The Mobilizing Effects of Aggressive Policing: Evidence from Anti-Gang Crackdowns

Daniel Naftel*

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Abstract

Aggressive, zero tolerance policing exposes many Americans to high levels of police surveillance and coercion. Yet little is known about how these tactics affect voting in the broader communities they target. I overcome difficulties in identifying the causal effect of aggressive policing on turnout by exploiting hyperlocal variation in exposure to a series of geographically targeted, anti-gang crackdowns in Los Angeles. Using administrative data on voting, a geocoded panel survey, and a within-neighborhoods, difference-in-difference design, I show that these crackdowns led to large, durable increases in political participation. These mobilization effects are concentrated among Black and Latino residents, who became significantly more likely to report police discrimination. I find corresponding increases in support for criminal justice reform, but minimal changes to perceived crime and safety. These results suggest that communities targeted by harsh police crackdowns may mobilize to resist these policies when viewed as ineffective and racially targeted.

*Postdoctoral Fellow, John F. Kennedy School of Government, Harvard University, danielnaftel[at]hks.harvard.edu.
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Introduction

Aggressive police tactics have become a focal point in political efforts to reform law enforcement in the United States. Exemplified by stop-and-frisk, broken windows, and zero tolerance policing, these policies empower officers to crack down on minor offenses and to stop large numbers of people for "furtive" or "suspicious" behavior. These tactics have made involuntary encounters with law enforcement a fact of daily life for millions of Americans, and contributed to vast inequalities in how citizens can expect to be treated by the police (Fagan et al. 2016; National Academies of Sciences, Engineering, and Medicine 2018). High levels of unfocused and indiscriminate enforcement often expose entire communities to aggressive police behavior, helping to create an environment in which wrongdoing is assumed (Muñiz 2015). Exposure to these forms of overly aggressive policing has been linked to a variety of negative outcomes including low levels of police legitimacy (Fagan, Tyler, and Meares 2016; Braga, Brunson, and Drakulich 2019), acute psychological distress, physical ailments, and reduced educational attainment (Bacher-Hicks and Campa 2020; Rios 2011; Legewie and Fagan 2019; Brunson 2007).

Despite this, we know surprisingly little about how chronic exposure to aggressive policing affects *political* behavior in highly policed communities. Numerous studies find that being stopped and arrested by the police reduces turnout (White 2019; Ben-Menachem and Morris 2022; Weaver and Lerman 2010). But the effects of policing are thought to extend far beyond these individual encounters to the broader communities in which they take place (Walker 2020a; Anoll, Epp, and Israel-Trummel 2022). The prevailing view is that living in a heavily policed neighborhood should discourage voting (Lerman and Weaver 2020). Indirect exposure to aggressive policing can undermine faith in elections and government responsiveness more broadly (Lerman and Weaver 2020), while high levels of arrests may undermine community norms around political participation (Burch 2013).

Yet these tactics may also encourage political activity precisely because they are so punitive. Aggressive policing practices are viewed as degrading and racially discriminatory by many Americans, particularly Americans of color (Epp, Maynard-Moody, and Haider-Markel 2014; Walker 2020a). Witnessing these tactics may activate a sense of injustice or group threat that has been shown to encourage voting and acts of political resistance in other contexts (White 2016; Walker 2020a). This may be particularly likely to occur when aggressive, place-based policing tactics are implemented in racially segregated environments, where salient, negative policy changes can generate electoral resistance among those who come to view their community as being unfairly targeted by the state on the basis of race and place (Nuamah and Ogorzalek 2021).

Existing evidence on the community-level effects of aggressive policing is mixed, speaking to the challenge of measuring police behavior and questions of causal identification. Researchers rarely have access to information on the strategies that police departments are using or the exact populations that are being (in)directly exposed to police aggression. While some papers have used surveys to ask about experiences with the police, or rely on geographic concentrations of stops and searches in administrative data, these approaches have known issues (White 2019; Knox, Lowe, and Mummolo 2020; Neil and Winship 2019). Voters who are most likely to be mobilized by policing may also be more likely to remember indirect police encounters and to describe police behavior as "aggressive." And administrative police stop data only captures a small subset of police-citizen interactions and can misrepresent how biased or heavy-handed officers are being (Knox, Lowe, and Mummolo 2020; Neil and Winship 2019). Finally, the nonrandom distribution of policing and its close links to social, economic, and racial inequalities make it difficult to imagine a plausible counterfactual when trying to separate the effects of policing from other drivers of neighborhood-level political participation (White 2022). Recent work leveraging variation in the timing and location of specific events, such as police killings, addresses some of these concerns (Ang and Tebes 2024; Morris and Shoub 2024). But police killings represent a tiny fraction of all police-citizen encounters, and the findings from this work may not generalize to less extreme forms of police enforcement and surveillance that are experienced over many years.

In this paper, I address each of these challenges by leveraging hyperlocal exposure to a geographically bounded policy change, allowing me to credibly estimate the effects of aggressive police tactics on political attitudes and behavior. Between 1993 and 2013, the City of Los Angeles successfully pursued a series of civil restraining orders that dramatically expanded the legal authority of officers to crack down on suspected gang activity within predetermined geographic areas known as "safety zones." By criminalizing many ordinary behaviors, increasing criminal punishments, and enabling the use of widespread investigative stops within the safety zones, these anti-gang injunctions empowered law enforcement to aggressively surveil, question, and detain residents. This presents an ideal test case for several reasons. First, gang injunctions closely mirror other racialized, coercive police tactics that arose during the wars on drugs and crime in the United States (Murch 2015; Soss and Weaver 2017), ensuring that the "treatment" aligns with existing theory on the effects of punitive and discriminatory policing. Second, the well-defined boundaries of the safety zones, which were determined by court orders, created substantial *within-neighborhood* variation in exposure to these tactics that endured for many years. This provides unique causal leverage while also allowing me to identify long-term effects. Combining original data on the timing and location of each gang injunction with geocoded administrative voter and election data from 1992 to 2020, I use a stacked difference-in-difference design with Census tract fixed effects to compare changes in registrations and voting in Census blocks covered by injunctions to changes in uncovered but demographically similar Census blocks in the same neighborhood. Identification rests on the assumption that within a given neighborhood (i.e. Census tract), observed trends in political participation in blocks outside the safety zone boundaries serve as a valid counterfactual for trends in blocks within the safety zone. A range of robustness checks presented below support this claim, including the finding that registrations and voting in the treated and untreated blocks trended similarly in the decade prior to the start of the injunctions.

I find that voting and registrations increased by 6% and 11%, respectively, within the blocks covered by gang injunctions—a result robust to alternate difference-in-difference specifications that use weighting and synthetic controls to account for potential differences in observable characteristics between the treated and control groups. These large effects are highly localized and durable—extending over a decade after the injunction orders were first put in place. To explore mechanisms I turn to a geocoded panel survey as well as election return data on local ballot initiatives that made changes to criminal sentencing. Consistent with extensive qualitative evidence that this increased participation was rooted in resident concerns with racial profiling and police aggression (Barajas 2007; Muñiz 2015), I find that: 1) Black and Latino men living within the safety zones were significantly more likely to report being treated unfairly by the police, 2) increases in political participation were almost entirely driven by Black and Latino residents, and 3) gang injunctions signficantly increased support for criminal justice reform.

I consider and rule out several alternative mechanisms by which aggressive policing might have increased electoral engagement. First, I consider the possibility that gang injunctions encouraged gentrification, which may have increased registrations and voting through higher population growth and the in-migration of affluent, high propensity voters. I find no evidence of compositional changes that could explain the mobilization effects I find; in fact, the populations of the safety zones slightly decreased relative to the surrounding areas. In contrast, panel survey data provides robust evidence of mobilization at the individual level. Second, I examine whether increased political participation might have been driven by reductions in crime—which may encourage voting and other forms of community engagement by making people feel safer (Trelles and Carreras 2012). The evidence suggests that gang injunctions did not improve feelings of safety within the safety zones. Nor do changes in turnout appear to be mediated by changes in crime. Finally, I show that my results hold after accounting for potential changes in political activism and police-community relations following the 1992 Los Angeles riots.

My results deepen our understanding of how the police shape political life in the United States. Previous work demonstrates that aggressive policing lowers the likelihood of voting among the millions of Americans who are stopped, arrested, or jailed by the police each year (Harrell and Davis 2020; Weaver and Lerman 2010). But these interactions do not happen in a vacuum, suggesting that the political effects of these policies extend much further. This article overcomes nontrivial data hurdles to show that collective experiences of police coercion or surveillance can have a causal effect on the political behavior of entire neighborhoods.

Importantly, I find that aggressive police tactics that fail to address crime concerns and that are viewed as racially targeted can generate broad electoral resistance, even in poor communities that lack the resources traditionally associated with voting. This is surprising given conventional wisdom about the demobilizing nature of policing, and points to the important role that race and neighborhood-level identities may play in moderating responses to invasive and punitive law enforcement practices. Indeed, these results are highly supportive of the idea that negative policy effects that are racially and geographically concentrated can encourage a sense of linked fate and collective mobilization among marginalized groups (Nuamah and Ogorzalek 2021). Together, my results provide new insights into political behavior in highly policed communities, and contribute to broader literatures on race, urban politics, policy feedback, and political participation.

The (De)Mobilizing Effect of Aggressive Police Tactics

Law enforcement in the United States has long been dominated by a "reactive" model of policing that prioritizes criminal investigations and apprehensions alongside rapid responses to citizen calls-for-service (National Academies of Sciences, Engineering, and Medicine 2018). But the perceived inability of police agencies to control crime in the 1980s and 1990s led a cohort of researchers, police chiefs, victim's rights groups, and policymakers in America's largest cities to embrace new, far more aggressive and proactive approaches to law enforcement (National Academies of Sciences, Engineering, and Medicine 2018; Soss and Weaver 2017). Policies such as stop-and-frisk, hot spots, and zero tolerance policing sought to preempt criminal activity by focusing police resources on high-risk places and people, and aggressively targeting low-level offenses with high levels of stops, searches, and arrests. While originally conceived as targeted interventions meant to improve public safety, in practice these tactics have often led to overly aggressive police behavior 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). This style of indiscriminate enforcement has often failed to control serious crime, undermined police legitimacy, and lead to widespread misdemeanor arrests that have deepened stark race- and class-based inequalities in the criminal justice system (Braga, Brunson, and Drakulich 2019; Fagan et al. 2016; Soss and Weaver 2017).

A rapidly growing literature documents how direct contact with the police, such as stops and arrests, can erode trust in government and depress turnout (Lerman and Weaver 2020; Ben-Menachem and Morris 2022). But there are strong reasons to expect the political consequences of these aggressive policies extend much further into the broader communities they target. Research on policy feedback and political behavior demonstrates that exposure to government policies need not be direct to be politically consequential (Soss and Schram 2007), and direct police encounters have been shown to influence the political attitudes and behavior of friends, family, and neighbors (Burch 2013; White 2019; Walker 2020b; Epp, Maynard-Moody, and Haider-Markel 2014). Even in the absence of these personal connections, scholars find that experiences with geographically concentrated policies can provide the public with insights into government and politics that have downstream consequences for political (de)mobilization (Michener 2017; Nuamah and Ogorzalek 2021). Tactics such as stop-and-frisk can blanket entire neighborhoods with heavy surveillance and enforcement, subjecting all residents to the legal, psychological, and physical risks of encounters with the police. Both witnessing aggressive officer behavior and living in a highly policed neighborhood have been linked to poor mental and physical health (Jackson et al. 2021; Sewell and Jefferson 2016), social isolation (Rios 2011), decreased educational attainment (Bacher-Hicks and Campa 2020), 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). These substantial harms may in turn depress political participation by limiting opportunities for political recruitment and mobilization, as well as reducing the time, resources, and mental energy needed to acquire civic knowledge and skills (Verba, Schlozman, and Brady 1995). Beyond these material effects, chronic and collective exposure to punitive institutions such as the police can foster a sense of legal estrangement, leading individuals to avoid interactions with the state, or withdraw from civic life altogether (Lerman and Weaver 2020; Bell 2017).

Yet other scholars have argued that the extreme nature of many of these tactics may help to create a "threatening policy environment capable of catalyzing mobilization" (Walker 2020b, 120). A large body of research documents how preemptive policing often targets disadvantaged minority neighborhoods (Fagan et al. 2016), exacerbating Black and Latino Americans' disproportionate exposure to stops, arrests, and incarceration (Baumgartner, Epp, and Shoub 2018; Gelman, Fagan, and Kiss 2007; Epp, Maynard-Moody, and Haider-Markel 2014). Blacks and Latinos are, in turn, significantly more likely than Whites to attribute these inequalities to institutionalized bias (Peffley and Hurwitz 2010), and often view preemptive police tactics as discriminatory and degrading (Gau and Brunson 2010; Epp, Maynard-Moody, and Haider-Markel 2014). Daily exposure to racially disparate policing outcomes may encourage political mobilization by fostering a sense of injustice and presenting threats to a group's social or economic status (Oskooii 2020; Walker 2020b), particularly among those for whom racial group membership is personally meaningful and understood in expressly political terms (Garcia-Rios et al. 2023). Race has long served as a powerful source of group consciousness for Black Americans, and many Latinos also perceive some some degree of racial linked fate that can be made politically salient by cross-cutting policy threats (Zepeda-Millán and Wallace 2013; White 2016). Even among individuals who do not initially hold a strong sense of group identity or who do not directly experience police contact, aggressive and racially disparate policing practices may act as a "race-making" institution that reinforces group membership and leads group members to see their individual interests as connected to those of the group as a whole (Lerman and Weaver 2020; Laird 2019). This sense of linked fate can facilitate mobilization by increasing political efficacy and cultivating beliefs in the importance of collective action (Dawson 1995), potentially counteracting the legal and political estrangement that arises from authoritarian policies (Garcia-Rios et al. 2023). As Nuamah and Ogorzalek (2021) demonstrate in the realm of education, this process of racialization and mobilization in opposition to aggressive policing may be particularly likely to occur when these tactics target racially and economically segregated communities. This form of geographic targeting may transform policing into both a race and neighborhood concern and lead residents to understand their community as being unfairly singled out by negative government action (Nuamah and Ogorzalek 2021).

Existing empirical evidence on the effects of policing at the neighborhood level has been mixed. Examining New York City's Stop, Question, and Frisk (SQF) program, Palmer (2024) finds that registered voters became less likely to vote as the number of arrests in their neighborhood increased, while Laniyonu (2019) finds a variable relationship between voter turnout and geographic concentrations of SQF stops and arrests. Using survey data, Anoll, Epp, and Israel-Trummel (2022) fail to find a significant relationship between political participation and racial disparities in police stops at the city level. However, Walker (2020b) does find correlations between indirect contact with the police (i.e. having a close friend or family member who has been stopped or arrested) and non-electoral forms of political participation among individuals who report aggressive and disrespectful police behavior in their neighborhood. To date, causal evidence for the community effects of policing has relied on relatively fleeting variation in experiences with the criminal justice system, such as the exact timing of police killings or the return of residents from prison near elections (e.g. Morris and Shoub 2024; Burch 2013). While this short-term variation provides a useful and plausible means for causal identification, existing theory is primarily motivated by large, long-term differences in policing across place. It is unclear if findings from single, extreme events like police killings can generalize to the effects of *chronic* exposure to police coercion and aggression.

Research Design

Gang Injunctions

Examining the long-run effects of police aggression on highly policed communities is far from straightforward. As noted above, policing is deeply entwined with preexisting racial and economic inequalities known to shape attitudes toward the government, police, and voting, which raises questions about causality. These issues are compounded by the difficulty in reliably measuring exposure to aggressive policing. I overcome these challenges by leveraging a series of restraining orders filed against gangs in Los Angeles which dramatically increased the power and discretion available to officers to arrest and detain individuals suspected of being gang members within pre-specified areas known as "safety zones." 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 below that this policy shift encouraged far more aggressive policing in these areas. By exploiting hyperlocal differences in exposure to these changes, my approach follows recent work on the causal effects of geographically targeted policies (e.g. Czurylo 2023).

As part of the broader national embrace of aggressive, "law and order" policing in the 1980s and 1990s, local governments in California pioneered the use of the civil court system as a tool to crack down on criminal street gangs. By suing gangs and having them declared a public nuisance, municipalities were able to secure civil restraining orders permitting them to impose sweeping restrictions on the movement and behavior of gang members in specific locations where the enjoined gang had engaged in nuisance activity.[1] These restraining orders, known as gang injunctions, were designed to "banish gang members from the public streets," (Werdegar 1999, 411) by criminalizing a variety of everyday behaviors such as using a cellphone, gathering in groups of more than two, riding a bike, or wearing certain clothing (Werdegar 1999; O'Deane 2011; Muñiz 2015), while increasing penalties for breaking existing laws with automatic fines and jail time.

While specifically targeted at gangs and gang members, two factors suggest that these harsh tactics impacted all residents of the targeted safety zones. First, injunctions decreased legal constraints on officer behavior by lowering the standard of suspicion needed to detain someone. This allowed officers to stop and and search anyone who ignored questions or walked away from an interaction, even if there was no other indication that individual had violated the law (Boga 1994; Owens, Mioduszewski, and Bates 2020). There are strong reasons to suspect that this would have 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).[2] This was a possibility recognized at the time, with one Los Angeles city prosecutor lamenting that within the LAPD, injunctions were often seen as a way to, "give cops the chance to stop anybody for any reason" (Muñiz 2015, 53).

1. This ranged from "quality of life" issues—such as vandalism and loud music—to more serious crimes like open drug-dealing and drive-by shootings (Harward 2014)

2. 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.

Second, because these cases were brought against gangs as legal entities (Harward 2014), the police were able to enforce the terms of the injunction on anyone later identified as a member of an enjoined gang, even if they were not originally named as a defendant. 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 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 restrictions.

In later sections, I provide evidence that gang injunctions dramatically increased the rate of negative experiences with law enforcement, particularly among Black and Latino men. 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 negative experiences with the police their families and wider social networks (O'Deane 2011; Muñiz 2015). In some places, these policies were met with substantial opposition, with youth-led activist groups organizing protests, attending community meetings, and staging voter registration drives (Muñiz 2015; Barajas 2007; Scott 2023). 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 state avoidance (Bell 2017; Lerman and Weaver 2020) that discouraged voting?

Data

I construct a rich dataset that allows me to estimate the impact of the injunction orders on 1) electoral participation using Census block-level counts of registrations and voting, 2) non-electoral civic participation using individual-level panel survey data, and 3) support for criminal justice reform using election returns for local ballot initiatives. Census block-level counts of the number of registered voters and votes cast in each general election between 1992 (2002 for votes) and 2020 come from the California Statewide Database.[5]

3. These criteria included living within a gang's territory and adopting "their style of dress, uses of hand signs, symbols, or tattoos" (Kim 1995, 270). The use of these vague criteria were limited in 2007 by stricter documentation requirements.

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. The Statewide Database is California's official redistricting database. Between 1992 and 2020 registration data is only reported for 2000 Census blocks, and between 2012 and 2020, voting and registration data is only reported for 2020 Census blocks. I covert these data to the 2010 Census block-level using the official Block Relationship Files provided by the U.S. Census Bureau. See Appendix Tables A.2 and A.3 for robustness checks on these block conversions.

These counts—which are based on geocoded, election-day voter file extracts and voter history data—provide an accurate election-day measure of electoral participation broken down by age, gender, partisan affiliation, and ethnicity using surname matching.[6]

While my key dependent variables are voting and registrations, I also analyze the effects of injunctions on non-electoral forms of political participation. Existing scholarship notes that an overly narrow focus on voting may obscure other ways in which residents of highly policed communities respond to and resist perceived injustices (Walker 2020b). To measure non-electoral political participation I rely on individual-level panel data from Waves I (2000-2001) and II (2007-2008) of the Los Angeles Family and Neighborhood Survey (L.A.FANS), mapped to Census block of residence using restricted-access files. The survey includes 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).

While this differs from many traditional political participation batteries, this measure aligns with common definitions of non-electoral politics in the race and ethnic politics literature as "any community or collectively oriented activity" (Bedolla 2005, 137). Even if not expressly political, community involvement and volunteerism are thought to be key ways in which members of racialized, historically disenfranchised groups mobilize to address collective problems, and can facilitate engagement in formal politics by fostering a sense of efficacy and group attachment (Bedolla 2005). Indeed, scholars find that these non-political community attachments can often facilitate activism and political organizing (Zepeda-Millán 2016).

To examine how gang injunctions may have affected preferences, I rely on election returns for three statewide ballot initiatives — Proposition 66 in 2004, Proposition 5 in 2008, and Proposition 36 in 2012 — that each would have reduced the severity of criminal punishments. Both Propositions 66 and 36 aimed to limit California's three-strikes law to violent or serious felonies, and Proposition 5 aimed to reduce penalties for nonviolent drug offenses and to expand drug treatment programs. Census block-level vote tallies for each proposition come from the California Statewide Database and are based on precinct-level data mapped to Census geographies using ecological inference methods.[7]

I merge these data with original data on the location and timing of the 50 gang injunctions that were

6. 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.

7. See (McCue 2011) for details on how the Statewide Database disaggregates precinct-level election data to Census geographies. While these block-level estimates contain some degree of measurement error, these errors would only bias my difference-in-difference estimates if they are correlated with across-time trends in the treatment or control group within Census tracts. This seems implausible—an assumption bolstered by placebo tests which find null effects of future injunctions on block-level election returns.

imposed in the City of Los Angeles between 1993 and 2013 (see Appendix Table A.1 for a full list of injunctions). This information comes from court documents made available by the Los Angeles City Attorney's Gang Unit, which 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 and mapped onto Census blocks 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).[8]

Empirical Approach

I estimate the effect of civil gang injunctions on electoral participation using a stacked difference-in-difference design, comparing changes in registrations and voting in Census blocks placed under an injunction to changes in untreated blocks in the same neighborhood. To avoid invalid comparisons that can arise in difference-in-difference models with multiple time periods and ensure that my results are robust to treatment effect heterogeneity across place and time (Goodman-Bacon 2021; Imai and Kim 2019), I stack my block-level panel data on registrations and voting by treatment timing cohort. Because outcomes are measured at each election, cohorts are defined by the election that blocks were first placed under an injunction order.[9] Each treatment timing cohort includes all Census blocks treated in the same election period (e.g. all blocks treated between the 2002 and 2004 general election), as well as the full set of never-treated blocks used as controls. Thus, blocks used as controls can appear in multiple cohorts but treated blocks will only appear in one. Using this stacked, block-level data on registrations and voting I estimate the following two way fixed effects model:

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

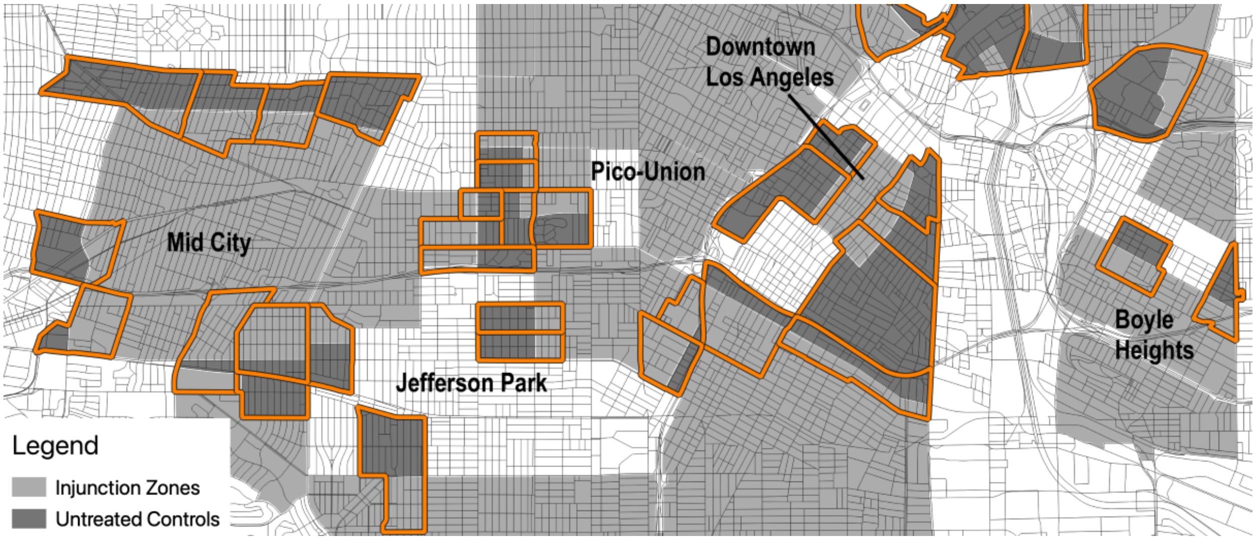
Where $y_{b,e,g}$ is either the log-transformed number of votes cast or voters registered in Census block b, election e and treatment timing cohort g and $\beta INJUNCTION_{b,e,g}$ is an indicator for whether or not a given block is covered by a gang injunction in that year.[10] To account for spatial spillovers, I include a set of indicators

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

9. I consider "treatment" to begin when a preliminary injunction was obtained by the city, which 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). I use the date of the permanent order if a preliminary injunction was not requested or granted. In cases where blocks were covered by multiple injunctions, I consider treatment to begin with the first.

10. In Appendix Table A.4, I show that my results are substantively similar when using a inverse hyperbolic sine transformation, as well as the untransformed count of registrations and votes.

Figure 1: **Visualization of Main Identification Strategy**



Note: Map of the maximum extent of civil 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.

capturing distance to the nearest injunction boundary in the set of time-varying, block level controls ($\mathbf{X}_{b,e,g}$). Given the stacked estimation strategy, block-by-treatment cohort 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 cohort 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 Census tract. Figure 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).[11]

The central identifying assumption underlying my approach is that in the absence of the injunction, treated blocks would have followed the same trends in electoral participation as untreated blocks in the same tract. I test the plausibility of this assumption by estimating a dynamic effects version of Equation 1 with leads and lags of treatment. The results, presented in Appendix Figure A.2, provide empirical support for the parallel trends assumption, with small and statistically insignificant pre-trends. Because this form of pre-testing can be underpowered (Freyaldenhoven, Hansen, and Shapiro 2019), I demonstrate below that my

11. 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.

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.

In addition to these empirical checks, I note that the legal process by which specific blocks were selected for inclusion in a safety zone does not appear to have been related to time-variant confounders such as sudden, localized crime waves or community-led campaigns pushing for increased public safety. In Los Angeles, injunctions 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 catalog 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 long evidence-gathering process needed to seek an injunction and narrow focus on gang "nuisance activity" suggests that these areas did not necessarily map onto overall 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).

Effects on Voting and Registrations

Table 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 6% 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 alternate specifications. In Models 2 and 6 I include year fixed effects interacted with voting age 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 include year fixed effects interacted with quartiles of the Black and Latino share of the population in 2000. This accounts for the possibility that blocks with high proportions of these groups may have been more likely to be selected

Table 1: **Effect of Gang Injunctions on Electoral Participation**

	Registrations				Votes Cast			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Injunction	0.106***	0.101***	0.082***	0.087***	0.057*	0.049*	0.046*	0.064**
	(0.019)	(0.019)	(0.020)	(0.020)	(0.022)	(0.022)	(0.022)	(0.024)
Census block FE's	✓	✓	✓	✓	✓	✓	✓	✓
Year-by-Census tract FE's	✓	✓	✓	✓	✓	✓	✓	✓
Pop.-by-Year FE's		✓				✓		
Race Comp.-by-Year FE's			✓				✓	
Proximity controls	✓	✓	✓	✓	✓	✓	✓	✓
Unbalanced panel				✓				✓
N. Observations	1633170	1633170	1633170	1826381	867290	867290	867290	964886
N. Blocks	24321	24321	24321	24743	22774	22774	22774	23100
Adj. R^2	0.917	0.918	0.918	0.924	0.904	0.905	0.904	0.917
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. Full results presented in Appendix Tables A.2 and A.3. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

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.

As noted above, I examine the robustness of these findings using two alternate strategies.[12] First, I employ a synthetic difference-in-difference 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-difference 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.11), while the disaggregated effects suggest that the earliest injunctions generated the largest mobilization effects. In Appendix Figure A.4, I present event study estimates using a doubly robust, propensity score weighting estimator (Callaway and Sant'Anna 2021; Sant'Anna and Zhao 2020), which allows me to relax the parallel trends assumption to hold conditional on voting age population and racial composition (i.e. percent Black and

12. 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.

Latino). I again find positive and statistically significant effects. Importantly, these effects on registrations and voting appear to be long-lasting, extending well over a decade after the injunctions were put in place.

Non-electoral Participation

I turn next to the effects of gang injunctions on non-electoral political behavior with an analysis of the L.A.FANS panel survey data. In addition to offering a more comprehensive view of the political effects of police crackdowns, these data allow me to compare *within-individual* changes in self-reported behavior. Given that survey responses were only recorded at two points in time, I use a standard two way fixed effects approach rather than the stacked specification in Equation 1. Specifically I estimate the following difference-in-difference model:

$$y_{i,w} = \beta INJUNCTION_{i,w} + \gamma_i + \gamma_w + \epsilon_{i,w} \tag{2}$$

Where $y_{i,w}$ is an indicator for whether or not survey respondent i reported some form of non-electoral community involvement in survey wave w, and $\beta INJUNCTION_{i,w}$ is an indicator for whether an individual's Census block of residence is within an active injunction safety zone. To improve the plausibility of the parallel trends assumption, I restrict the sample to individuals residing 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.12 for the full list of variables and balance statistics).

Table 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.15 and A.13). As a final check, in the Appendix (Table A.16) 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

Table 2: **Effect of Gang Injunctions: Panel Survey Evidence**

	Nonelectoral Participation		Crime Victimization		Neighborhood Safety	
	(1)	(2)	(3)	(4)	(5)	(6)
Injunction	0.255**	0.152*	−0.080	−0.074	−0.219	−0.037
	(0.071)	(0.068)	(0.064)	(0.053)	(0.171)	(0.164)
Ind. FE's	✓	✓	✓	✓	✓	✓
Survey Wave FE's	✓	✓	✓	✓	✓	✓
CBPS Weights		✓		✓		✓
Full sample		✓		✓		✓
N. Observations	408	2352	408	2354	406	2335
N. Individuals	207	1180	207	1180	207	1180
Adj. R^2	0.259	0.215	0.513	0.421	0.194	0.166
R^2 (within)	0.061	0.023	0.005	0.004	0.021	0.001

Note: Estimates from Equation 2 fit on binary indicators of self-reported, nonelectoral civic participation, crime victimization, and rating one's neighborhood as "safe." Models 1, 3, and 5 restrict the sample to respondents living in treated Census tracts. Models 2, 4, and 6 are estimated using the entire sample and CBPS weights, which provide balance on demographic and neighborhood-level characteristics (see Appendix Table A.12 for full list of variables and balance statistics). 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. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

causal effect on civic involvement that extends beyond electoral participation.

Is This Evidence of a Backlash Effect?

I next investigate the mechanisms behind this increase in electoral participation. Theories of racialized policy feedback predict that members of ethno-racial groups who have a sense of group consciousness and perceive their group to be unfairly targeted by negative government action will mobilize to resist those policies (e.g. Zepeda-Millán 2016; Walker 2020a; Nuamah and Ogorzalek 2021; Garcia-Rios et al. 2023). 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). Contemporaneous evidence does suggest that many residents of the safety zones viewed injunctions as unfair and racially targeted (Muñiz 2015; Barajas 2007; Scott 2023). 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).[13] If this mobilization effect is motivated by backlash to aggressive police tactics I would expect residents of the safety zones to become more likely to report police discrimination, and for mobilization to be driven by the groups most likely to experience the negative enforcement effects of the injunctions—in this case Black and Latino residents. I would also expect decreased support for harsh criminal punishments within the safety zones.

I begin first with experiences with the police. In Wave II of the L.A.FANS 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 unfair police encounters using Census tract fixed effects. The results in Table 3 suggest individuals who reside within injunction zones are significantly more likely to report an unfair police encounter 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.[14]

Because gang injunctions may have been implemented in areas with poor police-community relations, 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.[15]

Across all specifications the estimated effects are similar in magnitude and substantively 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 unfair treatment at the hands of a police officer is 5.9% if he lives within the boundaries of a future gang injunction, but 42.1% for an individual with the same traits whose block of residence is already covered by an injunction. 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

13. A 1992 report by the L.A. County District Attorney 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).

14. 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.

15. LAPD reporting district-level crime data comes from Ridgeway and MacDonald (2017), mapped to Census tracts using GIS software.

Table 3: **Effect of Gang Injunctions on Perceived Unfair Treatment and Police Abuses**

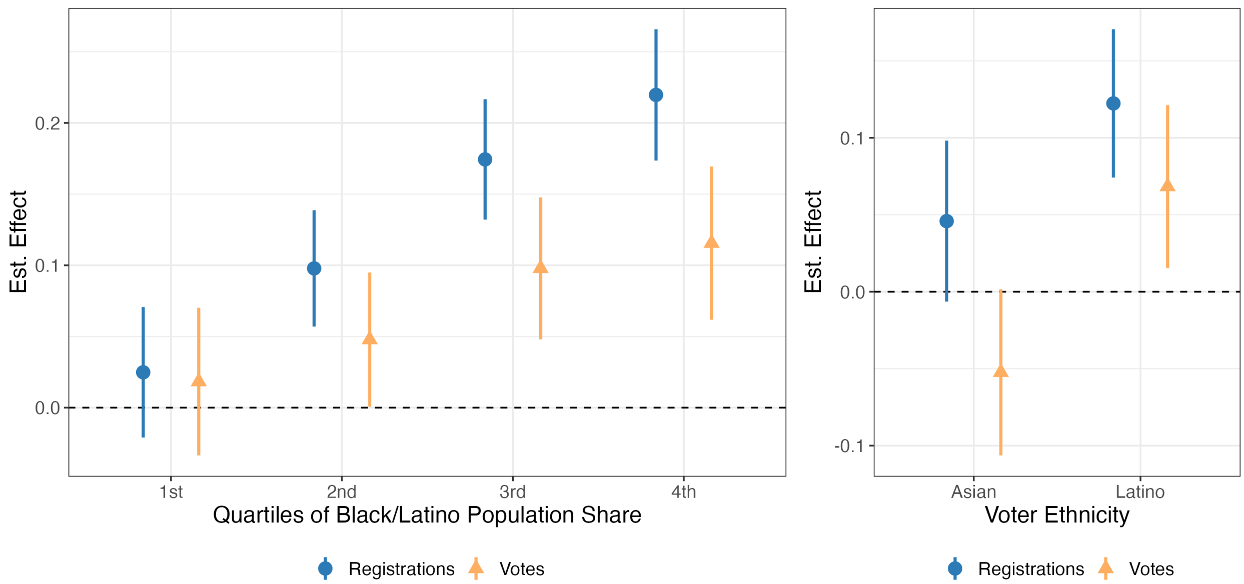
	(1)	(2)	(3)
Injunction	3.083^{*}	2.545^{*}	2.455^{**}
	(1.293)	(1.065)	(0.773)
Age		-0.052^{***}	-0.073^{*}
		(0.008)	(0.031)
Male		1.990^{***}	2.616^{***}
		(0.340)	(0.794)
Black		0.016	1.855
		(0.411)	(1.142)
Latino		0.129	1.074
		(0.326)	(1.069)
U.S. Born		1.010^{***}	1.344
		(0.301)	(1.083)
Food 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. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

and Latino respondents, who make up ≈ 92% of treated individuals in the data (Appendix Table A.19). In Appendix Table A.19, I also show that this effect appears to be driven almost entirely by men, who represent ≈ 95% of individuals in the statewide California Gang Database (Muñiz and McGill 2012).

To examine heterogeneity in electoral mobilization I leverage the racial composition of Census blocks, reestimating Equation 1 on block-level registrations and votes, interacting the post-treatment dummy with quartiles of the Black and Latino share of the population in 2000. As shown in Panel 1 of Figure 2, I find that the magnitude of the effect varies substantially with racial composition—increases in registrations and votes are largest in Census blocks with high percentages of Black and Latino residents. While the small size and relative racial homogeneity of Census blocks strongly suggests that Black and Latino residents of these blocks are responsible for these increases, in Panel 2 I cooborate these findings by taking advantage

Figure 2: **Heterogenous Effects**



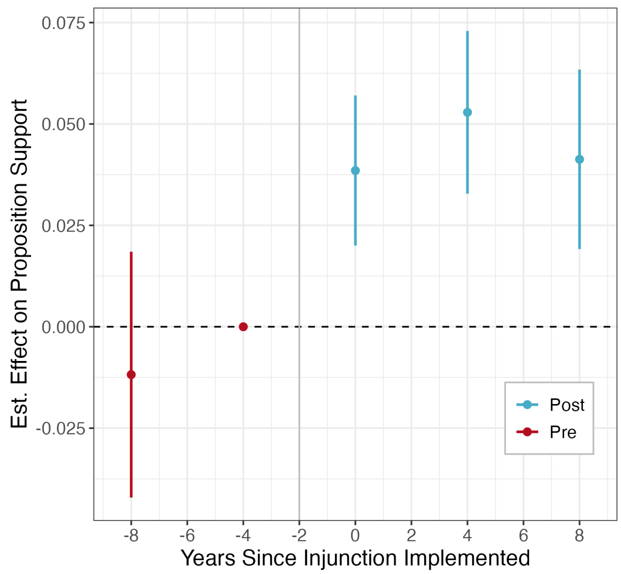
Note: Difference-in-difference estimates with 95% confidence intervals of the effect of civil gang injunctions on block-level registrations and votes. Panel 1 presents the marginal effect estimates from interacting the post-treatment dummy in Equation 1 with quartiles of block-level Black and Latino population share in 2000. Panel 2 presents separate effect estimates for Asian and Latino voters. Full model results in Appendix Tables A.7 and A.8. Robust standard errors are clustered by Census block.

of counts of registrations and votes by ethnicity provided by the Statewide Database, estimating Equation 1 separately for Latino and Asian registrations and votes. Gang injunctions led to large increases in Latino electoral participation, with an estimated 13% increase in registrations and 7% in votes cast. In contrast, the estimated effects on Asian participation are far smaller and statistically indistinguishable from zero—in the case of voting the point estimates are actually negative. In Appendix Table A.14, I show that these ethnic/racial differences also extend to individual differences in non-electoral participation, finding that gang injunctions only had significant mobilizing effects among residents who identified as Black or Latino.

Finally, I test whether gang injunctions increased support for criminal justice reform. To measure preferences for reform I use Census block-level support for three statewide ballot initiatives — Proposition 66 in 2004, Proposition 5 in 2008, and Proposition 36 in 2012 — that each sought to reduce penalties for nonviolent, low-level crimes. I estimate the effect of gang injunctions on the share of support for these three initiatives in 2004, 2008, and 2012 using a dynamic version of the stacked difference-in-difference specification given in Equation 1 with leads and lags of treatment.

The results are presented in Figure 3. Within Census tracts, I fail to find significant pre-treatment differences between treated and untreated blocks in support for these initiatives. Given the short pre-treatment period, I provide further support for the parallel trends assumption with a series of placebo

Figure 3: **Effects on Support for Criminal Justice Reforms**



Note: Dynamic difference-in-difference estimates with 95% confidence intervals of the effect of civil gang injunctions on block-level support for ballot propositions that decreased the severity of criminal punishments. Robust standard errors are clustered by Census block.

tests (Appendix Table A.17), showing that future treatment with an injunction does not predict within-neighborhood variation in support for the three ballot propositions. Following the implementation of a gang injunction, support for criminal justice reform increases significantly—by an average of 3.8% by the first post-injunction Presidential election, and 5.2% by the second. This represents a substantial shift in preferences (≈ 10-15% of the pre-treatment mean). While I am unable to determine the extent to which these shifts are due to changes in individual preferences versus changes to the composition of the electorate, this finding is consistent with mobilization being driven by new voters who were particularly likely to oppose current law enforcement practices.

Alternative Mechanisms

I consider several alternative mechanisms that may explain these effects. First, I consider the role of crime. Given previous findings that gang injunctions led to modest, short-term decreases in crime (Grogger 2002; Ridgeway et al. 2019), it is possible that some of the mobilization effects I find are the result of residents feeling safer and better served by law enforcement (Ley 2018; Trelles and Carreras 2012).[16] Conversely, residents may have taken injunctions as a signal that that crime and gang violence were on the rise and of particular concern in their neighborhood, mobilizing voters concerned with perceived disorder in their

16. While some individual survey evidence links crime victimization to increased political participation (e.g. Bateson 2012), I focus on the possibility that crime reduction is positively biasing my estimates.

community (Brown and Zoorob 2022).

If this result is primarily explained by crime I would expect the effect of gang injunctions to be partially mediated by changes in the official crime rate, and/or for injunctions to substantially alter residents' subjective experiences with crime and disorder. Using geocoded, incident-level crime data from 2010 to 2020, I first estimate the controlled direct effect of the four, post-2009 injunctions net crimes known to the police.[17] The results, presented in Appendix Table A.10, provide no evidence that voter mobilization is being mediated by changes in crime. To determine whether injunctions altered perceptions of crime I rely on two questions from the L.A.FANS survey which asked respondents about property crime victimization and neighborhood safety (see Appendix Section B for exact question wording). Following my analysis of non-electoral participation, I estimate Equation 2 on indicators for whether respondents reported being the victim of a crime, and whether they rated their neighborhood as "Completely" or "Fairly" safe. Estimates for both outcomes, presented in Table 2, are small and statistically insignificant. The coefficient for crime becomes significant with the inclusion of tract-by-wave fixed effects (Appendix Table A.13), 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.

Lastly, one could argue that the increases in registrations and votes I find are due to selective mobility in response to the injunctions. For example, gang injunctions may have facilitated gentrification (Muñiz 2015), displacing poor residents with more affluent and politically active ones (Schlozman, Verba, and Brady 2013). Several factors suggest this is unlikely. First, the "thinness" of the residential housing market imposes substantial limits on residential sorting within small geographic areas, such as Census tracts (Bayer, Ross, and Topa 2008). Second, Owens, Mioduszewski, and Bates (2020) find that injunctions reduced both property values and the share of White in-movers, which conflicts with documented patterns of gentrification in Los Angeles (Scott 2019).[18] Lastly, I find no evidence that gang injunctions changed overall mobility patterns or led to population increases. 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 that were and were not covered by an injunction (13.5% vs. 13.8%). And estimates from a difference-in-difference model with year-by-Census tract fixed effects comparing changes in population from 2000 to 2020 between treated and untreated blocks are negative and statistically insignificant (Appendix Table A.6). This indicates that if anything, the population of the safety

17. The LAPD only reports crime data prior to 2010 at the Reporting District (RD) level—a geographic unit similar in size to Census tracts, drawn to include roughly equal populations

18. It is possible that this relative depreciation in home values mobilized concerned homeowners (Hall and Yoder 2022). While I cannot rule this possibility, I find that much of the mobilizing effect is concentrated among young people (Appendix Table A.14), 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). It is unclear how sensitive home owners are to slower relative rates of price growth versus depreciation in the real value of their property.

zones slightly *decreased* relative to surrounding neighborhoods.

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 which often fail to meaningfully control crime. This paper provides evidence that these tactics can have profound political effects that extend far beyond those who are stopped and arrested. Leveraging a series of anti-gang crackdowns in Los Angeles, I find that localized expansions of police power and the criminal code led to large and sustained increases in registrations, voting, and non-electoral forms of community engagement. Consistent with contemporaneous accounts of community resistance to these policies (Muñiz 2015; Barajas 2007; Scott 2023), I show that civil gang injunctions produced large increases in perceived unfair treatment by the police among Black and Latino men, that mobilization was in turn driven by Black and Latino residents, and that voters became more supportive of criminal justice reform. These findings suggest that police tactics that are viewed as unjust and racially targeted can lead to widespread electoral backlash that outweighs the demobilizing effects of direct police encounters.

While this mobilization effect is consistent with research on group-based mobilization in race and ethnic politics (Nuamah and Ogorzalek 2021; Zepeda-Millán 2016), it contrasts with much of the previous literature on policing which finds that high levels of arrests are associated with low rates of turnout at the neighborhood level. The focus here on a single case prevents drawing sweeping conclusions. Nevertheless, I maintain that in many important respects gang injunctions present an ideal case to test the ability of aggressive and discriminatory policing to alter political behavior. By focusing on official changes in the policies of the police that led to long-term changes in the risk residents faced in being stopped, questioned, or harassed by officers, my analysis offers a close fit between existing theory and empirics. My approach also relies on a more credible identification strategy by exploiting hyperlocal and plausibly exogenous variation in exposure to these policies. It is notable, then, that this study and other recent work on the causal effect of police killings find strong evidence of mobilization (Ang and Tebes 2024). While more research is needed, electoral resistance to police overreach may be a more common outcome than previously thought.

Of course, there are several caveats regarding generalizability and the interpretation of the findings. First, despite being geographically targeted, gang injunctions came to cover wide swaths of Los Angeles—over 40% of the city's land area. This raises the possibility that gang injunctions primarily mobilized individuals in relatively affluent neighborhoods with low levels of police enforcement and crime, where the symbolism of having one's neighborhood targeted by an injunction generated backlash effects that outweighed the depressive effects of (in)direct police contact. While I lack data on officer movements, several pieces of evidence cast doubt on this explanation. First, in Appendix Table A.9 I show that mobilization was greatest in neighborhoods that historically had the highest levels of crime—places that generally feature high levels of economic disadvantage, and where the police patrol the most aggressively (Braga, Brunson, and Drakulich 2019). Second, qualitative evidence suggests 1) that residents of injunction safety zones were exposed to high levels of police raids, investigatory stops, and harassment by officers, and 2) that mobilization against gang injunctions was often motivated by these negative experiences with the police (Barajas 2007; Muñiz 2015). In the city of Oxnard, Latina/o/x youth who were frustrated with zero tolerance policing within the Colonia Chiques gang injunction safety zone played a key role in organizing resistance to the injunction by attending community meetings, protesting, and registering to vote (Barajas 2007). I similarly find the largest mobilization effects among the groups most likely to be labeled as gang members by the police—Black, Latino, and young individuals—who we would expect to be disproportionately likely to experience the negative enforcement effects of injunctions.

The clear, widely publicized boundaries of the safety zones may have also facilitated a sense of policy threat by making gang injunctions uniquely traceable to specific policy decisions and elected officials (Arnold 1990). While policing is generally thought to be a highly visible form of government action that can provide signals about state performance and responsiveness (Anoll, Epp, and Israel-Trummel 2022), many of the extreme inequalities seen in police presence and practices across neighborhoods may not be easily attributable to a specific policy or program (e.g. Chen et al. 2023). This raises questions about how much voters actually notice changes in police behavior in the absence of public policy declarations, and whether they can attribute those changes to a specific cause or political actor in ways that encourage voting. In the case of injunctions, evidence suggests that residents both perceived how much more aggressively the police were behaving within the safety zones, and were able to connect those inequalities to the official gang suppression policies that were being championed by the city government (Muñiz 2015). To the extent that this may have facilitated mobilization, it suggests that existing theories of proximal contact with the police should be more attentive to the role of policy design and the information environment.

Considering the role of context, Los Angeles' history of high profile police abuses may have made residents unusually predisposed to mobilize against this aggressive approach to crime control. In particular, one may wonder about the role of the 1992 Los Angeles riots, which inspired a high level of community activism in the area and contributed to a strong sense of racial threat in many Black and Latino communities (Bedolla 2005). Of course, the riots may have also attenuated the effect of the injunctions by undermining faith in elections and city government, or by mobilizing the voters most concerned with police aggression long before the injunctions were put in place. I explore the potential role of the 1992 police beating of Rodney King and subsequent uprising in the Appendix. While previous work finds that the political consequences of the riot were strongly correlated with distance to its geographic origin (Enos, Kaufman, and Sands 2019), I find no equivalent relationship between distance to the riot and mobilization from gang injunctions (Appendix Figure A.3). This is perhaps unsurprising given that the vast majority of L.A.'s gang injunctions were implemented ten to twenty years after the riot occurred. Moreover, the long period I study—nearly 30 years—casts doubt on claims that these results can be explained by single events or idiosyncratic fluctuations in the local political climate. Indeed scholars have noted that broad trends in crime, police scandals, racial tensions, and attempts at police reform in Los Angeles over this period mirror the events in other large American cities (Fagan and MacDonald 2012).

Mirroring documented instances of mobilization in other cities (e.g. Nuamah and Ogorzalek 2021; Walker 2020a), qualitative evidence does suggest that community organizations played a role in fostering a sense of group consciousness, framing gang injunctions as racist, and providing the information, resources, and coordination to facilitate mass mobilization (Scott 2023). At the same time, receptivity to activists' messages in Southern California appears to have varied widely by neighborhood, depending heavily on residents' concerns with crime and their lived experiences of criminalization and marginalization (Scott 2023). While beyond the scope of this paper, these observations suggest that further research into both activist networks and crime may deepen our understanding of how and when highly policed communities come to be mobilized.

Scholars have long recognized that policing is not limited to stopping specific individuals, but often subjects entire communities to increased surveillance and suspicion. Despite this, existing empirical scholarship on the political effects of the criminal justice system has overwhelmingly treated policing as an individual-level encounter, leaving open the question of how the character of policing in one's community impacts political attitudes and behavior. Here, I show that anti-crime crackdowns that expanded the criminal code and empowered officers to make large numbers of investigatory stops and arrests spurred significant electoral backlash. Despite their frequent characterization as demobilized, my findings suggest that highly policed communities can and do come to participate in elections at high rates in the face of unwanted police practices. This raises important questions about what this participation is able to achieve, and how local officials respond to the demands of politically marginalized communities. Answering these questions holds important implications for democratic accountability and the prospects of police reform.

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A Appendix

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Data Availability and Sharing

The L.A.FANS data are distributed by the Data Sharing for Demographic Research (DSDR) project housed within the Inter-university Consortium for Political and Social Research (ICPSR). This analysis relies on the restricted-access Versions 2 and 2.5 of the L.A.FANS data, which include the Census tract and block of residence for each household in the sample. To protect respondent confidentiality and minimize the risk of the deductive disclosure of respondents' identities, this granular geographic information can only be accessed through a restricted data contract. All analysis of the L.A.FANS data was performed within the ICPSR virtual data enclave, and output was reviewed for potential disclosure risk by ICPSR staff before leaving this secure computing environment.

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		
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		

Continued on next page

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

Case	Complaint Filed	Preliminary Injunction	Permanent Injunction	End Date	Resumed As
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		
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		

Continued on next page

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

Case	Complaint Filed	Preliminary Injunction	Permanent Injunction	End Date	Resumed As
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		
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		

Table A.2: **Effects on Registrations**

	(1)	(2)	(3)	(4)	(5)
Injunction	0.106^{***}	0.101^{***}	0.082^{***}	0.087^{***}	0.096^{***}
	(0.019)	(0.019)	(0.020)	(0.020)	(0.021)
100m	0.075^{***}	0.070^{***}	0.058^{***}	0.028^{**}	0.078^{***}
	(0.010)	(0.010)	(0.010)	(0.011)	(0.010)
500m	0.030^{***}	0.028^{***}	0.023^{***}	0.001	0.019^{***}
	(0.005)	(0.005)	(0.005)	(0.006)	(0.005)
1000m	0.004	0.004	0.001	-0.015^{**}	0.004
	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)
Census block FE's	✓	✓	✓	✓	✓
Year-by-Census tract FE's	✓	✓	✓	✓	✓
Pop.-by-Year FE's		✓			
Race Comp.-by-Year FE's			✓		
Unbalanced panel				✓	✓
Constant block boundaries					✓
N. Observations	1633170	1633170	1633170	1826381	1413821
N. Blocks	24321	24321	24321	24743	23698
Adj. R^2	0.917	0.918	0.918	0.924	0.929
R^2 (within)	0.00	0.00	0.00	0.00	0.00

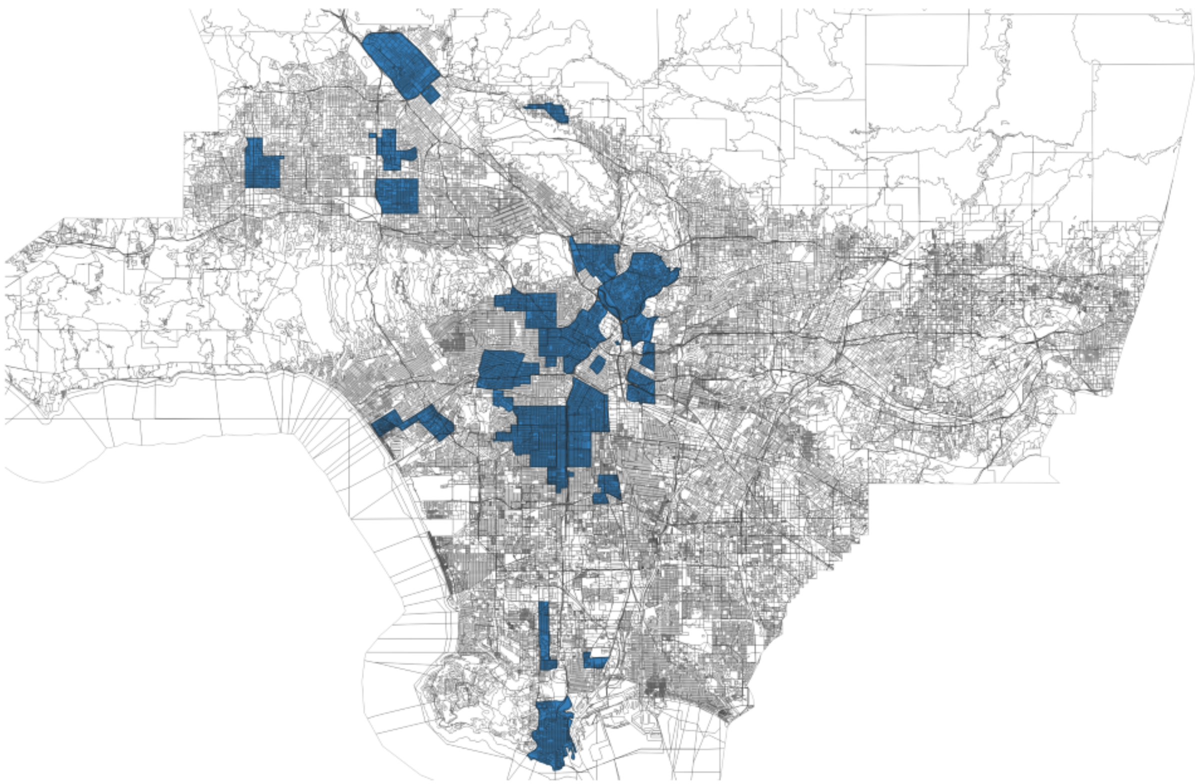
Note: OLS estimates. Model 1 presents the main specifications for registrations. Models 2 and 3 include deciles of the 2000 population-by-year fixed effects. Model 3 includes quartiles of Black and Latino share of the 2000 population-by-year fixed effects. Model 4 expands the sample to the full, unbalanced panel. Model 5 restricts the sample to Census observations that to not use areal weighting. Robust standard errors clustered by Census block given in parentheses. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

Table A.3: **Effects on Voting**

	(1)	(2)	(3)	(4)	(5)
Injunction	0.057^{*}	0.049^{*}	0.046^{*}	0.064^{**}	0.045^{*}
	(0.022)	(0.022)	(0.022)	(0.024)	(0.021)
100m	0.043^{**}	0.041^{*}	0.036^{*}	0.014	0.052^{***}
	(0.016)	(0.016)	(0.016)	(0.016)	(0.012)
500m	0.034^{***}	0.030^{***}	0.031^{***}	0.018^{*}	0.017^{**}
	(0.007)	(0.007)	(0.007)	(0.008)	(0.007)
1000m	0.020^{***}	0.020^{***}	0.019^{***}	0.011	0.013^{*}
	(0.005)	(0.005)	(0.005)	(0.006)	(0.005)
Census block FE's	✓	✓	✓	✓	✓
Year-by-Census tract FE's	✓	✓	✓	✓	✓
Pop.-by-Year FE's		✓			
Race Comp.-by-Year FE's			✓		
Unbalanced panel				✓	✓
Constant block boundaries					✓
N. Observations	867290	867290	867290	964886	842218
N. Blocks	22774	22774	22774	23100	
Adj. R^2	0.904	0.905	0.904	0.917	0.912
R^2 (within)	0.00	0.00	0.00	0.00	0.00

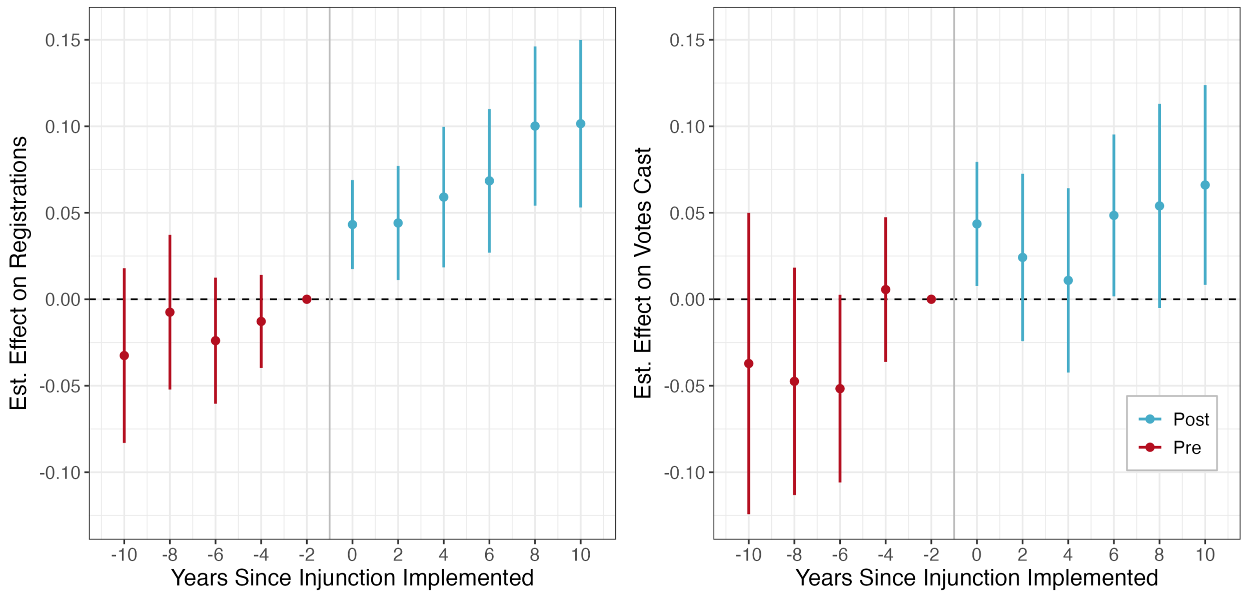
Note: OLS estimates. Model 1 presents the main specifications for votes. Models 2 and 3 include deciles of the 2000 population-by-year fixed effects. Model 3 includes quartiles of Black and Latino share of the 2000 population-by-year fixed effects. Model 4 expands the sample to the full, unbalanced panel. Model 5 restricts the sample to Census observations that to not use areal weighting. Robust standard errors clustered by Census block given in parentheses. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

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.

Figure A.2: **Effects on Voting and Voter Registrations: Event Study Estimates**



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 **??**, with the post-treatment indicator replaced by leads and lags of treatment.

Table A.4: **Effects on Voting and Voter Registrations: 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)
100m	0.075***	0.077***	3.719***	0.043**	0.043*	0.153
	(0.010)	(0.010)	(0.781)	(0.016)	(0.018)	(0.555)
500m	0.030***	0.029***	3.450***	0.034***	0.036***	1.764***
	(0.005)	(0.006)	(0.477)	(0.007)	(0.008)	(0.404)
1000m	0.004	0.002	0.876*	0.020***	0.021***	0.816**
	(0.004)	(0.005)	(0.362)	(0.005)	(0.006)	(0.293)
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. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

Table A.5: **Spillover Effects**

	(1)	(2)	(3)	(4)
In Safety Zone (Treated)	0.115 (0.021)***	0.107 (0.020)***	0.109 (0.021)***	0.082 (0.022)***
100m	0.083 (0.012)***	0.076 (0.012)***	0.072 (0.012)***	0.024 (0.013)
500m	0.039 (0.009)***	0.034 (0.009)***	0.034 (0.009)***	−0.003 (0.010)
1000m	0.012 (0.008)	0.010 (0.008)	0.010 (0.008)	−0.019 (0.009)*
1500m	0.009 (0.007)	0.007 (0.007)	0.009 (0.007)	−0.005 (0.008)
2000m	0.005 (0.006)	0.001 (0.006)	0.001 (0.006)	−0.004 (0.007)
2500m	0.005 (0.005)	0.003 (0.005)	0.005 (0.005)	0.014 (0.006)*
3000m	0.003 (0.004)	0.002 (0.004)	0.002 (0.004)	0.002 (0.004)
Census block FE's	✓	✓	✓	✓
Year-by-Census tract FE's	✓	✓	✓	✓
Pop.-by-Year FE's		✓	✓	
Race Comp.-by-Year FE's			✓	
Full sample				✓
N. Observations	1633170	1633170	1633170	1826381
Adj. R^2	0.917	0.918	0.919	0.924
R^2 (within)	0.00	0.00	0.00	0.00

Note: Difference-in-difference estimates on logged registrations using the specification from Equation 1 with mutually exclusive indicators of distance to the nearest safety zone boundary at 500 meter intervals up to 3 kilometers. Being within the boundary indicates a Census block was treated. In some cases, Census blocks included a 100 meter buffer zone in which the injunction restrictions applied. Robust standard errors clustered by Census block given in parentheses. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

Table A.6: **Effect of Injunctions on Population (2000 - 2020)**

	N. Residents	N. Registrants	Prop. Black	Prop. Latino	Prop. White
Injunction	−2.691	8.763***	−0.006	−0.009	0.029***
	(3.458)	(2.431)	(0.005)	(0.008)	(0.005)
Census Block FE's	✓	✓	✓	✓	✓
Year-by-Census tract FE's	✓	✓	✓	✓	✓
N. Observations	71301	71301	70246	69265	70246
Adj. R^2	0.945	0.868	0.887	0.944	0.922
R^2 (within)	0.00	0.00	0.00	0.00	0.00

Note: Difference-in-difference estimates of population change and racial composition between the 2000, 2010, and 2020 Decennial Censuses with fixed effects for year and Census block. In model two the estimated effect on the untransformed number of registered voters is included for comparison. Robust standard errors clustered by Census block given in parentheses. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

Table A.7: **Effect of Injunctions by Block Racial Composition**

	Registrations	Votes Cast
Injunction	0.025	0.018
	(0.023)	(0.026)
100m	0.075***	0.043**
	(0.010)	(0.016)
500m	0.030***	0.034***
	(0.005)	(0.007)
1000m	0.004	0.021***
	(0.004)	(0.005)
2nd Quartile × Injunction	0.073***	0.030
	(0.015)	(0.019)
3rd Quartile × Injunction	0.150***	0.080**
	(0.020)	(0.025)
4th Quartile × Injunction	0.195***	0.097***
	(0.023)	(0.027)
Census Block FE's	✓	✓
Year-by-Census tract FE's	✓	✓
N. Observations	1633170	867290
Adj. R^2	0.917	0.904
R^2 (within)	0.00	0.00

Note: Difference-in-difference estimates with interaction between post-treatment dummy and quartiles of the block-level Black and Latino share of the population in 2000. Robust standard errors clustered by Census block given in parentheses. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

Table A.8: **Effect of Injunctions by Ethnicity**

	Registrations		Votes Cast	
	Latino	Asian	Latino	Asian
Injunction	0.122***	0.046	0.068*	−0.052
	(0.025)	(0.027)	(0.027)	(0.028)
100m	0.044***	−0.069***	0.023	−0.057***
	(0.012)	(0.013)	(0.017)	(0.015)
500m	0.052***	−0.006	0.038***	−0.011
	(0.007)	(0.008)	(0.009)	(0.009)
1000m	0.019**	−0.012*	0.027***	−0.014*
	(0.006)	(0.006)	(0.007)	(0.007)
Census Block FE's	✓	✓	✓	✓
Year-by-Census tract FE's	✓	✓	✓	✓
N. Observations	1633170	1633170	867290	867290
Adj. R^2	0.906	0.821	0.886	0.795
R^2 (within)	0.00	0.00	0.00	0.00

Note: Difference-in-difference estimates fit separately for registrations and votes cast by Latino and Asian voters. Robust standard errors clustered by Census block given in parentheses. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

Table A.9: **Effect of Injunctions by Quintiles of Average Crime Between 1990 and 2000**

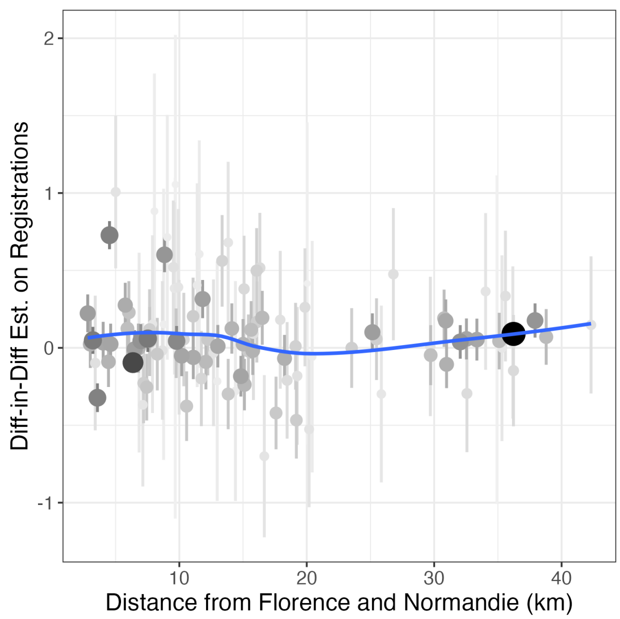
Crime Quintile:	1st	2nd	3rd	4th	5th
			Registrations		
Injunction	0.008	0.089	0.067	0.106**	0.153***
	(0.069)	(0.047)	(0.037)	(0.037)	(0.036)
N. Observations	271572	317730	337400	352632	353836
Adj. R^2	0.932	0.918	0.913	0.918	0.907
R^2 (within)	0.001	0.001	0.001	0.001	0.002
			Votes Cast		
Injunction	−0.029	0.070	0.017	0.048	0.111**
	(0.103)	(0.045)	(0.043)	(0.048)	(0.040)
N. Observations	148090	170510	177050	187110	184530
Adj. R^2	0.930	0.899	0.890	0.903	0.895
R^2 (within)	0.000	0.000	0.002	0.001	0.001
Census Block FE's	✓	✓	✓	✓	✓
Year-by-Census tract FE's	✓	✓	✓	✓	✓

Table A.10: **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. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

Figure A.3: **Relationship Between Estimated Effect of Injunctions and Distance to the Origin of the 1992 Riots**



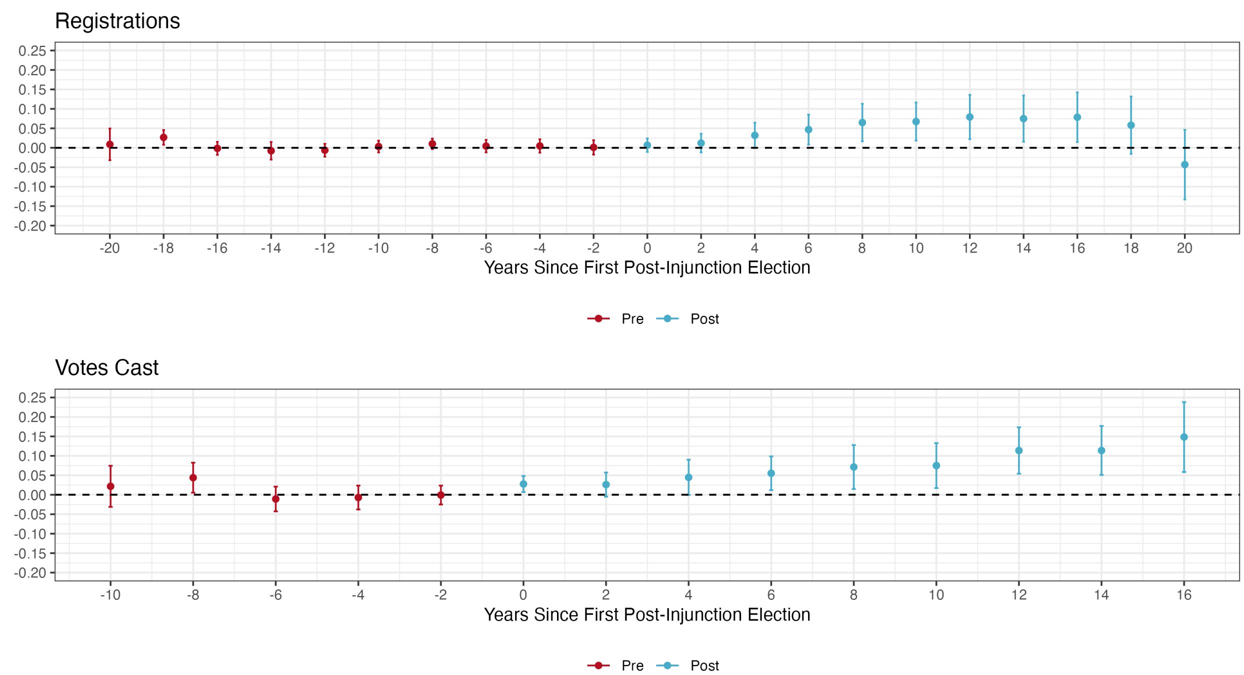
Note: Stacked difference-in-difference estimates on block-level registrations fit separately by Census tract, with loess curve weighted by the inverse standard error of the estimates. Larger, darker points indicate higher precision. Distance in meters is given from the centroid of each Census tract to the geographic origin of the riot at the intersection of Florence and Normandie Avenues in South Central Los Angeles (latitude and longitude coordinates come from Enos, Kaufman, and Sands (2019)).

Table A.11: **Synthetic Difference-in-Difference Estimates on Voting and Voter Registrations**

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-difference 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.

Figure A.4: **Event Study Estimates (Callaway and Sant'Anna Estimator)**



Note: Dynamic difference-in-difference 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 2000 voting-age population (logged) and the share of the 2000 population that is Latino and Black.

Table A.12: **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.

Table A.13: **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.14: **Effect Heterogeneity of Injunctions on Non-electoral Participation**

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

Table A.15: **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. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

Table A.16: **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. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

Table A.17: **Placebo Tests of Future Gang Injunctions on Ballot Proposition Support**

	Prop. 66 (2004)	Prop. 5 (2008)	Prop. 36 (2012)
Future Injunction	-0.009	0.014	-0.010
	(0.006)	(0.008)	(0.007)
Census tract FE's	✓	✓	✓
N. Observations	22680	22522	23033
Adj. R^2	0.637	0.551	0.691
R^2 (within)	0.00	0.00	0.00

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

Table A.18: **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.19: **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

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

B Survey Question Wording

Crime Victimization: "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? [Response Options: Yes; No]"

Neighborhood Safety: “How safe is it to walk around alone in your neighborhood after dark? Is it: [Response Options: [Completely safe; Fairly safe; Somewhat dangerous, or; Extremely Dangerous]”