

Civic Responses to Police Violence

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Roughly a thousand people are killed by American law enforcement officers each year, accounting for more than 5% of all homicides. We estimate the causal impact of these events on civic engagement. Exploiting hyperlocal variation in how close residents live to a killing, we find that exposure to police violence leads to significant increases in registrations and votes. These effects are driven entirely by Black and Hispanic citizens and are largest for killings of unarmed individuals. We find corresponding increases in support for criminal justice reforms, suggesting that police violence may cause voters to politically mobilize against perceived injustice.


INTRODUCTION


In recent years, acts of police violence have garnered significant public attention. The high profile killings of George Floyd and Breonna Taylor compelled an estimated 15–26 million Americans to protest against police brutality and systemic racism (Buchanan, Bui, and Patel 2020). This movement was part of a larger national reckoning, which saw violent counter-protests in Portland, player walkouts across professional sports leagues, and widespread calls for police reform. These events are not without historical precedent. Dating back to the 1965 Watts riots, the four largest episodes of urban unrest in America were all triggered by police use of force (DiPasquale and Glaeser 1998).

While recent events have raised questions about the role of state-linked violence in democratic societies, researchers know little about the effects of police violence on local political participation. A large literature has shown that interactions with the criminal justice system can have drastic demobilizing consequences (Lerman and Weaver 2014a; Weaver and Lerman 2010; White 2019a; 2019b). These studies tend to focus on individuals or families with direct contact with law enforcement or carceral systems. But as recent history demonstrates, police killings are often public and visible events that may incite concerns about institutional trust, racial discrimination, and procedural justice even among individuals with little or no direct relation to the deceased. Understanding the political ramifications of state-linked violence is thus central to questions of democratic governance and may bear important implications for our understanding of recent and future elections.

This article provides the first causal evidence of the impact of police violence on voter participation. To do so, we combine highly detailed voter registration data from Los Angeles County with novel incident-level data on the timing, location, and context of nearly three hundred police killings spanning almost a decade. As the occurrence of police killings is not random, a simple comparison of civic engagement in neighborhoods with high and low rates of police violence is likely to be confounded by a number of correlated factors. We instead employ a dynamic difference-in-differences (i.e., event study) design to leverage hyperlocal variation in exposure to police violence. This approach compares changes in civic behavior before and after a police killing in the exact Census block where the killing occurred to pre-post changes in other blocks in the same Census block group (i.e., their “neighborhood”).¹ Causal identification hinges on the assumption that counterfactual trends in civic behavior among Census blocks with police killings (i.e., treatment blocks) can be approximated by actual trends in civic behavior among Census blocks in the same neighborhood without police killings (i.e., control blocks). We provide empirical support for this assumption by showing that registration and voting trends closely mirror each other across treatment and control areas in the elections leading up to a police killing.

We find that police killings may mobilize local residents to engage with the electoral process. Registrations and votes in incident blocks increase by roughly 5% in the elections following a police killing. While gains in voter turnout are relatively short-lived, registration effects continue more than a decade later. However, these effects are highly localized, pointing to the potential role of information in mediating responses. For the 82% of sample killings that went unmentioned

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¹ In our main sample, Census block groups span, on average, 0.61 square miles and consist of about 11 Census blocks. Throughout the article, all references to “blocks” refer to Census blocks as defined in the 2010 Census. Census blocks have the advantage of taking into account natural geographical divisions, such as streets, highways, and waterways.

in local newspapers, we find no evidence of spillovers extending beyond the block of the incident. In contrast, for killings that did receive media coverage, we observe registration increases up to half a mile away.

To interrogate the role of family and household members, we first compare across police killings that occurred near and far from the home of the deceased. We find similar increases in registration and voting regardless of incident location. Searching individual-level voter registration files, we also find little evidence of increased participation among individuals sharing the same last name as the deceased. Together, these findings suggest that changes in registration and voting are unlikely to be driven by family or household members of the deceased, but rather by other local residents who may have seen or heard about the killing.

The aggregate effects mask significant heterogeneity across demographics. Increased civic engagement is driven entirely by Black and Hispanic citizens, who are 9% and 5% more likely to register as a result of exposure to local police killings, respectively. We find no statistical or practical impact on the political behavior of nearby whites and Asians. We also document stark differences across other dimensions. The largest effects come from younger voters, new registrants, and Democrats. We find no significant impact among Republicans or individuals over age 35. These findings accord with a host of survey evidence documenting deep racial and partisan divisions in views of law enforcement, with minorities and liberals far more concerned about police use of force than whites and conservatives.

To unpack mechanisms, we first explore differences in civic responses based on whether the person killed by police possessed a weapon. If changes in turnout reflect concerns about crime and support for more intensive policing, we would expect larger effects for police killings of armed individuals. In fact, our data support the opposite narrative. Point estimates of civic spillovers are three to four times larger following killings of unarmed individuals—those events in which police actions may have seemed the least justifiable. We corroborate these findings with data on local referenda voting and find that police killings significantly increase support for propositions designed to reduce criminal penalties for nonserious offenses. Together, our findings suggest that acts of police violence may drive some residents to the polls in an attempt to reform the criminal justice system.

THEORY

The core question of this article is how police killings affect local registration and voting. A large literature documents the potential demobilizing effects of the criminal justice system. Examining survey data from the Fragile Families and Child Wellbeing Study, Lerman and Weaver (Lerman and Weaver 2014a; Weaver and Lerman 2010) find that previously arrested, convicted, or incarcerated individuals are significantly less likely to report having registered or voted in recent

elections. These patterns are causally validated by White (2019b), who exploits random courtroom assignment and find that short jail spells reduce future turnout for first-time misdemeanor defendants. Related work documents demobilization spillovers among other individuals living in the same household (Lee, Porter, and Comfort 2014; White 2019a). These effects are likely driven in part by the downstream economic costs of a criminal record (Agan and Starr 2018; Western and Pettit 2010) as well as the stigma, emotional trauma, and institutional distrust that may accompany it. Consistent with the latter, recent work has shown that police killings may cause nearby adolescents to disengage from formal institutions by dropping out of school (Ang 2021).

At the same time, research has demonstrated that some groups may be mobilized by perceived injustice and political threats. While much of the empirical work centers on immigration policy (Gutierrez et al. 2019; Pantoja, Ramirez, and Segura 2001; White 2016; Zepeda-Millán 2017), there is significant reason to believe that police violence may fuel related concerns about fairness and justice (Weitzer and Tuch 2006).² The recent surge in Black Lives Matter demonstrations following the murder of George Floyd as well as the historic protests that erupted after the police beatings of Rodney King and Marquette Frye provide anecdotal support for such a pathway. Consistent with this, Walker (2014; 2020) finds a positive correlation between “proximal contact” with the criminal justice system—knowing a close friend or family member who has been arrested, charged, or questioned by police—and self-reported measures of informal political participation—such as signing a petition, attending a community meeting, or attending a demonstration or protest.³ As evidence of the role of perceptions of injustice, she shows that this relationship is partly mediated by the impact of proximal contact on an individual’s belief about unfair targeting by law enforcement.

These narratives point to opposite conclusions about the effects of police killings on local electoral participation. Nonetheless, there exists little causal evidence on this topic. One of the central challenges is that criminal justice contact—whether direct or indirect—may be correlated with a number of observed and unobserved factors that may affect an individual’s likelihood of voting. This is compounded by the scarcity of reliable data capturing direct or proximal contact much less linking those measures to voting behavior. While researchers have attempted to address these concerns by employing multivariate analysis of self-reported, cross-sectional survey data, such methods may still suffer from measurement error and do not address

² In a different context, recent studies have examined the mobilization effects of school shootings (Garcia-Montoya, Arjona, and Lacombe 2022; Hassell, Holbein, and Baldwin 2020) and find mixed results.

³ Walker (2020) also finds a positive, but insignificant, relationship between proximal contact and self-reported turnout in National Crime and Politics Survey data but a near zero relationship in American National Election Studies data.

the fundamental concern regarding selection bias and the endogeneity of police encounters.

Empirical research leveraging quasi-random variation and time series or panel analysis has sought to answer related questions about how officer use of force affects citizen cooperation with law enforcement (Ang et al. 2021; Cohen et al. 2019; Desmond, Papachristos, and Kirk 2016; Lerman and Weaver 2014b; Zoorob 2020). However, this work finds mixed effects on 311 and 911 calls for service and leaves unanswered the central question of this article: whether citizens strategically respond to police violence by engaging with electoral systems. A separate body of work interrogates the effects of political protests, including the Rodney King riots and other events that arose in the wake of high profile use of force incidents (Enos, Kaufman, and Sands 2019; Wasow 2020). However, as protests are themselves a consequence of police violence, such case studies may be unable to disentangle the impact of one from the other. By focusing on high profile events, these studies may also provide limited insight into the political ramifications of the vast majority of use of force incidents that receive little or no media attention.

Beyond the core question of how acts of police violence affect electoral participation is *who* is affected by these events and why. While researchers disagree about the role of racial bias in officer use of force (Durlauf and Heckman 2020; Fryer 2019; Hoekstra and Sloan 2022; Knox, Lowe, and Mummolo 2020), relative to their population shares, Black and Hispanic individuals are significantly more likely to die at the hands of police than their white and Asian counterparts (Edwards, Lee, and Esposito 2019). Perhaps not surprisingly, Black and Hispanic individuals are far more likely to believe that use of force is exercised in a racially biased manner and that police violence is a pressing social concern (AP-NORC 2015; Leiber, Nalla, and Farnworth 1998; Weitzer and Tuch 2002).⁴ In this light, one might expect that Black and Hispanic political participation would be particularly sensitive to police violence and that these responses may be driven by concerns about police accountability and racial discrimination.

DATA

Police Killings

Incident-level data on police killings come from the Los Angeles Times Homicide Database, which tracks all known officer-involved killings in Los Angeles county and includes 292 incidents between the 2002 and 2010 general elections.⁵ For each killing, the data include the

name, age, and race of the deceased, as well as the exact address and date of the event. We supplement this with information on whether the incident was reported by local newspapers.⁶ For roughly 85% of killings, we were also able to determine whether a weapon was recovered from the deceased. This information was hand-coded from Los Angeles County District Attorney reports as well as police reports and other sources.

Note that these contextual measures may provide an incomplete picture of the surrounding events. Often officers acted under faulty information. For example, in one incident, police killed a man who was reported to have a gun but who was actually holding a water hose nozzle. In other cases, killings were precipitated by seemingly innocuous encounters that quickly escalated—such as when a man lunged for an officer's gun after he was stopped for riding a bicycle on the sidewalk. Nonetheless, weapon information has the benefit of being objectively verifiable and can be found in all available incident reports.

Panel A of Table 1 provides a summary of the police killings data. Fifty-two percent of deceased individuals were Hispanic, 29% were Black, 16% were white, and 3% were Asian.⁷ Relative to their population shares, Black (Hispanic) individuals are roughly six (two) times more likely to be killed by police than whites. The vast majority of individuals (96%) were male and the average age was 30 years.

Consistent with national statistics, 47% of those killed were armed with a firearm (including BB guns and replicas), whereas 22% possessed some other type of weapon. This includes items like knives and pipes as well as individuals who attempted to hit someone with a vehicle. Fifteen percent of individuals were completely unarmed. We were unable to find contextual information for the remaining 16% of incidents. Notably, the vast majority of killings received little or no media coverage. Only 18% of sample killings were ever mentioned in any of the six local newspapers. Conditional on coverage, the median number of articles is 2. The most mentions of any incident was 28, far below the level of media attention garnered by recent high profile police killings.

Examining contextual factors separately by race, Black and Hispanic individuals killed by police were younger on average than white and Asian individuals (29 vs. 36 years old, respectively) and more likely to possess a firearm (53% vs. 24%). However, rates of media coverage are similar between groups (19% vs. 16%). Regardless of circumstance, involved officers were never prosecuted. The District Attorney did not pursue criminal charges against police following any of the 292 sample killings. This is consistent with national statistics, which find that criminal charges are filed

⁴ For example, a 2015 survey found that 75% of Black respondents and over 50% of Hispanic respondents believe that police violence is a very or extremely serious issue, relative to 20% of whites. Similar disparities exist when asked whether police “deal more roughly with members of minority groups” (AP-NORC 2015).

⁵ In a handful of cases, multiple individuals were killed in a single incident. The total number of distinct incidents is 286.

⁶ We searched for each incident by the name of the deceased in the print versions of six local newspapers: the Los Angeles Times, the Los Angeles Daily News, Pasadena Star News, San Gabriel Valley Tribune, Torrance Daily Breeze, and Whittier Daily News. The combined daily circulation of the papers is roughly one million copies.

⁷ Race categories are mutually exclusive.

miles and fewer than 100 adults. In addition to total registration and vote counts, disaggregated counts by ethnicity (i.e., Hispanic and Asian), party affiliation (i.e., Democrat, Republican, and Independent/other), age, and duration of registration are also available. We combine these data with block-level demographic information on the voting age population from the 2000 and 2010 Censuses.

In order to examine the impact of police killings on voter preferences, we leverage voting data from two referenda that proposed changes to the severity of criminal sentencing laws. The first—Proposition 66 in 2004—would have limited California’s “three strikes” law to apply only to violent and serious felonies. The second—Proposition 5 in 2008—would have enacted numerous measures to reduce criminal penalties for drug offenses, including the reduction of marijuana misdemeanors to infractions, and the expansion of drug treatment and rehabilitation programs. Block-level estimates of the share of ballots cast for and against each proposition come from the California Statewide Database.¹⁰

Analysis Sample

Since vote counts are only available until 2010, our main analysis focuses on the 2002–2010 general elections. To improve precision, we restrict the sample to blocks with five or more residents of voting age in the 2000 and 2010 Censuses. In robustness analyses, we find similar results under alternative sample restrictions and when extending the registration analysis to include more recent elections.¹¹

Panel B of Table 1 provides a summary of the voter registration data. Relative to the full sample (column 1), residents in Census blocks where a police killing occurred (column 2) are more likely to be Black, Hispanic, Democratic, and young (18–34 years old), and have lower registration and voter turnout rates. Notably, however, Census blocks with police killings are demographically and politically similar to other Census blocks in the same Census block groups (column 3). Registered voters in the former are roughly equally likely to be Black (27% vs. 26%), Democratic (62% vs. 61%), or young (32% vs. 30%) as registered voters in the latter. The two sets of areas also experienced similar voter turnout (36% vs. 39%) and registration rates (54% vs. 58%) in 2002. Indeed, the main difference between these areas is population size (190 vs. 100 adult residents). In the next section, we explain how our research design leverages the similarity of Census blocks in the same block group to estimate causal effects.

¹⁰ These estimates are generated using ecological inference, which combines precinct-level election results with individual-level address and turnout data from the county registrar. The resulting individual-level voting propensities are then aggregated to the Census block level. Additional information is available at <https://statewidedatabase.org/>.

¹¹ Research documentation and data that support the findings of this study are openly available in the American Political Science Review (APSR) Dataverse (Ang and Tebes 2023).

EMPIRICAL APPROACH

Exposure to Police Killings

This article is interested in understanding the causal impact of police violence on civic engagement. Police shootings, however, are not randomly distributed across neighborhoods. Instead, they are concentrated in low-income neighborhoods with higher shares of minority residents. Thus, regressing voter turnout rates on local measures of police violence would likely be biased by meaningful geographic differences in political preferences and participation. Even controlling for area fixed effects would not resolve such biases, if changes in local crime or law enforcement correlate with the timing and location of police killings.

To address these concerns, we adopt a difference-in-differences approach similar to Ang et al. (2021) that exploits hyperlocal, *within-neighborhood* differences in exposure to police killings. At its core, our empirical strategy compares changes in civic behavior before and after police killings in the Census blocks where killings occurred (treatment blocks) to changes in civic behavior among other blocks in the same Census block group (control blocks). This strategy exploits the fact that the vast majority of sample police killings received little or no media coverage, such that any effects are likely to be highly geographically concentrated.¹²

Our approach accounts for any time-invariant differences between treatment and control blocks as well as any time-varying factors that affect the entire Census block group similarly. However, time-varying factors that disproportionately affect treatment blocks relative to nearby control blocks would nonetheless bias our estimates. Thus, causal identification rests on the standard “parallel trends” assumption—absent police killings, treatment and control blocks in the same Census block group would have experienced similar trends in civic behavior. While this assumption is impossible to prove in the counterfactual, we provide supporting evidence by showing parallel trends in the elections leading up to police killings (i.e., see the discussion of Figure 2 under the Main Results section).

The validity of our strategy is further aided by two factors. First, as discussed earlier and demonstrated in Panel B of Table 1, Census blocks with police killings are strikingly similar to other blocks in the same Census block group in terms of racial, political, and demographic composition. Treatment and control blocks also experienced similar rates of civic engagement historically. Combined with their geographic proximity, these cross-sectional similarities provide greater confidence that treatment and control areas would have experienced similar trends in voting and registration in the counterfactual.

Second, police killings are quite rare and difficult to predict. Over three hundred thousand arrests and nearly 60,000 violent crimes occur in Los Angeles each year,

¹² Table 1 reports that 82% of shootings were never mentioned in the six most prominent local newspapers.

compared with fewer than 50 officer-involved killings. Furthermore, many police killings are entirely unaccompanied by violent crime, as only a quarter of events involved armed suspects who fired at others. Thus, while crime rates and policing intensity may differ across neighborhoods, the exact timing and location of officer-involved shootings within those neighborhoods is plausibly exogenous. Indeed, as we demonstrate later, we find little evidence of differential changes in local crime or policing either before or after police killings (i.e., see the subsection Potential Threats to Identification).

Graphical Evidence

To corroborate our empirical strategy and demonstrate the hyperlocal impact of police killings, we first examine how geographic proximity to a killing mediates changes in voter registration. To do so, we construct the following distance metric measuring each Census block's distance to a police killing: for each police killing, we estimate the minimum radius needed for a circle centered on the incident location to cover at least 75% of a block's area.¹³ Using this metric, we run the following generalized difference-in-differences regression:

$$y_{b,t} = \delta_b + \delta_{n,t} + \sum_d \alpha_d \text{Distance}_d + \delta_{POP_b \times ELEC_t} + \epsilon_{b,t}. \quad (1)$$

Here, $y_{b,t}$ is the number of registered voters in block b at election t . δ_b are Census block fixed effects. $\delta_{n,t}$ are neighborhood-by-election fixed effects, where neighborhoods are defined as Census block groups, which average about 11 blocks and 0.61 square miles. The inclusion of these fixed effects allows for election-specific behavior to differ at the neighborhood level and effectively limits comparisons to treatment and control blocks in the same Census block group. Because block-level population counts are only available from the decennial Census, we include interactions between election fixed effects and deciles of estimated voting age population in 2002 ($\delta_{POP_b \times ELEC_t}$) to account for the possibility of differential population growth between blocks. $\sum_d \text{Distance}_d$ are a set of mutually exclusive treatment indicators that track a block's distance to the nearest police killing that occurred prior to election t .¹⁴ We partition shootings first by whether or not the block was directly exposed to a killing (i.e., occurred in the block), and then into 0.1-mile bins up to 2 miles from the shooting. Drawing on Bertrand,

¹³ We estimate a minimum radius in 0.05-mile increments up to 2 miles. Reassuringly, over 95% of Census blocks containing a police killing are captured within a 0.35-mile radius based on this measure. We found other metrics, such as the geodetic distance between block centroids, to be noisier. In the Supplementary Material, we show that results are very similar when using this simpler geodetic distance measure.

¹⁴ For example, if a block's nearest killing before 2007 was 1.5 miles away and then experienced another killing 0.5 miles away in 2007, the distance is 1.5 miles for elections before 2008 and 0.5 miles for 2008 forward.

Duflo, and Mullainathan (2004), standard errors are clustered within each of the sample's 6,400 Census block groups.

Figure 1 plots α_d coefficients under our main specification (red diamonds). We also estimate an alternative specification replacing the neighborhood-by-election fixed effects with election fixed effects (blue dots). The coefficients represent the average difference between the pre-post change in registrations experienced by blocks with distance d and blocks in the omitted group (i.e., blocks in the "zero-impact" region between 0.7 and 0.8 miles from a killing).¹⁵ The graphical evidence suggests that impacts are indeed hyperlocal and that residents living in nearby blocks may serve as a valid control for residents in Census blocks with a police killing. Across specifications, Census blocks with a police killing experience a significant increase in registrations of roughly five counts (6% of the pre-killing mean). Consistent with the underpublicized nature of police killings, effects fall off dramatically with spatial distance, with near-zero estimates for blocks only 0.1 miles from where a killing occurred. Figure 1 also provides further corroboration of the exogeneity of police killings. For some unobserved confound to generate these effects, it would have to coincide with the exact dates and locations of nearly three hundred police killings spread across several years and thousands of square miles.

Primary Specification

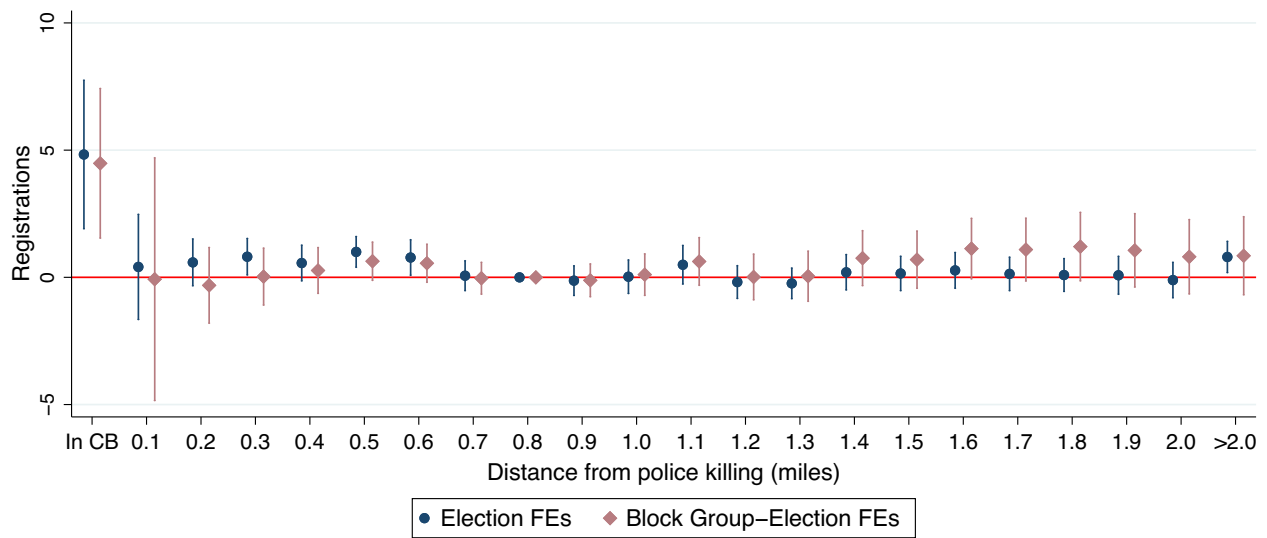
To estimate effects on civic engagement, we next employ a dynamic difference-in-differences (i.e., event study) model. Drawing on the distance analysis, treatment is defined as Census blocks that experienced a police killing and neighborhood is defined at the Census block group level. We estimate the following base equation on the block-level panel data:

$$y_{b,t} = \delta_b + \delta_{n,t} + \sum_{\tau \neq -1} \beta_\tau \text{Shoot}_{t,\tau} + \delta_{POP_b \times ELEC_t} + \epsilon_{b,t}. \quad (2)$$

This is essentially analogous to Equation 1 except that we replace the set of treatment distance indicators ($\sum \alpha_d \text{Distance}_d$) with a set of time to treatment indicators ($\sum \beta_\tau \text{Shoot}_{t,\tau}$), fixing treatment to the first killing that occurred in a Census block between the 2002 and 2010 general elections.¹⁶ The coefficients of interest (β_τ) represent the differential change between relative time τ and the last period prior to the police killing in the incident block relative to that same change over time among other blocks in the same Census block group.

¹⁵ In the block group specification, distance indicators are set to 0 for all blocks in Census block groups with a police killing so that nearby blocks are not mechanically biased downward through the neighborhood-by-election fixed effects.

¹⁶ Only one block experienced multiple separate incidents (i.e., killings that occurred on different days) over the sample period. Results are robust to excluding that block.

FIGURE 1. Effects by Distance from Police Killing

Note: This figure shows coefficients and 95% confidence intervals from the estimation of Equation 1 on registrations. Unit of observation is the Census block election. The sample spans the 2002–2010 general elections. Indicators are mutually exclusive and track a Census block's minimum distance to police killings that occurred prior to a given election. "In CB" refers to blocks where killings occurred. Other blocks are partitioned into 0.1-mile bins. Blue dots include election fixed effects. Red diamonds include Census block group by election fixed effects. Standard errors are clustered at the Census block group level. Full regression results are included in columns 1 and 2 of Table A.1 in the Supplementary Material.

Potential Threats to Identification

As discussed, the main threats to causal identification in our setting are shocks that both correlate with the timing of police killings *and* disproportionately affect civic engagement in incident blocks relative to non-incident blocks in the same Census block group.

Perhaps the most concerning of these potential confounds are changes in local crime, which could influence both the level of civic engagement (Bateson 2012; Sønderskov et al. 2022) and the presence of police—and in turn, police killings—in an area. However, given that we control for Census block group-by-election fixed effects, any biases would have to be hyperlocal, affecting individuals on one block but not the next within the same neighborhood. To examine this, Panel A of Figure A.1 in the Supplementary Material estimates Equation 2 on criminal homicides in a block-year. Since geocoded data for *all* crimes and arrests are only available after 2010, we replicate the exercise using information on the timing and location of police killings from 2010 to 2016 in Panels B and C. In all cases, we find little support for differential trends in local crime or policing activity before or after police killings, reinforcing the plausible exogeneity of these events.

Another potential concern is selective migration in response to police violence. Because block-level population is only measured each decade, we are unable to directly test for differential migration using our main event study model. However, several pieces of corroborating evidence suggest that migration is unlikely to be a serious threat. First, given the positive effects on

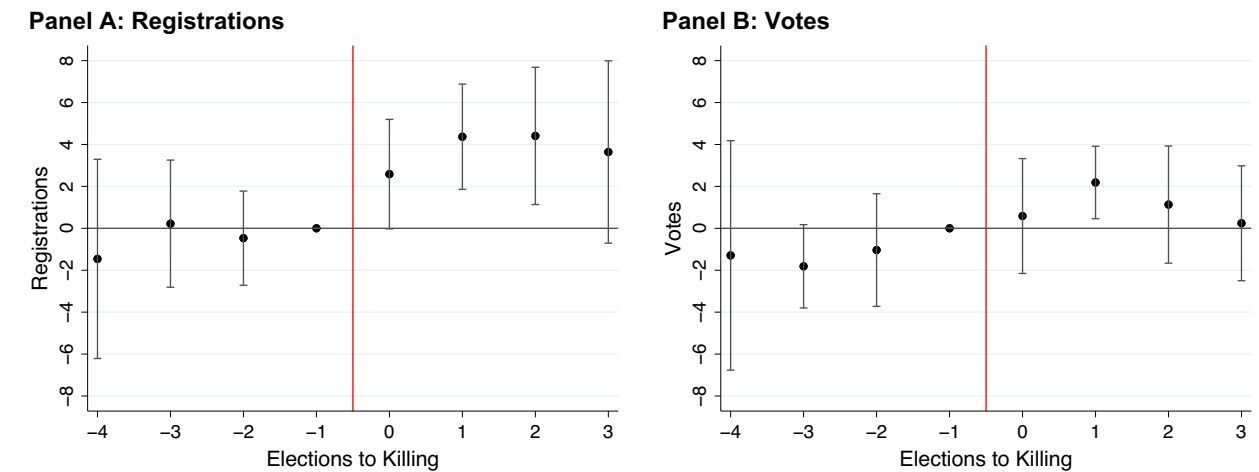
registration counts, the main concern would be if police killings *increased* population in a neighborhood. However, examining student transfer data in Los Angeles, Ang et al. (2021) find that, if anything, police violence leads to small, but insignificant, *decreases* in the local student population. Second, 2006–2010 ACS data show that the share of individuals residing in the same house 1 year prior to the survey is virtually identical between Census blocks that did and did not experience a police killing (86.6% and 86.8%, respectively). Third, a simple difference-in-differences regression comparing changes in log population from 2000 to 2010 between treatment and control blocks returns a precise zero estimate ($\beta = 0.006$, p -value = 0.78).¹⁷

EFFECTS ON REGISTRATION AND TURNOUT

Main Results

Turning to our primary results, Panel A of Figure 2 examines the impact of police killings on local registration counts. The omitted period is the last election prior to a killing and the sample spans the 2002 to 2010

¹⁷ Nonetheless, as we discuss in the following section, our main results are robust to including controls for local crime and population growth as well as to a host of other specifications. The Supplementary Material also provides evidence that effects are stable over the sample period, suggesting that larger political events and trends over time (such as, the election of the first Black president or the increasing political salience of immigration) are unlikely to explain our main findings.

FIGURE 2. Effects on Civic Engagement

Note: This figure shows coefficients and 95% confidence intervals from the estimation of Equation 2 on registrations (pre-killing mean = 81.6) and votes (pre-killing mean = 42.9). Unit of observation is the Census block election. Standard errors are clustered by Census block group. The sample spans the 2002–2010 general elections and treatment is defined by blocks where a police killing occurred during the sample period. The red vertical line marks when the police killing occurred. Full regression results are included in column 1 of Table A.3 in the Supplementary Material.

general elections. In the elections prior to a killing, we find strong evidence of parallel trends between treatment and control blocks. Treatment coefficients for $\tau < 0$ are near zero and statistically insignificant, both individually and jointly ($F = 0.33, p = 0.806$). These findings reinforce the plausible exogeneity of police killings and provide support for parallel trends in the counterfactual. Following police killings, registration increases significantly among nearby citizens. Treated blocks gain, on average, about 2.5 additional registrants in the election immediately following the killing and about 4.5 registrants within 4 years. Given that treated blocks contain an average of 80 registrants prior to treatment, these effects represent a meaningful increase of 3%–5%. The stability of point estimates 4–8 years after exposure suggests that effects on registration are persistent over time. As corroboration, Figure A.2 in the Supplementary Material expands the sample to include police killings and elections through 2016 and finds significant effects on registration more than a decade after a killing.¹⁸

Panel B presents analogous results for vote counts. We again find little evidence of differential trends in ballots cast in the lead-up to police killings. The pre-treatment coefficients are individually and jointly insignificant ($F = 1.09, p = 0.350$). After killings, we find a significant, if short-lived, increase of approximately two votes (5% of the pre-killing mean).

Table 2 presents a series of robustness checks under alternative specifications. For concision, column

1 estimates our base model with a single posttreatment indicator, representing the average treatment effect across future elections. We find that, on average, police killings lead to 3.6 more registrations and 1.7 more votes per election in treated blocks. To account for potential confounds due to local crime, column 2 controls for the number of homicides in a block in the 2 years preceding each election. Column 3 includes interactions between quintiles of minority population share in 2000 and election fixed effects to allow for differential voting trends among minority neighborhoods, which may be more likely to experience police killings. Given that younger individuals may have aged into voting eligibility during the sample period, column 4 includes interactions between quintiles of adolescent population (i.e., ages 10–17) in 2000 and election fixed effects. To account for differential population growth, column 5 includes interactions between percent population change from 2000 to 2010 and election fixed effects. In contrast, column 6 excludes all population controls from the regression. To demonstrate robustness to sample selection, column 7 drops the single Census block that experienced multiple distinct police killing incidents over the sample period, whereas column 8 expands the sample to include all Census blocks, even those with less than five adults in 2000 or 2010. Alternatively, column 9 restricts the sample to Census blocks with at least 10 registered voters in 2002.

We find similar results across all specifications, with significant and positive effects on voter registration and turnout. These findings provide evidence of the robust causal relationship between police killings and local political participation. In particular, we find that individuals are mobilized to register and vote by extreme acts of police violence.

¹⁸ As 2010 block-level total vote counts and age/ethnicity/party-specific registration counts are not available for elections after 2010, we are unable to extend the voting and heterogeneity analysis to more recent periods.

TABLE 2. Effects on Civic Engagement: Alternative Specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
<i>Panel A: DV = Registrations</i>									
Treat x Post	3.636*** (1.308)	3.638*** (1.308)	3.627*** >(1.306)	3.614*** (1.309)	3.484*** (1.295)	5.357*** (1.364)	3.495*** (1.306)	2.935*** (1.074)	3.293** (1.332)
Mean	81.59	81.59	81.59	81.59	81.59	81.59	80.72	69.14	85.13
<i>Panel B: DV = Votes</i>									
Treat x Post	1.743** (0.867)	1.745** (0.867)	1.814** (0.868)	1.742** (0.869)	1.666* (0.861)	3.511*** (0.909)	1.748** (0.871)	1.250* (0.732)	1.631* (0.877)
Mean	42.87	42.87	42.87	42.87	42.87	42.87	42.23	36.24	44.75
Model	Main	Homicide ctrls	Minority% × Elect	Pop 10–17 × Elect	Pop Δ × Elect	w/o Pop ctrls	w/o Multi- treaters	Full sample	2002 Reg ≥ 10
No. of obs.	341,420	341,420	341,420	341,420	341,420	341,420	341,415	547,815	306,340

Note: This table shows results from the estimation of Equation 2 on registrations and votes, replacing time to treatment indicators with a single posttreatment indicator. Column 1 examines our preferred specification. Column 2 controls for the number of homicides in a block in the 2 years preceding each election, yielding $\beta_{homicide}$ of 0.320 ($p = 0.079$) for registrations and 0.215 ($p = 0.079$) for votes. Column 3 includes interactions between minority share quintiles in 2000 and election fixed effects. Column 4 includes interactions between quintiles of population aged 10–17 in 2000 and election fixed effects. Column 5 includes interactions between percent population change (from 2000 to 2010) and election fixed effects. Column 6 removes all population controls (i.e., interactions between population decile and election fixed effects). Column 7 drops the single Census block that experienced more than one police killing during the sample period. Column 8 includes all Census blocks, even those with less than five people over the age of 18 in 2000 or 2010. Column 9 restricts the sample to Census blocks with 10 or more registered voters in 2002. Unit of observation is the Census block election. The sample period spans the 2002–2010 general elections. Standard errors are clustered by Census block group. Mean registrations and votes are reported for treatment blocks in the election prior to the killing. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.10$.

Heterogeneity

Voter and Deceased Race

Given large demographic and partisan differences in views of law enforcement, we examine heterogeneous responses to police violence. To explore how effects differ across voter race, we make use of vote and registration counts by ethnicity provided by the California Statewide Database. The database predicts Hispanic ethnicity from voter surname using the Census Bureau's Passel-Word list and Asian ethnicity using the surname dictionary of Lauderdale and Kestenbaum (2000).¹⁹ From these measures, we generate estimates of Black (white) vote and registration counts using the following formula:

$$VoteBlk_{b,t} = (VoteTot_{b,t} - VoteHisp_{b,t} - VoteAsn_{b,t}) \times \left(\frac{\%Blk_{b,2010} \times \%VoteBlk_t}{(\%Blk_{b,2010} \times \%VoteBlk_t) + (\%Wht_{b,2010} \times \%VoteWht_t) + (\%Oth_{b,2010} \times \%VoteOth_t)} \right), \quad (3)$$

where $VoteTot_{b,t}$, $VoteHisp_{b,t}$, and $VoteAsn_{b,t}$ are the number of total votes, Hispanic votes, and Asian votes in block b at election t , and $\%Blk_{b,2010}$, $\%Wht_{b,2010}$, and $\%Oth_{b,2010}$ are the share of residents over age 18 who are Black, white, and other race from the 2010 Census. To account for racial differences in voter turnout rates,

we weight by each racial group's statewide turnout rate in election t as estimated by the CPS Voting and Registration Supplement ($\%VoteBlk_t$, $\%VoteWht_t$, and $\%VoteOth_t$).²⁰ Essentially, we weight non-Hispanic, non-Asian votes and registrations in a given block election by each racial group's predicted vote and registration share relative to the other remaining racial groups.²¹

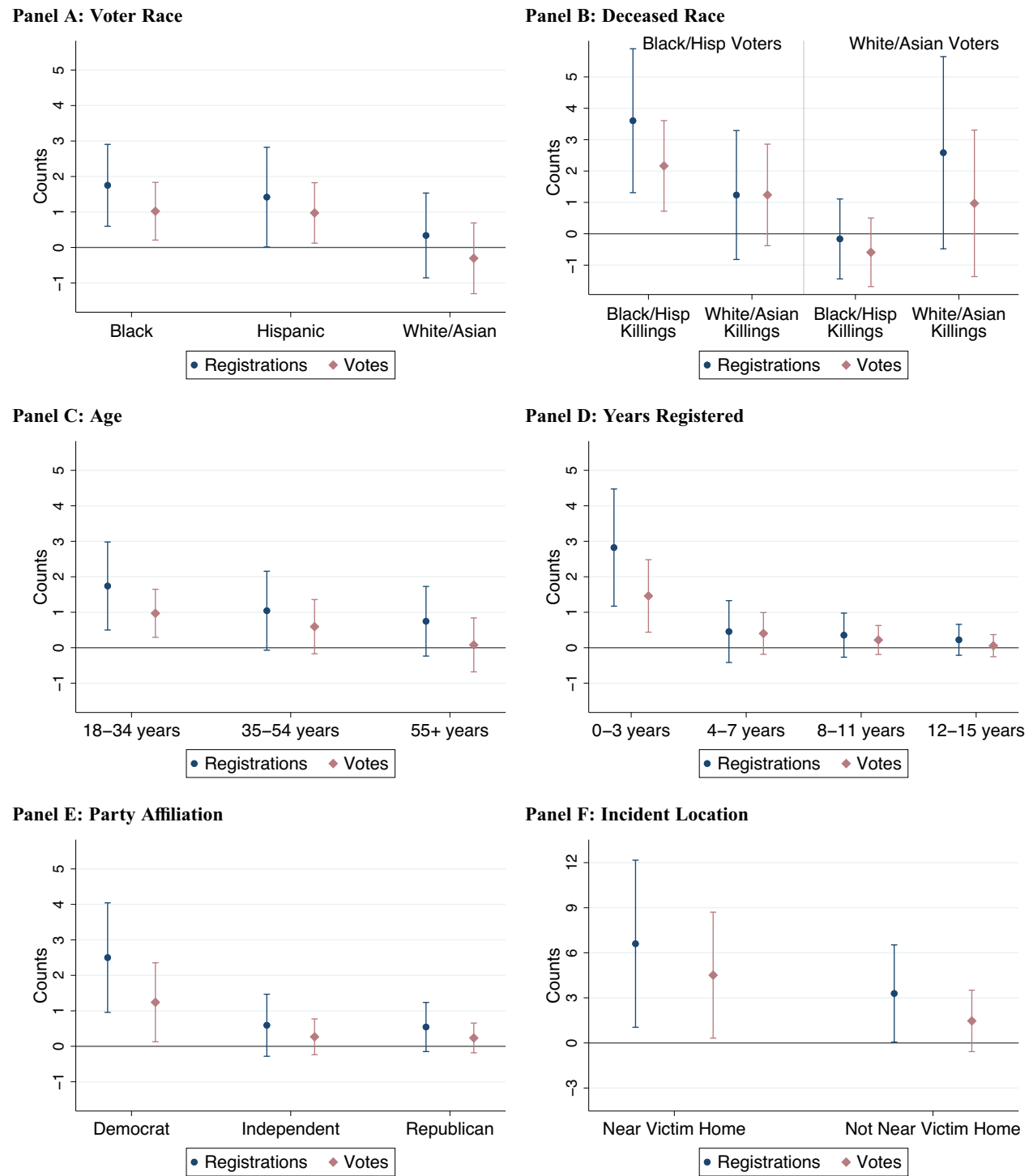
We then estimate our simplified version of Equation 2 on predicted counts by race. As shown in Panel A of Figure 3, a striking pattern emerges. Police killings lead to large increases in Black and Hispanic participation. On average, each police killing increases Black registrations by 1.8 and Black votes by 1.0. These estimates are highly significant and represent a 9%–11% increase over

¹⁹ The Passel-Word list has been shown to be more predictive of Hispanic ethnicity than directly collected Medicare measures (Morgan, Wei, and Virnig 2004; Wei et al. 2006).

²⁰ Due to the small sample size of the "other" racial group, we collapse CPS turnout rates for "other" into presidential and mid-term election averages.

²¹ To address concerns with ecological inference (King 2013), Figure A.3 in the Supplementary Material compares our race estimates for 2010 against estimates obtained by predicting individual race from surname and address in the full voter registration file and aggregating to the block level (Consumer Financial Protection Bureau 2014). Estimates are highly similar with mean differences near zero in both treatment and control areas.

FIGURE 3. Heterogeneous Effects



Note: The panels show coefficients and 95% confidence intervals from the estimation of Equation 2 on registrations and votes, collapsing the time to treatment indicators into a single posttreatment indicator. Panels A and C–E examine the effects separately for each voter group (i.e., by race, age, registration length, and party affiliation). Panels B and F include distinct posttreatment indicators corresponding to each incident type (i.e., by deceased race in Panel B and by proximity to deceased home in Panel F). Standard errors are clustered by Census block group. Unit of observation is the Census block election. The sample spans the 2002–2010 general elections and treatment is defined by blocks where police killings occurred during the sample period. Full regression results are included in Table A.5 in the Supplementary Material.

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the pre-killing mean (20.2 registrations and 9.2 votes). We find similar, if proportionally smaller, responses among Hispanics with increases of 1.4 registrations (5% of mean) and 1.0 votes (6% of mean). In contrast, we find no significant impact on white and Asian participation, with point estimates representing less than 2% of the pre-killing mean.

Further disaggregating by race of the deceased in Panel B, we find suggestive evidence that racial concordance between the voter and the deceased may lead to larger effects. Among Black and Hispanic voters, point estimates for registrations (votes) are roughly 200% (75%) larger for killings of Black and Hispanic individuals than for killings of white and Asian individuals. Similarly, for white and Asian voters, we find positive, if noisy, estimates for killings of white and Asian individuals, but near zero estimates for killings of different-raced individuals.

That effects are primarily concentrated among Black and Hispanic citizens is consistent with a host of evidence documenting large racial differences in perceptions of law enforcement. Researchers have found that race is the single strongest predictor of trust in police (Taylor et al. 2001) and that Black and Hispanic individuals are far more likely than others to believe that use of force is excessive, unjustified, or a serious social concern (AP-NORC 2015; Davis, Whyde, and Langton 2018; Weitzer and Tuch 2002).

Voter Age, Years Registered, and Political Affiliation

In Panel C, we find that gains in voter participation are driven by younger individuals. Following police killings, registrations among nearby 18–34 year-olds increase by about 7% (pre-killing mean = 26.8), whereas votes increase by roughly 9% (pre-killing mean = 10.4). In contrast, treatment estimates are statistically insignificant for older bins and decrease in magnitude with age (all estimates are less than 4% of the pre-killing mean). Consistent with this, Panel D demonstrates that increases in turnout come entirely from individuals who registered within 3 years of a given election. That point estimates for registration are near zero for longer-registered voters also provides evidence that the registration effects are not driven by differential migration (i.e., previously registered voters moving into treatment areas).

In Panel E, we show that effects are also concentrated among registered Democrats. We find no significant impact on registration or vote counts among Republicans or independents. These results are reflective of long-standing partisan gaps in views of law enforcement. Survey evidence from 1970 found that Democrats were more likely to oppose police use of force than Republicans (Gamson and McEvoy 1970) and Democrats today remain much more skeptical of police accountability and discretion (Morin et al. 2017).²²

²² For example, 27% of Democrats versus 74% of Republicans believe police do a good job “using the right amount of force.”

Family and Household Responses

To interrogate the possibility that effects are driven by family and household members of the deceased, we obtained full-count voter registration files from 2004 to 2010 and searched for new registrants who lived in the Census block of a killing and bore the same last name as the deceased. Across 204 killings that occurred in this window, we identified 25 total matches or roughly 0.12 new surname-match registrants per killing. While this is obviously an imperfect measure of kinship networks, it suggests that increased participation among family members of the deceased cannot account for the significant effects on voter participation, which average 3.5 new registrants in the surrounding block.

As further corroboration, we examine whether effects differ depending on whether a police killing occurred near the home of the deceased. While we do not observe the exact home address of all individuals killed by police, we are able to infer whether a killing occurred near the deceased’s residence based on district attorney incident report descriptions.²³ If increased participation was driven primarily by the deceased’s household, we would expect police killings that occurred far from the deceased’s home to have little impact on registration rates in the block of the incident. However, as shown in Panel F of Figure 3, we find significant effects regardless of incident location. This suggests that less proximal individuals, such as witnesses, neighbors, and other local community members—as opposed to family or household members—likely account for much of the mobilization effects.

Role of Media

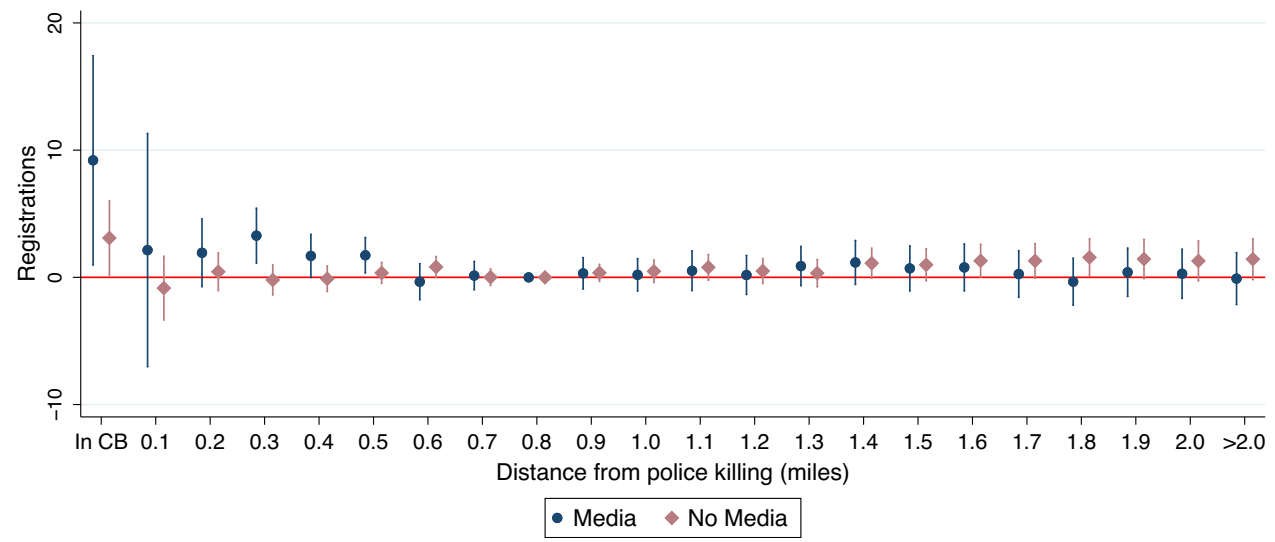
Given the recent proliferation of viral footage capturing acts of police violence, we interrogate the role of media in community responses to these events. Specifically, we examine how spatial spillovers vary by media coverage. To do so, we estimate separate distance gradients for killings that were and were not mentioned in local newspapers using Equation 1.

As shown in Figure 4, we find suggestive evidence of wider spread effects following police killings that were covered in the media than those that were not. While effects for unmentioned killings are contained to the immediate block, we find statistically significant effects extending to blocks nearly 0.50 miles away for killings mentioned in the media. While media coverage may simply reflect (rather than influence) community awareness or perceptions of an incident, these patterns may nonetheless help to explain the discrepancy between the national responses to recent high-profile police killings and the highly localized effects we observe here.

MECHANISMS

While our results indicate that police violence may increase local civic participation, the motivation behind

²³ Of the 292 sample killings, 48 were identified as occurring in or outside the deceased’s home.

FIGURE 4. Effects by Media Coverage

Note: This figure shows coefficients and 95% confidence intervals from the estimation of Equation 1 on registrations, separately for killings that were and were not reported in local newspapers. Unit of observation is the Census block election. The sample spans the 2002–2010 general elections. Indicators are mutually exclusive and track a Census block’s minimum distance to police killings that occurred prior to a given election. “In CB” refers to blocks where killings occurred. Other blocks are partitioned into 0.1-mile bins. The Census block group by election fixed effects and Census block group-clustered standard errors are included. Full regression results are included in columns 3 and 4 of Table A.1 in the Supplementary Material.

these responses is theoretically ambiguous. For example, if officer-involved killings cause citizens to perceive higher rates of local crime, changes in turnout could reflect support for more intensive policing (Cummins 2009). On the other hand, these events may raise concerns about institutional discrimination or police accountability such that citizens are spurred to reform the criminal justice system.

To disentangle mechanisms, we examine differential effects based on whether the person killed by police possessed a weapon. If voters are motivated by heightened concerns about crime, we would expect larger effects following police killings of armed suspects, which likely involved more gunfire or individuals who posed greater danger to the community. If instead voters are mobilized by perceptions of government misconduct, we would expect the largest effects to stem from killings of unarmed individuals.

Figure 5 presents results separately for police killings of armed and unarmed individuals.²⁴ Notably, estimates are small or insignificant for police killings of individuals armed with a knife, gun, or other weapon (Panel A), with average treatment effects of 2.7 registrations ($p = 0.047$) and 1.6 votes ($p = 0.131$).²⁵ However, police killings of unarmed individuals lead to large increases in participation (Panel B). The average treatment effects of 10.4 registrations ($p = 0.047$) and 4.7 votes ($p = 0.040$) correspond to nearly 15% of the pre-killing means and are

three to four times larger than effects for armed killings.²⁶ That police killings of unarmed individuals generate such large relative spillovers suggests that voters are responding to the perceived “reasonableness” of officer actions as much as to the violence itself.

As corroboration, we test whether police killings affect support for criminal justice reforms using data on referenda voting. Specifically, we examine block-level vote shares for California Proposition 66 in 2004 and California Proposition 5 in 2008, both of which sought to reduce criminal penalties for lower-level offenses. While both propositions were narrowly defeated, they provide a local measure of policy preferences and potential insight into beliefs about law enforcement.²⁷

We estimate the following difference-in-differences model:

$$y_{b,t} = \delta_b + \delta_t + \beta Treat_b \times Post_t + \epsilon_{b,t}, \quad (4)$$

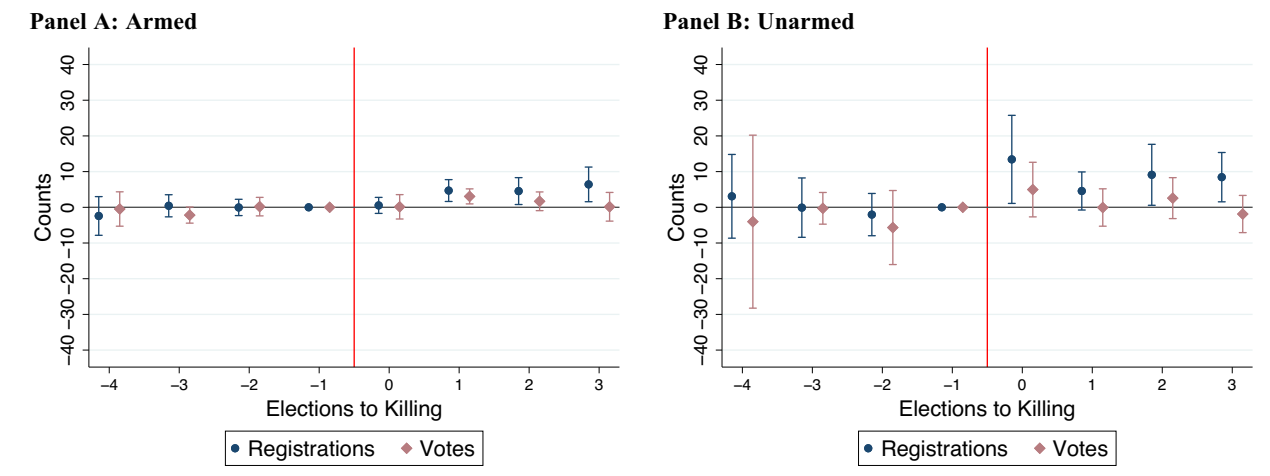
where $y_{b,t}$ is the share of Yes ballots cast for Proposition 66 in 2004 and the share of Yes ballots cast for Proposition 5 in 2008. $Treat_b$ is an indicator for Census blocks that experienced a police killing between the 2004 and 2008 elections, whereas $Post_t$ is a 2008 indicator. To improve internal validity, the sample is restricted to blocks in treated Census block groups. Standard errors are clustered by Census block group.

²⁴ Figure A.4 in the Supplementary Material shows similar results when excluding population controls.

²⁵ Average treatment effects are derived from regressions with a single posttreatment indicator in place of the full set of leads and lags.

²⁶ Similar relative differences persist when including the 14% of sample killings with unknown weapon status.

²⁷ Proposition 66 failed by a 52.7% to 47.3% margin and Proposition 5 by a 59.5% to 40.5% margin.

FIGURE 5. Effects by Deceased Weapon

Note: This figure shows coefficients and 95% confidence intervals from the estimation of Equation 2 on registrations (pre-killing mean = 81.6) and votes (pre-killing mean = 42.9). Unit of observation is the Census block election. Panel A defines treatment as Census blocks with a police killing of an individual armed with a knife, gun, or other weapon. Panel B defines treatment as Census blocks with a police killing of an unarmed individual. Estimates are similar when including incidents with unknown weapon type. Standard errors are clustered by Census block group. The sample spans the 2002–2010 general elections. The red vertical line marks when the police killing occurred. Full regression results are included in columns 2 and 3 of Table A.3 in the Supplementary Material.

As shown in column 1 of Table 3, we find that support for criminal justice reform increased significantly in blocks that experienced a police killing relative to other blocks in the same Census block group. These effects represent a meaningful change in policy preferences—the 5.2 percentage point increase in pro-reform ballot share is equivalent to nearly 15% of the 2004 treatment mean.

Column 2 disaggregates this effect across armed and unarmed killings and finds significant gains in support for criminal justice reform following both types of events. However, point estimates for unarmed killings are more than twice as large as those for armed killings (0.108 vs. 0.043). Along with our findings for registration and voting, these results suggest a consistent picture of civic behavior—acts of police violence that appear less justified provoke more skepticism of the criminal justice system and increase political support for reform efforts.

Given that this analysis relies on only two elections, we are unable to examine pre-trends in political sentiment. We instead conduct a placebo test examining how support for criminal justice reform changed in blocks that would experience a police killing *after* the 2008 election. That is, we estimate Equation 4 for referenda voting in 2004 and 2008 on the sample of blocks in Census block groups with a police killing between 2008 and 2010. The placebo treatment group is, therefore, comprised of the exact blocks with a police killing after 2008, whereas the control group contains blocks without a police killing in the same Census block groups. Results are shown in columns 3 and 4. Notably, placebo treatment estimates are insignificant and very near zero in all cases. Analogous to parallel pre-trends, these results suggest that our actual treatment

estimates reflect the impact of police killings as opposed to other confounds correlated with the timing and location of those events.

Taken together, these results indicate that civic responses to police violence are driven by individuals opposed to law enforcement actions. Our findings suggest that these individuals may be mobilized by killings that appear the least justified and may seek to reform the criminal justice system, at least partly, through the electoral process.

CONCLUSION

This article documents the causal impact of police killings on local political participation. We find that acts of extreme police violence significantly increase voter registration and turnout among nearby residents. These effects are driven by new registrations among historically under-enfranchised groups—young Black and Hispanic individuals. Strikingly, gains in civic engagement are largest following police killings of unarmed individuals and are accompanied by increased support for criminal justice reforms. Together, our results add to emerging evidence of the social consequences of police use of force (Ang 2021; Bor et al. 2018; González and Prem 2020; Legewie and Fagan 2019).

The direction of our findings differs from research documenting the demobilizing impact of direct and familial criminal justice contact. One explanation for this divergence is that our results do not appear to be driven by family or household members of the deceased—those individuals who may bear the greatest personal and economic loss from a police killing. Given the public nature of many police shootings, it seems instead that

TABLE 3. Effects on Support for Criminal Justice Reform

	<i>DV = Support for criminal justice reform (%)</i>			
	Actual treatment		Placebo treatment	
	(1)	(2)	(3)	(4)
Treat x Post	0.052*** (0.019)		0.001 (0.021)	
× Armed		0.043** (0.021)		0.024 (0.033)
× Unarmed		0.108** (0.051)		-0.015 (0.072)
Mean	0.397		0.431	
Year of killing	2004–2008		2008–2010	
No. of obs.	2,622		1,302	

Note: This table reports results from the estimation of Equation 4 on the fraction of ballots cast in favor of referenda that would have weakened penalties for lower-level offenses (i.e., California Proposition 66 in 2004 and California Proposition 5 in 2008). Unit of observation is the Census block election. Data are restricted to Census block groups that experienced a police killing between 2004 and 2008 in the “Actual treatment” sample and that experienced a killing between 2008 and 2010 in the “Placebo treatment” sample. Treatment is defined by blocks where a police killing occurred. Columns 1 and 3 report the changes in the pro-reform support between 2004 and 2008 for blocks with actual (placebo) police killings, whereas columns 2 and 4 interact the treatment (placebo) indicator with indicators for whether the deceased was armed or unarmed. Standard errors are clustered at the Census block group level. Mean support for treatment and placebo blocks in 2004 are listed. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.10$.

concerned residents, witnesses, and other, less proximal individuals are responsible for the gains in turnout. This is consistent both with work by Walker (2020) suggesting that vicarious police contact can mobilize groups that feel targeted by law enforcement and with research by Lerman and Weaver (2014b) showing that the effects of policing on local citizen engagement can vary—even directionally—according to the intensity and nature of police–civilian interactions.

With the proliferation of social media and the advent of the Black Lives Matter movement in 2013, an important question is whether our results generalize to more recent years. While this can only be answered with further research, there are several reasons to expect qualitatively similar effects today. First, in Figure A.5 in the Supplementary Material, we examine whether registration effects differ between more and less recent police killings. Notably, local registration effects are positive and remarkably stable in magnitude over the extended sample period (i.e., 2002–2016). Second, we presented evidence that police killings covered in local newspapers produced wider-spread—but directionally similar—effects as unpublicized incidents. This suggests that growing public attention to police violence may lead to the propagation of similar mobilization effects at the state or national level. Third, data indicate that Joe Biden’s 2020 presidential victory was fueled partly by record turnout among minority youth, the same demographic that is most likely to support the Black Lives Matter movement and for which we observe the largest responses to police violence (Pew Research Center 2020; Tufts College 2020).

Considering whether this article’s results may extend to other cities is a similarly important and speculative

exercise. Given Los Angeles’ particular history with police brutality as well as evidence of the mediating role of perceived injustice, officer-involved killings in areas with less fraught police–community relationships may not incite the same electoral responses. On the other hand, the racial and demographic pattern of effects we observe are mirrored in national surveys examining group perceptions of law enforcement. Thus, while specific electoral contexts may differ across cities, significant concerns about police accountability and use of force are shared by minority communities throughout the country.

Together, these findings highlight the pivotal role that law enforcement and social justice concerns may play in shaping civic engagement and provide empirical complement to long-standing concerns about race and policing (Kirk and Papachristos 2011; Tyler, Fagan, and Geller 2014; Weitzer and Tuch 2006). In 1968, the Kerner Commission reported on the deep-rooted belief in a “double-standard of justice and protection” underlying widespread civil unrest. Our findings suggest that such beliefs continue to permeate communities of color today and are exacerbated by acts of police violence. They tell a nuanced story about Black and Hispanic citizens responding to these concerns by strategically engaging with formal electoral systems in an effort to hold institutions accountable.

At the same time, we caution that our estimates represent average effects and may fail to capture the wide range of responses that police violence may engender. Our findings do not rule out the possibility that some individuals are demobilized by police killings or that already disenfranchised groups are further disempowered. Rather, they tell us only that any demobilizing effects are outweighed by increased

participation among other citizens. Indeed, that increased registrations are driven by younger voters suggests that individuals may grow numb to state violence over time. As fatal shootings comprise less than 1/10th of 1% of all use of force encounters (Davis, Whyde, and Langton 2018), it is also possible that the long-run consequences of repeated exposure to traumatic police encounters may be far different from the marginal effects documented here. Further interrogation of these questions is critical to understanding the role of law enforcement in democratic engagement and representation.

SUPPLEMENTARY MATERIAL

To view supplementary material for this article, please visit <https://doi.org/10.1017/S0003055423000515>.

DATA AVAILABILITY STATEMENT

Research documentation and data that support the findings of this study are openly available at the American Political Science Review Dataverse: <https://doi.org/10.7910/DVN/EOIIEV>.

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CONFLICT OF INTERESTS

The authors declare no ethical issues or conflicts of interests in this research.

ETHICAL STANDARDS

The authors affirm this research did not involve human subjects.

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