

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 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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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, the impact of police violence on local political participation is theoretically ambiguous. 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; Insler et al., 2019; Ang, 2021). 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 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.

¹In fact, existing work 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.

We find that police killings mobilize local residents to engage with the electoral process. Registrations and votes in exposed 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. 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 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 support for more intensive policing (Cummins, 2009), we would expect larger effects for killings of armed individuals, who may have posed the greatest threats to their communities. 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 voting patterns for criminal justice referenda and find that police killings significantly increase support for propositions designed to reduce criminal penalties for non-serious offenses. Together, these results suggest that residents seek to reform the criminal justice system through civic engagement.

This paper makes several contributions. First, it complements a burgeoning literature on the social spillovers of police violence, much of which demonstrates negative effects on psychological well-being (Bor et al., 2018), academic achievement (Legewie and Fagan, 2019), and institutional engagement. For example, Ang (2021) finds that exposure to police killings causes Black and Hispanic students to drop out of high school. In other domains, police violence has been shown to reduce student protests in Chile (González and Prem, 2020), while evidence in the U.S. finds negative (Desmond et al., 2016) or null effects (Cohen et al., 2019; Zoorob, 2020) on local 911 calls. In contrast, we find that police violence galvanizes under-represented groups to further engage with the electoral institutions, suggesting a strategic political response aimed at addressing concerns with law enforcement agencies.

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 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. As fatal shootings account for less than one-tenth of one percent of all use of force encounters (Davis et al., 2018), our findings suggest the potentially outsized role that far more common forms of aggressive policing may have on civic engagement.²

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. These findings are echoed by recent voting patterns. Data from the 2020 presidential election suggest that President Biden’s victory was fueled in part by increased participation among young minority voters, the same demographic group where we observe the largest responses to police violence (Tufts College, 2020). Together, the evidence highlights the pivotal role that law enforcement and social justice concerns may play in shaping institutional trust and engagement (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 use of force incidents, greater interrogation of this relationship may have important implications for future elections and for addressing longstanding racial disparities in 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.³

²Estimating 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.

³In a handful of cases, multiple individuals were killed in a single incident. The total number of distinct incidents is 286.

For each killing, 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.⁴ 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.

[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.⁵ 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 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, 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%).

⁴We 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.

⁵Race categories are mutually exclusive.

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 2010.⁶ 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.⁷ In addition to total registration 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, 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.

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

⁶Vote 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.

⁷Due 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.

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 (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.

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 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.⁸ 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 . 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 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 .⁹

[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).¹⁰

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 employ an event study model. Drawing on the distance analysis, treatment is defined as Census blocks that experienced a police killing

⁸We 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.

⁹For 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.

¹⁰In 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.

and neighborhood is defined at the Census block group-level. We estimate the following base equation on the block-panel data:

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

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.¹¹ 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.

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, Panel A of Figure A.I of the Appendix estimates Equation 2 on criminal homicides in a block-year. Given that geo-coded data on *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. Given that 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 migration 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 education data from Los Angeles, Ang (2021) finds little effect of exposure to police killings on school

¹¹Only 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.

out-transfers among nearby students. In fact, point estimates are near zero and positive, suggesting – if 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 prior to 2010 returns a precise zero estimate ($\beta = 0.006$, p-value=0.78).

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 ($F = 0.33, p = 0.806$). 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.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 ($F = 1.09, p = 0.350$). After killings, we find a significant, if short-lived, increase of approximately 2 votes (5% of the pre-killing mean).

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

[Table II about here.]

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, 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 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.¹³ 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 8 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 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 dictionary.¹⁴ From these measures, we generate estimates of Black (white) vote and registration counts using the following formula:

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

¹⁴The 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).

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

where $VoteTot_{b,t}$, $VoteHisp_{b,t}$ and $VoteAsn_{b,t}$ are the number of total votes, Hispanic votes and Asian votes in block b at election t and $\%Blk_{b,2010}$, $\%Whit_{b,2010}$, 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 ($\%VoteBlk_t$, $\%VoteWhit_t$, and $\%VoteOth_t$).¹⁵ 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.¹⁶

[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 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.

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 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 issue (Weitzer and Tuch, 2002; AP-NORC, 2015; Davis et al., 2018).

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

¹⁶To 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.

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 = 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 C 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 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).¹⁷

Media Coverage, Incident Location and Deceased Race

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.¹⁸

The right pane of Panel E compares killings that occurred near the home of the deceased against those that occurred in other areas.¹⁹ We find similar estimates across incident location. This suggests that gains in participation are unlikely to be driven solely by friends, family or loved ones of the deceased.²⁰ Rather, they may instead be the result of increased

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

¹⁸It is also important to note that media coverage may itself be endogenous and a function of community reactions to an event.

¹⁹We infer whether a killing occurred in or near the person’s home based on incident descriptions provided in DA reports.

²⁰As further evidence, work by Hobbs et al. (2014) suggests that family deaths *decrease* voter turnout.

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).

Finally, Panel F explores heterogeneity across race of the deceased. 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 large, if noisy, estimates for killings of white and Asian individuals, but near zero estimates for killings of other race individuals.

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 whether the deceased possessed a weapon. If voters are motivated by heightened concerns about crime, we would expect larger effects following police killings of armed suspects, which may have involved more gunfire or individuals who posed greater 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.

[Figure IV about here.]

Full event study results are displayed in Figure IV. Appendix Figure A.IV shows similar results when excluding population controls. Notably, estimates are small or insignificant for police killings of individuals armed with a knife or a gun, with average treatment effects of 2.7 registrations ($p = 0.042$) and 1.5 votes ($p = 0.133$).²¹ However, police killings of unarmed individuals lead to large increases in participation. 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.²² That

²¹Average treatment effects are derived from regressions with a single post-treatment indicator in place of the full set of leads and lags.

²²Similar relative effects exist when including the 14% of sample killings with unknown weapon status.

police killings of unarmed individuals generate such large relative spillovers suggests that voters are responding to the perceived “reasonableness” of officer-involved actions as much as to the violence itself.

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 law enforcement.²³

We estimate the following difference-in-differences model:

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

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

[Table III about here.]

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 the same neighborhoods. The 5.3 percentage point increase in pro-reform ballot share is 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 “reasonable” 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 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.

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

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.

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 and Fagan, 2019; Ang, 2021).

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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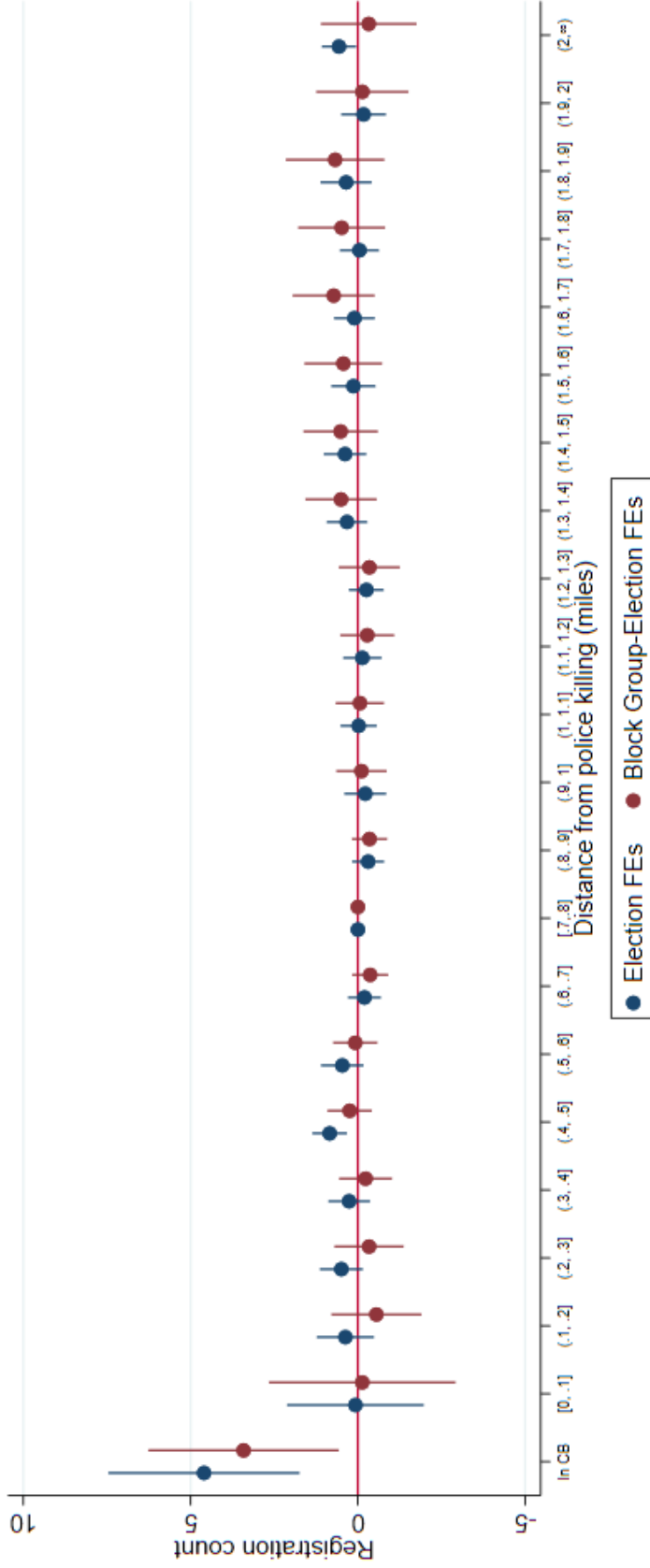
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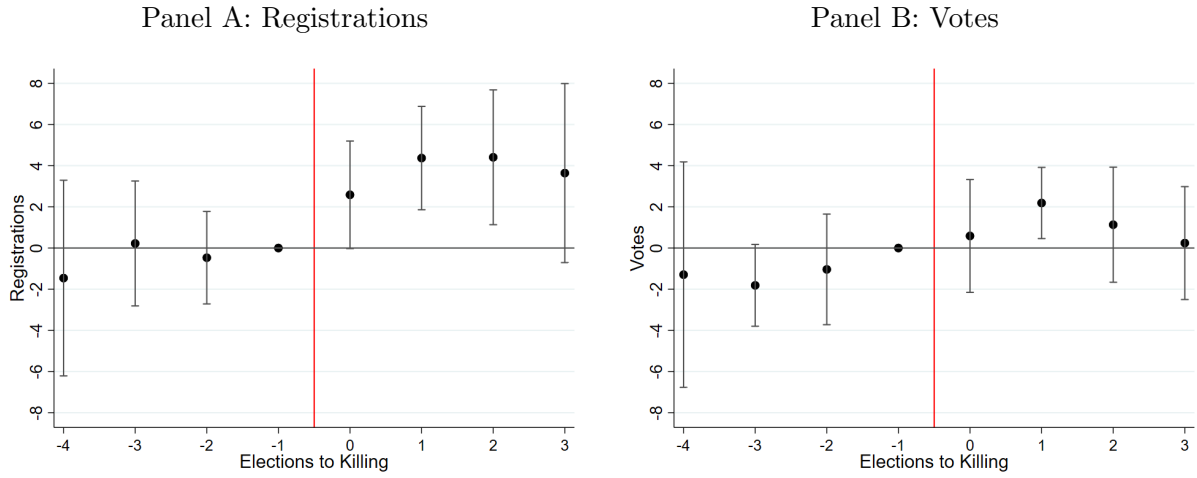
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Figure I: Effects by Distance From Police Killing



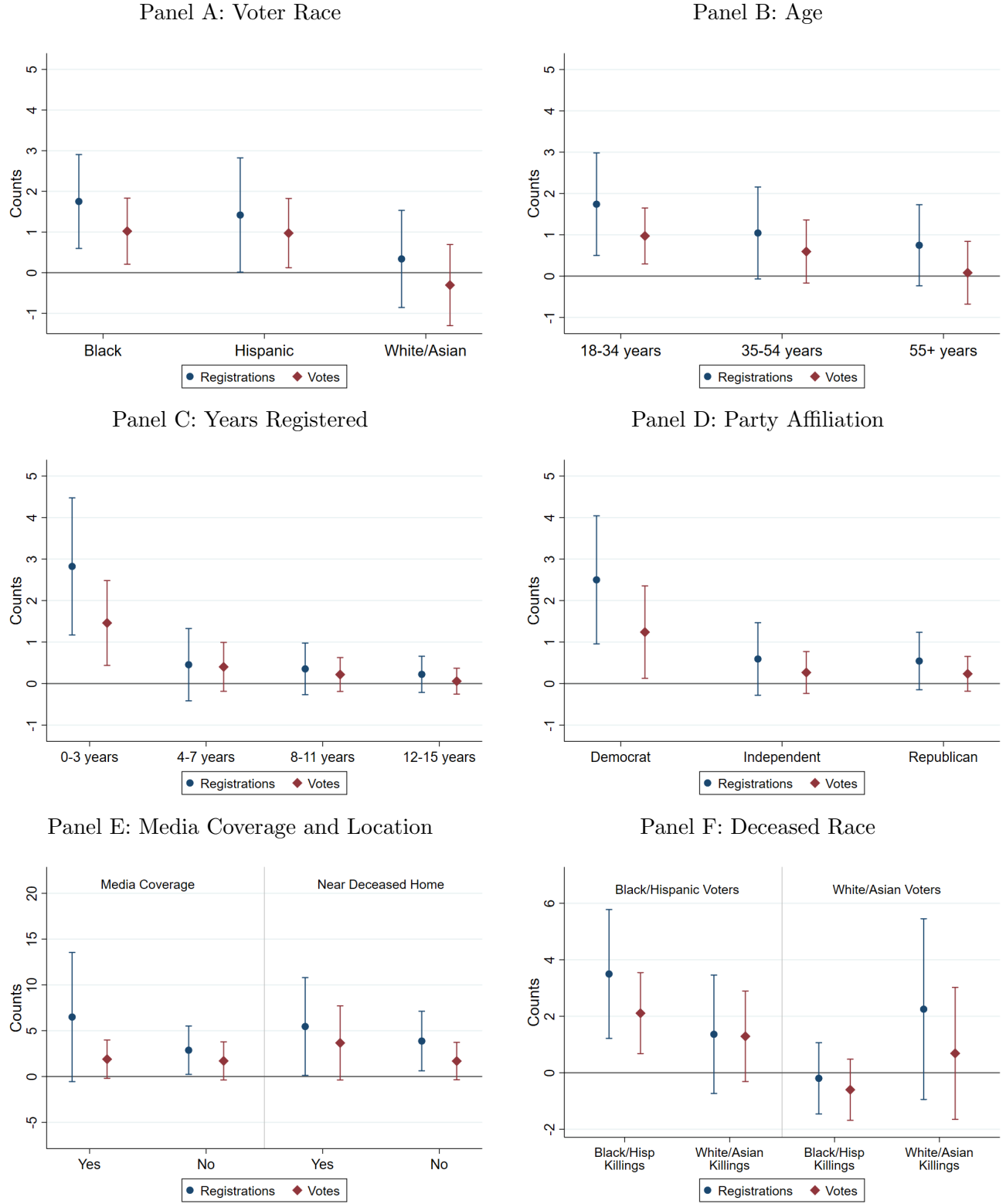
Notes: Figure reports coefficients from regressions of registration count on mutually-exclusive indicators describing the distance of a given Census block to a police killing. The sample spans 2002 to 2010 general elections. Indicators track a census block's (cumulative) minimum distance to shootings 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



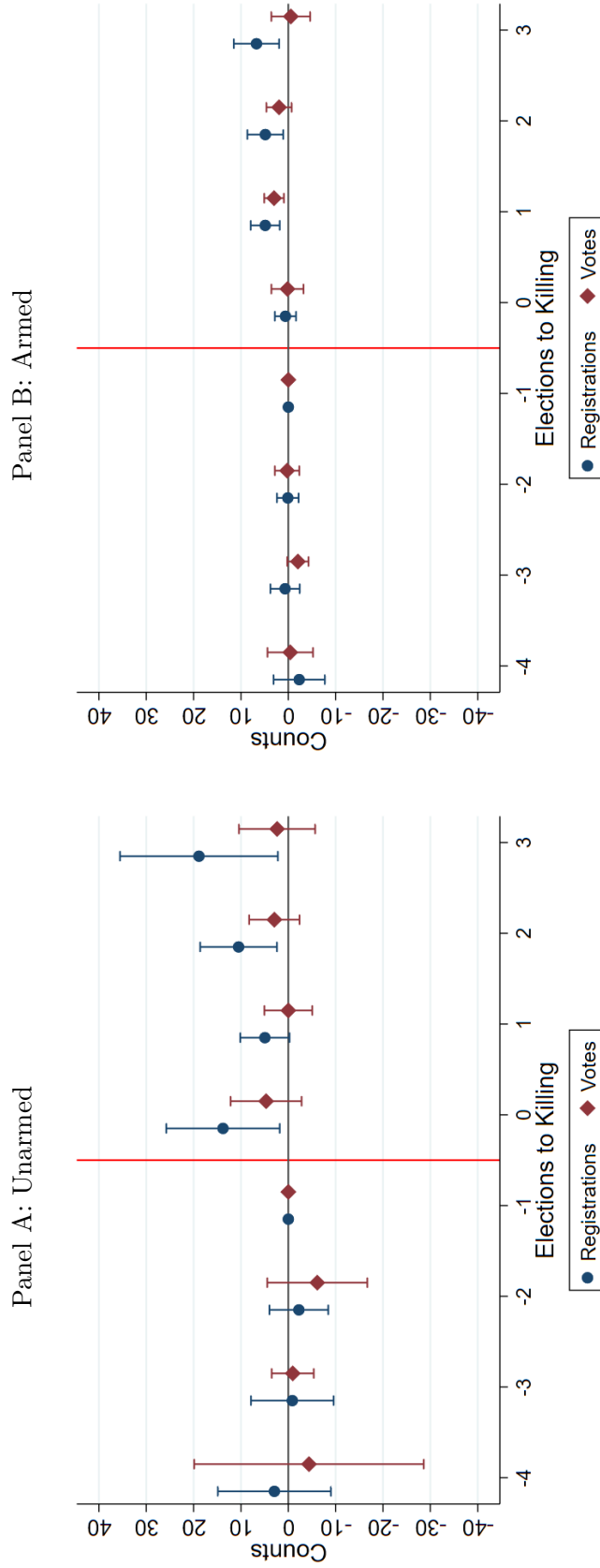
Notes: Figure shows treatment estimates and 95 percent confidence intervals from estimation of Equation 2 on registrations (pre-treatment mean = 81.6) and votes (pre-treatment mean = 42.9). Unit of observation is registrations/votes in a Census block-election. Standard errors are clustered by Census 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.

Figure III: Heterogeneous Effects



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. Each pane of Panels E and F reports coefficients from a single regression with 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 race 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.

Figure IV: 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 who were unarmed. Panel B restricts treatment group to killings of individuals armed with a knife or gun. 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.

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
<i>Deceased Demographics</i>				<i>Demographics (2000 Census)</i>				
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
<i>Newspaper Mentions</i>				<i>Voter Registration (2002)</i>				
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
<i>Weapon Type</i>				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.

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. 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 III: Effects on Support for Criminal Justice Reform

	<i>DV = Support for Criminal Justice Reform (%)</i>			
	Actual Treatment		Placebo Treatment	
	(1)	(2)	(3)	(4)
Treat x Post	0.053*** (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 Obs.		2004-2008 2,614		2008-2010 1,302

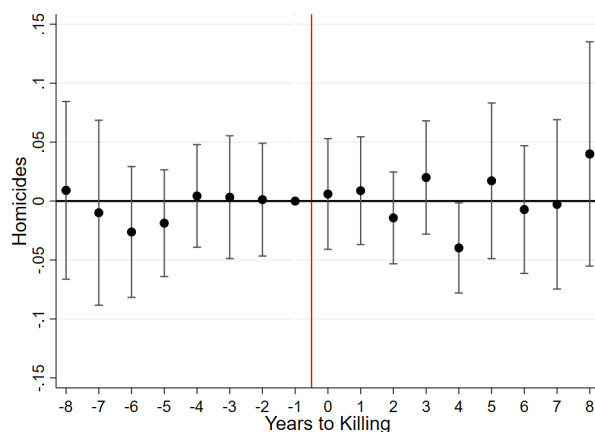
Table reports results from estimation of Equation 4 on the fraction of ballots cast in favor of referenda that would have lessened criminal justice penalties for lower-level offenses (i.e. California Proposition 66 in 2004 and California Proposition 5 in 2008). Data are restricted to Census block groups 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 * $p < 0.10$.

Civic Responses to Police Violence

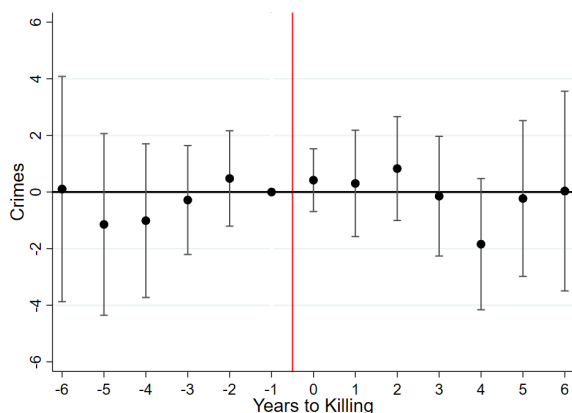
Desmond Ang and John Tebes
Online Appendix

Figure A.I: Effects on Local Crime and Arrests

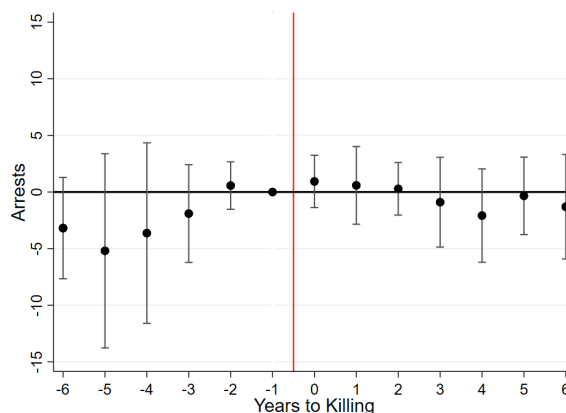
Panel A: Homicides



Panel B: Crimes

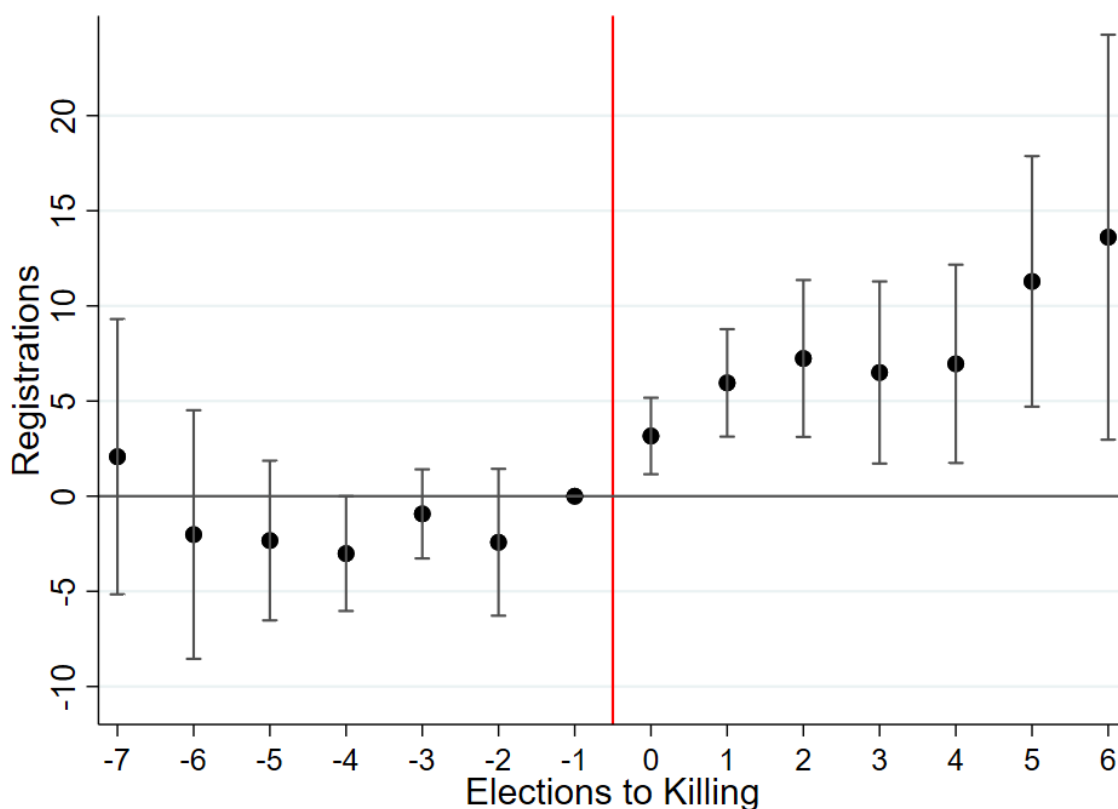


Panel C: Arrests



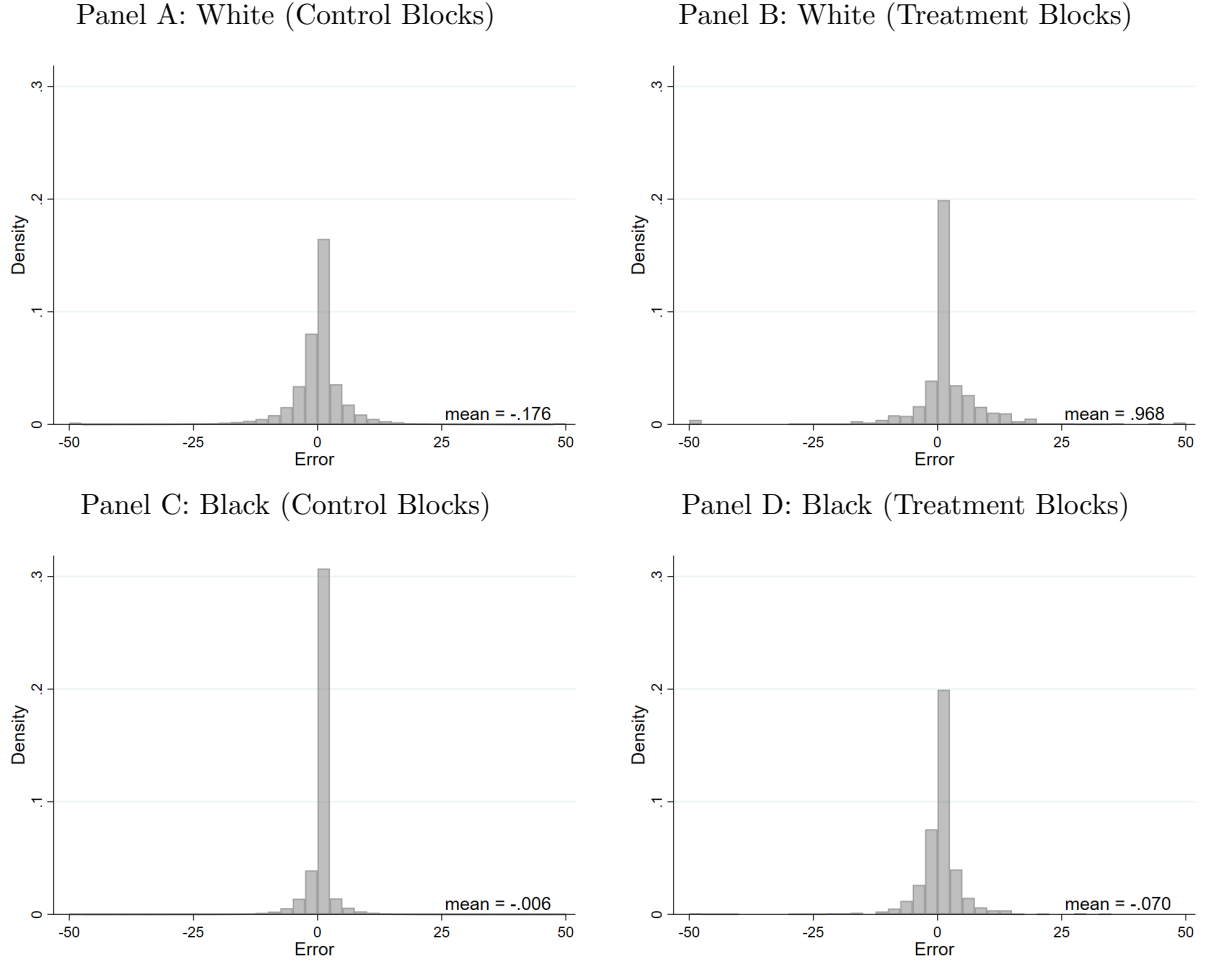
Notes: Figure shows treatment estimates and 95 percent confidence intervals from estimation of Equation 2 on homicides, total crimes and arrests. Unit of observation is number of homicides, crimes and arrests in a Census block-year. Standard errors are clustered by Census block group. For Panel A, the 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.

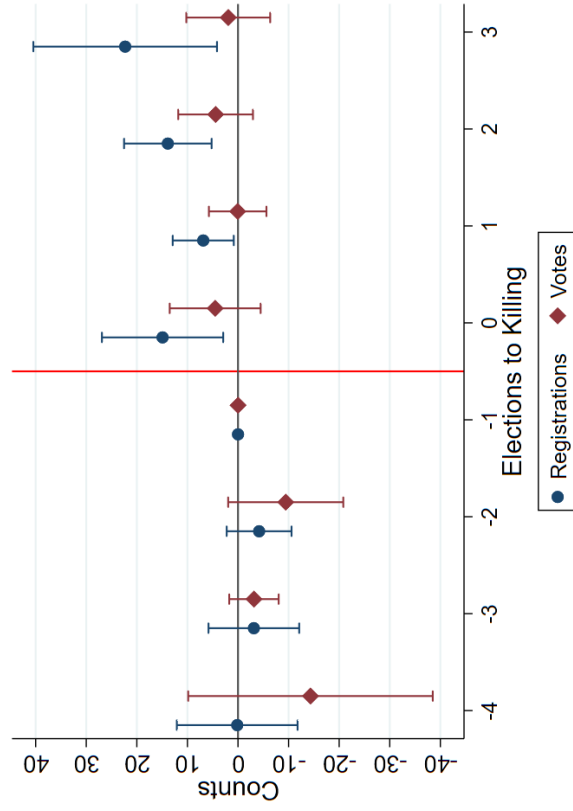
Figure A.III: Validation of Predicted Race Counts



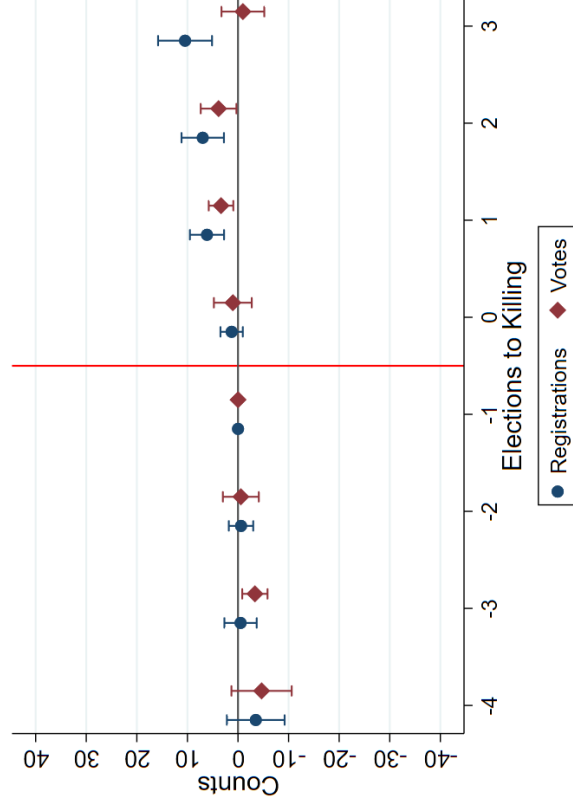
Notes: Figure shows histograms validating estimated vote counts by race obtained from Equation 3 against estimates predicted from individual-level voter registration file extracted on February 4, 2011. Each voter's race is predicted from the registration file using surname and address based on the Consumer 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

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 = 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 who were unarmed. Panel B restricts treatment group to killings of individuals armed with a knife or gun. 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.