

# Civic Responses to Police Violence\*

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July 15, 2021

## Abstract

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 hyper-local 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.

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\*We are very grateful for helpful comments from Amy Finkelstein, Edward Glaeser, Lawrence Katz, Amy Lerman, Andrei Shleifer, Ariel White, seminar participants at University of California, Berkeley, Massachusetts Institute of Technology and the 2020 APSA conference. Tebes benefited from generous financial support from the Stone Scholar Fellowship for Social Policy and Inequality Research. All errors are our own.

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# I 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 to 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 walk-outs 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 can have drastic demobilizing consequences (Weaver and Lerman 2010; Lerman and Weaver 2014b; White et al. 2019; White 2019). 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 paper provides the first causal evidence of the impact of police violence on voter participation. We combine highly detailed voter registration data from Los Angeles County with novel incident-level data on the timing, location and context of 294 police killings spanning nearly a decade. To identify the effects of police killings, we leverage a flexible difference-in-differences design exploiting hyper-local variation in exposure to these events. This allows us to compare voting patterns in the exact Census block a police killing occurred to adjacent blocks in the same neighborhood.

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 80% of sample killings that went unmentioned 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 other local residents who may have seen or heard about the killing.

However, the aggregate effects do mask significant heterogeneity across demographics. Increased civic engagement is driven entirely by Black and Hispanic citizens, who are 8% 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 striking differences along 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 roughly three 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 non-serious 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.

## II Theory

The core question of this paper is how police killings affect local registration and voting. A large literature documents the potential demobilizing effects of the criminal justice system. This has been shown for incarcerated individuals (Weaver and Lerman 2010; Lerman and Weaver 2014b) and those receiving lower-level misdemeanors (White et al. 2019) with evidence of spillovers to others living in the same household as well (White 2019). This effect is likely driven in part by the downstream economic consequences of a criminal record (Western and Pettit 2010; Agan and Starr 2018) 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 enforcement (White 2016; Gutierrez et al. 2019), there is significant reason to believe that police violence may fuel related concerns about fairness and justice (Weitzer and Tuch 2006). The recent rise of the Black Lives Matter movement following George Floyd’s killing 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. Academic research similarly highlights a positive correlation between “proximal contact” with the criminal justice system — simply knowing someone who has experienced direct contact — and forms of political activity other than voting (Walker 2014).

These narratives point to opposite conclusions about the effects of police killings on local electoral participation. Nonetheless, there exists little empirical work exploring this question. One related strand of literature explores how officer use of force affects citizen cooperation with law enforcement (Lerman and Weaver 2014a; Desmond, Papachristos, and Kirk 2016; Zoorob 2020). However, this work finds mixed results and leaves unanswered whether citizens strategically respond to police violence by engaging with the electoral systems that may help to mitigate it. A separate body of work interrogates the effects of political protests, including those like the Rodney King riots 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 (Fryer Jr 2019; Knox, Lowe, and Mummolo 2020; Hoekstra and Sloan 2020; Durlauf and Heckman 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 (Leiber, Nalla, and Farnworth 1998; Weitzer and Tuch 2002; AP-NORC 2015).<sup>1</sup> In this light, one might expect

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<sup>1</sup>For example, a 2015 survey found that 75% of Black respondents and over 50% of Hispanic respondents

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.

## III Data

### A 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 294 incidents between the 2002 and 2010 general elections.<sup>2</sup> For each killing, the data includes 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.<sup>3</sup> For 253 of the 294 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 I provides a summary of the police killings data. 53% of deceased individuals were Hispanic, 29% were Black, 15% were white and 3% were Asian.<sup>4</sup> 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 old.

Consistent with national statistics, 56% of those killed were armed with a firearm (including BB guns and replicas), while 27% possessed some other type of weapon. This includes

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believe that police violence is a very or extremely serious issue, relative to 20% of whites. Similar disparities arose when asked whether police “deal more roughly with members of minority groups” (AP-NORC 2015).

<sup>2</sup>In a handful of cases, multiple individuals were killed in a single incident. The total number of distinct incidents is 286.

<sup>3</sup>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 1 million copies.

<sup>4</sup>Race categories are mutually exclusive.

Table I: Summary Statistics

<i>Panel A: Police Killings</i>				<i>Panel B: Registration and Voting Sample</i>				
	All	Black/ Hispanic	White/ Asian		All	Killing	No Killing	
							Treat Blk Grp	Ctrl Blk Grp
<b><i>Deceased Demographics</i></b>				<b><i>Demographics (2000 Census)</i></b>				
Black	0.29	0.35	0.00	Pop. 18+	99.28	190.55	100.26	98.84
Hispanic	0.53	0.65	0.00	White	0.39	0.16	0.20	0.40
White	0.15	0.00	0.85	Black	0.09	0.16	0.16	0.08
Asian	0.03	0.00	0.15	Hispanic	0.38	0.57	0.52	0.37
Male	0.96	0.97	0.92	Asian	0.12	0.10	0.10	0.12
Age	30.34	28.98	36.47	Other	0.02	0.01	0.02	0.03
<b><i>Newspaper Mentions</i></b>				<b><i>Voter Registration (2002)</i></b>				
Any	0.18	0.19	0.17	Registrations	56.30	79.93	48.20	56.53
Total	0.70	0.77	0.40	White/Asian	0.56	0.31	0.35	0.57
Median (if any)	2.00	2.00	1.00	Black	0.13	0.27	0.26	0.12
<b><i>Weapon Type</i></b>				Hispanic	0.28	0.39	0.37	0.28
Unarmed	0.17	0.15	0.28	Age 18-34	0.26	0.32	0.30	0.26
Knife	0.27	0.24	0.38	Age 35-54	0.40	0.39	0.39	0.40
Gun	0.56	0.61	0.33	Democrat	0.53	0.62	0.61	0.52
				Republican	0.27	0.17	0.19	0.28
				Reg < 4 yrs	0.36	0.42	0.39	0.35
				Votes	24.64	29.46	18.51	24.87
Killings	294	240	53	Blocks	68,326	285	2,675	65,366

*Panel A* provides summary statistics for the police killings data, separately for killings of Blacks and Hispanics and killings of individuals of whites and Asians. Unless otherwise noted, mean values reported. Newspaper mentions come from a search of each incident by suspect name in six local newspapers including the Los Angeles Times. Any is an indicator for whether the incident was mentioned in any article, Total is the number of articles mentioning the incident. Median is the median number of articles, conditional on being mentioned. Unarmed refers to suspects that did not have a weapon, gun refers to suspects with firearms (including BB guns and replicas), knife refers to suspects with any other type of weapon (values are the share of each weapon type among incidents with contextual details). We were unable to obtain contextual information about weapon type from District Attorney reports and other sources for 14% of killings.

*Panel B* provides summary statistics for the Census blocks included in the main analysis. The remaining columns are mutually-exclusive sub-samples. “Killing” refers to blocks where a killing occurred during the sample period. “Treat Blk Grp” refers to untreated blocks in the same block group as a block with a killing. “Ctrl Blk Grp” refer to all other blocks (i.e., blocks in block groups without police killings). Pop. 18+ refers to population 18 or older in the 2000 Census. Race categories are mutually exclusive categories from the 2000 Census as defined the main text. Registration data are restricted to 2002 general election.

The reason there are 285 killing blocks versus 294 killings is because some incidents involved multiple deaths. In total, there were 286 distinct incidents, two of which occurred in the same block on separate dates.

items like knives and pipes as well as individuals who attempted to hit someone with a vehicle. The remaining 17% of individuals were completely unarmed.

Notably, the vast majority of killings received little or no media coverage. Only 18% of sample killings were ever mentioned in any of six local newspapers. Conditional on coverage, the median number of articles is two. 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 (61% vs. 33%). However, rates of media coverage are similar between groups (19% vs. 17%).

Regardless of circumstance, involved officers were never prosecuted. The District Attorney did not pursue criminal charges against police following any of the 294 sample killings. This is consistent with national statistics, which find that criminal charges are filed against police in fewer than half a percent of all officer-involved shootings.

## B Voter Registration, Turnout and Preferences

Police killings are geocoded to Census blocks and merged to voting information from the California Statewide Database. The database contains information on the number of individuals registered to vote and the number of ballots cast at the 2010 Census block-level for each general election from 2002 to 2010.<sup>5</sup> The advantage of these data relative to standard voter registration files is that they capture registration and voting *at the date of each election*, which allows us to more precisely measure effects.<sup>6</sup> This is particularly important when considering geographies as small as Census blocks, which average less than 0.10 square miles and fewer than 100 adults. In addition to total registration and vote counts, disaggregated counts by ethnicity (i.e., Hispanic, Asian), party affiliation (i.e., Democrat, Republican, 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

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<sup>5</sup>Vote counts are only available through 2010, while registration counts are available through 2018. In the Appendix, we extend the registration sample and find highly persistent effects lasting more than a decade.

<sup>6</sup>Due to irregular registration purges and resident mobility, voter files obtained months after an election can have registration counts that differ substantially from known election-day aggregates.

enacted numerous measures to reduce the 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 comes from the California Statewide Database.<sup>7</sup>

## C 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 analysis, we show similar results under alternative sample restrictions and when extending the registration analysis to include more recent elections.

Panel B of Table I provides a summary of the voter registration data. Blocks that experienced a police killing had, on average, 190 adult residents in 2000, compared to roughly 99 residents in other blocks. Notably, treated blocks are quite similar to untreated blocks in the same Census block group (i.e. the effective control group) in terms of the racial, political and age characteristics of residents and registered voters. These areas also experienced similar rates of turnout (37 vs. 38%) and registration (42% vs. 48%) in 2002.

## IV Empirical Strategy

### A Exposure to Police Killings

A primary concern for identification is that police shootings are likely not random. Thus, a naive comparison of areas where police killings are relatively prevalent and areas where they are not could be confounded by correlated neighborhood factors. Furthermore, if changes in local conditions predict police killings, biases could remain even when including area fixed effects in panel analysis.

To address this, we adopt a similar empirical strategy as Ang (2021) and exploit *within-neighborhood* variation in the location of police killings. Identification comes from comparing changes in voting over time in blocks where police killings occurred to neighboring blocks in the same neighborhood. Thus, except for the police killing itself, local conditions are likely to be similar in level and trend across both areas.

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<sup>7</sup>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/d10/Creating%20CA%20Official%20Redistricting%20Database.pdf>

The validity of this strategy is aided by two factors. First, police killings are quite rare and difficult to predict. Over 300,000 arrests and nearly 60,000 violent crimes occur in Los Angeles each year, compared to 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 likely exogenous.

Second, in contrast to the handful of high-profile events in recent years, the vast majority of police killings receive little or no media coverage. Consistent with Ang (2021), who finds that educational spillovers of police violence are limited to less than 0.50 miles, living in one block versus the other is likely highly correlated with even knowing about the existence of a police killing. This provides meaningful treatment heterogeneity within neighborhoods.

## Graphical Evidence

As evidence, we first examine how effects differ by geographical proximity to a killing. To do so, we construct the following distance metric to capture a given Census block’s proximity to a police killing: for each police killing, we estimate the minimum radius needed for a circle centered on the police killing to cover at least 75% of a block’s area.<sup>8</sup> Using this metric, we run the following least-squares regression to explore how impacts on registration count dissipate with distance from a killing:

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

$y_{b,t}$  is the number of registered voters in block  $b$  at election  $t$ .  $\delta_b$  and  $\delta_{n,t}$  are Census block fixed effects and neighborhood-by-election fixed effects. 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. However, as we will demonstrate, results are robust to excluding these population controls.  $\sum_d \text{Distance}_d$  are a set of mutually-exclusive treatment indicators that track a block’s distance to the nearest police killing that occurred prior to election  $t$ .<sup>9</sup>

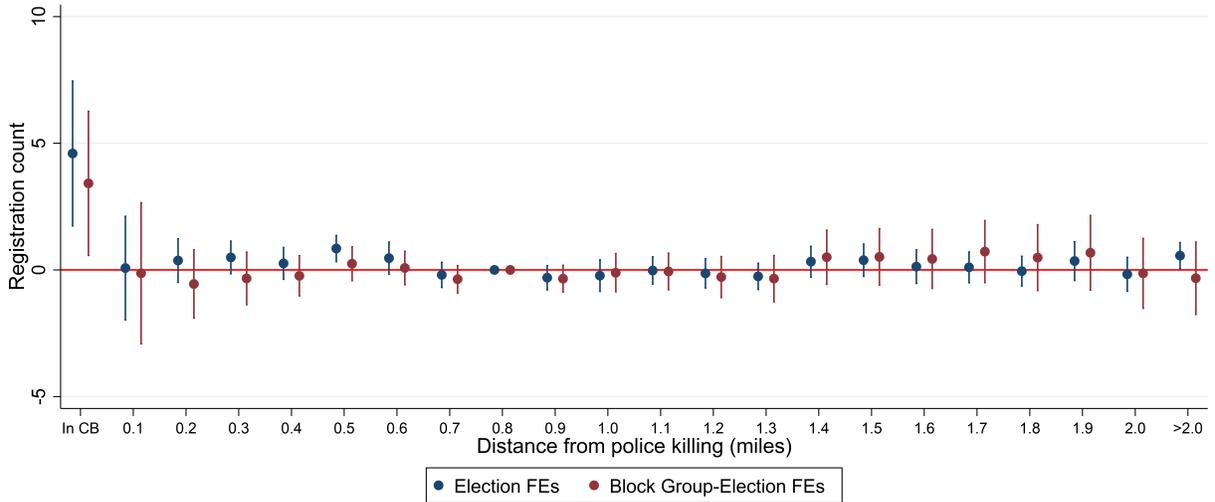
We partition shootings first by whether or not the block was directly exposed to a killing

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<sup>8</sup>We estimate minimum radius in 0.05-mile increments up to 2 miles. Reassuringly, over 95% of blocks containing a police killing are within 0.35 miles of the killing based on this measure. We found other metrics, such as the distance to a block’s centroid, to be substantially noisier.

<sup>9</sup>For example, if a block’s nearest killing before 2007 was 1.5 miles away then experienced another killing 0.5 miles away in 2007, distance is 1.5 miles for elections before 2008 and 0.5 miles for 2008 forward.

Figure I: Effects by Distance From Police Killing



*Notes:* Figure reports coefficients from estimation of Equation 1 on registration counts. Sample spans 2002 to 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 by 0.1-mile bins. Block group by election fixed effects and block group-clustered standard errors are included. Blue dots include election fixed effects. Red dots include Census block group by election fixed effects. Standard errors are clustered at the Census block group-level.

(i.e. occurred in the block), and then into 0.1-mile bins up to 2 miles from the shooting. Figure I plots  $\alpha_d$  coefficients under two different specifications: controlling for election fixed effects across the entire sample and at the Census block group level. Impact is normalized to 0 for blocks 0.7 to 0.8 miles from the killing (since these blocks are within the “zero-impact” region).<sup>10</sup>

The graphical evidence suggests that impacts are indeed hyper-local. Across specifications, treated Census blocks experience a significant increase in registrations of between 3.5 and 5 counts (4-6% of the pre-killing mean). Consistent with the under-publicized nature of police killings, effects fall off dramatically with spatial distance, with zero or near zero estimates for neighboring blocks even slightly further away.

## B Estimating Equation

To estimate effects on civic engagement, we next employ an 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 fol-

<sup>10</sup>In the block group specification, distance indicators are set to 0 within treated block groups so that neighboring blocks are not mechanically biased downwards through the neighborhood-election fixed effects.

lowing base equation on the block-level panel data:

$$y_{b,t} = \delta_b + \delta_{n,t} + \sum_{\tau \neq -1} \beta_{\tau} Shoot_{t,\tau} + \delta_{POP_bxELEC_t} + \epsilon_{b,t}. \quad (2)$$

This is essentially analogous to Equation 1 except we replace the set of treatment distance indicators ( $\sum \alpha_d Distance_d$ ) with a set of time to treatment indicators ( $\sum \beta_{\tau} Shoot_{t,\tau}$ ), fixing treatment to the first killing that occurred in a Census block between the 2002 and 2010 general elections.<sup>11</sup> Neighborhood-time fixed effects ( $\delta_{n,t}$ ) are defined by Census block groups, which include about 11 blocks and measure less than one square mile in area, on average. The coefficients of interest ( $\beta_{\tau}$ ) represent the differential change between relative time  $\tau$  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 neighborhood. Drawing on Bertrand, Dufflo, and Mullainathan (2004), standard errors are clustered by Census block groups, allowing for correlation of errors within each of the sample’s 6,400 Census block groups.

## Crime and Migration

One potential threat to identification is that trends in violent crime may influence both the level of civic engagement (Bateson 2012; Sønderskov et al. 2020) and the presence of police killings in an area. However, given that we control for block group-election fixed effects, any biases would have to be hyper-local, affecting individuals on one street but not the next within the same neighborhood. To test this, Panel A of Figure A.I of the Appendix estimates Equation 2 on criminal homicides in a block-year. Since geocoded data for *all* crimes and arrests is 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 threat is selective migration in response to police violence. As block-level population counts are only measured every decade, we are unable to directly test for differential migration using our main event study model. However, several pieces of corroborating evidence suggest that it is unlikely to be a serious concern. First, given the positive effects on registration and vote counts, the main threat would be if police killings *increased* population in a neighborhood. However, examining administrative schooling data from Los Angeles, Ang (2021) finds little effect of exposure to police killings on school transfers among nearby students. In fact, point estimates are near zero and suggest, if

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<sup>11</sup>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.

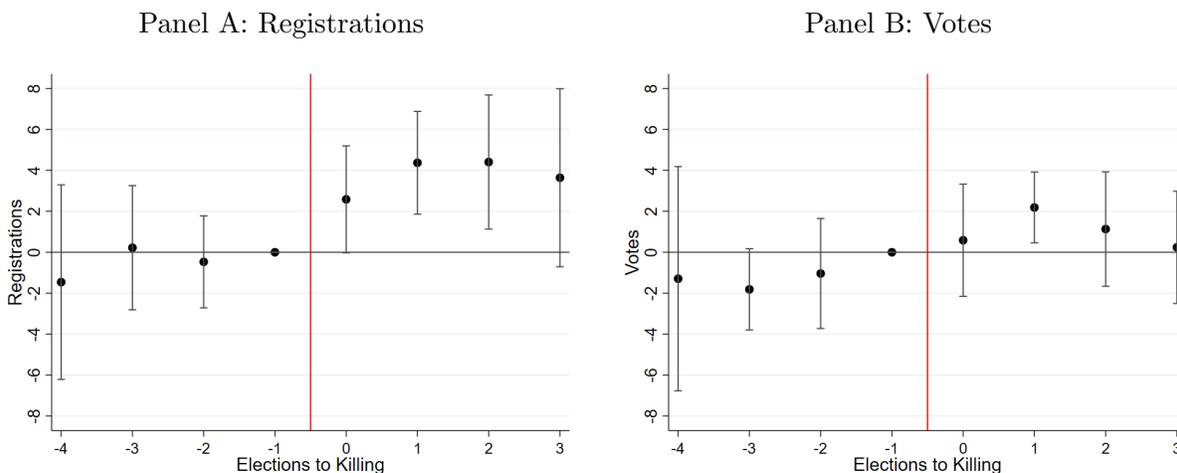
anything, small *reductions* in local population. Second, 2006-2010 ACS data indicates that the share of individuals who reported residing at the same house one year prior is virtually identical between Census blocks that did and did not experience a police killing (86.6% and 86.8%, respectively). Third, a simple difference-in-differences regression comparing changes in log population from 2000 to 2010 across blocks that did and did not experience a killing returns a precise zero estimate ( $\beta = 0.006$ , p-value=0.78).

## V Effects on Registration and Turnout

### A Main Results

Turning to our primary results, Panel A of Figure II 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 general elections.

Figure II: Effects on Civic Engagement



*Notes:* Figure shows treatment estimates and 95 percent confidence intervals from estimation of Equation 2 on registrations (pre-treatment mean = 81.6) and votes (pre-treatment mean = 42.9). Unit of observation is registrations/votes in a Census block-election. Standard errors are clustered by Census block group. The sample spans the 2002 to 2010 general elections and treatment is defined by blocks where police killings occurred during the sample period. Red vertical line represents time of treatment.

In the elections prior to a killing, we find strong evidence of parallel trends between treatment and control areas. 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 four years. Given that treated blocks contain an average of 80 registrants prior to treatment, these effects represent a meaningful increase of 3 to 5%. The stability of point estimates four to eight years after exposure suggests that effects on registration are persistent over time. As corroboration, Appendix Figure A.II expands the sample to include elections through 2016 and reports significant effects on registration more than a decade after a killing.<sup>12</sup>

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 2 votes (5% of the pre-killing mean).

Table II: Effects on Civic Engagement: Alternative Specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
<i>Panel A: Registrations</i>									
Treat x Post	3.636*** (1.308)	3.638*** (1.308)	3.632*** (1.308)	3.614*** (1.309)	3.483*** (1.295)	5.357*** (1.364)	3.495*** (1.306)	2.935*** (1.074)	3.196** (1.331)
Pre-Treat Mean	81.59	81.59	81.59	81.59	81.59	81.59	79.91	69.14	84.00
<i>Panel B: Votes</i>									
Treat x Post	1.743** (0.867)	1.745** (0.867)	1.722** (0.866)	1.742** (0.869)	1.666* (0.861)	3.511*** (0.909)	1.748** (0.871)	1.250* (0.732)	1.510* (0.878)
Pre-Treat Mean	42.87	42.87	42.87	42.87	42.87	42.87	41.81	36.24	44.10
Model	Main	Homicide Controls	% Minority x Elect. FE	Pop. 10-17 x Elect. FE	Pop. Change x Elec. FE	Drop Pop. Drop Controls	Drop Multi- Treater	Full Sample	2002 Reg. ≥ 10
Obs.	341,420	341,420	341,420	341,420	341,420	341,420	341,415	547,815	308,060

Table shows results from estimation of Equation 2 on registrations and votes, replacing time to treatment indicators with a single post-treatment dummy. Column 1 examines our preferred specification. Column 2 controls for the number of homicides in a block in the two years preceding each election. Column 3 includes minority share quintile by election fixed effects. Column 4 adds quintiles of population aged 10-17 in 2000 by election fixed effects. Column 5 includes interactions between percent population change (from 2000 to 2010) and election fixed effects. Column 6 excludes all population controls (i.e., population decile by 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 5 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 block-election. The sample period spans the 2002 to 2010 general elections. Standard errors clustered by Census block group. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , and \*  $p < 0.10$ .

Table II presents a series of robustness checks under alternative specifications. Column 1 shows our base model with a simple post-treatment indicator. We find that, on average,

<sup>12</sup>As block-level vote counts are not available for elections after 2010, we are unable to replicate this analysis with voting.

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 two years preceding each election. Column 3 includes quintiles of minority population share by election fixed effects to allow for differential voting patterns 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 election fixed effects and the population of 10 to 17 year-olds in a block in 2000. 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.<sup>13</sup> To demonstrate robustness to sample selection, Column 7 drops the single Census block that experienced multiple distinct police killing incidents over the sample period, while 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.

## B 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, which predicts Hispanic ethnicity from voter surname using the Census Bureau’s Passel-Word list and Asian ethnicity using Lauderdale and Kestenbaum’s (2000) surname dictionary.<sup>14</sup> From these measures, we generate estimates of Black (white) vote and registration counts using the following formula:

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<sup>13</sup>Full event study results excluding the population decile-by-election fixed effects are discussed in Section VI and included in Appendix A.IV.

<sup>14</sup>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).

$$\begin{aligned}
VoteBlk_{b,t} = & (VoteTot_{b,t} - VoteHis_{b,t} - VoteAsn_{b,t}) \times \\
& \left( \frac{\%Blk_{b,2010} \times \%VoteBlk_t}{(\%Blk_{b,2010} \times \%VoteBlk_t) + (\%Wht_{b,2010} \times \%VoteWht_t) + (\%Oth_{b,2010} \times \%VoteOth_t)} \right)
\end{aligned}
\tag{3}$$

where  $VoteTot_{b,t}$ ,  $VoteHis_{b,t}$  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 turnout rate in California during election  $t$  as estimated by the CPS Voting and Registration Supplement ( $\%VoteBlk_t$ ,  $\%VoteWht_t$ , and  $\%VoteOth_t$ ).<sup>15</sup> 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.<sup>16</sup>

We then estimate our simplified version of Equation 2 on predicted counts by race. As shown in Panel A of Figure III, a striking pattern emerges. Police killings lead to large increases in Black and Hispanic participation. On average, each police killing increases Black registrations by 1.7 and Black votes by 1.0. These estimates are highly significant and represent an 8 to 11% increase over the pre-killing mean (20.2 registrations and 9.2 votes). We find similar, if proportionally smaller, responses among Hispanics with increases of 1.5 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 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 150% (60%) 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.

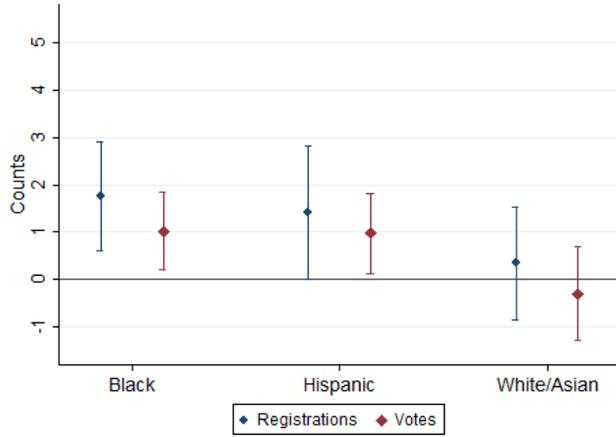
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<sup>15</sup>Due to the small sample size of the “other” racial group, we collapse CPS turnout rates for “other” into presidential and mid-term election averages.

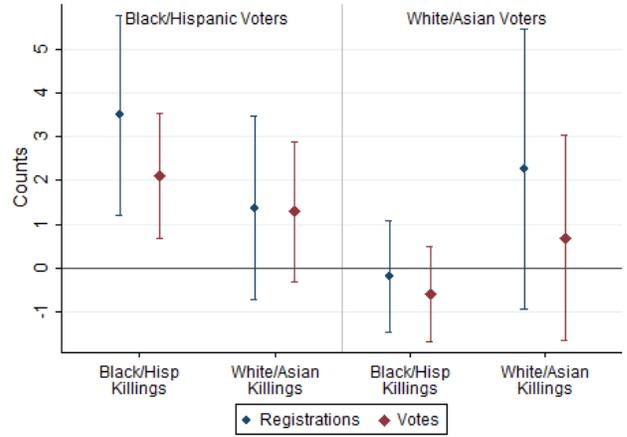
<sup>16</sup>To address concerns with ecological inference (King 2013), Appendix Figure A.III compares our race estimates for 2010 against estimates obtained by predicting individual race from surname and address in the full voter registration file and aggregating to the block-level (Bureau 2014). Estimates are highly similar with mean differences near zero in both treatment and control areas.

Figure III: Heterogeneous Effects

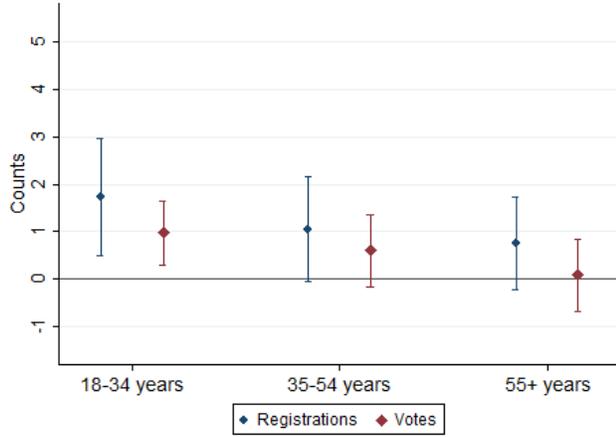
Panel A: Voter Race



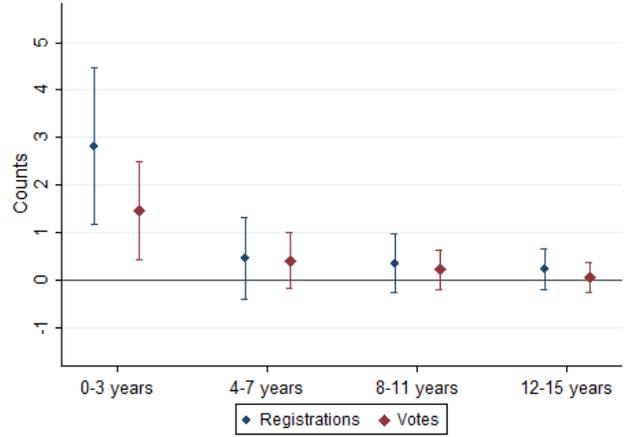
Panel B: Deceased Race



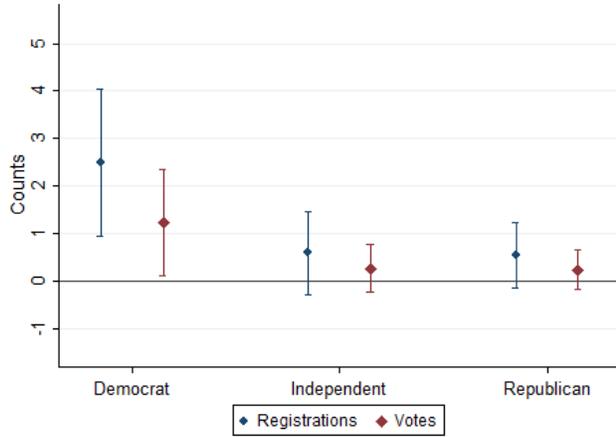
Panel C: Age



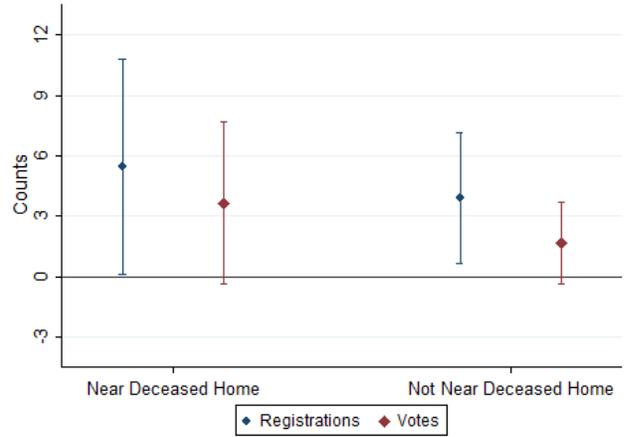
Panel D: Years Registered



Panel E: Party Affiliation



Panel F: Incident Location



Notes: Panels report results from estimating Equation 2 separately for each voter group (i.e., by race, age, registration length and party affiliation), replacing the time to treatment indicators with a single post-treatment indicator. Panels B and F include distinct post-treatment 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 to 2010 general elections and treatment is defined by blocks where police killings occurred during the sample period.

Researchers have found that race is the single strongest predictor of trust in police (Taylor et al. 2001) and Black and Hispanic individuals are far more likely than others to believe that use of force is excessive, unjustified or a serious social concern (Weitzer and Tuch 2002; AP-NORC 2015; Davis, Whyde, and Langton 2018).

### **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- to 34-year-olds increase by about 7% (pre-killing mean = 26.8), while votes increase by roughly 10% (pre-killing mean = 10.4). In contrast, treatment estimates for individuals over 35 years old are statistically insignificant and small in magnitude (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 longstanding 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).<sup>17</sup>

## **C 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.

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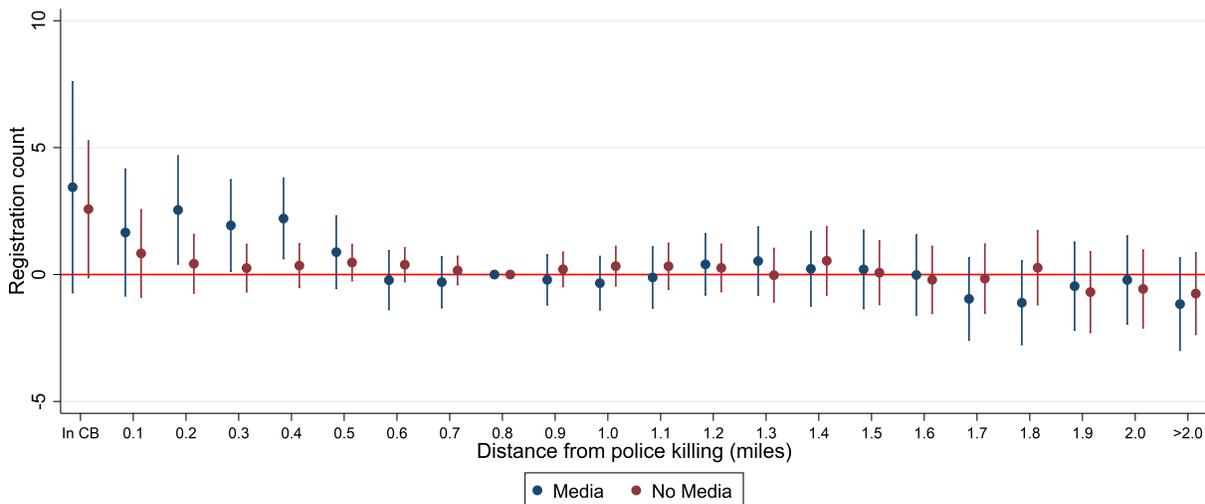
<sup>17</sup>For example, 27% of Democrats versus 74% of Republicans believe police do a good job “using the right amount of force.”

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 DA incident report descriptions.<sup>18</sup> If increased participation was driven primarily by the deceased’s household, then 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 III, 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.

## D Role of Media

Given the recent proliferation of viral footage capturing acts of police violence, we further 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 distance gradients separately for killings that were and were not mentioned in local newspapers using Equation 1.

Figure IV: Effects by Media Coverage



*Notes:* Figure reports coefficients from estimation of Equation 1 on registration counts, separately for killings that were and were not reported in local newspapers. Sample spans 2002 to 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 by 0.1-mile bins. Block group by election fixed effects and block group-clustered standard errors are included.

<sup>18</sup>Of the 294 sample killings, 51 were identified as occurring in or outside the deceased’s home.

As shown in Figure IV, 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.

## VI Mechanisms

While our results indicate that police violence may increase local civic participation, the motivation behind 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 V presents our event study results separately for police killings of armed and unarmed individuals.<sup>19</sup> Notably, estimates are small or insignificant for police killings of individuals armed with a knife or a gun (Panel A), with average treatment effects of 2.7 registrations ( $p = 0.042$ ) and 1.5 votes ( $p = 0.133$ ).<sup>20</sup> However, police killings of unarmed individuals lead to large increases in participation (Panel B). The average treatment effects of 11.5 registrations ( $p = 0.025$ ) and 5.0 votes ( $p = 0.025$ ) correspond to nearly 15% of the pre-killing means and are three to four times as large as effects for armed killings.<sup>21</sup> 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.

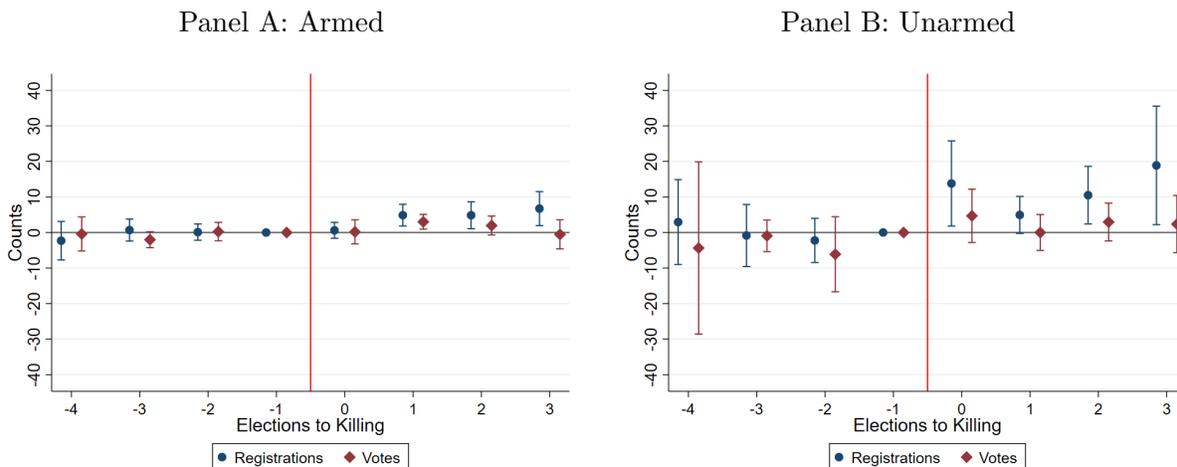
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<sup>19</sup>Appendix Figure A.IV shows similar results when excluding population controls.

<sup>20</sup>Average treatment effects are derived from regressions with a single post-treatment indicator in place of the full set of leads and lags.

<sup>21</sup>Similar relative effects exist when including the 14% of sample killings with unknown weapon status.

Figure V: Effects by Deceased Weapon



*Notes:* Figure shows treatment estimates and 95 percent confidence intervals from estimation of Equation 2 on registrations (pre-treatment mean = 81.6) and votes (pre-treatment mean = 42.9). Unit of observation is registrations/votes in a Census block-election. Panel A restricts treatment group to killings of individuals armed with a knife or gun. Panel B restricts treatment group to killings of individuals who were unarmed. Estimates are similar when including incidents with unknown weapon type. Standard errors are clustered by Census block group. The sample spans the 2002 to 2010 general elections and treatment is defined by blocks where police killings occurred during the sample period. Red vertical line represents time of treatment.

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.<sup>22</sup>

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, while  $Post_t$  is a 2008 indicator. To improve internal validity, the sample is restricted to blocks in treated block groups. Standard errors are clustered by block group.

As shown in Column 1 of Table III, we find that support for criminal justice reform increased significantly in blocks that experienced a police killing relative to other blocks in

<sup>22</sup>Proposition 66 failed by a 52.7% to 47.3% margin and Proposition 5 by a 59.5% to 40.5% margin.

Table III: Effects on Support for Criminal Justice Reform

<i>DV = Support for Criminal Justice Reform (%)</i>				
	<b>Actual Treatment</b>		<b>Placebo Treatment</b>	
	(1)	(2)	(3)	(4)
Treat x Post	0.053*** (0.019)		-0.004 (0.021)	
x Armed		0.041* (0.021)		0.020 (0.033)
x Unarmed		0.115** (0.053)		-0.019 (0.072)
2004 Mean	0.397		0.422	
Shootings	2004-2008		2008-2010	
Obs.	2,614		1,302	

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

the same neighborhoods. These effects represent a meaningful change in policy preferences – the 5.3 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 roughly three times as large as those for armed killings (0.115 versus 0.041). This is consistent with the differential effects on registration and turnout, and suggests that acts of police violence that appear less justified may provoke more skepticism of the criminal justice system.

Given that this analysis relies on only two elections, we are unable to examine pre-trends. To address lingering validity concerns, we instead conduct a placebo test examining how support for criminal justice reform changed in *future* treatment areas. That is, we estimate Equation 4 for referenda voting in 2004 and 2008 on the sample of neighborhoods that experienced a police killing between 2008 and 2010. The placebo treatment group is

comprised of blocks treated after 2008, while the control group is limited to untreated blocks from those same block groups.

These results are shown in Columns 3 and 4. Notably, placebo treatment estimates are insignificant and very near zero in all cases. This is analogous to support for parallel pre-trends and suggests that our actual treatment estimates reflect the impact of police killings as opposed to other confounds correlated with the 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.

## VII Conclusion

This paper 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 growing evidence of the social consequences of police use of force (Bor et al. 2018; Legewie and Fagan 2019; Ang 2021; González and Prem 2020).

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 members or other close relations 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 likely that concerned residents, witnesses and other, less proximal individuals are responsible for the gains in turnout. This would be consistent with work by Lerman and Weaver (2014a), who find 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 cellphone cameras, public awareness and scrutiny of police violence has likely only grown in recent years. As fatal shootings account for less than one-tenth of one percent of all use of force encounters (Davis, Whyde, and Langton 2018), our findings point to the potentially outsize role that aggressive policing may have on future election outcomes.<sup>23</sup> Indeed, data from the 2020 presidential election suggests that President Biden’s

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<sup>23</sup>Estimating the causal effects of these events is complicated by the fact that “data on lower level uses of

victory was fueled partly by record turnout among young minorities, the same demographic group for which we observe the largest responses to police violence (Tufts College 2020).

Together, these findings highlight the pivotal role that law enforcement and social justice concerns may play in shaping civic engagement (Weitzer and Tuch 2006; Kirk and Papachristos 2011; Tyler, Fagan, and Geller 2014) and provide empirical complement to longstanding concerns about race and policing. In 1968, the Kerner Commission reported on the deep-rooted belief of a “double-standard of justice and protection” underlying civil unrest across the nation. 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 citizens responding to these events by strategically engaging with formal electoral systems in an attempt to reform the institutions responsible for them.

At the same time, it is important to note that our estimates are averages and may fail to capture the wide range of responses that police violence may engender. It is possible that some individuals are disenfranchised by police killings and that these effects are simply outweighed by increased participation by other citizens. Indeed, that our results are driven by new registrations among younger voters suggests that individuals may become inured to state violence over time. Further interrogation of this and other questions is central to understanding the role of law enforcement in democratic engagement and representation.

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force” are “virtually non-existent” (Fryer Jr 2019). Furthermore, the prevalence of less extreme use of force is often directly determined by law enforcement strategy, rendering causal inference more difficult.

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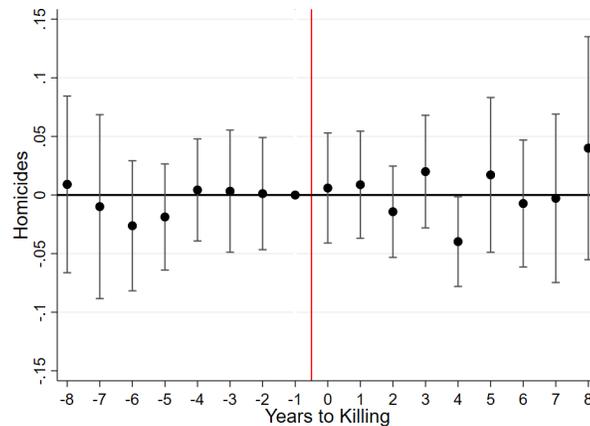
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# Civic Responses to Police Violence

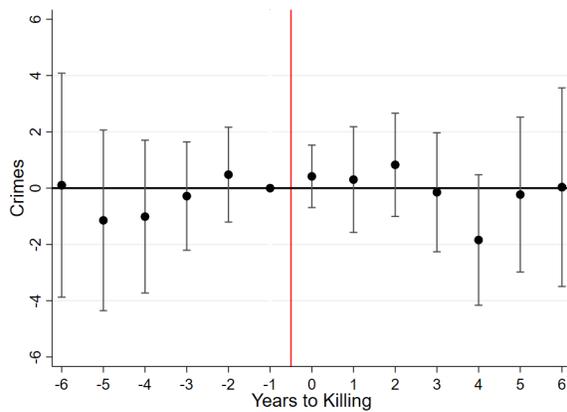
Desmond Ang and John Tebes  
Online Appendix

Figure A.I: Effects on Local Crime and Arrests

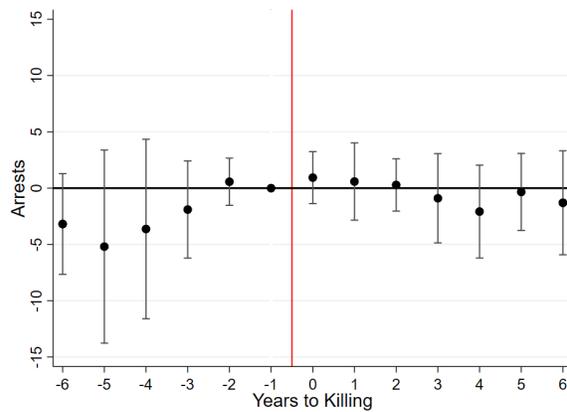
Panel A: Homicides



Panel B: Crimes

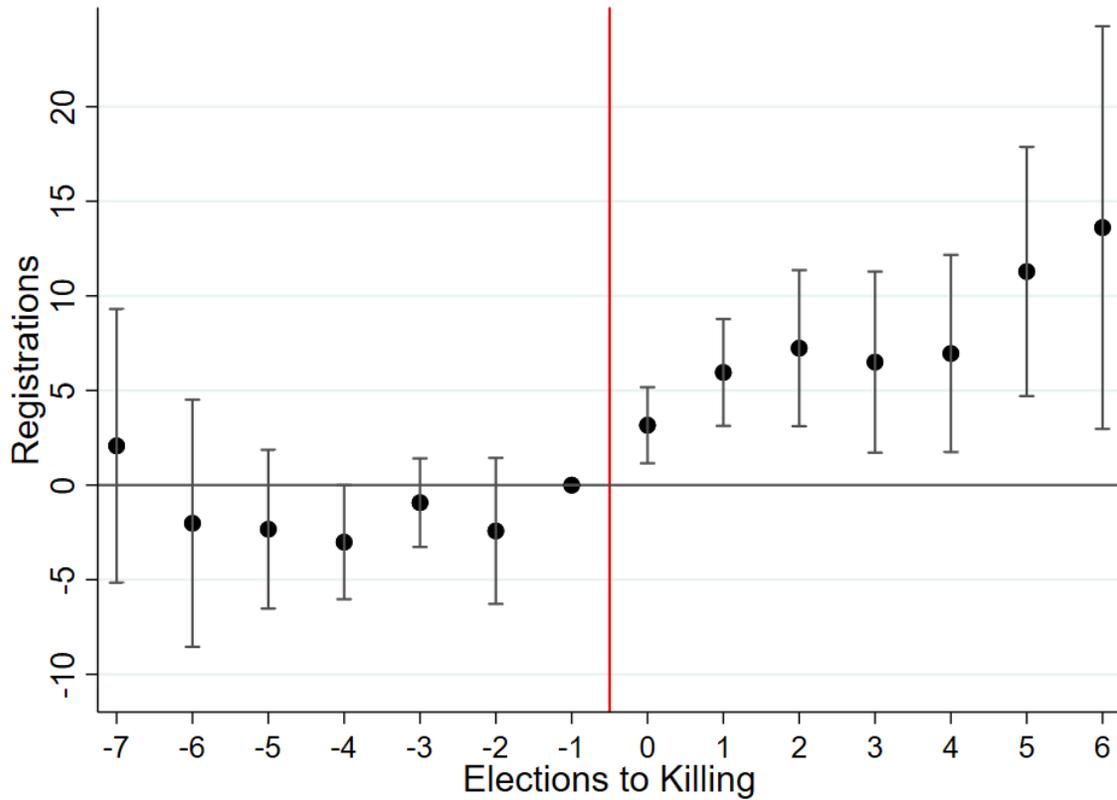


Panel C: Arrests



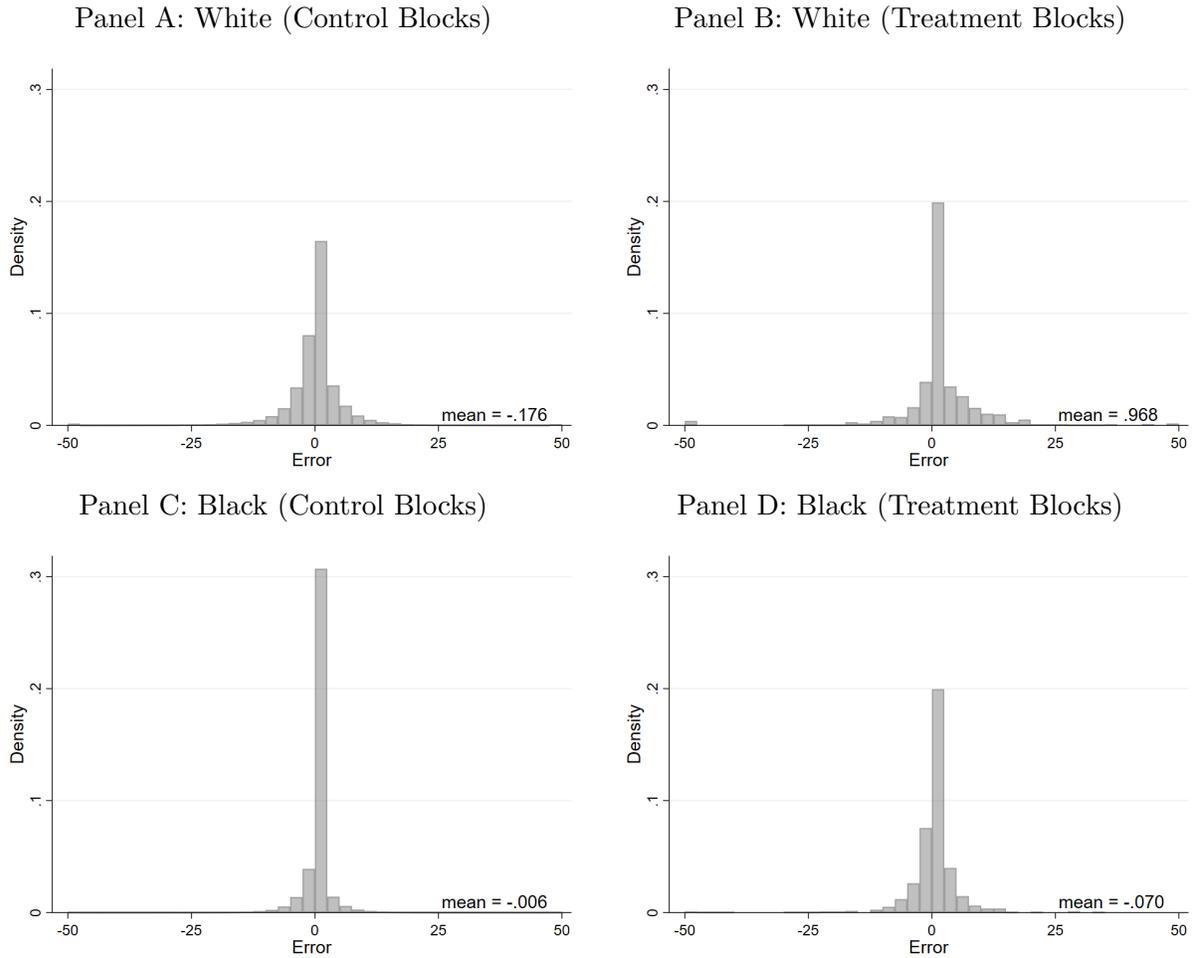
*Notes:* Figure shows treatment estimates and 95 percent confidence intervals from estimation of Equation 2 on homicides, total crimes and arrests. Unit of observation is number of homicides, crimes and arrests in a Census block-year. Standard errors are clustered by Census block group. For Panel A, the sample spans the 2002 to 2010 elections and treatment is defined by blocks where police killings during the sample period. As data on crimes and arrests is only available after 2010, for Panels B and C, the sample spans 2010 to 2016 and treatment is defined by blocks where police killings occurred from 2010 to 2016. Red vertical line represents time of treatment.

Figure A.II: Effects on Voter Registration (2002-2016)



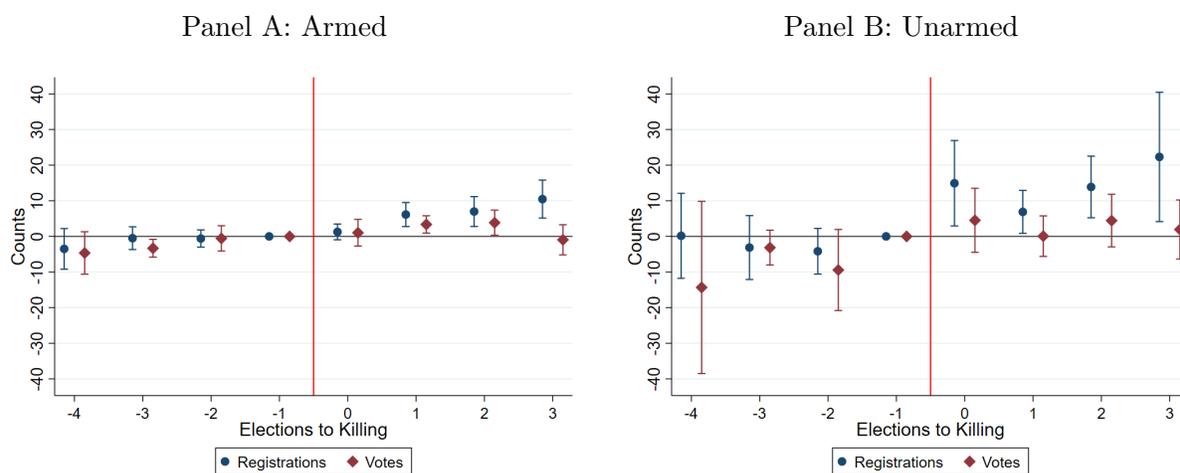
*Notes:* Figure shows treatment estimates and 95 percent confidence intervals from estimation of Equation 2 on registrations (pre-treatment mean = 85.7) on the extended sample. Unit of observation is registrations in a Census block-election. Standard errors are clustered by Census block group. The sample spans the 2002 to 2016 general elections and treatment is defined by blocks where police killings occurred during the sample period. Red vertical line represents time of treatment.

Figure A.III: Validation of Predicted Race Counts



*Notes:* Figure shows histograms validating estimated vote counts by race obtained from Equation 3 against estimates predicted from individual-level voter registration file extracted on February 4, 2011. Each voter's race is predicted from the registration file using surname and address based on the Consumer Financial Protection Bureau's Bayesian Improved Surname Geocoding method (Bureau 2014). A voter is classified as Black (white) if his/her predicted probability of being Black (white) exceeds that of any other race group. Vote counts are then aggregated to Census block. Histograms show the algebraic difference between 2010 estimates from Equation 3 and corresponding voter file estimates, separately for whites/Blacks and Census blocks that did/not experience a police killing during the sample period.

Figure A.IV: Effects by Deceased Weapon without Population Controls



*Notes:* Figure shows treatment estimates and 95 percent confidence intervals from estimation of Equation 2 on registrations (pre-treatment mean = 81.6) and votes (pre-treatment mean = 42.9) excluding population decile by election fixed effects. Unit of observation is registrations/votes in a Census block-election. Panel A restricts treatment group to killings of individuals armed with a knife or gun. Panel B restricts treatment group to killings of individuals who were unarmed. Estimates are similar when including incidents with unknown weapon type. Standard errors are clustered by Census block group. The sample spans the 2002 to 2010 general elections and treatment is defined by blocks where police killings occurred during the sample period. Red vertical line represents time of treatment.