercept)	0.371**	0.398**	0.400**	0.374**	0.400**	0.402**
Number of observations	7694	7694	7694	7694	7694	7694

* Standard errors clustered at the county level.
† Significance levels: +p < .10; *p < .05; **p < .01.

Table C3: Reported Trust in Government and Local Rates of Violence

	Trust in government					
	(1)	(2)	(3)	(4)	(5)	(6)
Violent crime rate	-0.009**	-0.007*	-0.004			
	(0.003)	(0.003)	(0.005)			
Violent crime rate * race (white)			-0.005			
			(0.007)			
Homicide rate				-0.002	-0.000	-0.002
				(0.003)	(0.003)	(0.005)
Homicide rate * race (white)						0.003
						(0.007)
Race (white)		0.020**	0.022**		0.023**	0.022**
		(0.008)	(0.008)		(0.007)	(0.008)
Income		0.003	0.003		0.003	0.003
		(0.010)	(0.010)		(0.010)	(0.010)
Age		-0.001**	-0.001**		-0.001**	-0.001**
		(0.000)	(0.000)		(0.000)	(0.000)
(Intercept)	0.313**	0.328**	0.326**	0.311**	0.325**	0.325**
Number of observations	7694	7694	7694	7694	7694	7694

[*] Standard errors clustered at the county level.
[†] Significance levels: +p < .10; *p < .05; **p < .01.

Zipcode-level OLS Results

The geographic distribution of violence is hyper-concentrated within specific block groups and neighborhoods, meaning that individual experiences of violence can vary widely even within the same county. As a result, county-level analyses may underrepresent those most likely to be exposed to violence. To address this concern, I reverse geo-coded homicides from the Washington Post database to their corresponding ZIP codes, linked aggregate homicide counts to RCS survey responses, and replicated the county-level analyses at the ZIP code level, clustering standard errors by ZIP code.

While the Washington Post database provides precisely measured, geo-coded homicide data, linking it with the RCSS survey introduces limitations. Because the WP data only cover the 50 largest U.S. cities, restricting the sample to recent homicides (within the prior year) substantially reduces the number of observations (N = 1,508). Although the limited power of this analysis (Table C4) cautions against strong conclusions, the results suggest a modest relationship between localized violence and fear. Specifically, a one standard deviation increase in the ZIP code homicide rate is associated with a 2-percentage-point increase in reported fear of victimization. In contrast, I find no consistent association between ZIP code–level homicide rates and trust in government. As in the main manuscript (Tables 8 and 9), I include the homicide rate as a lagged variable to assess the possibility of longer-term relationships between local violence and the proposed mechanisms.

Table C4: OLS Results of RCSS Survey Questions and Zip-Code Homicide Data

	Fear of victimization			Trust in government		
	(1)	(2)	(3)	(4)	(5)	(6)
Homicide rate	0.019*	0.007	0.001	-0.007	-0.002	-0.003
	(0.009)	(0.009)	(0.010)	(0.007)	(0.008)	(0.008)
Homicide rate * race (white)			0.061+			0.000
			(0.034)			(0.028)
Race (white)		-0.110**	-0.093**		0.043**	0.043*
		(0.020)	(0.022)		(0.015)	(0.017)
Income		0.025	0.027		0.004	0.004
		(0.035)	(0.035)		(0.028)	(0.028)
Age		0.001*	0.001**		-0.001**	-0.001**
		(0.001)	(0.001)		(0.000)	(0.000)
(Intercept)	0.446**	0.419**	0.416**	0.308**	0.341**	0.341**
Number of observations	1423	1423	1423	1508	1508	1508

[*] Standard errors clustered at the zip-code level.
[†] Significance levels: +p < .10; *p < .05; **p < .01.

Data Visualization - Fear and County Homicide Rate

Figure C1 plots the survey items that gauge fear of victimization, corresponding to the analysis in Table 7. Mirroring the main results, the figure shows a clear positive interaction between county-level homicide rates and respondents' race: White respondents become markedly more fearful as local violence increases, whereas Black respondents' reported fear remains high and relatively constant across the entire homicide distribution.

This pattern supports my hypothesis (H4a and H4b) regarding racially heterogeneous treatment effects. Since black respondents are disproportionately represented in the most violent block groups, it is not surprising that their overall fear of victimization is higher than that of white respondents. Moreover, the relatively flat line for black respondents may alleviate some concerns about unequal sentiments of fear between the treatment and control groups. Although all block groups fall within the high exposure category, the treated block groups have, on average, more homicides than their control group counterparts. Thus, the trend observed among black respondents may further support the idea that block groups are comparable regarding one of the key mechanisms proposed in the manuscript: the impact of proximate exposure via fear, which induces acute behavioral responses.

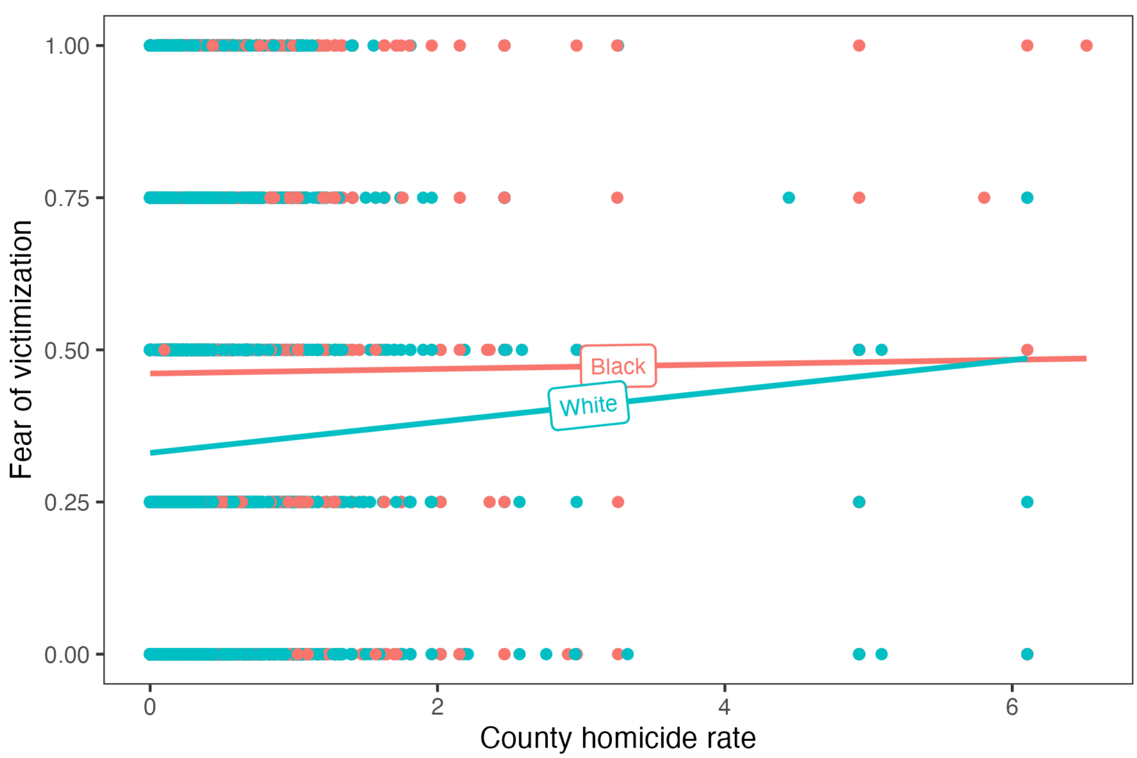


Figure C1: Scatterplot of Homicide Rates and Fear of Victimization

Data Visualization - Trust and County Homicide Rate

Figure C2 presents scatter plots of the survey item measuring respondents' trust in government, corresponding to the analysis in Table 9 of the manuscript. While the interaction between county homicide rates and race is not statistically significant, the visualization provides suggestive evidence of a negative association between trust and homicide rates among Black Americans. Although limited statistical power may attenuate this relationship in the OLS regressions, the observed pattern is consistent with theoretical expectations.

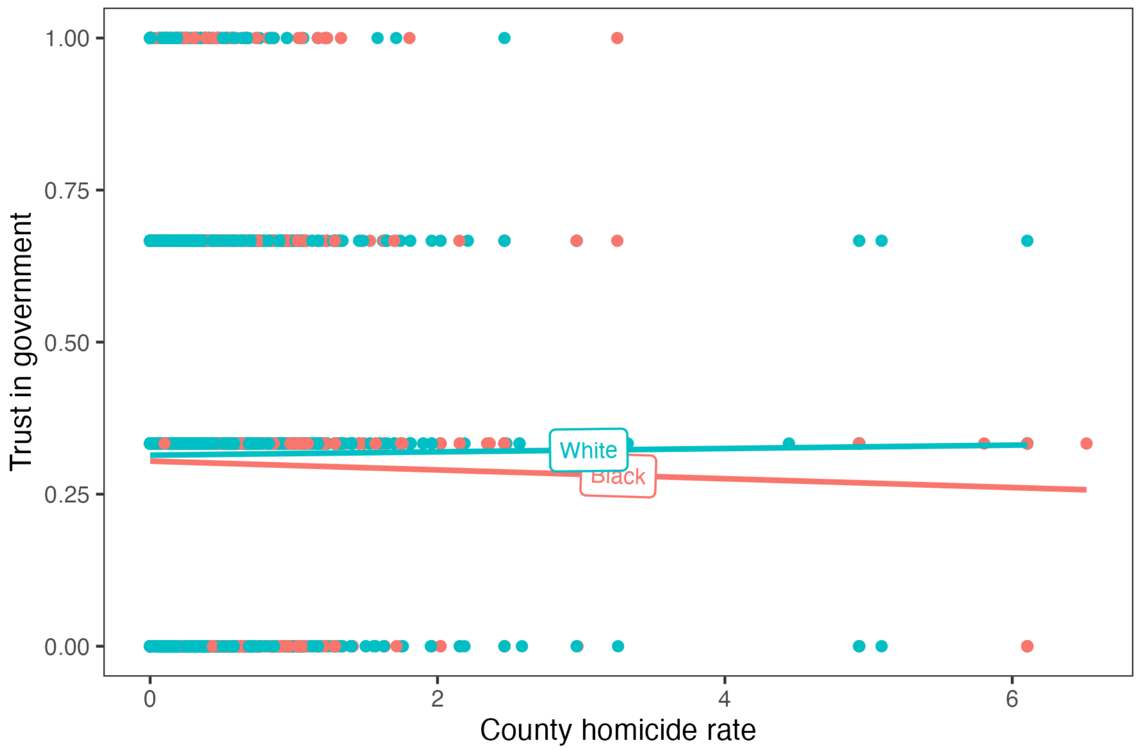


Figure C2: Scatterplot of Homicide Rates and Trust in Government