Can Policing Increase Participation? Evidence from a Randomized Field Experiment

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September 28, 2025
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Abstract

Existing scholarship documents the ways in which involuntary contact with police can harm and demobilize the public, but policing can also provide important benefits in the form of reassurance and crime control. Because of this, the aggregate consequences of high levels of policing are unclear. To test the effects of policing on turnout, I leverage a unique field experiment in which the presence of the police and the tactics they used were randomized. Some neighborhoods experienced high-intensity patrols that generated large numbers of stops and arrests; others experienced patrols that emphasized non-enforcement, community-oriented tactics; while others experienced no changes in policing. I find that both interventions increased turnout in subsequent elections by roughly 3 percentage points despite large differences in officer behavior. These findings offer the strongest evidence to date that increasing the presence of the police in neighborhoods with high crime rates can generate positive feedback effects that encourage voting.

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Introduction

Policing and the provision of safety are considered some of the most basic functions of government (Hobbes 1929). Yet law enforcement provides a combination of both benefits and harms to the public. Targeted increases in the presence of the police deter crime and can reassure the public (Braga et al. 2019), but also increase residents' exposure to stops, citations, and arrests — costly encounters that can serve as a visible reminder of an intrusive or oppressive state. These countervailing forces make the aggregate consequences of the presence of the police unclear. Determining whether, and when, the benefits of heightened police presence ultimately outweigh its social and civic costs remains a critical and unresolved question.

Understanding the aggregate effects of policing is important because police contact is both widespread and politically consequential; the downstream effects of law enforcement shape which citizens make it to the polls and, by extension, whose interests are represented in government. Each year roughly 50 million Americans experience some sort of contact with the police (Tapp and Davis 2024), and countless more people witness those encounters. Such moments communicate powerful cues that can influence trust in institutions, perceptions of state capacity, and decisions about whether to participate in politics (Anoll, Epp, and Israel-Trummel 2022; Burch 2013; Lerman and Weaver 2020; Morris and Shoub 2024; White 2019). But despite a rapidly expanding literature on the political feedback effects of the carceral state (White 2022), there remain significant gaps in our understanding of policing's impacts on the electorate. Existing empirical evidence is largely based on forms of police coercion and violence that are rare, extreme, or unambiguously harmful, such as being involuntarily stopped, arrested, and incarcerated, or spatial proximity to fatal shootings and egregious cases of racial profiling (e.g. Ang and Tebes 2024; White 2019; Naftel 2025). These events are important, but they are far from the modal way that people encounter law enforcement. Officers spend the vast amount of their time on patrol and responding to calls for service. What political lessons do citizens draw from that visibility? And to what extent are these responses conditioned on the propensity of officers to rely on coercion?

This paper provides new evidence on the causal effect of policing on political behavior using a large-scale, randomized policing experiment in Jacksonville, Florida. Between January and April 2009, the Jacksonville Sheriff's Department identified 83 hotspots of violent crime — overwhelmingly located in poor, predominantly Black neighborhoods — and randomly assigned them to one of two treatments or a control group. Half of the treated hotspots were saturated with an average of 53 patrol hours per week, which resulted in a 85% increase in investigatory stops and arrests during the study period (Taylor, Koper, and Woods 2011). The other half received similar increases in the presence of the police; however, officers in this treatment arm were trained to use community-oriented problem solving approaches to reduce violence, and encouraged to avoid enforcement whenever possible. Arrest data and activity logs confirm that these officers were far less likely to rely on coercion. Instead, officers conducted surveys of residents and coordinated with property owners and other city agencies to improve infrastructure and access to social services (Taylor, Koper, and Woods 2011). Previous work has established the efficacy of these interventions in reducing crime, yet has ignored their potential *political* effects. These substantial, randomized changes in the presence of the police, as well as in the tactics officers used, present a unique opportunity to study the causal effects of police surges in communities with high crime rates.

I leverage these exogenous changes in policing by linking individual voter files and local election returns to the experimental hotspots to assess their effects on political behavior. I find that the experimental policing interventions raised voter turnout in subsequent elections by roughly three percentage points, with the clearest effects among Black voters. Strikingly, this mobilizing effect is statistically indistinguishable across the two treatment arms, despite large differences in the propensity of officers to engage in stops and arrests. Drawing on precinct-level election returns, local news coverage, and police computer-aided dispatch data, I find no evidence consistent with a backlash mechanism — such as peripheral-voter mobilization, anti-incumbent swings, protests, or spikes in fear-driven calls. Instead, I argue that the presence of officers increased turnout by reassuring residents concerned with crime, and provide suggestive evidence consistent with this mechanism.

These findings make several contributions. First, they offer the most direct causal evidence to-date of how police deployment decisions affect political behavior. The positive turnout effects I find, coupled with the absence of measurable electoral backlash, suggest that even short-lived policing interventions can create conditions more conducive to civic life. The modest declines in crime that these interventions achieved strongly suggest that the presence of officers had meaningful interpretive effects on residents, rather than indirect effects via crime reduction. Second, by distinguishing between police visibility and enforcement, this finding contributes to literatures in policy feedback, demonstrating that symbolic cues of state presence can mobilize citizens.

Theoretical Background

Policing As a Source of Negative Political Learning

When departments deploy more personnel to a neighborhood they increase the probability that residents of those places will be stopped, ticketed, searched, or arrested. A large and growing body of evidence shows that these involuntary encounters depress political participation. Scholars find that a variety of forms of contact with the police and criminal justice system, including traffic stops and misdemeanor arrests, can demobilize voters (Ben-Menachem and Morris 2022; White 2019). This is thought to be due to the material costs of criminal justice involvement — which can range from license suspensions to loss of employment and heavy fines — as well as the civic lessons these encounters transmit. Work by Lerman and Weaver (2020) shows that these encounters can signal to people that at best, the government is cruel and unresponsive, and at worst, that the government is something to be actively avoided.

Importantly, the political consequences of policing are not confined to those who experience direct contact; they spill outward through neighborhood networks to shape the attitudes and behavior of the broader community (Burch 2013). Members of the public often learn about stops, searches, and arrests vicariously from relatives, friends, social media, and local news, and these second-hand accounts color expectations about their own prospects of being targeted, the fairness of the legal process, and the state's willingness to protect them (Anoll, Epp, and Israel-Trummel 2022). Because involuntary encounters are often densely concentrated in poor, predominatly Black neighborhoods, these vicarious experiences can contribute to a shared understanding of the police as predatory and the law as selectively enforced (Bell 2017). Scholars describe this orientation as legal cynicism: a belief that, "the law and the agents of its enforcement, such as the police and courts, are viewed as illegitimate, unresponsive, and ill-equipped to ensure public safety" (Kirk and Papachristos 2011, 1191). As with direct contact, legal cynicism is often thought to erode civic engagement by encouraging the avoidance of institutions and lowering the perceived returns of electoral participation (Laniyonu 2019; Lerman and Weaver 2020). But in rare cases — typically after highly publicized violence or abuse — the same shared understandings can catalyze collective action, prompting protests and electoral mobilization among those who feel targeted on the basis of race or class (Ang and Tebes 2024; Naftel 2025; Garcia-Rios et al. 2023).

Policing's Benefits

However, policing also delivers collective benefits that may generate *positive* feedback effects at the community level — a possibility that has received far less empirical attention. A substantial body of evidence finds that visible patrols deter crime and reduce visible signs of disorder (Braga et al. 2019). While isolated instances of criminal victimization can mobilize voters (Bateson 2012), a growing body of work suggests that chronic exposure to violence and other forms of criminal activity imposes substantial, social, economic, and psychological harms that can discourage political engagement, particularly among Black Americans (Moffett-Bateau 2023; Jones 2024). Exposure to crime, as well as direct victimization, have been linked to chronic stress, social isolation, and weak neighborhood ties (Cornaglia, Feldman, and Leigh 2014; Dustmann and Fasani 2016; Ross, Mirowsky, and Pribesh 2001; Moffett-Bateau 2023), reducing the resources and opportunities individuals have at their disposal to become involved with politics. Any police intervention that alleviates these burdens may therefore raise turnout.

The police can also reshape neighborhood social dynamics in ways that encourage participation. Rinehart Kochel and Weisburd (2019) describe a mechanism through which an increase in the perceived reliability of the police can embolden residents of neighborhoods with high crime rates to work to solve collective problems and enforce social norms. Studying a randomized hotspots experiment in St. Louis County, they find that a substantial increase in police patrols increased residents' sense of closeness with their neighbors and willingness to intervene to stop signs of social disorder. Improved trust among residents and a stronger sense of collective efficacy could in turn lead to greater civic engagement. Ties with neighbors can induce social pressure to participate in politics, facilitate the diffusion of electoral information, and provide opportunities to become involved with campaigns or community projects (McClurg 2003; Sinclair 2012; Verba, Schlozman, and Brady 1995). A climate of informal social control also reduces the personal risks — real or perceived — of public activity in high-crime areas, making it easier to attend meetings, host registration drives, or simply walk to the polls after dark.

The presence of the police may also increase participation more directly by improving views of law enforcement and government institutions. Both qualitative and survey evidence demonstrate that while many Black Americans routinely condemn police harassment and violence, they also believe the police do not protect them from crime (Carr, Napolitano, and Keating 2007; Meares 1997). This "dual frustration" is thought to be rooted as much in under-protection as in over-policing, with Black people experiencing both a disproportionate exposure to coercion and a deficit of basic service. When killings go unsolved or officers only appear in moments of crisis, residents can come to understand law enforcement as indifferent, a judgment that is thought to generalize to other government institutions and discourage political engagement (Jones 2024). A sustained surge in patrols that succeeds in reducing violence may therefore send a countervailing signal that state is paying attention and is capable of providing basic security, making voting feel worthwhile.

To date, only a handful of studies have attempted to empirically test whether the presence of the police might generate these positive feedback effects. Turner and Shum (2025) find weak, positive correlations between day-to-day police exposure, institutional trust, and political participation, but they rely on cross-sectional differences within a single city, and use a rough proxy for police presence (distance to the nearest police station). Other, causally identified work has also been limited in important respects. Building off work by Mello (2019), which leverages quasi-random variation in size of local police forces induced by the Federal COPS hiring program to show that hiring more police officers can reduce crime, Romero (2025) shows that this program also increased voting participation among Black men — a group disproportionately at risk of criminal victimization. While this demonstrates that policing may sometimes generate voting gains in the aggregate, it tells us little about the effects of *exposure* to policing activity, particularly in poor, racially segregated communities that are heavily policed. Relying on indicators like police hiring also raises questions about the importance of the behavior of officers, given the widespread assumption that the effects of policing depend on the character of police contact (Anoll, Epp, and Israel-Trummel 2022; Lerman and Weaver 2014; Turner and Shum 2025). I explore these questions by examining the effects of policing in neighborhoods that directly experienced sudden surges of law enforcement activity, leveraging a setting that allows me to separate the effects of the presence of the police from the tactics they use.

The Question of Police Tactics

Given the substantial material and social harms that come from experiencing a police stop or arrest, scholars often treat coercion as the core channel through which policing reshapes civic attitudes and behaviors (e.g. Laniyonu 2019; Palmer 2024). Yet most contact with the police is indirect, and there is strong evidence that the presence of uniformed patrol officers and police vehicles has strong perceptual effects on the public that can alter individual behavior and communicate ideas about legitimacy, accountability, and the prevalence of crime (Collazos et al. 2021; Simpson 2017; Telep, Mitchell, and Weisburd 2014). There are several reasons to think that in communities with high crime rates and low levels of trust in law enforcement, sudden changes in the visibility of the police may often have a greater effect on voters than the propensity of officers to make stops and arrests.

First, evidence suggests that encounters with the police, particularly indirect ones, often send ambiguous signals. Subjective evaluations of officer behavior among those who are stopped by the police are only weakly correlated with the ratings of external observers (Worden and McLean 2017), and that the evaluations of witnesses themselves diverge greatly, with prior beliefs and contextual information playing a significant role in shaping evaluations of whether the actions of officers are acceptable and justified (Braga et al. 2014; Nagin and Telep 2020; Waddington et al. 2015). This ambiguity may lead the public to notice changes in the visibility of the police, but be comparatively insensitive to the tactics officers are using. A striking illustration of this comes from an aggressive order-maintenance experiment carried out in Lowell, Massachusetts, where officers dispersed groups of loiterers, cracked down on public drinking and drug selling, and conducted frequent stop-and-frisks (Braga and Bond 2008). Despite the aggressiveness of this intervention, post-intervention surveys showed that residents noticed that there were more police in the neighborhood, but reported no changes in the demeanor of officers or the strategies they were using (Braga and Bond 2009).

Second, residents of neighborhoods that are poor or predominantly Black often enter into interactions with the police with deeply ingrained, negative expectations rooted in early socialization and contact with criminal justice institutions (Brunson 2007; Carr, Napolitano, and Keating 2007). These prior expectations may limit the effects of new experiences with the police on attitudes (Anoll and Engelhardt 2023). Indeed, scholars such as Bell argue that the distrust toward the police among poor Black people is connected to a deeper, "marginal and ambivalent relationship with society, the law, and predominant social norms that emanates from institutional and legal failure" (Bell 2017, 2082). In this view, changes in the behavior of individual officers are unlikely to meaningfully change deeper attitudes that people attach to the police and government institutions.

In line with this, an accumulating experimental record shows that both crackdowns on crime as well as interventions meant to improve how officers interact with the public often fail to generate substantial or long-lasting effects on evaluations of police fairness or legitimacy (Weisburd et al. 2021; Weisburd et al. 2011; Kochel and Weisburd 2017). A review of the literature conducted by the National Academies echoes this finding, reporting little evidence that proactive police interventions either generate backlash or improve views of law enforcement (National Academies of Sciences, Engineering, and Medicine 2018). Together, this suggests that while the public may notice and react to sudden changes in the presence of the police, they may be less sensitive to short-term changes in the propensity of officers to make stops and arrests.

However, testing these claims poses a variety of challenges. Existing work attempting to measure the character of policing in a given neighborhood often relies on self-reports, or infers the tactics used by officers with administrative data on stops and arrests (e.g. Lerman and Weaver 2014; Walker 2020). Both of these approaches have well-documented limits. Self-reported encounters with the police rely on respondents' perceptions, which may or may not reflect the actual behavior of officers (Worden and McLean 2017). And while large-scale police administrative databases offer a more direct measure of police behavior, they are often limited to coercive encounters such as stops, citations, and arrests, omitting the many hours officers spend on patrol and responding to calls for service. These routine activities may have distinct political effects from enforcement, and without this information researchers risk conflating where officers spend their time with their propensity to make stops and arrests. Some papers rely on benchmark tests to measure the character of policing, but the validity of these measures rests on strong, unverifiable assumptions about police behavior (Neil and Winship 2019).

A further challenge faced by the literature on the community effects of policing is the difficulty in causally separating the effects of policing from the issues that officers are responding to, such as poverty and crime. Neighborhoods with high levels of police activity often suffer a variety of other social and economic ills that are likely to reduce participation (White 2022). Some scholars have addressed this challenge by leveraging quasi-random variation in the timing and location of police killings and gang crackdowns (e.g. Ang and

Tebes 2024; Naftel 2025), but these are extreme examples of police violence and overreach that are unlikely to generalize to most other forms of police contact.

The Jacksonville hotspots experiment overcomes these limitations by randomly varying both patrol intensity and enforcement style. This permits valid estimation of policing's causal effect on voter turnout and an unusually direct comparison of community-based policing to more traditional, enforcement-based tactics. Beyond its methodological appeal, this experiment allows me to measure the impact of a ubiquitous feature of modern American policing. Despite receiving little attention in political science, hotspots-style policing interventions are extremely common — surveys of U.S. law enforcement agencies find that 91% of large agencies report using some form hotspots-style tactics to control crime (National Academies of Sciences, Engineering, and Medicine 2018). This follows decades of academic research demonstrating that crime is highly spatially concentrated in small, microgeographic areas — such as specific streets, intersections, and addresses — and that focusing police resources in those places is an efficient and effective way to reduce crime (Braga et al. 2019). These hotspots are highly stable over time and are small enough to suggest that most streets, even in "high-crime" neighborhoods, are relatively free of crime (Weisburd, Groff, and Yang 2012).[1] Scholars also find that attitudes toward the police map closely onto these pockets of crime and disadvantage, often generating more variation in attitudes within neighborhoods than between them (Wheeler et al. 2020; Weisburd et al. 2024). Together, this implies that the micro-geography of crime is a particularly relevant way in which people experience both crime and law enforcement that may have consequences for political behavior.

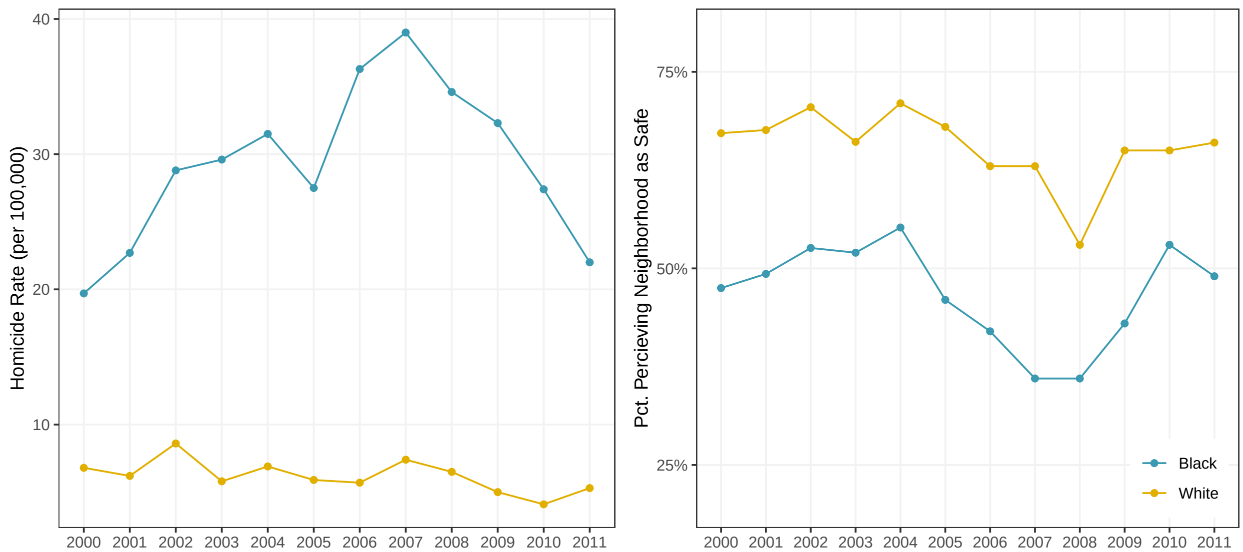
Background: The Jacksonville Hotspots Experiment

In the early 2000s Jacksonville, Florida experienced a marked reversal in a decade-long decline in violent crime. This increase in violence and homicide was covered extensively in local news outlets, who frequently referred to the city as "the murder capitol of Florida." Public opinion surveys show that crime was a top issue for voters during this period, and local elected officials frequently talked about public safety. Mirroring broader patterns of concentrated disadvantage and racial segregation in the city, this violence was highly concentrated in poor, predominantly Black neighborhoods, and exposed Black residents of Jacksonville to far higher levels of criminal victimization than Whites. As can be seen in Figure 1, the homicide rate for Black people was more than three times greater than for Whites, and Black residents of Jacksonville were far more likely to view their neighborhood as unsafe.[2] In 2007, 63% of Whites in Jacksonville reported feeling

1. This finding has held across a diverse array of places and across time, leading some scholars to refer to this tendency as "law of crime concentration at places" (Weisburd 2015).

2. Homicide statistics come from the Florida Department of Health's Bureau of Vital Statistics. Survey data comes from the Jacksonville Community Council (JCCI), which has conducted annual "Quality of Life" surveys of the city's residents since

Figure 1: **Homicide and Perceptions of Safety in Jacksonville, 2000 to 2011**



Note: Panel 1 shows the homicide rate in Duval County by race per 100,000 people. Panel two displays the percentage of Blacks and Whites in Duval County who report feeling safe when walking around their neighborhood alone at night.

safe to walk alone in their neighborhood at night, while only 38% of Blacks did.

In response to public concerns with crime, in 2006 the Duval County Sheriff (who acts as an elected police chief within Jacksonville's combined city-county government) announced an initiative called Operation Safe Streets (OSS) which sought to direct police resources into areas of the city with high crime rates. The department originally identified roughly 55 square miles with high crimes rates and targeted those areas with a variety of anti-crime measures, including increased patrols, crackdowns on high frequency offenders, gun buy-back programs, and tens of thousands of police door knocks to increase community engagement and crime reporting.[3]

As a way to measure the effectiveness of these various interventions, the Jacksonville Sheriff's Office (JSO) collaborated with a group of external researchers from the Police Executive Research Forum (PERF) to design and conduct a randomized controlled field trial that would compare the relative efficacy of traditional, enforcement-based police tactics with a more community-based approach to crime reduction. Using Unified Crime Reports (UCR) data from 2006 to early 2008, analysts within the JSO identified 83 hotspots of "street violence" (i.e. non-domestic violent crime incidents), which averaged 0.02 square miles in size and included

1985, donated by the survey firm *American Public Dialogue*. Each wave of the survey includes between 400 and 500 respondents drawn from a random sample of phone numbers in Duval County, which was stratified to achieve balance on race, gender, and geography. To measure feelings of safety, respondents were given the following prompt: "A feeling of safety is also part of the quality of life. Do you feel safe walking alone at night in your neighborhood?"

3. This original area targeted by OSS was far larger than the hotspots targeted by the experiment.

a mix of both residential and nonresidential areas (Taylor, Koper, and Woods 2011).[4] These hotspots were then randomized to receive one of two policing interventions or assigned to a control group for a period of 90 days. Hotspots in the control condition (n = 40) received standard, "business as usual" policing services. Both the existence and location of these sites were known only to a handful of analysts and commanders during the experiment, and subsequent analysis by the PERF researchers found no changes in the number of field stops or self-initiated policing activities in the control hotspots, confirming that routine operations in these areas continued undisturbed (Taylor, Koper, and Woods 2011).

Half of the treated hotspots (n = 21) were assigned to a "saturation" condition, where the presence of officers was increased dramatically, but where officers were not given specific direction on *how* to police those areas. Pairs of officers (who were either on-duty or working overtime) were assigned to patrol these areas at "high risk days and times" when crime incidents were most likely to occur, with each location receiving an average of 53 hours of police patrols per week (Taylor, Koper, and Woods 2011). Officers were required to maintain implementation logs, which were tracked by supervisors to ensure that they remained in their assigned hotspots. This was further verified by the external PERF researchers, who conducted ride-alongs and detailed interviews with both officers involved in the study and those in the surrounding patrol beats to ensure that they were following treatment protocols (Taylor, Koper, and Woods 2011). Consistent with an increase in police presence, this intervention lead to a large increase in enforcement activity, including an 85% increase in investigatory stops ($p < 0.01$) along with a $\approx 200\%$ increase in "self-initiated activities" which includes traffic stops and other investigatory police actions ($p < 0.001$) (166).

The remaining treated hotspots (n = 22) were assigned to a community-oriented, "problem-solving" intervention that increased the presence of officers, but also made substantial changes to the tactics they used. A total of 60 officers were selected to patrol these areas based on their reputations for being "change makers" and having positive relationships with the community.[5] These officers were paired with dedicated crime analysts, and given three days of intensive training in Problem-Oriented Policing (POP) — a proactive style of policing in which officers work to identify the underlying causes of criminal activity in a given context and use innovative solutions to address those problems (Cordner and Biebel 2005). Officers were given wide discretion in how they chose to address crime problems in their assigned hotspots, but were discouraged by project supervisors from relying on traditional enforcement whenever possible (Taylor, Koper, and Woods 2011).

This produced sharply different patterns of officer behavior from the saturation arm — self-initiated

4. The number of violent incidents over this period ranged from 9 to 163, a high rate considering that some hotspots were as small as a single intersection.

5. Given these criteria, this can be considered a "strong" intervention that combined both training and officer selection. In contrast, officers in the saturation condition were chosen based on availability and willingness to work overtime.

activities increased by only 50% percent ($p < 0.05$) over baseline, and increases in investigatory stops and arrests were small and statistically insignificant (Taylor, Koper, and Woods 2011). Instead of relying primarily on enforcement, officers engaged in a broad menu of crime-prevention projects, which were documented in regular briefings to the lieutenants in charge of the project. Officers most commonly worked with business owners, property managers, and city agencies to improve security (e.g. improving lighting or installing fences and road barriers); addressed nuisances and signs of physical disorder; conducted outreach to gather community input; and engaged in targeted code enforcement. The JSO devoted approximately 2,100 officer-hours per week to this effort, or about ninety-five hours per hot spot (Taylor, Koper, and Woods 2011). Although officers in this condition were not required to remain inside their assigned hotspots for that entire period, many of their crime mitigation strategies — including resident outreach, property-owner interviews, surveillance, code enforcement, and follow-up inspections — required they be on site.

In short, the experiment led to large increases in the presence of the police, as well as substantial differences in police behavior across treatment arms. While the saturation arm produced sharp increases in stops, searches, and arrests, officers in the community-oriented arm worked to reduce crime by working with residents to address the "root causes" of crime in those areas. While the effects of both interventions on crime were modest (consistent with either no effect or slight decreases across a variety of crime outcomes) (Braga et al. 2019; Taylor, Koper, and Woods 2011), the surge in officer presence would have been noticeable to residents of the hotspots given the small size of these areas and magnitude of the treatments. Studies of other experimental hotspot interventions have found that residents are able to recognize increases in police presence and efforts to address local crime and disorder problems in their neighborhood (e.g. Braga and Bond 2008; Bryant, Collins, and Villa 2014). In some cases, these interventions can shift the public's evaluations of neighborhood conditions, the police, and local officials, as well as their willingness to cooperate with officers (Bryant, Collins, and Villa 2014; Hinkle and Weisburd 2008; Blattman et al. 2021; Kochel and Weisburd 2017). My analysis contributes to this body of work by assessing the political consequences of these interventions.

Design

I measure the effects of the experiment by comparing turnout in the treated and control hotspots in three elections: the August 2010 statewide primary, the November 2010 general election, and the April 2011 Duval County election. This setting allows me to assess the effects of the intervention on both on- and off-cycle elections, including a local election in which the incumbent sheriff (who was ultimately responsible for the intervention) was up for reelection. I use the 2011 local election to assess the impacts of the experiment on vote choice.

The hotspots treated in this experiment experienced very high rates of violence. In environments where shootings and assaults are routine, even a relatively short-lived surge in visible patrols can lower the risk of victimization, dampen fear, and increase residents' sense of collective efficacy (Braga et al. 2019; Collazos et al. 2021; Rinehart Kochel and Weisburd 2019). This reassurance may contribute to greater participation by reducing social isolation and freeing up resources for people to become involved with politics. Improvements in service provision in under-served areas may increase the perceived responsiveness of government and the expected returns to political participation (Verba, Schlozman, and Brady 1995). I therefore expect that increasing the presence of the police in these places should increase voter turnout. I also expect these effects to be largest in hotspots with the highest pre-treatment levels of crime.

The experimental design also permits a comparison of two types of policing tactics — community-oriented patrols and a saturation strategy that generated many additional stops and arrests. The demobilizing effects of direct, involuntary encounters are well-documented (Ben-Menachem and Morris 2022; Lerman and Weaver 2020). Yet most residents interact with policing only indirectly — seeing officers, hearing sirens, or learning about encounters vicariously through neighborhood networks (Anoll, Epp, and Israel-Trummel 2022) — and the cues that the public draws from this exposure are not always clear. Additional enforcement may dampen turnout by generating fear and distrust (Kirk and Papachristos 2011), yet prior field experiments find little evidence that short-term crackdowns generate these effects (National Academies of Sciences, Engineering, and Medicine 2018; Weisburd et al. 2011). It is therefore an open question whether we should expect to see meaningful differences in the effects of these two interventions.

Data

For this study I obtained the 2012 Florida voter file, which includes information on each voter's race, sex, partisan affiliation, date of registration, and date of birth.[6] The voter file includes voter history for the 2010 statewide primary, the November 2010 general election, and the April 2011 local election. I restrict my analysis to turnout in these elections. I also obtained precinct-level election returns and GIS data from the Duval County Supervisor of Elections.

To identify voters who resided in the experimental intervention sites, I geocoded voter addresses using the ArcGIS USA Geocoder API, and then merged these records with shapefiles of the 83 experimental hotspots which were obtained from the Jacksonville Sheriff's Office via a series of public records requests. Of the 593,793 voters registered in Jacksonville in both 2008 and 2012, over 99.4% were successfully geocoded.[7]

6. To avoid issues of post-treatment registration bias or selective mobility in response to the experiment, I focus my analysis on voters who were registered to vote before the 2008 election, several months prior to the start of the experiment.

7. A small number of individuals had either missing or non-matching addresses. Florida state law allows a limited number of

Table 1: **Descriptive Statistics of Registered Voters Living Within Crime Hotspots**

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

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

I identify a total of 10,234 registered voters whose address lies within one of the 83 crime hot-spots identified by the JSO. As can be seen in Table 1, these voters are far more likely to be Black and to be registered as Democrats than the broader population of Jacksonville. Voters in these areas are also slightly younger, more likely to be women, and less likely to have turned out to vote in the previous election. While these hot-spot locations are not representative of the broader city population, they reflect many neighborhoods that are considered highly policed, given the close relationship between crime, segregation, racialized poverty, and the use of preemptive policing tactics (Walker 2020; Sampson, Raudenbush, and Earls 1997; Soss and Weaver 2017; Gelman, Fagan, and Kiss 2007).

The randomization of the treatment implies that characteristics of voters should be unrelated to exposure to either policing intervention. I test this assumption by conducting balance tests on a variety of individual-level pre-treatment characteristics, including age, race, sex, partisanship, and prior turnout. Appendix Table B.1 presents means and normalized differences across covariates for each treatment group.[8] The results suggest that randomization created well balanced groups of voters; for only one characteristic — the number of voters in a given hotspot — do I find evidence of imbalance. Reassuringly, this covariate appears to have no relationship with my outcome of interest. A simple regression of hotspot size on pre-treatment turnout in 2008 returns a precise null (Appendix Table B.3). Moreover, a joint F-test indicates that these pre-treatment covariates do not jointly predict treatment assignment ($p = 0.222$ for the community-oriented group, and 0.478 for the saturation group).

individuals to petition for their addresses to be removed from the public voter file, including recent victims of stalking, abuse, and domestic violence, as well as "high risk professionals," such as investigative personnel and other government employees whose work "could lead to criminal prosecution or administrative discipline" (Florida State Code s.119.071(4)(d)2.r).

8. Following Imbens and Rubin (2015), normalized differences are differences between groups divided by the sample standard deviation.

Finally, I obtained computer-aided dispatch (CAD) records from the Jacksonville Sheriff's Office through a public records request. Like most modern police departments, the JSO tracks every 911 and non-emergency call it receives through a CAD system. Each entry logs the call's time and location, the basis of the call, when an officer was dispatched, when the incident was resolved, and the final disposition (e.g. arrest, citation, investigation, verbal warning, or unfounded call). The system also records this information for officer-initiated calls every time an officer contacts dispatchers by radio or mobile computer. Because of this, the CAD system captures a variety of detective and administrative activities that are not captured in official crime reports, along with crime incidents that may have not been officially reported. Although officers may not contact dispatch when engaged in low stakes tasks where they do not feel that safety is a concern, CAD data remains the most detailed, minute-by-minute administrative record of where officers are and how they spend their time (Wu and Lum 2017).

As outlined in Appendix G, I obtained incident-level data on a total of 157,665 citizen-initiated calls and 139,072 officer-initiated calls originating from the experimental hotspots from January 2007 to May 2011.[9] In addition to accounting for potential post-treatment changes in crime and police behavior, these data allow me to replicate the manipulation checks done by the original PERF researchers (Taylor, Koper, and Woods 2011). In Appendix Figure G.1 I find that the saturation condition lead to an average of 148 additional officer-initiated calls per hotspot (a 200% increase, $p < 0.05$) during the 90 day intervention period, and 28 additional stops, citations, and arrests (a 150% increase, $p < 0.05$). Consistent with officers relying less on enforcement and engaging in problem-solving activities that are less likely to be recorded in the CAD data, the community-oriented condition lead to an average of 54 additional officer-initiated calls per hotspot (a 60% increase, $p < 0.05$), and only 5 additional stops, citations, and arrests, which is not statistically significant.

Estimation

To estimate the intent-to-treat effect of the two interventions, I compare turnout in the saturation and community-oriented policing hotspots to turnout in the control hotspots. To account for the clustered nature of the intervention and improve the precision of the estimates I rely on regression adjustment, using the estimator proposed by Lin (2013).[10] Specifically I estimate the following model:

9. The JSO would only release incident-level CAD data for specific addresses I supplied them with, and provided the data in the form of PDF-based image files that had been manually redacted to exclude calls that are not considered public record, such as those related to sexual assault. I received over 20,000 pages of data in this format. Due to the extremely costly and time-intensive nature of obtaining these records, my data is limited to incidents which occurred at addresses within the hotspots.

10. Adjusting for cluster size (i.e. the count of registered voters within each hotspot) is necessary because the difference-in-means estimator does not give an unbiased estimate of the average treatment effect under cluster-randomized assignment when clusters contain different numbers of treated units (Aronow and Middleton 2013). This potential source of bias can be mitigated by block randomizing on cluster size (Imai, King, and Nall 2009), which was not done in this case.

$$Vote_{ih} = \beta COP_h + \beta SAT_h + \beta COP_h \times (\mathbf{X}_i - \overline{\mathbf{X}}_i) + \beta SAT_h \times (\mathbf{X}_i - \overline{\mathbf{X}}_i) + \epsilon_{ih} \quad (1)$$

where i indicates an individual voter, h indicates the hotspot they reside in, COP and SAT are the treatments, and $\mathbf{X}$ is a vector of pre-treatment covariates which are centered and fully interacted with the treatment indicators.[11] In addition to cluster size, racial composition (i.e. percent Black), and pretreatment crime, I control for individual-level characteristics plausibly related to voting behavior, including race, age, gender, household size, and pre-treatment turnout in 2006 and 2008. In Appendix Figure C.1 I present estimates both with and without controls. The addition of covariates substantially reduces the variance of the estimates by accounting for other individual-level predictors of turnout, while returning nearly identical point estimates. This minimizes concerns that the results are reliant on functional form assumptions or other forms of model dependence.

Finally, because I am testing the effects of two treatments across three elections, I use a Bonferroni correction for these 6 tests, substituting the 95% confidence interval with the more conservative $1 - \frac{\alpha}{6}$, where $\alpha = 0.05$.

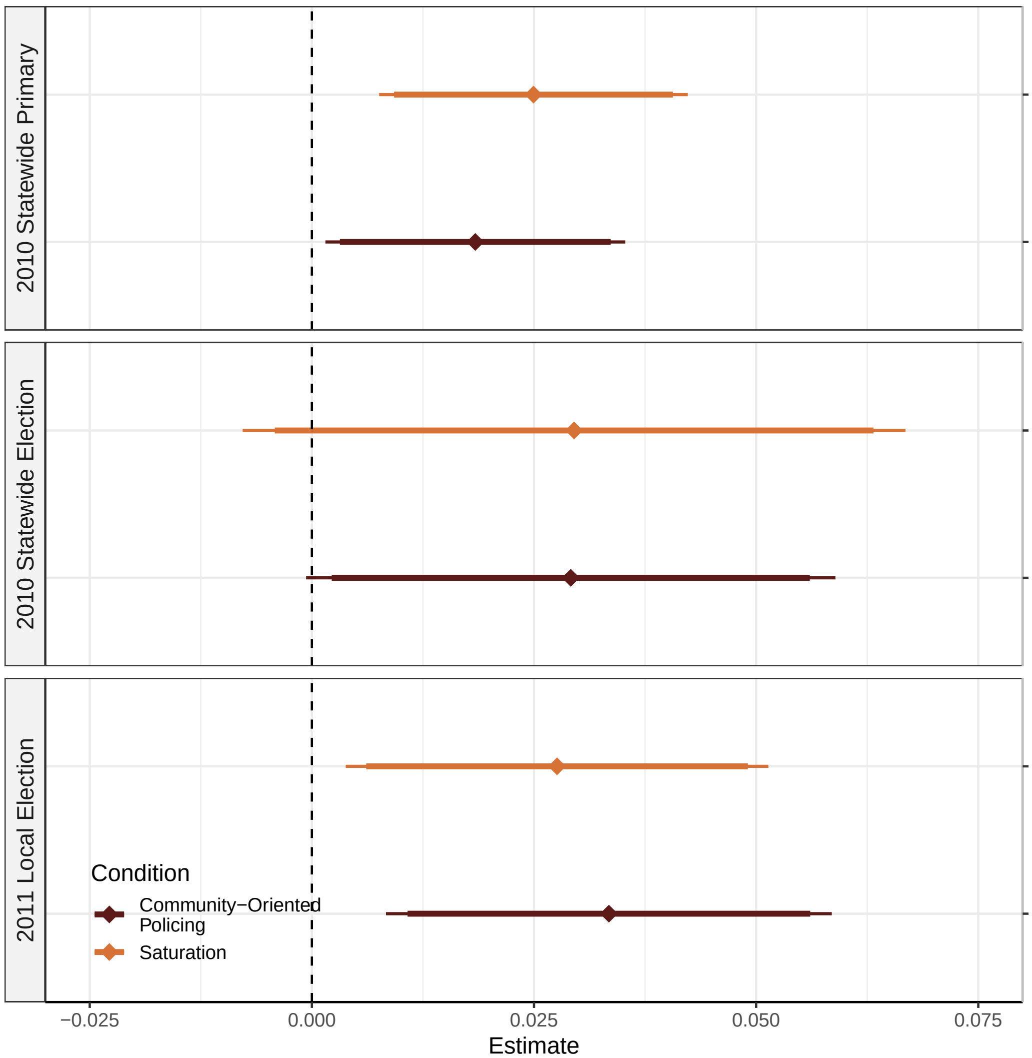
Results

In Figure 2, I present the estimated intent-to-treat effects for both the saturation and community-oriented interventions on individual-level turnout. The horizontal lines represent the adjusted 90% and 95% confidence intervals on the difference between treated and control groups. In line with my theoretical expectations, the point estimates are positive across all three elections. I find that the saturation condition increased turnout in the 2010 statewide primary by 2.5% and by 2.8% in the local election of early 2011 ($p < 0.05$). While the point estimate for turnout in the Nov. 2010 general election is similar in magnitude, it falls just short of standard significance levels, consistent with large increases in turnout (> 5%), as well as null effects.

I find that the community-oriented intervention lead to similar increases — 1.8% in the 2010 primary and 3.3% in the 2011 local election ($p < 0.05$). Given the low turnout in these elections — 22.5% in the 2010 primary and 29.7% in the 2011 election — these are large effects, and surpass the effect sizes of many get-out-the-vote efforts (Green and Gerber 2019). Importantly, the point estimates for both conditions are consistently positive, similar in magnitude, and statistically indistinguishable.

11. This approach is conceptually related to both post-stratification and blocking (Miratrix, Sekhon, and Yu 2013). While my outcome of interest is binary, I rely on OLS, which unlike generalized linear models, gives asymptotically correct confidence intervals under model misspecification (Freedman 2008; Lin 2013).

Figure 2: **Estimated Treatment Effect of Hot-spots Policing on Turnout**



Note: Intent-to-treat estimates of the effect of the Jacksonville hot-spots policing interventions on voter turnout in elections held in 2010 and 2011 ($n = 10,234$ individuals in 69 crime hot-spots). 95% confidence intervals derived from Huber–White heteroscedasticity- and cluster-robust standard errors, along with a Bonferroni correction for 6 hypothesis tests.

Robustness Checks

I assess the robustness of the effects in several ways. To account for potential spillover effects between the hotspots, I reestimate Equation 1, adding indicators for being within x feet of a treated hotspot. I also add inverse propensity of treatment weights to account for differences in the probability of being exposed to spillovers, which arise mechanically from the geographic clustering of the intervention sites. I recover the design-based probability of receiving the observed combination of direct and spillover treatments by re-running the original randomization process 5000 times.[12] To avoid extreme weights, I restrict the analysis to hotspots that had between a 10% and 90% probability of receiving the observed combination of treatment and spillovers. I test for spillovers within a 500ft and 1500ft radius, using randomization inference to recover p-values under the sharp null of no effects.[13]

The results are presented in Appendix D. While the trimmed sample leads to a loss of statistical power, I recover very similar estimates of the direct effects of the two treatments when accounting for spillovers. Importantly, I fail to find evidence of clear spillover effects onto nearby hotspots — with small, statistically imprecise estimates that are consistent with positive or negative effects on turnout. In other words, I find no evidence the positive effects on turnout that I find are an artifact of the treatments depressing turnout in nearby control sites, or other forms of spatial interference.

Given the 1 year gap between the conclusion of the experiment and the first primary election in which I measure turnout, another potential concern is that these effects are driven by post-experimental actions taken by the Jacksonville Sheriff's Department. Because there was some evidence that the community-oriented intervention reduced violent crime, the department attempted to institutionalize this style of policing, setting up a special unit of 20 officers dedicated to problem-solving work (Roush and Koper 2012). A total of 26 hotspots were exposed to this nonexperimental intervention, which could bias my estimates if selection into this intervention was systematically related to treatment assignment. Discussions with former JSO staff indicate that prior to the hotspots experiment, the area covered by Operation Safe Streets had been divided into twelve large "OSS Zones" (see Appendix Figure E.1 for an example). The problem-solving unit focused their efforts in hotspots that lay within four of these zones where crime was thought to be on the rise.

12. The original experiment relied on block randomization with strata derived from the count of violent street crimes during the pre-treatment observation period. While I do not have access to the crime data these strata were constructed from, I create pseudo-blocks using calls for service from January 2007 to May of 2008 related to violence. To the extent that these randomization blocks differ from those in the original experiment, this should make my estimates overly conservative by introducing additional noise into the estimates, but should not induce directional bias as treatment probabilities did not differ between blocks.

13. Randomization inference allows for valid hypothesis testing in this setting by generating an empirical distribution of all estimated treatment effects that could arise based on the experimental design and observed outcomes. I generate a distribution of treatment effects under the null hypotheses of no effect by re-estimating the weighted models using the 5000 placebo treatment assignments, and measuring the percentage of these placebo effects that are at least as large as the observed estimates. Importantly the validity of this test does not rely on correctly modeling the structure of spillovers (Bowers and Fredrickson, 2013).

Reassuringly, this indicates that selection into the post-experimental problem-solving effort was based on geography and neighborhood-level shifts in crime rather than treatment assignment or crime conditions in the hotspots themselves.

To verify that this nonrandomized problem-solving initiative is not contaminating my estimates, I estimate the controlled direct effect of the original randomized patrols using the sequential g-estimator (Acharya, Blackwell, and Sen 2016). This involves two steps. First, I estimate the baseline turnout model while adding a post-treatment covariate that flags exposure to the problem-solving unit. Second, I "de-mediate" the outcome by subtracting, for each observation, the turnout component attributed to that post-treatment indicator, effectively setting the mediator's influence to zero. I present the results in Appendix Table C.1, finding that re-estimating the treatment coefficients on this adjusted outcome yields virtually the same results. This suggests that the department's follow-up community-policing program does not appear to either explain or obscure the effects of the original experiment.[14]

Of course, police behavior could have shifted in other ways after the experiment. Because patrol officers learned which street segments had been classified as "high-crime," they might have focused extra enforcement on those locations once the study ended. Conversely, command staff could have orchestrated crackdowns in the control hot spots, depressing turnout there.[15] To probe these possibilities I examine the post-treatment CAD records, focusing on the period from the end of the experiment to the 2010 statewide primary. In the Appendix (Figure G.2), I fail to find clear evidence of systematic treatment-control differences in (1) the number of officer-initiated activities within each hotspot, (2) the share of CAD incidents that are initiated by officers, or (3) the share of officer-initiated activities producing arrests or citations. In short, observable patrol activity after the experiment looks the same in treated and control areas, making it unlikely that unmeasured behavioral changes account for the turnout effects I find.

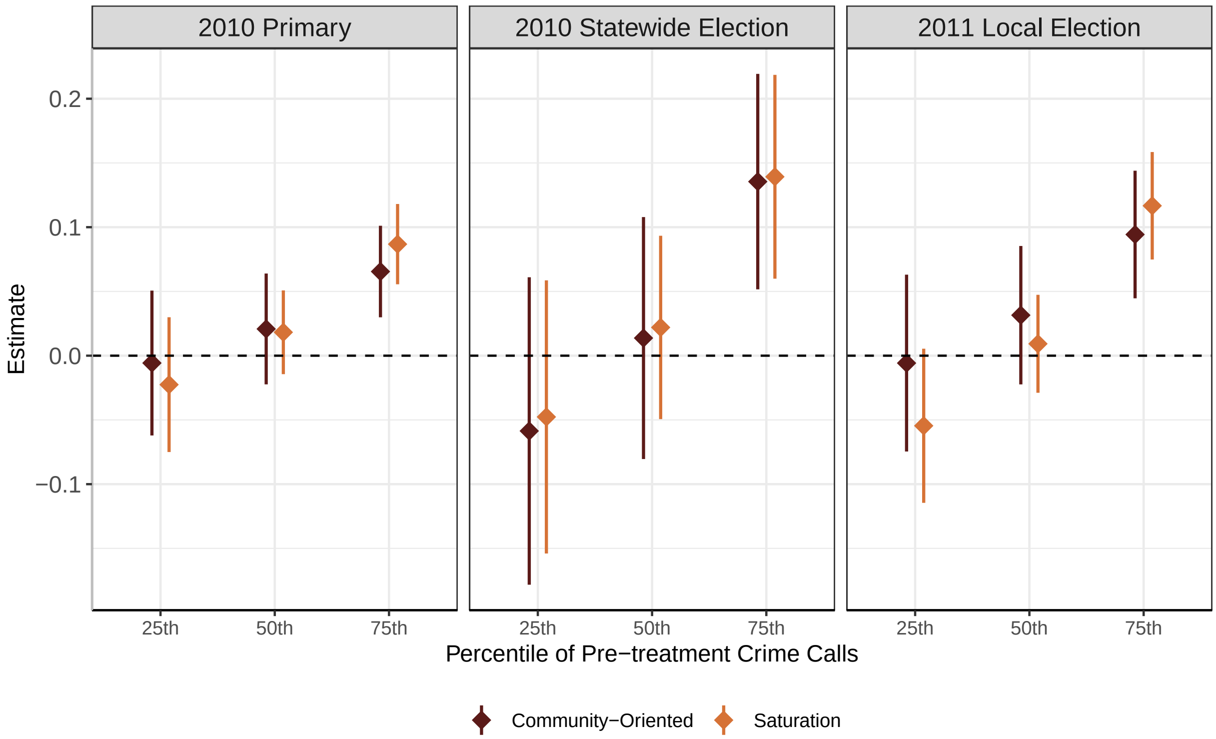
Heterogenous Effects

I next turn to heterogeneity in the effect of the interventions. If this increase in turnout was the result of residents feeling safer or better served by law enforcement, I would expect to see the largest turnout increases in hotspots with high levels of pre-treatment crime. Figure 3 presents estimated effects of the two treatments by different levels of pre-treatment crime-related CAD incidents during the same 90 day window exactly one year before the start of the experiment. In line with my expectations, I find clear evidence of an interaction between pre-treatment crime and turnout, with increases in turnout driven almost entirely by hotspots with

14. One reason that this later initiative does not appear to have had an impact on voting behavior may be that it was far less intensive than the randomized intervention.

15. There is no documented evidence that this occurred. Moreover, conversations with former JSO staff indicate that there was strong institutional attachment to the much larger OSS Zones used prior to the experiment, and that police work went back to focusing on these areas.

Figure 3: **Estimated Effects by Pre-treatment Crime**



the highest crime rates.

While I find similar aggregate effects of the two interventions, it is possible that the two interventions are mobilizing different sets of voters. Across racial groups, older voters consistently report higher satisfaction with law enforcement (Haberman et al. 2016; Weitzer and Tuch 2005) and are more supportive of increased police patrols and spending (Goldstein 2021; Metcalfe and Bolaji 2024). This thought to be due in part to the fact that older individuals are more fearful of criminal victimization — older voters are more likely to have directly experienced the crime wave of the 1960s through 1990s, and are more likely to local television news which places heavy emphasis on sensationalized crime coverage (Goldstein 2021). Older individuals are also less likely to be viewed with suspicion by officers (Dunham et al. 2005), experience signficantly less involuntary police contact (Davis, Whyde, and Langton 2018), and are significantly less likely to report being treated unfairly by the police (Bjornstrom 2015). This suggests that older people are both more predisposed to have preferences for high levels of police presence, and are less likely to experience the negative consequences of increased enforcement. Given this, we may expect the saturation condition to have been particularly mobilizing for this group.

Panel A of Figure 4 presents heterogeneous effects by age. While there is some suggestive evidence that the saturation condition primarily mobilized residents over fifty, the interaction term falls short of statistical significance and this pattern does not carry over to the other elections. I further explore potential sources of heterogeneity by race and partisanship. While the small number of Republican and non-Black voters in the sample yield imprecise estimates for these two groups, the results show clear evidence of mobilization among Black voters across both treatment arms. Importantly, I fail to find a consistent demographic divergence in the effects of the two conditions. In other words, there is no evidence that the two interventions were systematically mobilizing different sets of voters.

Vote Choice

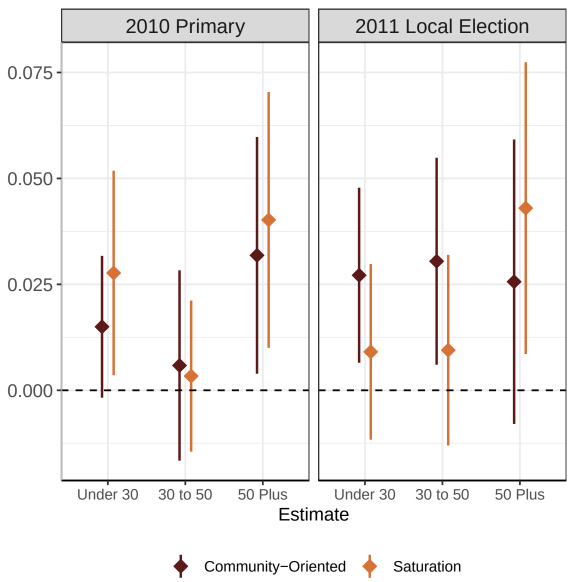
I next turn to the affects of the interventions on vote choice. A large literature shows that voters often engage in retrospective evaluations of basic government services, as well as social and economic conditions, and reward the officials they believe are responsible for improvements (e.g. Burnett and Kogan 2017). If residents viewed their neighborhood as safer following the intervention, this may have translated into electoral rewards for incumbents, especially in local elections given that policing policy is set at the local level and public safety is often an important issue in local races (Brown and Zoorob 2022). The 2011 election may have been particularly conducive to attribution given that Republicans held unified control over city government at the time, and the incumbent Republican sheriff, who serves as the head of the Jacksonville police, was on the ballot.

Because hotspots are far smaller than election precincts, I use the geocoded voter file to calculate the percentage of voters registered before the 2008 election within each precinct that live within an experimental hotspot. I also calculate the percentage of voters that live within these hotspots that were assigned to either the community oriented or surveillance condition and take this as my main treatment measure.[16] I additionally control for the percentage of voters who were registered as Democrats in each precinct before the start of the experiment, and merge the precinct boundaries with 2010 Census geographies to account for racial composition (i.e. percent Black), density of children, single-parent households, educational attainment and rates of home vacancy, long-term unemployment, poverty, and welfare receipt. The results, presented in Figure 5, show that coverage by either condition is not associated with voting for the incumbent sheriff, or for vote choice for mayor or for city council. These null results are robust to dropping both partisan and demographic controls, as well as calculating hotspots coverage in terms of geographic area (square miles), rather than geocoded voters.

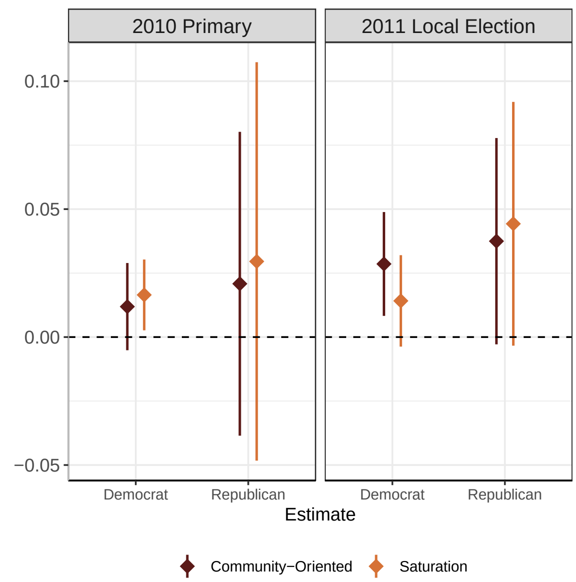
16. This mechanically limits the analysis to the 61 precincts (28.3% of total) that contain at least one registered voter living in a crime hotspot.

Figure 4: **Heterogeneous Effects**

(a) Age



(b) Partisanship



(c) Race

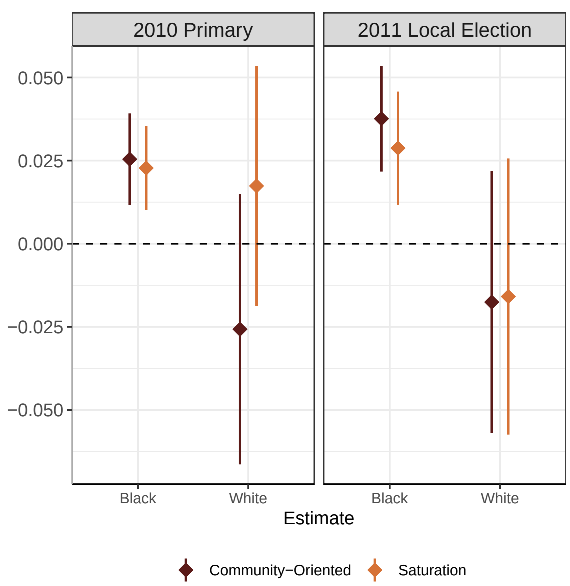
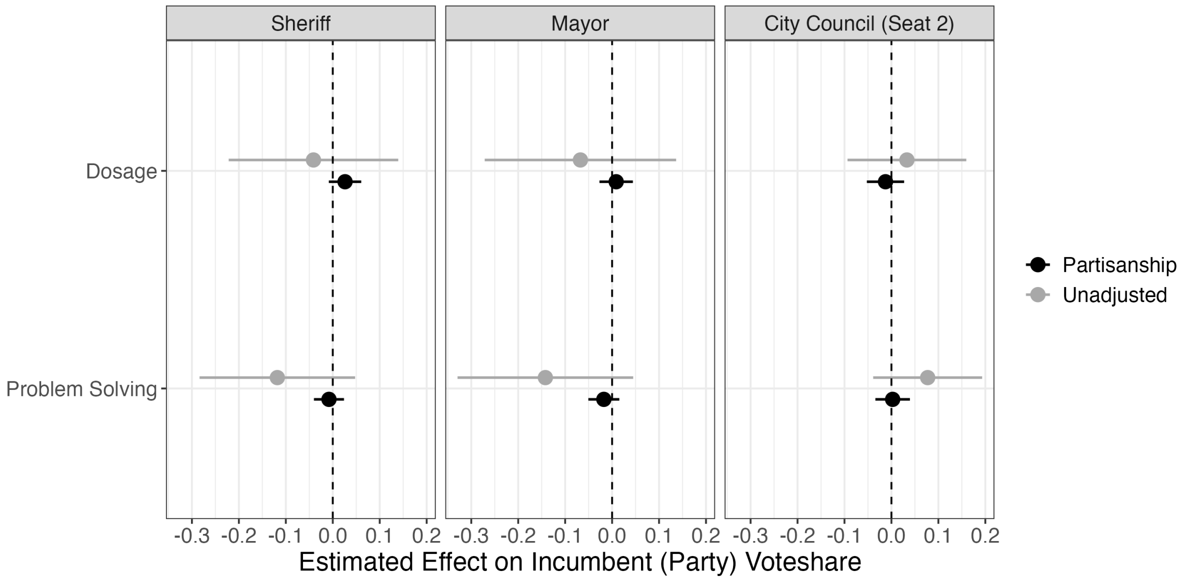


Figure 5: **Treatment Condition and Vote Choice in 2011 Local Election**



Alternate Mechanisms

Could the observed gains in participation reflect backlash rather than reassurance? Previous work finds that sudden surges in police activity can sometimes backfire by *increasing* fear of crime (Hinkle and Weisburd 2008), which in turn may mobilize those concerned with disorder (Brown and Zoorob 2022). To test this possibility I again turn to the CAD data, examining citizen-initiated calls for "minor," nuisance-related issues. If residents had become more fearful, I would expect these calls to be higher in the treated areas relative to calls about more serious crimes, which suffer from fewer issues of under-reporting. I rely on several measures of call inflation, including the proportion of citizen-initiated calls that are rated as low priority by dispatchers,[17] the ratio of calls about violent incidents relative to those related to suspicious-persons or vehicles, the proportion of citizen-initiated calls determined to be unverified or unfounded, and the proportion of citizen-initiated calls for service diverted to the department's Teleserv unit, which handles certain low priority complaints by phone, rather than dispatching patrol officers. I find null effects of the two treatments on all four outcomes (Appendix Figure G.3). Each of these measures is imperfect — for example, the rate of unverified calls is likely the product of the true crime rate, the propensity of citizens to call the police, and officer response times. However, the consistency of this null result across measures gives little reason to think the patrol surge heightened fear. More broadly, qualitative evidence suggests that residents of the crime hotspots were both aware of, and concerned with, the violence occurring in

17. While there may be racial and gender bias in the types of incidents that dispatchers consider to be serious, this would only pose a threat to inference if this bias varied systematically with treatment assignment. Because dispatchers were not directly involved with the experiment, this seems unlikely.

their communities. Media interviews of residents of the Grand Park neighborhood — the location of one of the control hotspots — document how residents were fearful of spending time outside in the neighborhood due to the fear of violence, and expressed that they would feel safer if the police spent more time in the neighborhood rather than reacting to crimes after they occurred (Schoettler 2010). This was a frequent complaint among many residents of neighborhoods with high crime rates, with one resident complaining that "[i]t takes a kid to get shot to bring the police here...[t]hey only show up when something happens" (Lewis 2006). In this environment, it is highly unlikely that increasing the presence of the police would lead to inflated perceptions of crime. Indeed, existing experimental evidence in criminology indicates these "backfire" effects are uncommon and largely limited to places with far lower baseline rates of crime (e.g. Weisburd et al. 2021).

Another potential explanation is that this mobilization is the result of backlash to excessive police surveillance, particularly in the saturation condition which generated large numbers of stops and arrests. Existing scholarship suggests that mobilization against police violence and overreach is usually driven by young, infrequent voters and accompanied by other signs of civic resistance such as protests (Ang and Tebes 2024; Naftel 2025). As demonstrated in Figure 4, I fail to find similar demographic turnout patterns here. To investigate potential protest activity I draw on data from Olzak (2021) on protests against police violence in major American cities from 1990 to 2018. The data are from manually coded news reports from *Newsbank* and include a variety of protest-level characteristics. Because this data is based on a random sample of years and focuses specifically on violence, I supplement the data with additional news reports between 2006 and 2011 from the following Jacksonville newspapers: *The Florida-Times Union*, *The Jacksonville Free Press*, *The Florida Star*, and the *Jacksonville Daily Record*.[18] Following Olzak (2021), I searched digitized archives for each paper using various string combinations (e.g. "demonstrat* AND police," "protest* AND police," "police brutality," "police-involved shooting") to identify potential protest activity related to police brutality, discrimination, or overreach.[19] While newspapers often fail to cover small demonstrations that are not disruptive or violent, relying on multiple local papers which vary in their editorial stance, topical focus, and audience demographics can reduce the likelihood of false negatives (Earl et al. 2004).

I find no documented evidence of police-related protest activity in the years leading up to the experiment, or in the post-treatment period from 2009 to 2011 — indeed, several articles remarked on the conspicuous *lack* of protests against police violence in Jacksonville during this period given the high rate of fatal police shootings in the city (Galnor 2008). While it is possible that some of the residents of the hotspots were mobilized by these concerns, accounts from the time suggest that many residents responded positively to

18. Both the *The Jacksonville Free Press* and *The Florida Star* are historically Black newspapers.

19. News reports were accessed via the following sources: ProQuest, Florida Times-Union Archives (Newsbank), and the Florida Newspaper Archive.

the increase in police patrols. Interviews in the summer of 2009 with residents of an apartment complex treated by the saturation intervention discuss how the sudden influx of police helped to discourage loitering and made residents feel safe enough to walk to the local park. One resident remarked, “[p]olice made their presence felt, and now the people are taking back their home” (Coleman 2009).[20] In an article on the Eastside neighborhood — the location of one of the community-oriented policing hotspots — a member of the local neighborhood association credited the decrease in crime the neighborhood was experiencing to the police, who had become more visible, closed a well-known drug house, and increased their community engagement (Patton 2009).

A final possibility is that the results reflect a “monitoring effect,” in which officers behaved more respectfully and professionally during the experiment due to the greater level of observation they were under. While greater oversight can alter how officers engage with the public (Mummolo 2018), the monitoring in the saturation condition was meant to ensure adherence to the assigned patrol schedules, not to alter the demeanor of officers or their stopping rates. An agency-wide survey in 2012 found that 80% of officers believed that their supervisors routinely used technology to track their daily activities (Koper et al. 2015), suggesting that the experiment should not have materially changed oversight. And while it’s possible that participating in the experiment led to a short-term decrease in the propensity of officers to engage in more serious acts of misconduct, these behaviors are likely too rare for these changes to have been noticeable within areas as small as the intervention sites. Internal affairs data from the JSO shows that the department investigated roughly 600 officer misconduct complaints per year in the three years preceding the intervention. Even if the true rate of serious misconduct was twice as high, the baseline probability of directly witnessing one of these incidents within a given hotspot during the 90 day window of the experiment would have been vanishingly small.

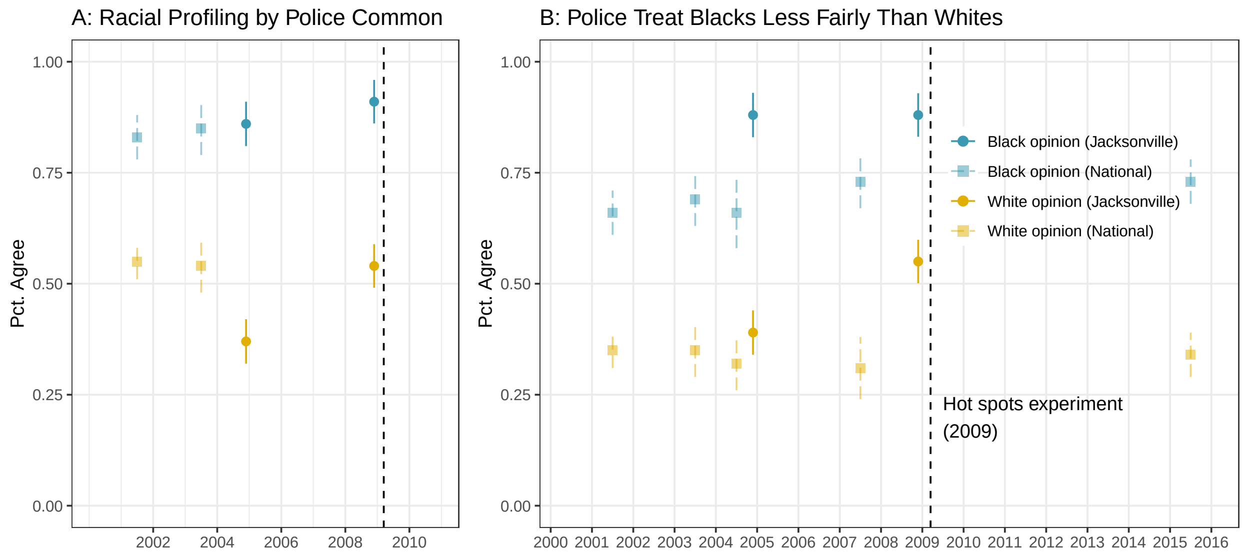
Is Jacksonville Unique?

A limitation of these findings is they analyze voting behavior in a single city.[21] Because Jacksonville is more ideologically conservative than most large U.S. cities (Tausanovitch and Warshaw 2014), a potential concern it that residents — especially Black residents — were unusually sympathetic to law enforcement. To assess this I draw on two surveys of Jacksonville residents conducted in 2004 and 2008 by the Jacksonville

20. There was no indication those living in the hotspots were aware that this sudden increase in policing activity was connected to an experiment. A search through local newspapers uncovered only two brief articles that mentioned the experiment specifically — both referred to the intervention as a community policing initiative and neither noted it was a part of a randomized controlled trial.

21. Although dozens of hot-spots experiments have been fielded in the past two decades, extending this analysis to other cities raises a variety of challenges. Many interventions cover only a handful of street segments for a brief period, making durable shifts in voting behavior unlikely. More critically, geographic information is almost never publicly released — in studies that include resident surveys, these records are typically expunged to protect respondent confidentiality and officer safety.

Figure 6: **Public Opinion on Police by Race**



Community Council. Both surveys include oversamples of Black residents (n = 400 and 424, respectively), as well as questions on policing that closely match those asked in the Gallup's Annual *Minority Relations Poll*, allowing me to directly compare attitudes by racial group within Jacksonville to the country more broadly. As can be seen in Figure 6, I find that in 2008, 88% percent of Black respondents in Jacksonville agreed that local police treated Blacks less fairly than Whites (versus 73% percent nationally the year previously), and 91% percent believed racial profiling by the police was widespread (versus 85% percent nationally). In short, at the time of the experiment Black residents of Jacksonville were at least, if not more, critical of the police as Black Americans elsewhere.

Conclusion

This study leverages a randomized hot-spots experiment to show that changes in where officers spend their time can affect the electorate in measurable ways. Large increases in the presence of the police lead to durable increases in political engagement in neighborhoods experiencing high levels of violence, particularly among Black voters. This effect does not appear to be mediated by the tactics used by the police, contrary to many dominant expectations in the literature.

Why, then, did the arrest-heavy saturation arm fail to depress turnout relative to the lower-enforcement, community-oriented arm? One possibility is that the police primarily stopped people who did not live in the hotspots, and are thus missing from the analysis. Another, normatively troubling, potential explanation is that officers primarily stopped individuals who were not registered to vote. If those who are most at risk of involuntary contact are already politically disengaged, any additional demobilization they experienced would be invisible in aggregate turnout among registered voters.

At the same time, the absence of a turnout penalty in the saturation arm is itself informative. It likely indicates that the broader community neither viewed the spike in stops and arrests as evidence of arbitrary or hostile policing nor responded especially favorably to the community-oriented alternative. Instead, residents appear to have reacted primarily to the shared, highly visible element of both treatments — the increased patrol presence — rather than to differences in enforcement.

These findings have implications for a number of literatures. First, this analysis advances policy feedback scholarship by showing that the visibility of the state can sometimes outweigh sizable differences in policy design. While prior research has often focused on the formal design of policies, recent work suggests the feedback relationships may be conditioned on preexisting attitudes (Anzia, Jares, and Malhotra 2022). This may be particularly true in the case of policing, where many members of the public hold strong prior expectations of state actors, and must often rely on imperfect and indirect policy cues. Second, they complicate policing-and-politics research: routine patrols can create positive civic spillovers that coexist with, and are distinct from, the demobilizing effects of jail spells and police stops. Third, my examination of vote choice contributes to scholarship on elections and local politics. The null effects I find suggest weak electoral attribution; low information and strong partisan heuristics in a racially polarized city may have obscured links between service improvements and office holders.

Taken together, my results show that targeted, short-term patrols in high-crime areas can provide important community benefits that can expand the electorate. At the same time, the null difference between enforcement-heavy and problem-solving tactics cautions that higher stop and arrest rates were unnecessary to produce this democratic dividend, and may therefore represent avoidable social costs. In short, the police can generate meaningful improvements in communities without leaning primarily on enforcement.

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A Appendix

List of Figures

List of Tables

Table B.1: **Descriptive Statistics: Registered Voters Living Within Crime Hotspots**

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

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

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

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

B Balance Checks

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

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

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

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

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

* 0 outside the confidence interval.

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

Lastly, Figure B.1 compares the distributions of cluster size at both the cluster- and individual-level (Imai, King, and Velasco Rivera 2020). The results suggest that there are not any extreme outliers with high leverage that could to induce model dependence.

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

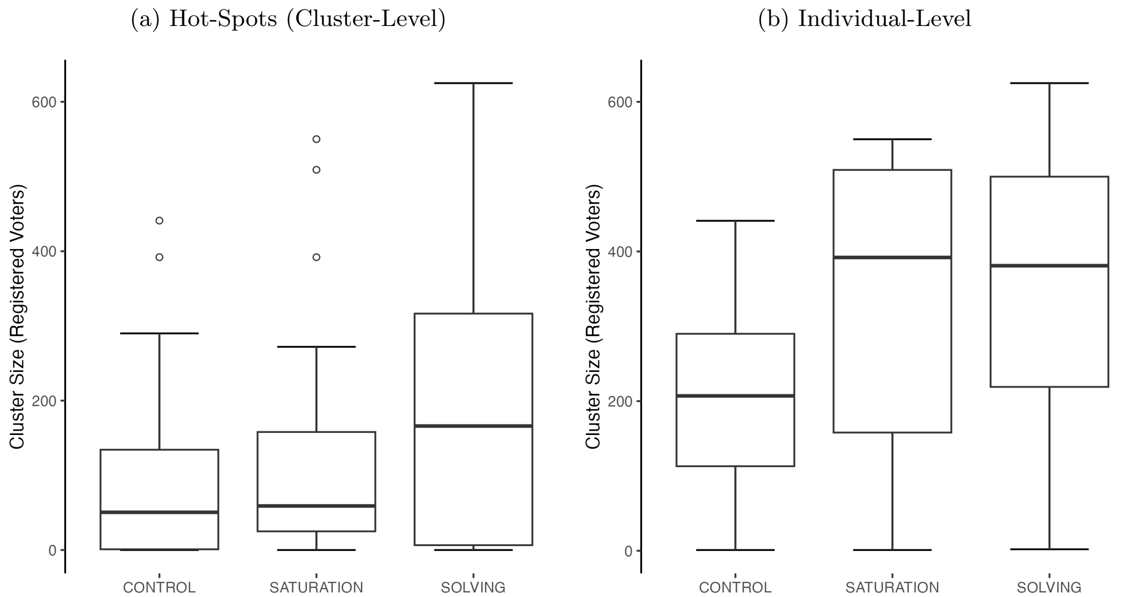


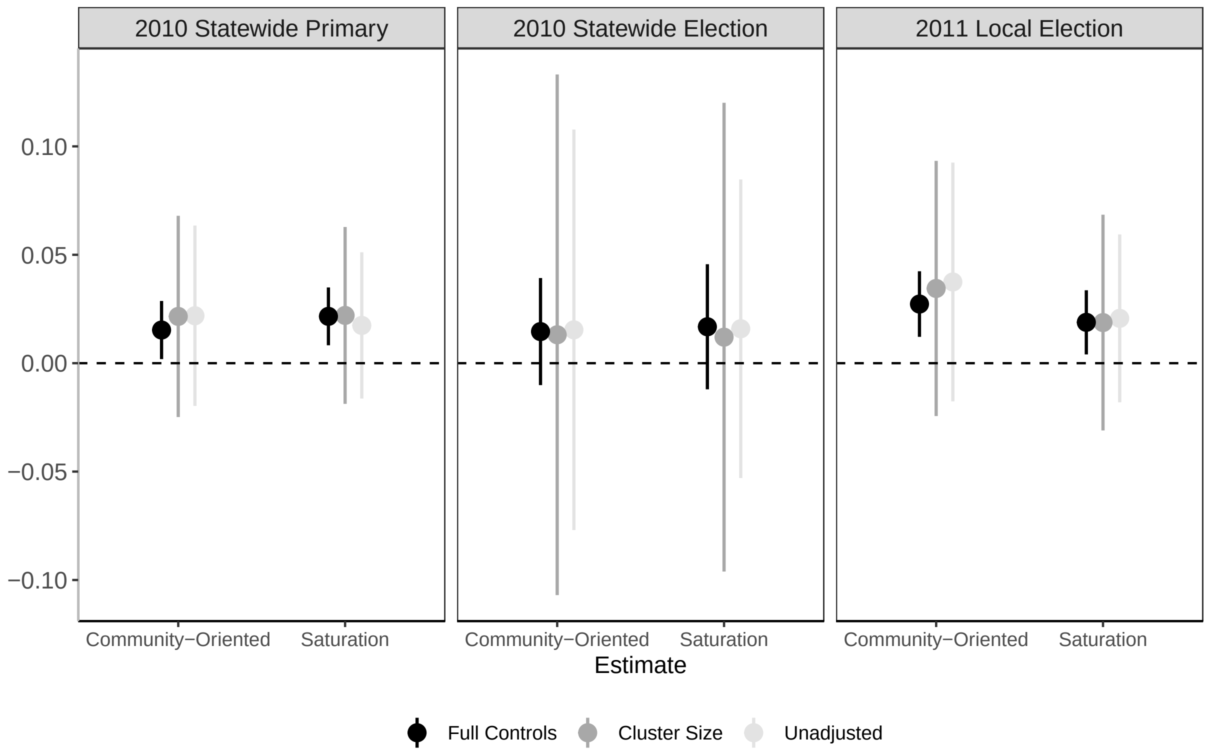
Table B.3: **Association Between Cluster Size and Pre-treatment Turnout (2008)**

	Estimate
(Intercept)	0.71
	[0.55; 0.88]
Cluster Size (logged)	−0.01
	[−0.05; 0.02]
Adj. R^2	0.00
N. Observations	9470
N. Clusters	69

Note: OLS estimates. 95% confidence intervals derived from Huber–White heteroscedasticity- and cluster-robust standard errors.

C Main Results

Figure C.1: **Intent-to-Treat Effects of Experiment**



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

Table C.1: **Comparison of CDE and ATE**

	2011 Election		2010 Primary	
	ATE	ACDE	ATE	ACDE
Community-Oriented	0.033	0.034	0.018	0.018
	[0.015, 0.052]	[0.000, 0.077]	[0.006, 0.031]	[-0.010, 0.051]
Saturation	0.028	0.026	0.025	0.026
	[0.010, 0.045]	[-0.010, 0.060]	[0.012, 0.038]	[-0.001, 0.055]

D Spatial Spillovers

Table D.1: **Spillovers at 500ft**

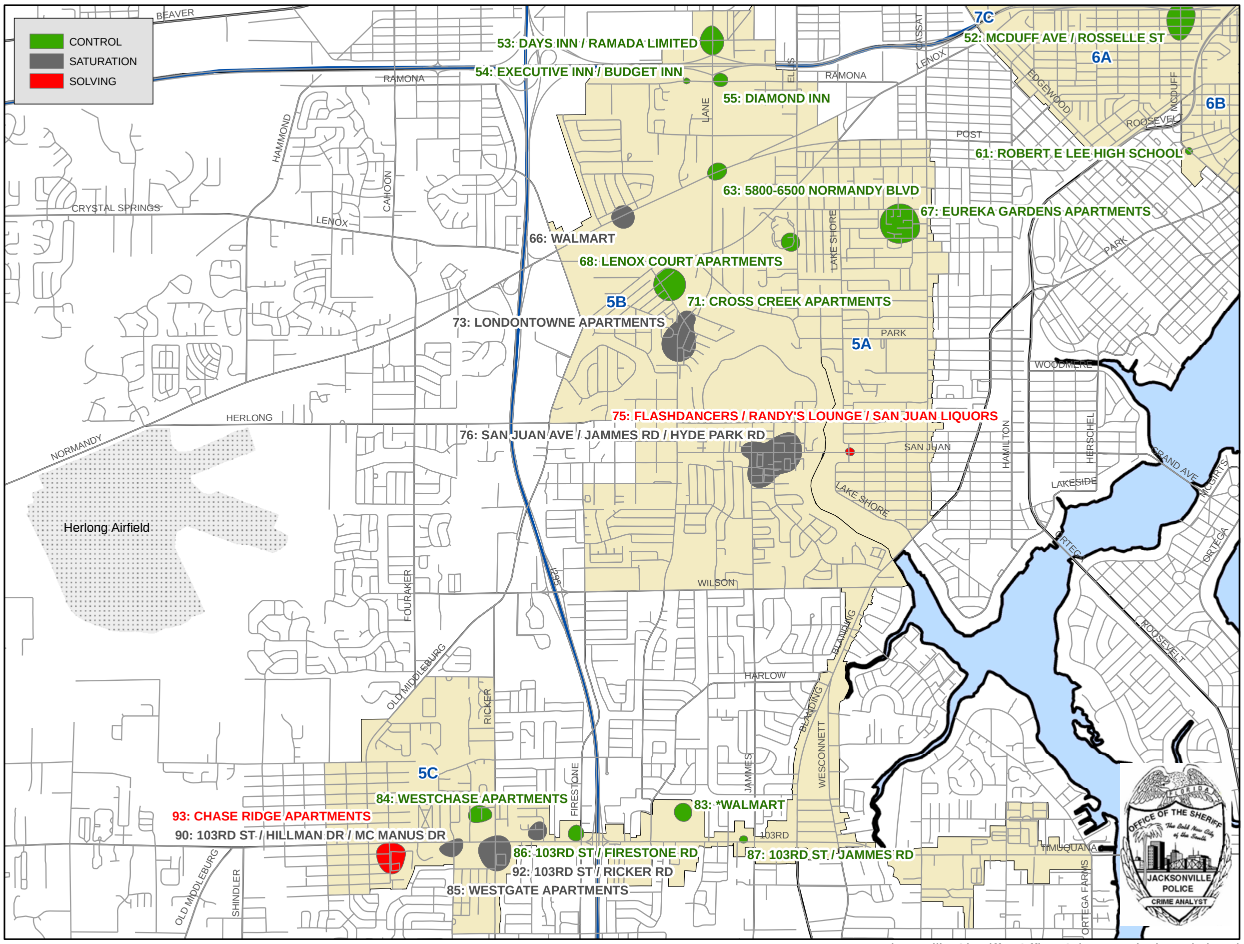
	2010 Primary Election		2011 Local Election		
	Estimate	RI p-value	Estimate	RI p-value	N. Hotspots
Direct Effects:					
Saturation	0.019	0.120	**0.034**	0.050	11
Community-Oriented	0.020	0.105	0.032	0.057	16
Indirect Effects:					
Saturation	0.002	0.342	0.020	0.095	3
Community-Oriented	0.008	0.238	-0.002	0.404	5
				Tot. Hotspots:	58

Table D.2: **Spillovers at 1500ft**

	2010 Primary Election		2011 Local Election		
	Estimate	RI p-value	Estimate	RI p-value	N. Hotspots
Direct Effects:					
Saturation	**0.040**	0.024	0.015	0.197	8
Community-Oriented	0.010	0.238	**0.034**	0.050	9
Indirect Effects:					
Saturation	-0.006	0.671	-0.007	0.549	7
Community-Oriented	-0.010	0.778	-0.028	0.958	10
				Tot. Hotspots:	47

E Map of OSS Zone and Hotspots

Figure E.1: **Map of OSS Zone 5 and Experimental Hotspots**



Date Created: December 1, 2008

Jacksonville Sheriff's Office Crime Analysis Unit (JLR)

F Public Opinion on Police in Jacksonville

Question wording:

- JCCI Poll: "Just your impression, are blacks in Jacksonville treated less fairly than whites in dealing with the police?"
- Gallup Poll: "Just your impression, are Black people in your community treated less fairly than White people in the following situations? How about – in dealing with the police, such as traffic incidents?"

G Police Computer-Aided Dispatch Data

G.1 Data Preparation and Cleaning

I obtained incident-level computer aided dispatch (CAD) data covering January 1st, 2007 to May 1st, 2011 from the JSO after an extensive, 9 month public records request process. Because the JSO would only release incident-level data for specific addresses, all incidents come from the list of addresses lying within the hotspots (plus a 100 foot buffer), which I generated using parcel-level shapefiles obtained from the Florida Department of Revenue.

The CAD data are stored on two systems. Data from the older system, covering calls prior to 2010, includes a unique incident identifier; the source of the call (911, non-emergency phone line, or an officer); the call location (street address); the basis for the call; whether the call was resolved by the department's Teleserv unit (which handles certain low priority complaints by phone, rather than dispatching patrol officers); the date and time the call was received, responded to, and closed; and the outcome of the call. The newer system, covering 2010 onward, contains less temporal information about each incident (only the date and time the call was received), but includes two additional fields: the ID number of the responding officer, and the priority given to the incident by dispatchers (on a scale of 1 to 6).

I received the data as $\approx 20,000$ pages of PDF image files, which I converted into machine-readable, tabular data using the Mistral Optical Character Recognition API, which is based on a large vision language model (`mistral-ocr-2505`).[22] While the model is closed-source, it was chosen for its ability to handle the complex layout of the data. This process resulted in a dataset of 296,741 calls for service. To verify that the correct number of calls had been extracted from the PDFs, I summed the true counts of calls (which are listed at the end of each PDF document) and cross-checked this total against the OCR output. I find an extremely low error rate — the number of calls is within $\approx 0.01\%$ of the true total.

To further assess the accuracy of the OCR model, I checked the observed values of variables in the data with a known format or range. The results for data from the newer system (which had a more complicated table layout) are summarized in the table below. The results speak to the high accuracy of the OCR model. Not a single date fell outside the known date ranges of the data. For only one variable, signal code, was there more than a handful of invalid values. Nearly all of these discrepancies were caused by characters being incorrectly transcribed as similar looking numbers or letters. For example, the signal "IJ" being written incorrectly as "U" or the letter "O" appearing as the number "0." Once these minor errors were corrected, there were only 64 calls with missing/invalid signal codes (less than 0.1% of the total observations).

As a final check, I took two random samples of 100 rows of the data — one from the pre-2010 data,

22. Mistral claims a 96.12% accuracy rate in table recognition/extraction.

Table G.1: **OCR Validation**

Variable	**Check**	**Percent Passed**
Call Source	Is equal to "MDT/OFFICER," "PHONE," or "911"	99.9%
Date	Is date (YYYY-MM-DD)	100%
Date	Is in range: 2010-01-01 to 2011-05-01	100%
Priority Rank	Is in range: 1 to 6	100%
Signal Code	Is in set of valid signal codes	97.2%

and one from newer system. Comparing these calls for service to the to the original images, I found no discrepancies in the key variables of interest: address, date, source, and signal code.

CAD incidents related to certain types of crimes, including sexual assault and child abuse, are not considered public record in the state of Florida and were redacted by the JSO. In the pre-2010 data, the JSO manually redacted the alphanumeric codes detailing the type of incident and the call's outcome, but not the call's date or location. The JSO's newer system can filter incidents by type, so redacted incidents are missing completely. To main consistency across the two time frames I drop the partially redacted incidents from the older data.[23]

Because there is no standardized coding system for CAD data, I used the signal codes and descriptions used by the JSO to group calls into twelve general categories based in part on the categorizations developed by Lum, Koper, and Wu (2022). The first three categories apply only to officer-initiated calls. A small number of these events were labeled as citizen-initiated, which was likely the result of incorrect labeling. I exclude these from the main analyses, but find similar results when they are retained. The remaining categories cover both officer- and citizen-initiated calls.

Officer-Initiated Calls

1. *Administrative:* these calls involve a variety of officer-specific actions including assisting other agencies, consulting with evidence technicians, assisting other officers, and serving warrants, as well as home visits by school resource officers.

2. *Investigation:* this is a catch-all category used by the JSO involving a variety of investigative activities and follow-ups with citizens.

3. *Special Assignment:* this includes proactive problem-solving, hot spots targeting, and door knocks. (Very few incidents were given this label)

Officer or Citizen-Initiated Calls

4. *Disorder:* this includes any call related to social or physical disorder. I follow the commonly used definition of disorder given by Skogan (2012), which encompasses "unsettling or potentially threatening and perhaps unlawful public behaviors," as well as "overt signs of negligence or unchecked decay [and] the visible consequences of malevolent misconduct" (175). This includes noise complaints, illegal parking or dumping, vandalism, fireworks, abandoned vehicles, animals, intoxicated individuals, disputes, drug use, juvenile complaints, fights, obscene or threatening phone calls, and prostitution.

23. In several instances the JSO mistakenly sent me both the redacted and unredacted versions of the same documents. Based on this, it appears that nearly all of the redacted incidents were related to sexual assault.

5. *Interpersonal:* this involves non-violent interpersonal incidents including stalking, missing persons, and kidnapping.[24]
6. *Medical:* this includes calls related to medical and mental health emergencies, including cardiac arrest, suicide, or injured persons. While these calls are typically handled by emergency medical services, the calls in the CAD system are those in which a police officer was also dispatched.
7. *Property:* this covers all property-related crimes, including theft, burglary, fraud, identify theft, counterfeits, shoplifting, arson, and damaged property.
8. *Service Requests:* this involves non-crime-related incidents such as requests for information or property checks, assisting a motorist, and reports of downed wires, gas leaks, fires, etc.
9. *Suspicion:* this includes reports of suspicious persons, "prowlers," "peeping toms," mentally ill individuals, and armed individuals not classified as engaging in any particular crime.
10. *Traffic:* this includes all traffic-related incidents including reports of reckless/drunk driving, accidents, and hit-and-runs. For officer-initiated incidents this also includes traffic stops.
11. *Violence:* this includes homicide, fights, assaults, robbery, and home invasions, along with other crimes or potential criminal activity that involve weapons, such as armed burglary, disputes, or kidnapping. While this category includes crimes such as sexual assault, these cases are not considered public record in Florida and were redacted by the JSO.
12. *Other:* this involves miscellaneous non-crime incidents including "strike," "report to hospital," "industrial accident," and "unverified 911 call."

To identify unfounded incidents, as well as those ending in a citation or arrest, I rely on the standardized disposition codes in the data. From the 2010 data onward, these were replaced by text-based descriptions, which I convert into standardized codes using string matching.

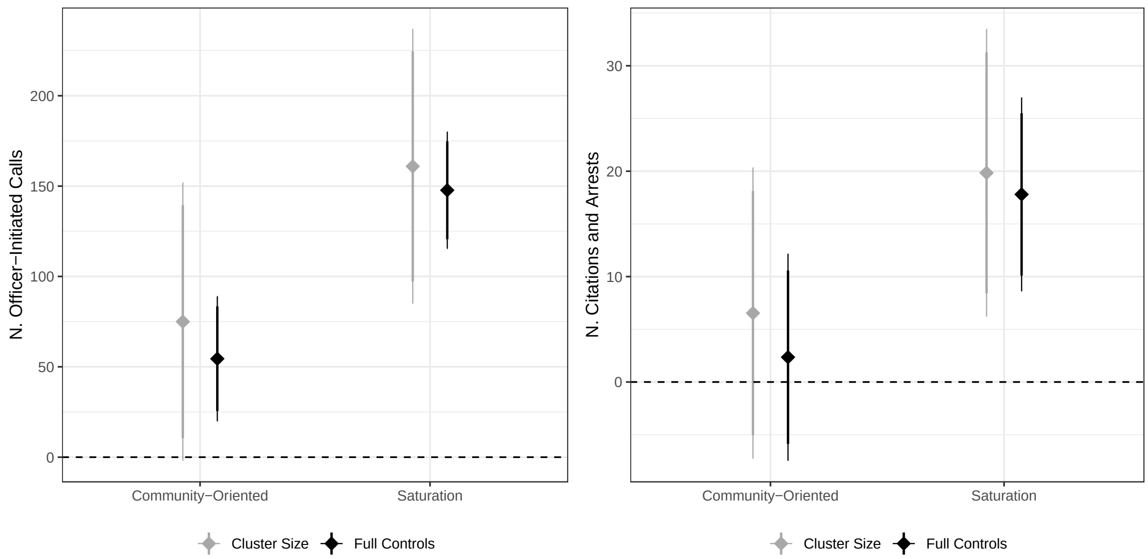
24. While kidnapping is officially classified as a violent crime in some states, the overwhelming majority of kidnapping cases are thought to involve custody disputes over children (Tillyer, Tillyer, and Kelsay 2015). I classify kidnapping as a violent incident only when it was coded as involving a weapon.

G.2 Manipulation Checks

Figure G.1: **Effect of Treatment on Officer Behavior During Experimental Period**

(a) Officer-Initiated Calls

(b) Citations and Arrests

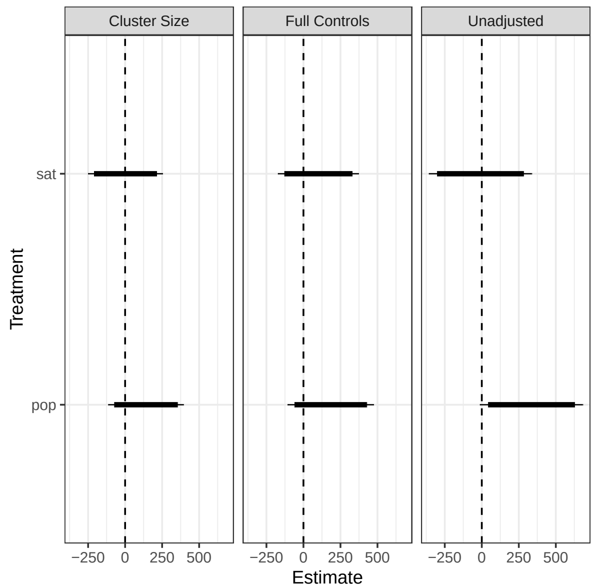


Note: OLS Estimates with 90 and 90% confidence intervals. All outcomes measured at the hotspot level. Covariate-adjusted estimates control for the count of violence-related calls for service from January 2007 through May, 2008; total population, percent Black, and percent under the age of 18 in 2008; as well as the poverty rate and share of female-headed households in 2000. Census block-level data is mapped to hotspots using areal interpolation.

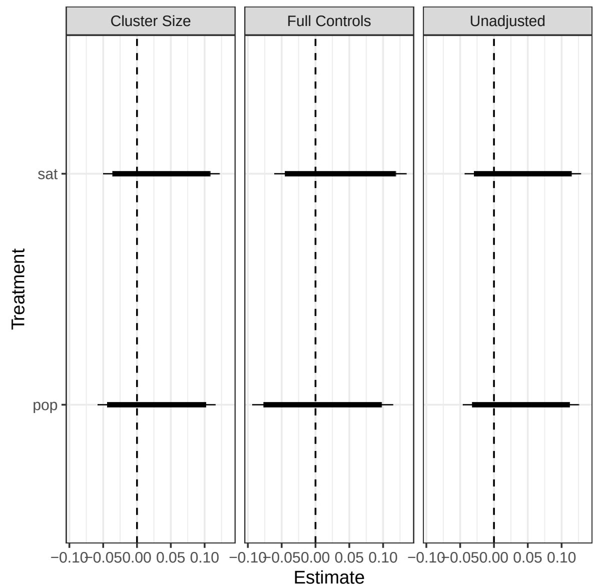
G.3 Post-Treatment Call Patterns

Figure G.2: **Post-Treatment Officer Behavior**

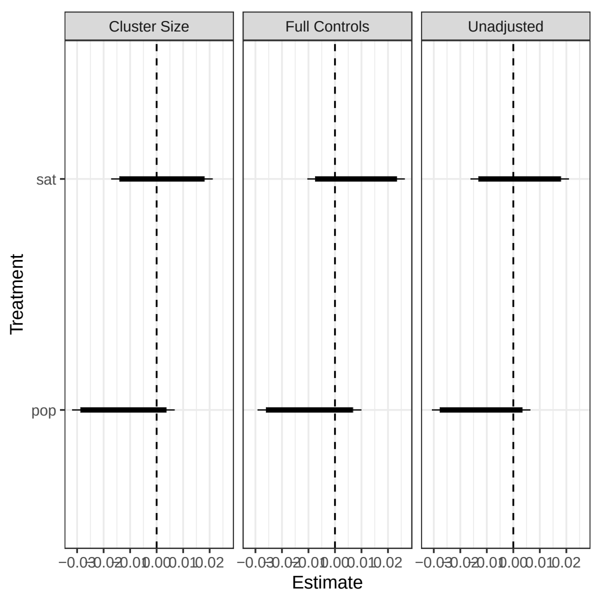
(a) Count of Officer-Initiated Calls



(b) Pct. Calls Initiated by Officers



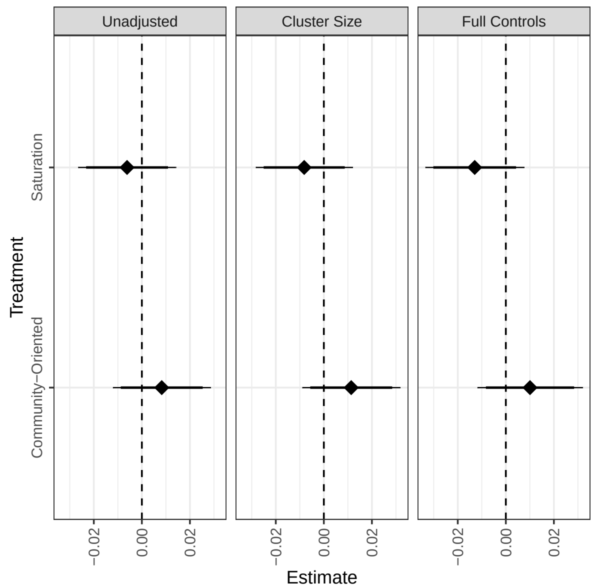
(c) Pct. Officer Initated Calls Ending in Citation or Arrest



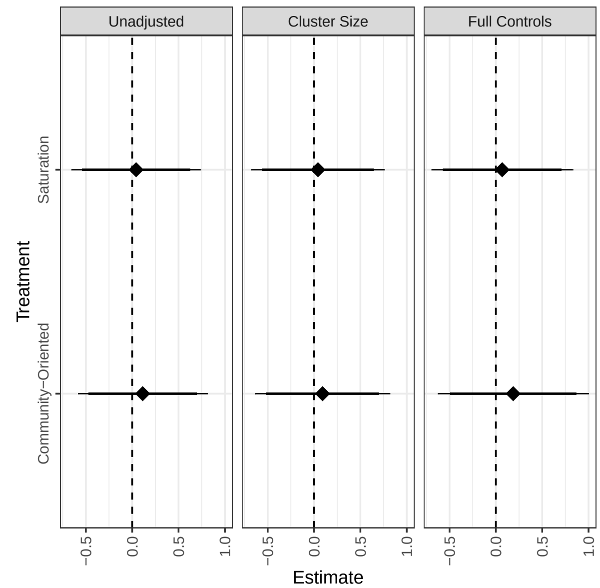
Note: OLS Estimates with 90 and 90% confidence intervals. All outcomes measured at the hotspot level. Covariate-adjusted estimates include controls for estimated total population, percent Black, and percent under the age of 18 in 2008, as well as the poverty rate and share of female-headed households in 2000. Census block-level data is mapped to hotspots using areal interpolation.

Figure G.3: **Post-Treatment Citizen-Initiated Calls**

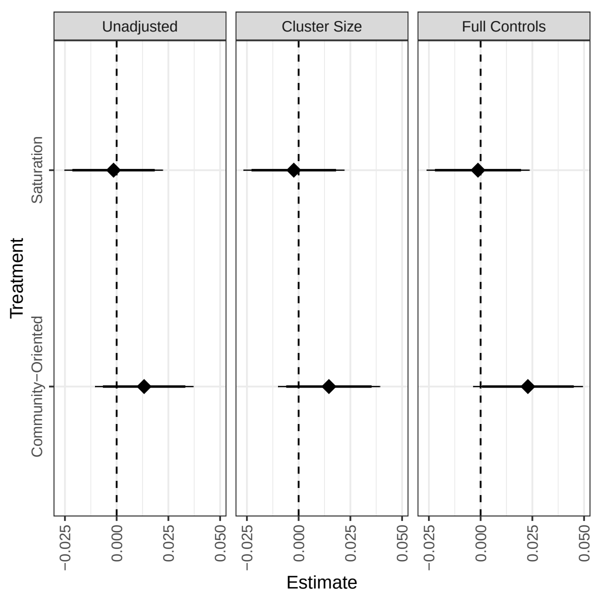
(a) Pct. Low Priority Calls



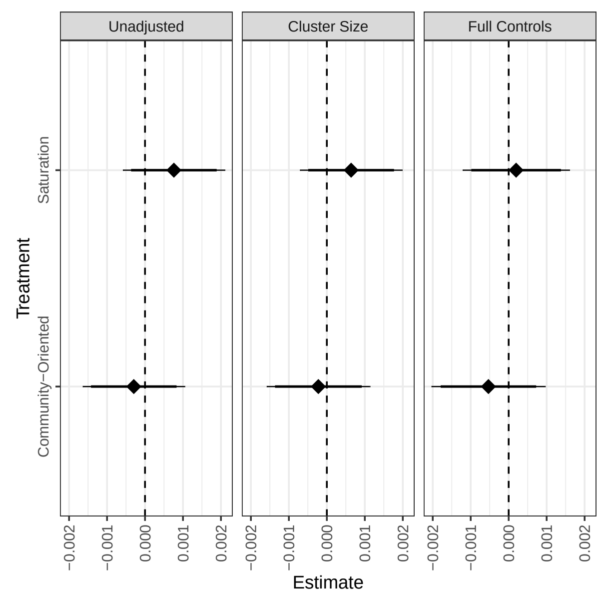
(b) Ratio of Calls Concerning Violent Crime to those about Suspicious Activity



(c) Pct. Calls Diverted to Teleserv Unit



(d) Pct. Nonverified Calls



Note: OLS Estimates with 90 and 90% confidence intervals. All outcomes measured at the hotspot level. Covariate-adjusted estimates include controls for estimated total population, percent Black, and percent under the age of 18 in 2008, as well as the poverty rate and share of female-headed households in 2000. Census block-level data is mapped to hotspots using areal interpolation.

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