Identifying the Effects of Policing on Voter Behavior

DISSERTATION

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Abstract

Proactive policing strategies that generate large numbers of police-citizen interactions are considered a key tool in addressing crime problems, and can lead to meaningful improvements in public safety. However, the use of frequent and aggressive enforcement actions in many poor, racially segregated neighborhoods can be viewed by community members as intrusive and discriminatory, and have been linked to a variety of negative outcomes, ranging from low trust in the police to acute psychological distress and physical harm.

Existing theory and evidence give us strong reason to expect aggressive police tactics also have political consequences. Many scholars have tied high levels of police surveillance, and the widespread use of investigatory stops and misdemeanor arrests, to the low rates of voting seen in highly policed neighborhoods, claiming that police aggression can lead community members to disengage from the formal political system whether or not they directly encounter law enforcement. Other scholars, however, claim that residents may mobilize to resist proactive policing tactics that are viewed as unjust.

Given the central role that the ballot box is thought to play in empowering citizens to resist unwanted government action, these debates hold significant implications for whether widespread calls for police reform can be translated into lasting policy change. Despite these high stakes, limits to existing theory, measurement, and identification make it difficult to know when and how the presence and behavior of the police might influence citizens' propensity to vote. Police behavior is difficult to measure, includes a diverse array of strategies, and is highly correlated with other known drivers of civic engagement, including racial segregation, poverty, and crime. In this dissertation, I address these challenges by employing a combination of experimental and quasi-experimental empirical strategies that separate the use of aggressive police tactics from the underlying social and

economic conditions that drive them, allowing me to identify their effects on political attitudes and behavior.

In **Chapter 1**, I study the political impacts of civil gang injunctions in Los Angeles — an anti-crime policy that created substantial geographic variation in the use of aggressive police tactics and the severity of criminal punishments. Exploiting this variation using a within-neighborhoods, difference-in-difference design, I find that this policy led to a 10% increase in voter registrations, comparable to the participatory impacts of automatic voter registration. This large mobilizing effect is surprising for two reasons: (1) most existing work finds that involuntary encounters with law enforcement discourage voting, and (2) this voter mobilization occurred in poor, racially segregated neighborhoods where political engagement is typically low.

In **Chapter 2**, I draw on crime statistics, panel survey data, individual voter files, and precinct-level election returns to uncover the mechanisms behind this mobilization effect. I find that gang injunctions led to large increases in self-reported discriminatory encounters with the police, were particularly mobilizing for young, Black, and Latino voters, and are correlated with increased support for criminal justice reform. Consistent with models of racialized policy threat, these findings support the claim that certain aggressive policing interventions can generate substantial backlash effects, with residents of highly policed communities going to the ballot box to demand change.

In **Chapter 3**, I leverage a randomized policing experiment conducted in Jacksonville, Florida, pairing administrative data on voting with the location of the intervention sites along with information on officer training and behavior. This allows me to compare the electoral effects of two very different styles of policing interventions in crime hot-spots — one that focused on merely increasing the presence of officers, and another which encouraged officers to decrease their reliance on enforcement actions and work with the community to address the root causes of crime. Despite large differences in officer tactics and training, I find that these interventions led to similar increases in electoral participation. These positive effects on turnout are seen in off-cycle, local elections and primarily driven by Black voters.

To my middle school principal, who told me I was a terrible writer

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List of Abbreviations

JSO Jacksonville Sheriff's Office. 50, 51, 56, 58

L.A.FANS Los Angeles Family and Neighborhood Survey. 21, 22, 25, 26, 28, 29, 31, 33

LAPD Los Angeles Police Department. 10, 11, 15, 25, 27, 30

PERF Police Executive Research Forum. 43, 50–52, 58, 63

POP Problem Oriented Policing. 43, 44, 50–55, 58, 59, 63

SARA Scanning, Analysis, Response, and Assessment. 50

UCR Unified Crime Reports. 51, 52, 102

Chapter 1

When Policing Mobilizes: Neighborhood Responses to Anti-Gang Crackdowns

Abstract

The presence and practices of the police vary substantially across place. Frequent and aggressive enforcement is often highly concentrated in specific neighborhoods, where officers are directed to preempt crime with tactics that generate large numbers of stops and arrests. How do these aggressive policies affect political behavior in the places they target? I exploit a policy that led to substantial within-neighborhood variation in the power and practices of the police, and show that residents reacted strongly to these changes. Beginning in the 1990s, a series of court-ordered injunctions against Los Angeles gangs established areas of the city where police powers were expanded, aggressive enforcement was encouraged, and the civil liberties of suspected gang members were severely curtailed. Drawing on a wide array of data sources, including aggregate and individual-level registration and turnout data, revealed preferences from ballot initiatives, and a panel survey, I find these harsh anti-gang crackdowns led to large increases in both electoral and non-electoral participation, particularly among Black, Latino, and young individuals. I find corresponding increases in support for criminal justice reform and self-reported discriminatory encounters with the police, consistent with claims that gang injunctions led to widespread racial profiling. Together, these findings suggest that concentrated anti-crime measures can have substantial electoral effects that extend far beyond those who are directly stopped and questioned by the police.

1.1 Introduction

With the promise of stopping crimes before they are committed, preemptive policing tactics have become a key feature of American law enforcement over the past three decades. Exemplified by stop-and-frisk, broken windows, and zero tolerance policing, these policies empower officers to aggressively respond to minor offenses and to stop large numbers of people for "furtive" or "suspicious" behavior. While evidence suggests that certain forms of preemptive policing can reduce crime (National Academies of Sciences, Engineering, and Medicine 2018), these policies have become a focal point in political efforts to reform policing, with observers arguing that these practices encourage racial profiling, lead to the the arrest of innocent people, and create an environment in which criminal wrongdoing is assumed (Muñiz 2015; Lerman and Weaver 2020).

Despite the central role that aggressive policing plays in contemporary debates about police reform, it is unclear how these policies affect political engagement in the places they target. Do individuals living in neighborhoods subject to harsh anti-crime crackdowns become less likely to vote, or do they become more politically active? Prior research finds that being stopped or arrested by the police can change an individual's political views and depress turnout (White 2019; Ben-Menachem and Morris 2022; Weaver and Lerman 2010), yet these direct encounters are thought to have broader consequences for the communities they occur in (Walker 2020a), influencing those who observe the actions of police officers in their neighborhood, or who hear about incidents through word-of-mouth and the media (e.g. Anoll, Epp, and Israel-Trummel 2022; Morris and Shoub 2024).

Studying the effects of policing on an entire community's political engagement is difficult for a variety of reasons. Inferences can depend on how police behavior is measured and exposure is defined, while the nonrandom distribution of police powers, presence, and practices in the United States raises serious issues when making causal claims. Existing patterns of police stops and arrests are driven by — and help to reinforce — patterns of racialized poverty and segregation (Western 2006; Capers 2008; Meehan and Ponder 2002). Given the mutually reinforcing links between policing, poverty, and racial segregation — in most cases it is difficult to imagine a clear counterfactual when trying to measure the effects of police tactics absent all other neighborhood characteristics (White 2022), or to separate the effects of aggressive and preemptive policing from the conditions that drive it.

I address these challenges by exploiting a policy that created abrupt changes in the power and practices of the police across both geography and time. Beginning in 1993, the City of Los Angeles successfully pursued a series of civil restraining orders against gangs operating in the area. These court-ordered "gang injunctions" empowered city government to curb gang activity by establishing far more punitive policing regimes in specific parts of the city where gangs were deemed a public nuisance. The changes in policy brought about by these injunction orders (e.g. increased criminal penalties, a lower standard of suspicion for officers to initiate stops and searches, and severe limitations on the movements and social interactions of suspected gang members) allow me to examine the effect of these tactics on voting behavior, while minimizing concerns that the results are being confounded by crime rates, neighborhood characteristics, or the political climate. Additionally, the scope of injunction restrictions and their manner of enforcement — including infringements on civil liberties, harsh penalties for minor crimes, and strong evidence of racial profiling — mirror the tactics that have transformed policing in "race-class subjugated communities" (Soss and Weaver 2017), ensuring that the "treatment" aligns with existing theory on the effects of punitive and discriminatory policing, without the need to infer officer behavior from use-of-force and hit-rate tests, which only capture a small subset of police-citizen interactions and can misrepresent police tactics (Knox, Lowe, and Mummolo 2020; Neil and Winship 2019).

Using a difference-in-differences design, I combine data on the timing and geography of gang injunctions in Los Angeles with detailed voter registration data, comparing changes in electoral engagement in Census blocks that were subject to anti-gang crackdowns to changes in untreated blocks in the same neighborhood. I find that voting and registrations increased by 7% and 11%, respectively, in neighborhoods placed under gang injunctions — a result that is robust to a variety of alternative specifications and estimators. I then present evidence that resident concerns over racial profiling and police aggression within the targeted areas were a key driver of this increased participation, in line with ethnographic work that documents youth-led community efforts to resist injunction policies through protests and voter registration drives (Muñiz 2015; Barajas 2007).

First, using data from the individual voter file I show substantial heterogeneity in the effect of injunctions, with mobilization driven primarily by Black, Latino, and young residents. Injunctions were particularly mobilizing for these voters when crackdowns targeted gangs associated with their ethno-racial group. In other words, Black (Latino) votes and registrations increased most in places where injunctions targeted majority-Black (Latino) gangs. Given observations that race was often used as a proxy for gang membership by police officers enforcing injunctions (Muñiz 2015), this suggests that the largest electoral effects came from the groups most likely to be the targets of increased police scrutiny and aggression.

I further explore the motivations behind these participatory effects using a panel survey. After replicating my findings on increased civic engagement using non-electoral forms of community involvement, I show that this change in turnout does not appear to reflect improvements in safety or satisfaction with more intensive policing. Indeed, my results suggest the opposite — I fail to find significant changes in self-reported crime victimization or perceptions of neighborhood safety, but do find evidence that gang injunctions led to large increases in self-reported experiences with police discrimination. Consistent with a backlash to perceived police overreach, I find that injunctions significantly increased support for local ballot propositions that were designed to reduce criminal penalties, while decreasing support for those that provided additional funding for prisons and the police.

Together these findings suggest that individuals are attentive to the behavior of the police in their neighborhood, and that this behavior is politically consequential. In some cases, aggressive policing tactics may generate substantial political resistance as individuals go to the polls to demand change, particularly when those tactics are seen as unjust or racially targeted.

1.2 Place and Inequality in Policing

Scholarship on the political effects of the criminal justice system has long been motivated by its wide reach and disparate impact across race and class in the United States (White 2022). Inequality in criminal justice outcomes is thought to be driven in large part by the vast differences in both the quantity and character of policing across place (Braga, Brunson, and Drakulich 2019). While some communities enjoy rapid response times to calls for service, effective crime control, and procedurally just interactions with citizens, policing in other neighborhoods is characterized by extractive or punitive tactics that render negative and dehumanizing contact with law enforcement a constant feature of daily life.

There are strong reasons to expect that these neighborhood-level differences in policing should matter for political participation. A long tradition in political science documents how individual's policy preferences and voting behavior are influenced by the environment they live in. Daily experiences can influence determinants of voting such as well-being and social ties, and individuals are thought to take cues from their environment when forming policy-relevant attitudes about the economy, inter-group relations, and the quality of government services (Enos 2014; Hopkins 2010; Newman et al. 2015). Routine encounters with government agencies and agents can be particularly influential — a large body of work documents how direct contact with law enforcement through stops, arrests, and incarceration influences electoral and non-electoral political participation (Lerman and Weaver 2020; White 2019). These effects can spill out to influence the attitudes and behavior of friends and family (White 2019), and likely extend beyond personal connections into the political life of the wider community (Anoll, Epp, and Israel-Trummel 2022; Enns 2014).

Policing is a highly visible form of government action — individuals may learn about the character of policing in their neighborhood by witnessing officer behavior, hearing the accounts of friends and neighbors, or seeing incidents portrayed in media (Morris and Shoub 2024). These experiences can provide signals about the quality of public services and the types of people targeted by coercive state action, which in turn may influence political behavior by setting expectations about the responsiveness of government more broadly (Lerman and Weaver 2020), and by generating policy demands based in the perceived benefits or threats of existing police practices.

1.3 The (De)Mobilizing Effect of Aggressive Police Tactics

Existing empirical and theoretical work on the political effects of policing has focused primarily on "punitive," "aggressive," and "preemptive" tactics in highly policed communities, which generate large numbers negative police-citizen interactions and disproportionately impact Americans of color (e.g. Lerman and Weaver 2014; Walker 2020b; Laniyonu 2019). While aggressive and preemptive policing can be an effective tool in addressing longstanding concerns over public safety in poor, racially segregated neighborhoods, in practice, these tactics are often ineffective at controlling serious crime (Braga, Brunson, and Drakulich 2019), exacerbate racial disparities in police stops (Baumgartner, Epp, and Shoub 2018; Pierson et al. 2020; Gelman, Fagan, and Kiss 2007), and undermine trust in law enforcement (Braga, Brunson, and Drakulich 2019; Brunson 2007). Policies such as stop-and-frisk, hot spots, and zero tolerance policing are often characterized by unfocused and indiscriminate enforcement that targets anyone who appears in a crime hot spot (National Academies of Sciences, Engineering, and Medicine 2018), or inappropriately defines entire neighborhoods as "high crime" places where residents are subject to high numbers of intrusive stops, searches, and seizures (Fagan et al. 2016). Despite these widespread impacts, existing scholarship gives competing expectations as to whether these tactics may demobilize the public or encourage political action and resistance.

Using surveys and qualitative case studies, many scholars have linked exposure to aggressive policing with feelings of alienation, isolation, and distrust in the criminal and legal systems (Epp, Maynard-Moody, and Haider-Markel 2014; Stoudt, Fine, and Fox 2011; Bobo and Thompson 2006; Gibson and Nelson 2018). For some individuals this can lead to avoidance of public spaces and state institutions (Lerman and Weaver 2020; Rios 2011; Bell 2017), contributing to the low rates of voting and political engagement seen in disadvantaged minority neighborhoods (Cohen and Dawson 1993).

Yet other work suggests that these perceived injustices could be politically mobilizing, particularly for Blacks and Latinos. Consistent with longstanding racial gaps in perceptions of law enforcement (Gibson and Nelson 2018), scholars find that Black (and to a lesser extent Latino) Americans view preemptive policing tactics as discriminatory, tying them to broader narratives of racial profiling and institutionalized bias in the criminal justice system (Epp, Maynard-Moody, and Haider-Markel 2014). This is particularly true in disadvantaged and minority communities with high levels of crime and disorder (Gau and Brunson 2010), where negative encounters with police are common (Fagan et al. 2016).

Narratives that recast individual interactions with the police in terms of group-based inequalities can foster feelings of injustice (Walker 2020a) and elicit a sense of linked fate that motivates political resistance among individuals who view their group as being unfairly targeted by negative government action (Oskooii 2020; Garcia-Rios et al. 2023a). Race often serves as a powerful source of group consciousness for Black (Dawson 1995) and Latino Americans (Zepeda-Millán and Wallace 2013), and policies that are seen as unfair and racially targeted have been shown to drive political mobilization in a variety of contexts (Dawson 1995; Cho, Gimpel, and Wu 2006; Nuamah and Ogorzalek 2021). Consistent with this mechanism, Walker (2020b) finds correlations between proximal contact with the police and non-electoral forms of political participation, particularly among individuals who view the behavior of the police in their neighborhood as unfair or unjust. Both Ang and Tebes (2024) and Morris and Shoub (2024) find that proximity to police killings increases both turnout and support for criminal justice reform, particularly when a killing fits preexisting narratives of unjustified killings of unarmed Black civilians (Morris and Shoub 2024). However, this is a rare and extreme form of police violence which may have very different effects than everyday experiences with the police.

Adjudicating between these competing claims is made difficult by the endogenous nature of policing, which as noted above is deeply entwined with preexisting racial and economic inequalities known to shape attitudes toward the government, police, and voting. These issues are compounded by the difficulty inherent in reliably measuring exposure to aggressive policing. Self-report data can suffer from reporting bias given that how an individual characterizes policing in their neighborhood is likely correlated with other drivers of political engagement. Administrative records can also lead to faulty or incomplete inferences about officer behavior (Neil and Winship 2019; Knox, Lowe, and Mummolo 2020). In one of the few studies linking administrative data on police stops to neighborhood-level turnout, Laniyonu (2019) uses the per capita rate of stops and frisks in a given neighborhood as a proxy for exposure to aggressive policing. A high geographic concentration of investigative stops may have indicated that officers were aggressively cracking down on a small number of high-risk individuals who threatened the safety of other residents, or may have indicated a poorly focused intervention that subjected all individuals in the area to increased suspicion and surveillance. While both patterns of officer behavior may be considered "aggressive," it is not obvious that these different enforcement priorities should have similar electoral effects at the aggregate level.

I address these challenges by leveraging sudden, geographically-bounded shifts in police policy generated by gang injunction orders in Los Angeles, which dramatically increased the power and discretion available to officers to arrest and detain individuals suspected of being gang members within certain areas of the city. In line with previous findings that the character of police-citizen interactions can be shaped by oversight and institutional directives (Mummolo 2018; Epp and Erhardt 2021; Baumgartner, Epp, and Shoub 2018), I present evidence that this policy shift encouraged far more aggressive policing, led to widespread accusations of racial profiling, and generated increased legal restrictions and risks for the residents of the target neighborhoods. Importantly, these changes provide a plausible counterfactual to study voting behavior in neighborhoods placed under more punitive policing regimes.

1.4 Historical Context and Background: Gang Injunctions

Throughout the 1980s and 1990s, concerns over high homicide rates and drug dealing associated with gangs led to the proliferation of aggressive anti-gang crackdowns as police departments and legislatures responded to public demands for action. In California, local municipalities pioneered the use of public nuisance laws against gangs, seeking injunctive relief in civil court against egregious gang behavior that ranged from "quality of life" issues — such as vandalism and loud music — to more serious crimes, including open drug-dealing, verbal harassment, threats of violence, drive-by shootings, and frequent murders (Harward 2014). The restraining orders local governments obtained allowed them to impose sweeping restrictions on the movement and behavior of suspected gang members within a specific target area, empowering the police to essentially "banish gang members from the public streets and in a growing number of targeted neighborhoods" (Werdegar 1999, 411), without the need to provide defendants with legal council or a jury trial.

The Supreme Court of California upheld the legality of this tactic in the 1997 case *People ex rel. Gallo v. Acuna*, allowing cities and local municipalities to use public nuisance laws to expand police powers, heighten criminal penalties, and severely limit individuals' movements and social interactions within a pre-specified "safety zone" that often covered dozens of city blocks (Harward 2014; Werdegar 1999).[1] Gang injunctions quickly proliferated following the *Acuna* decision — today

1. Injunctions prevented gang members from engaging in a variety of legal activities, criminalizing everyday behaviors such as using a cellphone, gathering in groups of more than two, riding a bike, or wearing certain clothing

there are hundreds of active injunctions throughout California and the Western United States, with 46 in Los Angeles alone.[2]

Despite the promise of enhanced public safety and their initial popularity, gang injunctions have faced criticism from activists and legal scholars, who argue that injunctions are ineffective at addressing gang-related crime, encourage racial profiling, and violate the due process rights of the accused (Werdegar 1999; Muñiz 2015; Miranda 2007). By creating harsh penalties for minor crimes and empowering the police to engage in frequent and aggressive stops, injunctions helped to create substantially more punitive policing regimes in the areas they targeted, mirroring many of the tactics used in the wars on drugs and crime that have made high levels of police contact a constant feature of life in poor, minority communities (Soss and Weaver 2017).

While their main feature was to impose harsh restrictions on the behavior and movements of specific individuals, gang injunctions had substantial impacts that extended to all residents of the identified safety zones. Because gang membership is often unstable, most cities brought their suits against gangs as legal entities (Harward 2014), allowing the police to enforce the terms of the injunction on anyone identified as a member of the enjoined gang, even if they were not named as a defendant in the original suit. Critics have argued that the criteria for identifying gang members at the time were so subjective and broad that they could be applied to "[v]irtually every young African American or Latino male living in a neighborhood where gangs are active" (Werdegar 1999, 423), raising serious concerns about racial profiling (Muñiz 2015) and violations of due process rights (Werdegar 1999).[3] Moreover, it was often extremely difficult for individuals to determine if they had been identified as a gang member by the State of California (Owens, Mioduszewski, and Bates

(Werdegar 1999; O'Deane 2011; Muñiz 2015), while also increasing penalties for breaking existing laws with automatic fines and jail time.

2. While most of these injunctions are still officially in effect, a Federal court blocked the Los Angeles Police Department (LAPD) from enforcing them in 2018 due to suspected violations of due process rights. In a settlement reached in 2020, the city agreed to only enforce injunctions against named defendants who had been given the opportunity to challenge their gang designation in court.

3. Throughout the 1990s and 2000s, many law enforcement agencies at the state and local level in California would identify an individual as a gang member if: (1) they directly admitted to involvement with a gang, (2) they were identified as a member by a reliable informant, (3) they were arrested in the presence of other gang members for offenses consistent with gang activity, or (4) they lived in or often visited a gang's territory, associated with gang members, and adopted "their style of dress, uses of hand signs, symbols, or tattoos" (Kim 1995, 270). In 2007, Los Angeles formalized gang injunction protocols with a more strict definition of a criminal street gang and increased required documentation of gang membership.

2020) or to remove themselves from gang databases,[4] suggesting that anyone whose social networks included gang members, or who "fit the profile" could reasonably expect to be affected by the injunction.

Beyond the harsh new penalties that targeted a broad and poorly defined segment of the community, injunctions also decreased legal constraints on officer behavior. To detain someone, the police are legally required to have a specific, well-defined reason to suspect that the individual in question has violated the law. In most circumstances, people have a constitutional right to decline a request by an officer, and failure to comply cannot be taken as grounds for suspicion (Boga 1994). Within an injunction safety zone, however, failure to comply could be considered evidence of evasion, meaning that an officer could stop and search anyone who ignored questions or walked away from an interaction (Boga 1994; Owens, Mioduszewski, and Bates 2020). There are strong reasons to suspect that this would have altered police behavior and increased the rate of involuntary police-citizen interactions, given previous scholarship finding that patterns of stops and arrests are highly responsive to legal oversight (Prendergast 2021), and institutional directives (Mummolo 2018).[5] This was a possibility recognized at the time, with one Los Angeles city prosecutor lamenting that within the Los Angeles Police Department (LAPD), injunctions were often seen as a way to, "give cops the chance to stop anybody for any reason" (Muñiz 2015, 53).

The gang injunctions enforced by the LAPD generated changes in policing tactics and authority across both geography and time, creating substantially harsher policing regimes within the associated safety zones that encouraged aggressive enforcement and racial profiling. Scholars have noted that community knowledge of the injunctions was high, driven by media coverage, observed changes in police behavior, and accounts of the targeted individuals, who shared their experiences with their families and wider social networks (O'Deane 2011; Muñiz 2015). These policies were also met with substantial opposition, with youth-led activist groups organizing protests, attending community

4. Prior to 2007, no person added to a gang list in Los Angeles had been removed, likely due to a requirement that the person publicly renounce membership in the gang, which could generate retaliation (O'Deane 2011, 400). Even after reforms were made to this process, removal was extremely rare.

5. To have an effect on officer behavior, the police had to be aware of the existence, terms, and targets of a given injunction and willing to enforce them. Throughout the 2000s, city, county, and state entities in California regularly held workshops to ensure that officers were aware of existing injunctions and procedures, and to encourage aggressive enforcement (O'Deane 2011). A small survey of police officers conducted by (O'Deane 2011) suggests that awareness of injunctions among gang units was high and that most officers felt that enforcing them was a good use of their time and resources.

meetings, and staging voter registration drives (Muñiz 2015; Barajas 2007). I investigate whether this observed political action was emblematic of wider community mobilization against these policies. In other words, did residents of neighborhoods targeted by anti-gang crackdowns organize against these policies, or did gang injunctions contribute to a sense of "legal estrangement" (Bell 2017) that discouraged voting?

1.5 Empirical Approach

To measure the effects of these injunction orders on civic participation, I leverage their staggered adoption and limited geography in a difference-in-differences design, comparing changes in registrations and voting among those whose block was placed under an injunction to similar residents who were not covered by a gang injunction but lived in the same neighborhood. Rather than comparing turnout in neighborhoods that vary widely in terms of race, income, crime, and police behavior, this approach allows us to measure the effect of a substantial change in policing policy, and the community response to the introduction of more punitive police tactics.

1.5.1 Data on Gang Injunctions and Voting

Data on the location and timing of the 50 injunctions that were imposed in the City of Los Angeles between 1993 and 2013 come from court documents made available by the Los Angeles City Attorney's Gang Unit. These court rulings include the date of the initial complaint, the gangs named in the case, the list of prohibited activities, the date the permanent injunction was granted, and the boundaries of the safety zone, which I digitized using GIS software. I supplemented this with additional information on legal proceedings and enforcement actions gathered from City Attorney press releases, local news stories, government reports, county court records, and several empirical studies of gang injunctions and crime (O'Deane 2011; Ridgeway et al. 2019; Grogger 2002; Los Angeles County Civil Grand Jury 2004).[6]

Injunctions require a lengthy legal process, with months — sometimes years — between the initial complaint issued by the city and the final injunction order. I consider "treatment" to begin

6. Three local newspapers were searched for articles on gang injunctions: The Los Angeles Times, The Los Angeles Daily News, and La Opinión.

when a preliminary injunction was obtained by the court — the preliminary order is typically requested when the initial case is filed and allows the city to enforce the injunction restrictions as the lawsuit is pending (O'Deane 2011). In cases where a preliminary injunction was either not sought or approved, I use the date of the permanent order.[7] In cases where blocks were placed under multiple injunctions, I consider treatment to begin with the first injunction.

The digitized safety zone boundaries associated with each injunction were mapped onto 2010 Census blocks, and merged with redistricting data from the California Statewide Database. These data report the number of registered voters and the number of ballots cast on the day of each general election between 1992 and 2020 at the Census block-level.[8] These counts — which are based on geocoded individual records in the California voter file — are also broken down by age, gender, partisan affiliation, and ethnicity using surname matching.[9] I combined this information with block-level data from the 2000 and 2010 Decennial Censuses.[10] The analysis sample consists of a set of "stacked" datasets, each corresponding to a separate treatment timing cohort (g), which includes all blocks placed under an injunction between two consecutive Federal elections (e.g. all injunctions put in place between December, 2002 and October, 2004), as well as the set of never-treated Census blocks.

1.5.2 Primary Specification

For my main analysis on the effects of injunctions on political participation I estimate the following model with the stacked, block-level panel data:

$$ln(y+1)_{b,e,g} = \beta INJUNCTION_{b,e,g} + \beta \mathbf{X}_{b,e,g} + \gamma_{b,g} + \gamma_{t,e,g} + \epsilon_{b,e,g} \tag{1.1}$$

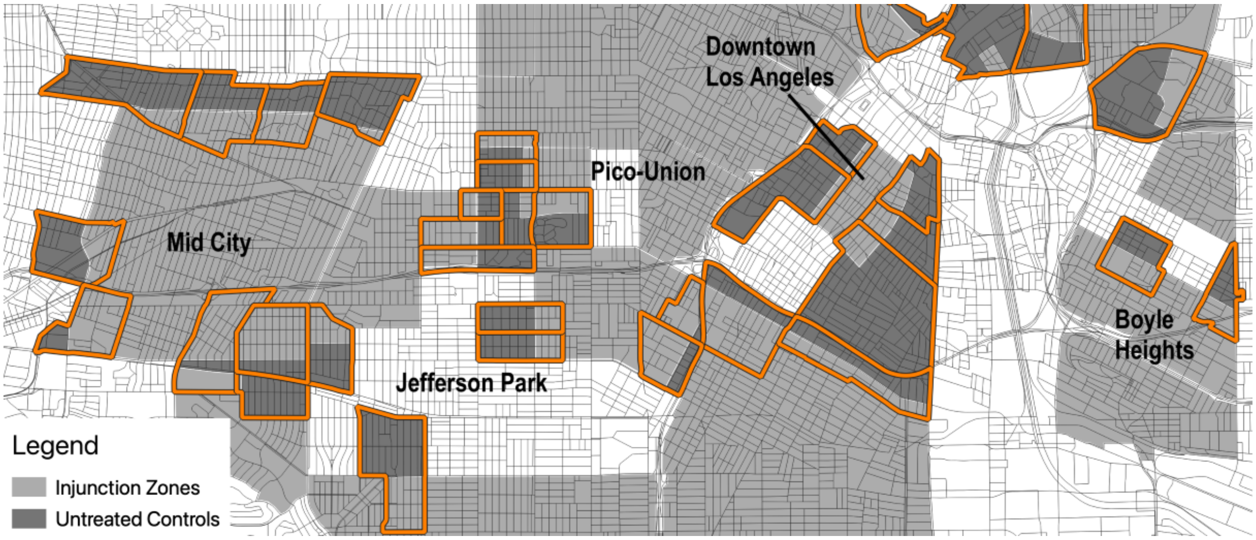
7. See Appendix Table A.1 for a full list of injunctions. Two of the sample injunctions were suspended due to the Rampart police corruption scandal and a third lapsed under civil procedural rules — all three safety zones were incorporated into later injunctions (Ridgeway et al. 2019).

8. Voting information is only available after 2000.

9. The Statewide Database uses the U.S. Census Bureau's Passel-Word dictionary for Hispanic surnames and the Lauderdale and Kestenbaum (2000) dictionary for Asian surnames. Previous research has found that surnames are highly predictive of ethnic self-identification (Lauderdale and Kestenbaum 2000; Henderson, Sekhon, and Titiunik 2016). The Census Bureau's Passel-Word Spanish surname list has a true positive rate of more than 93%, and a false positive rate of less than 5% (Word and Perkins 1996).

10. The U.S. Census Bureau provides Block Relationship Files, which I used to convert 2000 Census data to 2010 Census geographies. As an additional robustness check, I re-run my main analyses using only census blocks that could be converted without the use of areal interpolation — either because there were no changes between the 2000 and 2010 block boundaries, or because multiple 2000 blocks were contained in one 2010 block — and find similar results.

Figure 1.1: **Visualization of Main Identification Strategy**



Note: Map of the maximum extent of Gang Injunctions in Central Los Angeles. Census tracts with identifying variation are outlined in orange. The within-neighborhoods design compares changes in registrations and votes cast in treated Census blocks (light gray) to changes in registrations and votes cast in untreated Census blocks (dark gray) that are in the same tract.

Where $y_{b,e,g}$ is either the log-transformed number of votes cast or voters registered in Census block b, election e and treatment timing group g,[11] and $\beta INJUNCTION_{b,e,g}$ is an indicator for whether or not a given block is covered by a gang injunction in that year. To account for spatial spillovers, I include a set of indicators capturing distance to the nearest injunction boundary in the set of time-varying, block level controls ($\mathbf{X}_{b,e,g}$). Block-by-treatment group fixed effects ($\gamma_{b,g}$) control for time invariant characteristics of Census blocks that might shape turnout and registration rates, while election-by-Census-tract-by-treatment group fixed effects ($\gamma_{t,e,g}$) control for common shocks to voting and registration in a given election year.

Intuitively, this approach recovers the effect of injunction policies on electoral participation by comparing within-Census block changes in registrations and voting in blocks that are under an injunction to within-block changes in behavior among untreated blocks that are in the same

11. Taking this "stacked regression" approach allows me to avoid comparisons between sets of already treated blocks that can lead to uninterpretable two-way fixed effect estimates in the presence of treatment effect heterogeneity across either units or time (Goodman-Bacon 2021; Imai and Kim 2019). In Appendix Table A.3, I show that my results are substantively similar when using the inverse hyperbolic sine function, as well as the untransformed count of registrations and votes.

Census tract. Figure 1.1 visualizes this identification strategy, displaying Census blocks covered by gang injunctions in Central Los Angeles (light gray), alongside the untreated Census blocks used as controls (dark gray).[12]

1.5.3 The Plausibility of Parallel Trends

The central identifying assumption underlying my approach is that within a given Census tract, there are no unobserved, time-variant confounders related to both voting and selection into treatment. In the next section, I take a variety of approaches to assess the validity of this claim, including estimating dynamic effects with leads and lags of treatment, conditioning counterfactual trends on a variety of pre-treatment covariates, and matching on pre-exposure outcomes. In addition to these empirical checks, I note that the legal process by which specific blocks were selected for inclusion does not appear to have been related to time-variant neighborhood characteristics that are correlated with civic involvement, such as sudden, localized crime waves or community-led campaigns pushing for increased public safety.

Previous scholarship suggests injunctions enforced by the LAPD were implemented via a standardized, top-down process with limited community involvement (Muñiz 2015; Maxson, Hennigan, and Sloane 2003). City prosecutors would identify neighborhoods with high crime rates where gangs were suspected to have claimed territory (Werdegar 1999), and then work with local police and gang informants to identify individual gang members and catalogue any illegal or disruptive activity they were engaged in (Maxson, Hennigan, and Sloane 2003; Allan 2004). While the officers documenting gang activity would often seek the testimony of community members, participation in this process was low due to distrust of the police and city government, weak neighborhood institutions, and fear of retaliation by gangs (Grogger 2002; Miranda 2007; Allan 2004).

The geographic extent of the safety zones was designed to cover the areas where prosecutors claimed the gang constituted a "public nuisance," with the exact boundaries frequently following major roads and the boundaries of LAPD reporting districts to make them easier for both officers and gang members to identify (O'Deane 2011). These areas did not necessarily align with overall

12. This subset of treated and control Census blocks is used for illustrative purposes, for a full map of Los Angeles gang injunctions see Appendix Figure A.1.

crime rates, or with the gang's territory, given that a substantial amount of gang violence in Los Angeles occurs outside the territory gangs claim and operate in (Brantingham et al. 2012).

1.5.4 Other Potential Threats to Identification

An additional concern raised by the use of aggregate data is that any post-treatment changes in political participation are the result of selective mobility into and out of the injunction safety zones, rather than changes in individual-level political behavior. However, there are several reasons to think that changes in population size and/or composition are unlikely to be a major concern. First, the "thinness" of the residential housing market imposes limits on residential sorting within small geographic areas, such as Census tracts (Bayer, Ross, and Topa 2008). Second, the positive effects I find on registrations and turnout suggest that the main threats to identification would come from population increases, and/or an influx of individuals with a higher propensity to vote. Yet I find that between 2000 and 2020, the population of Census blocks covered by injunctions slightly *decreased* relative to uncovered blocks in the same Census tracts. The results of a difference-in-differences model comparing changes in population between treated and untreated blocks with year-by-Census tract fixed effects are close to zero and statistically insignificant (Appendix Table A.2).

While the population within the safety zone boundaries did not increase, it is also possible that selective migration into and out of the safety zones may have changed the demographic composition of the neighborhood in ways known to be associated with increased political participation, such as socioeconomic status (Schlozman, Verba, and Brady 2013). For example, injunctions may have encouraged the in-migration of more affluent and educated residents, who voiced support for the increase in police presence (Muñiz 2015; Barajas 2007), while encouraging poorer Black and Latino individuals to leave given that members of these groups were far more likely to be labeled as gang members (Muñiz and McGill 2012), and thus subject to the harsher criminal penalties of the injunction.

Despite concerns that injunctions have been used as tools to gentrify poor neighborhoods (Muñiz 2015), several pieces of evidence suggest that injunctions did not lead to the displacement of poor residents with more affluent ones. First, I find that injunctions do no appear to have an effect on overall residential mobility. Using 2008-2012 ACS data, I find that the share of households reporting that they moved in the past year is similar in block groups covered by an injunction and those that were not, even after restricting comparisons to treated Census tracts (13.5% vs. 13.8%). Second, the economic and demographic changes seen in injunction safety zones do not match known patterns of gentrification. While previous scholarship has found that the gentrification of poor neighborhoods in Los Angeles has coincided with substantial increases in property values and influxes of White residents (Scott 2019), well-identified estimates from Owens, Mioduszewski, and Bates (2020) find that both property values and the share of White in-movers decreased within injunction safety zones following their implementation.[13][14]

1.6 Main Results

Table 1.1 gives the main results. Because the outcome is log-transformed, exponentiated coefficients can be interpreted as the percent change in voting and registrations between blocks put under an injunction and the control group. I find that injunctions had a powerful mobilizing effect on the communities in which they are implemented. Estimates from the main specification (Models 1 and 5) suggest that being placed under an injunction led to a 7% increase in votes cast in a given block, and an 11% increase in the number of registrations. This is similar in magnitude to the 11% increase in registered voters seen in Los Angeles in the two elections following the introduction of automatic voter registration in 2015 from the two elections prior, which corresponded to an average of ≈ 6.5 additional registrations per Census block.

This result is robust to a variety of alternative specifications. In Models 2 and 6 I include year fixed effects interacted with population deciles from the 2000 Census to account for possible confounds that vary with block-level population, such as differential population growth. In Models 3 and 7 I additionally control for neighborhood racial composition, interacting year fixed effects with quartiles of the Black and Latino share of the population. This accounts for the possibility that

13. It is possible that this relative depreciation in home values mobilized homeowners to protect their home values (Hall and Yoder 2022). While I cannot rule this out as a possible mechanism, below I find that much of the mobilizing effect is concentrated among young people, who are less likely to own a home. I also note that the period I study overlaps with a substantial appreciation in home values (Scott 2019), and it is unclear how sensitive home owners are to slower relative rates of price growth as opposed to depreciation in the real value of their property.

14. In Appendix Table A.2, I present a difference-in-differences regression which suggests that the White population increased slightly in injunction safety zones relative to other racial groups; however, these effects are small and in later sections I present evidence that mobilization was almost entirely driven by Black and Latino residents.

Table 1.1: **Difference-in-Difference Estimates: Effect of Gang Injunctions on Electoral Participation**

	Registrations				Votes Cast			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Injunction	0.106***	0.099***	0.081**	0.091***	0.073**	0.067*	0.060*	0.060*
	(0.025)	(0.025)	(0.025)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)
Census block FE's	✓	✓	✓	✓	✓	✓	✓	✓
Year-by-Census tract FE's	✓	✓	✓	✓	✓	✓	✓	✓
Pop.-by-Year FE's		✓	✓			✓	✓	
Race Comp.-by-Year FE's			✓				✓	
Proximity controls	✓	✓	✓	✓	✓	✓	✓	✓
Full sample				✓				✓
N. Observations	1624632	1624632	1624632	1720663	848843	848843	848843	913870
N. Blocks	20810	20810	20810	22599	19309	19309	19297	21412
Adj. R^2	0.91	0.91	0.91	0.90	0.90	0.90	0.90	0.91
R^2 (within)	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00

Note: OLS estimates. Models 1 and 5 present the main specifications for registrations and votes, respectively. Models 2, 3, 6, and 7 include deciles of the 2000 population-by-year fixed effects. Models 3 and 7 include quartiles of Black and Latino share of the 2000 population-by-year fixed effects. Models 4 and 8 expand the sample to the full, unbalanced panel. Robust standard errors clustered by Census block given in parentheses. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

blocks with high proportions of ethno-racial minorities may have been more likely to be selected into injunction safety zones and displayed differential trends in voting participation. Finally, in Models 4 and 8, I include the full, unbalanced panel. Across all specifications the coefficients are substantively similar, consistent with a significant increase in registrations and voting in blocks that were included in injunction zones.

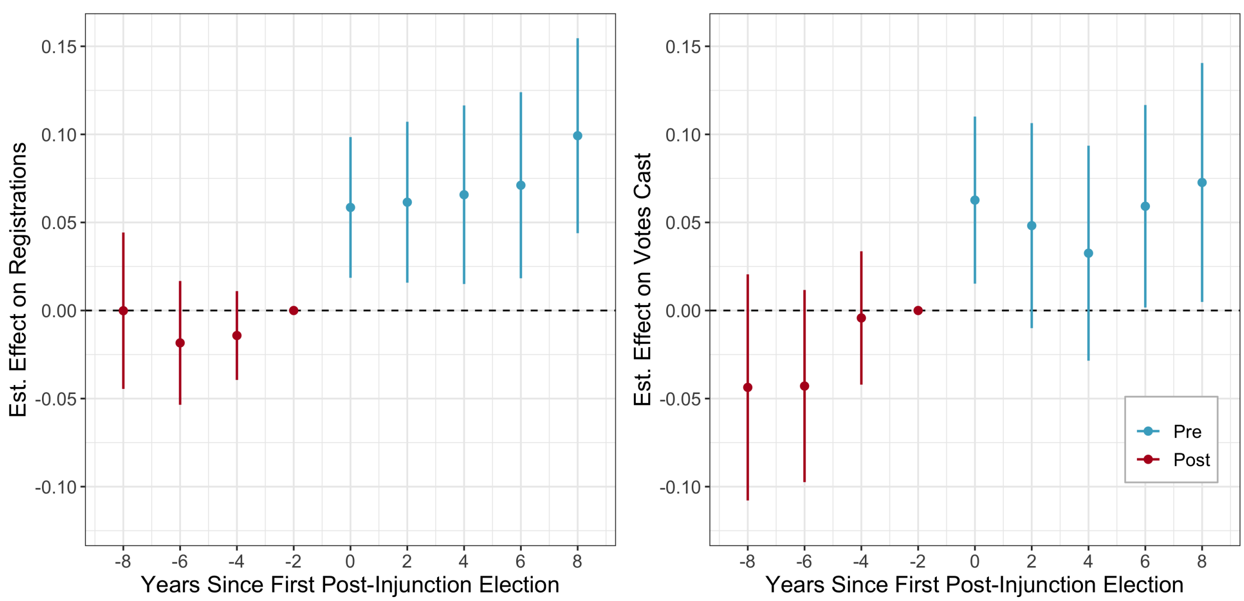
In Figure 1.2 I present dynamic effect estimates which provide empirical support for the parallel trends assumption, with small and statistically insignificant estimates for the pre-treatment period. To address concerns that this form of pre-testing can be underpowered (Freyaldenhoven, Hansen, and Shapiro 2019), I demonstrate that my results hold with the use of synthetic controls to ensure parallel trends in the pre-treatment period, as well as with a semi-parametric, propensity-score weighted estimator that relaxes the parallel trends assumption to hold after conditioning on a set of observed pre-treatment covariates.[15]

First, I employ a synthetic difference-in-differences estimator (Arkhangelsky et al. 2021), fitting separate models for each treatment timing cohort. In addition to Census block and year fixed effects, the synthetic DID method applies unit weights to ensure that pre-treatment trends in the outcome are parallel between treated and untreated units, as well as time weights that balance outcomes in the pre- and post-treatment periods for the control group. This improves the plausibility of the difference-in-differences design by eliminating pre-trends and placing more weight on pre-treatment elections in which electoral participation in the control blocks is similar to its post-injunction values. Taking the weighted average effects for each treatment timing cohort recovers estimates that are nearly identical to my main results (Appendix Table A.4), while the disaggregated effects suggest that the earliest injunctions generated the largest mobilization effects.

In Appendix Figures A.2 and A.3, I present difference-in-differences and event study estimates using a doubly robust, propensity score weighting estimator (Callaway and Sant'Anna 2021; Sant'Anna and Zhao 2020), which incorporates covariates likely to be related to selection into gang injunction safety zones as well as across-time trends in voting and registrations. This approach produces valid estimates if blocks with the same demographic characteristics would have followed

15. Because these alternate approaches do not include Census tract fixed effects, I restrict the sample to Census tracts that contain at least one treated block to ensure comparability with my main results.

Figure 1.2: **Event Study Estimates: Effect of Gang Injunctions on Electoral Participation**



Note: Event study estimates of the effect of gang injunctions on registrations (left) and votes (right) along with 95% confidence intervals. Specification is identical to the difference-in-difference model presented in Equation 1.1, with the post-treatment indicator replaced by leads and lags of treatment.

the same trend in electoral participation in the absence of treatment. Controlling for population, racial composition (i.e. percent Black, Latino, and White), median income, the fraction of population receiving public assistance, and the average annual crime rate from 1991 to 2000, I again find positive and statistically significant effects.

1.6.1 Non-Electoral Participation

Previous scholarship notes the importance of non-electoral forms of political participation in many highly policed communities (e.g. Walker 2020a; Weaver, Prowse, and Piston 2020), which may be particularly true in the poor, immigrant neighborhoods targeted by many gang injunctions, where many residents may lack the right to vote (Bedolla 2005; Zepeda-Millán 2016). Previous work finds that non-political community groups and organizations often take on political roles in these communities (Zepeda-Millán 2016), providing the resources and information needed to overcome socioeconomic barriers to participation (Schlozman, Brady, and Verba 2018; Walker 2020a).

Given this, I turn to individual-level panel data from Waves I (2000-2001) and II (2007-2008) of the Los Angeles Family and Neighborhood Survey (L.A.FANS) to examine whether the mobilizing effect of gang injunctions extends beyond the ballot box. L.A.FANS included a battery of questions related to community/civic involvement in the past 12 months, including participation in a (1) neighborhood or block organization meeting, (2) business or civic group, (3) nationality or ethnic pride club, or (4) local or state political organization, as well as volunteering in a (5) local organization. These responses were used to create a non-electoral civic participation scale ranging from 0 to 5 (Mean = 0.45, st. dev. = 0.88). Because the baseline level of participation was low (approx. 73% of respondents reported no community involvement), I treat this index as binary variable in the analyses that follow.[16]

Using restricted-use data, I mapped residents to injunction safety zones using their Census block of residence at Wave I. Residents were considered treated if their Census block was placed under an injunction order after they had completed Wave I (2000-2001) and before completing Wave II (2007-2008) of the survey—of the 1193 adults who completed both survey waves, 181 are considered treated over this time period, while 313 (26.2%) of respondents lived in Census blocks that were

16. In Appendix Table A.7 I show my results are robust to modeling participation as a count.

Table 1.2: **Difference-in-Difference Estimates: Effect of Gang Injunctions on Non-Electoral Civic Participation**

	(1)	(2)
Injunction	0.255**	0.152*
	(0.071)	(0.068)
Ind. FE's	✓	✓
CBPS Weights		✓
Full sample		✓
N. Observations	408	2352
N. Individuals	207	1180
Adj. R^2	0.259	0.215
R^2 (within)	0.061	0.023

Note: OLS estimates. Model 1 presents the main specification with individual and survey wave fixed effects. Model 2 expands the sample to all survey respondents, and includes covariate-balanced propensity score (CBPS) weights. All models include weights provided by L.A.FANS to account for attrition between survey waves. Robust standard errors clustered by household and Census tract given in parentheses. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

already covered by an injunction or would be in the future.

To assess the effects of injunctions on community involvement, I fit a series of difference-in-difference models with individual and survey wave fixed effects. To improve the plausibility of the parallel trends assumption, I restrict the sample to individuals in Census tracts that were covered by an injunction between the two survey waves. In a second set of models, I use covariate-balanced propensity score (CBPS) weights (Imai and Ratkovic 2014) on the entire sample to obtain balance in observed characteristics likely to be correlated with both treatment assignment and changes in non-electoral participation over time (see Appendix Table A.5 for the full list of variables and balance statistics).

Table 1.2 gives the main results. Based on the estimates from Model 1, I find that individuals living in a Census block placed under a gang injunction order became significantly more likely to report civic involvement in their communities. I find similar results using CBPS weights in Model 2, which are consistent with a roughly 15% increase in the linear probability of reporting non-electoral forms of community involvement.

These results are robust to modeling participation as a count, as well as the inclusion of tract-by-wave fixed effects (Appendix Tables A.7 and A.6). As a final check, in the Appendix (Table A.8) I present the results of a placebo test, examining the effect of *future* injunctions (i.e. those that were put in place between 2008 and 2014) on civic participation. Using the same specification as Model 1, I find negative, statistically insignificant pre-trends for this group. To the extent that later-treated individuals are similar to those placed under injunction zones earlier in time, this supports the claim that my estimates are not being upwardly biased by unobserved, time-variant confounders.

These findings provide evidence of a robust causal effect on civic involvement that extends beyond electoral participation. Importantly, this provides additional support for the claim that the mobilizing effects of gang injunctions are the result of changes to *individual* political behavior as residents responded to the imposition of gang injunction policies in their neighborhoods.

Chapter 2

Exploring Mechanisms: What Explains the Mobilizing Effect of Gang Injunctions?

While I find compelling evidence that political participation increased in neighborhoods subject to anti-gang crackdowns in Chapter 1, it is unclear what is driving this effect. On the one hand, previous scholarship finds that gang injunctions had some success in curbing criminal gang activity (Grogger 2002; Ridgeway et al. 2019; O'Deane 2011), which may have in turn increased political participation in injunction safety zones by lowering the (perceived) costs and risks associated with voting (Ley 2018) and increasing trust in political institutions (Trelles and Carreras 2012).[17] However, gang injunctions were also viewed as unfair and racially discriminatory by many neighborhood residents and outside observers (Werdegar 1999; Barajas 2007), leading to widespread accusations of racial profiling of young Black and Latino men (Muñiz 2015; Werdegar 1999). Even in the absence of bias among individual officers, the fact that gang injunctions were overwhelmingly put in place in majority Black and Latino neighborhoods along with the substantial racial skew in individuals listed in police gang databases suggests that Blacks and Latinos were far more likely to be negatively impacted by this policy than other racial groups (Muñiz and McGill 2012).[18] In

17. While some individual survey evidence links crime victimization to increased political participation (e.g. Bateson 2012), I focus on the possibility that crime reduction may explain the positive effects on registrations and turnout that I find.

18. A 1992 report by the Los Angeles County District Attorney Ira Reiner found that that 47% of Black men between the ages of 21 and 24 residing in L.A. County were listed in police gang databases. As of 2012, the statewide CalGang database included over 10% of L.A. County's Black residents in that age group, though the number was likely far higher for Black men ($\approx$ 95% of individuals in CalGangs are men). This can be compared to 3.5% of Latinos and 0.3% of Whites in that age range (Muñiz and McGill 2012).

other settings, scholars find that policies viewed as unjust and unfairly targeted at certain groups can generate widespread electoral backlash (e.g. Zepeda-Millán 2016; Walker 2020a; Nuamah and Ogorzalek 2021; Garcia-Rios et al. 2023b). This may be particularly true when negative, racially concentrated policy changes are implemented in segregated urban environments, activating spatial and racial identities that can mobilize those who were not directly impacted by a policy but who feel their community is under threat (Nuamah and Ogorzalek 2021). In short, the political effects of injunction policies may have extended beyond documented cases of community activism (Muñiz 2015; Barajas 2007), mobilizing neighborhood residents who sought to end their use.

If electoral mobilization in gang injunction safety zones is being driven primarily by crime, I would expect that crime would mediate the effect of injunction policies on registrations and electoral turnout ($H1$), and I would expect residents to report lower rates of crime victimization and/or improvements in how safe they report feeling in their neighborhood after injunctions were put in place ($H2$). If instead injunctions are generating an electoral backlash, I would expect to observe an increase in negative experiences with the police ($H3$). I would also expect mobilization to be concentrated primarily among those who are most negatively impacted by the policy ($H4$). Lastly, I would expect to observe evidence of attitude change, with individuals exposed to injunction policies becoming more supportive of criminal justice reform, and less supportive of punitive police tactics ($H5$).

2.1 Data and Analysis

To evaluate these competing mechanisms I draw on a wide array of data sources, which are summarized in Table 2.1. First, to evaluate the role of crime I rely on geocoded, incident-level crime data from 2010-2020 provided by the LAPD which allow me to test whether across-time changes in registrations and voting were mediated by crime. I additionally use the two waves of the Los Angeles Family and Neighborhood Survey (L.A.FANS), which includes questions about crime victimization and perceptions of neighborhood safety. These detailed, individual-level data allow me to address concerns about crime reporting biases and the potential disconnect between crimes known to the police and how safe individuals perceive their neighborhood to be.

Second, I use self-reported, discriminatory experiences with the police measured in Wave II of the L.A.FANS survey. These geocoded data allow me to examine how injunctions changed residents' experiences with the police using within-neighborhood comparisons, and to compare the reported experiences of respondents living in injunction safety zones to respondents whose neighborhood will be covered by an injunction in the near future.

Third, I extend the analyses of registrations, voting, and non-electoral civic engagement in Chapter 1 to test for heterogeneity by race and age. For the electoral participation analysis, I create block-level estimates of new voter registrations by race and age using full-count, individual voter file data, and classify gangs by race using labels used by local law enforcement. Specifically, I use a list of active gangs produced by the Los Angeles County Probation Department, which includes a code for whether a gang is Hispanic, Asian, White, or associated with the Bloods or Crips — both labels that have been predominantly adopted by Black gangs (Juvenile Automated Index, 2022).[19]

Lastly, I measure attitudes toward the criminal justice system and policing using precinct-level election returns to a series of statewide initiatives making changes to criminal sentencing that appeared on the California ballot between 2004 and 2012. I join these data with Census, crime, and injunction-boundary data, allowing me to compare support for criminal justice reform at a low level of geographic aggregation to similar, untreated places using a selection-on-observables approach paired with placebo tests.

2.2 The Role of Crime

2.2.1 Evidence From Administrative Data

I explore whether reductions in reported crimes mediate the effect of injunction policies on political behavior using sequential g-estimation (Acharya, Blackwell, and Sen 2016). This approach estimates the controlled direct effect of injunctions on turnout when crime is set to the same level for all blocks. If the observed changes in participation are primarily explained by crime, I would expect the coefficient for $\beta INJUNCTION_{b,e,g}$ to shrink.

19. I find that the race labels used by the Probation Department align with descriptions in media and in online forums dedicated to gang life, such as `streetgangs.com`.

Table 2.1: **Data Sources and Outcome Measures**

Data Source	**Measure**	**Date Range**	**Unit of Analysis**
LAPD	Total and violent crime	2010-2020	Census block
L.A.FANS	Perceived crime and safety	2000, 2008	Individual
	Police discrimination	2008	Individual
	Non-electoral mobilization by race/age	2000, 2008	Individual
Voter File	Turnout by race/age	2018	Individual
	Registrations by race/age	2018	Census block
Statewide Database	Ballot measure support	2004, 2008, 2012	Voting precinct

Table 2.2: **Controlled Direct Effect of Gang Injunctions on Voter Registration**

	(1)	(2)	(3)
Injunction	0.121^{*}	0.121^{**}	0.121^{**}
	(0.055)	(0.039)	(0.039)
Census block FE's	✓	✓	✓
Year-by-Census tract FE's	✓	✓	✓
Proximity controls	✓	✓	✓

Note: OLS Estimates. Model 1 re-estimates Equation 1 on the subset of blocks treated after 2010, using post-2009 data. Model 2 estimates the controlled direct effect (CDE) of injunctions on registrations net block-level crime. Model 3 estimates the CDE using only violent crimes. Robust standard errors clustered by Census block given in parentheses. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

I use geo-coded crime-incident data from the LAPD to construct block-level counts of the number of crimes committed within each election cycle. Because this data is only available after 2009, my analysis is limited to the four injunctions that were put in place between 2010 and 2013.[20]

Model 1 of Table 2.2, presents the estimated effect of the four, post-2009 injunctions on registrations, which is similar in magnitude to the estimate using the entire sample. Model 2 presents the controlled direct effect of injunctions on registrations net all crimes that occur between each election cycle. The coefficient for $\beta INJUNCTION_{b,e,g}$ is identical to that in Model 1, suggesting that the mobilizing effect of injunctions is not being mediated by block-level variation in crimes

20. Data prior to 2010 is only available at the Reporting District (RD) level–a geographic unit used by the LAPD similar in size to census tracts, drawn to include roughly equal populations.

known to the police. This finding holds in Model 3 after restricting the mediator to only violent crimes (i.e. robberies, assaults, and homicides), which are generally more likely to be reported to the police (Tarling and Morris 2010), and thought to have a larger impact on civic participation (Ley 2018).

One potential explanation for this null finding is that there are strong theoretical reasons to suspect that the deterrent effects of injunctions on crime would spill over into surrounding neighborhoods. As noted by Owens, Mioduszewski, and Bates (2020), the increased criminal penalties and surveillance of suspected gang members may have deterred these individuals from committing crimes,[21] both inside and outside the safety zone (Owens, Mioduszewski, and Bates 2020). Gang injunctions also likely increased the presence of officers within the safety zones, which should deter crime nearby by reducing response times for calls for service, and by increasing the presence of officers as they traveled back and forth to the safety zone (Owens, Mioduszewski, and Bates 2020). Lastly, while individuals appear to be sensitive to high levels of crime near their place of residence (Weisburd, White, and Wooditch 2020), individual's perceptions of crime are also influenced by the characteristics of other places they spend time in throughout the day (Solymosi, Bowers, and Fujiyama 2015).[22] To the extent that individuals just outside the safety zones travel through these areas, the level of crime within safety zones should also be relevant to the attitudes and behavior of those within the same neighborhood but outside the safety zone boundary.

2.2.2 Evidence From Survey Data

While I fail to find evidence that this mobilizing effect is mediated by changes in crimes known to the police, it is possible that the increase in aggressive enforcement led to important changes either in non-reported crimes, or perceptions of safety. Previous work in criminology has found that while concentrated enforcement can reduce crime, high levels of police presence can make residents feel less safe and more concerned with crime (National Academies of Sciences, Engineering, and Medicine 2018). In both survey waves, L.A.FANS asked respondents about crime victimization

21. Given the often arbitrary criteria used to identify gang members, it is not clear if these individuals participated in more criminal activity than the general population.

22. Independently of crimes known to the police, labeling the target areas as "high crime" may have increased the perceived threat of gang-related crime within the safety zones. However, I would expect this to *reduce* civic participation rather than increase it.

Table 2.3: **Effect of Gang Injunctions on Crime Victimization and Perceived Safety**

	Crime Victimization		Perceived Safety	
	(1)	(2)	(3)	(4)
Injunction	−0.080	−0.074	−0.219	−0.037
	(0.064)	(0.053)	(0.171)	(0.164)
Ind. FE's	✓	✓	✓	✓
CBPS Weights		✓		✓
Full sample		✓		✓
N. Observations	408	2354	406	2335
N. Individuals	207	1180	207	1180
Adj. R^2	0.513	0.421	0.194	0.166
R^2 (within)	0.005	0.004	0.021	0.001

Note: OLS estimates. Models 1 and 2 present the main specification with individual and survey wave fixed effects. Models 2 and 4 expand the sample to all survey respondents, and includes covariate-balanced propensity score (CBPS) weights. All models include weights provided by L.A.FANS to account for attrition between survey waves. Robust standard errors clustered by household and Census tract given in parentheses. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

with the following question: “While you have lived in this neighborhood, have you or anyone in your household had anything stolen or damaged inside or outside your home, including your cars or vehicles parked on the street?” Respondents were also asked about how safe they felt with the following question: “How safe is it to walk around alone in your neighborhood after dark?” with a four level scale ranging from “Completely Safe” to “Extremely Dangerous.” In the following models, I use a binary indicator for whether or not individuals responded that it would be (“Completely” or “Fairly”) safe.

Table 2.3 gives the main results using the same approach used to model non-electoral civic engagement in Chapter 1 (Table 1.2). Estimates for both property crime victimization and perceived safety are small and statistically insignificant, consistent with either a null or slightly negative effect of injunctions on both outcomes. The coefficient for crime becomes significant with the inclusion of tract-by-wave fixed effects (Appendix Table A.6), suggesting that injunctions led to a 2.9% decrease in the linear probability of reporting property crime (weighted pretreatment mean = 47%). Importantly, I fail to find evidence of substantial changes in individuals' subjective experiences with crime that could plausibly explain the large mobilization effects I find.

2.3 Experiences With the Police

Next, I analyze self-reported experiences with the police, testing the hypothesis that gang injunctions led to increased incidence of discriminatory police behavior. In Wave II of the survey, respondents were asked if they had been "unfairly stopped, searched, questioned, physically threatened or verbally abused by the police" in the past five years. Because this question was not asked in Wave I, I compare within-neighborhood differences in self-reported discriminatory police encounters using Census tract fixed effects. The results in Table 2.4 suggest individuals who reside in injunction zones are significantly more likely to report police discrimination than those residing outside the injunction boundary but in the same neighborhood, even after controlling for a variety of demographic characteristics including race, age, gender, educational attainment, country of birth, family income, and welfare recipiency.[23]

To account for the possibility that gang injunctions were implemented in areas where the level of negative encounters with the police was already high, in Column 3, I leverage differences between current and future treated individuals (i.e. those whose block was placed under an injunction order after Wave 2). In place of Census tract fixed effects, I use Tract-level data from the 2000 Census to control for a variety of well-established neighborhood-level measures related to crime and concentrated poverty (Sampson, Morenoff, and Gannon-Rowley 2002) that are closely related with both the frequency and character of police-citizen interactions (Soss and Weaver 2017). This includes indices of neighborhood disadvantage, immigrant concentration, and residential stability, as well as the Black share of the population, and the crime rate between 1999 and 2001.[24]

Following previous work in sociology (e.g. Sampson, Raudenbush, and Earls 1997) I measure neighborhood disadvantage using the scores from a factor analysis with seven items: the percentage of the population living under the poverty line, families receiving public assistance income, resi-

23. The analysis sample includes both the 1193 adult panel respondents, as well as 445 child respondents who had turned 18 by Wave 2 and were thus administered the Adult Survey Module. Information on family income and welfare recipiency (i.e. SNAP benefits) are reported at the household level and come from Wave I.

24. LAPD reporting district-level crime data comes from Ridgeway and MacDonald (2017), mapped to Census tracts using GIS software. Neighborhood-level demographic characteristics are identical to those used to construct the CBPS weights for the difference-in-difference analyses.

dents with less than a high school education, residents without a college degree, population under 18, families headed by single women, and residents who are unemployed. I measure immigrant concentration using the first principle component of the Latino and foreign-born shares of the population, while residential stability is the first principle component of the share of owner-occupied housing units and residents who moved in the past five years (see Browning et al. 2017 for a similar approach).

The results are similar in magnitude to Model 2, which together suggest that injunction policies led to a significant change in the character of police-citizen interactions, with individuals becoming significantly more likely to report negative encounters with law enforcement after injunctions were put in place. These differences are large — based on the estimates of Model 3, the predicted probability that a 35-year-old Latino man with a high school education reports experiencing police discrimination is 5.9% if he lives in a future injunction zone, but 42.1% for an individual with the same traits whose neighborhood is already within a safety zone. While I lack the statistical power to determine if these differences vary by racial group, I recover similar estimates when refitting the model using only Black and Latino respondents, who make up $\approx$ 92% of treated individuals (Appendix Table A.10). In Appendix Table A.10, I also show that this effect appears to be driven almost entirely by men, who represent $\approx$ 95% of individuals in the statewide California Gang Database (CalGang) (Muñiz and McGill 2012).

2.4 Heterogeneous Responses to Injunctions

2.4.1 Non-Electoral Participation

I first examine heterogeneity in the effect of injunctions by race and age on self-reported, non-electoral forms of community involvement by replicating the analysis of L.A.FANS survey data presented in Chapter 1 (Table 1.2), interacting the post-treatment dummy with indicators for race and age. Consistent with theoretical expectations, the results, presented in Table 2.5, suggest that mobilization is greatest among Black and Latino residents, as well as those under the age of 30.[25]

25. While I find substantively similar results modeling age as a continuous variable, the sample size and small number of Asian and White respondents living in injunction zones prevent me from reliably modeling differences between specific racial groups (over 90% of individuals in the treatment group identify as either Black or Latino). However, as I note previously there are strong theoretical reasons to expect that these two groups experienced injunction policies

Table 2.4: **Gang Injunctions and Self-reported Experiences of Police Discrimination**

	(1)	(2)	(3)
Injunction	3.083*	2.545*	2.455**
	(1.293)	(1.065)	(0.773)
Age		−0.052***	−0.073*
		(0.008)	(0.031)
Male		1.990***	2.616***
		(0.340)	(0.794)
Black		0.016	1.855
		(0.411)	(1.142)
Latino		0.129	1.074
		(0.326)	(1.069)
U.S. Born		1.010***	1.344
		(0.301)	(1.083)
Food Stamps		0.109	−0.314
		(0.241)	(0.982)
College		0.338	2.961**
		(0.456)	(1.033)
No High School		0.682*	0.088
		(0.334)	(0.733)
Family Income (logged)		0.081	0.287*
		(0.061)	(0.141)
Constant			−11.388***
			(3.404)
Census tract FE's	✓	✓	
Neighborhood controls			✓
N. Observations	1534	1500	296
Pseudo R^2	0.073	0.265	0.642

Note: Logistic regression estimates. Models 1 and 2 include Census tract fixed effects. Model 3 restricts the sample to respondents living in current and future injunction safety zones. Robust standard errors clustered by household and Census tract given in parentheses. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

Table 2.5: **Effect of Gang Injunctions on Non-Electoral Participation by Race and Age**

	(1)	(2)
Injunction	0.218*	0.074
	(0.069)	(0.089)
Injunction × Black/Latino		0.212*
		(0.070)
Injunction × Under 30	0.102*	
	(0.040)	
N. Observations	408	408
N. Individuals	207	207
Adj. R^2	0.263	0.274
R^2 (within)	0.071	0.084

Note: OLS estimates. Models 1 and 3 present the main specification with individual and survey wave fixed effects. Models 2 and 4 expand the sample to all survey respondents, and include covariate-balanced propensity score (CBPS) weights. All models include weights provided by L.A.FANS to account for attrition between survey waves. Robust standard errors clustered by household and Census tract given in parentheses. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

2.4.2 Individual Turnout

I next examine heterogeneity in voting with an individual-level analysis using the 2018 California voter file, which includes information on age, gender, partisanship, date of registration, and turnout history.[26][27] To impute the race of individual voters I follow prior work which uses Bayes' Rule to calculate the probability of belonging to a given racial group, conditional on surname and geographic location (e.g. Enos, Kaufman, and Sands 2019). After merging the voter file with the US Census Bureau Surname list and block-level data on race and ethnicity from the 2010 Census, I use the *wru* package (Imai and Khanna 2016) to estimate the posterior probability that an individual identifies as Asian, Black, Latino, White, or Other Race, assigning individuals to the race with the largest

differently than other groups.

26. The Statewide Database includes counts by ethnicity, age, and gender, but does not break out counts of new registrants by race, age, and gender.

27. Because gender is an optional field on California voter registration forms, there was a high degree of missingness ($\approx$ 49%). Missing gender information was estimated using data on first names and sex assigned at birth from the Social Security Administration (SSA). For individuals with less common names that do not appear in the SSA dataset, I use the genderize.io API, which similarly assigns gender probabilities conditional on first name using social media data. Following Dion et al. (2018), I label individuals as a "woman" if ($P_{female}|$ Name) ≥ 0.7 and a "man" if ($P_{female}|$ Name) ≤ 0.3. Using these approaches I was able to assign a gender to 98% of individuals in the data, and find a high degree of concordance between these estimates and self-reported gender (error rate = 0.04).

associated probability.[28]

After subsetting the data to individuals who were registered to vote before the start of the injunction, I model turnout using a stacked difference-in-differences design similar to the block-level analysis in Chapter 1, with individual and election-by-tract fixed effects, computing counterfactual trends using only other registered voters within the same Census tract. To examine if turnout among Asian, Black, Latino, and White voters are differently affected by injunction policies, I interact a post-treatment dummy with an indicator for each racial group.

Table 2.6 presents the results, which suggest significant differences in the effect of injunctions by race and age. While the estimated effect on turnout among Whites is negative and statistically insignificant (Model 1), the estimates suggest that turnout among Black voters increased by 2%. Contrary to my expectations, however, I find significant demobilization among Latino voters, with estimated turnout decreasing 3% among this group post-injunction. In Model 2, I find a negative and significant interaction between treatment and age at the start of the injunction, suggesting a mobilizing effect among young people that decreases with age. These results are robust to the addition of individual linear trends, as well as exact-matching on pre-treatment turnout (Appendix Table A.12).

2.4.3 Registrations

While I find substantial increases in turnout among Black and young voters, I find that most of the increase in electoral participation is explained by newly registered voters (Appendix Figure A.4). Evidence that the mobilizing effect of injunctions is primarily driven by an expansion of the electorate is consistent with previous findings that salient political events can drive individuals to register to vote and subsequently participate in elections (Meredith et al. 2009; Enos, Kaufman,

28. Other scholars have found that this approach produces precise and accurate estimates (Imai and Khanna 2016), which are unbiased if an individual's surname provides no information about geographic location or other demographic characteristics after conditioning on race. While there are plausible violations of this assumption, such as correlations between class and surname within racial groups or geographic variation in rates of interracial marriage, my within-neighborhoods design makes it highly unlikely that this form of bias would lead to over-estimates of racial differences in the effect of injunctions.

Previous work has found this approach produces higher error-rates for African Americans, who are often misclassified as White (Imai and Khanna 2016). This may lead to attenuation bias that understates Black-White racial differences in the effect of injunctions; however, the high levels of racial segregation in Los Angeles increases the accuracy of these predictions (Enos, Kaufman, and Sands 2019), and my estimates for Black racial identity are reasonably precise (median probability = 0.873).

Table 2.6: **Effect of Gang Injunctions on Individual Turnout by Race and Age**

	(1)	(2)
Injunction	-0.011	0.051^{***}
	(0.007)	(0.007)
Injunction × Asian	-0.028^{***}	
	(0.006)	
Injunction × Black	0.031^{***}	
	(0.005)	
Injunction × Latino	-0.019^{***}	
	(0.004)	
Injunction × Age		-0.001^{***}
		(0.000)
Individual FE's	✓	✓
Year-by-Census tract FE's	✓	✓
Proximity controls	✓	✓
N. Observations	8175784	8175784
N. Ind.	390888	390888
Adj. R^2	0.455	0.455

Note: OLS estimates. Models 1 and 2 include both individual and year fixed effects. The omitted race category in Model 1 is non-Hispanic White. Robust standard errors clustered by individual given in parentheses. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

and Sands 2019). Again using the individual voter file, I find suggestive evidence that this increase in registrations was driven by Black and Latino residents as well as those under the age of 35.

After mapping voters to Census blocks, I use date of registration information to create block-level estimates of new registrations by race, age, and gender in a given calendar year.[29] Comparing the actual number of newly registered voters at the time of each election provided in the Statewide Database (i.e. those registered within the past two years of a given election) to my constructed estimates using address and date of registration information in the voter file, I find that the actual and estimated counts of new voters are more highly correlated in recent years than earlier ones (0.935 from 2014-2016 vs. 0.720 from 2002-2004). This may produce bias attenuating the estimated effect of injunctions toward zero, but would only introduce directional bias if the over-time discrepancy in registration counts varies between treated and untreated blocks, such as differences in moving or mortality.

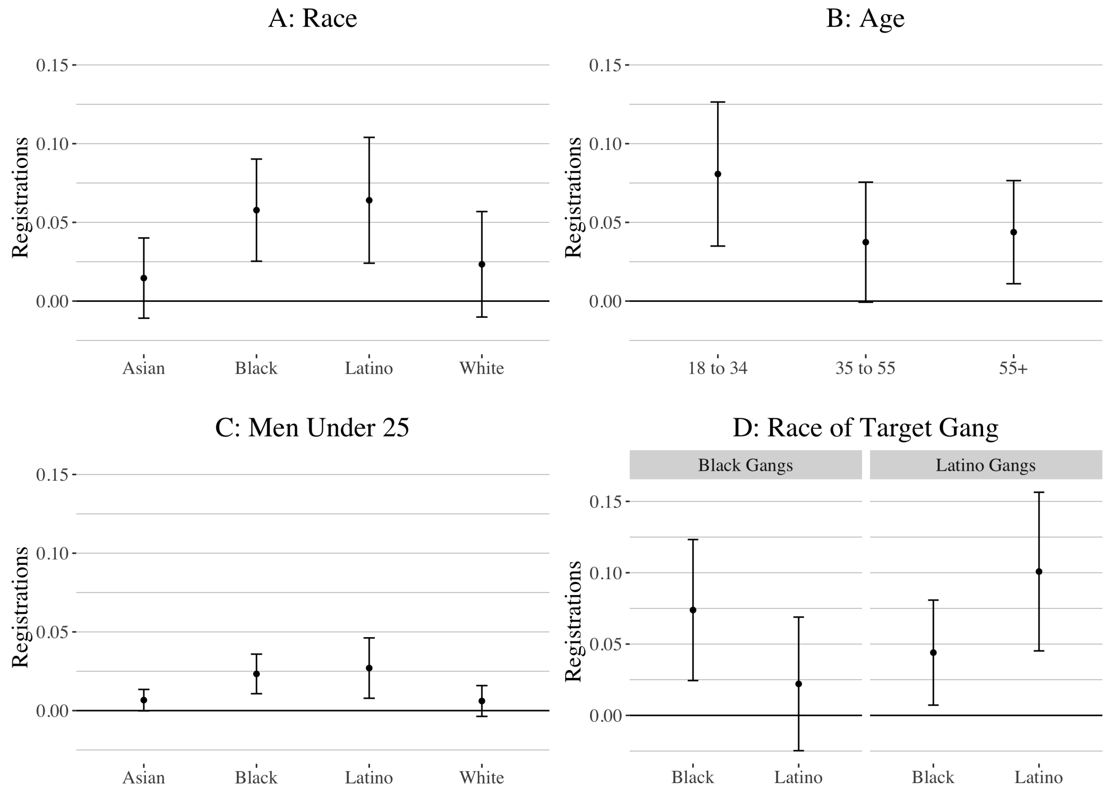
Using a version of Equation 1.1 indexed by year rather than election, I model the predicted number of newly registered voters in a given Census block and calendar year, separately by race and age. Reassuringly, I find that the overall number of new registrants increased by 7% which is nearly identical to estimates obtained using the Statewide Database, where I find that injunctions led to a 7.4% increase in newly registered voters (pre-injunction mean = 13.7).

As can be seen in Figure 2.1, I also find substantial heterogeneity by race, with the largest increases in new registrations seen among Latino and Black residents. On average, I estimate that injunction orders led to a $\approx 6\%$ increase in new Black registrations, and a $\approx 6.5\%$ increase in Latino registrations, while the estimates for Whites and Asians are small and statistically insignificant. I also find skewed patterns of mobilization by age. I estimate that injunctions increased the number of 18-34 year-olds registering to vote by 8.4% — more than twice the estimated increase among those older than 35.

In Panel C, I explore mobilization among the group most likely to be targeted by anti-gang crackdowns — young men under the age of 25. Among this group, I find racial differences that mirror the population as a whole, with significant increases in new registrations among young

29. While this approach provides far more demographic information than is available in the Statewide Database, relying on the snapshot provided by the voter file leads to undercounts of the number of newly registered voters that increases with time due to moving, deaths, and voter roll-offs (Ansolabehere and Hersh 2012; Kim and Fraga 2022).

Figure 2.1: **Heterogeneous Effects by Race, Gender, and Age**



Note: Difference-in-difference estimates of the effect of gang injunctions on new voter registrations by race, age, gender, and racial composition of target gang, along with 95% confidence intervals. See Appendix Tables A.13 and A.14 for full model results.

Black and Latino men, and no change in registrations among Asians and Whites. The increase in registrations in this group is particularly notable given the negative impact that police interactions can have on voter eligibility (White 2022).

I next leverage the racial composition of the target gangs, running separate models on injunctions that enjoined predominantly Black gangs and those that enjoined Latino gangs.[30] While contemporary gang membership is typically driven by shared experiences of marginalization rather than racial or ethnic ties (Klein and Maxson 2010; Vigil 2007), most gangs are still primarily made up of members of a particular ethno-racial group, and are stereotyped as such by the media and law enforcement (Klein and Maxson 2010). Qualitative evidence suggests that these stereotypes

30. Both models include the 6 Gang Injunction, which targeted both Black and Latino Gangs.

shaped who officers considered to be a potential gang member, and thus who was targeted with increased surveillance and enforcement in a given safety zone (Muñiz 2015). Given this, I would expect mobilization based in injustice or a sense of racialized policy threat to be greatest for Black and Latino individuals when the enjoined gang shares their ethno-racial identity.

In Panel D I find suggestive evidence that the magnitude of the effect of injunctions on Black and Latino registrations varies with the race of the target gang. The point estimates suggest that registrations among Latinos increased 10.6% in the 37 safety zones that targeted Latino gangs — while the effect of injunctions that targeted majority-Black gangs is roughly four times smaller and not statistically significant. And while the race of the target gang appears to matter less for Black mobilization, the estimate for Black registrations is larger for injunctions that specifically targeted majority-Black gangs.

Together, this suggests that injunctions were particularly mobilizing for Black residents and young people. The findings for Latinos are more mixed, with injunctions reducing turnout among registered voters, even as the number of Latinos registering to vote increased. A possible explanation for this is age — subsetting down to individuals who were already registered to vote before the injunction was put in place implies that the turnout analysis relies on a sample older than the true voting age population in the post-treatment period. Previous scholarship has found that younger Latinos express a stronger sense of linked fate (Smith, Lopez Bunyasi, and Smith 2019), which can lead individuals to interpret negative or discriminatory experiences with the police through a lens of injustice that motivates increased political action (Garcia-Rios et al. 2023a). And indeed, scholars have noted that Latino activism against gang injunctions was driven by youth, who felt targeted by the police and engaged in fundraising, protests, and registration drives (Barajas 2007).

2.5 Attitude Change and Preferences for Criminal Justice Reform

I analyze the impact of gang injunctions on political attitudes using precinct-level election returns to a series of statewide initiatives that appeared on the ballot between 2004 and 2012 (See Appendix Section A.6.1 for full list and voter guide language). Given that negative interactions with the police and distrust in the criminal justice system can undermine support for punitive criminal justice policies (Peffley and Hurwitz 2010), I selected 6 initiatives which specifically made changes to criminal sentencing. For example, 2008's Proposition 5 asked voters to limit the authority of courts to "incarcerate offenders who commit certain drug crimes, break drug treatment rules or violate parole." While others concerned changes to the state's three-strikes system, and abolition of the death penalty.

My dependent variable is the share of votes cast in a given precinct in support of harsher criminal justice punishments and an expansion of the carceral state. The key independent variable is *Injunction*, a dummy indicating whether at least 50% of the population of a precinct lived within an injunction safety zone boundary in a given election year. To capture spillover effects, I included a series of dummy variables at 500 meter intervals indicating whether the centroid of a precinct was within at least two kilometers of a safety zone boundary.[31]

Because gang injunctions are not randomly assigned, naive comparisons between treated and untreated precincts likely capture differences driven by the vast demographic, political, and socioeconomic differences between the neighborhoods that were placed under injunction orders and those with less punitive policing regimes. To bolster the claim that any differences in attitudes are driven by the injunction itself, rather than some other confounding variable, I condition my estimates on a rich set of precinct-level characteristics.

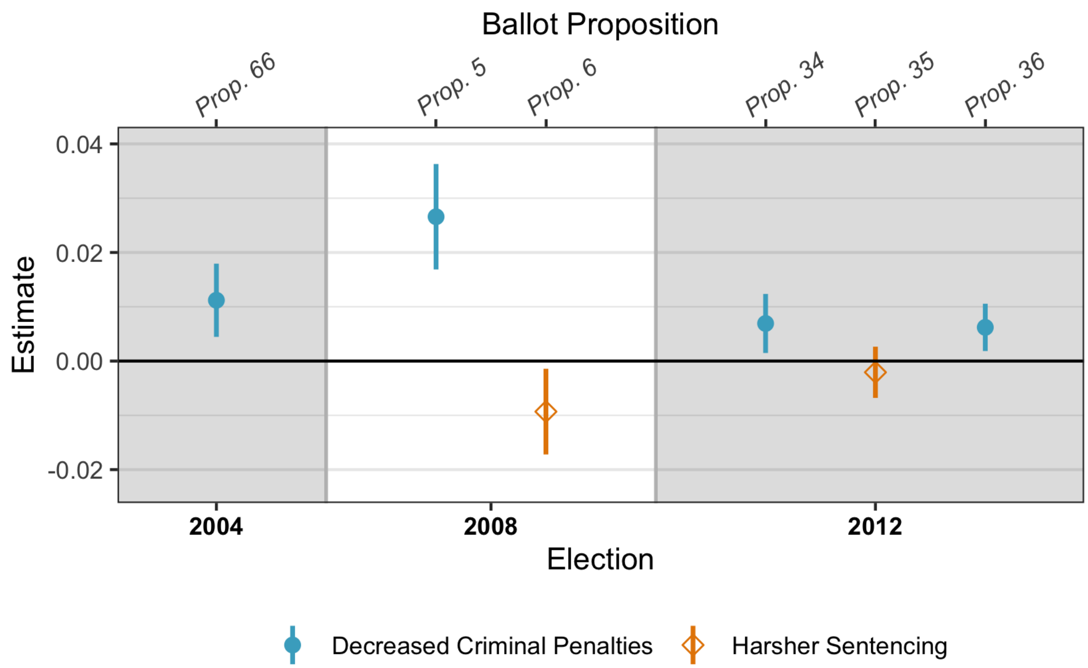
First, I control for partisanship and a broad range of other policy preferences using presidential vote returns and all other statewide initiatives that were on the ballot in the same year. These propositions allow me to control for a wide array of preferences on other issues, including taxation, education, healthcare, and the environment.[32] Additionally, I include a wide array of demographic and socioeconomic controls, conditioning on precinct characteristics that may be related to both the propensity for a neighborhood to be placed under an injunction order and preferences toward criminal justice policy.[33] Lastly, I control for crime (i.e. the number of aggravated assaults,

31. Precinct-level returns come from the California Statewide Database, which I merged with Census tract-level demographic and socioeconomic Census data using provided relationship files. Injunction safety zone boundaries and distances were mapped onto precincts using GIS software.

32. Because scholars have often theorized that these negative experiences with government might reduce support for other types of government services as well, this represents a conservative specification.

33. Specifically, I control for the total number of votes cast, total registrations, total precinct population, percentage Black residents, percentage Latino residents, percentage White residents, percentage Asian residents, poverty rate, unemployment rate, median income, percentage naturalized citizens, percentage non-citizen residents, and percentage of residents over 25 with either a high school diploma or bachelor's degree. For the 2004 and 2008 elections,

Figure 2.2: **Support for Criminal Justice Reform**



Note: OLS Estimates with 95% confidence intervals. Separate models fit for each ballot proposition with controls for partisanship, vote returns for the presidential race, vote returns for non-criminal-justice-related ballot initiatives, total number of votes cast, total registrations, total precinct population, poverty rate, unemployment rate, median income, crime rate, and percentage of residents who are: Asian, Black, Latino, White, naturalized citizens, non-citizens, and have a high school diploma or bachelor's degree.

homicides, burglaries, and robberies) reported by the LAPD in the three reporting quarters before the election.

The results in Figure 2.2 reveal that neighborhoods that are covered by gang injunction orders are on average 1% less (more) supportive of punitive (lenient) criminal justice policies, even after the inclusion of a wide array of demographic, economic, and attitudinal controls. In 2008, the coefficient for *Injunction* suggest that gang injunctions increased support for *Prop. 5* by 2.7%, and decreased support for *Prop. 6* by 0.9%. These results are similar when modeling gang injunction exposure as a percentage rather than a binary variable, and other forms of spatial controls.

demographic and economic estimates come from the 2000 decennial Census. For the 2012 election, demographic information come from the 2010 decennial Census, while data on economic characteristics was taken from the 2012 American Community Survey 5-year estimates.

Of the six ballot questions I analyze, in only one case (Prop. 35) is gang injunction coverage not related to support for the measure at $p < 0.05$. This null result is potentially explained by the nature of the initiative, which combined harsher criminal justice penalties for those convicted of human trafficking with additional services for victims and mandatory training for police officers. The measure was endorsed by a wide swath of civil society including both the California Democratic and Republican parties, received little organized opposition, and was overwhelmingly popular, with 81.35% of voters voting in support.

I next leverage the staggered roll-out of gang injunction safety zones, rerunning my analyses of the 2004 and 2008 elections with precincts that were not covered by an injunction, but which would be in the future. If gang injunctions are having a causal effect on attitudes, rather than merely being put in place in neighborhoods where support for punitive criminal justice policies were already low, we would expect initiative support to be unrelated to future treatment status. The results of this placebo test, presented in Appendix Table A.15, suggest that being placed under an injunction in the future is not significantly related to current precinct preferences.

2.6 Discussion and Conclusion

In many poor, racially segregated neighborhoods in the United States, law enforcement relies on aggressive and punitive tactics that are seen by many as discriminatory and unjust, and which often fail to meaningfully control crime. Rather than turning to the ballot box to call for change, theories of legal estrangement suggest that many members of highly policed communities may respond by turning further away from formal politics, ensuring that those who are most negatively impacted by law enforcement are also the least likely to have their voices heard in elections. Here, I provide causal evidence that anti-gang crackdowns in Los Angeles led to strikingly large increases in both voting *and* non-electoral forms of civic participation. Consistent with a backlash effect, these effects were driven by new registrations among Black and Latino individuals — groups which have some of the highest exposure to police abuses and violence — and were accompanied by increases in both self-reported police discrimination and support for criminal justice reform.

These findings extend previous work on the mobilizing effects of rare and extreme acts of police violence (e.g. Ang and Tebes 2024; Morris and Shoub 2024) to the day-to-day tactics and behaviors of the police. The mobilization I find stands in contrast with much of the previous correlational work examining how preemptive and intrusive police tactics shape the civic lives of residents in heavily policed communities, and points to the ways in which proximal contact with the police can spur political action (Walker 2020a). While gang injunctions mirror many of the tactics that previous scholarship has examined in poor communities of color, particular characteristics of these policies or contextual factors may have made them particularly mobilizing. "Aggressive" and "preemptive" policing covers a wide range of tactics which vary in their ability to control crime (National Academies of Sciences, Engineering, and Medicine 2018), and likely have very different impacts on political behavior. Gang injunctions may have been particularly mobilizing given that their highly publicized nature and well defined geographies made it very clear which communities were being targeted with infringements on civil liberties, and because they had limited impacts on reducing crime. Future work might explore how the effects of gang injunctions compare to other types of place-based police tactics, and to examine whether community responses to gang injunctions are similar across neighborhoods and municipalities with different preexisting networks of community organizations and different histories of activism and police-community relations.

My results also contribute to our understanding of political behavior in highly policed communities, and to how local, negative policy changes can spur political action in communities that lack the social and economic resources typically associated with voting (Nuamah and Ogorzalek 2021). While I find that injunction policies led to increased participation in statewide and national elections, future work should explore if this extends to local races, whether voters are able to demand accountability by linking their experiences with law enforcement to the elected officials ultimately responsible for setting policy, and whether these electoral demands produce meaningful policy change.

Chapter 3

What Randomized Policing Experiments Can Teach Us About the Political Effects of Policing

3.1 Introduction

Studying the political effects of policing poses a variety of challenges — what it means for a neighborhood to be "policed" is difficult to define and measure, and law enforcement's close relationship with crime and neighborhood disadvantage raises substantial barriers to causal identification. And while existing theory and evidence has been motivated by the civic consequences of high-profile cases of police harassment and violence, long-standing research in criminology finds that many of the tactics that scholars have classified as "aggressive policing," fail to produce significant backlash effects and can even make residents feel safer (National Academies of Sciences, Engineering, and Medicine 2018), raising questions about how to interpret changes in political attitudes and behavior that result from to high levels of police activity.

In this Chapter, I address these challenges by exploiting sudden, exogenous changes in the presence, training, and tactics of the police created by a large, randomized policing experiment. Conducted by the Police Executive Research Forum (PERF) in 2009, this experiment targeted 83 "hot-spots" of violence in Jacksonville, Florida, assigning these areas to three conditions: 1) "business as usual" policing, 2) a "Saturation" condition in which police patrols were increased to an average of 7 hours a day, and 3) a "Problem Oriented Policing (POP)" condition in which officers were trained to reduce their reliance on enforcement actions, instead spending their time engaged in community outreach and coordination with other city agencies to address the root causes of crime (Taylor, Koper, and Woods 2011). Random assignment and granular measures of officer behavior allow allow me to separate the effect of two very different styles of policing interventions at crime hot-spots from residential sorting and other place-based drivers of voter behavior. Comparing voting rates between the treated and control conditions, I answer the question: "how to voters respond to significant increases in police presence on their street?" and "do the actions that officers take meaningfully moderate these responses?"

I find that following the experimental intervention, voter turnout increased in both the Problem Oriented Policing and Saturation condition. While the effect of the two interventions was similar, the size of the effect varied by both the type of election and voter race. I found no increase in turnout in the 2010 statewide election, but find a 2-3% increase in voting participation in the off-cycle, local election that occurred four months later. This increase is driven by Black voters, who represent nearly 75% of voters living in the experimental hot-spots. I argue that these increases are unlikely to be explained by direct voter backlash to the actions of the police, but are consistent with a variety of other mechanisms, including an increased sense of safety coming from improved crime control, or an increase in the salience of crime which may have prompted residents to engage more deeply in a local election where public safety was a key campaign issue. Together, this work suggests that neighborhood-level exposure to high levels of police activity can have a meaningful effect on turnout, particularly in local elections, but these effects are theoretically ambiguous and should prompt political scientists to consider a wider range of possible mechanisms when studying the political effects of policing.

3.2 Policing, Place, and Political Behavior

To maximize efficiency, minimize response times, and proactively prevent crime, most law enforcement agencies utilize some combination of place-based approaches which focus police resources on "high risk" places with elevated crime rates (National Academies of Sciences, Engineering, and Medicine 2018). At the individual-level too, officers have been shown to behave differently depending on the neighborhood they are patrolling in (Sun, Payne, and Wu 2008). Together, this has generated vast inequalities across neighborhoods in where officers spend time (Chen et al. 2023), and how they treat members of the community (Fagan et al. 2016; National Academies of Sciences, Engineering, and Medicine 2018). These place-based differences in the character of policing are thought to have profound effects on residents, particularly in neighborhoods where police-citizen interactions are common (Braga, Brunson, and Drakulich 2019; Soss and Weaver 2017). Scholars have linked policing that is considered violent, racially biased, punitive, predatory, and/or extractive to low levels of police legitimacy (Fagan, Tyler, and Meares 2016; Braga, Brunson, and Drakulich 2019) as well as to acute psychological distress, physical ailments, and reduced educational attainment (Bacher-Hicks and Campa 2020; Rios 2011; Legewie and Fagan 2019; Brunson 2007).

In addition to generating significant negative effects among those who are arrested or subject to police abuses, these tactics are thought to generate substantial community-wide impacts as individuals hear about the behavior of the police through their personal networks, media coverage, and daily observations (Morris and Shoub 2024). Building from this work, a growing body of research has sought to uncover the *political* effects of policing, with both observational and qualitative work linking place-based differences in policing to voting (Palmer 2024; Laniyonu 2019), non-electoral participation (Walker 2020b; Anoll, Epp, and Israel-Trummel 2022), engagement with government agencies (Lerman and Weaver 2014), and attitudes toward democracy and the state (Weaver, Prowse, and Piston 2020).

Despite the immense scholarly and social interest in these questions, debates over when, why, and how place-based differences in policing influence political behavior remains largely unsettled. Mixed findings in existing work speak to the difficulty of studying the effects of both policing and place, and to the limitations posed by existing approaches to identification, measurement, and theory.

3.3 Challenges of Separating Policing From Place

All observational research on the effects of context faces issues with confounding, but these issues are particularly difficult to overcome in the study of policing. Neighborhoods that experience different levels of policing vary in profound ways, with the activities of the police closely connected to demographic and socioeconomic factors such as neighborhood disadvantage, physical disorder, and racial segregation (National Academies of Sciences, Engineering, and Medicine 2018). These differences raise obvious concerns about omitted variable bias when making static comparisons across place. There are also strong reasons to think that police behavior contributes to these differences, making it difficult to determine what controls are appropriate to include. For example, housing instability might confound the relationship between police surveillance and electoral participation, given that evictions can create substantial social and economic costs that reduce electoral participation (Slee and Desmond 2023), and the police are more likely to patrol in poor neighborhoods where evictions are high. But conditioning on housing instability would produce biased estimates in cases where police activity was also contributing to aggregate eviction rates. This might occur if aggressive police behavior makes a neighborhood less desirable, leading individuals who are easily able to rent elsewhere to move out and increasing the proportion of individuals in the neighborhood with past eviction records or other issues finding housing.

Panel data may address some of these issues by enabling comparisons between changes in voting behavior and policing across time, but is unable to account for unobserved, across-time variation in characteristics that could drive both police behavior and turnout, such as neighborhood disorder (e.g. Brown and Zoorob 2022; Michener 2013) or crime waves not adequately captured in administrative data due to under-reporting.

3.4 Challenges in Defining and Measuring the Treatment

Theories of policing's effects often make reference to the presence, tactics, and racial bias of officers in a given neighborhood, yet researchers often lack the data to credibly measure these quantities. Research has often relied on subjective evaluations of residents (e.g. Walker 2020b; Weaver, Prowse, and Piston 2020; Weitzer and Brunson 2009), which provides valuable information on how individuals experience policing in their communities, but makes it difficult to demonstrate that these experiences *cause* changes to political behavior. Other work has used administrative police data on stops and arrests to define the treatment of interest, such as neighborhood-level exposure to “aggressive policing” (Lerman and Weaver 2014; Palmer 2024; Laniyonu 2019). These records give a more direct measure of police enforcement activity, but they lack crucial information about the processes that produced the observed patterns of arrests, including how much time officers are spending on patrol in a given area, the types of offenses they are prioritizing, and the decision rules they are using to initiate stops. Without this information, any inferences drawn from administrative data must rely heavily on the researcher’s assumptions about what the police are doing — when these assumptions are off, inferences may also be invalid (Neil and Winship 2019).

Two examples illustrate these points. Using police administrative data from New York City, Lerman and Weaver (2014) take hit rates (i.e. the percentage of stops that result in arrest or the discovery of contraband) as a measure of the character of policing in a given neighborhood, theorizing that aggressive policing will induce avoidance of government institutions. Their assumption is that lower hit rates indicate officers are using lower standards for searching individuals, which can serve as a proxy for the level of police disrespect, rudeness, or harassment that residents experience.

While low hit rates may indicate lower evidentiary standards, this approach assumes away any other plausible explanation for these differences. The average hit rate on police stops is known to be influenced by a variety of factors, including the severity of offenses that the police are pursuing, the distribution of stops across individuals (i.e. whether officers are repeatedly stopping high risk individuals such as gang members or people on the street at random), and the interaction between population-level characteristics and police decision rules (Neil and Winship 2019). Even if low hit rates were correlated with the construct of interest (i.e. police aggression), measurement errors from these other factors could introduce severe directional bias. For example, residents of many disadvantaged communities report going out of their way to avoid officers on the street due to past negative encounters with law enforcement (Weitzer and Brunson 2009), or historic legacies of racial discrimination in the criminal justice system (Pickett, Graham, and Cullen 2022). If the police are using “furtive movements” as grounds for suspicion to initiate stops, this fear among residents may lead to lower hit rates as nervousness or other signs of evasion become less predictive of criminal wrongdoing. In this scenario, differences in hit rates would be the consequence, rather than the cause of our outcome of interest—avoidance of the police or other government institutions.

Other scholars instead rely on the geographic clustering of stops and arrests to measure how policed an area is (e.g. Palmer 2024). Even if we are able to fully account for neighborhood characteristics that could confound the relationship between arrests and political behavior, these data again do not tell us *why* the police are arresting more people in a given place and cannot account for the fact that the processes generating arrests likely differ across neighborhoods, making it unclear what exactly is being measured or how we should interpret the subsequent findings.

In this study I take a different approach, rather than attempting to measure policing directly through police-citizen interactions, I use experimental interventions that carefully control the training and patrol assignments that officers are given. In line with evidence on the importance of institutional directives in determining officer behavior (e.g. Mummolo 2018; Owens et al. 2018) these interventions lead to rapid, well-defined changes in both the presence and behavior of the police.[34]

3.5 Theoretical Challenges

A variety of mechanisms are thought to connect policing to community-level differences in political attitudes and behavior. Building from research on policy feedback and political socialization, scholars have argued that the behavior of the police might change the perceived value of voting by altering beliefs about democracy and political efficacy (Lerman and Weaver 2020; Weaver, Prowse, and Piston 2020), institutional quality and responsiveness (Anoll, Epp, and Israel-Trummel 2022), or the personal and group-based threats posed by government institutions and policies (Garcia-Rios et al. 2023b; Lerman and Weaver 2014). In short, the quality and character of policing may signal to residents how likely the government is to listen to their needs, how effective it is in addressing their concerns, and how likely it is to respect the rights of "people like me."

In addition to these political socialization effects, policing is thought to have substantial material and social effects on neighborhoods that are closely connected to political participation. The tactics used by the police and their ability to control violent crime are thought to be tied to residents' feelings of social isolation, sense of belonging in their neighborhood, and perceptions of collective efficacy (Rinehart Kochel and Gau 2021; Moffett-Bateau 2023). Arrests may also undermine community social networks and residents' access to social capital, particularly when highly spatially concentrated (Burch 2013; Palmer 2024).

34. A focus on institutional directives also ensures that work is policy-relevant.

While these different mechanisms are not mutually exclusive, they make interpreting the effects of policing challenging, add uncertainty around whether we should expect policing to mobilize or demobilize, and point to a variety of channels that may produce observationally equivalent outcomes. Existing scholarship in political science, heavily influenced by research into the direct effects of arrests and incarceration, has largely focused on socialization mechanisms, particularly the idea that community exposure to aggressive policing depresses turnout by generating beliefs that that the government is cruel, unresponsive, or threatening (Lerman and Weaver 2020; Brayne 2014). But most of this work is unable to isolate trust in the police and government as the causal force. It is difficult to measure changes in policing and attitudes simultaneously over time, and scholars typically lack measures to adequately account for policing's other social and material effects.

3.6 Contribution of This Study

I draw on the large experimental literature in criminology along with geo-coded voter files to identify the effect of police behavior on voting in high crime neighborhoods. I argue that these experimental interventions provide a rare opportunity to causally identify the effect of police behavior. Analyzing a "hot-spots" policing intervention conducted by the Jacksonville, FL, Sheriff's Office, I demonstrate that voters are sensitive to sudden shifts in police behavior. Importantly, information on the training officers received and the actions they took while on patrol allows me to compare the effects of two very different styles of policing — one that primarily increased police surveillance and enforcement, and another which encouraged community outreach and alternate approaches to crime control. The results suggest that contrary to expectations in much of the existing literature, both styles of hot-spots policing can increase turnout, and these increases do not appear to be driven by backlash effects.

A unique element to this approach is that it compares sudden, geographically limited shifts in police behavior, rather than highly publicized, city-wide policy changes such as SQF (Palmer 2024) and gang injunctions (see Chapter 1), or longer-term, place-based differences in the character of policing. Existing theory has largely been silent on whether the duration or geographic extent of a particular policing intervention may induce the same political response, but there are good reasons to believe that these changes would impact voting behavior. Existing (quasi-)experimental work in criminology finds that even short of interventions have led to substantial drops in criminal activity (National Academies of Sciences, Engineering, and Medicine 2018), and can have meaningful impacts on how residents view the police (Braga and Bond 2008).

3.7 Setting: The Jacksonville Policing Experiment

The Jacksonville Policing Experiment was a collaboration between the Police Executive Research Forum (PERF) and the Jacksonville Sheriff's Office (JSO), designed to compare the efficacy two styles of policing—saturation patrol and problem oriented policing—relative to standard police tactics in violent crime hot-spots.[35] Saturation, or directed patrol policing involves dramatically increasing the presence of the police in a given place by assigning officers to patrol small geographic areas and freeing them from responding to calls for service. This might be considered a "dosage" intervention, as the amount of patrol time an area receives is altered, but officers are not given specific directives on *how* to police those areas or what activities the assigned beat officers should engage in to control crime.

Problem Oriented Policing (POP), on the other hand, directly concerns the tactics that officers engage in when they spend time in crime hot-spots. At its most basic, POP is a proactive, rather than a reactive style of policing, where officers are encouraged to identify the underlying causes of criminal activity in a given context and to use innovative solutions to address those problems (Cordner and Biebel 2005). POP has become one of the most widely adopted policing innovations in American police departments, supported by federal agencies, national policing groups, and dedicated centers (Hinkle et al. 2020). While scholars have reported a variety of shortcomings in its real-world application (Cordner and Biebel 2005), the canonical framework for implementing POP is the Scanning, Analysis, Response, and Assessment (SARA) model proposed by Eck and Spelman (1987). Using SARA, officers are encouraged to systematically seek out information to identify

35. A large body of evidence in criminology finds that criminal activity is highly spatially clustered, with the majority of reported crimes in a given jurisdiction occurring in a small subset of micro-geographic units (e.g. specific addresses, intersections, or city blocks) known as hot-spots. This finding has held across a diverse array of places and across time, leading some scholars to refer to this tendency as "law of crime concentration at places" (Weisburd 2015). These hot spots are highly stable over time and are small enough to suggest that most micro-places, even in "high crime" neighborhoods, are relatively free of crime (Weisburd, Groff, and Yang 2012).

problems, analyze these problems to develop the most promising solutions, and then evaluate the effectiveness of these actions. For example, a police agency might find that gang violence is on the rise in their jurisdiction (Scanning), and then direct investigatory resources toward identifying violent offenders and social ties that explain the observed patterns of violence (Analysis). Using the results of this analysis, the agency or relevant division might choose to use a "pulling levers" strategy, increasing surveillance and sanctions of likely offenders and working with outreach workers to connect gang members to social services and other opportunities (Response). After the implementation of these measures, the agency might compare violence among the gangs to overall trends in violent non-gang crime to determine if the response is having the intended effect (Assessment).

Because POP is considered a process for approaching crime prevention, rather than a set of specific strategies, the responses officers develop can vary widely. Some POP interventions have led to wide-scale enforcement crackdowns and concentrated misdemeanor arrests (e.g. Braga et al. 1999); however, the focus on the "root causes" of crime often encourages the police to engage in greater levels of community outreach and non-enforcement activities than traditional, reactive police tactics. This is especially true in the case of the Jacksonville experiment, where officers were strongly encouraged to use alternatives to enforcement and to coordinate with community groups and other government agencies to address the issues they identified (Taylor, Koper, and Woods 2011).

Using Unified Crime Reports (UCR) data from 2006-2008, the PERF research team identified 83 spots of street violence (i.e. non-domestic violent crime incidents) in Jacksonville, FL (Taylor, Koper, and Woods 2011). These 83 micro-places—which included intersections, street segments, and specific addresses in addition to a 100ft buffer zone—averaged 0.02 square miles and included both residential and nonresidential areas (159). In the Saturation/Directed Patrol condition, pairs of officers in separate cars were assigned to patrol hots spots (n = 21) based on a consistent schedule created by the JSO designed to maximize coverage of high-risk times and locations for violence. Supervision of officers was focused on time spent in the assigned beats, with the average hot spot receiving 53 hours of directed patrols per week (157).

In the Problem Oriented Policing (POP) condition (n = 22), officers received three days of training in POP and intelligence-led policing and were assigned to small teams to work in crime hot spots alongside a dedicated crime analyst. Officers were encouraged to explore the root causes of violence in hot spots and were given wide discretion in trying to solve these issues, implementing over 200 different interventions across the intervention sites (157 - 158). According to Taylor, Koper, and Woods (2011), the most common interventions were situational crime prevention measures, such as installing street lights and road barriers, and improving the security of specific properties (158). Other POP measures were connected to "community organizing (e.g., conducting community surveys and other forms of citizen outreach), social services (e.g., improving recreational opportunities for youth), code enforcement, aesthetic community improvements (e.g., removing graffiti or cleaning up a park), and nuisance abatement" (158). While officers used criminal investigations and increased enforcement in some hot spots, this was discouraged by project supervisors in the sheriff's department whenever possible. Supporting this assertion, the PERF researchers found significant increases in discretionary or "self-initiated" police activity (e.g. investigations, traffic stops, and voluntary contacts) in the POP treatment sites, but did not find significant increases in investigatory detentions or arrests as compared to baseline (163 - 164).

The directed patrol and POP interventions occurred concurrently over a period of 90 days between January and April 2009, with an additional 90 day post-treatment evaluation period to assess the longer-term effects of the interventions (Taylor, Koper, and Woods 2011). To ensure adherence to the treatment assignment, the location of officers and their activities were tracked by police supervisors and independently validated by the PERF researchers through extensive interviews and "ride-alongs" with officers on patrol (159). During the 90-day intervention period, officers were only observed leaving their assigned beats in rare, emergency situations (Taylor, Koper, and Woods 2011).

Based on an analysis of UCR data in the 90 day period following the intervention, Taylor, Koper, and Woods (2011) find that the POP intervention led to a significant, 33% decrease in street violence relative to the 40 control hot-spots, as well as reductions in property crime and calls for service that were in the expected direction but not statistically significant. While crime decreased in the expected direction in hot-spots covered by the directed patrol intervention, crime appeared to rebound to pre-treatment levels in the three months following the conclusion of the experiment (Taylor, Koper, and Woods 2011).

Several proposed explanations for these statistically insignificant effect sizes include the short duration of the intervention — in contrast POP interventions in Jersey City and Lowell, MA, that generated more statistically robust decreases in crime both lasted over one year (Braga et al. 1999; Braga and Bond 2008). While not a unique concern to this intervention, the researchers also present evidence of shifts in crime reporting patterns that may have muted the results. This is particularly concerning in the POP condition given that increased community outreach may have encouraged residents to report crimes to the police.

Despite the mixed findings related to crime, it is likely that the increase in police activity and community outreach was noticed by residents, and may have had consequences for political behavior in the surrounding areas. Other experimental hot spots interventions have found that residents are able to recognize increases in police presence and efforts to address local crime and disorder problems in their neighborhood (e.g. Braga and Bond 2008; Bryant, Collins, and Villa 2014). In some cases, these interventions can shift the public's evaluations of neighborhood conditions, the police, and local officials, as well as their willingness to cooperate with officers (Bryant, Collins, and Villa 2014; Hinkle and Weisburd 2008; Braga 1997; Blattman et al. 2021; Kochel and Weisburd 2017).

3.8 Theoretical Expectations

Using the hot-spots policing treatments from the Jacksonville Policing Experiment along with election turnout in the 2010 and 2011 elections as my main outcome, I test the following hypotheses derived from the existing theoretical and empirical literature on policing and political participation:

> **H1: The Demobilizing Effects of Aggressive Policing.** Crime hot-spots that received the Saturation/Directed Patrol treatment were under increasd surveillance by officers for over 7 hours a day, and experienced an 85% increase in investigatory stops ($p < 0.01$) along with a $\approx$ 200% increase in traffic stops and other investigatory police actions ($p < 0.001$) (Taylor, Koper, and Woods 2011, 166). Existing scholarship argues that constant surveillance in highly policed communities can send messages that the government is a repressive and invasive authority that should be avoided (Brayne 2014; Lerman and Weaver 2020). This is thought to be particularly true of investigatory stops, which have been described by scholars as

"detain[ing] people simply to see what they're up to, who they are, where they are going, and why" (Soss and Weaver 2017), and are often viewed as a form of arbitrary, biased surveillance, particularly among Black individuals who are subjected to these stops at far higher rates (Epp, Maynard-Moody, and Haider-Markel 2014). In line with these accounts, experimental evidence from St. Louis finds that this style of Saturation/Directed Patrol policing can lead to decreases in perceived procedural justice and police legitimacy, though these effects were short-lived (Kochel and Weisburd 2017).[36] *I therefore expect electoral turnout to either remain constant or to decrease among voters exposed to the Saturation intervention relative to voters in the control condition who did not experience these changes.*

H2: Demobilization and Intervention Type. While crime hot-spots that received the Problem Oriented Policing (POP) treatment similarly saw large increases in police activity, officers patrolling these areas were discouraged from relying on involuntary stops and arrests, and engaged in a variety of alternatives to punitive policing that sought to address the "root causes" of violence in the community. After conducting community outreach to get a sense of the problems the community was facing, officers coordinated with community groups, property owners, and other city agencies to increase access to social services, improve physical security infrastructure (e.g. fixing fences, installing street lights, and erecting road barriers), and remove physical signs of disorder (e.g. trash and graffiti removal) (Taylor, Koper, and Woods 2011, 158). To the extent that residents perceive these changes in police activity (Weisburd et al. 2011), higher levels of officer presence and community engagement are associated with increased trust in, and satisfaction with, the police (Koper et al. 2022). Voters may tie these improved views of the police to their broader evaluations of the performance and responsiveness of the state (Anoll, Epp, and Israel-Trummel 2022) in ways that would encourage greater rates of engagement. Taylor, Koper, and Woods (2011) also find that the POP was effective at reducing rates of violent crime, which is thought to be a key barrier to civic/community participation in high-crime neighborhoods (Moffett-Bateau 2023). Because of this, *I expect voting participation to remain constant or increase in the areas targeted by*

36. Similar to the crime hot-spots in Jacksonville, the experimental intervention in St. Louis occurred in predominantly low income, majority-Black neighborhoods.

the POP intervention.

I additionally consider the role that race and election type might play in moderating these effects:

H3: Demobilization and Race. Stark racial differences exist in both attitudes toward, and experience with, the police. Black Americans consistently report lower levels of trust and satisfaction with the police than other racial groups (Pickett, Graham, and Cullen 2022), differences that are thought to be deeply rooted in childhood socialization and cumulative, racialized experiences with the police and criminal justice system (Peffley and Hurwitz 2010; Western 2006; Weaver, Prowse, and Piston 2020; Gibson and Nelson 2018). *These widely divergent attitudes and experiences with the police strongly suggest that the effect of the intervention should vary by race.* However, the direction of this effect is not clear:

H3(a): Prior attitudes or expectations about the police may act as strong priors that reduce the impact of new experiences among Black individuals (Anoll and Engelhardt 2023; though see Dan-Irabor, Slocum, and Wiley 2023). If so, *we might expect that changes in electoral turnout to be smaller for Black voters than for other groups.*

H3(b): Conversely, Black individuals may be more likely to be directly impacted by the increase in police stops and surveillance during the Directed Patrol intervention, and to interpret involuntary police encounters through the lens of racial discrimination (Epp, Maynard-Moody, and Haider-Markel 2014). In the POP condition, increased responsiveness and engagement from police officers may have a greater impact on Black voters by diverging from their prior views of police officers (Christiani and Shoub 2022). If so, *we might expect that the (de)mobilizing effects of hot-spots policing to be greater for Black voters, particularly when compared to Whites.*

H4: Demobilization and Election Type. While negative experiences with the police and criminal justice system are often though to generate a general aversion to government (e.g. Brayne 2014; Lerman and Weaver 2020), there are strong reasons to expect that the effects of policing should be particularly influential in altering behavior in *local* elections. Local governments are ultimately responsible for police policy and personnel decisions, and public safety is consistently a central question of local elections (Brown and Zoorob 2022). This was true in the case of Jacksonville, where crime, public safety, and police funding were considered key issues in the 2011 mayoral race (*Florida Times-Union* 2011). In addition to the mayoral ballot, this election included races for a variety of important local offices including Duval County Sheriff and 14 of Jacksonville's 19 city council seats. While issue salience may further mobilize voters interested in issues of crime and policing, off-cycle, local elections place substantial information burdens on residents (Hochberg and Hersh 2023), which may further discourage disengaged voters from participating. Together, this suggests that *changes to electoral behavior should primarily be seen in local, vs. national or statewide elections.*

3.9 Empirical Strategy

3.9.1 Data

Data on voting participation comes from the 2012 Florida voter file, which also includes information on each voter's race, sex, partisan affiliation, date of registration, and date of birth.[37] To identify voters who resided in the experimental intervention sites, I geocoded voter addresses using the ArcGIS USA Geocoder API, and then merged these records with shapefiles of the 83 violent crime hotspots which were provided by the Jacksonville Sheriff's Office. Of the 593,793 voters registered in Jacksonville in both 2008 and 2012, over 99.4% were successfully geocoded.[38]

I identify a total of 10,234 registered voters whose address lies within one of the 83 crime hotspots identified by the JSO. As can be seen in Table 3.1, these voters are far more likely to be Black and to be registered as Democrats than the broader population of Jacksonville. Voters in these areas are also younger, more likely to be women, and less likely to have turned out to vote in the previous election. While these hot-spot locations are not representative of the broader city

37. To avoid issues of post-treatment registration bias or selective mobility in response to the experiment, I focus my analysis on voters who were registered to vote before the 2008 election, several months prior to the start of the experiment. While there are likely errors in these records, for example a voter's address may not have been properly updated between moves, it seems highly unlikely that this type of measurement error would be correlated with treatment assignment.

38. A small number of individuals had either missing or non-matching addresses. Florida state law allows a limited number of individuals to petition for their addresses to be removed from the public voter file, including recent victims of stalking, abuse, and domestic violence, as well as "high risk professionals," such as investigative personnel and other government employees whose work "could lead to criminal prosecution or administrative discipline" (Florida State Code s.119.071(4)(d)2.r).

Table 3.1: **Descriptive Statistics of Registered Voters Living Within Crime Hot-Spots**

	Experimental Hot-Spots	Jacksonville
POP Treatment	39.4%	-
Saturation Treatment	25.2%	-
Voted in 2008	63%	66.1%
Black	73.6%	28.4%
Hispanic	3.3%	3.9%
White	18.6%	62.3%
Democrat	74.2%	43%
Republican	11.1%	36.6%
Male	38.4%	45.5%
Age (mean)	40	43
Total	10234	593793

Note: Percentages for gender and race are calculated among those without missing data. The missingness rate for both race and gender is 1.4%. I control for missingness in the models that follow.

population, they match closely with the existing theoretical and empirical literature on the close relationship between crime, segregation, and racialized poverty (Sampson, Raudenbush, and Earls 1997) and the concentrated use of preemptive policing tactics in poor communities of color (Walker 2020b; Soss and Weaver 2017; Gelman, Fagan, and Kiss 2007).

3.9.2 Empirical Strategy

Although random assignment ensures that the experimental conditions are balanced across covariate values in expectation, the cluster, or hot-spot-level assignment of the policing interventions creates two challenges in estimating their effects on turnout. The first is that the clusters do not contain equal numbers of voters—ranging from 0 to over 600.[39], which leads to population imbalances across experimental conditions (see Appendix Table B.1). Because of this variability, the difference-in-means estimator does not give an unbiased estimate of the average treatment effect (Aronow and Middleton 2013), particularly when the number of clusters is small.[40] The second is that

39. A total of 14 hot-spot locations had no registered voters, driven by non-residential locations and are relatively balanced across experimental conditions.

40. To mitigate this source of potential bias it is considered best practice to block randomize on cluster size (Imai, King, and Nall 2009), which was not done in the case of the Jacksonville experiment.

cluster-level assignment generally produces far higher sampling variability than complete random assignment, which can lead to imprecise estimates and imbalance in individual-level characteristics. In Appendix Table B.1, I present balance statistics which show good balance across individual-level covariates, but not cluster size.

To account for variation in cluster size and to improve the precision of the estimates I rely on regression adjustment, fitting a series of linear models with controls for individual-level, pre-treatment characteristics plausibly associated with turnout (i.e. race, age, gender, and turnout in the 2006 and 2008 general elections).[41] I also control for cluster size (i.e. the number of registered voters living within each hotspot), and for whether a hotspot was included in a short followup POP initiative run by the JSO several months after the conclusion of the original experiment.[42][43]

3.10 Results

3.10.1 Main Results

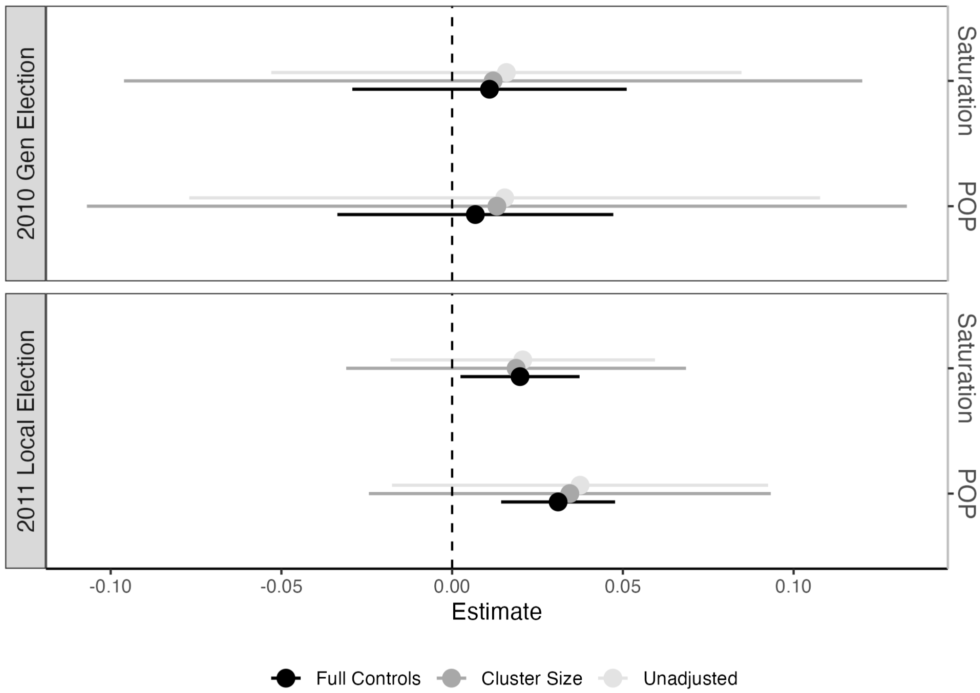
The top panel of Figure 3.1 presents the estimated intent-to-treat effects for both the Problem Oriented Policing (POP) and the Saturation/Directed Patrol interventions on individual-level turnout in the 2010 statewide general election both with (*in black*) and without (*in gray*) covariate adjustment, along with 95% confidence intervals. If existing hypotheses on the demobilizing effects of high-intensity policing are correct, we would expect the point estimate for the Saturation/Directed Patrol condition and its associated 95% confidence interval to appear to the left of the vertical line at zero (*H1*). Instead, point estimates are small and statistically insignificant—even after the considerable precision gains from regression adjustment. Indeed, the bottom panel of Figure 3.1 suggests that turnout actually *increased* by 2% ($p < 0.05$) in the 2011 election among those exposed

41. Specifically, I rely on the estimator proposed by Lin (2013) which includes the full set of centered, pre-treatment covariates interacted with the treatment indicator(s). This adjustment is conceptually related to both post-stratification and blocking (Miratrix, Sekhon, and Yu 2013), which can minimize bias and increase the precision of the estimates. While my outcome of interest is binary, I rely on OLS, which, unlike generalized linear models, gives asymptotically correct confidence intervals under model misspecification (Freedman 2008; Lin 2013).

42. This follow-up initiative appears to have been randomly assigned—additional checks done by the PERF researchers did not find evidence that inclusion was related to the original experimental treatment assignments (Taylor, Koper, and Woods 2011).

43. Due to a lack of crime data, I am unable reconstruct the randomization strata used in the original experiment. Because hotspots were assigned to treatment groups with equal probability within each block, their exclusion should not alter the estimated treatment effects, but can reduce the precision of the estimates (see Weisburd and Gill 2014). I also note that the randomization strata were not included in the analysis of the original experiment.

Figure 3.1: **Estimated Treatment Effect of Hot-spots Policing on Turnout**



Note: Intent-to-treat estimates of the effect of the Jacksonville hot-spots policing interventions on voter turnout in the 2010 and 2011 elections ($n = 10,234$ individuals in 69 crime hot-spots). Covariate-adjusted estimates are reported, as well as estimates from models that include either limited (i.e. cluster size only) or no pre-treatment controls. 95% confidence intervals derived from Huber–White heteroscedasticity- and cluster-robust standard errors.

to the Saturation/Directed Patrol condition.

Turning to the POP intervention, the estimates are null or positive, consistent with theoretical expectations (*H2*). While the estimates for the 2010 general election are not distinguishable from zero, I find that exposure to POP policing led to a 3.1% increase ($p < 0.05$) in the probability of voting in the 2011 municipal election that occurred four months later. Together, this suggests that both interventions increased turnout, but this increase only occurred in a low turnout, off-cycle election in which the local officials who were ultimately responsible for instituting police policies were on the ballot (*H4*).

I look for heterogenous effects by race with interactions between race and the treatment dummies, leaving White individuals as the omitted reference category.[44] As noted previously, I expect differential effects, particularly between Black and White individuals; however, the direction of this difference is not clear (*H3a* and *H3b*). As can be seen in Table 3.2, I fail to find a significant interaction between race and treatment assignment when individual- and cluster-level covariates are included in the model. While this might indicate similar effects across racial groups, the relatively small number of non-Black voters produces limitations in statistical power that make it difficult to detect effects. As can be seen in Figure 3.2, the estimated marginal treatment effects for White and Hispanic[45] voters have a high degree of uncertainty, consistent with moderate increases or decreases in participation. Despite this, the point estimates for both White and Hispanic voters are consistently smaller than those for Black voters. For example, point estimates for the 2011 municipal election indicate that turnout among Black voters increased by nearly 4%, while the estimated effect for White voters is statistically insignificant and close to zero (0.081%).

In sum, I find that despite the divergent tactics used, both styles of hot-spots policing interventions produced similar increases in turnout. I also find suggestive evidence that this mobilizing effect was driven primarily by Black voters, who make up the large majority of residents of the experimental intervention sites.

3.11 Discussion and Future Directions

The results presented here provide strong support for the claim that individuals respond to the day-to-day presence and tactics of the police in their neighborhood, which can in turn influence voting behavior. I find that large increases in the presence of the police in places where violent crime is frequent led to a 2% increase in turnout in the following local election, and a similar 3% increase in voting in places where police offers engaged intensively with residents to identify and address sources of violence in the community. The fact that these positive effects were only seen in local, as opposed to state-wide or national elections, may suggest that voters are able to connect their experiences with law enforcement and crime to the local officials who are ultimately responsible for setting policy related to these issues. Importantly, this participatory response was primarily

44. For this analysis, I replaced the interacted, demeaned dummy variables for race in the main model specification with dummy variables that that are not mean-centered.

45. I follow the Florida File and use the term Hispanic, rather than Latino/a/x.

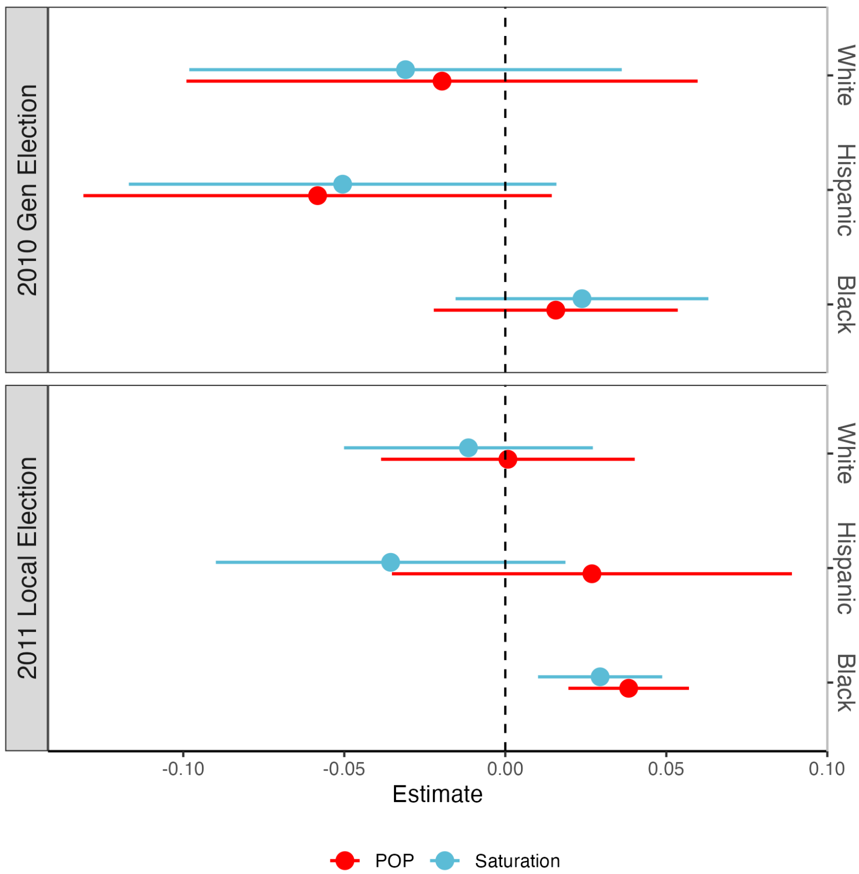
Table 3.2: **Heterogeneous Effects of Interventions by Race**

	2010 General Election		2011 Local Election	
	(1)	(2)	(3)	(4)
POP	−0.020	0.005	0.001	0.020
	[−0.100; 0.061]	[−0.137; 0.146]	[−0.039; 0.041]	[−0.064; 0.103]
Saturation	−0.031	−0.058	−0.011	−0.023
	[−0.099; 0.037]	[−0.164; 0.049]	[−0.051; 0.028]	[−0.095; 0.049]
Black	**−0.053**	**−0.129**	−0.016	**−0.072**
	[−0.090; −0.015]	[−0.192; −0.066]	[−0.036; 0.003]	[−0.108; −0.036]
Hispanic	−0.036	**−0.137**	−0.032	**−0.099**
	[−0.104; 0.031]	[−0.247; −0.027]	[−0.067; 0.002]	[−0.143; −0.055]
POP × Black	0.035	0.030	0.038	0.030
	[−0.036; 0.106]	[−0.118; 0.178]	[−0.006; 0.081]	[−0.066; 0.127]
POP × Hispanic	−0.039	−0.107	0.026	−0.026
	[−0.139; 0.062]	[−0.281; 0.068]	[−0.056; 0.108]	[−0.133; 0.080]
Saturation × Black	0.055	**0.105**	0.041	0.064
	[−0.001; 0.111]	[0.007; 0.202]	[−0.004; 0.086]	[−0.010; 0.138]
Saturation × Hispanic	−0.020	−0.023	−0.024	−0.039
	[−0.098; 0.059]	[−0.145; 0.100]	[−0.080; 0.032]	[−0.108; 0.030]
Constant	**0.316**	**0.368**	**0.149**	**0.192**
	[0.258; 0.374]	[0.283; 0.454]	[0.131; 0.167]	[0.153; 0.230]
Controls	✓		✓	
N. Observations	10234	10234	10234	10234
Adj. R^2	0.315	0.012	0.238	0.008

* Null hypothesis value outside the 95% confidence interval in bold.

Note: Interaction between treatment assignment and racial categories for both the 2010 and 2011 elections. The omitted reference category is White voters. Coefficients and interactions with other racial categories (e.g. Asian, Alaskan Native, etc.) are not displayed due to small sample sizes a lack of theoretical expectations. Models 1 and 3 include the full set of centered, interacted controls (Lin, 2013). 95% confidence intervals derived from Huber–White heteroscedasticity- and cluster-robust standard errors are presented in brackets.

Figure 3.2: **Heterogenous Effects by Race**



Note: Marginal effect estimates of the two hot-spots policing interventions on voter turnout in the 2010 and 2011 elections by racial group. Estimates come from covariate-adjusted models presented in Columns 1 and 3 of Table 3.2. 95% confidence intervals derived from Huber–White heteroscedasticity- and cluster-robust standard errors.

seen among Black voters living in disadvantaged, majority-Black neighborhoods with high levels of serious crime — a population that typically reports extremely low levels of satisfaction with the police (Braga, Brunson, and Drakulich 2019), and a strong sense of alienation from politics and the promise of multi-racial democracy (Weaver, Prowse, and Piston 2020).

While these findings provide important new evidence of the causal effect of commonly used place-based policing practices, this analysis is not without limits. The close spatial proximity of the experimental hot-spots to one another suggests that spillover effects may have attenuated the effects toward zero.[46] Among the 83 intervention sites, the average distance to the next nearest hot-spot was only 477.4 meters (median = 204.8m),[47] with some separated by less than a city block. The original PERF researchers do not find evidence of significant crime displacement effects (Taylor, Koper, and Woods 2011); however, these interventions may have had important community spillovers as residents were exposed to neighboring hot-spots through their daily travel patterns and personal networks.[48]

Without data on individual attitudes it is also unclear what is driving the mobilization effects I find. In line with my theoretical expectations, I find that the POP condition — which was able to meaningfully reduce violence, reduce physical disorder, and establish lasting cooperative exchanges between residents, community partners, and beat officers — led to increased electoral engagement. Policies that provide tangible benefits and positive encounters with government agents can mobilize constituencies (Soss 1999; Mettler and Soss 2004), while reductions in community violence may have fostered the types of community ties that can facilitate greater engagement with politics (Moffett-Bateau 2023). Yet a similar positive effect was seen in the Saturation/Directed Patrol condition, which gave officers little direction in how they should spend their time, and led to huge increases

46. Current data limitations prevent me from assessing the roll of spillovers. Standard analyses of spatial spillover effects that use bins of distance to the treatment typically require the inclusion of inverse probability of treatment weights to account for the non-random spatial clustering of hot-spots, which mechanically expose some individuals to a higher probability of being assigned to the spillover condition (Blattman et al. 2021). This could generate bias if individuals with a higher probability of being exposed to spillovers have observable characteristics that are associated with turnout, such as living in an area of the city with higher overall levels of crime or physical disorder. These weights can be precisely estimated by re-running the original randomization procedure a large number of times; however, I lack the randomization strata needed for this step.

47. These distances do not include the 100ft buffer zones which were considered part of the intervention sites in the original experiment.

48. While scholars have found that the political effects of sudden events such as police killings are hyper-local and do not spill over into the surrounding neighborhood (Ang and Tebes 2024), this is likely not the case for sustained changes in police presence and behavior.

in stops and other enforcement actions without corresponding decreases in violent crime.

The increase in electoral participation among voters exposed to the Saturation/Directed Patrol may be consistent with an electoral backlash effect; however, there are strong reasons to expect that this is not the case. Unlike other cases of documented electoral backlash to police practices (such as gang injunctions or police killings), there is no evidence to suggest that this intervention was well publicized or involved the types of egregious police behavior that has been found to resonate with mobilizing narratives about racial injustice in policing (Morris and Shoub 2024). And while experimental evidence from similar interventions finds that the negative effects of saturation policing on police legitimacy are small and temporary (Kochel and Weisburd 2017), the mobilization effects I find are durable, influencing turnout in an election that occurred nearly two years later. Future work might explore this question further by examining post-treatment changes in crime over a longer period of time, and as well as by evaluating the effects of the two interventions on precinct-level vote choice (though the small size of the hot-spots along with their proximity to one another may pose challenges to estimation).

One potential explanation for the similar effects across conditions and the different effects across election types I find is that both interventions unintentionally increased the salience of crime. Existing quasi-experimental evidence from New Jersey finds that even if interventions are effective in reducing crime rates, *fear* of crime can increase as nearby residents take high rates of police activity as a signal that their neighborhood is dangerous (Hinkle and Weisburd 2008). Concerns about crime, safety, and neighborhood disorder are consistently some of the top issues in local elections and are particularly mobilizing for residents who perceive adverse changes occurring in their neighborhoods (Brown and Zoorob 2022). This may have been particularly true in Jacksonville, which was experiencing the highest rate of violent crime in the state of Florida at the time of the intervention (Taylor, Koper, and Woods 2011).

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Appendix A

A.1 Los Angeles Gang Injunctions

Table A.1: **List of Injunctions**

Case	Complaint Filed	Preliminary Injunction	Permanent Injunction	End Date	Resumed As
Blythe Street Gang	02/22/1993	04/27/1993	02/17/2000		
18th Street Gang (Jefferson Park Injunction)	03/21/1997	07/11/1997	02/08/2005		
18th Street Gang (Pico-Union Injunction)	08/01/1997	08/29/1997	11/10/1998	10/22/1999	*Idem*
Mara Salvatrucha (MS-13)	03/04/1998	04/13/1998	*None*	09/18/2003	*Idem*
Shatto Park Locos and Columbia Lil Cycos	05/01/1998	06/30/1998	*None*	03/02/2001	10 Gang Injunction
Harpy's Gang	06/16/1998	08/04/1998	07/17/2000		
Langdon Street Gang	03/26/1999	05/20/1999	02/17/2000		
Culver City Boys	04/23/1999	06/03/1999	03/27/2001		
Venice Shoreline Crips	05/21/1999	07/21/1999	10/18/2000		
Harbor City Gang and Harbor City Crips	11/12/1999	01/12/2000	01/27/2000		
Venice 13	02/04/2000	03/17/2000	01/12/2001		

Continued on next page

Table A.1: **List of Injunctions** (Continued)

Case	Complaint Filed	Preliminary Injunction	Permanent Injunction	End Date	Resumed As
Pacoima Project Boys	03/20/2001	*None*	08/22/2001		
Eastside and Westside Wilmas Gangs	05/23/2001	*None*	03/09/2004		
Canoga Park Alabama	01/29/2002	02/25/2002	04/24/2002		
18th Street Gang (Pico-Union Injunction)	04/16/2002	*None*	10/18/2002		
Krazy Ass Mexicans (KAM)	10/03/2002	10/25/2002	01/16/2003		
The Avenues	12/17/2002	01/29/2003	04/07/2003		
Rolling Sixty Crips	07/08/2003	10/01/2003	11/24/2003		
Bounty Hunter Bloods	08/26/2003	10/01/2003	12/02/2003		
18th Street Gang (Hollywood Injunction)	11/04/2003	12/08/2003	03/16/2004		
Mara Salvatrucha (MS-13)	03/09/2004	04/08/2004	05/10/2004		
18th Street Gang (Wilshire Injunction)	04/06/2004	05/07/2004	06/29/2004		
38th Street Gang	07/28/2004	08/18/2004	11/22/2004		
Varrio Nueva Estrada	08/12/2004	09/21/2004	11/15/2004		
42nd, 43rd, and 48th Street Gangster Crips	12/16/2004	01/18/2005	04/07/2005		
Grape Street Crips	03/10/2005	04/15/2005	05/25/2005		
Hoover and Trouble Gangs	03/15/2005	05/24/2005	06/29/2005		
18th Street, Crazy Riders, DIA, Krazy Town, La Raza Loca, Orphans, Rockwood Street Locos, Varrio Vista Rifa, Wanderers, and Witmer Street Locos (10 Gang Injunction)	05/02/2005	06/03/2005	09/11/2005		
Hazard Grande	06/28/2005	08/16/2005	09/09/2005		
School Yard and Geer Street Crips	03/23/2006	06/08/2006	09/22/2006		

Continued on next page

Table A.1: **List of Injunctions** (Continued)

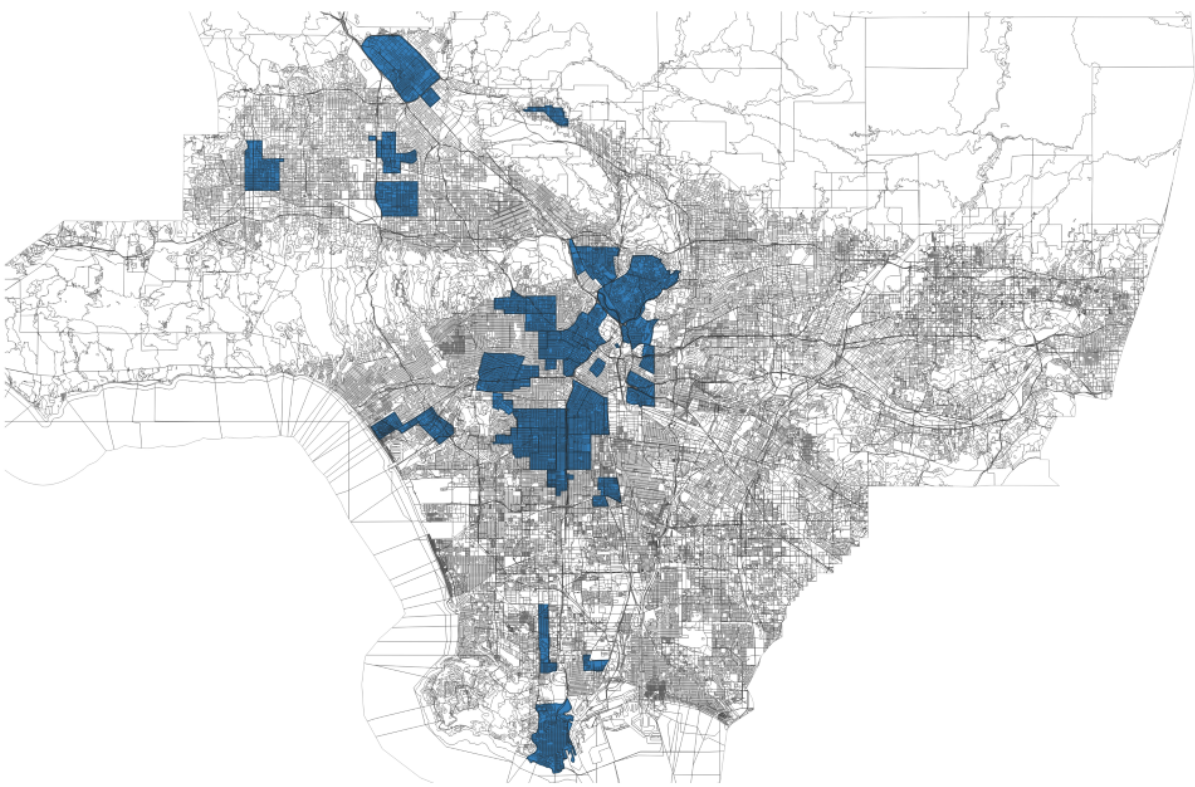
Case	Complaint Filed	Preliminary Injunction	Permanent Injunction	End Date	Resumed As
Playboys	05/08/2006	07/14/2006	09/21/2006		
Black P-Stones	05/25/2006	07/25/2006	09/21/2006		
White Fence	06/08/2006	07/24/2006	10/03/2006		
Clover, Eastlake, and Lincoln Heights Gangs	09/20/2006	10/23/2006	01/09/2007		
Dogtown Gang	10/06/2006	11/13/2006	12/13/2006		
Highland Parque Gang	10/06/2006	11/13/2006	02/16/2007		
Rolling 40's, 46 Top Dollar Hustler, and 46 Neighborhood Crips	11/05/2007	01/29/2008	03/08/2008		
5th and Hill Gang	11/16/2007	02/05/2008	01/06/2009		
204th Street and East Side Torrance Gangs	12/07/2007	03/04/2008	07/07/2008		
San Fer	04/10/2008	06/24/2008	08/11/2008		
All for Crime, Barrio Mojados, Blood Stone Villans, Florencia 13, Oriental Boyz, and Pueblo Bishops (6 Gang Injunction)	09/05/2008	10/03/2008	01/14/2009		
East Side Pain/Ghost Town Bloods	10/10/2008	12/17/2008	06/11/2009		
Temple Street Gang	11/03/2008	12/30/2008	03/27/2009		
Toonerville Gang	11/14/2008	01/28/2009	03/18/2009		
Barrio Van Nuys	05/06/2009	06/03/2009	09/02/2009		
Swan Bloods, Florencia 13, Main Street Crips, and 7-Trey Hustlers/Gangster Crips (Fremont Injunction)	06/12/2009	08/24/2009	12/15/2009		
Grape Street Crips (Central City Injunction)	04/07/2010	11/30/2010	02/02/2011		
Rancho San Pedro	4/27/2011	06/03/2011	07/11/2011		

Continued on next page

Table A.1: **List of Injunctions** (Continued)

Case	Complaint Filed	Preliminary Injunction	Permanent Injunction	End Date	Resumed As
Columbus Street Gang	2/20/2013		06/27/2013		
Big Top Locos, Mayberry Crazys, Diamond Street Locos, Echo Park Locos, Frogtown Rifas, and Head Hunters (Glendale Corridor Injunction)	6/11/2013		09/24/2013		

Figure A.1: **Map of Gang Injunctions within Los Angeles County**



Note: Census blocks covered by active injunctions in 2014 are shaded in blue. This represents the maximum geographic extent of injunction safety zones within Los Angeles.

A.1.1 Timeline of Major Events

The following is a timeline of other events that were likely to have influenced the scope and impact of injunction orders or voting behavior:

1998 The LAPD launches an investigation into allegations of illegal behavior among members of several gang units. During this period the LAPD changed its policies, with any complaint against an officer automatically triggering an internal investigation into potential misconduct (Prendergast 2021).

2001 The LAPD is put under a Federal Consent Decree. Under the decree, the LAPD created stricter policies on use of force and officer misconduct, and officers were required to document street stops.

2002 In line with recommendations from the Consent Decree Independent Monitor, the LAPD changes its complaints policy to make dismissal easier (Prendergast 2021).

2007 The LAPD formalizes gang injunction protocols, raising the standard of evidence needed for an individual to be identified as a gang member. Before 2007, no person added to a gang list had been removed, likely due to a requirement that the person publicly renounce membership in the gang, which could generate retaliation (O'Deane 2011, 400).

2015 The California New Motor Voter Act (AB 1461) is signed into law, making voter registration automatic.

2016 A state audit highly critical of CalGangs is released. Issues identified included: individuals listed without evidence, a failure to notify minors who were entered into the database, and abuse of the system by individual police departments.

2016 First elections held with same-day voter registration.

2017 7,300 individuals are released from gang injunctions in Los Angeles as the result of a city audit.

2017 Last off-cycle city elections held in Los Angeles.

2018 In the case *Youth Justice Coalition v. City of Los Angeles*, the City of Los Angeles is barred from enforcing gang injunctions by a Federal judge

A.2 Population Change

Table A.2: **Difference in Difference Estimates of Population Changes in Treated and Untreated Blocks (2000 - 2020)**

	N. Residents	N. Registrants	Prop. Black	Prop. Latino	Prop. White
Injunction	-0.10	8.67^{**}	-0.00	-0.02^{*}	0.02^{***}
	(3.84)	(2.82)	(0.01)	(0.01)	(0.01)
Census Block FE's	✓	✓	✓	✓	✓
Year-by-Census tract FE's	✓	✓	✓	✓	✓
N. Observations	370118	370118	367011	367011	367011
Adj. R^2	0.93	0.86	0.89	0.91	0.92
R^2 (within)	0.00	0.00	0.00	0.00	0.00

Note: Robust standard errors clustered by Census block given in parentheses. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

A.3 Main Analysis

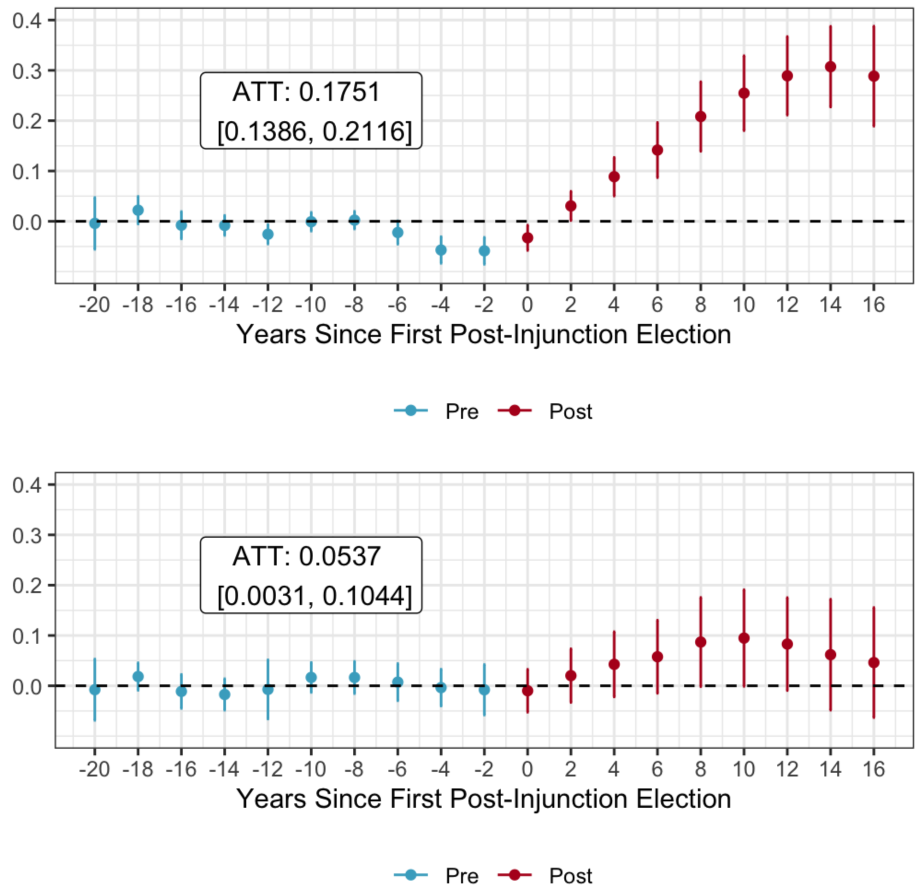
Table A.3: **Effect of Gang Injunctions: Alternate Transformations**

	Registrations			Votes Cast		
	$ln(y+1)$	$sinh^{-1}y$	y	$ln(y+1)$	$sinh^{-1}y$	y
Injunction	0.106^{***}	0.108^{***}	12.075^{***}	0.057^{*}	0.061^{*}	2.706^{*}
	(0.019)	(0.021)	(2.516)	(0.022)	(0.025)	(1.149)
Census Block FE's	✓	✓	✓	✓	✓	✓
Year-by-Census tract FE's	✓	✓	✓	✓	✓	✓
Proximity controls	✓	✓	✓	✓	✓	✓
N. Observations	1633170	1633170	1633170	867290	867290	867290
Adj. R^2	0.917	0.912	0.916	0.904	0.895	0.881
R^2 (within)	0.000	0.000	0.000	0.000	0.000	0.000

Note: Difference-in-difference estimates of the effect of gang injunctions on the log-transformed, inverse hyperbolic sine-transformed, and untransformed counts of registrations and votes, respectively. Robust standard errors clustered by Census block given in parentheses. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

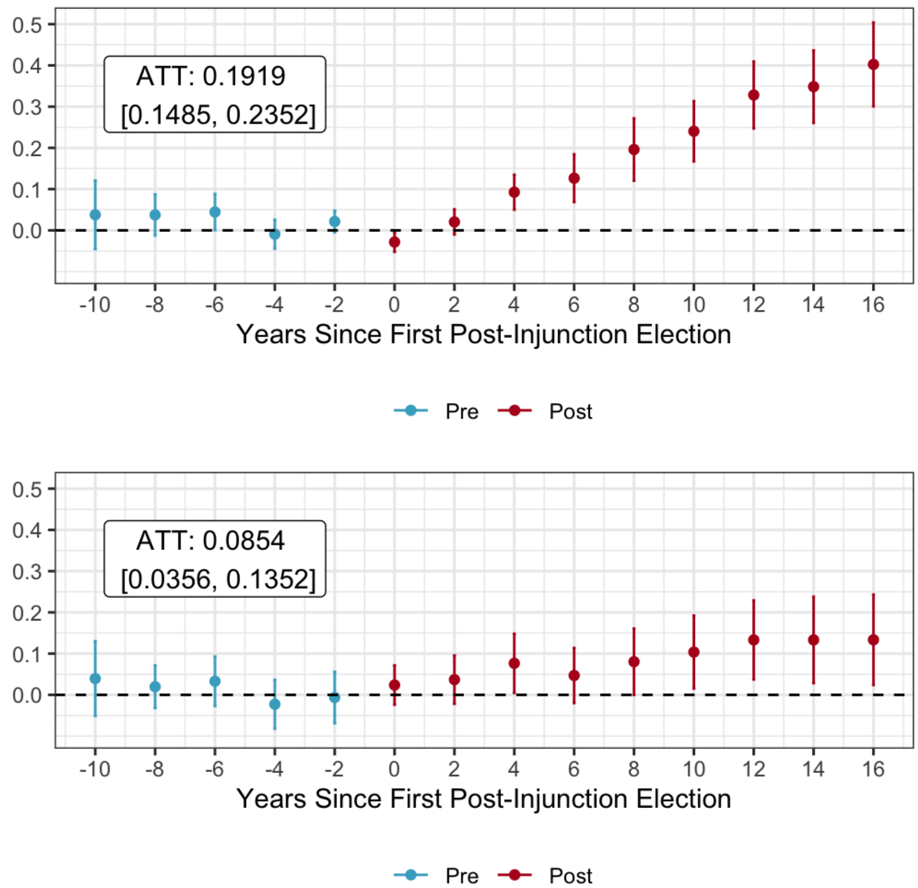
A.3.1 Alternate Approaches to Identification

Figure A.2: **Event Study Estimates of Gang Injunctions on Registrations (Callaway and Sant'Anna Estimator)**



Note: Dynamic difference-in-differences estimates using semi-parametric, propensity-score weighted methods developed by Sant'Anna and Zhao (2020) with bootstrapped 95% simultaneous CI's to account for multiple hypothesis testing. Bottom panels display estimates after conditioning on pre-treatment covariates which include: 1) 2000 population (logged); 2) the share of the 2000 population that is White, Latinx, and Black; 3) average annual crime from 1991-2000 (logged); median income (logged); and 4) the share of households receiving public assistance. Overall ATT's obtained by averaging over different lengths of exposure to treatment.

Figure A.3: **Event Study Estimates of Gang Injunctions on Votes Cast (Callaway and Sant'Anna Estimator)**



Note: Dynamic difference-in-differences estimates using semi-parametric, propensity-score weighted methods developed by Sant'Anna and Zhao (2020) with bootstrapped 95% simultaneous CI's to account for multiple hypothesis testing. Bottom panels display estimates after conditioning on pre-treatment covariates which include: 1) 2000 population (logged); 2) the share of the 2000 population that is White, Latinx, and Black; 3) average annual crime from 1991-2000 (logged); median income (logged); and 4) the share of households receiving public assistance. Overall ATT's obtained by averaging over different lengths of exposure to treatment.

Table A.4: **Synthetic Difference-in-Differences Estimates of Gang Injunctions on Registrations and Voting**

Treatment Group	log(Registrations)	log(Votes)
2002	0.136	–
	[0.101, 0.171]	–
2004	0.109	–
	[0.086, 0.132]	–
2006	0.134	0.086
	[0.11, 0.157]	[0.063, 0.11]
2008	0.123	0.090
	[0.098, 0.148]	[0.062, 0.118]
2010	0.09	0.049
	[0.057, 0.123]	[0.014, 0.084]
2012	0.026	0.014
	[-0.014, 0.067]	[-0.024, 0.052]
2014	0.043	0.049
	[0.019, 0.068]	[0.02, 0.078]
Aggregated Effect	0.116	0.071

Note: Estimates displayed with bootstrapped 95% CI's. Estimates for the effect of injunctions on votes are not available for the 2002 and 2004 treatment groups due to a lack of sufficient pre-treatment observations. The aggregate effect is the weighted average of cohort-specific estimates, with the weights derived from the proportion of the total number of block-year treatment observations that occur in each treatment-timing group (Arkhangelsky et al. 2021). To ensure comparability to the main difference-in-differences specifications with Year-by-Census tract fixed effects, each model is fit on a balanced panel subset down to Census blocks in tracts that are (partially) covered by an injunction at any point between 2000 and 2020.

A.4 L.A.FANS Analysis

A.4.1 Covariate Balanced Propensity Score (CPBS) Weights

Table A.5: **Balance Statistics, Covariate Balanced Propensity Score (CPBS) Weights**

Covariates	Diff. Unweighted	Diff. Weighted	Bal. Test
Propensity Score	1.306	0.115	
Latino	0.283	0.008	Balanced, <0.05
White	-0.266	-0.003	Balanced, <0.05
Black	0.023	-0.003	Balanced, <0.05
Asian	-0.037	-0.001	Balanced, <0.05
Age	-0.560	-0.028	Balanced, <0.05
Female	0.039	0.001	Balanced, <0.05
College Degree	-0.214	-0.004	Balanced, <0.05
Less than Highschool Degree	0.187	0.003	Balanced, <0.05
Child lives at home	0.061	0.004	Balanced, <0.05
Homeowner	-0.278	-0.007	Balanced, <0.05
Moved in past 2 years	-0.120	-0.005	Balanced, <0.05
Nbrhd. Residential Stability	1.718	0.076	Not Balanced, >0.05
Nbrhd. Disadvantage Score	1.522	0.018	Balanced, <0.05
Nbrhd. Immigrant Concentration	-1.458	-0.027	Balanced, <0.05
Nbrhd. Percent Black	0.391	0.009	Balanced, <0.05
Effective Sample Sizes	N. Unweighted	N. Weighted	
Treated	181	181	
Untreated	999	158	

Note: Neighborhood covariates come from Tract-level data from the 2000 Census. Residential stability is the first principle component of the share of owner-occupied housing units and residents who moved in the past five years, immigrant concentration is the first principle component of the Latino and foreign-born shares of the population. Neighborhood disadvantage is the weighted least squares score from a factor analysis of seven items: the percentage of the population living under the poverty line, families receiving public assistance income, residents with less than a high school education, residents without a college degree, population under 18, families headed by single women, and residents who are unemployed.

A.4.2 Alternative Specifications

Table A.6: **Effect of Injunctions on Participation and Perceived Safety (Tract by Wave Fixed Effects)**

	(1)	(2)	(3)
Injunction	0.278^{***}	-0.029^{***}	-0.500
	(0.065)	(0.002)	(0.317)
N. Observations	2367	2373	2350
Adj. R^2	0.308	0.351	0.367
R^2 (within)	0.002	0.000	0.011

Note: Robust standard errors clustered by household and Census tract in parentheses. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

Table A.7: **Count Models of Injunctions on Participation**

	(1)	(2)	(3)
Injunction	1.981^{**}	0.316	1.781^{*}
	(0.760)	(1.099)	(0.709)
Injunction × Black/Latino		1.929^{*}	
		(0.847)	
Injunction × Under 30			1.989^{**}
			(0.729)
N. Observations	110	110	110
Pseudo R^2	0.353	0.373	0.368
Pseudo R^2 (within)	0.050	0.079	0.072

Note: Robust standard errors clustered by household and Census tract in parentheses. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

A.4.3 Placebo Test Results

Table A.8: **Placebo Test of Future Injunctions on Civic Participation and Perceived Safety**

	Involvement	Crime Victimization	Perceived Safety
Future Injunction	-0.172	-0.024	0.490
	(0.115)	(0.090)	(0.180)
N. Observations	162	162	161
Adj. R^2	0.034	0.599	0.193
R^2 (within)	0.013	0.000	0.096

Note: Robust standard errors clustered by household and Census tract in parentheses. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

A.4.4 Experiences of Police Discrimination

Table A.9: **Effect of Injunctions on Self-reported Experiences of Police Discrimination (Full Model Results)**

	(1)	(2)	(3)
Injunction	3.083*	2.545*	2.455**
	(1.293)	(1.065)	(0.773)
Age		−0.052***	−0.073*
		(0.008)	(0.031)
Male		1.990***	2.616***
		(0.340)	(0.794)
Black		0.016	1.855
		(0.411)	(1.142)
Latino		0.129	1.074
		(0.326)	(1.069)
U.S. Born		1.010***	1.344
		(0.301)	(1.083)
Food Stamps		0.109	−0.314
		(0.241)	(0.982)
College		0.338	2.961**
		(0.456)	(1.033)
No High School		0.682*	0.088
		(0.334)	(0.733)
Family Income (logged)		0.081	0.287*
		(0.061)	(0.141)
Nbrhd. Percent Black			0.104*
			(0.045)
Nbrhd. Disadvantage Score			−0.646
			(1.008)
Nbrhd. Residential Mobility			−1.629
			(1.011)
Nbrhd. Immigrant Concentration			−3.222***
			(0.898)
Crime Rate 2001			−0.621
			(0.414)
Crime Rate 2000			0.482**
			(0.170)
Crime Rate 1999			0.381
			(0.538)
Constant			−11.388***
			(3.404)
N. Observations	1534	1500	296
Pseudo R^2	0.073	0.265	0.642

***$p < 0.001$; **$p < 0.01$; *$p < 0.05$

Table A.10: **Effect of Injunctions on Experiences of Discrimination by Race and Gender**

	Main Results	Black/Latino Respondents	Gender
Injunction	2.455^{**}	1.984^{***}	0.751
	(0.773)	(0.593)	(0.900)
Male	2.616^{***}	2.601^{***}	-0.586
	(0.794)	(0.743)	(1.006)
Injunction × Male			3.636^{**}
			(1.369)
Age	-0.073^{*}	-0.068^{*}	-0.073^{*}
	(0.031)	(0.030)	(0.029)
Black	1.855	0.645	1.221
	(1.142)	(0.762)	(0.942)
Latino	1.074		0.674
	(1.069)		(0.993)
U.S. Born	1.344	1.240	1.458
	(1.083)	(1.132)	(1.134)
Food Stamps	-0.314	-0.204	-0.304
	(0.982)	(0.958)	(0.996)
College	2.961^{**}	3.042^{**}	3.268^{**}
	(1.033)	(1.022)	(1.000)
No High School	0.088	-0.056	0.059
	(0.733)	(0.812)	(0.763)
Family Income (logged)	0.287^{*}	0.285	0.277^{*}
	(0.141)	(0.149)	(0.137)
Nbrhd. Percent Black	0.104^{*}	0.105^{*}	0.100^{*}
	(0.045)	(0.049)	(0.042)
Nbrhd. Disadvantage Score	-0.646	-0.987	-0.714
	(1.008)	(0.863)	(0.934)
Nbrhd. Residential Mobility	-1.629	-1.989^{*}	-1.908^{*}
	(1.011)	(0.902)	(0.961)
Nbrhd. Immigrant Concentration	-3.222^{***}	-3.203^{***}	-3.264^{***}
	(0.898)	(0.855)	(0.833)
Crime Rate 2001	-0.621	-0.639	-0.758
	(0.414)	(0.441)	(0.413)
Crime Rate 2000	0.482^{**}	0.417	0.568^{***}
	(0.170)	(0.231)	(0.141)
Crime Rate 1999	0.381	0.517	0.488
	(0.538)	(0.491)	(0.544)
Constant	-11.388^{***}	-9.337^{**}	-9.373^{**}
	(3.404)	(2.988)	(3.182)
N. Observations	296	264	296

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

A.5 Voter File Analysis

A.5.1 Individual Turnout

Table A.11: **Difference in Difference Estimates of Injunctions on Individual Turnout**

	(1)	(2)	(3)
Injunction	−0.012	−0.015	−0.010
	(0.006)	(0.009)	(0.007)
Individual FE's	✓	✓	✓
Year-by-Census tract FE's	✓	✓	✓
Proximity controls	✓	✓	✓
Individual linear trends		✓	
Matched sample			✓
N. Observations	8175784	8175784	4890837
Adj. R^2	0.41	0.45	0.40
R^2 (within)	0.00	0.00	0.00

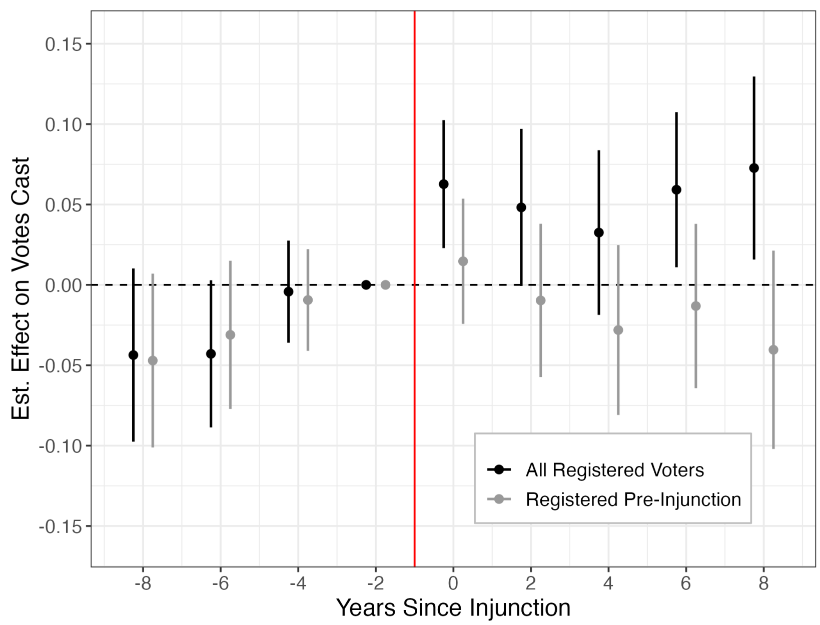
Note: Robust standard errors clustered by individual given in parentheses. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

Table A.12: **Difference in Difference Estimates of Injunctions on Individual Turnout by Race and Age**

	(1)	(2)	(3)	(4)	(5)	(6)
Injunction	-0.011	-0.015	0.002	0.051^{***}	-0.034^{***}	0.075^{***}
	(0.007)	(0.010)	(0.009)	(0.007)	(0.010)	(0.009)
Injunction:Asian	-0.028^{***}	-0.013	-0.044^{***}			
	(0.006)	(0.008)	(0.008)			
Injunction:Black	0.031^{***}	0.026^{***}	0.018^{**}			
	(0.005)	(0.007)	(0.007)			
Injunction:Hispanic	-0.019^{***}	-0.019^{**}	-0.033^{***}			
	(0.004)	(0.006)	(0.006)			
Injunction:Other	0.073^{*}	0.087	0.036			
	(0.034)	(0.050)	(0.046)			
Injunction:Age				-0.001^{***}	0.000^{***}	-0.002^{***}
				(0.000)	(0.000)	(0.000)
Individual FE's	✓	✓	✓	✓	✓	✓
Year-by-Census tract FE's	✓	✓	✓	✓	✓	✓
Proximity controls	✓	✓	✓	✓	✓	✓
Individual linear trends		✓			✓	
Matched sample			✓			✓
N. Observations	8175784	8175784	4890837	8175784	8175784	4890837
Adj. R^2	0.408	0.455	0.403	0.408	0.455	0.403
R^2 (within)	0.000	0.000	0.000	0.000	0.000	0.000

Note: Robust standard errors clustered by individual given in parentheses. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

Figure A.4: **All Votes Cast Versus Votes Cast by Voters Registered Pre-Injunction**



A.5.2 New Registrations

Table A.13: **Difference in Difference Estimates of New Registrations by Race and Age**

	Black	Latino	White	Asian	18 to 34	35 to 54	55+
Injunction	0.058***	0.064**	0.023	0.015	0.081***	0.037	0.044**
	(0.017)	(0.020)	(0.017)	(0.013)	(0.023)	(0.019)	(0.017)
100m	0.020	−0.001	−0.019	−0.012	−0.020	0.006	−0.021
	(0.022)	(0.025)	(0.021)	(0.015)	(0.029)	(0.026)	(0.021)
500m	0.002	−0.003	−0.001	0.009	−0.001	0.009	−0.000
	(0.011)	(0.015)	(0.014)	(0.011)	(0.019)	(0.015)	(0.012)
1000m	−0.009	−0.008	−0.016	−0.003	−0.029*	−0.001	−0.012
	(0.008)	(0.010)	(0.011)	(0.007)	(0.013)	(0.011)	(0.008)
N. Observations	1630656	1630656	1630656	1630656	1630656	1630656	1630656
Adj. R^2 (full model)	0.689	0.653	0.713	0.518	0.616	0.521	0.455
R^2 (Within)	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Note: Robust standard errors clustered by Census block given in parentheses. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

Table A.14: **Difference in Difference Estimates of New Registrations by Race of Gang**

	Black Gangs		Latino Gangs	
	Black	Latino	Black	Latino
Injunction	0.074**	0.022	0.044*	0.101***
	(0.025)	(0.024)	(0.019)	(0.028)
100m	0.020	−0.000	0.020	−0.001
	(0.023)	(0.025)	(0.023)	(0.025)
500m	0.002	−0.003	0.002	−0.003
	(0.011)	(0.015)	(0.011)	(0.015)
1000m	−0.009	−0.008	−0.009	−0.009
	(0.008)	(0.011)	(0.008)	(0.011)
N. Observations	1567008	1567008	1595340	1595340
Adj. R^2	0.691	0.645	0.685	0.652
R^2 (Within)	0.000	0.000	0.000	0.000

Note: Robust standard errors clustered by Census block given in parentheses. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

A.6 Ballot Initiatives Analysis

Table A.15: **Placebo Test: Support for Criminal Justice Reform as Function of Future Treatment Assignment**

	Prop. 66	Placebo	*Prop. 5*	Placebo	*Prop. 6*	Placebo
	(1)	(2)	(3)	(4)	(5)	(6)
Injunction	**0.01****		**0.03*****		**−0.01***	
	(0.00)		(0.00)		(0.00)	
Pre-treatment Dummy		−0.00		−0.00		0.01
		(0.00)		(0.01)		(0.01)
Constant	−0.06	−0.06	**0.32****	**0.26***	0.08	0.09
	(0.08)	(0.08)	(0.12)	(0.12)	(0.09)	(0.09)
N. Observations	1114	1114	716	716	716	716
Adj. R^2	0.96	0.96	0.91	0.90	0.74	0.74

Note: Robust standard errors clustered by precinct given in parentheses. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

A.6.1 Ballot Proposition Language

2004 Propositions

Prop 66 *Limitations on "Three Strikes" Law. Sex Crimes. Punishment. Initiative Statute*

Summary: Limits "Three Strikes" law to violent and/or serious felonies. Permits limited resentencing under new definitions. Increases punishment for specified sex crimes against children. Fiscal Impact: Over the long run, net state savings of up to several hundred million dollars annually, primarily to the prison system; local jail and court-related costs of potentially more than ten million dollars annually.

Arguments For: PROPOSITION 66 RESTORES THREE STRIKES TO ITS ORIGINAL INTENT—ensuring criminals currently serving time for violent offenses are kept in prison, SAVING TAXPAYERS BILLIONS OF DOLLARS currently wasted imprisoning shoplifters and other nonviolent, petty offenders for life. PROPOSITION 66 PROTECTS CHILDREN WITH TOUGHER 1-STRIKE SENTENCES FOR CHILD MOLESTERS.

Arguments Against: Proposition 66 is opposed by Governor Schwarzenegger, the Attorney General, all 58 District Attorneys, the state's leading law enforcement, taxpayer, and child protection groups. Costs millions and threatens public safety by creating a legal loophole that could release an estimated 26,000 convicted felons— including rapists, child molesters, and murderers. www.Keep3Strikes.org

2008 Propositions

Prop 5 *Nonviolent Drug Offenses. Sentencing. Parole and Rehabilitation. Initiative Statute*

Summary: Allocates $460,000,000 annually to improve and expand treatment programs. Limits court authority to incarcerate offenders who commit certain drug crimes, break drug treatment rules or violate parole. Fiscal Impact: Increased state costs potentially exceeding $1 billion annually primarily for expansion of offender treatment programs. State savings potentially exceeding $1 billion annually on corrections operations. Net one-time state prison capital outlay savings potentially exceeding $2.5 billion.

Arguments For: Proposition 5 safely reduces prison overcrowding. For youth, it creates drug treatment programs. None now exist. For nonviolent offenders and parolees, it expands rehabilitation. Prop. 5 enlarges successful, voter-approved Proposition 36 (2000), providing treatment with close supervision and strict accountability for nonviolent drug offenders. Prop. 5 saves $2.5 billion.

Arguments Against: Shortens parole for methamphetamine dealers from 3 years—to 6 months. Loophole allows defendants accused of child abuse, domestic violence, vehicular manslaughter, and other crimes to effectively escape prosecution. Strongly opposed by Mothers Against Drunk Driving (MADD). Establishes new bureaucracies. Reduces accountability. Could dramatically increase local costs and taxes.

Prop 6 *Police and Law Enforcement Funding. Criminal Penalties and Laws. Initiative Statute*

Summary: Requires minimum of $965,000,000 of state funding each year for police and local law enforcement. Makes approximately 30 revisions to California criminal law. Fiscal Impact: Increased net state costs exceeding $500 million annually due to increasing spending on criminal justice programs to at least $965 million and for corrections operating costs. Potential one-time state prison capital outlay costs exceeding $500 million.

Arguments For: Every California Sheriff supports Proposition 6. YES on 6 is a comprehensive anti-gang and crime reduction measure that will bring more cops and increased safety to our streets. It returns taxpayers' money to local law enforcement without raising taxes and will increase efficiency and accountability for public safety programs.

Arguments Against: Proposition 6 WILL take $1,000,000,000 from schools, healthcare, fire protection, and proven public safety programs. Prop. 6 WON'T guarantee more police on the street and WON'T even fund proven gang prevention programs. Prop. 6 WILL spend more money on prisons and jails. Vote NO on Prop. 6!

2012 Propositions

Prop 34 *Death Penalty. Initiative Statute*

Summary: Repeals death penalty and replaces it with life imprisonment without possibility of parole. Applies retroactively to existing death sentences. Directs $100 million to law enforcement agencies for investigations of homicide and rape cases. Fiscal Impact:

Ongoing state and county criminal justice savings of about $130 million annually within a few years, which could vary by tens of millions of dollars. One-time state costs of $100 million for local law enforcement grants.

Arguments For: 34 guarantees we never execute an innocent person by replacing California's broken death penalty with life in prison without possibility of parole. It makes killers work and pay court-ordered restitution to victims. 34 saves wasted tax dollars and directs $100 million to law enforcement to solve rapes and murders.

Arguments Against: California is broke. Prop. 34 costs taxpayers $100 million over four years and many millions more, long term. Taxpayers would pay at least $50,000 annually, giving lifetime healthcare/housing to killers who tortured, raped, and murdered children, cops, mothers and fathers. DA's, Sheriffs and Police Chiefs say Vote No.

Prop 35 *Human Trafficking. Penalties. Initiative Statute*

Summary: Increases prison sentences and fines for human trafficking convictions. Requires convicted human traffickers to register as sex offenders. Requires registered sex offenders to disclose Internet activities and identities. Fiscal Impact: Costs of a few million dollars annually to state and local governments for addressing human trafficking offenses. Potential increased annual fine revenue of a similar amount, dedicated primarily for human trafficking victims.

Arguments For: YES on 35—STOP HUMAN TRAFFICKING. PREVENT THE SEXUAL EXPLOITATION OF CHILDREN. Traffickers force women and children to sell their bodies on the streets and online. Prop. 35 fights back, with tougher sentencing, help for victims, protections for children online. Trafficking survivors; children's and victims' advocates urge: YES on 35.

Arguments Against: Proposition 35 actually threatens many innocent people "My son, who served our country in the military and now attends college, could be labeled a human trafficker and have to register as a sex offender if I support him with money I earn providing erotic services." — Maxine Doogan Please Vote No.

Prop 36 *Three Strikes Law. Repeat Felony Offenders. Penalties. Initiative Statute*

Summary: Revises law to impose life sentence only when new felony conviction is serious or violent. May authorize re-sentencing if third strike conviction was not serious or violent. Fiscal Impact: Ongoing state correctional savings of around $70 million annually, with even greater savings (up to $90 million) over the next couple of decades. These savings could vary significantly depending on future state actions

Arguments For: Restores the original intent of the Three Strikes law by focusing on violent criminals. Repeat offenders of serious or violent crimes get life in prison. Nonviolent offenders get twice the ordinary prison sentence. Saves over $100,000,000 annually and ensures rapists, murderers, and other dangerous criminals stay in prison for life.

Arguments Against: Proposition 36 will release dangerous criminals from prison who were sentenced to life terms because of their long criminal history. The initiative is so flawed some of these felons will be released without any supervision! Join California's Sheriffs, Police, Prosecutors, and crime victims groups in voting No on Proposition 36.

Appendix B

B.1 Balance Checks

In their original analysis of the experiment, Taylor, Koper, and Woods (2011) fail to find substantial pre-treatment differences between the three treatment arms across a wide range of measures of crime and officer behavior. This includes self-initiated policing activity, police field stops, calls for service, arrests, and UCR crime incidents (162). Hot-spots in the three conditions were also similar in terms of their geographic size and physical characteristics (i.e. residential, commercial, or mixed-use).

I compare the average pre-treatment characteristics of individual voters using normalized differences (Table B.1). While any imbalances between conditions can induce estimation error (Imai, King, and Stuart 2008; Miratrix, Sekhon, and Yu 2013), I assess whether these imbalances are sufficiently large to induce model dependence and pose potential threats to inference using the rule of thumb suggested by Imbens and Rubin (2015) of 0.25. Using this threshold, I find that imbalances in individual-level characteristics such as race, gender, age, partisan affiliation, and turnout history are small. However, I find that cluster size is highly imbalanced across experimental conditions, with individuals in the control condition residing in hot-spots with more that 100 fewer registered voters on average.

Table B.2 presents regression-based balance tests, predicting treatment assignment with the full vector of individual- and cluster-level covariates. No covariates significantly predict treatment assignment at the 0.05 level and the results of an F-test of joint significance suggest that the sample is balanced along these observed characteristics.

Lastly, Figure B.1 compares the distributions of cluster size at both the cluster- and individual-

Table B.1: **Descriptive Statistics: Registered Voters Living Within Crime Hot-Spots**

	Control	POP	Saturation	Control-POP	Control-Saturation	POP-Saturation
Variables		Mean			Normalized Diff.	
Age	39.75	40.91	41.31	[-0.069]	[-0.095]	[-0.024]
Race:Black	0.71	0.79	0.69	[-0.180]	[0.047]	[0.227]
Male	0.34	0.4	0.4	[-0.121]	[-0.121]	[0.000]
Democrat	0.73	0.78	0.72	[-0.119]	[0.021]	[0.141]
Republican	0.13	0.09	0.12	[0.098]	[0.018]	[-0.081]
Voted (2008)	0.64	0.62	0.64	[0.055]	[-0.004]	[-0.060]
Voted (2006)	0.51	0.57	0.62	[0.070]	[-0.019]	[-0.089]
N. Voters	225.38	372.37	339.82	**[-1.027]**	**[-0.712]**	[0.186]

Note: Table includes means for each pre-treatment covariate by treatment group. Normalized differences are given by:

$$\Delta_{norm} = \frac{\overline{X_c} - \overline{X_t}}{\sqrt{\frac{s_c^2+s_t^2}{2}}} \tag{B.1}$$

where $\overline{X}$ is the group mean and s is the sample standard deviation (Imbens and Rubin 2015). Differences greater than 0.25 are presented in bold.

level (Imai, King, and Velasco Rivera 2020). The results suggest that there are not any extreme outliers with high leverage that could to induce model dependence.

Table B.2: **Tests for Pre-Treatment Balance**

	POP	Saturation
(Intercept)	−0.64	0.20
	[−1.53; 0.25]	[−0.59; 1.00]
Voted (2008)	−0.05	0.01
	[−0.12; 0.03]	[−0.03; 0.05]
Voted (2006)	0.04	−0.01
	[−0.01; 0.10]	[−0.05; 0.03]
Democrat	0.02	−0.01
	[−0.02; 0.05]	[−0.04; 0.01]
Republican	0.01	−0.04
	[−0.07; 0.09]	[−0.09; 0.01]
JSO Follow-up	0.04	−0.20
	[−0.36; 0.44]	[−0.49; 0.09]
Age	0.00	0.00
	[−0.00; 0.00]	[−0.00; 0.00]
Male	0.04	0.02
	[−0.02; 0.10]	[−0.02; 0.05]
Race:Black	0.08	−0.02
	[−0.14; 0.29]	[−0.15; 0.11]
Cluster Size (logged)	0.16	0.02
	[−0.01; 0.34]	[−0.15; 0.19]
Adj. R^2	0.09	0.05
N. Observations	9470	9470
RMSE	0.47	0.42
N. Clusters	69	69

* 0 outside the confidence interval.

Note: Columns 1 and 2 report the results of an OLS regression of all covariates on indicators for the two treatment conditions. Robust standard errors are clustered at the hot-spot level. The p-values on the F-tests for joint significance for the two models are 0.222 and 0.478, respectively.

Figure B.1: **Distribution of Cluster Size by Treatment Assignment**