Civic Responses to Police Violence[*]

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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 Blacks and Hispanics 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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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 et al., 2020). This movement was just part of a larger national reckoning, which saw violent counter-protests in Portland, player walk-outs across professional sports leagues, and widespread calls to defund the police. 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).

However, little is known about the political impacts of police violence. On the one hand, recent events suggest that communities may be mobilized to redress perceived injustices. In line with this, research in developing country contexts finds that violent conflict may increase voter turnout (Bellows and Miguel, 2006; Blattman, 2009). On the other hand, acts of police violence may traumatize civilians or undermine their faith in government leading them to disengage from social and civic institutions (Desmond et al., 2016; Ang, 2020). Consistent with this, robust evidence suggests that contact with the criminal justice system can have drastic demobilizing effects (Weaver and Lerman, 2010; Lerman and Weaver, 2014b).

The empirical literature provides little clarity and focuses primarily on the political fallout of high-profile acts of police violence, such as the death of Freddie Gray in Baltimore (White et al., 2018) or the Rodney King riots in Los Angeles (Enos et al., 2019). While over a thousand officer-involved killings occur each year in the United States (Finch et al., 2019), only a small number ever garner local – much less national – attention. Thus, case studies of landmark events may provide limited insight into the vast majority of incidents that go unnoticed by the wider public. They are also unable to disentangle the political consequences of widespread rioting and protests from the impact of the underlying use of force incident.[1]

This paper provides the first causal evidence of the impact of police violence on civic engagement. 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 mobilize local residents to engage with the electoral process.

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[1] In fact, existing literature in political science (Gilens and Page, 2014; Mazumder, 2018; Wasow, 2020) and economics (Collins and Margo, 2007; Madestam et al., 2013) focuses specifically on the effects of protests rather than their antecedent events.

Registrations and votes in exposed blocks increase by roughly 5% in the elections following a police killing. While gains in voter turnout persist for one to two election cycles, registration effects continue more than a decade later. Consistent with the fact that over 80% of sample killings were unmentioned in local newspapers, these effects are highly localized. We find little evidence of spillovers even among neighboring blocks.

The aggregate effects mask significant heterogeneity. Increased civic engagement is driven entirely by Blacks and Hispanics, 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 examine voting patterns in criminal justice referenda. We find that police killings lead to significant increases in local support for propositions seeking to reduce criminal penalties for non-serious offenses. These results suggest that residents may seek to reform the criminal justice system through civic engagement. As corroboration, we provide evidence that civic spillovers are largest following killings of unarmed individuals — those events in which police actions may have seemed the least justifiable.

This paper makes several contributions. First, it adds to a burgeoning literature examining the social spillovers of police violence (Bor et al., 2018; Legewie, 2019). In contrast to research by Ang (2020) showing that police killings cause nearby Black and Hispanic students to drop out of school, our results indicate that these events may also lead underrepresented minorities to further engage with – and affect change through – existing institutions. As fatal shootings account for less than one-tenth of one percent of all use of force encounters (Davis et al., 2018), our findings hint at the potentially outsize role that far more common forms of police violence may have on civic engagement.[2]

More broadly, our findings tie into a large literature exploring how interactions with the criminal justice system affect political participation. While research suggests that even brief contact with the carceral state can significantly reduce voter turnout among incarcerated individuals (White et al., 2019) and their family members (White, 2019), we find that indirect exposure to extreme acts of police aggression – such as witnessing or learning about an

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[2]Estimating the effects of these events is complicated by the fact that "data on lower level uses of force" are "virtually non-existent" (Fryer Jr, 2016). Furthermore, the prevalence of less extreme use of force is often directly determined by law enforcement strategy, rendering causal inference more difficult.

officer-involved killing in the neighborhood – can have the opposite effect. This divergence is consistent with work by Lerman and Weaver (2014a), who find that the impact of law enforcement on civic engagement can vary – even directionally – according to the intensity and nature of police-civilian interactions.

Lastly, this paper provides important context for recent social unrest roiling the nation. Theoretical work suggests that emotional reactions to unfair government treatment may cause individuals to protest (Passarelli and Tabellini, 2017). We find that controversial police killings may drive citizens to the polls for similar reasons. While these results carry important implications for upcoming elections, they also highlight the pivotal role that police may play in shaping institutional trust (Weitzer and Tuch, 2006; Kirk and Papachristos, 2011; Tyler et al., 2014). Given the intensity of policing in many communities of color and the increasing media attention received by officer use of force, greater interrogation of this relationship may be critical to addressing longstanding racial disparities in civic engagement and political representation.

II Data

A Police Killings

Incident-level data on police killings come from the Los Angeles Times Homicide Database and includes 294 police killings that occurred between the 2002 and 2010 general elections. For each incident, the data records name, age and race of the deceased as well as the exact address and date of the event. We supplement this with information on media coverage in local newspapers.[3] For 253 of the 294 incidents, we were also able to determine whether a weapon was recovered from the deceased and if so, what type. 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

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[3]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.

being objectively verifiable and can be found in all available incident reports.

[Table I about here.]

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.[4] Relative to their county 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 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 incidents 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, nowhere near the level of media attention garnered by recent nationally-reported 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 were filed against police in fewer than half a percent of all officer-involved shootings.

B   Voter Registration and Turnout

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.[5] The advantage of these data relative to standard voter registration files is that they capture registration and voting *at the date of each election* allowing for precise measurement of impacts.[6] In addition to total registration

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[4]Race categories are mutually exclusive.

[5]Vote counts are only available through 2010, while registration counts are available through 2018. In the Appendix, we extend the registration sample to assess the persistence of effects.

[6]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.

and vote counts, disaggregated counts by ethnicity (i.e., Hispanic, Asian), party affiliation (i.e., Democrat, Republican or 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 merge data on block-level voting patterns in 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 the criminal penalties for drug offenses, including the reduction of marijuana misdemeanors to infractions, and the expansion of drug treatment and rehabilitation programs.

C   Analysis Sample

Since vote counts are only available until 2010, our main analysis focuses on the 2002-2010 general elections. To improve precision of our estimates, we restrict the sample to blocks with five or more residents aged 18 or older as of the 2000 Census. In the Appendix, we show similar results under alternative sample restrictions.

Panel B of Table I provides a summary of the voter registration data. Blocks that experienced a police killing had, on average, 189 adult residents in 2000, compared to roughly 97 residents in other blocks. Notably, treated blocks are quite similar to untreated blocks in the same 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.

III   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 (2020) 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.

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 (2020), 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 learning 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.[7] Using this metric, we run the following least-squares regression to explore how impacts on registration count dissipate with distance from a killing:

$$y_{b,t} = \delta_b + \delta_{n,t} + \sum_d \alpha_d Distance_d + \delta_{POP_b x ELEC_t} + \epsilon_{b,t} \qquad (1)$$

$y_{b,t}$ is the number of registered voters in block b at election t. δ_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 voting age population in 2000 ($\delta_{POP_b x ELEC_t}$) to account for the possibility of differential population growth between blocks. $\sum_d Distance_d$ 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.[8]

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[7]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.

[8]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 about here.]

We partition shootings first by whether or not the block was directly exposed to a killing, and then into 0.1-mile bins up to 2 miles from the shooting. Figure I plots α_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).[9]

The graphical evidence suggests that impacts are indeed hyper-local. Across specification, 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 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 following base equation on the block-panel data:

$$y_{b,t} = \delta_b + \delta_{n,t} + \sum_{\tau \neq -1} \beta_\tau Shoot_{t,\tau} + \delta_{POP_b x ELEC_t} + \epsilon_{b,t}. \tag{2}$$

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.[10] Neighborhood-time fixed effects ($\delta_{n,t}$) are defined by Census block groups, which average less than one square mile in area. The coefficients of interest (β_τ) then represent the differential change between relative time τ and the last period prior to the police killing among blocks exposed to a killing relative to that same change over time among unexposed blocks in the same neighborhood. Drawing on Bertrand et al. (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.

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[9]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.

[10]Only one block experienced multiple killings over the sample period. Results are robust to excluding that block.

Crime and Migration

One potential threat to identification is that trends in violent crime may affect the prevalence of both civic engagement (Bateson, 2012; Sønderskov et al., 2020) and police killings. 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, Figure A.I of the Appendix estimates Equation 2 on criminal homicides in a block-year. As shown, we find little evidence of differential trends in violent crime before or after police killings, reinforcing the plausible exogeneity of these events.[11]

Another potential threat is selective migration in response to police violence. However, 2006-2010 ACS data suggests that this is unlikely to be a serious concern, as 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).

IV   Effects on Registration and Turnout

A   Main 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 about here.]

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 ($F = 0.40, p = 0.671$). 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 additional registrants in the election immediately following the killing and about 4 registrants within four years. Given that treated blocks contain an average of 80 registrants prior to treatment, these effects represent a meaningful increase of 2.5 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

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[11]Ang (2020) finds similar null results when examining effects on all crimes and arrests from 2010-2016.

A.II expands the sample to include elections through 2016 and reports significant effects on registration more than a decade after a killing.[12]

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 ($F = 0.35, p = 0.702$). After killings, we find a significant, if short-lived, increase of approximately 2.0 votes (5% of the pre-killing mean).

Appendix Table A.I 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, police killings lead to 3.8 more voter registrations and 1.8 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 population of 10-17 year-olds in a block in 2000. Column 5 replaces our 2000 voting population measures with 2010 measures. Column 6 drops Census blocks that experienced multiple killings over the sample period. To demonstrate robustness to sample selection, Column 7 expands the sample to include all Census blocks, even those with less than five adults in 2000. Alternatively, Column 8 restricts the sample to Census blocks with at least 10 registered voters in 2002.

We find similar results across all specifications, with significant treatment 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 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 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

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

dictionary.[13] From these measures, we generate estimates of Black (white) vote and registration counts using the following formula:

$$VoteBlk_{b,t} = (VoteTot_{b,t} - VoteHisp_{b,t} - VoteAsn_{b,t}) \times$$

$$\left(\frac{\%Blk_{b,2010} \times \%VoteBlk_t}{(\%Blk_{b,2010} \times \%VoteBlk_t) + (\%Wht_{b,2010} \times \%VoteWht_t) + (\%Oth_{b,2010} \times \%VoteOth_t)} \right)$$
(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 turnout rate in California during election t as estimated by the CPS Voting and Registration Supplement ($\%VoteBlk_t$, $\%VoteWht_t$, and $\%VoteOth_t$).[14] Essentially, we weight non-Hispanic, non-Asian votes (registrations) in a given block-election by each racial group's predicted vote (registration) share relative to the other remaining racial groups.[15]

[Figure III about here.]

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 8-11% increase over the pre-killing mean (20.6 registrations and 9.0 votes). We find similar, if proportionally smaller, responses among Hispanics with increases of 1.5 registrations (5% of mean) and 1.0 votes (7% of mean). In contrast, we find no significant impact on White and Asian participation, with point estimates representing less than 2% of the mean.[16]

The pattern of effects is consistent with a host of evidence documenting large racial differences in perceptions of law enforcement. Researchers have found that race is the single

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

[14]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.

[15]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.

[16]Appendix Figure A.IV explores heterogeneity across race of the deceased. We find suggestive evidence that racial concordance matters with larger point estimates for same race killings. Figure A.IV also compares effects between presidential and midterm elections and between killings that occurred less/more than a year from the next election. We find little evidence of differential effects in either case.

strongest predictor of trust in police (Taylor et al., 2001) and Blacks and Hispanics are far more likely than others to believe that use of force is excessive, unjustified or a serious social issue (Weitzer and Tuch, 2002; AP-NORC, 2015; Davis et al., 2018).

Voter Age, Years Registered and Political Affiliation

In Panel B, 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 = 25.7), while votes increase by roughly 11% (pre-killing mean = 9.0). In contrast, treatment estimates for individuals over 35 years old are statistically insignificant and small in magnitude (less than 5% of the pre-killing mean). Consistent with this, Panel C demonstrates that increases in turnout come entirely from individuals who registered within 3 years of a given election.[17]

In Panel D, 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).[18]

Media Coverage and Incident Location

The left pane of Panel E compares effects for killings that were and were not mentioned in local newspapers. We find suggestive evidence that registration effects may be larger for killings that were covered in the media than for those that were not. This may help to explain the discrepancy between recent responses to high-profile police killings and the highly-localized effects we observe here. However, this is only suggestive as estimates are not statistically different from each other and there is limited variation in media intensity across sample killings.[19]

The right pane of Panel E compares killings that occurred near the home of the deceased against those that occurred in other areas.[20] We find similar estimates across incident

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[17]That point estimates for registration are near zero for longer-registered voters provides evidence that the registration effects are not driven by differential migration (i.e. previously registered voters moving into treatment areas).

[18]For example, 27% of Democrats versus 74% of Republicans believe police do a good job "using the right amount of force."

[19]It is also important to note that media coverage may itself be endogeneous and a function of community reactions to an event.

[20]We infer whether a killing occurred in or near the person's home based on incident descriptions provided in DA reports.

location. This suggests that gains in participation are unlikely to be driven solely by friends, family or loved ones of the deceased.[21] Rather, they may instead be the result of increased engagement among nearby residents who are concerned about procedural fairness (Sunshine and Tyler, 2003) or "feel at risk" of meeting a similar fate (Zimring, 2017).

V Mechanisms

While our results indicate that police violence may increase local civic participation, interpretation of these effects 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 system.

To disentangle mechanisms, we examine differential effects based on the type of weapon possessed by the deceased. If voters are motivated by heightened concerns about crime, we would expect larger effects following police killings of armed suspects, which may have resulted in the the most gunfire or involved individuals who posed the greatest danger to the community. If instead voters are mobilized by perceptions of government misconduct, then we would expect the largest effects to stem from killings of unarmed individuals.

Results are shown in Panel F of Figure III.[22] Notably, estimates are small or insignificant for police killings of individuals armed with a knife or a gun and for incidents where we do not have weapon information. However, police killings of unarmed individuals lead to large increases in participation of 11.5 registrations (14% of the pre-killing mean, $p = 0.025$) and 4.7 votes (12% of the pre-killing mean, $p = 0.041$). Though coefficients are only different from each other at the 10% level due to limited power ($p = 0.087$ comparing β_{reg} for unarmed killings versus all other killings combined), the stark pattern of effects suggest that voters are responding to the perceived "reasonableness" of officer-involved actions as much as to the violence itself (Braga et al., 2014).

As corroboration, we test whether police killings affected support for criminal justice reforms using referenda data. 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 these propositions were narrowly defeated, they provide a local measure of policy preferences and potential insight into beliefs about

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[21]As further evidence, work by Hobbs et al. (2014) suggests that family deaths *decrease* voter turnout.

[22]Full event studies providing evidence of parallel pre-trends are included in Appendix Figure A.V.

law enforcement.[23]

We estimate the following difference-in-differences model:

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

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 only to blocks in treated block groups. Standard errors are clustered by block group.

[Table II about here.]

As shown in Column 1 of Table II, we find that support for criminal justice reform increased significantly in blocks that experienced a police killing relative to other blocks in the same neighborhoods. Column 2 disaggregates the treatment effect across armed and unarmed killings and finds marginally significant increases in support for criminal justice reform across both types of events. However, point estimates for unarmed killings are three times as large as those for armed killings (0.133 versus 0.044). This is consistent with the differential effects on turnout and registration and suggests that acts of police violence that appear less "reasonable" provoke more skepticism of the criminal justice system.

Given that only two relevant referenda exist during our sample period, 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 on 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 in 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 among future treaters and suggests that our actual treatment estimates reflect the impact of police killings as opposed to other confounds.

Taken together, these results indicate that the effects of police violence on civic engagement 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 partially, through the electoral process.

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[23]Proposition 66 failed by a 52.7% to 47.3% margin and Proposition 5 by a 59.5% to 40.5% margin.

VI 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 blacks and Hispanics. 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 sequelae of police use of force (Bor et al., 2018; Legewie, 2019; Ang, 2020).

In doing so, this paper provides empirical complement to a long history of concerns around race and policing. In 1968, the Kerner Commission reported on the deep-rooted belief among many minorities of a "double-standard of justice and protection." Our findings suggest that such beliefs continue to permeate communities of color today and that acts of police violence may exacerbate those concerns. While the existing literature has focused primarily on the relationship between law enforcement and crime (Evans and Owens, 2007; Chalfin and McCrary, 2018; Mello, 2019), this paper highlights the need for further research exploring the multi-faceted impact of police on other aspects of community well-being. Such an accounting is critical both to the design of law enforcement policy and to the welfare of underserved neighborhoods.

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Figure I: Effects by Distance From Police Killing
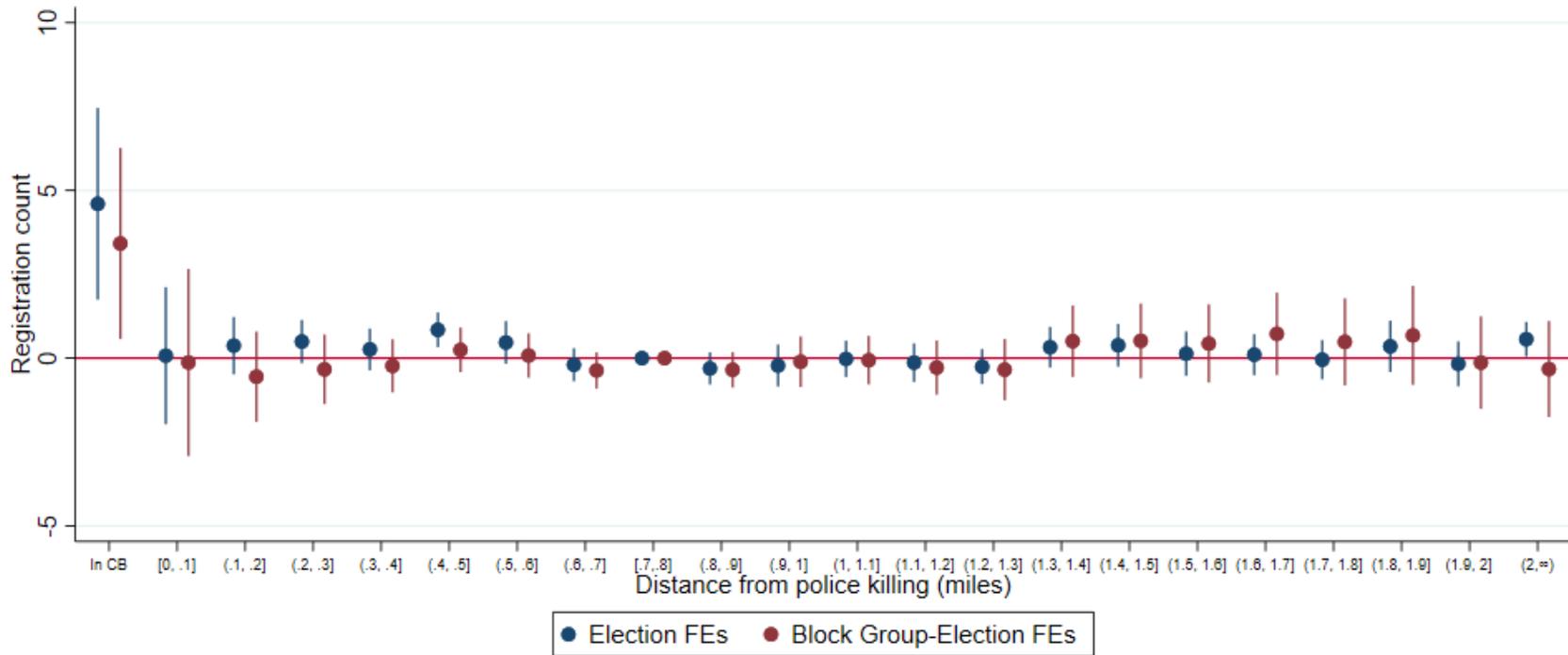
Notes: Figure reports coefficients from regressions of registration count on mutually-exclusive indicators describing the distance of a given Census block to a police killing. The sample spans 2002 to 2010 general elections. Indicators track a census block's (cumulative) minimum distance to shootings that occurred prior to a given election. The first indicator – "In CB" – describes whether or not the killing occurred inside the given census block. The rest partition Census blocks not directly exposed to a killing into 0.1-mile bins. We set the reference group to blocks between 0.7 and 0.8. We include Census block and population-decile-by-election fixed effects in all specifications. For red dots, we additionally include Census block group (CBG) by election fixed effects. In the CBG specification, distance indicators are set to 0 within CBG so that neighboring blocks are not mechanically biased downwards through the neighborhood-election fixed effects. All standard errors are clustered at the CBG-level.

Figure II: Effects on Civic Engagement
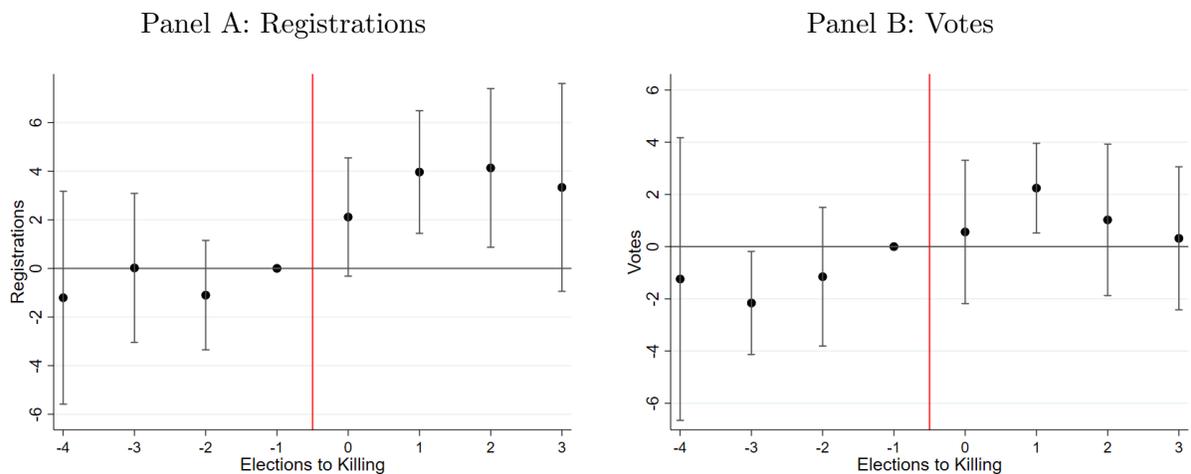
Panel A: Registrations   Panel B: Votes

Notes: Figure shows treatment estimates and 95 percent confidence intervals from estimation of Equation 2 on registrations (pre-treatment mean = 79.6) and votes (pre-treatment mean = 39.1). 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.

Figure III: Heterogeneous Effects
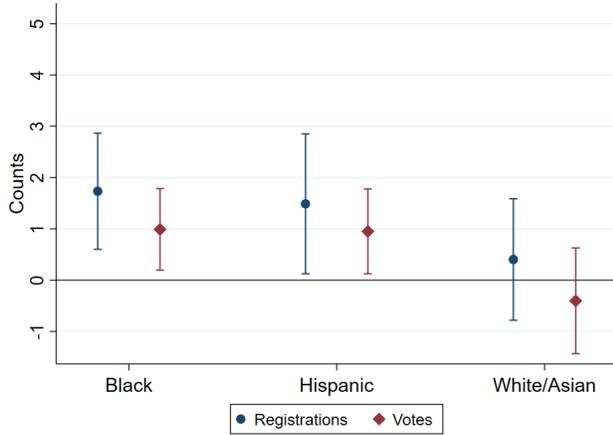
Panel A: Voter Race · Panel B: Age
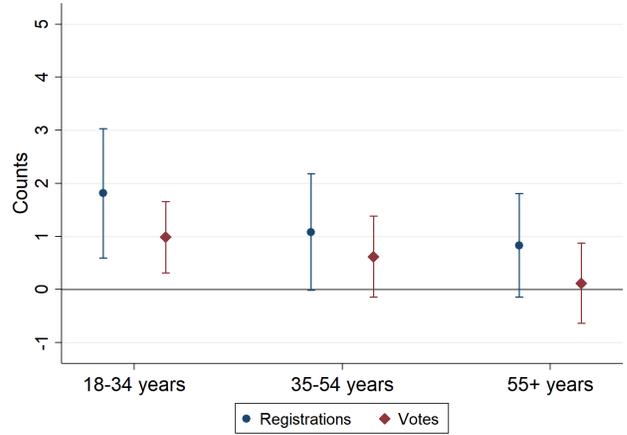
Panel C: Years Registered · Panel D: Party Affiliation
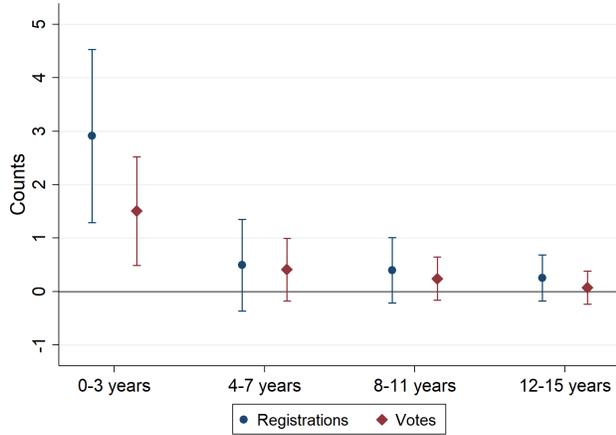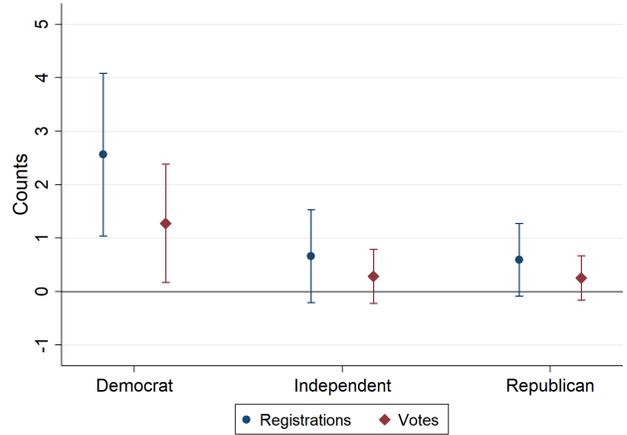
Panel E: Media Coverage and Location · Panel F: Deceased Weapon
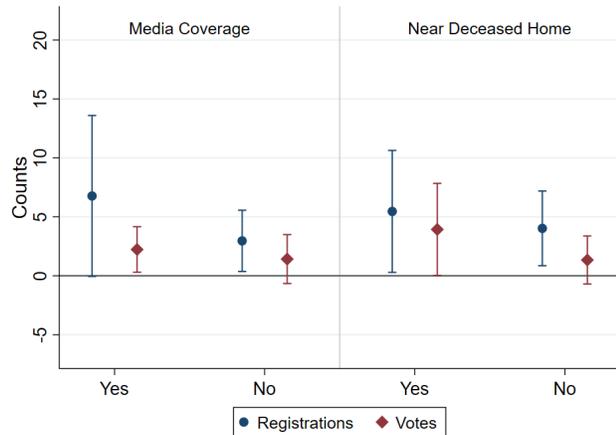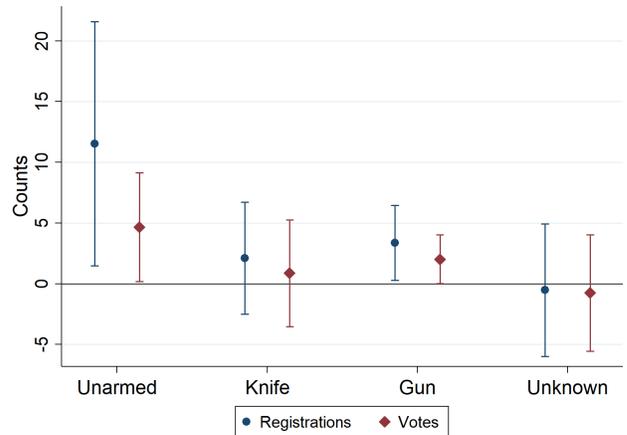
Notes: Panels A-D report coefficients from separate regressions of registrations/votes for a given voter group (i.e. by race in Panel A, age in Panel B, years registered in Panel C and party affiliation in Panel D) on a single post-treatment indicator. Panels E and F report coefficients from a single regression of total registrations/votes on separate post-treatment indicators for each incident type (i.e., with/without media coverage in left pane of Panel E, near/far from the deceased's home in right pane of Panel E, and across deceased weapon 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.

Table I: Summary Statistics

| | Panel A: Police Killings | | | | Panel B: Registration and Voting Sample | | | |
| | | | | | | | No Killing | |
	All	Black/ Hispanic	White/ Asian		All	Killing	Treat Blk Grp	Ctrl Blk Grp
Deceased Demographics				**Demographics (2000 Census)**				
Black	0.29	0.35	0.00	*18+ Count*	97.38	189.03	97.30	96.99
Hispanic	0.53	0.65	0.00	White	0.39	0.16	0.21	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	0.51	0.37
Male	0.96	0.97	0.92	Asian	0.12	0.09	0.10	0.12
Age	30.34	28.98	36.47	Other	0.02	0.02	0.02	0.03
Newspaper Mentions				**Voter Registration (2002)**				
Any	0.18	0.19	0.17	*Reg. Count*	54.96	79.24	46.78	55.19
Total	0.70	0.77	0.40	White/Asian	0.59	0.34	0.38	0.60
Median (if any)	2.00	2.00	1.00	Black	0.13	0.28	0.26	0.13
				Hispanic	0.28	0.39	0.36	0.28
				Ages 18-34	0.26	0.31	0.30	0.25
Weapon Type				Ages 35-54	0.40	0.39	0.39	0.41
Unarmed	0.17	0.15	0.28	Democrat	0.53	0.62	0.61	0.52
Knife	0.27	0.24	0.38	Republican	0.28	0.17	0.20	0.28
Gun	0.56	0.61	0.33	Reg. < 4 yrs	0.36	0.42	0.39	0.35
				Vote Count	24.04	29.21	17.97	24.27
Incidents	294	240	53	Blocks	70,215	288	2,808	67,119

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 16% of incidents.

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.

Table II: Effects on Support for Criminal Justice Reform

	DV = Support for Criminal Justice Reform (%)			
	Actual Treatment		Placebo Treatment	
	(1)	(2)	(3)	(4)
Treat x Post	0.057**		0.000	
	(0.029)		(0.030)	
x Armed		0.043$^\wedge$		0.024
		(0.031)		(0.046)
x Unarmed		0.133*		-0.018
		(0.078)		(0.102)
2004 Mean	0.403		0.419	
Shootings	2004-2008		2008-2010	
Obs.	2,736		1,322	
Blocks	1,368		661	

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 armed (i.e., knife, gun or unknown) and unarmed indicators. All standard errors are clustered at the block group level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$, and $^\wedge$ $p < 0.20$.

# Civic Responses to Police Violence
Desmond Ang and John Tebes
Online Appendix

Figure A.I: Effects on Homicides
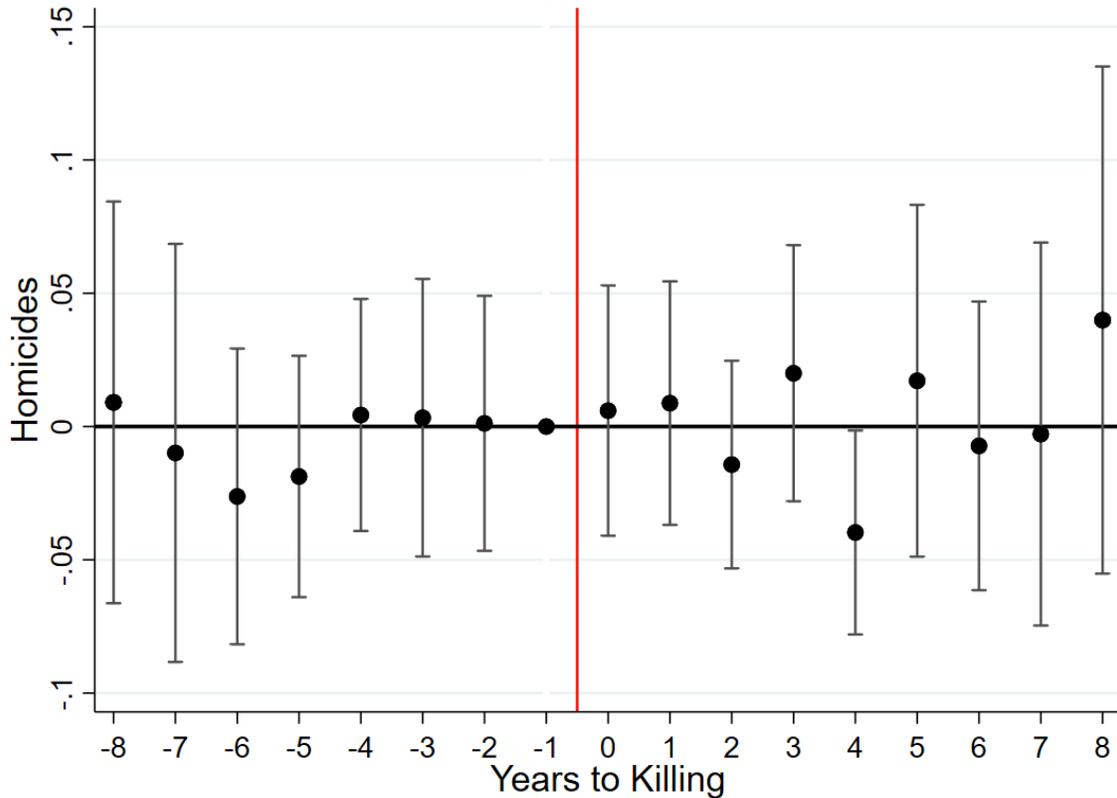
Notes: Figure shows treatment estimates and 95 percent confidence intervals from estimation of Equation 2 on homicides. Unit of observation is number of homicides in a Census block-year. Standard errors are clustered by Census block group. The sample spans the 2002 to 2010 elections and treatment is defined by blocks where police killings occurred during the sample period. Red vertical line represents time of treatment.

Figure A.II: Effects on Voter Registration (2002-2016)
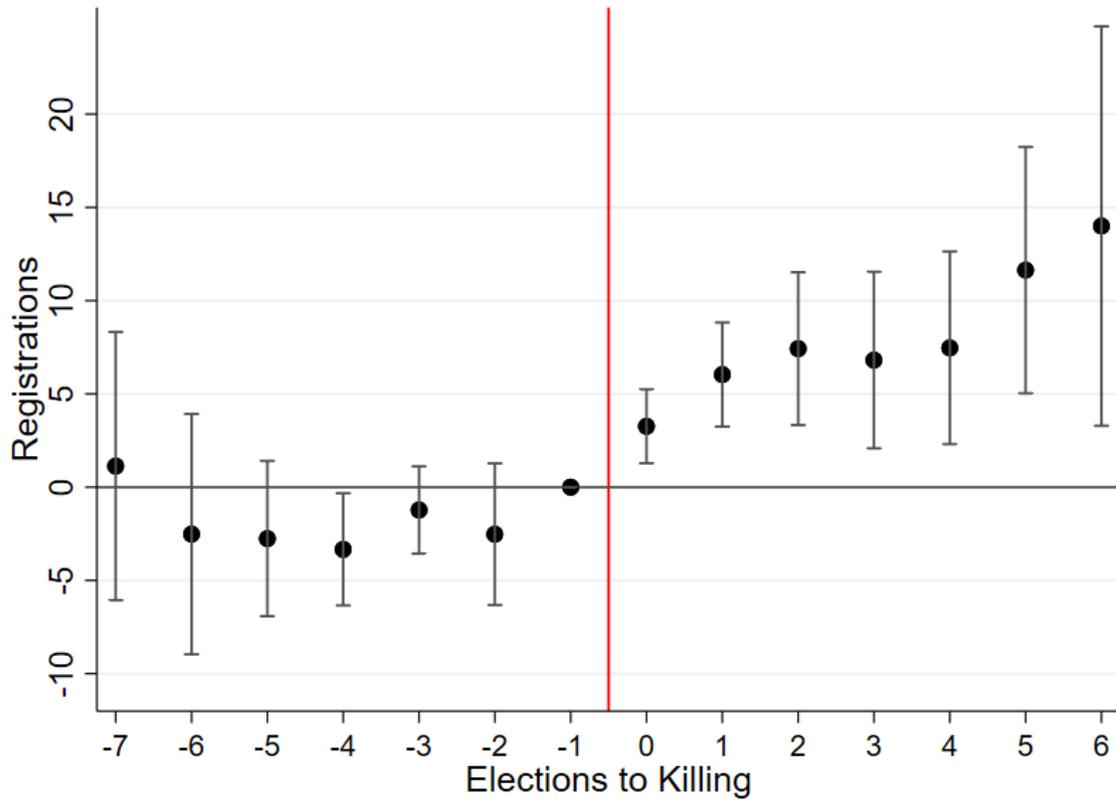
Notes: Figure shows treatment estimates and 95 percent confidence intervals from estimation of Equation 2 on registrations (pre-treatment mean = 85.7) on the extended sample. Unit of observation is registrations in a Census block-election. Standard errors are clustered by Census block group. The sample 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

Panel A: White (Control Blocks)   Panel B: White (Treatment Blocks)

Panel C: Black (Control Blocks)   Panel D: Black (Treatment Blocks)
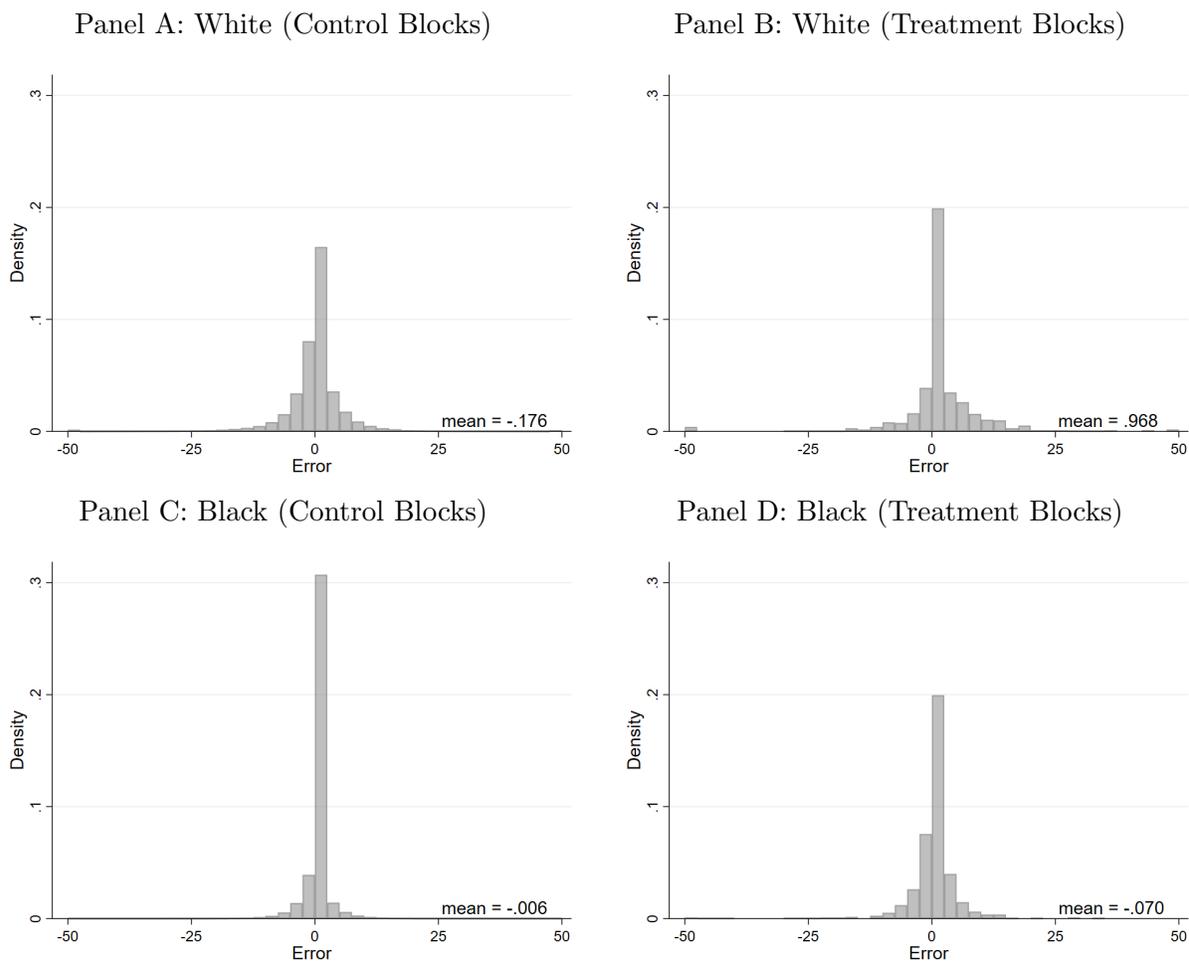
Notes: Figure shows histograms validating estimated vote counts by race obtained from Equation 3 against estimates predicted from individual-level voter registration file extracted on February 4, 2011. Each voter's race is predicted from the registration file using surname and address based on the Consumer 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: Additional Heterogeneity

Panel A: Deceased Race

Panel B: Election Type

Panel C: Incident Timing
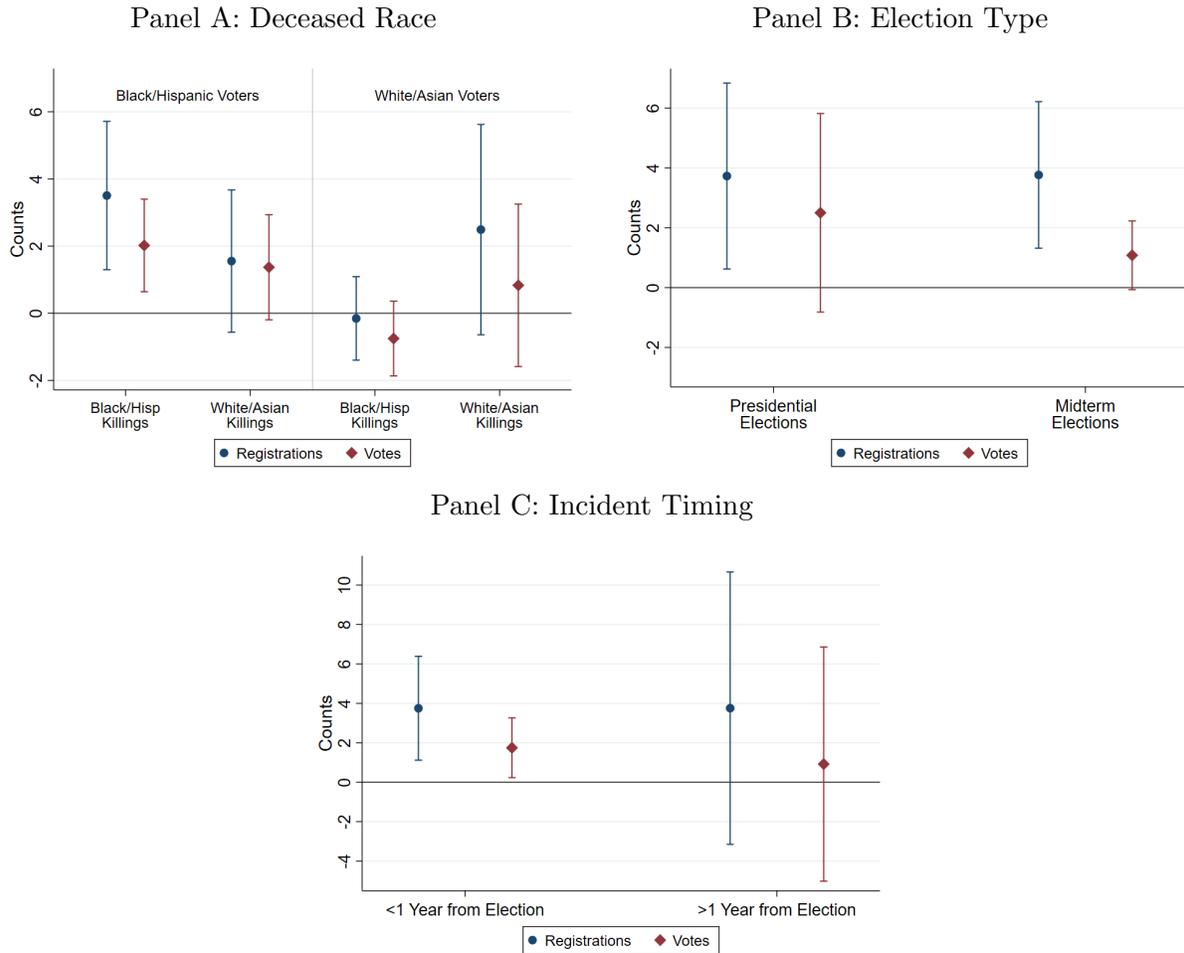
Notes: Panel A reports coefficients from regression of registration/votes on separate post-treatment dummies for Black/Hispanic killings and white/Asian killings. Left pane examines effects on Black/Hispanic participation, right pane on white/Asian participation. Panel B reports coefficients for separate post-treatment dummies for presidential and midterm elections. Panel C reports coefficients for separate post-treatment dummies for killings that occurred less than 1 year from the next general election and for killings that occurred between 1 and 2 years from the next general election. 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.

Figure A.V: Effects by Deceased Weapon (Event Study)
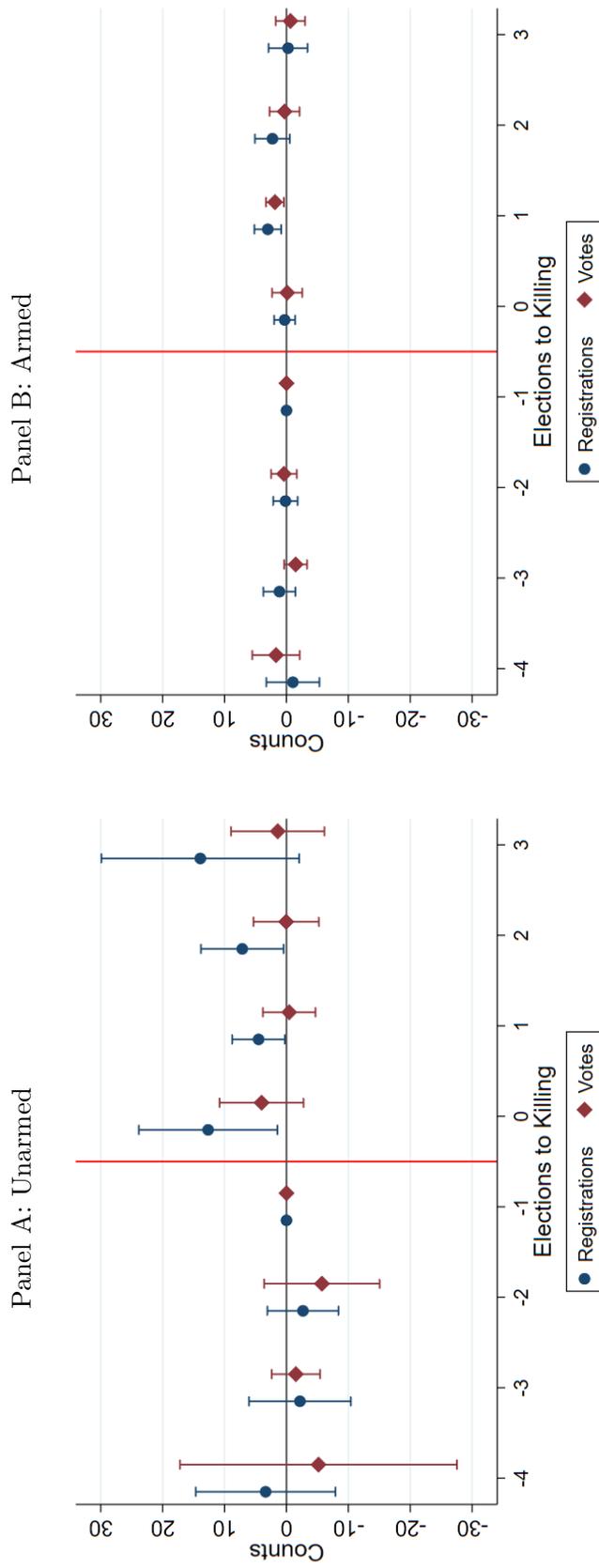
Panel A: Unarmed

Panel B: Armed

Notes: Figure shows treatment estimates and 95 percent confidence intervals from estimation of Equation 2 on registrations (pre-treatment mean = 79.6) and votes (pre-treatment mean = 39.1). Unit of observation is registrations/votes in a Census block-election. Panel A restricts treatment group to killings of individuals who were unarmed. Panel B restricts treatment group to killings of individuals armed with a knife or gun or whose weapon was unknown. 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.

Table A.I: Effects on Civic Engagement: Alternative Specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel A: Registration				
Treat x Post	3.820***	3.823***	3.814***	3.764***	3.311***	3.682***	3.202***	3.396***
	(1.291)	(1.291)	(1.291)	(1.292)	(1.275)	(1.289)	(1.079)	(1.313)
Pre-Treat Mean	79.59	79.59	79.59	79.59	79.59	78.84	68.57	82.79
				Panel B: Votes				
Treat x Post	1.815**	1.817**	1.787**	1.792**	1.498*	1.820**	1.394*	1.638*
	(0.869)	(0.869)	(0.867)	(0.869)	(0.846)	(0.872)	(0.744)	(0.877)
Pre-Treat Mean	39.05	39.05	39.05	39.05	39.05	38.67	33.60	40.61
Model	Main	Homicide Controls	% Minority x Elect. FE	Pop. Aged 10-17 Quin. x Elect. FE	Use 2010 Census Pop.	Drop Multi-Treaters	Full Sample	Reg. in 2002 ≥ 10
Obs.	351,075	351,075	351,075	351,075	351,075	351,070	547,910	315,020

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 replaces 2000 voting population estimates with 2010 voting population measures. Column 6 drops the single Census block that experienced more than one police killing during the sample period. Column 7 includes all Census blocks, even those with less than 5 people aged 18 or older in 2000. Column 8 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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$, and $^\wedge$ $p < 0.15$.