

# Contested Killings: The Mobilizing Effects of Community Contact with Police Violence

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**R**ecently, we have witnessed the politicizing effects of police killings in the United States. This project asks how such killings might (de)mobilize voters at the local level. We draw on multiple theoretical approaches to develop a theory of community contact with the police. We argue that when a highly visible event tied to government actions occurs—like a police killing—it can spur turnout. This is especially true where public narratives tie such events to government and structural causes. By comparing neighborhoods near a killing before and after election day, we estimate the causal effect on turnout. We find a mobilizing effect. These effects are larger when they “trend” on Google, occur in Black communities, or if the victim is Black. Proximity to a killing also increases support for abolishing the police. We conclude that police violence increases electoral participation in communities where narratives about racially unjust policing resonate most.


**I**n 2020, the police killed over one thousand people in the United States. That year was no anomaly: each year between 2015 and 2019, just under one thousand recorded deaths occurred during police interactions (Sullivan et al. 2019). These deaths at the hand of the state are hardly justified: at least half, and potentially as many as 80%, of police killings are unnecessary by any definition (Zimring 2017; 2020). Some of these deaths ignited protests—both locally, such as the death of David Jones in Philadelphia (Sasko 2017), and nationally, such as the murder of George Floyd in May of 2020 (Buchanan, Bui, and Patel 2020)—, and they spurred on political activism and movements pushing for change, such as Black Lives Matter and #DefundThePolice. Studies find greater police reform occurring in those areas where protests occurred (Olzak 2021), at least for protests occurring between 1990 and 2019, and that they temporarily shift public opinion (Reny and Newman 2021) and political interest (Burch 2021). However, it is unclear whether such killings typically spur on political action and activism, as not all incidents garner media attention nor spur protests. Further, few studies have questioned whether police killings are linked to other forms of political participation, such as voting, the focus of this paper.


Prior research provides conflicting expectations on whether the public may be mobilized to vote in the

wake of a police killing. Research on contact with the police and carceral state tells us that personal contact demobilizes (Burch 2011; Lerman and Weaver 2014; White 2019b), whereas proximal contact can sometimes mobilize people with respect to extra-voting activities and has a limited relationship with voter turnout (Walker 2014; 2020; White 2019a; 2022). Conversely, research on place-based mobilization tells us that voter turnout may increase in the wake of localized (negative) policy change, where a narrative connects a place-based policy to a cause, to an action (Nuamah and Ogorzalek 2021). Racialized policy feedback theory produces a similar expectation relying on similar logic: narratives provide the connective tissue between one’s racial identity and policy or experiences, and from there to political action (Garcia-Rios et al. 2021). In each case, social movements provide interpretive frames that ordinary citizens use to make sense of state-led action. These theories tell us that mobilization is more likely and effects may be amplified if that narrative resonates with the community. We argue that the same processes may be at play when it comes to events, such as police killings.

To test the mobilizing effects of police violence, we first offer a novel empirical study of the effect of police-involved killings on neighborhood-level voter turnout in 2016 and 2020 using nationwide voter files. Although recent scholarship leveraging detailed survey data has indicated that police killings can mobilize voters (Burch 2021), this project marks the first use of administrative records to test these survey-based studies in multiple elections, using real turnout rather than self-reports. While survey data gives us great insight into the individual psychological processes at play, administrative records geocoded to a very low geographic-level offer an unparalleled opportunity to test the external validity of these findings and investigate the dynamic role of

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physical proximity. We leverage the as-if random timing of a police killing shortly before or after election day and a regression discontinuity in time (RDIT) approach to test whether police killings increase turnout. We find that local communities near a police killing turn out to vote at higher rates, but only when these killings receive community attention (proxied by trending on Google). Additionally, we demonstrate that the effects we observe are concentrated in Black neighborhoods, and when the victim is Black.

Of course, knowing *that* Black neighborhoods turned out at higher rates following a police killing provides no evidence on *how* they voted. We turn to precinct-level results from a 2021 Minneapolis ballot initiative that proposed police abolition and show that voting precincts closer to a police killing were more supportive of the initiative. While this relationship is not causal, it is consistent with a causal mechanism that leads voters exposed to police violence to (a) be less supportive of the police and (b) turn to the ballot box to voice displeasure.

Our results documenting the politicizing effects of police violence allow us to make multiple contributions. First, we advance our understanding of local political mobilization by explicitly distinguishing between three types of contact—personal, proximal, and community—with the criminal legal system (CLS). We show that, in contrast to personal and sometimes proximal contact, community contact can increase turnout. In doing so, we extend the theory of place-based mobilization to span not just sudden, geographically bound *policy changes* but also *events* that can be tied to government policy or action. We draw attention to the role of social movements in providing interpretive frames (e.g., Benford and Snow 2000), noting that effects are larger following killings that more closely align with these narratives.

Second, building on prior work focused on specific localities that may have unique histories or contexts, we construct a national test showing mobilizing effects across the United States using administrative data. This builds on recent survey and survey experimental work showing that Black and white Americans understand and respond to police killings differently (Burch 2021; Jefferson, Neuner, and Pasek 2021) by not only probing identified mechanisms in a different manner but also questioning the geographical bounds of the findings and their external validity.

## PRIOR WORK ON POLICE CONTACT AND PERCEPTIONS

### Contact with the Criminal Legal System

In recent years, scholars have looked extensively at how contact (such as incarceration) with the CLS structures political participation, both inside and outside of the ballot box. Studies on the effects of police killings, on the other hand, have largely focused on nonvoting outcomes. While these studies have examined protest (Williamson, Trump, and Einstein 2018), public

opinion (Reny and Newman 2021; Wasow 2020), educational outcomes (Ang 2021), and government contact (Cohen et al. 2019) in the wake of police shootings, we focus on voter turnout, as it is one of the most common forms of political participation and closely tied to who holds elected office. We begin by summarizing recent literature on personal and proximal contact with the CLS, before considering how police killings differ from more quotidian contact with the CLS.

Prior work on contact with the carceral state has primarily focused on *personal contact* (first-hand or direct contact an individual has, such as an arrest) and *proximal contact* (close but second-hand contact an individual has via a family member, friend, or loved one's personal contact).<sup>1</sup> Carceral contact reduces well-being, which in turn may undermine political participation (Davis 2021). Scholars studying personal and proximal contact tend to focus on two explanations for how such contact structures participation in electoral politics: (1) resources are stripped from an individual, such as time and funds, limiting or barring participation even if desired; and (2) it alienates and stigmatizes individuals by communicating that they do not belong, teaching that they should not participate in government or the system, resulting in what Monica Bell (2017) terms “legal estrangement.” Similarly, Davis (2020) finds that lower participation is an indirect effect of reduced civic duty and governmental trust attributable to carceral contact.

While these studies have uncovered demobilizing effects from personal contact using survey data (e.g., Lerman and Weaver 2014) and causal estimates derived using observational data (Ben-Menachem and Morris 2022; White 2019b) alike, studies of proximal contact find either no relationship or a time-limited demobilizing effect between proximal contact and voting. This is likely because the proposed mechanisms (resources and socialization) are only indirectly imposed on those with proximal contact.

In contrast, multiple studies show *increased* likelihoods of extra-voting participation, such as protesting or contacting elected officials, among those with proximal contact (e.g., Anoll and Israel-Trummel 2019; Walker 2019; 2020), and that closer proximal ties are associated with larger substantive relationships (Walker 2014). A key mechanism in the translation of proximal contact into political participation is that individuals understand the experience of their family member or friend in a broader context tethering their experience to a narrative of injustice centered on the government as cause prompted by one's racial or socioeconomic group membership. Indeed, Walker (2019; 2020) shows that this is true for Black Americans, pointing to social movements as one mechanism through which Black Americans come to a more structural understanding of their place in the CLS.

In fact, much of the established work shows that participatory responses vary by respondent race. On average, work here shows that Black individuals

<sup>1</sup> For an overview, see White (2022).

deactivate when they come into contact with the police (e.g., Lerman and Weaver 2014), whereas white individuals may actually become more likely to participate when they experience light police contact (Christiani and Shoub 2022, but see Ben-Menachem and Morris 2022). Key reasons for these differential results may lie with their theoretical mechanisms: when Black individuals are engaged by the state in this manner they encounter a reinforcing message of their place vis-à-vis the state, whereas white individuals may encounter new and angering messages concerning their status and citizenship. Similar patterns have been shown for those with personal contact with other government bureaucracies that stigmatize or transmit messages that one does not belong (e.g., García-Montoya, Arjona, and Lacombe 2021; Michener 2018; Soss 1999).

Our focus here, however, is on *community contact*, which is diffuse contact an individual has with the police via community incidents, word of mouth, and/or the media. In other words, this sort of contact comes not from direct, personal experience, nor the experience of a loved one—rather, it is mediated through an individual's relationship with their community. While she does not call it community contact, Burch (2013) questions whether greater levels of surveillance in the form of higher rates of contact with the carceral state (i.e., higher incarceration and parole rates) relate to differential levels of *community* political participation. She finds that communities with high incarceration or parole rates perceive and internalize messages that they do not belong and that communities under greater surveillance turn out to vote at lower levels. These findings are mirrored by Morris (2021), who finds evidence that the recent incarceration of community members reduces neighborhood-level turnout. Similarly, Anoll, Epp, and Israel-Trummel (2022) find that residing in areas with large racial disparities in traffic stop outcomes depresses opinion of the police. However, they do not find a relationship between racial disparities in policing outcomes and political participation, similar to Laniyonu's (2019) study finding mixed effects from NYC's Stop, Question, and Frisk program. Each of these works focus on geographically localized effects, which we adopt here as well. We build on these studies by placing them in conversation with additional theoretical frameworks.

### Perceptions of the Police

In addition to this work specifically examining political behavior in response to contact with the CLS, studies have questioned how the public views the police and government more generally—with some studies specifically questioning how police killings alter evaluations and opinions of police departments. These studies find that Black and white civilians differ in how they view the police and CLS more broadly, with Black Americans viewing the police and the CLS less favorably and perceiving greater levels of inequality and injustice than their white counterparts (e.g., Gibson and Nelson 2018; Peffley and Hurwitz 2010). One important aspect to note about these studies is that a majority of

respondents typically have never had either personal or proximal contact with the police. Impressions of the police are instead formed by community contact, interactions with peers and social movements, interactions online, the news media, or television, among other possible sources.

There are two proposed key reasons as to *why* this gap exists. First, Black and white Americans had and continue to have vastly different experiences and relationships with the police and the carceral state, with evidence of this ranging from historical accounts tracing the origins of today's police to slavery (e.g., Blackmon 2009) and of targeted police violence against Black communities in the United States (e.g., Hinton 2021), to large-n studies of racial disparities in traffic stops (e.g., Baumgartner, Epp, and Shoub 2018; Epp, Maynard-Moody, and Haider-Markel 2014; Pierson et al. 2020). Further, studies show that one does not need direct contact with an incident of injustice to be motivated by it—especially if such injustice is understood to be targeted based on group affiliation, such as race (Anoll 2022) and to have shaped the social experience of being Black in America (White and Laird 2020). Second, the lessons imbued and messages sent to Black and white Americans when they interact with the police or otherwise observe them are different, which produces different responses in individuals belonging to those groups and their broader communities (e.g., García-Montoya, Arjona, and Lacombe 2021; Maltby 2017).

Work exploring the effects of police use of force, and police killings in particular, have built on these observations. Beyond showing that Black and white Americans perceive and evaluate police shootings differently, they find that the public responds differently to a police shooting or use of force incident depending on the civilian's race (e.g., McGowen and Wylie 2020; Streeter 2019), as civilian race implicitly taps into latent associations with deservingness (Israel-Trummel and Streeter 2022) and criminality and threat (Porter, Wood, and Cohen 2021). Others show that the extent to which a killing fits the narrative of a wholly unjustified killing based not on threat but on race—the killing of an unarmed Black civilian complying with the officers—moderates responses (Burch 2021; Jefferson, Neuner, and Pasek 2021). We also now know that living in areas with a greater number police killings may decrease perceived external efficacy, especially among Black respondents (Branton, Carey, and Martinez-Ebers 2021). Our study joins this recent scholarship and moves it forward by using administrative and observational data, rather than survey responses, to study the effects of killings on turnout.

### THEORIZING COMMUNITY CONTACT AND LOCAL MOBILIZATION

While prior work has provided key insights in this space, our focus may be sufficiently distinct that different theoretical mechanisms may be more appropriate. Community contact is distinct from personal and

proximal contact, but police killings may also be distinct vis-à-vis other forms of police contact, as it is extremely harsh and far less common. More pointedly, for any given individual in the United States, only one in two thousand men across their lifespans and one in thirty-three thousand women are at risk of being shot and killed, with these rates being lower for white individuals and higher for Black ones (Edwards, Lee, and Esposito 2019), whereas the risk for being pulled over, being arrested, or being incarcerated is much higher.

The (relative) rarity of police killings means that most voters will not have personal exposure to the event. Instead, as we discuss below, frames provided by social movement actors that draw attention to structural forces may help transform exposure at the community-level into more expressly political responses. Moreover, while one's personal risk of being shot and killed is rare, living in proximity to a police killing is not: according to our data, 70% of Americans live in a county with a killing within 6 months of the 2016 or 2020 elections, 63% live in a block group within 10 miles of one of these killings, 27% within 3 miles, and 6% within 1 mile.

We draw on two theoretical streams to situate our theorizing of community contact. We begin by discussing how theories of place-based mobilization inform expectations about community contact, and then move to a discussion of how the intersection of police killings and recent social movements might create conditions ripe for mobilization at the local level.

### Place-Based Mobilization

In their theory of place-based mobilization, Nuamah and Ogorzalek (2021) describe how citizens respond to geographically concentrated policy change, where mobilization and engagement are based on community context. Three key characteristics of geographically concentrated policy effects are: (1) they occur in some geographical areas of a city or state but not others; (2) members of the community who are not directly, personally affected can still discern and feel the effects of the policy; and (3) members of the community understand the policy change as particularly targeted at them, which is communicated via narratives. In the face of geographically concentrated *negative* policy effects—or policy threat—community members will mobilize to oppose them. For example, in 2012, Chicago initiated the closure of 49 public schools throughout the city that disproportionately targeted communities of color, which narratives surrounding the closures highlighted. In the wake of these closures, which resonate with a narrative of the Black community in Chicago being targeted, Nuamah and Ogorzalek (2021) document increased mobilization (turnout and other forms of participation), and an uptick in support for reform of the structure of the city's school board. Of particular import for our study is the mountain of work detailing the role race plays in policy threat: as Garcia-Rios et al. (2021) discuss, communities of color, particularly Black Americans, have both a group consciousness and understand that many government

polices are targeted at their community. Studies have found that electoral mobilization from racialized policy threat can extend to Latinos (Barreto et al. 2009; White 2016), Arab Americans (Tam Cho, Gimpel, and Wu 2006), and Asians (Phoenix and Arora 2020).

While both Nuamah and Ogorzalek's (2021) place-based mobilization theory and these policy-threat-based explanations center on responses to specific (proposed) policies, the underlying logic may also apply to *events*—specifically those that can be understood to be caused by government decisions, actions, or inaction. Police killings may be one such event, especially those that “fit the script” (e.g., Porter, Wood, and Cohen 2021) of what Americans have come to understand as unjust or unnecessary killings (e.g., Burch 2021; Jefferson, Neuner, and Pasek 2021). Further, such narratives may particularly resonate with Black communities and Black individuals: prior research shows that Black individuals in the US respond differently than white individuals to police violence (Gibson and Nelson 2018; Peffley and Hurwitz 2010).

Following the logic of Nuamah and Ogorzalek (2021), we do not expect that everyone will respond in the same manner. Rather, as with geographically targeted policies, events or incidents occurring within a (discrete) geographic area are likely to impact those in the immediate area to a greater extent than those farther away. Additionally, those in the immediate area may be more likely to know something happened via local media, (online or in person) community groups, or local organizations, which means even though they are not directly, personally affected they can still discern the effects of the incident. Further, community contact may be meaningful because of emotional connections to the place and community heightening the response to events occurring within the community, and it increases the possibility of a given individual actually witnessing some part of the incident or aftermath (e.g., walking by the police tape, hearing helicopters or sirens, etc.) by being physically proximate or having a traceable, close degree of connection with the victim.

### Social Movements and Community Engagement

Since the killing of Trayvon Martin in 2012, the Black Lives Matter movement has “inspired a new generation of protests against police and vigilante violence toward Black people, one that would go on to highlight systemic racism in nearly every aspect of American life” (Thebault 2022). Place-based police killings are not occurring in a vacuum but rather take place amidst a rich host of narratives that help ordinary citizens make sense of police violence.

In fact, social movement literature indicates that principal actors in movements such as #BLM are expressly engaged in the political work of creating meaning out of social phenomena. In the case of Black Lives Matter, activists have drawn connections between discrete acts of police violence and larger patterns of racial inequality in the history and contemporary reality of the United States (e.g., Clark,

Dantzer, and Nickels 2018), drawing on and building from prior narratives used to understand incidents of police violence and racially disparate policing. These conceptual maps that social movements help individuals to draw are known as “frames,” which, Benford and Snow (2000, 614) explains, “help to render events or occurrences meaningful and thereby function to organize experience and guide action.” BLM activists used multiple frames, as Phelps, Ward, and Frazier (2021) details using interviews and archival materials in Minneapolis following the Floyd murder. There were both “radical” and “reformist” frames, seeking to fundamentally alter Americans’ views of the police (in the first instance), or to work within traditional models of police reform (in the second). Regardless of the *specific* frame used, however, movement organizers offered narratives to move residents from viewing a police killing as a single, isolated event to viewing it as part of a broader pattern of state-sponsored violence. These frames were often developed in a distributed nature on Twitter and other social media sites (Ince, Rojas, and Davis 2017).

While much of the focus of the media has been on BLM action outside the voting booth, the links between protest movements and electoral politics are clear. The Tea Party Movement, for instance, was responsible for boosting Republicans in the 2010 midterms (Madestam et al. 2013), while the 2017 Women’s March increased female candidates’ vote shares (Larreboure and González 2021). Wasow (2020) shows how protests for racial justice in the 1960s—protests echoed by contemporary social movements—influenced the media, political elites, and public opinion. This work is part of a long history documenting how prior social movements and campaigns provide platforms for communities to push for change: by organizing and then mobilizing, which includes going to the polls (e.g., Alonso and Mische 2017; Minkoff 1997; Shaw 2009).

These examples also extend to the particular case of Black Lives Matter. BLM protests in the summer of 2020 have been linked to higher support for Democratic candidates that fall (Klein Teeselink and Melios 2021), and BLM activists encourage supporters to engage in electoral politics in addition to other political activities (Tillery 2019). And, while 2020 saw widespread actions all across the United States, there is evidence of localized effects: police killings are linked to greater protest formation (Williamson, Trump, and Einstein 2018), and protests focused on police brutality play a role in the implementation of reforms such as citizen review boards (Olzak 2021). Of course, given the explicitly racial dimension of BLM, these frames are likely to be particularly resonant when the violence is directed against Black Americans.

### A Theory of Local Mobilization

Recent work from political scientists document how particularly salient local policy changes can spur political action at the very local level (e.g., García-Montoya, Arjona, and Lacombe 2021; Nuamah and Ogorzalek 2021); at the same time, long-standing work from

scholars of social movements shows how activists can deploy frames to help residents understand local events in broader contexts, and provide the tools for reinterpretation of government action (Benford and Snow 2000; Minkoff 1997). Our theory of community contact with the carceral state lies at the intersection of these theoretical streams. In short, we expect police killings to serve as catalytic “policy” events that fall on ground made ripe for politicization thanks to social movement actors.

Our theory of community contact implies that: *In communities where a police killing recently occurred, voter turnout will increase (H1)*. Here, this means the turnout of census block groups near a police killing immediately before the 2016 and 2020 elections relative to those near killings immediately afterwards.

To test the influence of social movement actors on the turnout of the community, we turn to data from Google Trends. Much of the BLM activism in recent years has taken place in internet spaces, with activists leveraging Twitter, Facebook, and other social media platforms to draw attention to police brutality (Carney 2016; Cox 2017; Mundt, Ross, and Burnett 2018; Tillery 2019; Wilkins, Livingstone, and Levine 2019). In the absence of direct measures of BLM activity on the ground or social media surrounding each killing, Google searches provide some indication that community members both gain information about a local police killing, and increase the likelihood the searcher encounters narratives linking killings to political action. Thus, we should see an increase in Google search activity related to the victim when a particular killing is taken up by social movement activists. We expect that *voter turnout will increase more in communities near police killings when those killings trend on Google (H2)*.

A key aspect of racialized policy feedback and place-based mobilization is that narratives will resonate differently with those belonging to historically marginalized communities in the US. Similarly, racial identity and positionality are central to the Black Lives Matter movement and the political action that has followed. This is borne out in studies using survey data and survey experiments to examine how the public views the police: as discussed above, Black Americans view the criminal justice system and the police more negatively than their white counterparts, which translates into different responses when presented with police misconduct and uses of force. Further, the standard narrative that maps police violence to political action casts a specific type of individual as the victim of unjustified police violence: Black civilians and often Black men. Killings that adhere to this script more easily can be connected to political action as a response echoing work on how the social constructions of target populations inform policy and policy feedback (Boushey 2016; Schneider and Ingram 1993). In turn, these points imply that: *turnout effects will be concentrated in Black neighborhoods (H3a) and/or following the killing of a Black victim (H3b)*.

Further, we would expect not only turnout to increase—especially among Black communities and if the victim is Black—but also support for policies

addressing the problem. Voters who feel targeted by government action—police killings being an extreme example—appear to view the ballot box as an apt venue for registering their anger (e.g., Tam Cho, Gimpel, and Wu 2006; Towler and Parker 2018; White 2016), and the social movement frames providing the mobilization are of course critical of the police (e.g., Phelps, Ward, and Frazier 2021). Surge in turnout, then, more likely reflects anger with—and not affirmation of—the state’s practices, just as past work on place-based mobilization uncovered support on the side of proposed reforms (García-Montoya, Arjona, and Lacombe 2021; Nuamah and Ogorzalek 2021). Policing reforms have infrequently been put to a popular vote, so we focus on one recent proposal appearing on a local ballot in 2021: abolition of the police in Minneapolis. We expect that: *geographical proximity to a police killing will be associated with support for policing reforms on the ballot (H4).*

Before proceeding, it is important to note that the setup of our study cannot specifically distinguish between proximal and community contact. Our measures of neighborhood-level turnout will surely include some individuals who personally knew the victim, and who were in proximal contact with the killing. Moreover, there is no bright-line dividing proximal and community contact; residents may well interpret the extrajudicial killing of a neighbor, who they knew personally, using frames newly provided by BLM activists. We expect, however, that *most* individuals in a neighborhood did not closely or personally know a victim. The average police killing was within a half-mile of block groups home to roughly 2,500 citizens of voting age, and when we expand the radius to a mile, this number increases to 8,900. With such a large number of “treated” citizens, we expect community contact to best explain observed responses. Nevertheless, future work should more directly explore how individuals who might be in community with a victim—without knowing the victim personally—are politicized by the carceral state.

## DATA ON POLICE KILLINGS

To identify where police killings occurred, we rely on data about extrajudicial killings from two sources: the *Washington Post’s* Fatal Force Database,<sup>2</sup> and Mapping Police Violence.<sup>3</sup> The Fatal Force data begins in January 2015, whereas the Mapping Police Violence data begins in January 2013. These two datasets record about 6,500 unique police killings between January 1, 2016, and August 31, 2021. The data collected by the *Washington Post* is compiled by “culling local news reports, law enforcement websites, social media, and by monitoring independent databases” (Tate et al. 2016). The Mapping Police Violence data, meanwhile, comes

from official use of force data collection programs in some states and the crowdsourced Fatal Encounters database. Although the Fatal Force data include only information about individuals killed by a police *shooting*, Mapping Police Violence includes all known killings, regardless of how the individual died. This may bias our results toward (well-)known killings, especially in cities and states that do not have a mandatory reporting systems. However, there is not a more comprehensive, national source than these commonly-used open sources of data, as official government sources systematically under-report police violence (GBD 2019 Police Violence Subnational Collaborators 2021).

We include all police killings that appear in at least one of the two datasets, with most police killings observed in both (87%). About 81% of all records are already geocoded. We geocode the remaining records using the Texas A&M GeoServices tool,<sup>4</sup> retaining only those records coded with at least 95% certainty. Between the original providers and the Texas A&M tool, we have the coordinates for 91% of recorded killings that occurred between 2016 and 2021.

We specifically look at killings that occurred in the 6 months before or after the presidential elections in 2016 and 2020. As the map in Figure 1 shows, killings occurred all around the country in 2020. While killings are concentrated in urban areas, with the exception of Rhode Island, *every state and Washington, D.C.* saw a police officer kill someone in the 6 months before and/or after the 2020 election. In 2016, every single state and Washington, DC was home to a killing; that map is presented in the Supplementary Material.

These data are then combined with shapefiles made available by the U.S. Census Bureau for each block group in the country to measure neighborhood proximity to a police killing. We calculate the distance between each neighborhood’s geographical center (“centroid”) and each police killing to construct our proximity measure. Our use of block group distance to police killings probably biases our results toward denser areas, where the smaller geographical size of block groups increases the number whose centroids are physically close to any given shooting. The average killing within 6 months of either the 2016 or 2020 elections was within a half-mile of 2.6 block groups’ centroids and a mile of 9.2. About 4.5 million citizens of voting age lived in a block group whose centroid was within a half-mile of a police killing in this window; this number rises to 14.7 million when we expand the radius to 1 mile.

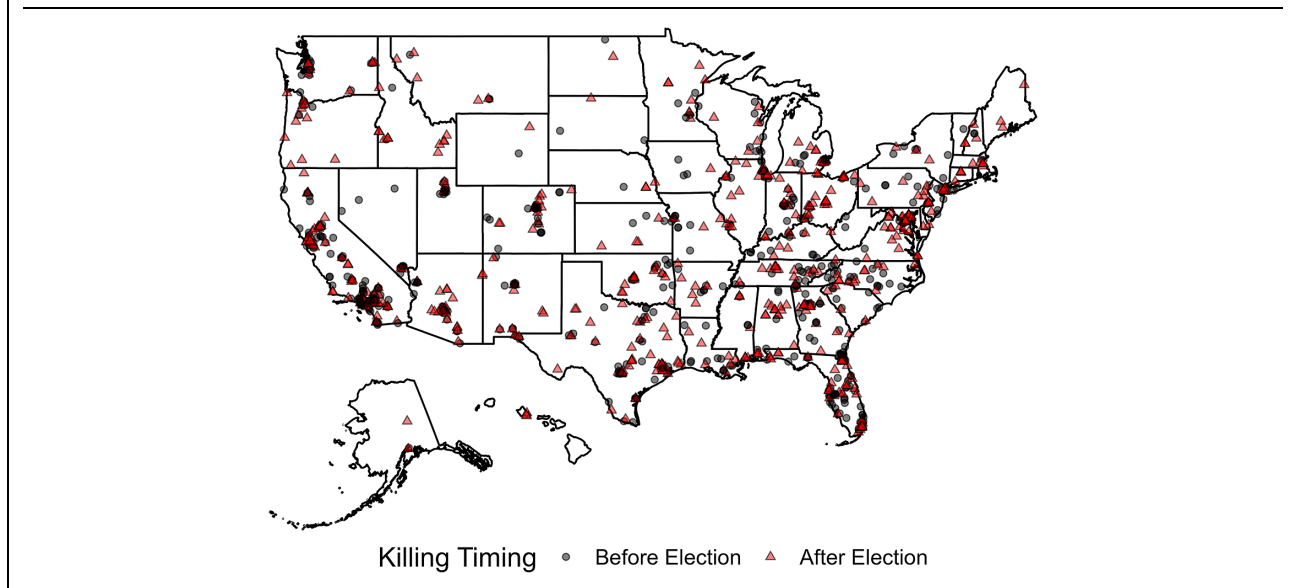
## TESTING THE EFFECTS ON VOTER TURNOUT

We begin by testing whether extrajudicial police killings structure voter turnout in the surrounding area. To estimate turnout, we leverage two datasets: Snapshots of the registered voter file, and citizen voting age

<sup>2</sup> <https://www.washingtonpost.com/graphics/investigations/police-shootings-database/>.

<sup>3</sup> <https://mappingpoliceviolence.us/>.

<sup>4</sup> <https://geoservices.tamu.edu/>.

**FIGURE 1. Police Killing within 6 Months of 2020 Election**

population (CVAP) estimates from the U.S. Census Bureau. L2 Political collects and distributes these registered voter files, which are widely used in political science research. The records are geocoded and indicate whether voters participated in a given contest. We specifically use snapshots of each state's registered voter file from L2 shortly following each of the 2014–2020 federal elections.<sup>5</sup> By aggregating these individual-level turnout records, we estimate the number of ballots cast in each block group for these four elections. Turnout estimates from 2014 and 2018 are used as controls in our primary models. Due to the size of these individual-level files, we opt to include aggregated block group level turnout estimates in the Dataverse, and include the code used to construct these datasets.<sup>6</sup>

Measuring turnout as a share of *registered voters* has theoretical drawbacks, as voter files are not always reflective of who is currently residing in the area and eligible to vote. To circumvent this problem, we estimate turnout by dividing the number of ballots cast in each block group by the CVAP of that block group. Turnout in each year uses the 5-year CVAP estimates ending with the year of that election.<sup>7</sup>

Using the estimated turnout for each block group in the country in 2016 and 2020, we leverage a RDiT to test whether proximity to police killings influences turnout. An RDiT requires an observable “cut-point” date, with only observations on one side of the cut-

point receiving the treatment. The cut-point must also be unrelated to the treatment. Here, election day serves as the cut-point, and we assume that police killings occur as-if at random with respect to election day. More specifically, block groups near a police killing *before* election day are the treatment group, whereas those near a police killing *after* election day are controls. Because a police killing that occurs after election day cannot influence turnout, any discontinuity between pre- and post-election block groups can be attributed to the police killing.

To make credible causal claims using the regression discontinuity framework, we must assume observations on either side of the cut-point are highly comparable except for their placement relative to the cut-point. Ensuring the validity of this assumption is perhaps especially important in this case: residential segregation coupled with past findings about racially distinct responses to police violence means that ensuring the comparability of neighborhood composition across the cut-point is key. Using 5-year Census Bureau's American Community Survey (ACS) estimates, [Table 1](#) shows that this may be too strong of an assumption. Block groups where a police killing occurred before the election are whiter, less Black, and had higher turnout in the previous midterm election. The table displays the characteristics of all block groups whose centroid was within 0.3 miles of a police killing in the 6 months before or after the election.

To account for differences between observations on either side of the cut-point, we employ entropy balancing, which has previously been used to improve the validity of regression discontinuity designs (Hainmueller, Hall, and Snyder 2015). Entropy balancing assigns control observations weights, such that the control group mirrors the treatment group along a set of identified covariates. Here, block groups where a police killing occurred after the election are weighted

<sup>5</sup> Following the recommendation of Kim and Fraga (2022), we list the date of each snapshot in the Supplementary Material.

<sup>6</sup> Although North Dakota does not have voter registration, L2 also makes a list of participating voters available for that state which we use for our turnout estimates.

<sup>7</sup> To maintain constant neighborhood definitions across time (and avoid the re-drawing of block groups following the 2020 census), we use 2010–2019 block group definitions throughout. This means that the 2020 numbers use 2019 ACS CVAP and demographic estimates.

**TABLE 1. Demographics of Block Groups with Police Killings**

	Not in dataset	Treated	Unweighted controls	Weighted controls
<b>2016</b>				
% White	64.3%	33.4%	30.6%	33.4%
% Black	12.7%	22.1%	28.7%	22.1%
% Latino	15.4%	34.0%	30.5%	34.0%
% Asian	4.5%	6.8%	6.7%	6.8%
Median age	40	35.2	35.2	35.2
% with some college	57.9%	49.9%	48.6%	49.9%
Median income	\$60,890	\$46,466	\$45,362	\$46,466
Population density	6,215	17,453	20,553	17,453
Previous turnout	35.9%	26.8%	25.5%	26.8%
Number of block groups	209,558	426	500	500
Number of killings	0	237	236	236
<b>2020</b>				
% White	62.9%	36.1%	30.5%	36.1%
% Black	12.7%	17.9%	25.5%	17.9%
% Latino	16.2%	36.4%	34.3%	36.4%
% Asian	4.8%	6.0%	6.1%	6.0%
Median age	40.6	36.2	35.9	36.2
% with some college	59.6%	52.1%	49.8%	52.1%
Median income	\$69,322	\$53,930	\$51,360	\$53,930
Population density	6,271	18,585	22,646	18,585
Previous turnout	49.6%	42.2%	38.5%	42.2%
Number of block groups	205,224	413	413	413
Number of killings	0	243	227	227

such that they mirror block groups where a killing occurred before election day. Table 1 shows that this weighting process was successful at removing systematic differences between observations on either side of the cut-point. In addition to entropy balancing weights, we use OLS covariate adjustments in the local linear equations with the same variables (and including year fixed effects).

A primary strength of the RDiT setup is that it allows us to use neighborhoods where police killings occur as controls. This means that we account for the unobservable characteristics associated with being home to a police killing (such as general exposure to the police), in addition to the observed characteristics we explicitly control for using OLS and entropy balancing. The estimated treatment effects should thus not be interpreted as the *cumulative* effect of all police encounters in these neighborhoods on turnout but rather the *marginal* effect of a killing in these neighborhoods, which may—and probably often does—come after countless lower-level community contacts with the police.

As a first look, we present in Figure 2 a binned scatter plot of the full set of data. Underlying observations are plotted in light gray (each dot represents a block group within 0.3 miles of a police killing), and the red line represents a polynomial function fit to the data on either side of the cut-point. Figure 2 shows that turnout increases when block groups are “treated” in the days leading up to the election, with a visible discontinuity. However, the figure does not indicate whether this

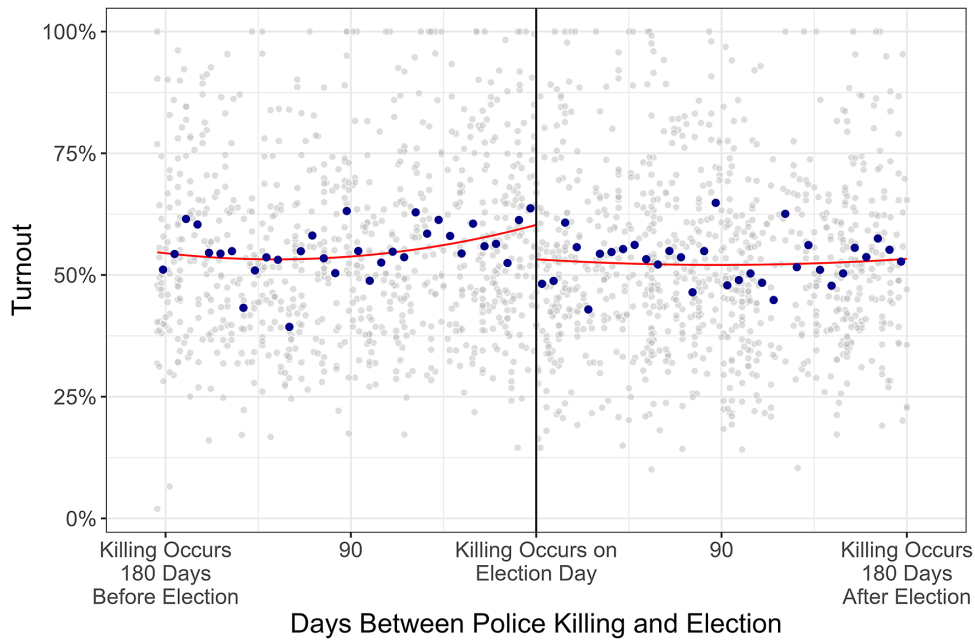
observed discontinuity is statistically significant; it does not correct for demographic differences in block groups on either side of the cut-point; and the bandwidth is arbitrarily set to 6 months, not informed by the underlying properties of the data.

Our formal RDiT models are estimated using the *rdr* package (Calonico, Cattaneo, and Titiunik 2015) in R, which allows for the estimation of nonparametric bandwidths (i.e., a bandwidth around the cut-point that is driven by the inherent properties of the data and not selected by the researcher), bias-corrected point estimates, and robust standard errors. We thus sidestep the major problems recently associated with RDD designs (Stommes, Aronow, and Sävje 2021). We use a local polynomial of 1 to avoid over-fitting the data. In each case, we use the MSE-optimal bandwidth and a triangular kernel function. The effective sample sizes, point estimates, *p*-values, and other statistics for all RDiT models in the body of the manuscript can be found in the Dataverse.

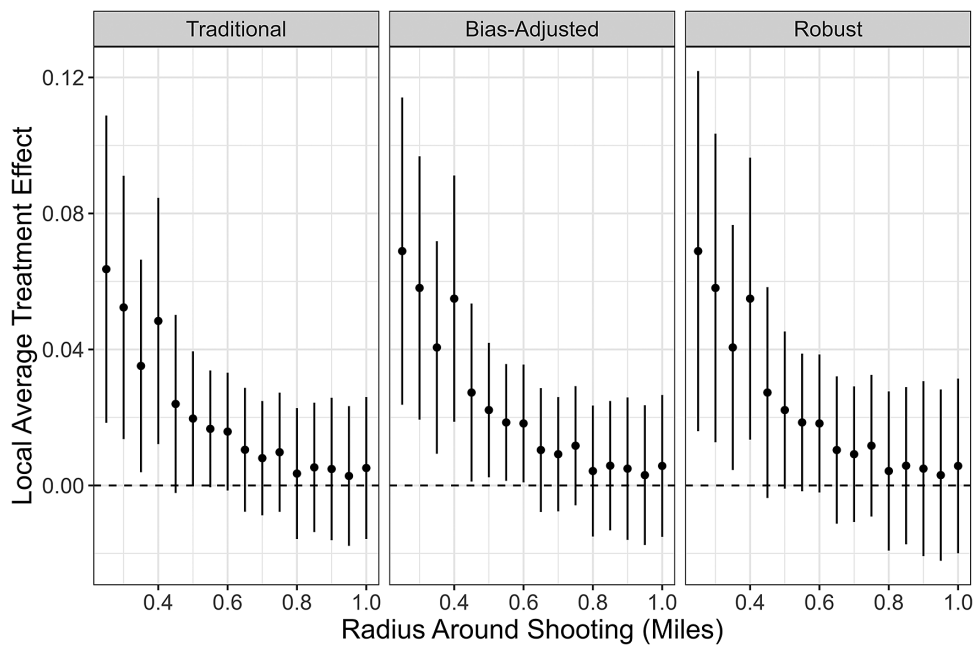
In Figure 3, we plot the point estimates and 95% confidence intervals for the 0.3-mile and other thresholds. As multiple block groups can be within the geographical threshold of a single killing—that is, multiple block groups can be “treated” by the same killing—standard errors are clustered by killing ID. At the far left of each panel, we test the discontinuity among block groups within 0.25 miles of a police killing before or after the election. We gradually expand that buffer until reaching a 1-mile buffer. Entropic weights are re-calculated each time new treated and control



**FIGURE 2. Regression Discontinuity Plot of Voter Turnout in Block Groups within 0.3 Miles of a Police Killing**



**FIGURE 3. LATE by Distance from Killing**



observations are included inside the buffer; these weights and OLS covariates are used in each model. In the left-most panel, we plot traditional point estimates and confidence intervals; the middle panel presents the bias-corrected bandwidths; and the right-most panel uses these bias-corrected bandwidths with robust standard errors.

Figure 3 makes a number of things immediately clear. First, the local average treatment effect (LATE) of a police killing on turnout is highly geographically concentrated. The block groups closest to police killings saw their turnout increase *dramatically*—by as much as 6 or 7 percentage points. To put this effect size in context, it is estimated that presidential

campaigns can increase statewide turnout on the order of 7–8 points (Enos and Fowler 2018). While these effects are concentrated at very small geographic units, they nevertheless are quite large relative to traditional GOTV efforts.

This LATE—which is statistically significant at the 95% level of confidence for block groups very near killings—decays relatively quickly, however. By the time block groups whose centroids were a mile from the killing are included, the point estimate has dropped to nearly 0 and is no longer statistically significant. Figure 3 provides strong evidence in favor of Hypothesis H1: police killings appear to substantially increase turnout, and these effects are highly geographically constrained. The regression estimates for the models in Figure 3 can be found in Table B1 in the Dataverse.

Of course, these results and our theory imply that voters *actually know* that a police killing has occurred. Additionally, by pointing to community rather than proximal contact, there is an implication that most do not personally know the victim. Given this, we would expect the results to be driven by neighborhoods where the killings received more attention and where members of the community sought out information about the killing. We use data from Google Trends to test this assumption, separating the killings into those where the victim’s name “trended” in the days of and following the killing, and those where it did not. Our approach draws from the work of Burch (2021) and a detailed description of our methodology can be found in the Supplementary Material. In Figure 4, the top plots show the LATEs for the neighborhoods near a non-trending killing; the bottom ones show those near killings that trended on Google. The regression

estimates for these models can be found in Tables B2 and B3 in the Dataverse, respectively.

Figure 4 shows that, as expected, the treatment effects are substantially larger when the victim’s name trended on Google. In fact, there is little evidence that non-trending killings have any impact at all on turnout. This is strong evidence in support of H2.

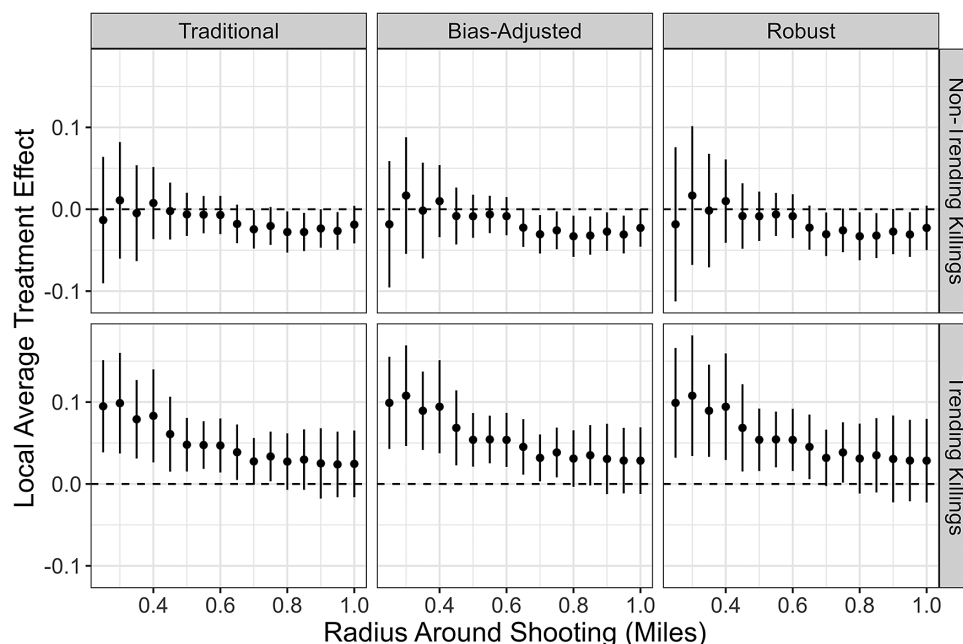
To be sure, there are certain limitations to the Google Trends data. Firstly, we cannot fully rule out that some of the mobilization observed is due to proximal—and not only community—contact. However, an average of 3,519 citizens of voting age live in a block group “treated” by a trending killing at the 0.5-mile threshold, where the point estimate in Figure 4 is 5.4 pp. This roughly translates to a trending police killing increasing turnout by 190 ballots. Such an increase in the number of voters participating seems unlikely to be explained by proximal contact alone.

Similarly, news of a police killing could travel in many ways: social media, community meetings and organizations, TV, and others. While we would expect Google searches to move in tandem with these mechanisms, testing this assumption is beyond the scope of this project. While our results support the theory of community contact, future work should further explore the precise mechanisms through which this political socialization occurs.

### Robustness Checks

For a full discussion of our robustness checks, please see the Supplementary Material. We note here, however, that our results are highly robust to other methodological choices. Our results do not depend on the

**FIGURE 4. LATE by Distance from Killing, Trending, and Non-Trending Killings**



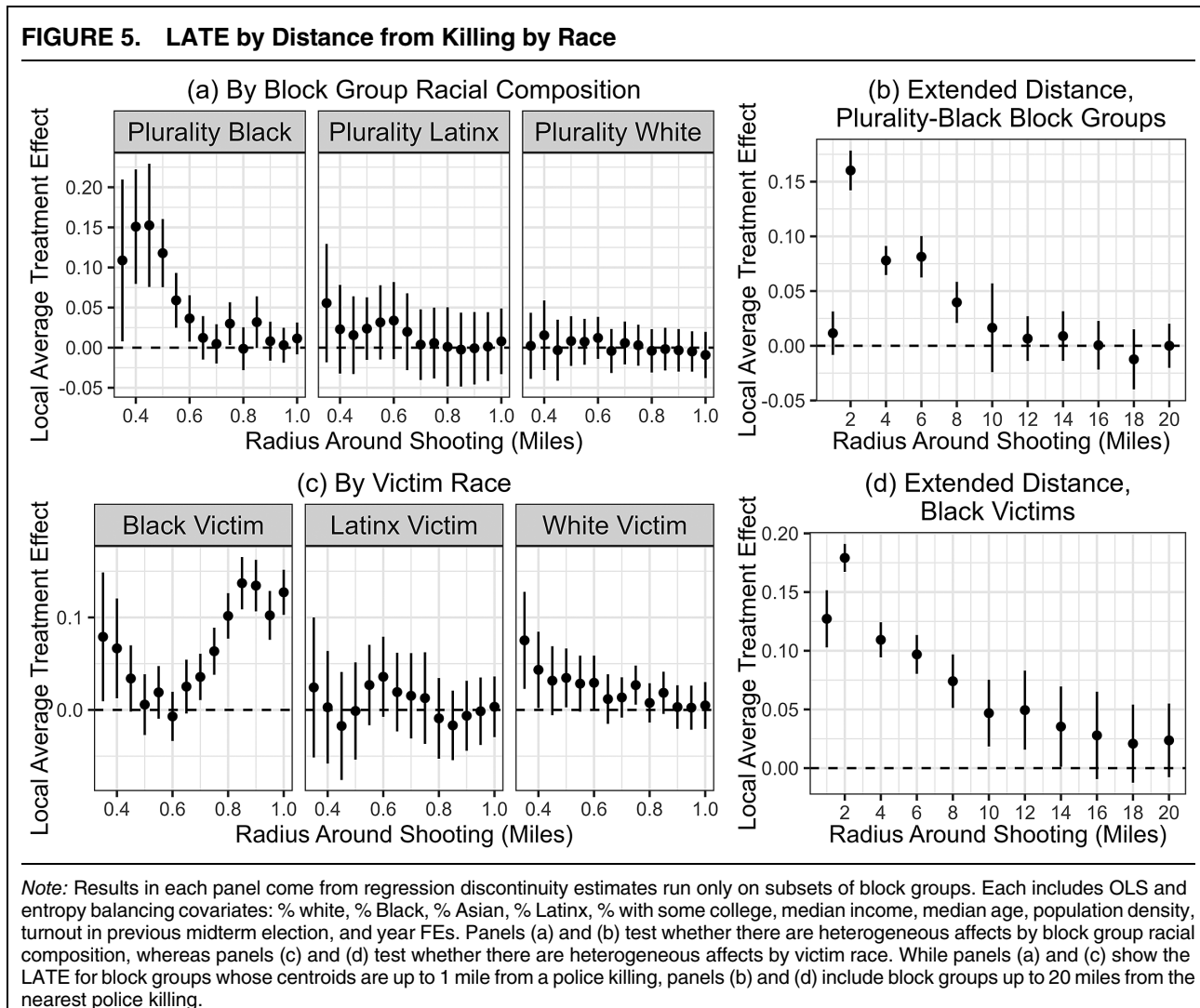
local polynomial chosen, and our findings hold under a variety of different manual bandwidth specifications around election day. A series of placebo regressions in which we move the cut-point from 2 weeks before to 2 weeks after the election indicates that the election day cut-point is meaningful; other placebo regressions in which we set the cut-point to a year before election day similarly underscore the validity of our findings. Our results are virtually identical whether we use turnout (as presented in the body of this paper) or change in turnout from the prior federal election as our dependent variable. Similarly, although we use covariate adjustment and entropy balancing to ensure comparability in the body of this paper, our results are robust to using either approach independently, as well as to using no adjustments at all.

### DIFFERENTIAL DEGREES OF MOBILIZATION BY RACE

In our first extension, we question whether turnout increases more where we expect narratives of injustice

to be more resonant. We test the possible racialized component of police killings in two ways: with respect to (a) the racial composition of the community and (b) the race of the victim. All data come from the dataset built in the previous section, with information on the racial composition of the community coming from the 5-year ACS estimates and victim race information coming from the Mapping Police Violence and Fatal Force datasets. Of the 87% of victims whose race is recorded in the datasets, 27% were Black individuals, 20% were Latinx individuals, and 49% were white individuals. Using this information, we fit separate RDITs based on racial composition of the community and by victim race. In Figure A13 in the Supplementary Material, we show that our results continue to hold when using modeled race for the victims whose race is unknown. We report the estimates using nonparametric bandwidths, bias-corrected point estimates, and robust standard errors. The regression estimates for these models can be found in Tables B6–B9 in the Dataverse.

Figure 5 asks whether the regression discontinuity estimates presented in the previous section differ based



on the racial composition of the neighborhood (Figure 5a,b) or by the race of the victim (Figure 5c, d). Figure 5a indicates that the LATE differs markedly depending on the composition of the neighborhood, with substantially larger effects in plurality-Black block groups. The patterning of the coefficients is striking in Figure 5a: though there is some evidence of immediate spatial decay in plurality-Black block groups, the pattern is not monotonic. Block groups immediately adjacent to the killing and those between 1 and 10 or so miles were mobilized by a killing (see Figure 5b). These magnitudes are generally in line with the results reported by Burch (2021), though a direct comparison between point estimates is difficult given the different empirical approaches. In no case are the estimates for plurality-white or Latinx neighborhoods statistically significant, though they trend toward 0 as they become further removed from the nearest killing.

Figure 5c,d also uncovers marked heterogeneity by the victim's race, particularly for Black victims. Mirroring Figure 5a,b, we see that the spatial decay of a mobilizing LATE attributable to a police killing is unique for Black victims. In neighborhoods, very close to the killing of a Black individual, we see very noisy estimates, perhaps reflecting competing mechanisms for individuals who may have known the victim or feel particularly targeted by the killing. Block groups a mile or two from the nearest killing of a Black individual—where narratives may dominate and proximal contact or identification with the victim is less likely—saw turnout increases in excess of 10–15 points. In Figure 5d, we again show that the LATE for Black victims remains large for a very wide geographic area, though we see a similar pattern where midrange neighborhoods exhibit very small treatment effects.

Figure 5c shows that there is no discernible pattern to the mobilizing effect of a Latinx victim at the local level, whereas the mobilizing effect of White victims follows a similar pattern to the previous section. This might be the case as these killings do not fit the more common narrative of injustice—that negative police outcomes seen by Black individuals can be tied to systemic sources—and may better fit into alternative narratives concerning immigration, gang violence, or the dangers of policing.

These findings provide evidence in support of both H3a, with respect to the racial composition of the community, and H3b, with respect to the victim's race: turnout effects are concentrated in the places where we would expect narratives about racial injustice in the CLS to resonate.

## INCREASED SUPPORT FOR ABOLISHING THE POLICE?

In the previous sections, we provide evidence that local communities are mobilized by a police killing occurring immediately before a presidential election—and that these effects are greatest among Black communities and when a Black person is killed. However, the question stands as to whether increased mobilization is

directed at reforming the system as we hypothesize. It is to this hypothesis and question we now turn.

In 2020, a Minneapolis Police Officer murdered resident George Floyd. Floyd's murder was captured on video and sparked a massive wave of protests all across the United States. In the aftermath of the protests, the Minneapolis City Council moved to abolish the Minneapolis Police Department and replace it with a different agency. This proposal came before Minneapolis voters as a ballot initiative in November 2021, but failed to pass with 44% of voters casting their ballots in favor of police abolition.

Although the ballot initiative did not pass, it can still offer insight into how citizens exposed to police violence respond. We use precinct-level results from the ballot initiative to test whether precincts closer to a police killing supported abolition at higher rates (H4). Because the initiative was only on the ballot in 2021, and because of the relative rarity of killings in a single city, we move out of a causal identification framework and instead leverage a cross-sectional model with one observation for each voting precinct in Minneapolis.

Information on the share of voters in a precinct who supported abolishing the police department (the dependent variable) comes from the Minnesota Secretary of State.<sup>8</sup> Precinct-level support for abolishing the police ranges from 17% to 78%, with a mean vote share of 45%. As before, the key independent variable is the distance from a police killing. Here, this is calculated as the distance between the precinct's centroid (coming from shapefiles posted by the State of Minnesota<sup>9</sup>) and the nearest police killing between January 1, 2013 (the earliest date available in the Mapping Police Violence data) and the 2021 election. The police killed 11 individuals in Minneapolis during this time, 10 of which could be geocoded reliably.<sup>10</sup> We widen the time span here, as police killings are relatively rare in a single city, and focus only on deaths occurring *before* the 2021 election as we are no longer in the RDiT framework. Figure 6 shows the bivariate relationship between distance to a police killing and support for abolition in 2021. Proximity to a police killing appears to be associated with greater support for the abolition ballot measure. While this provides some support for our fourth hypothesis, this does not control for other potential explanations.

To that end, we fit three regressions, which are presented in Table 2, using this constructed dataset and including controls.<sup>11</sup> Police killings may be correlated with other exposure to the CLS, which could be associated with support for police abolition. We therefore include the logged number of police stops in each precinct between January 1 and November 1, 2021, as well as the logged number of crimes. To control for

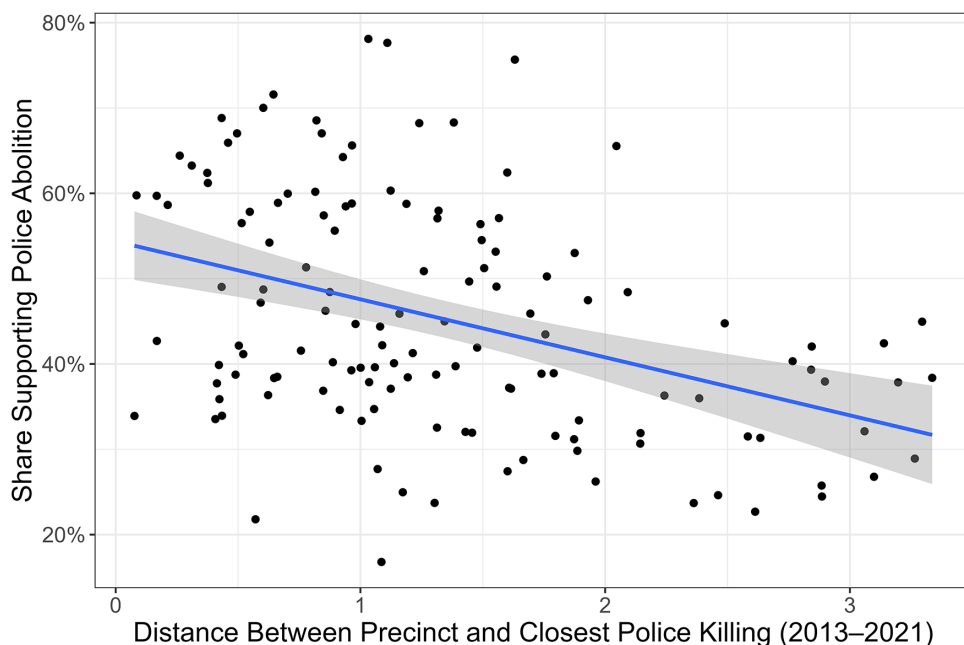
<sup>8</sup> <https://electionresults.sos.state.mn.us/Results/Index?ersElectionId=142&scenario=ResultsByPrecinctCrosstab&QuestionId=1323>.

<sup>9</sup> <https://gisdata.mn.gov/dataset/bdry-votingdistricts>.

<sup>10</sup> The inclusion of the 11th, which was coded with 93.4% accuracy, results in a higher point estimate that remains statistically significant at the 95% level.

<sup>11</sup> For the full table, see Table A2 in the Supplementary Material.

**FIGURE 6. Support for Police Abolition and Distance to Police Killing in Minneapolis, MN**



**TABLE 2. Support for Abolishing Minneapolis Police Department**

	Share of precinct		
	Supporting abolition		
	Model 1	Model 2	Model 3
Intercept	0.544*	0.492*	0.255
	(0.032)	(0.131)	(0.229)
Distance to closest police killing since 2015	-0.068*	-0.059*	-0.026*
	(0.022)	(0.016)	(0.010)
Logged number of police stops in 2021		0.032	-0.028
		(0.048)	(0.027)
Logged number of crimes in 2021		-0.017	0.035
		(0.046)	(0.028)
Demographic controls	N	N	Y
No. of obs.	134	134	134
$R^2$	0.161	0.172	0.765
Adj. $R^2$	0.154	0.160	0.744

*Note:* Standard errors clustered by nearest killing. Demographic controls include the percent of the population that is non-Hispanic Black, non-Hispanic white, Latinx, Asian, and with some college. Additionally, they include median income, median age, logged population density, and Biden’s 2020 vote share. \* $p < 0.05$ .

underlying differences in the political preferences of different precincts, we also control for the vote share won by Joe Biden in the 2020 presidential election 1 year earlier. Robust standard errors are again clustered by the ID of the closest police killing.

Table 2 shows that the relationship between distance to the closest police killing and support for abolition is large and statistically significant at the 95% confidence level. Net of other covariates, each additional mile between a precinct and a police killing is associated

with a 2.6 percentage point decrease in support for police abolition; put differently, precincts exposed to this sort of violence voted for the disbanding of the police at substantially higher rates. Somewhat surprisingly, the numbers of police stops and crimes in 2021 are entirely unrelated with a precinct’s vote on the ballot initiative ( $p = 0.52$  in Model 2 and 0.34 in Model 3 for stops,  $p = 0.71$  in Model 2 and 0.24 in Model 3 for crimes). While these cross-sectional regression estimates are not causal in nature, they are consistent with

the hypothesis that police killings lead citizens to use the ballot to hold the state accountable, and support H4.

It is worth noting that our results point to relatively long-term effects; when we construct *Distance to Closest Police Killing* such that it measures the distance between each precinct and the nearest police killing in just the 2 years before the ballot initiative, our results do not hold. This could be because we systematically underestimate the distance to a police killing of neighborhoods known to be exposed to a killing earlier in the period. We cautiously interpret this to point to a longer-term effect of proximity to a police killing. This may mean that the salience of a community police killing can be re-activated at the ballot box in the presence of the social movement frames developed by Minneapolis activists in 2020 (Phelps, Ward, and Frazier 2021).

## DISCUSSION

Access to the ballot box has long been considered necessary for citizens seeking to protect themselves from an aggressive state. From the fight over the 15th Amendment following the Civil War, to the Civil Rights Era's focus on the Voting Rights Act of 1965, to contemporary organizing around the Freedom to Vote: John R. Lewis Act, Black Americans have articulated the importance of voting to the securing of civil rights. Here, we provide novel evidence that these beliefs and narratives extend beyond national rhetoric and into the communities most impacted by extreme police violence. Ordinary Black Americans, it seems, also turn to the ballot box following the police killing of a member of their local community.

Our findings have implications for a number of literatures and raise questions for future research. First, this study contributes to the general literature on how policy and interactions with bureaucrats informs political participation—and specifically to the literature on how contact with the CLS shapes participation. We do so by explicitly differentiating community contact from personal and proximal contact, contributing to our broader understanding of how contact relates to participation. Our analysis of Google Trends makes clear that our theory of community contact—where voters are perhaps mobilized at the local level by narratives made especially salient thanks to the work of social movement activists—best explains the mobilization that occurs following a police killing.

We also contribute to our understanding of local mobilization by extending and tying together Nuamah and Ogorzalek's (2021) place-based mobilization theory and Garcia-Rios and colleagues' (2021) work on racialized policy feedback. This study again underscores the role that narrative may play *and* indicates that events attributable to a government cause can mobilize in a manner similar to policy changes and proposals. Further, we do so in a broader manner than previous studies in this area by looking across the

United States rather than at one or a small handful of cities or events at a time, and by employing administrative records from the more than two hundred million registered voters in the country rather than turning to survey data. While we show support for the idea that political organizing occurs and lasts—at least for a few months—we cannot directly test whether more organizations are formed, they gain members, or become more active following a police killing. Additionally, we cannot test how long these results last or why they do—or do not. Future research should explore each of these questions.

Our work spurs additional questions outside the scope of the current study that demand attention, such as whether police killings are unique compared with other forms of community contact with the police and how experiences with different forms of police contact and activities relate to mobilization. Our results from Minneapolis provide preliminary evidence that this is the case but do not examine racial disparities in stops or other forms of contact. Additional and more targeted research should be done on this question.

Results presented in our Supplementary Material also prompt important questions about the interplay of *national* social movement and *local* government action. Specifically, we find that our effect sizes are considerably larger in 2016 than in 2020, though results from both years show the same pattern of spatial decay. We posit that this may be attributable to the fact that, in a sense, all Americans were “treated” by exposure to police violence in 2020 thanks to the widespread protests that summer. In our theoretical setup, we argue that exposure to social movement frames and a catalyst prompting a reevaluation of the state lead to mobilizing effects. It seems plausible that our “control” units in 2020 (neighborhoods home to a police killing shortly after the election) might be the sorts of over-policed neighborhoods where news of George Floyd's murder might have struck a nerve. Does that reduce the marginal shock—and, therefore, mobilization—of a localized police killing? Our results suggest this might be the case, but future social movement work should interrogate how local and national events jointly structure political action.

Together, these results broaden our understanding of how threatening governmental actions translate into political behavior. Theories of legal estrangement indicate that when the government becomes illegitimate in the eyes of citizens, they withdraw from *all* engagement with the state. Studies examining the participatory consequences of personal and proximal contact with the CLS support this theory, but our results add nuance to this conversation. It seems possible that different messages are communicated and internalized dependent on contact type. As such, community contact with police violence seems to activate a group-based response more in line with responses to policy threat, rather than individual-level withdrawal as a means of self-preservation.

## SUPPLEMENTARY MATERIAL

To view supplementary material for this article, please visit <https://doi.org/10.1017/S0003055423000321>.

## DATA AVAILABILITY STATEMENT

Research documentation and data that support the findings of this study are openly available in the APSR Dataverse at <https://doi.org/10.7910/DVN/DMG0XD>. Limitations on data availability are discussed in the text and/or Supplementary Material.

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## AUTHOR CONTRIBUTIONS

Authors are listed alphabetically and contributed equally.

## CONFLICT OF INTEREST

The authors declare no ethical issues or competing interest in this research.

## ETHICAL STANDARDS

The authors affirm this research did not involve human subjects.

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