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REStat Fellowship Committee
March 27, 2024

Dear Members of the Selection Committee,

I am writing to apply for the Review of Economics and Statistics Fellowship for the NBER Summer Institute. I am a Ph.D. candidate in Economics at Harvard University, where I expect to complete my degree in May 2024. My primary research interests are in the fields of labor economics and the economics of crime. I believe that my research interests and personal experiences make me a strong candidate for this fellowship.

My research focuses on understanding racial disparities in criminal justice, education, and health, and on discovering effective ways to address these disparities. In my job market paper, "What Does Federal Oversight Do to Policing and Public Safety? Evidence from Seattle," I investigate the impact of a 2011 federal investigation into the Seattle Police Department on policing and community behavior. While racial disparities in policing outcomes have been well documented in prior research, the effectiveness of proposals to address these disparities remains a matter of ongoing inquiry. Federal investigations conducted by the Department of Justice are crucial to the government's ability to ensure constitutional policing, and it is important to understand their impact on the communities whose police departments are investigated. My study shows that such investigations can lead to reductions in policing intensity and to more equitable policing between minority and nonminority neighborhoods without significantly increasing serious crime or affecting civilian reporting. These results suggest that adequate oversight of police departments can help to mitigate the potential harms of policing to communities without seriously compromising public safety. In addition to this study, I have also worked on projects examining language disparities in police officers' report writing based on a suspect's race, as well as exploring the potential benefits of greater diversity among clinical trial principal investigators and teachers.

Beyond my research, I also provide mentorship to other students from immigrant, first-generation college graduate, low-income, and racial minority backgrounds. My own experiences allow me to provide useful insights to these students as they apply to or navigate Ph.D. programs in economics and related fields at universities across the U.S. We form a community of scholars, able to mutually support one another and forge solidarity through our shared experiences and collective aspirations to make economics more welcoming and accessible.

I have enclosed the necessary documents with my application. Thank you for your consideration, and I look forward to hearing from you.

Sincerely,
Romaine A. Campbell

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Education

Harvard University
Ph.D. Economics, 2018 to 2024 (expected)
M.A. Economics, 2021
Wesleyan University
B.A. Mathematics/Economics, High Honors, 2013

Fields

Labor Economics
Economics of Crime

References

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Professor Amanda Pallais
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Fellowships & Awards

Chae Family Economics Research Fund, Harvard University, 2022
James M. and Cathleen D. Stone Scholar in Inequality, Harvard Kennedy School, 2020
National Science Foundation Graduate Research Fellowship, Harvard University, 2018
Phi Beta Kappa, Wesleyan University, 2013
Plukas Thesis Prize, Wesleyan University, 2013
Wilde Prize for Excellence in Economics, Wesleyan University, 2013
White Prize for Advanced Undergraduate Study in Economics, Wesleyan University, 2013
American Economic Association Predoctoral Fellowship, 2012
Lebergott-Lovell Award for Best Paper in Economics, Wesleyan University, 2012
Plukas Teaching Apprentice Award, Wesleyan University, 2012
Phi Theta Kappa, Capital Community College, 2009

Teaching

"An Economics Perspective on Racial Inequality," Harvard University, Course Designer & Instructor, 2021
"A Libertarian Perspective on Economic and Social Policy," Harvard University, head teaching fellow for Professor Jeffrey Miron, 2020

Employment

Betsy Lehman Center for Patient Safety, Data Analyst, 2018
Analysis Group, Inc., Senior Analyst, 2013 – 2017
Capital Community College, Mathematics Tutor, 2008 – 2013

Research

Research Assistant, Harvard University, Professor Isaiah Andrews, 2019
Research Assistant, MIT Sloan School of Management, Professor Susan Perkins, 2012

Job Market Paper

"What Does Federal Oversight Do to Policing and Public Safety? Evidence from Seattle"

Policing reform advocates have proposed increased oversight to improve quality and reduce officer misconduct. Opponents, however, fear that greater scrutiny of officers will increase crime and harm public safety. I examine a 2011 federal investigation into the Seattle Police Department,

focusing on policing responses and the impact on serious crime. In response to heightened scrutiny from the investigation, officers significantly reduced stops, particularly traffic and suspicious activity stops. Stop reductions were larger in minority neighborhoods and among officers with higher pre-period arrest rates. After the investigation, stops rebounded but remained below pre-period levels in minority neighborhoods. Comparing neighborhoods that experienced larger versus smaller stop reductions, I find no detectable differences in serious crime, though the estimates are imprecise. I also find no significant differences in serious crime rates when comparing Seattle to jurisdictions without a federal investigation. These estimates can rule out large, but not modest, crime increases. Increased oversight can reduce costly policing, particularly in minority neighborhoods, without significantly increasing serious crime.

Working Papers

"Officer Language and Suspect Race: A Text Analysis of Police Reports"
(with Connor Redpath).

We evaluate whether police officers use adjectives and adverbs that systematically differ by the suspect's race (race-predictive language) and whether race-predictive language use relates to officer characteristics. We leverage a novel data set containing police report text from a single large urban police department. We identify race-predictive language using a linear combination of LASSO and ridge regression regularization with word counts, and we then use predicted race to construct an officer-level measure of race-predictive language. We find evidence that officers use different adjectives and adverbs in reports for Black versus white suspects. Black officers use less race-predictive language. Race-predictive language use correlates positively with officer inexperience and the number of instances of use-of-force.

"Investigator Racial Diversity and Clinical Trial Participation"
(with Marcella Alsan, Lukas Leister, and Ayotomiwa Ojo). NBER Working Paper 31732.

We investigate whether increased racial diversity of clinical trial principal investigators could increase the enrollment of Black patients, which currently lags population and disease-burden shares. We conducted a survey experiment in which respondents were shown a photo of a current NIH investigator in which race (Black/White) was randomized. Sex was also randomized as a relevant benchmark. Black respondents reported 0.35 standard deviation units higher interest in participating in a clinical study led by a race concordant investigator (a 12.6% increase). Sex concordance had no effect. Further analyses indicates that perceived trustworthiness and attractiveness are the most important factors explaining these results.

"Spillover Effects of Black Teachers on White Teachers' Racial Competency: Mixed Methods Evidence from North Carolina"
(with Seth Gershenson, Constance A. Lindsay, Nicholas W. Papageorge, and Jessica H. Rendon). IZA discussion paper 16258.

The US teaching force remains disproportionately white while the student body grows more diverse. It is therefore important to understand how and under what conditions white teachers learn racial competency. This study applies a mixed-methods approach to investigate the hypothesis that Black peers improve white teachers' effectiveness when teaching Black students. The quantitative portion of this study relies on longitudinal data from North Carolina to show that having a Black same-grade peer significantly improves the achievement of and reduces the suspension rates of white teachers' Black students. These effects are persistent over time and largest for novice teachers. Qualitative evidence from open-ended interviews of North Carolina public school teachers reaffirms these findings. Broadly, our findings suggest that the positive impact of Black teachers' ability to successfully teach Black students is not limited to their direct interaction with Black students, but is augmented by spillover effects on early-career white teachers, likely through peer learning.

"The 'Gorilla in the Closet:' Regulatory Federalism in Environmental Enforcement"
(with Jenna Anders, her *Job Market Paper*)

How does federal regulatory capacity affect state enforcement outcomes? In this paper, we provide a model in which a stronger federal regulatory agency can either strengthen or weaken states' negotiating position with their regulated entities. The optimal federal enforcement for the states is one that maximizes state-level negotiated penalties. We apply this in the context of environmental regulation to test whether the EPA is too strong or too lenient in two environmental programs. First, using an EPA database of state-issued penalties, we show that when EPA's budget was cut in 2011, state-issued penalties for Clean Air Act violations shrank by 15%. Second, using a dataset with information about environmental remediation projects under state jurisdiction, we show that firms are more likely to begin cleanup projects during Democratic federal administrations. Our remediation analysis identifies the mechanism: while firm cleanup behavior is affected by EPA strength, cleanups conducted by the state itself are not, providing evidence that the effects operate through firm-state bargaining. We conclude that over one third of EPA's effect on environmental penalties is through its spillovers to state outcomes, and that states would benefit from a stronger EPA.

Papers in Progress

"Can Diversity Improve Equity? Achieving Racial Parity in Leader Assessment of Minority Team Members" (with Aaron Phipps).

"The Impact of Education on Successful Re-entry" (with Logan Lee).

Publications

Cost-analysis model of colonoscopy preparation using split-dose reduced-volume oral sulfate solution (OSS) and polyethylene glycol with electrolytes solution (PEG-ELS) (with Mark Cleveland, Matt Davis, Francis Farraye, Lynn Huynh, Mihran Yenikomshian, and Sander Yermakov), *Journal of Medical Economics*, 2016, 19(4): 356-363.

Seminars & Conferences

Wesleyan University, 2023
Western Economic Association Annual Conference, 2023
American Economic Association Summer Pipeline Conference, 2023
Midwest Economics Association Annual Conference, 2023
Western Economic Association Annual Conference (discussant), 2022
American Economic Association Summer Pipeline Conference, 2012

Academic Service

Member, Harvard Graduate Economics Peer Support Network, 2019 – Present
Nonresidential Undergraduate Advisor, Lowell House, Harvard University, 2019 – Present
Chair, Harvard Graduate Economics Professional Development Committee, 2019 – 2021

Software skills

STATA, SQL, SAS, R

Personal information

Citizenship: USA

What Does Federal Oversight Do to Policing and Public Safety? Evidence from Seattle*

Romaine A. Campbell†

December 2023

Please click here for the most recent version.

Abstract

Policing reform advocates have proposed increased oversight to improve quality and reduce officer misconduct. Opponents, however, fear that greater scrutiny of officers will increase crime and harm public safety. I examine a 2011 federal investigation into the Seattle Police Department, focusing on policing responses and the impact on serious crime. In response to heightened scrutiny from the investigation, officers significantly reduced stops, particularly traffic and suspicious activity stops. Stop reductions were larger in minority neighborhoods and among officers with higher pre-period arrest rates. After the investigation, stops rebounded but remained below pre-period levels in minority neighborhoods. Comparing neighborhoods that experienced larger versus smaller stop reductions, I find no detectable differences in serious crime, though the estimates are imprecise. I also find no significant differences in serious crime rates when comparing Seattle to jurisdictions without a federal investigation. These estimates can rule out large, but not modest, crime increases. My results suggest that increased oversight can reduce costly policing, particularly in minority neighborhoods, without significantly increasing serious crime.

*I am tremendously grateful for the continued guidance of Ed Glaeser, Larry Katz, Amanda Pallais, and Jesse Shapiro. I also thank Marcella Alsan, Bocar Ba, Felipe Gonçalves, Claudia Goldin, Sarah Jacobson, Aurélie Ouss, Emily Owens, Roman Rivera, and Marianne Wanamaker. For valuable discussions and feedback, I thank Jenna Anders, CJ Enloe, Shresth Garg, Ross Mattheis, Ayushi Narayan, Leah Shiferaw, and Audrey Tiew, as well as seminar participants at Harvard, the AEAMP pipeline conference, and WEAI. Omar Abdel Haaq provided excellent research assistance. I am grateful for the insightful conversations with the Seattle Police Department and Municipal Court staff, which helped me better understand the data and context. Finally, I gratefully acknowledge financial support from the National Science Foundation under Grant No. DGE1745303, the AEAMP, the Stone PhD Scholar Fellowship, and the Chae Family Economics Research Fund. All errors are my own.

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1 Introduction

In recent years, a series of highly-publicized incidents of police misconduct has renewed public discourse on the need for policing reform in the United States. Advocates of police reform suggest that increased oversight of law enforcement will improve policing quality, reduce officer misconduct, and build civilian trust. However, opponents worry that crime will rise and public safety will be compromised if officers are subjected to increased scrutiny (Ba and Rivera, Forthcoming; Prendergast, 2021). Since the passage of the 1994 Violent Crime Control and Law Enforcement Act, the Department of Justice (DOJ) has conducted over 70 federal investigations into local law enforcement agencies for unconstitutional policing behavior, usually following high-profile incidents of police misconduct. Federal investigations, commonly known as "Pattern or Practice" investigations, are currently the primary means for the federal government to provide external oversight of law enforcement agencies and combat unconstitutional policing practices.[1] A looming concern is whether these investigations may lead to increased crime as a result of reduced policing, as recent studies have suggested (Devi and Fryer Jr, 2020; Shi, 2009). In this paper, I examine how a 2011 federal investigation into the Seattle Police Department (SPD) impacted policing behavior and how changes in policing behavior affected crime and other measures of community well-being.

Estimating the causal relationship between policing activity and crime is difficult because changes in local policing activity often correlate with changes in local crime patterns. Additionally, researchers frequently lack granular data on policing activity. To address identification challenges, I exploit the timing of a 2011 federal investigation into the SPD for excessive use of force and racially-biased policing. The investigation increased scrutiny on police officers but did not affect the department's staffing or explicitly mandate changes in policing behavior. Furthermore, I employ granular administrative data from the SPD that allows me to track when and where different policing activities change. My study is the first to differentiate between policing behavior during the active investigation phase, when scrutiny is potentially at its highest, and behavior after the investigation's conclusion. This distinction allows me to better isolate the impact of increased scrutiny from federal oversight. I then leverage the shifts in policing behavior in response to the federal investigation to examine which specific policing activities may be reduced without increasing serious crime.[2]

The canonical model of crime, proposed in Becker (1968), suggests that crime supply is

[1] Recent policing reform legislative proposals also include key provisions that would expand the Department of Justice's ability to investigate misconduct within law enforcement agencies and establish a grant program enabling state attorneys general to conduct similar investigations.

[2] I define serious crimes as the eight Part 1 index crimes: murder, aggravated assault, forcible rape, robbery, car theft, burglary, larceny, and arson.

influenced by police presence and productivity, because both increase the likelihood of detection and apprehension. Subsequent empirical studies have demonstrated that increased police presence reduces crime (Chalfin and McCrary, 2017; Evans and Owens, 2007; MacDonald, Fagan and Geller, 2016; Mello, 2019; Weisburd, 2021). However, our understanding of which policing activities are crucial for crime reduction and which can be safely reduced without adverse effects remains limited (Cho, Gonçalves and Weisburst, 2023). Understanding the effectiveness of various policing activities is critical because, while the police play a vital role in reducing crime and promoting public safety, policing also has costs. Police–civilian interactions, such as stops and potential subsequent actions like arrests or the use of force, can reduce the physical, psychological, and general well-being of not only the civilians directly involved, but also that of the broader surrounding communities (Geller et al., 2014). Ang (2021) shows that students, particularly Black and Hispanic students, who live close to a police killing experienced persistent decreases in GPA, increased incidence of emotional disturbance, and lower rates of high school completion and college enrollment. Police violence can also erode public trust in law enforcement, undermine police legitimacy, and jeopardize civilian engagement with governmental and political institutions (Owens and Ba, 2021).

I first present a stylized model that illustrates how a federal investigation affects the allocation of police effort among different policing activities with different levels of productivity, and how these changes might affect serious crime rates. This model yields two insights that guide my empirical analyses. First, when officers are subject to federal oversight, which increases the cost of misconduct, they will decrease stops. Second, the impact of stop reductions on crime is uncertain. While fewer stops may lead to increased crime, greater focus on other policing activities may decrease crime. The net impact hinges on the magnitude of the reduction in stops and the relative productivity of stops compared to other policing activities in reducing crime.

I use detailed administrative data from the SPD to track the nature, location, and officers dispatched for all officer-involved dispatches in Seattle. I focus on two types of dispatches: officer-initiated stops, wherein individual officers assign themselves to respond to incidents observed while patrolling, and 911 calls, which result from public requests, typically made through calls, texts, or emails to the 911 system. The data also include details on all crimes reported to the SPD, including the associated police reports. I include the length of police reports in words as an additional measure of officer effort, making my study among the first to do so. My ability to link police department and municipal court records allows me to glean insights from multiple criminal justice agencies. I further incorporate monthly crime data from the Federal Bureau of Investigation (FBI) Uniformed Crime Reporting (UCR) program and traffic fatalities data from the National Highway Traffic Safety Administration

(NHTSA) to compare Seattle to control jurisdictions.

I use interrupted time series, difference-in-differences, and synthetic control models to assess changes in police and community behavior in response to the federal investigation. First, I show that, during the federal investigation, SPD officers significantly reduced officer-initiated stops, particularly traffic and suspicious activity stops. I also find a decrease in the number of arrests from stops and the stop arrest rate.[3] I uncover important neighborhood and officer heterogeneity in these reductions. Minority neighborhoods experienced larger decreases in officer-initiated stops but smaller decreases in stop arrest rates relative to non-minority neighborhoods.[4] Larger decreases in stops and smaller decreases in the stop arrest rate in minority neighborhoods together suggest stop prioritization by officers, as forgone stops were less likely than average to result in arrest. After the investigation's conclusion, stops rebounded but remained below pre-period levels in minority neighborhoods. In contrast, arrests from stops remained low across all neighborhoods after the investigation ended. The reduction in stops can mitigate potentially costly interactions between civilians and the police, as well as interactions between civilians and later stages of the criminal justice system. These findings suggest that federal police oversight is potentially beneficial to minority neighborhoods if reduced stops do not lead to increases in costly crimes.

Examining officer-level responses, I find that Black officers, female officers, and more experienced officers reduced stops less than their peers during the investigation. By contrast, officers with higher arrest rates (adjusted for dispatch characteristics) in the pre-period reduced stops more than their peers during the investigation. When I examine the length of crime reports, I find that after the complaint by the American Civil Liberties Union (ACLU) requesting the federal investigation into the SPD, officers produced significantly longer reports, especially in cases where arrests were made. Report lengths continued to increase after the investigation's conclusion. These increases in report length could indicate officer effort to improve the documentation of their activities as a precaution against complaints or other unwanted attention.

Next, I assess the impact of reductions in policing intensity on serious crime and public safety. I examine effects on the incidence and social cost of serious crime using two complementary approaches. In the first approach, I exploit neighborhood heterogeneity in officer-initiated stop reductions to assess changes in serious crime within Seattle. In the second approach, I use the synthetic control method to compare serious crime rates in Seattle and in control jurisdictions whose police departments were not subject to federal investiga-

[3] I define the stop arrest rate as the number of arrests per 1,000 officer-initiated stops.

[4] I define minority neighborhoods as census block groups with less than a 50 percent share of non-Hispanic White residents.

tions. Despite the sudden reduction in officer-initiated stops, there is no detectable impact on the incidence or social cost of serious crime in Seattle. These findings indicate that it is possible to reduce stops and arrests on the margin without significantly increasing serious crime. Moreover, my results also suggest that the impact of policing intensity reductions on serious crime rates, holding officer presence constant, will depend on the magnitude of the reductions as well as the specific activities being reduced.

In addition to examining the impact on serious crime, I investigate the impact of the investigation on other measures of public safety, including 911 calls and fatal crashes. While some crimes are reported via 911 calls, 911 call volume does not necessarily reflect crime rates. However, elevated 911 call volume might indicate a community in distress. Because 911 calls increased over the time period in question, I examine changes in call volume using calls for in-progress or recently occurred suicides or suicide attempts, which are unlikely to suffer from strategic changes in civilian reporting, for comparison. My estimates indicate that the volume for most call types increased at similar rates during and after the investigation period. Next, I present additional suggestive evidence indicating that the investigation did not influence civilian criminal behavior, crime reporting, or short-term trust or confidence in the police. Lastly, I examine fatal crashes and find no significant differences between the incidence of fatal crashes in Seattle and in control cities whose police departments were not subject to federal investigations.

This study contributes to several bodies of work in the economics of crime literature. First, I add to our understanding of the impact that external oversight, increased public scrutiny, or other changes to the policy environment in which officers work have on police behavior and crime. I demonstrate that federal oversight can reduce more intensive policing activities on the margin without significantly increasing serious crime, even in police departments without a history of strict low-level offense enforcement. Mas (2006) finds that following police labor contract disputes in which an arbitrator sided with the police department rather than the police union, there are significant reductions in arrest rates as well as a significant increase in crime. Devi and Fryer Jr (2020) find that federal investigations, when preceded by viral incidents, decrease arrest activity and result in increases in crime due to reduced officer effort. Ba and Rivera (Forthcoming) show that oversight, without public scandals or outrage, reduces misconduct without affecting either officer effort or crime in Chicago. In another related recent study, Tebes and Fagan (2022) find that following a 2012 federal lawsuit that severely restricted pedestrian stops as an investigative technique for NYPD officers, there were significant reductions in stop-and-frisk stops without increased serious crime.[5]

[5]While the NYPD is known for its aggressive adoption of "broken windows" policing, the SPD is not

I also contribute to the literature on the potential consequences of reduced low-level offense enforcement. Shi (2009) shows that increased scrutiny reduced low-level arrests, particularly in heavily African American neighborhoods, and increased felony crime rates in Cincinnati.[6] In contrast, Cho, Gonçalves and Weisburst (2023) show that reduced arrest activity for low-level offenses after peer line-of-duty deaths does not significantly affect crime or civilian reporting.[7] My study demonstrates that stops and arrests can be substantially reduced, particularly in minority neighborhoods, without significantly affecting serious crimes or civilian reporting. Finally, my study adds to the broader literature on policing and crime by combining multi-agency criminal justice data with other publicly available data sources.

The remainder of this study is structured as follows. Section 2 provides the institutional background, while Section 3 presents a framework of policing and crime. In Section 4, I describe my data and provide summary statistics. Section 5 focuses on estimating the effects of the investigation on officer activity. In Section 6, I estimate the impacts on public safety, including serious crime rates, 911 calls, and fatal crashes. Lastly, Section 7 concludes.

2 Institutional Context

In this section, I provide background information on federal investigations. I include an overview of the Department of Justice's authority, the standard investigation process, and details on the federal investigation into the SPD.

Federal Investigations. In 1994, the US Congress passed the Violent Crime Control and Law Enforcement Act, granting the federal government unprecedented authority to investigate and litigate cases against state and local law enforcement agencies that exhibit a pattern or practice of unconstitutional or unlawful policing behavior (Center for American Progress, 2021; Donnelly and Salvatore, 2019). Since then, the Department of Justice (DOJ) has initiated over 70 "Pattern or Practice" investigations into law enforcement agencies suspected of systemic misconduct. While these investigations often follow high-profile incidents such as the 1991 beating of Rodney King by Los Angeles Police Department officers (United

known to subscribe to this policing philosophy (Lum and Vovak, 2018). The "broken windows" policing approach advocates for aggressive enforcement of low-level offenses to deter more serious crimes. New York City widely adopted "broken windows" policing in the 1990s (Corman and Mocan, 2002), and this approach is often credited for the substantial decline in crime rates in New York City since then (Zimring, 2011). While Becker's theory provides some theoretical support for this style of policing, the empirical evidence has been mixed (Chalfin and McCrary, 2017; Harcourt, 2005; Weisburd et al., 2015).

[6] Heaton (2010) also shows that official and public scrutiny of police profiling behavior reduced arrests of minorities, but increased crime in minority areas.

[7] In a similar vein, Agan, Doleac and Harvey (2023) find that forgoing prosecution of nonviolent misdemeanor offenses significantly reduces future criminal activity over the next two years.

States Department of Justice Civil Rights Division, 2017), pattern or practice cases focus on systemic police misconduct rather than isolated instances of wrongdoing. The investigations are critical to the federal government's efforts to hold local police departments accountable and combat unconstitutional policing practices, such as police brutality and racial profiling. The DOJ can initiate investigations based on information gathered from various sources, including news reports, public complaints, or complaints from nonprofit organizations like the ACLU. The Special Litigation Section of the Civil Rights Division within the DOJ conducts these investigations and can file civil lawsuits against law enforcement agencies for unlawful conduct.

A Typical Investigation. The Civil Rights Division staff first assesses whether to investigate a law enforcement agency. While many jurisdictions often meet the basic criteria to be investigated, the division prioritizes among viable investigations to best direct its resources (United States Department of Justice Civil Rights Division, 2017). A key factor is the potential for an investigation into a particular agency to help set reform standards for other jurisdictions facing similar issues. If an investigation is initiated, Civil Rights Division officials promptly meet with local law enforcement, police unions, political leaders, and community groups to discuss the investigation's basis, scope, and next steps (United States Department of Justice Civil Rights Division, 2017).

The duration of the investigation varies significantly based on factors like agency size and the scope and complexity of the investigation. During an investigation, the DOJ, often with the assistance of external experts, conducts a comprehensive review of an agency's policies, practices, training, data handling, accountability systems, and community engagement. To gather additional insights, officials also observe training sessions, ride along with officers, and interview command staff, officers, and community members (United States Department of Justice, 2015). The primary objective of the investigation is to uncover systemic problems contributing to unconstitutional behavior and create a plan to address these issues. However, the process is not always transparent to the agencies under investigation, occasionally causing disagreement over the investigation's findings and the need for reform (Bureau of Justice Assistance, 2015).

If the investigation yields insufficient evidence, the division closes the case. However, if there is sufficient evidence of a pattern or practice of unconstitutional or unlawful behavior, a "Findings Letter" is issued. After the findings announcement, Civil Rights Division officials meet with police leadership, unions, and community stakeholders to explain the findings and plan next steps. If the agency is willing to cooperate, the DOJ begins negotiations over a reform agreement. While the division considers input from all stakeholders on how to address

the issues identified in the findings letter, direct bilateral negotiations remain confidential. If an agreement is reached, reforms are overseen by a federally-appointed independent monitor through a court-enforceable consent decree. In challenging cases, the Civil Rights Division may exercise its authority under the 1994 act to file civil lawsuits for court-ordered reforms.

The SPD's Investigation. Figure 1 shows the timeline for the federal investigation into the SPD. On December 3, 2010, the ACLU of Washington and other community organizations filed a complaint against the SPD with the U.S. Attorney's Office and the DOJ requesting an investigation into the SPD (ACLU of Washington, n.d.). The complaint alleged several examples of excessive force, particularly against people of color, by SPD officers. A noteworthy example was the killing of John T. Williams, a 50-year-old, Native American woodcarver, by an SPD officer on August 30, 2010 (ACLU of Washington, 2010; NPR, 2016; Seattle Times Staff, 2018). The complaint alleged that Williams was confronted by the officer while he was crossing the street in a crosswalk, holding a piece of wood and a woodcarving knife. The officer stopped his car, got out, and yelled at Williams to drop his knife. Approximately 5 seconds after stopping his car, the officer had shot and killed Williams (NPR, 2016).

In February 2011, DOJ representatives met with Seattle Mayor Michael McGinn, other community and city leaders, and SPD personnel and union members to discuss structural challenges facing the SPD (United States Department of Justice, 2011). Finally, on March 31, 2011, during a joint press conference with U.S. Attorney for the Western District of Washington Jenny Durkan, Assistant Attorney General for the Civil Rights Division of the DOJ Thomas Perez announced a federal investigation into the SPD for excessive use of force and racial bias in policing. U.S. Attorney Durkan noted that the investigation would not focus on charging officers for their roles in past episodes, but would lead to formal changes in departmental policies if federal laws had been violated (Yardley, 2011).

In response to the federal investigation into the SPD, Seattle Chief of Police John Diaz welcomed the inquiry and encouraged the DOJ to make its investigation "as wide as possible." During an interview with the *Seattle Times*, he said, "I'm just looking at this as a way of getting a free audit from the Department of Justice" (Yardley, 2011). On December 16, 2011, the DOJ announced its findings that the SPD had engaged in a pattern or practice of using excessive force, concluding the investigation. In addition, while the DOJ did not conclude that the SPD had engaged in a pattern or practice of discriminatory policing, the findings letter raised concerns that some of the SPD's policies and practices could result in unlawful discriminatory policing. Specifically, the findings letter noted confusion among SPD officers regarding the distinction between "casual, social interactions and investigative

detentions" (United States Department of Justice, 2011). The DOJ emphasized the importance that "officers understand that, unless they have a sufficient factual basis to detain someone, a person is free to walk away from police and free to disregard a police request... [and] in such circumstances, the decision to 'walk away' does not by itself create cause to detain" (United States Department of Justice, 2011).

After the investigation's conclusion, in an effort to avoid prolonged litigation, the SPD began negotiations with the federal government over a reform agreement to address the investigation's findings. On July 27, 2012, the SPD entered into a consent decree with the federal government. The consent decree required the SPD to implement reforms under the supervision of a federally-appointed monitor, and it remains in effect today. As part of the reforms, the SPD had to revise its policies, practices, and training related to investigatory stops and detentions, the use of weapons (particularly firearms and less-lethal options), and the use of force. Additionally, the city was required to establish a community police commission that would work collaboratively with the court-appointed monitor to provide recommendations and oversight on the implementation of the settlement agreement.[8]

3 A Model of Policing

Before delving into the empirical analysis, I present a static model with a single decision-maker allocating effort between different policing activities to formalize the relationship between policing activity and crime. The model generates two key predictions, which I test in Sections 5 and 6. First, if the federal investigation increases the cost of misconduct from stops, then stops should decrease. Second, assuming there are alternative productive policing activities, the effect of stop reductions on crime is ambiguous ex ante because, while a decrease in stops may increase crime, substitution to other policing activities may decrease crime. In this section, I describe the basic set-up of the model and the predictions generated. Formal model derivations are presented in Appendix A.

3.1 Set-up

A police captain manages a department of police officers. Her jurisdiction includes N neighborhoods. The number of officers assigned to each neighborhood is fixed. Officers can engage in stops, S_n; other productive policing practices, including observing and being present in the neighborhood, G_n; or unproductive activities such as leisure. The police captain chooses

[8]For additional information, please refer to the settlement agreement at https://www.justice.gov/sites/default/files/crt/legacy/2012/07/31/spd_consentdecree_7-27-12.pdf.

the levels of S_n and G_n to maximize a neighborhood-specific objective function. The model implies that the police captain can induce these actions from officers through monitoring and other incentives, which I have omitted to concentrate on the trade-off between different policing activities.

To simplify the analysis, I present the framework for a single neighborhood and normalize the department to include a single officer. The police captain's objective function, $V(\cdot)$, decreases in realized crime, R, and the costs she incurs to enforce policing activity. Realized crime is defined as the exogenous crime level, Θ, minus any crime reductions from policing activities, S and G. I assume that S exhibits diminishing marginal returns to crime reduction so that $\gamma < 1$, while G exhibits constant or diminishing marginal returns so that $\rho \leq 1$. I use the parameter A to capture the relative productivity of S to G for reducing crime (i.e., I normalize the productivity of G to 1). The police captain incurs costs to enforce her desired level of policing. Stops, S, are costless to enforce, but police misconduct, m, arises from stops with probability δ_m. When misconduct occurs, the police captain incurs costs, c_m, which includes the costs to the officers and the department. Therefore, the expected cost of misconduct is $c_m\delta_m$. The police captain must pay linear costs, c_g, to enforce other productive policing, G; otherwise, the officer might not engage in the desired level of G and instead engage in leisure. I further assume that the police captain's objective function is concave in realized crime so that $\tau > 1$:

$$\begin{aligned} V &= -c_m\delta_m S - c_g G - \beta R^\tau \\ &= -c_m\delta_m S - c_g G - \beta\left(\Theta - AS^\gamma - G^\rho\right)^\tau, \end{aligned} \tag{3.1}$$

where V is the police captain's pay-off for a given choice of S and G, and the parameters β and τ capture the severity of the police captain's penalty for realized crime. I assume that $\beta > 0$.

3.2 The Police Captain's Decision

The police captain chooses S^* and G^* to maximize her objective function. This yields the following first-order conditions, which implicitly define S^* and G^*:

$$V_S(S^*, G^*) = -c_m\delta_m + \beta\tau A\gamma(S^*)^{\gamma-1}\left(\Theta - AS^{*\gamma} - (G^*)^\rho\right)^{\tau-1} = 0 \tag{3.2}$$

$$V_G(S^*, G^*) = -c_g + \beta\tau\rho(G^*)^{\rho-1}\left(\Theta - A(S^*)^\gamma - (G^*)^\rho\right)^{\tau-1} = 0 \tag{3.3}$$

These first-order conditions provide a system of two equations that relates parameters to the police captain's choices of S^* and G^* and imply that the police captain sets the marginal

productivity of each policing activity equal to its marginal cost. This produces an expansion path with optimal choices of S and G given a cost budget and crime tolerance level. The police captain chooses the point along this path that maximizes her pay-off. Based on the functional forms I have chosen, the second-order conditions are always satisfied. When the federal investigation occurs, it increases the cost of misconduct, c_m, thereby increasing the marginal cost of stops and affecting the police captain's choices of S^* and G^*.

Proposition 1. *An increase in the cost of misconduct decreases stops if* $\tau > 1$, $\gamma < 1$, *and* $\rho \leq 1$.

Increasing the cost of misconduct reduces the number of stops if stops and other forms of productive policing are substitutes, and stops exhibit diminishing returns to reducing crime, while other forms of productive policing exhibit constant or diminishing returns.[9] Indeed, some degree of substitutability between different policing activity is consistent with prior studies evaluating the relative effectiveness of various forms of problem- and community-oriented policing (Gonzalez and Komisarow, 2020; Owens, 2020; Owens and Ba, 2021; Weisburd and Telep, 2014).

Proposition 2. *The impact of an increase in the cost of misconduct on realized crime depends on the relative magnitudes of* $\rho(G^*)^{\rho-1}\frac{\partial G^*}{\partial c_m}$ *and* $A\gamma(S^*)^{\gamma-1}\frac{\partial S^*}{\partial c_m}$. *If* $A\gamma(S^*)^{\gamma-1}\frac{\partial S^*}{\partial c_m}$ *is sufficiently large, then crime will increase; conversely, if* $\rho(G^*)^{\rho-1}\frac{\partial G^*}{\partial c_m}$ *is sufficiently large, then crime will decrease.*

The impact of the reduction in stops on crime is ambiguous because of two opposing forces. While the reduction in stops is likely to increase crime, substitution to other productive policing activities may decrease crime. The net impact on crime depends on the relative magnitude of these two forces: the lost productivity from decreasing stops and the gained productivity from increasing other productive policing practices. If the lost productivity from decreasing stops dominates, for example, if stops are highly productive (i.e., if A is sufficiently large) or the change in stops is sufficiently large, then crime increases. However, if the gained productivity from increasing other policing activities dominates, then crime decreases. Finally, in the knife-edge case, the two are roughly equal and crime remains unchanged.

[9]This criteria can also be weakened to if $\tau > 1$ and $\gamma \leq 1$ and $\rho \leq 1$, with at least one of the γ or ρ inequalities holding strictly (i.e., $\gamma < 1$ or $\rho < 1$), or $\tau = 1$ and $\gamma < 1$ and $\rho < 1$.

4 Data

My study draws data from multiple sources, including the Seattle Police Department, the Seattle Municipal Court, the Federal Bureau of Investigation (FBI) Uniform Crime Report (UCR) program, the 2014 American Community Survey five-year estimates, the National Crime Victimization Survey (NCVS), the Gallup Confidence in Institutions Survey, and the National Highway Traffic Safety Administration (NHTSA). I discuss each in detail below.

4.1 Seattle Administrative Data

I use records from the Seattle Municipal Court and detailed administrative data obtained through a research agreement with the SPD. The SPD data cover the department's entire geographical jurisdiction from June 2009 to December 2013. This timeframe includes 22 months before the investigation's launch and two years after its conclusion.

Seattle Municipal Court Data. I use misdemeanor case data, which include case type, charges involved, a unique defendant identifier, and charge disposition. I use the charge disposition field to identify guilty findings and negotiated plea deals. Importantly, the data contain a unique incident identifier that allows me to link these records to the computer-aided dispatch data from the SPD.

Computer-Aided Dispatch Data. Virtually all modern police departments in the United States use computer-aided dispatch (CAD) systems to assist 911 call-takers and dispatchers, particularly in large urban areas. The CAD system manages the high volume of requests for police services, collects information from callers, monitors real-time patrol unit availability, and dispatches appropriate resources. Dispatches include community-initiated dispatches (e.g., 911 and other telephone calls) and officer-initiated dispatches (e.g., officer-initiated stops). I focus my analysis on officer-initiated stops and 911 calls, which together constitute 77 percent of weekly dispatch activity in Seattle between June 2009 and December 2013.[10]

Officer-initiated (OI) stops refer to instances in which individual officers assign themselves to respond to incidents observed while patrolling. In contrast, 911 calls are requests for assistance from the public, often related to emergencies. While OI stops are primarily at the discretion of the officer, 911 calls are conditionally randomly assigned based on officer proximity, availability, and the required resources needed for the call.[11] For each dispatch, I

[10]The remaining dispatches primarily originate from other telephone calls, including 311.

[11]I test the conditionally random assignment of 911 calls in Appendix Table E1.

observe the officer(s) dispatched, date, time, location (beat, sector, and approximate coordinates), priority code, initial and final case type, and final disposition.[12] The initial and final case types are short text descriptions of the nature of the call. Initial case type describes the initial reason for the dispatch and is assigned at dispatch by the officer for OI stops or the dispatcher for 911 calls. The final case type may ultimately differ from the initial case type depending on how a dispatch progresses. The final disposition describes the outcome of the dispatch including, for example, whether an arrest was made.

I use the initial case type description field to categorize OI stops and 911 calls for my analysis.[13] Specifically, I classify OI stops into four distinct categories: premises check stops, suspicious activity stops, traffic stops, and other types of stops. Premises check stops involve inspecting specific locations to address potential issues or ensure site security. Suspicious activity stops are similar to stop-and-frisk stops and are initiated when officers have a reasonable suspicion of potential criminal activity. Traffic stops involve stopping vehicles in response to potential law violations. Combined, premises check, suspicious activity, and traffic stops constitute nearly 62 percent of all officer-initiated stops. Table 1 shows summary statistics for weekly dispatches. On average, approximately 4,503 OI stops occur each week and about 4,507 911 calls. Weekly OI stops, on average, comprise about 841 premises check stops, 1,135 suspicious activity stops, and 787 traffic stops. The average weekly arrest rate for officer-initiated stops (the stop arrest rate) is about 87 arrests per 1,000 stops.

Reported Crime Data. These data contain all reported criminal activity within the SPD's geographical jurisdiction. For each crime, I observe the date; location; National Incident-Based Reporting System (NIBRS) offense code, which I use to identify crime types; and the redacted officer narrative describing the incident.[14] I focus my analysis on serious (index) crimes. Index crimes represent the most serious violent and property offenses and are defined and tracked nationally by the FBI. Violent index crimes include homicide, rape,

[12]The data do not include officer shift assignments, so I construct shifts based on the watch hours used in the department: the first watch runs from 3:00 to 11:00 AM, the second from 11:00 AM to 7:00 PM, and the third watch runs from 7:00 PM to 3:00 AM. The priority code is an ordinal ranking from 1 to 9 that describes the urgency of the dispatch as assigned by the officer for OI stops or the dispatcher for 911 calls. The highest priority calls are assigned a 1 and suggest great urgency, usually because there is a possible threat to a person's life. They include but are not limited to medical emergencies, in-progress calls with a threat to life, all responses to aid the fire department, caller-abandoned emergency calls, and serious assaults. Priority 2 calls are urgent and involve situations that could escalate without an immediate response. Priority 3 calls are events that require prompt but not emergency response. Nonemergency calls for service are designated priority 4, 5, or 6, depending on the severity of the report. Traffic stops are priority 7, and administrative busy codes (for example, administrative duties for desk officers) are priority 9.

[13]For additional information on the classification of OI stops and 911 calls, see Appendix B.

[14]As a condition of the research partnership, the department removed protected information (such as names and addresses) from all officer narratives and replaced that information with the word "redacted" using the `NLTK` library in a `Python` script, which the department shared with me.

robbery, and aggravated assault, while property index crimes include car theft, burglary, larceny, and arson. Narrowing the scope in this way offers several benefits. Index crimes are particularly costly to victims and to society more generally (Bhatt et al., 2023; Chalfin, 2015; Cho, Gonçalves and Weisburst, 2023; Tebes and Fagan, 2022). Crucial for my analysis is that index crimes are also generally more reliably observed and measured than lower-level crimes (Devi and Fryer Jr, 2020).

My analysis uses four crime measures. Because most insurance companies require a police report for car theft claims to be processed, car thefts serve as my primary measure as they provide a reliable gauge of criminal activity and allow me to minimize bias from changes in civilian reporting behavior.[15] I also include violent crimes and other property crimes, as well as the social cost of index crimes in US$1,000s, as additional outcomes to broaden my analysis. I calculate the social cost of index crimes using the estimates presented in Bhatt et al. (2023), which I deflate to 2009 dollars.[16]

Table 1 provides summary statistics for weekly reported crimes. On average, there are approximately 74 car thefts, 581 other property crimes, 66 violent crimes, and 649 non-index crimes each week during my sample period. The average weekly social cost of index crimes, measured in US$1,000s, is approximately 8,170. The average length of a crime report is approximately 187 words.

Officer Data. Finally, I use officer data provided by the SPD, which include a unique officer identifier as well as officer race, sex, and most recent hire year. I use the most recent hire year variable to construct officer experience as of 2009 for my analyses. When using this variable, I focus on officers whose last hire year is no later than 2009 and whom I observe in the data both before and after the federal investigation begins on March 31, 2011. Officer demographic summary statistics are reported in Table 1. Eight percent of the officers in my analysis are Black, 5 percent are Hispanic, and the overwhelming percentage are White (70 percent). About 13 percent of officers are female. The average officer has about 14 years of experience as of 2009.

I also create three measured officer traits (based on officer fixed effects) using the computer-aided dispatch data and municipal court records. I use these traits in addition to officer demographic variables to explore heterogeneous officer responses to the federal investigation. The first trait is the OI arrest officer fixed effect, which captures an officer's arrest propensity

[15] Bhatt et al. (2023) show that car thefts have the second-highest reporting rate to the police after homicides. While homicides are often used in the literature to credibly measure crime, given their relative rarity and the geographic granularity at which much of the analysis is conducted, homicides are not a feasible primary outcome.

[16] The social cost of each index crime in 2009 dollars is presented in Appendix Table E2.

in OI stops, conditional on stop characteristics. I estimate these fixed effects using ordinary least squares (OLS) regressions on officer–dispatch-level data containing all OI stops between June 2009 and the investigation launch. The second trait is the 911 arrest officer fixed effect, which captures an officer's arrest propensity in 911 calls conditional on call characteristics. I estimate these fixed effects using OLS regressions on officer–dispatch-level data containing all 911 calls between June 2009 and the investigation launch. The final trait is the conviction officer fixed effect, which captures the likelihood that misdemeanor charges associated with an officer lead to a guilty finding, conditional on charge characteristics. I estimate these officer fixed effects using OLS regressions on officer–charge-level data containing all charges filed between June 2009 and the investigation launch. Finally, I use the estimated fixed effects to create three indicator variables set to 1 for officers with values above the median for each trait. I include detailed information on the construction of these measured officer traits in Appendix C and show the correlation between officer traits in Appendix Table E3.

4.2 Other Data

American Community Survey. In my analysis, I use census block groups to define neighborhoods and augment the SPD data with demographic information from the 2014 American Community Survey (ACS) five-year estimates for each census block group in Seattle. Specifically, I incorporate information on the racial composition of the residential population in each census block group. I classify a census block group as a minority neighborhood if the share of non-Hispanic White residents is less than 50 percent. Summary statistics for the racial composition of neighborhoods are presented in Table 1. On average, minority neighborhoods in Seattle are composed of approximately 31 percent non-Hispanic White residents, 18 percent non-Hispanic Black residents, 29 percent non-Hispanic Asian residents, and approximately 14 percent Hispanic residents. In contrast, non-minority neighborhoods are approximately 76 percent non-Hispanic White residents, 4 percent non-Hispanic Black residents, 9 percent non-Hispanic Asian residents, and approximately 6 percent Hispanic residents. Figure 2 illustrates the racial composition of neighborhoods in Seattle.

Federal Bureau of Investigation (FBI) Uniform Crime Report (UCR) Program. I also employ monthly crime data from the FBI UCR program to compare changes in crime in Seattle to other jurisdictions in the U.S. without federal investigations. Specifically, I use the UCR Offenses Known and Clearances by Arrest data set, which contains monthly index crimes reported by law enforcement agencies across the United States (Kaplan, 2022).[17] The

[17] I use the version of this data set that has been cleaned and formatted by Jacob Kaplan.

FBI UCR program is a nationwide, cooperative effort, and participation by individual law enforcement agencies is voluntary. However, as of 2014, the law enforcement agencies active in the program covered approximately 98 percent of the U.S. population. I impose the following restrictions on the data. First, I restrict my data set to law enforcement agencies that are core local police departments. Additionally, I restrict it to agencies representing jurisdictions with at least 150,000 but no more than 750,000 residents as of 2010, and I require that agencies have consistently reported monthly crime data for at least 10 years between 2005 and 2015. Finally, I exclude any agency, except the SPD, that was subject to a federal investigation before 2015.[18] The final data set includes the SPD and 82 control agencies.[19]

Other. Finally, I complement my analyses with data on fatal crashes from the National Highway Traffic Safety Administration, data on crime victimization and reporting from the National Crime Victimization Survey's MSA Public-Use data files, and data on confidence in the police from Gallup's Confidence in Institutions survey.

5 The Effect of the Investigation on Officer Activity

In this section, I present my main results for the effect of the federal investigation on officer activity. I focus on four measures: the number of officer-initiated (OI) stops; the number of arrests resulting from OI stops; the OI stop arrest rate, which I define as the number of arrests per 1,000 OI stops; and the length of police reports in words. In the sections that follow, I analyze the outcomes related to OI stops. Consistent with the model's prediction, I show that when the federal investigation increases the cost of misconduct, stops decrease.[20] Next, I explore heterogeneous stop reductions by stop type, neighborhood race, and officer traits to provide a more complete picture of how and where policing changed in response to the federal investigation. These heterogeneity analyses relate to key parameters in my model and yield insights for my analysis of the effects on serious crime in subsequent sections.

5.1 Empirical Strategy: Interrupted Time Series

To estimate changes in aggregate weekly OI stops in response to the federal investigation, I implement an interrupted time series (ITS) design on weekly data spanning June 2009 to

[18] For a list of departments with investigations, refer to Devi and Fryer Jr (2020).

[19] I provide the list of donor agencies in Appendix Table E4.

[20] My model assumes no significant changes in the number of officers, which I demonstrate in Appendix Figure D1.

December 2013 for Seattle as a whole. This framework compares average weekly outcomes in the 18 months preceding the ACLU complaint (June 2009 to November 2010) with average weekly outcomes in the subsequent months, extending to 2 years after the investigation's conclusion. I use the following OLS specification:

$$\begin{aligned} Y_t =& \beta_0 + \beta_1 \text{Complaint}_t + \beta_2 \text{Investigation}_t \\ &+ \beta_3 \text{Post-Investigation}_t + \beta_4 \text{Consent Decree}_t + \tau_t + \epsilon_t, \end{aligned} \tag{5.1}$$

where Y_t is the number of OI stops, the number of arrests, or the OI stop arrest rate (arrests per 1,000 OI stops) in week t. Complaint_t is an indicator for the weeks between the ACLU complaint on December 3, 2010, and the investigation launch on March 31, 2011; Investigation_t is an indicator for the weeks between the investigation launch and the findings report on December 16, 2011; $\text{Post-Investigation}_t$ is an indicator for the weeks between the findings report and the consent decree on July 27, 2012; and Consent Decree_t is an indicator for the weeks after the consent decree through December 2013. τ_t represents week-of-the-year fixed effects to adjust for seasonality, and ϵ_t is the error term. Standard errors are computed using the Newey-West method (Newey and West, 1987). The identifying assumption is that, without the investigation, outcomes would have been similar to outcomes during the pre-period.

5.1.1 Results

Figure 3 shows seasonally adjusted weekly OI activity. Panel A plots weekly stops, while Panel B shows weekly arrests from stops. The shaded area in each panel represents the federal investigation period. The figure shows a decrease in both OI stops and arrests during the investigation period. During the post-investigation period, when the SPD was negotiating a reform agreement with the federal government, stops increased but remained below pre-period levels, and they remained relatively stable during the consent decree period. In contrast, arrests from OI stops remained low during both the post-investigation and consent decree periods.

Table 2 reports the regression results from estimating Equation 5.1 and confirms the visual evidence in Figure 3. During the investigation period, weekly OI stops fell by about 1,374 (standard error: 117.7) or approximately 26 percent of the pre-period mean. The estimate is statistically significant at the 1 percent level. Weekly arrests from stops decreased by 154 (standard error: 15.2) or about 30 percent of the pre-period mean. The estimate is statistically significant at the 1 percent level. The estimate for the weekly OI arrest rate is statistically significant at the 5 percent level and suggests a decrease of about 6 arrests per

1,000 stops or about 6 percent of the pre-period mean. During the post-investigation period, average weekly stops, arrests from stops, and the stop arrest rate remained lower than pre-period levels. These estimates are all significant at the 1 percent level. Finally, during the consent decree period, weekly stops were higher than during the investigation period but still 858 (standard error: 99.3) stops lower than pre-period levels or down approximately 16 percent from the pre-period mean. Weekly arrests from stops decreased by 208 (standard error: 9.3) relative to pre-period levels or about 40 percent of the pre-period mean. The stop arrest rate decreased by about 28 arrests per 1,000 stops or about 28 percent of the pre-period mean. All estimates during the consent decree period are statistically significant at the 1 percent level. These findings suggest that during the first 17 months under the consent decree, officers made more stops compared to the investigation period but significantly fewer than during the pre-period and that these stops were less likely to result in arrests. An important caveat is that I cannot determine whether these stops were less likely to result in arrests because the stops themselves did not warrant arrests or because officers were generally disinclined to make arrests.

5.2 Effect on Weekly OI Stops by Stop Type

For simplicity, my model assumes a single parameter A captures the relative productivity of stops (i.e., all stop types are equally productive) in reducing serious crime. However, different types of stops may be differentially productive at reducing serious crime. For instance, stops related to potential traffic violations may not be highly productive at reducing serious crime or may be just as effective as officers remaining in the neighborhood without making these stops. In this section, I leverage the granularity of my data to examine which types of stops officers changed in response to the federal investigation. While one approach could involve officers reducing all stops proportionally in response to the federal investigation, an alternative strategy, supported by discussions with SPD staff, suggests that officers might reduce stops where they have greater discretion or where effort is potentially less productive, either because the stop is unlikely to deter crime or because the benefits of the stop are unlikely to outweigh the potential costs of misconduct. Stops related to traffic violations or suspicious activity are prime candidates for such reductions, especially because many of these stops may not be initiated in response to observing actual crimes.[21] I show that when the federal investigation increases the cost of misconduct, thereby increasing the expected cost of stops, stops for traffic violations and suspicious activity are disproportionately affected.

[21] Stops for victimless crimes that require proactive officer effort, such as those for prostitution, would also be good candidates. While I observe decreases in stops for prostitution, such stops represent a relatively small share of overall stops in my data.

5.2.1 Empirical Strategy: Interrupted Time Series

I examine the change in different types of OI stops by estimating Equation 5.1 separately for the four different stop-type categories: traffic stops, suspicious activity, premises checks, and all other stops.[22] Standard errors are computed using the Newey-West method (Newey and West, 1987).

5.2.2 Results

Figure 4 shows seasonally adjusted weekly officer-initiated activity by stop type. Panel A plots weekly stops, and Panel B shows weekly arrests from stops. This figure shows a significant decrease in both traffic and suspicious activity stops during the investigation period. Other stops also decreased during the investigation period, but premises check stops were unaffected. I observe a notable spike in the number of stops in the "other" category during the post-investigation period. This increase likely reflects changes in how stops were being classified in response to the concerns raised by the DOJ about investigative detention stops. In Panel B, I show that arrests fell mostly among traffic and suspicious activity stops during the investigation period.

Table 3 presents regression results for the change in OI stops for each of the four stop-type categories. I show that the majority of the decrease in OI stops during the investigation is driven by decreases in traffic and suspicious activity stops. During the investigation, average weekly traffic stops decreased by 546 (standard error: 30.2) or about 46 percent of the pre-period mean. Weekly arrests from traffic stops decreased by 27 (standard error: 2.8) or about 37 percent of the pre-period mean. Both estimates are statistically significant at the 1 percent level. I also find a significant increase in the weekly stop arrest rate during the investigation of approximately 10 arrests per 1,000 stops or about 16 percent of the pre-period mean. This estimate is statistically significant at the 5 percent level. During the investigation, suspicious activity stops decreased by 731 (standard error: 33.1) or about 42 percent of the pre-period mean. Weekly arrests from suspicious activity stops decreased by 87 (standard error: 7.3) or about 38 percent of the pre-period mean. Both estimates are statistically significant at the 1 percent level. I find a marginally significant increase in the weekly stop arrest rate during the investigation of approximately 8 arrests per 1,000 stops or about 6 percent of the pre-period mean. Weekly "other" stops also decreased during the investigation by 115 (standard error: 45.4) or about 8 percent of the pre-period mean. The estimate is statistically significant at the 5 percent level. Weekly arrests from other stops fell by 38 (standard error: 9.3). This estimate is statistically significant at the 1 percent level.

[22] For additional information on how these stop types are defined, see Section 4.

The weekly stop arrest rate for other stops also fell by about 17 arrests per 1,000 stops or about 12 percent of the pre-period mean. By contrast, the estimate for premises check stops during the investigation is positive and is not statistically significant. I find a marginally significant decrease in weekly arrests from premises check stops. The weekly stop arrest rate for premises check stops decreased by about 2 arrests per 1,000 stops or about 31 percent of the pre-period mean. The estimate is statistically significant at the 5 percent level.

5.3 Effect on Weekly OI Stops by Neighborhood Race

The complaint against the SPD alleged that officers engaged in racially-biased policing, specifically excessive use of force against people of color. It is reasonable to hypothesize that the resulting investigation might be more salient in minority neighborhoods. One way to capture this possibility in my model is through a neighborhood-specific cost of misconduct, $c_{m,n}$. Some neighborhoods may have higher perceived costs of misconduct if, for example, residents are more politically organized or engaged. Conversely, some neighborhoods may have lower perceived costs of misconduct if residents are considered more likely to engage in criminal activity and stops are seen as more necessary. Residents in minority neighborhoods may be less likely to complain, for example, due to mistrust of the police or the political process, and they may also be less likely to have their complaints taken seriously. This rationale is also consistent with a recent study showing that officers spend more time patrolling in neighborhoods with larger Black, Hispanic, or Asian populations (Chen et al., Forthcoming). If the federal investigation sets a sufficiently high neighborhood-agnostic cost of misconduct, $\bar{c_m}$, so that misconduct is equally costly regardless of where it occurs, then I would expect that neighborhoods that had lower initial c_m would have larger decreases in stops because the gap between initial c_m and $\bar{c_m}$ is larger. In this section, I analyze whether there was a differential response based on the racial composition of Seattle neighborhoods, which I define by census block groups.

5.3.1 Empirical Strategy: Difference-in-Differences

To estimate differential changes in policing activity by neighborhood racial composition, I implement a difference-in-differences model on a balanced neighborhood weekly panel using

the following specification:

$$\begin{aligned} Y_{n,t} =& \gamma_1 \text{Minority}_n \times \text{Complaint}_t + \gamma_2 \text{Minority}_n \times \text{Investigation}_t \\ &+ \gamma_3 \text{Minority}_n \times \text{Post-Investigation}_t + \gamma_4 \text{Minority}_n \times \text{Consent Decree}_t \\ &+ \gamma_5 \text{Complaint}_t + \gamma_6 \text{Investigation}_t + \gamma_7 \text{Post-Investigation}_t + \gamma_8 \text{Consent Decree}_t \\ &+ \eta_n + \tau_t + \epsilon_{n,t}, \end{aligned} \tag{5.2}$$

where $Y_{n,t}$ is the number of OI stops, the number of arrests, or the OI stop arrest rate (arrests per 1,000 stops) in neighborhood n in week t; Minority_n is an indicator for whether the share of non-Hispanic White residents in neighborhood n is less than 50 percent; η_n are neighborhood fixed effects; τ_t are week-of-the-year fixed effects to adjust for seasonality; and $\epsilon_{n,t}$ is the error term. Standard errors are calculated using the Newey-West method. The identifying assumption is that, without the investigation, minority and nonminority neighborhoods would have common trends in outcomes.

Are Estimated Effects Equiproportional?

An important consideration is whether any estimated differences in stop reductions between minority and nonminority neighborhoods might reflect equiproportionate changes from pre-period means. In this scenario, if minority neighborhoods had more OI stops in the pre-period, they may appear to experience larger reductions in stops, even if the reductions are proportional to those in nonminority neighborhoods. To test for equally proportional responses between minority and nonminority neighborhoods, I add controls for each neighborhood's pre-period average weekly number of OI stops interacted with each time period indicator to Equation 5.2. If the estimated effects indicating differential responses to the investigation based on neighborhood race are entirely explained by differences in pre-period stop levels, then these effects should no longer remain statistically significant after I include controls for the pre-period mean interacted with the time period indicator.

5.3.2 Results

Figure 5 reproduces Figure 3 broken out for minority and nonminority neighborhoods. Panel A plots stops, and Panel B shows arrests from stops. Consistent with Chen et al. (Forthcoming), minority neighborhoods had more OI stops than nonminority neighborhoods in the pre-period. During the investigation, minority neighborhoods experienced a larger decrease in stops than nonminority neighborhoods. After the investigation, stops steadily increased in both neighborhood types, but stops remained below pre-period levels in minority neigh-

borhoods. Similarly, there were more arrests in minority neighborhoods than nonminority neighborhoods in the pre-period. During the investigation, minority neighborhoods again experienced larger decreases in arrests than nonminority neighborhoods. Unlike stops, arrests remained low in both minority and nonminority neighborhoods after the investigation ended.

Table 4 reports regression results from estimating Equation 5.2. These results are consistent with Figure 5. I find both larger decreases in stops and smaller decreases in the arrest rate of stops in minority neighborhoods, suggesting that forgone stops were less likely than average to have resulted in arrest. During the investigation, OI stops in minority neighborhoods decreased by an additional 1.6 (standard error: 0.26) stops per week or about 18 percent of the pre-period mean. This estimate is statistically significant at the 1 percent level. Weekly arrests from OI stops decreased in minority neighborhoods by an additional 0.2 (standard error: 0.05) arrests per week or about 25 percent of the pre-period mean. This estimate is statistically significant at the 1 percent level. By contrast, the arrest rate for OI stops in minority neighborhoods was about 9 arrests per 1,000 stops more or 16 percent higher than the pre-period mean. This estimate is significant at the 5 percent level. In the post-investigation period, average weekly stops and arrests were also significantly lower in minority neighborhoods, but I do not find significant effects for the stop arrest rate. During the consent decree period, weekly stops in minority neighborhoods dropped by 1.9 (standard error: 0.26) or about a 21 percent decrease from the pre-period mean. This estimate is statistically significant at the 1 percent level. Weekly arrests from OI stops in minority neighborhoods decreased by an additional 0.4 (standard error: 0.04) arrests or about 44 percent of the pre-period mean. This estimate is statistically significant at the 1 percent level. The arrest rate for OI stops in minority neighborhoods was about 6 arrests per 1,000 stops fewer or a 10 percent drop from the pre-period mean. However, this estimate is only marginally significant.

In Table 5, I report estimated effects when I additionally include interaction terms between each neighborhood's pre-period average weekly number of OI stops and the time period indicators to test for equiproportional responses across neighborhoods. These estimates suggest that the differential reductions in stops and arrests reported in Table 4 are not entirely driven by pre-period level differences in weekly outcomes.

5.3.3 Robustness

As a robustness check, I include an additional specification in which I re-estimate Equation 5.2 replacing week-of-the-year fixed effects with calendar-week fixed effects to flexibly control for time, and I cluster standard errors at the neighborhood level. These results are

reported in Appendix Table E6 and are qualitatively similar to the results in Table 4.

5.4 Effect on Weekly OI Stops by Officer Traits

A growing strand of the economics literature demonstrates the role of individual officer traits in shaping policing behavior. Black and female officers tend to engage in fewer stops, make fewer arrests, and use force less often than their peers (Ba et al., 2021; Hoekstra and Sloan, 2022). If these findings reflect officers with these traits perceiving higher costs of misconduct (c_m in my model) associated with their policing activities, then I posit that these traits may also impact officers' response to the federal investigation. Specifically, when the federal investigation establishes a new officer-agnostic $\bar{c_m}$, officers with higher initial c_m values in the pre-period may reduce their stops less than their peers with lower initial c_m values. In this section, I examine whether individual officer traits predict differential responses to the federal investigation using the officer traits described in Section 4.

5.4.1 Empirical Strategy: Difference-in-Differences

To estimate differential changes in OI stops by officer traits, I implement a difference-in-differences model on an unbalanced officer weekly panel using the following specification:

$$\begin{aligned} Y_{j,t} =& \alpha_1 \text{Officer Trait}_j \times \text{Complaint}_t + \alpha_2 \text{Officer Trait}_j \times \text{Investigation}_t \\ &+ \alpha_3 \text{Officer Trait}_j \times \text{Post-Investigation}_t + \alpha_4 \text{Officer Trait}_j \times \text{Consent Decree}_t \\ &+ \theta_j + \tau_{j,t} + \nu_{j,t}, \end{aligned} \tag{5.3}$$

where $Y_{j,t}$ is the number of OI stops by officer j in week t, and Officer Trait_j is one of the following officer traits: officer race (Black and Hispanic vs. other), sex (female vs. male), experience (high vs. low), misdemeanor conviction rate (high vs. low), OI stop arrest rates (high vs. low), or 911 call arrest rates (high vs. low).[23] θ_j are officer fixed effects, $\tau_{j,t}$ are home-sector-by-calendar-week fixed effects to flexibly control for time effects, and $\nu_{j,t}$ is the error term. I do not argue that these officer traits capture independent officer characteristics, so I do not include them collectively in the same regression. Moreover, it may not be appropriate to include them all in one regression. For example, one would not include both arrest fixed effect measures in the same regression due to potential collinearity. Because I lack officer assignment data for my analysis, I define an officer's home sector as the sector in which most of her OI stops occur in the pre-period, following Hoekstra and Sloan (2022). By including home-sector-by-calendar-week fixed effects in the specification,

[23] Please see Section 4 for more details on these officer traits.

I test for the role of officer traits beyond effects of their assigned location. Standard errors are calculated using the Newey-West method.

Are Estimated Effects Equiproportional?

Here again, it is important to consider whether any estimated differences in stop reductions might reflect equiproportionate changes from pre-period means. In this case, officers with fewer stops in the pre-period may appear to reduce stops less, even if they are reducing stops in proportion to their peers. To test for equally proportional responses across officer traits, I add controls for each officer's pre-period average weekly number of OI stops interacted with each time period indicator to Equation 5.3. If the estimated effects indicating a differential response to the investigation by an officer trait are entirely explained by differences in pre-period stop levels, then the estimates will not be statistically different from zero after I include controls for the pre-period mean interacted with the time period indicator.

5.4.2 Results

Figure 6 shows normalized seasonally adjusted OI stops for the different officer traits. I normalize each series by dividing by the pre-period mean so that these figures reflect proportional changes in weekly OI stops. Surprisingly, I do not find visual evidence of different proportional responses across most of these officer traits. The notable exception is officers with high versus low arrests rates among pre-period OI stops, adjusted for stop characteristics (i.e., the OI arrest fixed effect).

Table 6 reports the results from estimating Equation 5.3. Columns 1 and 2 present estimates for the regression with race as the officer trait in the difference-in-differences specification. Each subsequent column presents estimates for a different officer trait: Column 3 reports sex, Column 4 reports experience, and Columns 5 through 7 report conviction fixed effects, OI arrest fixed effects, and 911 call fixed effects, respectively. In this table, positive coefficients indicate smaller decreases in OI stops compared to the control group, and negative coefficients indicate larger decreases compared to the control group. I find that Black, female, and more experienced officers decreased OI stops less than their peers. I find that Black officers reduced their stops by 0.54 (standard error: 0.12) fewer stops than their non-Hispanic non-Black peers during the investigation. I find that female officers decreased stops by 0.48 (standard error: 0.11) fewer stops than their male peers. High-experience officers decreased stops by 2.15 (standard error: 0.08) fewer stops than their more junior colleagues. On the other hand, I find that officers with high arrest fixed effects reduced stops more than their peers. Officers with high OI arrest fixed effects reduced stops by 0.92 (standard error:

0.08) more stops than their peers with low OI arrest fixed effects. Similarly, officers with high 911 arrest fixed effects reduced stops by 0.64 (standard error: 0.08) more stops than their peers. All the reported estimates are statistically significant at the 1 percent level.

I report the results when I include interaction terms between an officer's pre-period average weekly number of officer-initiated stops and the time period indicators to test for equally proportional responses in Table 7. Only the estimate for officers with high OI arrest fixed effects remains significant. This finding implies an equally proportional response across the remaining officer traits, which is consistent with the visual evidence in Figure 6.

5.5 Effect on Police Report Length

Before turning to my analysis of crime effects, I present one additional measure of police officer activity: the length of police reports. Despite their relative importance in the criminal justice system, few studies in economics have examined police reports as a meaningful measure of officer activity (Campbell and Redpath, 2023). Police officers write police reports to document incidents to which they have responded or have been involved. A crime report is an officer's written record of reported criminal activity occurring in her department's jurisdiction. Such reports are ubiquitous in police departments across the United States and can link police departments to later stages of the criminal justice system. For example, prosecutors use reports to build their cases and can occasionally use reports as evidence in court.

In response to increased scrutiny, officers may produce more detailed records as a way to safeguard themselves against complaints or other unwanted attention, a hypothesis supported by my discussion with SPD staff. In this section, I examine whether the length of police reports changes in response to the events surrounding the federal investigation. I begin with Figure 7, which shows monthly average police report length based on how crimes were discovered (the full sample, crimes discovered via OI stops, crimes discovered through the 911 call system, and crimes discovered through another source). To account for potential changes in the composition of crime types over time, I hold crime type composition fixed at pre-period levels.[24] Across the panels of the figure, I show that report length increased after the ACLU complaint, remained consistently high during the investigation period, and further increased during the consent decree period, especially in cases where an arrest was made. A natural question is whether the increases in report length could be selection driven, i.e., as officers engage less, the forgone police reports would have been for less-serious incidents and therefore would have been shorter. Consider the case of police reports for crimes discovered

[24]The takeaways are qualitatively similar when examining the raw unadjusted series, which I show in Appendix Figure D2.

from OI stops. If increased report length were entirely selection driven, I would expect that as officers make fewer stops and arrests during the complaint and investigation period, as I demonstrate in Figure 3, average report length would steadily increase. Instead, Panel A of Figure 7 shows that report length is relatively flat over this period. Moreover, despite stops and arrests being relatively stable during the consent decree period, I find another increase in report length, suggesting that observed increases in report length are unlikely to be entirely selection driven.

I formally estimate the change in police report length using the following difference-in-differences specification on report-level data:

$$\begin{aligned} Y_{r,t,l} =& \lambda_0 \text{Arrest}_r + \lambda_1 \text{Arrest}_r \times \text{Complaint}_t + \lambda_2 \text{Arrest}_r \times \text{Investigation}_t \\ &+ \lambda_3 \text{Arrest}_r \times \text{Post-Investigation}_t + \lambda_4 \text{Arrest}_r \times \text{Consent Decree}_t \\ &+ \lambda_5 \text{Complaint}_t + \lambda_6 \text{Investigation}_t + \lambda_7 \text{Post-Investigation}_t \\ &+ \lambda_8 \text{Consent Decree}_t + X_{r,t,l} + \epsilon_{r,t,l}, \end{aligned} \tag{5.4}$$

where $Y_{r,t,l}$ is the length of the police report, in words, written for reported crime r, which occurred at time t. Arrest_r is an indicator for whether crime r resulted in an arrest. $X_{r,t,l}$ includes month-of-the-year fixed effects to adjust for seasonality, beat and shift fixed effects to control for location and time, and NIBRS code fixed effects to control for crime types. Finally, $\epsilon_{r,t,l}$ is the error term. Standard errors are clustered at the beat level.

5.5.1 Results

Table 8 presents the results from estimating Equation 5.4. I estimate the regression on the full sample of crime reports as well as subsamples based on how crimes were discovered. These results are consistent with the visual evidence depicted in Figure 7. During the investigation, police reports for crimes in which arrests were made were on average 46 (standard error: 4.5) words longer or about 29 percent longer than the pre-period mean. I find similar effects when I examine crimes by discovery source. The police reports for crimes discovered from OI stops were 33 (standard error: 8.4) words longer or about 17 percent longer than the pre-period mean. For crimes discovered from 911 calls, reports were 57 (standard error: 6.2) words longer or about 28 percent longer than the pre-period mean. Lastly, crime reports for crimes discovered from other sources were 39 words longer or about 33 percent longer than the pre-period mean. All reported estimates are statistically significant at the 1 percent level. These findings suggest that officers might have been putting more effort into report writing following the events that spurred the investigation, a conclusion consistent with my discussions with SPD personnel. While an analysis of what is additionally being included

in these longer reports is out of scope for this study, it is a fruitful path for potential future work.

6 The Effect on Public Safety

6.1 Effects on Serious Crime

In the previous section, I demonstrated a 26 percent decrease in weekly OI stops during the federal investigation, concentrated among traffic and suspicious activity stops. Given this evidence, a crucial question arises: did the decline in OI stops impact serious crime rates? My model suggests that the impact of reductions in stops on crime is ex ante unclear and depends on the relative productivity of stops compared with other policing activity in reducing serious crime and the magnitude of stop reductions. To address this question, I employ two complementary approaches. First, I compare crime rates between Seattle neighborhoods that experienced larger versus smaller reductions in OI stops. If the reductions in OI stops increased crime, I might expect neighborhoods that saw larger stop reductions to experience larger crime increases. In the second approach, I compare serious crime rates in Seattle to serious crime rates in jurisdictions whose police departments were not subject to federal investigations. Similarly, if the reduction in OI stops in Seattle led to increased crime, I would anticipate that crime in Seattle would be higher than crime in control jurisdictions.

6.2 Crime Effects Using Within-Seattle Variation

To begin my examination of crime effects, I exploit variation across Seattle neighborhoods in OI stop reductions during the investigation. In Section 5, I showed that minority neighborhoods experienced larger decreases in weekly OI stops than nonminority neighborhoods. I exploit this findings to examine whether minority and nonminority neighborhoods experienced differential changes in crime.

6.2.1 Empirical Strategy: Difference-in-Differences

I examine within-Seattle effects on crime by estimating Equation 5.2 on a balanced weekly neighborhood panel data set. My dependent variables are the weekly number of car thefts, violent crimes, property crimes, and the social cost of index crimes in US$1,000s. Standard errors are calculated using the Newey-West method.

6.2.2 Results

Figure 8 shows seasonally adjusted weekly crimes for minority and nonminority neighborhoods. Panel A shows car thefts, Panels B shows violent crimes, Panel C shows property crime, and Panel D shows the social cost of index crimes in US$1,000s. The figures do not show noticeable differences in crime changes across neighborhoods during the investigation, although the noisiness of the series make this difficult to discern.

Table 9 shows the regression results from estimating Equation 5.2 with weekly crime outcomes. In line with the visual evidence displayed in Figure 8, I do not find significant differences between serious crime in minority and nonminority neighborhoods. The estimates from the model are generally small and are not statistically significant. However, the standard errors for these estimates are sufficiently large that I cannot rule out economically meaningful increases among some crimes. The implied 95% confidence interval for car thefts cannot rule out up to a 20 percent increase in car thefts. On the other hand, the implied 95% confidence interval for violent crimes can rule out more than a 2.5-percent increase in violent crimes. While the implied 95% confidence interval for property crimes can rule out an increase of more than 5.7 percent, the 95% confidence interval for the social cost of index crimes cannot rule out up to a 42-percent increase in costs.

In Table 10, I calculate implied estimates for crimes per 1,000 OI stops averted based on the estimates in Tables 4 and 9. I also report the 95% credible interval from performing Bayesian bootstrapping across neighborhoods with 1,000 replications (Rubin, 1981). I include estimates for each time period of my analysis, as well as an estimate for *Post*, which combines all time periods after the ACLU complaint. The estimates during the investigation imply 7 additional car thefts, 7 fewer violent crimes, 6 additional property crimes, and 199 lower social cost of index crimes for every 1,000 stops averted. The estimates on *Post* imply 2 fewer car thefts, 6 fewer violent crimes, 1 additional property crime, and 298 lower social cost of index crimes for every 1,000 stops averted. Moreover, the 95% credible intervals can rule out more than 5 additional car thefts and 3 additional violent crimes per 1,000 stops averted. However, the 95% credible intervals for property crimes and social costs of index crime cannot rule out up to 110 additional property crimes and 4,494 in additional social cost of index crimes per 1,000 stops averted.

6.3 Crime Effects Using Across-US Variation

Next, I utilize data from the FBI UCR program to determine whether Seattle experienced differential increases in serious crime compared with jurisdictions whose police departments did not undergo federal investigations. I construct a balanced agency-by-month panel from

June 2009 to 2013 that includes the SPD and 82 control agencies without federal investigations and employ the synthetic control (SC) method (Abadie, Diamond and Hainmueller, 2010), implemented through the `synthdid` package (Arkhangelsky et al., 2021) in `R`, to estimate the impact of the investigation on crime in Seattle. The SC method offers a valid approach to estimating effects and conducting inference in settings with a single treated unit and multiple control units (Abadie, 2021). The SC method constructs a counterfactual for Seattle by reweighting control units so that the weighted average outcomes of these units match the pre-treatment outcomes of the treated unit as closely as possible in terms of pre-treatment levels and time trends. The synthetic control estimator captures the average causal effect of a treatment, $\hat{\beta}^{sc}$:

$$(\hat{\beta}^{sc}, \hat{\mu}, \hat{\gamma}) = \arg\min_{\mu,\gamma,\beta} \sum_{c=1}^{C} \sum_{t=1}^{T} (Y_{c,t} - \mu - \gamma_t - W_{c,t}\beta)^2 \hat{\omega}_c, \tag{6.1}$$

where $Y_{c,t}$ is monthly car thefts, violent crimes, property crimes, and social cost of index crimes in US$1,000s per 100,000 residents for agency c in month t. μ is a constant, γ_t represents time fixed effects, $W_{c,t}$ is an indicator that equals one for Seattle in the months after the ACLU complaint, and $\hat{\omega}_c$ are the weights for control agencies selected to match the pre-treatment outcomes of Seattle. The SC model then attributes any post-treatment divergence between the post-treatment outcome of Seattle and the post-treatment outcome of the control agencies weighted by $\hat{\omega}_c$ to the treatment. The identifying assumption is that, without the investigation, there would be no systematic differences between Seattle and the weighted outcomes of control agencies. Standard errors are calculated using the placebo method, as described in Arkhangelsky et al. (2021), with 500 replications.

6.3.1 Synthetic Control Results

I show monthly crimes per 100,000 residents in Seattle compared to its synthetic control counterfactual in Figure 9. Despite being seasonally adjusted, the crime outcome variables exhibit notable variation. Nevertheless, the figure provides compelling visual evidence that there were no detectable increases in crime in Seattle up to 2 years following the conclusion of the investigation.

I present the estimated effects in Table 11.[25] For car thefts, the estimated effect is -0.05 (standard error: 0.55). The estimate is not statistically different from zero and is modest

[25]For consistency, I focus on the crime outcomes used throughout my analysis, but I report separate SC estimates for monthly homicides per 100,000 residents in Appendix Figure D3 and Appendix Table E7. The estimate is negative and not statistically significant. However, the implied 95% confidence interval cannot rule out up to a 34-percent increase in homicides.

relative to the pre-period monthly average of 49 car thefts per 100,000 residents in Seattle. Furthermore, the implied 95% confidence interval rules out more than a 2.1-percent increase in car thefts during the 37 months following the ACLU complaint. For violent crimes, the estimated effect is 0.14 (standard error: 0.63). The estimate is not statistically different from zero and is small relative to the pre-period monthly average of 50 violent crimes per 100,000 residents in Seattle. The implied 95% confidence interval rules out more than a 2.7-percent increase in violent crimes during the 37 months following the ACLU complaint.

For property crimes, the estimated effect is 0.16 (standard error: 1.71). The estimate is not statistically different from zero and is small relative to the pre-period monthly average of 430 property crimes per 100,000 residents in Seattle. The implied 95% confidence interval rules out more than a 0.8-percent increase in property crimes during the 37 months following the ACLU complaint. Finally, the estimated effect on the social cost of index crimes in US$1,000s is -$5.64 (standard error: $275.53). The estimate is not statistically different from zero and is small relative to pre-investigation monthly average social cost of $5,910 per 100,000 residents in Seattle. However, the implied 95% confidence interval cannot rule out up to a 9-percent increase in social cost during the 37 months following the ACLU complaint.

As a robustness check, I also employ the synthetic difference-in-differences (SDID) method (Arkhangelsky et al., 2021) to estimate the effect of the investigation on crime in Seattle. In contrast to the SC method, the SDID method reweights control units to roughly match the pre-treatment trends of the treated unit, allowing for constant differences between treated and control units. Standard errors are calculated using the placebo method, as described in Arkhangelsky et al. (2021), with 500 replications. I present the results for the estimated effects on crime in Seattle using the SDID method in Appendix Figure D4 and Appendix Table E8. These estimates, while more imprecise, are qualitatively similar to those obtained using the SC method.

6.4 Other Measures of Community Well-being

In the previous section, I showed that despite the significant decrease in OI stops in Seattle, there was no detectable difference between crime changes in neighborhoods that experienced larger versus smaller decreases in OI stops or between crime changes in Seattle compared to jurisdictions whose police departments did not have federal investigations. A related question is whether the investigation affected other measures of public safety or community well-being, such as 911 calls or fatal car crashes. I begin with an analysis of 911 calls, which represent a community response that is related to but distinct from crime. While some crimes are reported through 911 calls, a large share of 911 calls are not related to crimes. If

residents valued some share of OI stops, they might call 911 for assistance when these stops are reduced in response to the investigation. On the other hand, studies like Ang et al. (2021) show that acts of police violence, like the killing of John T. Williams by an SPD officer, can reduce residents' trust in the police and their willingness to make 911 calls. To complement my 911 calls analysis, I also present suggestive evidence indicating that the investigation did not influence civilian criminal behavior, crime reporting, or short-term trust or confidence in the police in Seattle. Finally, because of the significant decrease in traffic stops during the investigation, I examine fatal crashes in Seattle compared to control cities whose police departments were not subject to federal investigations.

6.4.1 Effect on Weekly 911 Calls

I begin my analysis of the effect on 911 calls with Figure 10, which shows seasonally adjusted weekly 911 calls spanning from June 2009 to December 2013. Additionally, I include seasonally adjusted OI stops, as previously presented in Figure 3, for comparison. Notably, unlike OI stops, 911 calls exhibit an overall increasing trend over time, and it is difficult to discern whether this upward trend results from the investigation. While not conclusive evidence, it is informative that 911 calls remained relatively steady during the investigation period, while OI stops were decreasing. Furthermore, in the post-investigation period, 911 call volume increased even as OI stops increased.

To formally assess whether the increases in 911 calls resulted from the investigation, I compare the growth in calls across different 911 call types. I classify 911 calls into seven distinct categories using the initial case type field: disturbance, domestic violence, suicide, suspicious activity, theft, traffic, and other.[26] I compare 911 calls in other categories to 911 calls for in-progress or recently occurred suicides or suicide attempts, a category that I hypothesize is unlikely to be affected by the activities surrounding the federal investigation or changes in civilian reporting behavior.

I estimate separate regressions for each 911 call type using the interrupted time series model outlined in Equation 5.1 and the log of weekly 911 calls. Standard errors are computed using the Newey-West method.

6.4.2 Results

Figure 11 shows the log of weekly 911 calls for the different 911 call types. Panel A includes 911 calls related to disturbance, suspicious activity, and traffic, while Panel B includes 911 calls related to theft, domestic violence, and all other matters. I include 911 calls for in-

[26]For additional information on the classification of 911 call types, please refer to Appendix B.

progress or recently occurred suicides or suicide attempts in both panels for comparison. I find no visual evidence that the different 911 call types are changing differently over my study period.

I present the results from estimating separate interrupted times series models on log weekly 911 calls for each 911 call type in Table 12. Consistent with the visual evidence in Figure 11, my estimates suggest that the 911 call volume for most call types increased at similar rates during and after the investigation. The two exceptions are 911 calls related to disturbances during the post-investigation and consent decree periods, and 911 calls related to domestic violence during the consent decree period, which experienced lower rate increases than other call types. These findings may reflect decreased willingness to call for less urgent matters after the announcement of the DOJ's findings.

6.4.3 Other Evidence on Community Responses

It is possible that the null effects I estimate on crime reflect changes in civilian reporting behavior or civilian trust in the police, although this seems unlikely in my setting given overall increases in 911 calls for all call types. In this section, I provide suggestive evidence that civilian criminal and reporting behavior did not noticeably change during the investigation by drawing on survey data from the National Crime Victimization Survey (NCVS) and Gallup's Confidence in Institutions survey. These survey data are only available annually and at coarser geographical levels than the data used elsewhere in my study; the NCVS's MSA Public-Use data only allow geographic identification at the metropolitan statistical area (MSA) level, while the Gallup data are available at the state level. Nonetheless, these are the best data available to me for assessing other changes in community behavior.

I present descriptive evidence from both NCVS and Gallup data in Figure 13. When using NCVS data, I compare the MSA encompassing Seattle with MSAs that do not include jurisdictions with federal investigations, and I compare Washington state with states that do not include jurisdictions with federal investigations when using Gallup data. Panel 1A shows the annual victimization rates for respondents to the NCVS, and Panel 1B shows the annual nonreporting rates to the police for these victimizations. I do not find visual evidence of an increase in crime victimizations or in nonreporting of victimizations to the police.

In Panel 2A of Figure 13, I present annual rates of nonreporting attributed to mistrust of the police. Here again, I do not find visual evidence that mistrust is changing differently in the Seattle MSA compared to control MSAs during the investigation period. Finally, in Panel 2B, I show annual shares of Gallup respondents reporting high confidence in the police as an institution. While confidence in the police is decreasing over the period of my analysis, I do not find visual evidence that confidence in the police changes differently in Washington

state than in control states.

6.4.4 Effect on Monthly Fatal Crashes

Lastly, I use the synthetic control method and data from the NHTSA to examine whether monthly fatal crashes in Seattle changed differently than in cities whose police departments were not subject to federal investigations. I show these results in Figure 12 and Table 13. I do not find visual evidence of significant changes in monthly fatal crashes. The synthetic control estimate affirms this; it is not statistically significant and is generally small compared to the pre-period mean of 2 fatal crashes per month. Moreover, the implied 95% confidence interval can rule out more than a 5.7-percent increase in fatal crashes.

7 Conclusion

This paper documents significant reductions in officer-initiated stops, particularly in traffic and suspicious activity stops, during a federal investigation into the Seattle Police Department. These reductions were more pronounced in minority neighborhoods and among officers with higher arrest rates among pre-period OI stops. Following the investigation's conclusion, OI stops steadily increased but remained lower than pre-period levels, particularly in minority neighborhoods. Despite the substantial decrease in OI stops, no significant increases in serious crime were detected in neighborhoods that experienced larger reductions in stops. Furthermore, I detect no differences in serious crime changes between Seattle and control jurisdictions, suggesting that the forgone policing activities had little crime reducing benefit (Cho, Gonçalves and Weisburst, 2023; Manski and Nagin, 2017). I also find no evidence of changes in civilian reporting, as measured by 911 calls, or other measures of public safety based on traffic fatalities and survey data. These findings suggest that federal oversight can reduce costly policing activities, at a given level of police presence, without significantly increasing serious crime or compromising public safety.

This study relies on the timing of a federal investigation in Seattle to estimate these effects. Therefore, the results presented here may not be generalizable to other jurisdictions subjected to federal investigations. In fact, previous studies such as Devi and Fryer Jr (2020) and Shi (2009) document crime increases in several jurisdictions following federal investigations. Several factors could contribute to these disparate findings, including the magnitude of officer pullback and the specific types of activities that were reduced in response to the investigation. My study demonstrates that a decrease in policing activity, holding police presence constant, is not necessarily linked to an increase in serious crime. A roughly 26% reduction in OI stops, primarily consisting of reductions in traffic and suspicious activity

stops, particularly in minority neighborhoods, was not associated with detectable increases in serious crimes in Seattle following a 2011 federal investigation into the SPD. Therefore, this study underscores the importance of better understanding the magnitude of reduced policing activity as well as the particular activities being reduced to evaluate its impact on crime. In addition to their importance as a policy lever to ensure constitutional policing in the United States, federal investigations also provide a useful source of variation in policing activity to explore the effectiveness of different policing activities in reducing crime. Further research is needed to document how federal investigations affect policing and communities and to assess what the changes in policing activity during federal investigations can teach us about the effectiveness of such activities in reducing serious crime.

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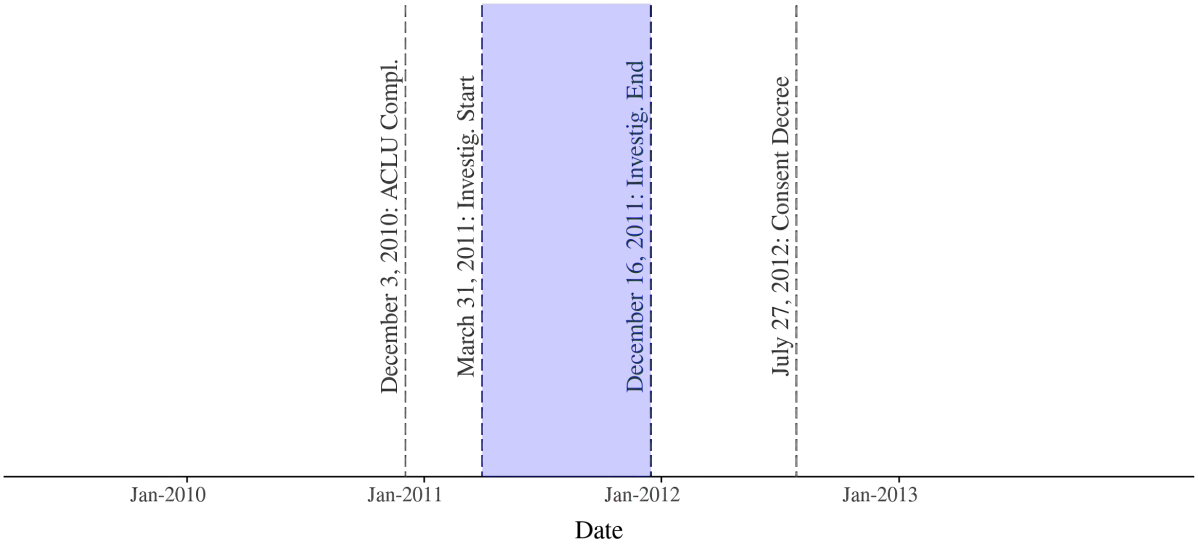
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Yardley, William. 2011. "Justice Department to Review Seattle Police's Use of Force." *New York Times*.

Zimring, Franklin E. 2011. *The City that Became Safe: New York's Lessons for Urban Crime and its Control*. Oxford University Press.

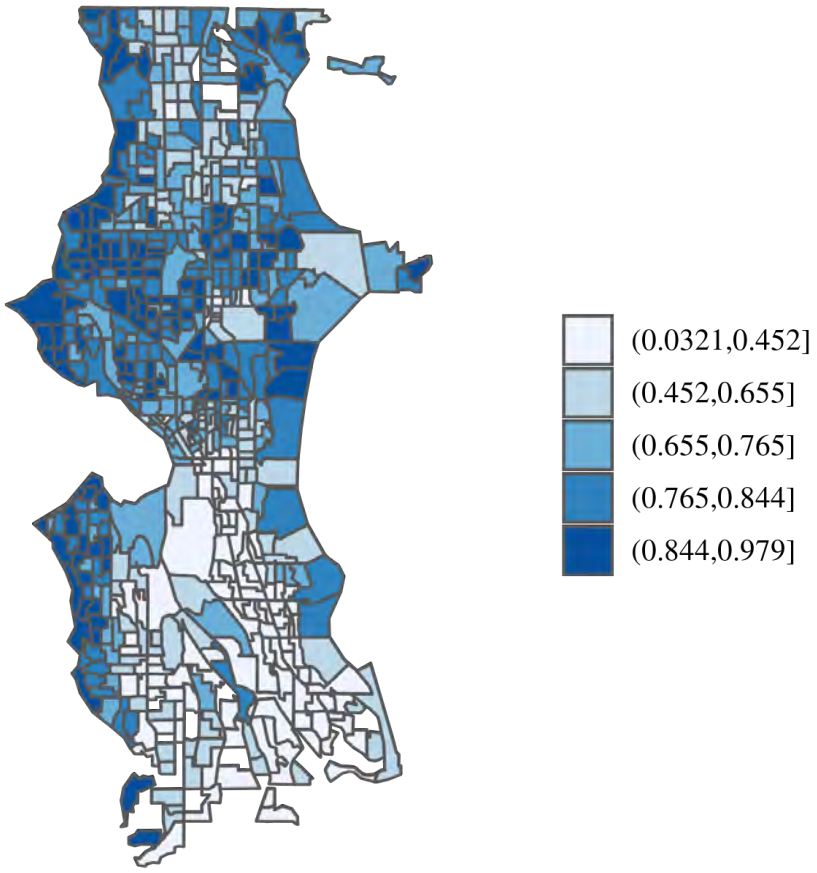
8 Figures

Figure 1: Timeline of the SPD's Federal Investigation



Notes: This figure shows the timeline for the federal investigation into the SPD. For more information, refer to Section 2.

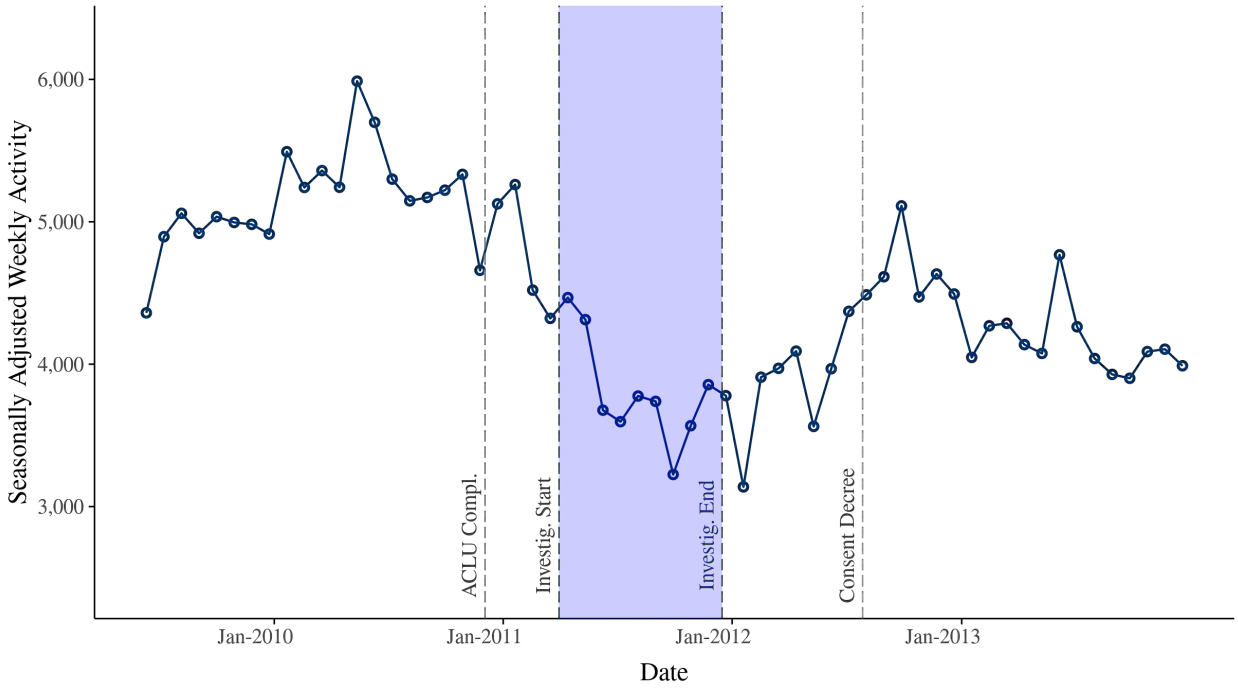
Figure 2: Neighborhood Racial Composition in Seattle



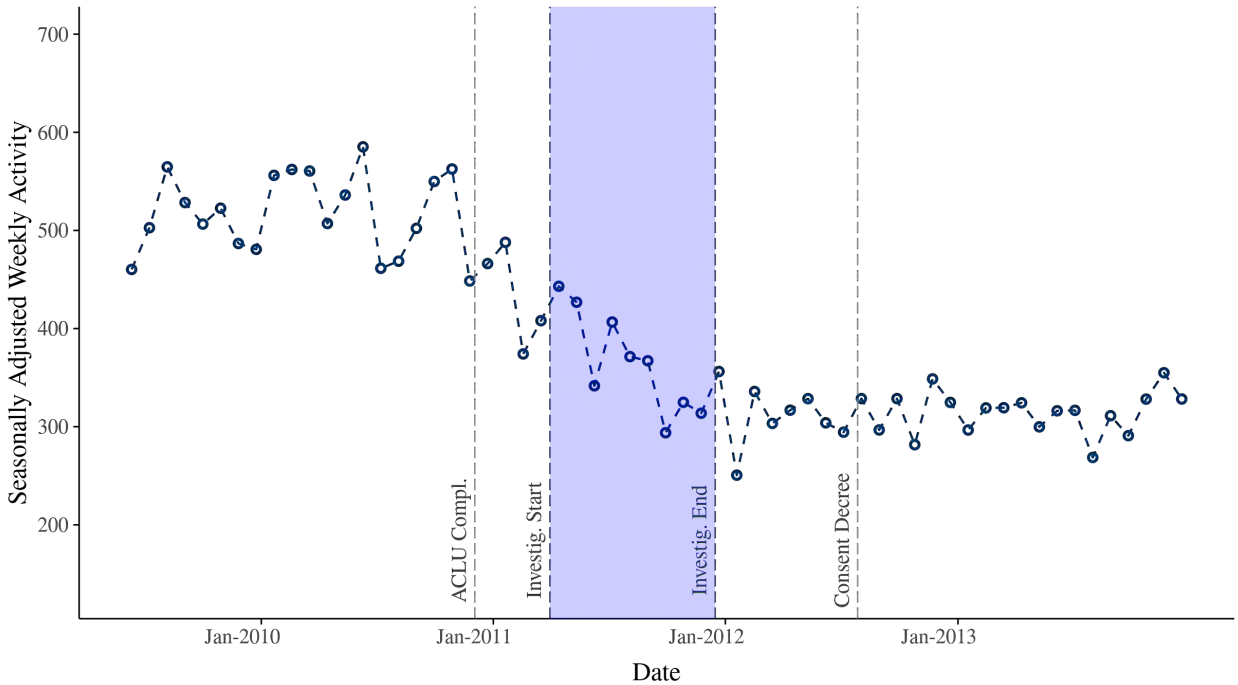
Notes: This figure plots the share of non-Hispanic White residents across neighborhoods in Seattle. Neighborhoods are defined as census block groups.

Figure 3: Weekly Officer-Initiated Activity

(A) Officer-Initiated Stops

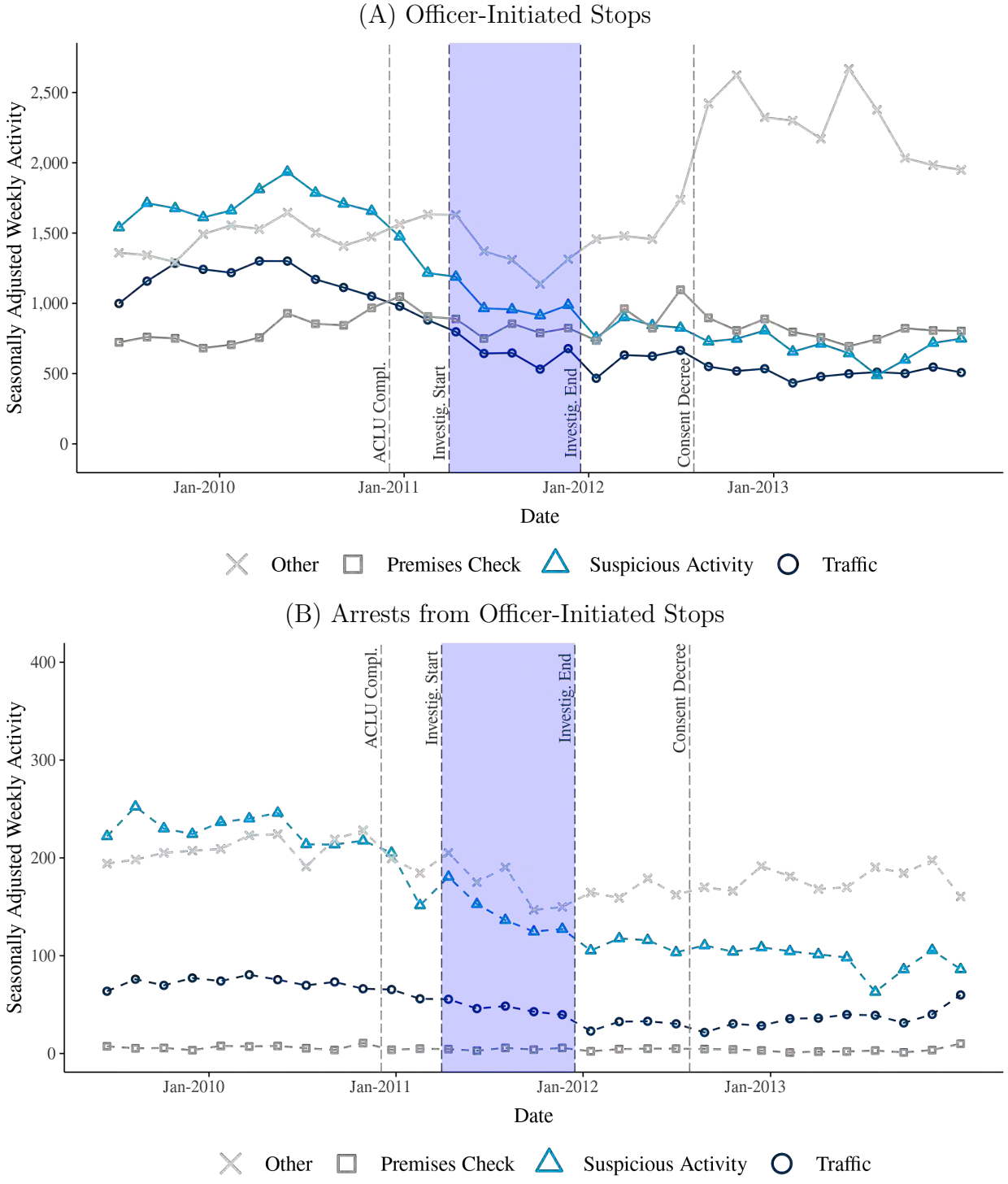


(B) Arrests from Officer-Initiated Stops



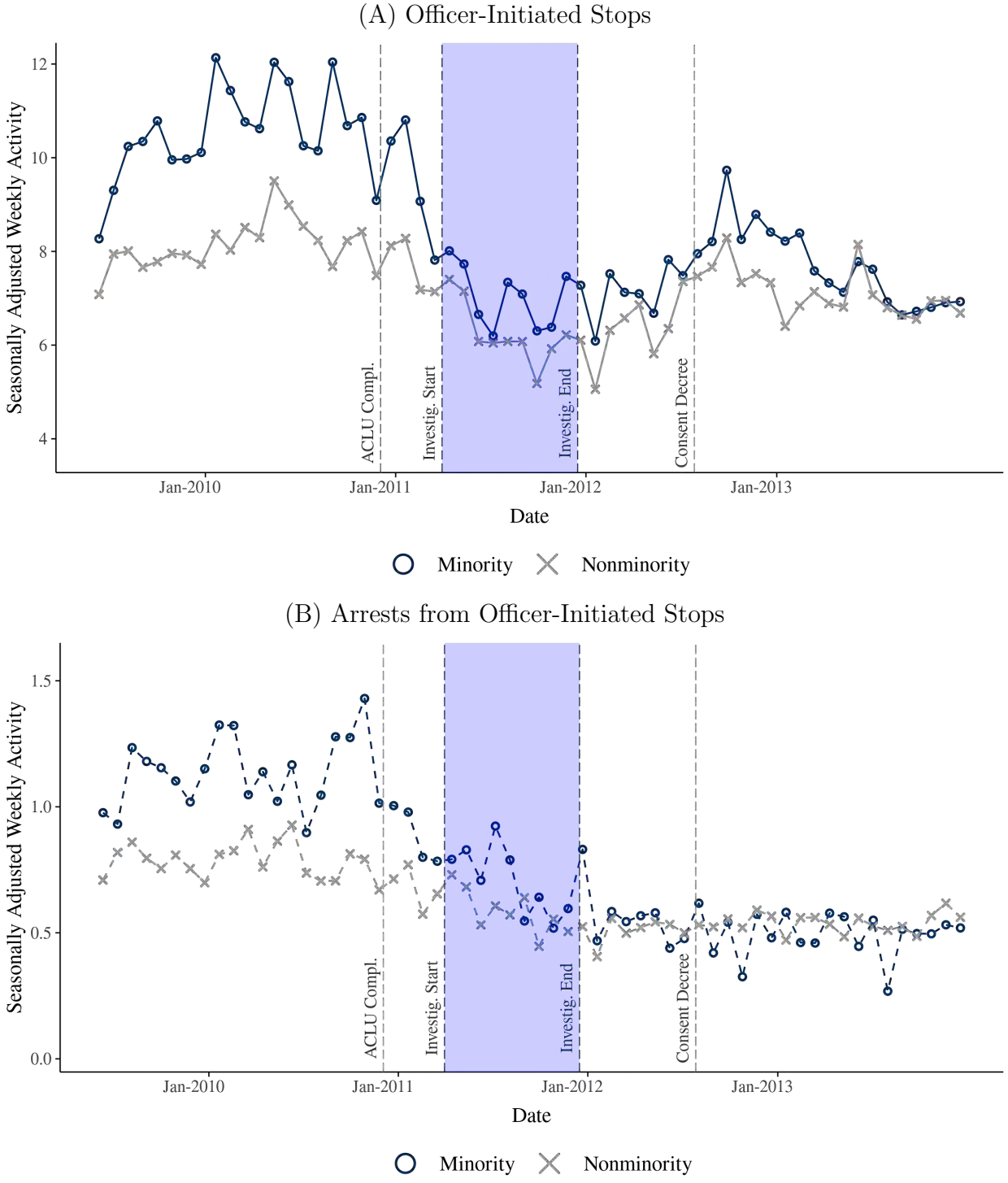
Notes: This figure plots seasonally adjusted weekly officer-initiated (OI) stops (Panel A) and arrests from OI stops (Panel B) from June 2009 to December 2013. To seasonally adjust values, I residualize by week-of-the-year fixed effects and add back the mean of the fixed effects.

Figure 4: Weekly Officer-Initiated Activity by Stop Type



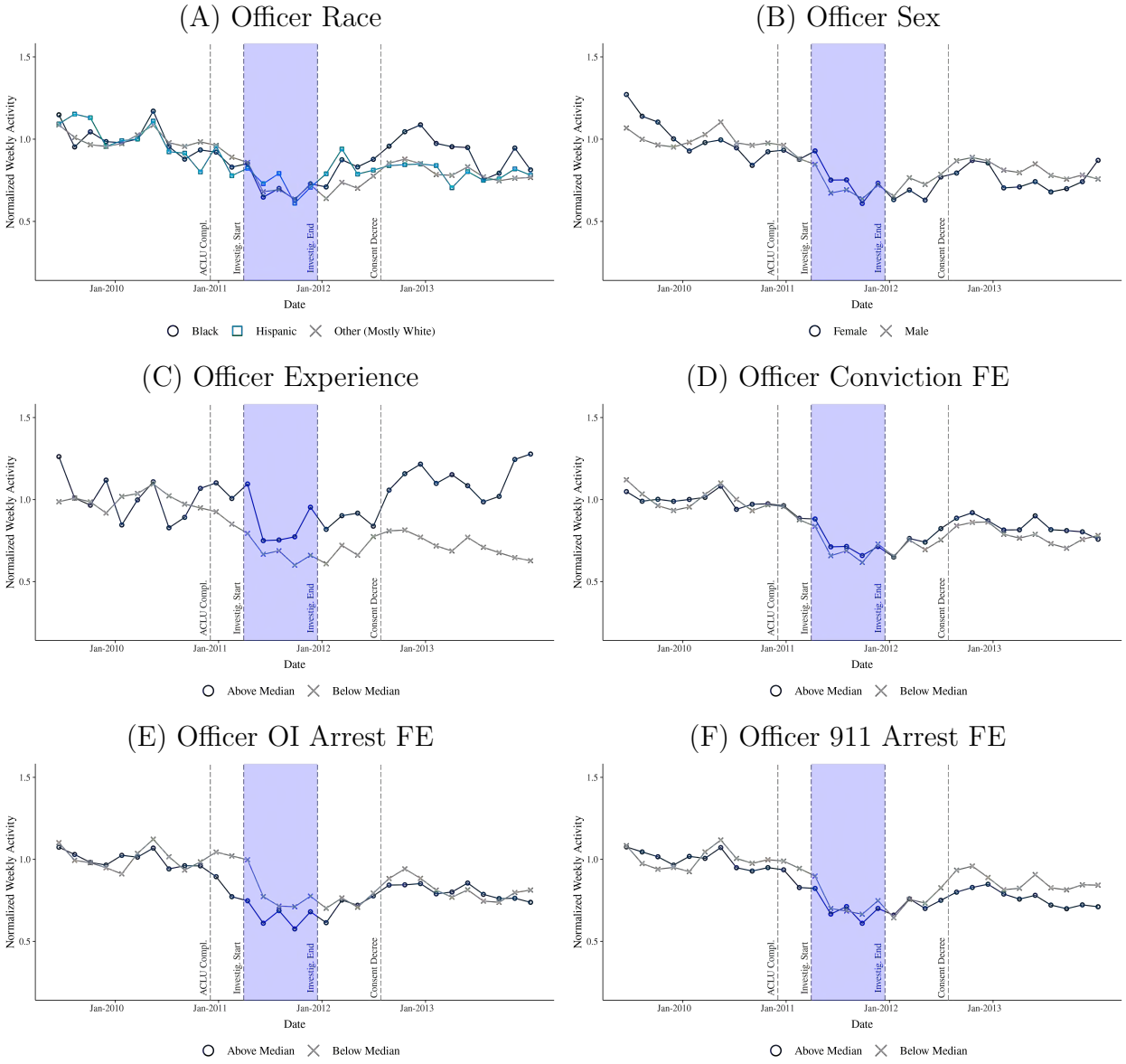
Notes: This figure plots seasonally adjusted weekly officer-initiated (OI) stops (Panel A) and arrests from OI stops (Panel B) from June 2009 to December 2013 for each OI stop type. To seasonally adjust values, I residualize by week-of-the-year fixed effects and add back the mean of the fixed effects.

Figure 5: Weekly Officer-Initiated Activity by Neighborhood Race



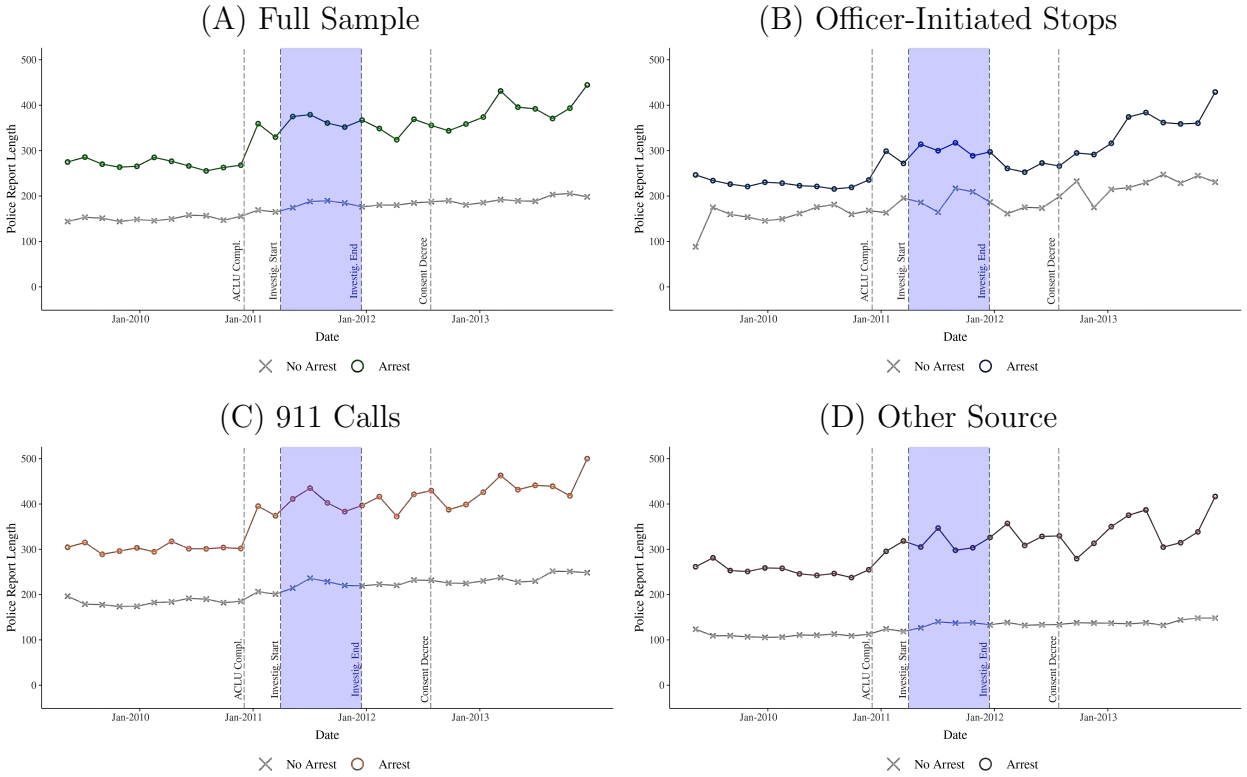
Notes: This figure plots seasonally adjusted weekly officer-initiated (OI) stops (Panel A) and arrests from OI stops (Panel B) from June 2009 to December 2013 for minority and nonminority neighborhoods. Minority neighborhoods are defined as census block groups with less than 50 percent non-Hispanic White residents. To seasonally adjust values, I residualize by week-of-the-year fixed effects and add back the mean of the fixed effects.

Figure 6: Weekly Officer-Initiated Stops by Officer Traits



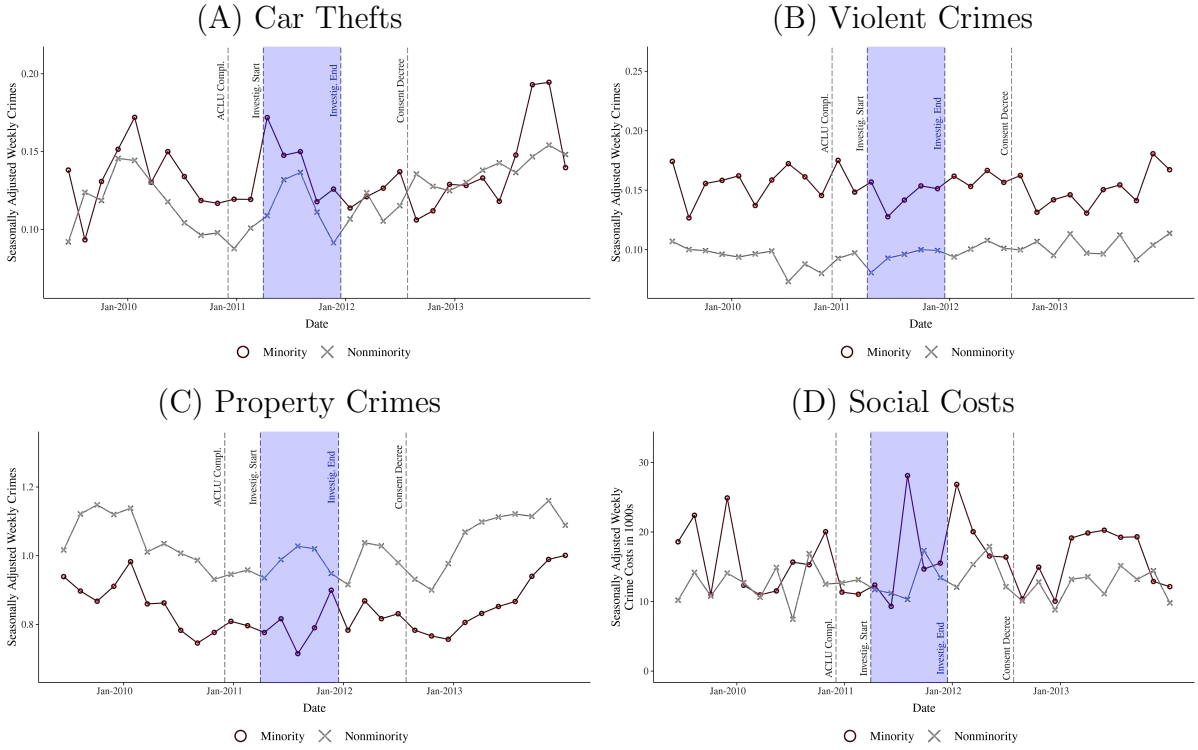
Notes: This figure plots normalized weekly officer-initiated (OI) stops from June 2009 to December 2013 for each officer trait described in Section 4. I normalize each series by its pre-period mean (e.g., the series for Black officers is divided by average weekly OI stops among Black officers in the pre-period). To seasonally adjust values, I residualize by week-of-the-year fixed effects and add back the mean of the fixed effects.

Figure 7: Police Report Length by Crime Discovery Source



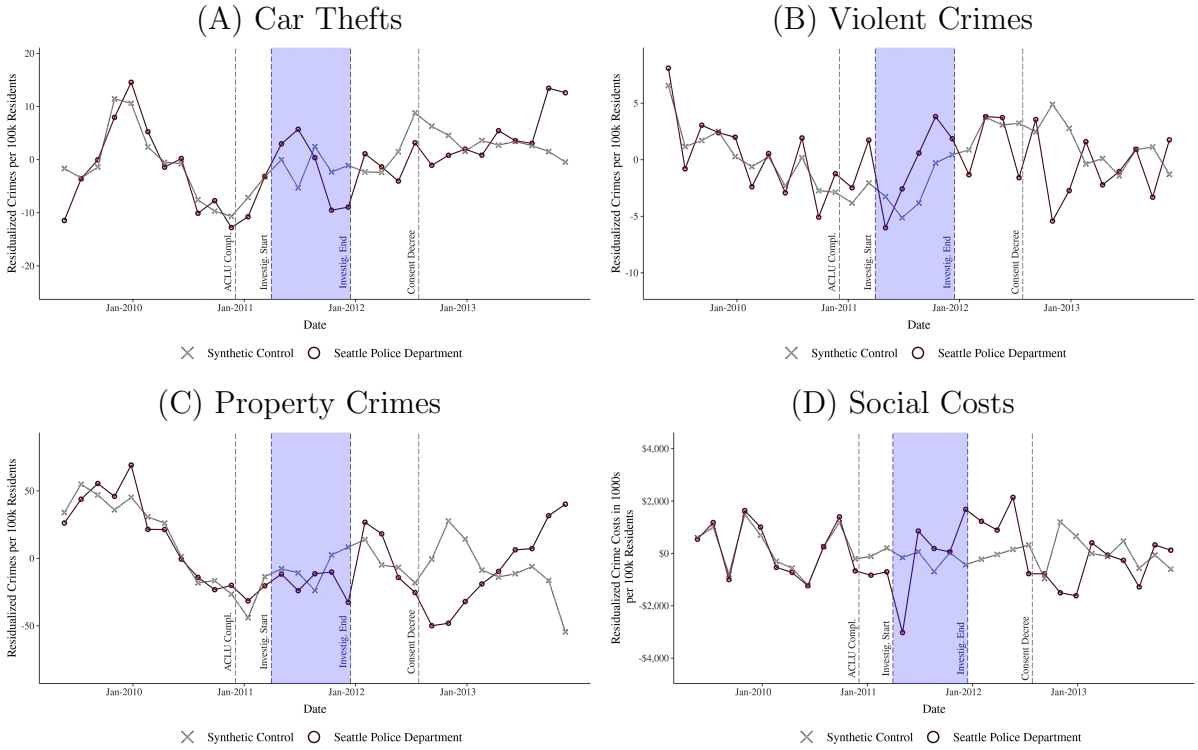
Notes: This figure plots the length, in words, of police reports from June 2009 to December 2013. Panel A shows report length for all crimes. Panel B shows report length for crimes discovered through officer-initiated (OI) stops, while Panel C and Panel D show report length for crimes discovered via 911 calls and other sources, respectively. To account for potential changes in the composition of crime types over time, I hold crime type composition fixed at pre-period levels.

Figure 8: Weekly Within-Seattle Crimes by Neighborhood Race



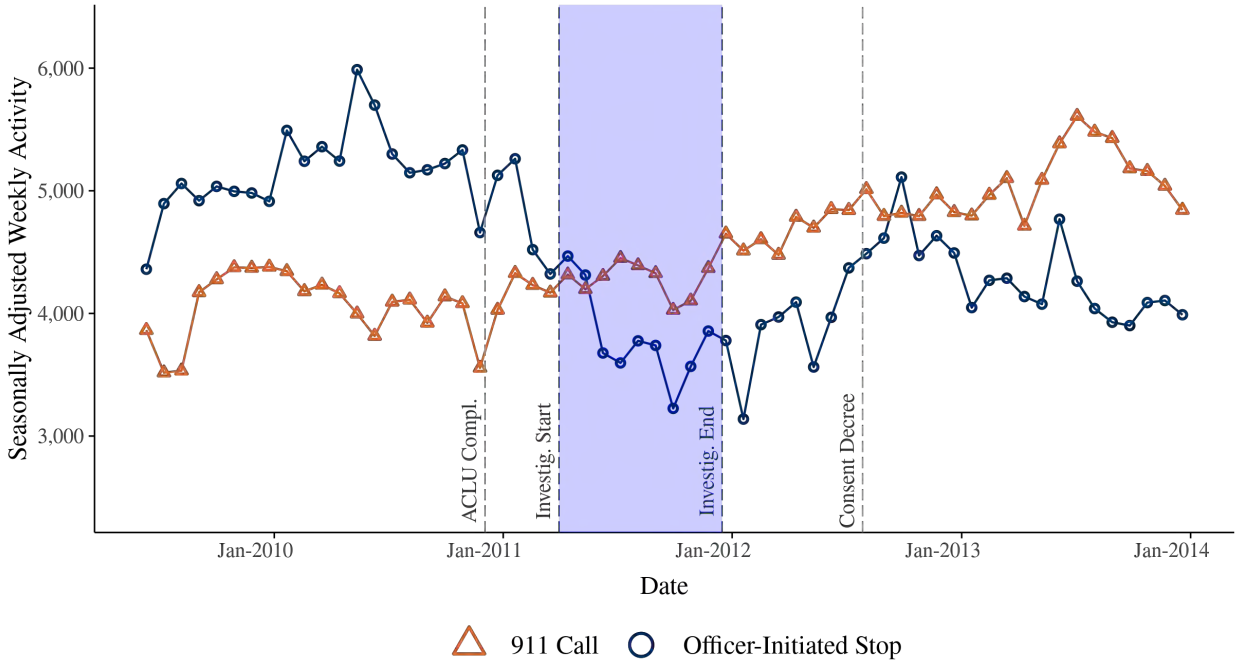
Notes: This figure plots seasonally adjusted weekly crimes for minority and nonminority neighborhoods from June 2009 to December 2013. Panel A reports car thefts, Panel B reports violent crimes, Panel C reports property crimes, and Panel D reports the social cost of index crimes in US$1,000s. To seasonally adjust values, I residualize by week-of-the-year fixed effects and add back the mean of the fixed effects.

Figure 9: Monthly Crimes per 100,000 Residents, Synthetic Control



Notes: This figure plots monthly crimes per 100,000 residents, residualized by month-of-the-year fixed effects, in Seattle compared to its synthetic control counterfactual from June 2009 to December 2013. Panel A reports car thefts, Panel B reports violent crimes, Panel C reports property crimes, and Panel D reports the social cost of index crimes in US$1,000s.

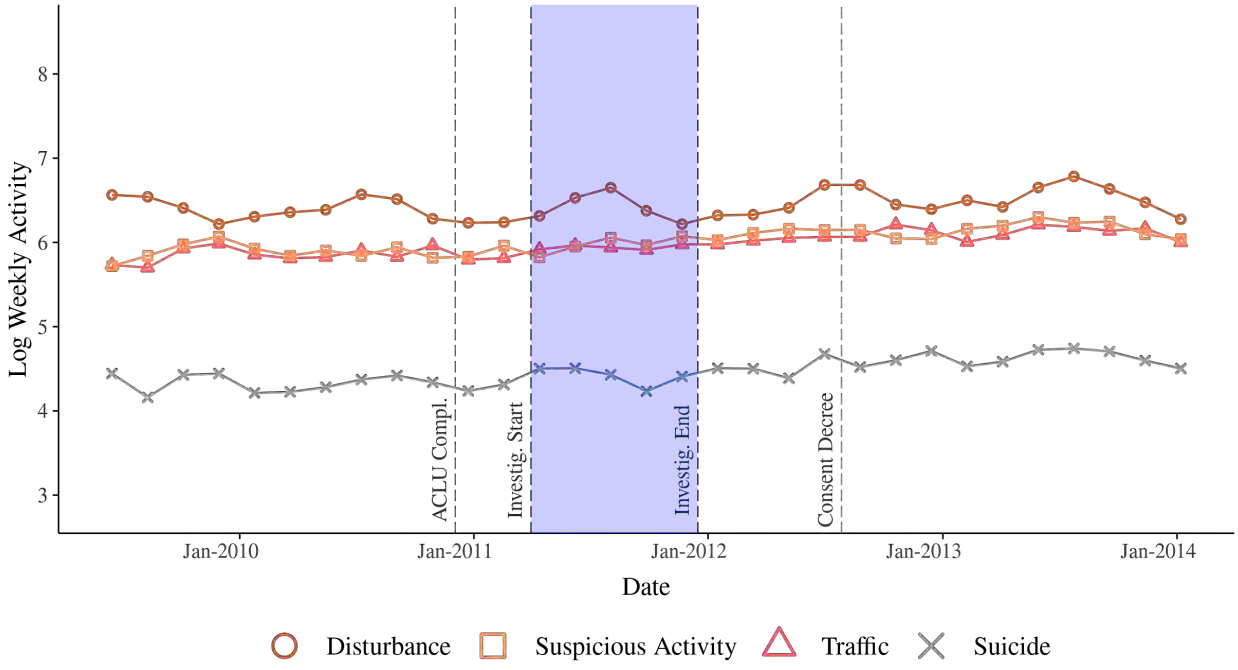
Figure 10: Weekly 911 Calls and Officer-Initiated Stops



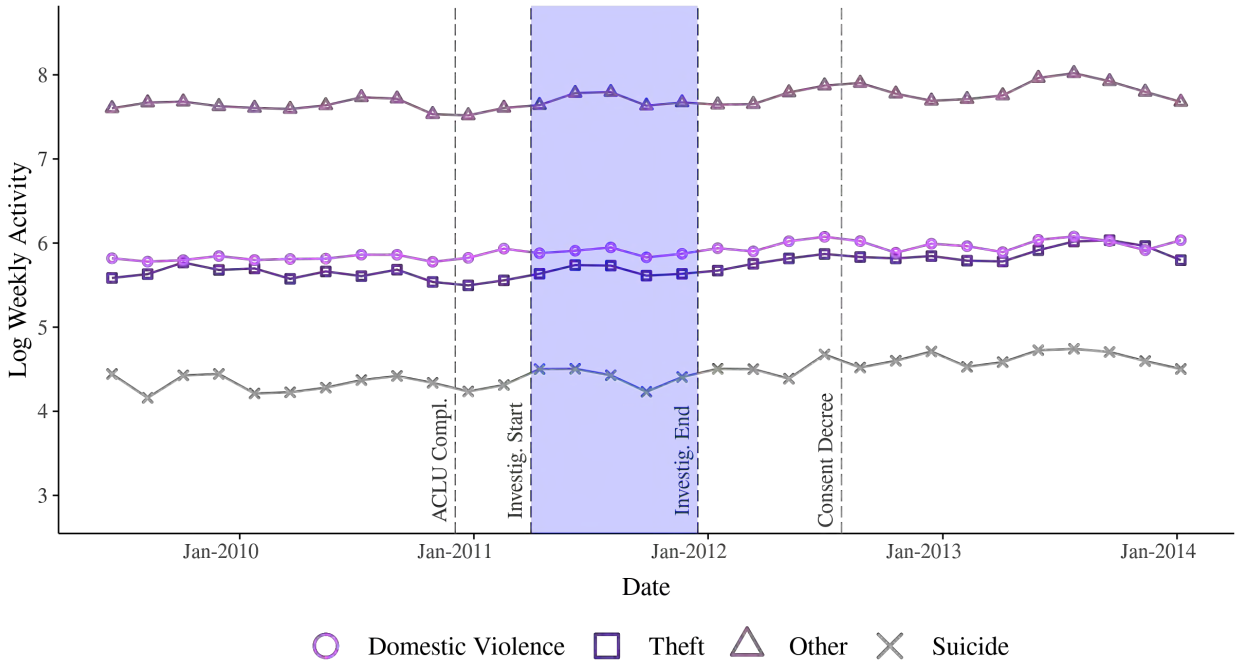
Notes: This figure plots seasonally adjusted weekly 911 calls and officer-initiated stops from June 2009 to December 2013. To seasonally adjust values, I residualize by week-of-the-year fixed effects and add back the mean of the fixed effects.

Figure 11: Log Weekly 911 Calls by Call Type

(A) Disturbance, Suspicious Activity, and Traffic vs. Suicide Calls

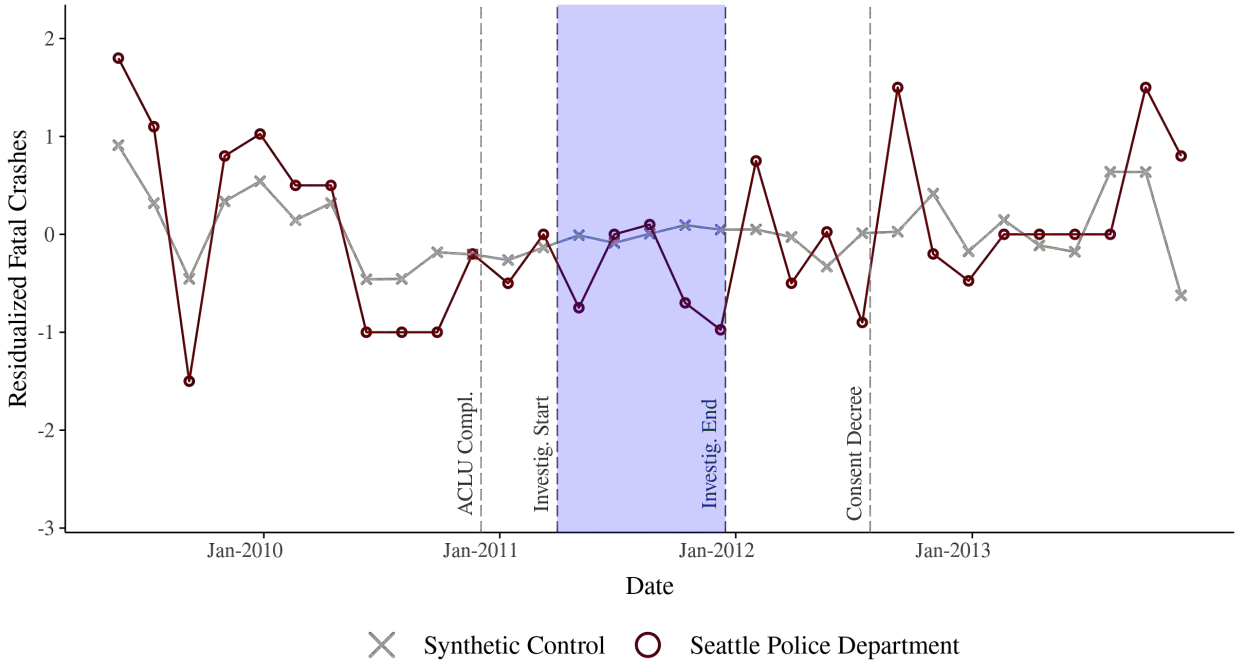


(B) Domestic Violence, Theft, and Other vs. Suicide Calls



Notes: This figure plots log weekly 911 calls from June 2009 to December 2013 for the different 911 call types. Panel A reports 911 calls for disturbance, suspicious activity, and traffic. Panel B reports 911 calls for domestic violence, theft, and other. I include 911 calls for in-progress or recently occurred suicides or suicide attempts in both panels for comparison.

Figure 12: Monthly Fatal Crashes, Synthetic Control

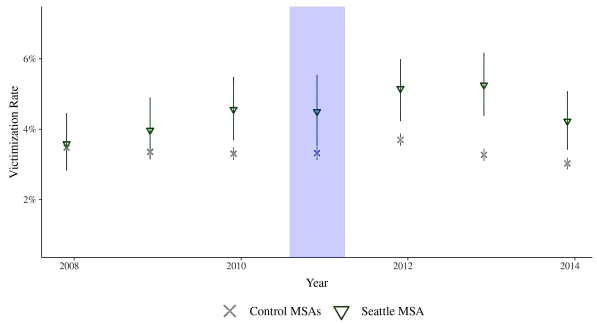


Notes: This figure plots monthly fatal crashes, residualized by month-of-the-year fixed effects, in Seattle compared to its synthetic control counterfactual from June 2009 to December 2013.

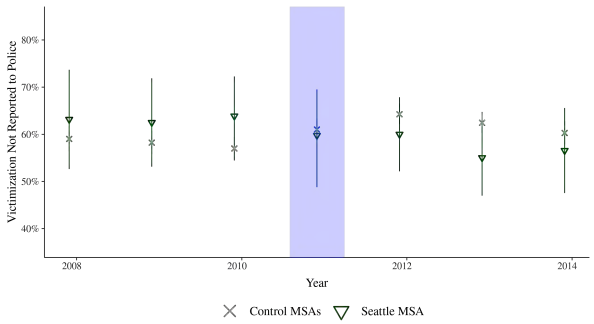
Figure 13: Other Community Responses

Panel 1: Victimization and Nonreporting over Time

(A) Civilian Victimization Rate

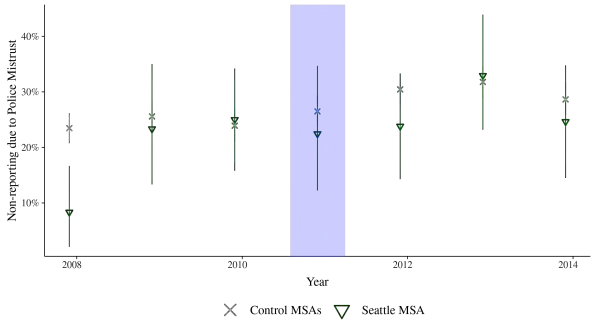


(B) Victimization Nonreporting Rate

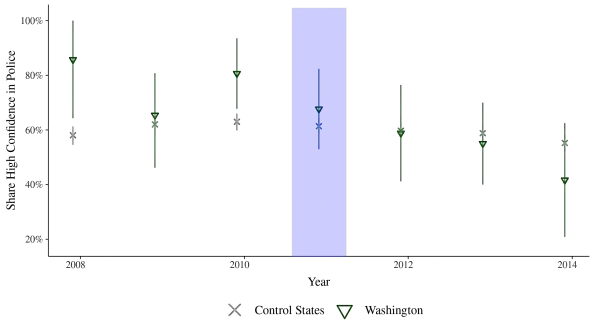


Panel 2: Perceptions of the Police over Time

(A) Nonreporting Due to Police Mistrust



(B) Confidence in Police



Notes: Panel 1A shows annual victimization rates from the National Crime Victimization Survey (NCVS). Panel 1B shows annual rates of nonreporting of victimizations to the police. Panel 2A shows annual rates of nonreporting attributed to mistrust of the police. Finally, Panel 2B shows annual shares of Gallup respondents reporting high confidence in the police as an institution. The shaded area is 2011, the year of the federal investigation into the SPD. I include 95% confidence intervals in each plot. The NCVS data (Panels 1A, 1B, and 2A) compare the metropolitan statistical area (MSA) encompassing Seattle with MSAs that do not include jurisdictions with police departments subject to federal investigations, while the Gallup data (Panel 2B) compare Washington state with states that do not include jurisdictions with police departments subject to federal investigations.

9 Tables

Table 1: Summary Statistics

		Mean	Std Dev	N
A. Weekly Dispatch Characteristics				
Dispatch Source	OI Stops	4,502.54	820.23	239
	911 Calls	4,507.44	622.91	239
	Other	2,758.14	540.59	239
OI Stop Type	Premises Check	841.18	169.42	239
	Suspicious Activity	1,135.00	489.96	239
	Traffic	787.38	324.63	239
	Other	1,738.97	510.83	239
Arrests per 1000 OI Stops		87.32	16.53	239
B. Weekly Reported Crime Characteristics				
Crime Type	Car Theft	73.88	14.93	239
	Property	581.16	62.88	239
	Violent	65.89	11.15	239
	Non-index	648.50	64.81	239
Social Cost in US$1,000s		8,169.67	3,462.49	239
Report Length		187.29	23.66	239
C. Officer Characteristics				
Black		0.08		1,098
Hispanic		0.05		1,098
White		0.70		1,098
Other Race		0.16		1,098
Female		0.13		1,098
Experience in 2009		13.85	9.01	1,098
D. Neighborhood (CBG) Characteristics				
Nonminority	Share Non-Hispanic Asian	0.09	0.07	448
	Share Non-Hispanic White	0.76	0.12	448
	Share Non-Hispanic Black	0.04	0.06	448
	Share Hispanic	0.06	0.05	448
Minority	Share Non-Hispanic Asian	0.29	0.18	144
	Share Non-Hispanic White	0.31	0.13	144
	Share Non-Hispanic Black	0.18	0.15	144
	Share Hispanic	0.14	0.14	144

Notes: Summary statistics are based on data from June 2009 to December 2013. Social costs represent the social cost of index crimes in US$1,000s calculated using cost estimates from Bhatt et al. (2023) deflated to 2009 dollars. Minority neighborhoods are census block groups with less than 50 percent non-Hispanic White residents.

Table 2: Effect on Weekly Officer-Initiated Activity

	OI Stops	OI Arrests	OI Arrest Rate
	(1)	(2)	(3)
Complaint	-334.54	-94.68***	-12.23***
	(209.49)	(22.24)	(2.55)
Investigation	-1,373.81***	-154.01***	-5.59**
	(117.66)	(15.15)	(2.83)
Post-Investigation	-1,469.30***	-221.78***	-18.92***
	(146.92)	(13.09)	(2.60)
Consent Decree	-857.94***	-208.18***	-27.88***
	(99.32)	(9.30)	(1.83)
Pre-period mean	5,227.13	522.01	100.00
Observations	239	239	239
Adjusted R^2	0.64	0.70	0.52
Week-of-Year FEs	X	X	X

Notes: This table reports the results of estimating Equation 5.1 on weekly time series spanning June 2009 to December 2013. The unit of observation is a calendar week. Column 1 reports the estimates for the weekly officer-initiated (OI) stops, Column 2 reports for weekly arrests from OI stops, and Column 3 reports for the weekly OI arrest rate, which I define as the number of arrests per 1,000 OI stops. Newey-West standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Effect on Weekly Officer-Initiated Activity by Stop Type

Panel A: Traffic and Suspicious Activity Stops

	Traffic			Suspicious Activity		
	Stops	Arrests	Arrest Rate	Stops	Arrests	Arrest Rate
	(1)	(2)	(3)	(4)	(5)	(6)
Complaint	-274.33***	-13.79***	0.01	-405.46***	-57.91***	-2.57
	(47.21)	(3.37)	(4.48)	(78.70)	(14.46)	(6.52)
Investigation	-545.68***	-26.66***	9.79**	-731.12***	-87.46***	8.40*
	(30.15)	(2.79)	(4.07)	(33.10)	(7.33)	(4.68)
Post-Investigation	-604.87***	-47.36***	-18.06***	-902.35***	-123.77***	-4.31
	(44.01)	(2.81)	(3.42)	(48.79)	(7.30)	(4.39)
Consent Decree	-681.71***	-38.86***	5.84	-1,044.98***	-134.16***	7.22**
	(25.29)	(2.43)	(3.86)	(32.28)	(5.86)	(3.51)
Pre-period mean	1,181.01	71.74	61.02	1,726.69	230.86	133.97
Observations	239	239	239	239	239	239

Panel B: Premises Check and Other Stops

	Premises Check			Other		
	Stops	Arrests	Arrest Rate	Stops	Arrests	Arrest Rate
	(1)	(2)	(3)	(4)	(5)	(6)
Complaint	218.51***	-2.00*	-4.02***	126.75**	-20.97**	-19.94***
	(62.12)	(1.05)	(1.40)	(55.73)	(9.87)	(5.05)
Investigation	17.62	-2.00*	-2.45**	-114.63**	-37.90***	-17.26***
	(35.40)	(1.03)	(1.10)	(45.42)	(9.29)	(5.03)
Post-Investigation	107.71*	-2.92***	-4.06***	-69.78	-47.72***	-24.00***
	(55.79)	(0.82)	(0.97)	(58.23)	(7.23)	(4.63)
Consent Decree	19.74	-3.31***	-4.02***	849.01***	-31.86***	-62.64***
	(31.64)	(0.70)	(0.81)	(65.94)	(6.07)	(3.66)
Pre-period mean	810.10	6.44	7.82	1,509.32	212.97	140.88
Observations	239	239	239	239	239	239

Notes: This table reports the results of estimating Equation 5.1 on weekly time series spanning June 2009 to December 2013 separately for each stop type. The unit of observation is a calendar week. Panel A reports the estimates traffic and suspicious activity stops, while Panel B reports the estimates for premises check and other stops. For each stop category, I report estimates for weekly officer-initiated (OI) stops, weekly arrests from OI stops, and the weekly OI arrest rate, which I define as the number of arrests per 1,000 OI stops. All regressions include week-of-the-year fixed effects to adjust for seasonality. Newey-West standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Effect on Weekly Officer-Initiated Stops by Neighborhood Race

	OI Stops	OI Arrests	OI Arrest Rate
	(1)	(2)	(3)
Minority × Complaint	-0.56	-0.09	5.18
	(0.36)	(0.07)	(4.98)
Minority × Investigation	-1.61***	-0.22***	9.48**
	(0.26)	(0.05)	(4.11)
Minority × Post-Investigation	-1.58***	-0.32***	1.58
	(0.29)	(0.05)	(4.24)
Minority × Consent Decree	-1.87***	-0.39***	-5.67*
	(0.26)	(0.04)	(3.10)
Complaint	-0.43***	-0.14***	-7.18***
	(0.16)	(0.03)	(2.53)
Investigation	-1.93***	-0.21***	-4.47**
	(0.12)	(0.02)	(1.79)
Post-Investigation	-2.10***	-0.30***	-15.38***
	(0.17)	(0.03)	(2.05)
Consent Decree	-0.00***	-0.26***	-9.37***
	(0.14)	(0.02)	(1.52)
Pre-period mean	8.83	0.88	57.98
Observations	141,488	141,488	87,991
Adjusted R^2	0.82	0.57	0.05
Neighborhood FEs	X	X	X
Week-of-Year FEs	X	X	X

Notes: This table reports the results of estimating Equation 5.2 on a balanced neighborhood weekly panel spanning June 2009 to December 2013. The unit of observation is a neighbor calendar week. Column 1 reports the estimates for weekly officer-initiated (OI) stops, Column 2 reports for weekly arrests from OI stops, and Column 3 reports for the weekly OI arrest rate, which I define as the number of arrests per 1,000 OI stops. Newey-West standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Testing for Equally Proportional Effect on Weekly Officer-Initiated Activity by Neighborhood Race

	OI Stops	OI Arrests	OI Arrest Rate
	(1)	(2)	(3)
Minority × Complaint	-0.49	-0.07	13.14***
	(0.37)	(0.07)	(5.05)
Minority × Investigation	-1.15***	-0.15**	16.40***
	(0.26)	(0.06)	(4.18)
Minority × Post-Investigation	-1.12***	-0.20***	10.22**
	(0.31)	(0.05)	(4.28)
Minority × Consent Decree	-2.35***	-0.30***	3.35
	(0.30)	(0.05)	(3.13)
Investigation	-0.36*	-0.03	20.48***
	(0.19)	(0.03)	(2.54)
Complaint	-0.19	-0.09**	20.51***
	(0.23)	(0.04)	(3.52)
Post-Investigation	-0.54**	-0.03	13.87***
	(0.27)	(0.03)	(2.80)
Consent Decree	-2.64***	-0.06**	22.76***
	(0.25)	(0.03)	(2.29)
Pre-period mean × Complaint	-0.03	-0.07	-0.52***
	(0.03)	(0.06)	(0.05)
Pre-period mean × Investigation	-0.19***	-0.22***	-0.46***
	(0.03)	(0.06)	(0.04)
Pre-period mean × Post-Investigation	-0.19***	-0.34***	-0.54***
	(0.04)	(0.04)	(0.04)
Pre-period mean × Consent Decree	0.20***	-0.25***	-0.59***
	(0.04)	(0.04)	(0.04)
Pre-period mean	8.83	0.88	57.98
Observations	141,488	141,488	87,715
Adjusted R^2	0.83	0.58	0.05
Neighborhood FEs	X	X	X
Week-of-Year FEs	X	X	X

Notes: This table reports the results of estimating Equation 5.2 on a balanced neighborhood weekly panel spanning June 2009 to December 2013. The unit of observation is a neighbor calendar week. Column 1 reports the estimates for weekly officer-initiated (OI) stops, Column 2 reports for weekly arrests from OI stops, and Column 3 reports for the weekly OI arrest rate, which I define as the number of arrests per 1,000 OI stops. Newey-West standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Effect on Weekly Officer-Initiated Stops by Officer Traits

	Estimates for Different Officer Traits						
	Black vs. Other	Hispanic vs. Other	Female vs. Male	High vs. Low Experience	High vs. Low Conviction FE	High vs. Low OI Arrest FE	High vs. Low 911 Arrest FE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Officer Trait × Complaint	0.10	-0.44	0.07	0.00***	-0.12	-0.99***	-0.53***
	(0.16)	(0.27)	(0.14)	(0.11)	(0.11)	(0.11)	(0.12)
Officer Trait × Investigation	0.54***	-0.12	0.48***	2.15***	-0.13	-0.92***	-0.64***
	(0.12)	(0.22)	(0.11)	(0.08)	(0.08)	(0.08)	(0.08)
Officer Trait × Post-Investigation	0.92***	0.57**	-0.09	2.30***	-0.17*	-0.56***	-0.63***
	(0.13)	(0.28)	(0.12)	(0.09)	(0.09)	(0.09)	(0.09)
Officer Trait × Consent Decree	1.01***	-0.27	-0.06	2.23***	0.09	-0.21***	-0.84***
	(0.11)	(0.18)	(0.10)	(0.07)	(0.07)	(0.07)	(0.07)
Pre-period mean	3.74	5.38	4.21	2.17	5.27	5.60	6.11
Observations	258,351	258,351	258,351	258,351	258,351	258,351	258,351
Adjusted R^2	0.48	0.48	0.48	0.49	0.48	0.48	0.48
Officer FEs	X	X	X	X	X	X	X
Home-Sector-Calendar-Week FEs	X	X	X	X	X	X	X

Notes: This table reports the results of estimating Equation 5.3 on an unbalanced officer weekly panel spanning June 2009 to December 2013 separately for each officer trait described in Section 4. The unit of observation is an officer calendar week, and the outcome variable is weekly officer-initiated (OI) stops. Columns 1 and 2 presents the results for race. Column 3 reports for sex, Column 4 for experience, Column 5 for conviction fixed effects, Column 6 for OI arrest fixed effects, and Column 7 for 911 arrest fixed effects. Newey-West standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Testing for Equally Proportional Effect on Weekly Officer-Initiated Stops by Officer Traits

	Estimates for Different Officer Traits						
	Black vs. Other	Hispanic vs. Other	Female vs. Male	High vs. Low Experience	High vs. Low Conviction FE	High vs. Low OI Arrest FE	High vs. Low 911 Arrest FE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Officer Trait × Complaint	-0.12	-0.40	-0.09	0.09	-0.04	-0.83***	-0.20*
	(0.15)	(0.27)	(0.14)	(0.14)	(0.11)	(0.11)	(0.11)
Officer Trait × Investigation	0.04	-0.04	0.12	0.09	0.08	-0.52***	0.12
	(0.11)	(0.21)	(0.11)	(0.10)	(0.08)	(0.08)	(0.08)
Officer Trait × Post-Investigation	0.37***	0.66**	-0.48***	0.03	0.06	-0.11	0.21**
	(0.13)	(0.28)	(0.11)	(0.11)	(0.09)	(0.09)	(0.09)
Officer Trait × Consent Decree	0.54***	-0.20	-0.41***	0.33***	0.29***	0.17**	-0.12*
	(0.10)	(0.18)	(0.09)	(0.09)	(0.07)	(0.07)	(0.07)
Pre-period mean × Complaint	-0.17***	-0.17***	-0.17***	-0.16***	-0.17***	-0.16***	-0.16***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Pre-period mean × Investigation	-0.37***	-0.37***	-0.37***	-0.37***	-0.37***	-0.37***	-0.37***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Pre-period mean × Post-Investigation	-0.40***	-0.40***	-0.41***	-0.40***	-0.40***	-0.40***	-0.41***
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)
Pre-period mean × Consent Decree	-0.35***	-0.35***	-0.36***	-0.34***	-0.36***	-0.36***	-0.35***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Pre-period mean	3.74	5.38	4.21	2.17	5.27	5.60	6.11
Observations	258,351	258,351	258,351	258,351	258,351	258,351	258,351
Adjusted R^2	0.50	0.50	0.50	0.50	0.50	0.50	0.50
Officer FEs	X	X	X	X	X	X	X
Home-Sector-Calendar-Week FEs	X	X	X	X	X	X	X

Notes: This table reports the results of estimating Equation 5.3 on an unbalanced officer weekly panel spanning June 2009 to December 2013 separately for each officer trait described in Section 4. I additionally include controls for each officer's average weekly stops during the pre-period. The unit of observation is an officer calendar week, and the outcome variable is weekly officer-initiated (OI) stops. Column 1 and 2 report the results for officer race. Column 3 reports for officer sex, Column 4 for officer experience, Column 5 for officer conviction fixed effects, Column 6 for officer OI arrest fixed effects, and Column 7 for 911 arrest fixed effects. Newey-West standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Effect on Police Report Length

	Report Length			
	Full Sample	911 Calls	OI Stops	Other Source
	(1)	(2)	(3)	(4)
Arrest	113.24***	113.89***	74.89***	124.05***
	(3.91)	(3.96)	(7.50)	(6.43)
Arrest × Complaint	41.56***	44.55***	36.11***	41.99***
	(5.83)	(7.26)	(8.79)	(13.89)
Arrest × Investigation	46.05***	57.29***	33.27***	39.32***
	(4.54)	(6.21)	(8.41)	(8.48)
Arrest × Post-Investigation	38.75***	52.22***	16.43**	46.21***
	(4.28)	(5.76)	(7.46)	(10.16)
Arrest × Consent Decree	77.12***	81.91***	61.44***	60.88***
	(8.99)	(6.96)	(17.82)	(10.13)
Complaint	15.32***	22.20***	13.19*	11.06***
	(1.51)	(2.60)	(6.86)	(1.65)
Investigation	30.29***	38.86***	27.37***	24.17***
	(1.77)	(2.53)	(6.45)	(1.68)
Post-Investigation	33.84***	42.31***	24.57***	26.39***
	(1.38)	(2.10)	(6.58)	(1.52)
Consent Decree	44.23***	54.83***	46.57***	29.00***
	(1.54)	(1.60)	(6.32)	(1.54)
Pre-period mean	157.76	202.72	194.92	117.62
Observations	327,164	121,810	46,617	158,737
Adjusted R^2	0.26	0.26	0.09	0.28
Month-of-Year FEs	X	X	X	X
Beat FEs	X	X	X	X
Shift FEs	X	X	X	X
NIBRS Code FEs	X	X	X	X

Notes: This table reports the results of estimating Equation 5.4 on report-level data spanning June 2009 to December 2013. The unit of observation is a report, and the outcome variable is the report length in words. Column 1 presents results for the full sample, Column 2 presents results for crimes discovered via 911 calls, Column 3 for crimes discovered through officer-initiated stops, and Column 4 for crimes discovered through other sources. Standard errors clustered at the beat level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: Effect on Weekly Crimes by Neighborhood Race

	Car Thefts	Violent	Property	Social Costs
	(1)	(2)	(3)	(4)
Minority × Complaint	0.010	0.008	0.032	-5.152**
	(0.009)	(0.009)	(0.031)	(2.470)
Minority × Investigation	0.011	-0.011	0.009	-0.320
	(0.007)	(0.007)	(0.025)	(3.085)
Minority × Post-Investigation	-0.006	-0.001	0.012	0.489
	(0.008)	(0.008)	(0.025)	(4.127)
Minority × Consent Decree	-0.013**	-0.015**	-0.013	0.110
	(0.006)	(0.006)	(0.020)	(2.486)
Complaint	-0.029***	0.002	-0.117***	0.389
	(0.005)	(0.004)	(0.018)	(1.966)
Investigation	0.001	0.001	-0.065***	0.241
	(0.004)	(0.003)	(0.013)	(1.399)
Post-Investigation	-0.010**	0.010***	-0.070***	3.211*
	(0.004)	(0.004)	(0.015)	(1.758)
Consent Decree	0.019***	0.009***	-0.000	-0.239
	(0.003)	(0.003)	(0.011)	(1.014)
Pre-period mean	0.12	0.11	1.01	13.61
Observations	141,727	141,727	141,727	141,727
Adjusted R^2	0.09	0.28	0.64	0.02
Neighborhood FEs	X	X	X	X
Week-of-Year FEs	X	X	X	X

Notes: This table reports the results of estimating Equation 5.2 on a balanced neighborhood weekly panel spanning June 2009 to December 2013. The unit of observation is a neighbor calendar week. Minority neighborhoods are census block groups with less than 50 percent non-Hispanic White residents. Column 1 reports the results for weekly car thefts, Column 2 for weekly violent crimes, Column 3 for weekly property crimes (excluding car thefts), and Column 4 for weekly social cost of index crimes in US$1,000s. The social cost of index crimes are calculated using cost estimates from Bhatt et al. (2023) deflated to 2009 dollars. Newey-West standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: Implied Weekly Crimes per 1,000 Officer-Initiated Stops Averted

	Car Thefts	Violent	Property	Social Costs
Minority x Complaint	18.34	13.83	57.14	-9164.79
	[-28.28, 326.86]	[-26.98, 325.38]	[-108.58, 1,061.10]	[-168,057.11, -1,282.61]
Minority x Investigation	7.02	-7.06	5.83	-199.34
	[-5.92, 48.80]	[-40.80, 3.62]	[-83.66, 147.00]	[-7,371.83, 6,591.08]
Minority x Post-Investigation	-4.05	-0.5	8.02	306.49
	[-34.84, 8.80]	[-19.70, 20.20]	[-84.08, 114.31]	[-7,838.11, 12,595.64]
Minority x Consent Decree	-6.83	-8.09	-7.14	59.14
	[-69.92, 0.64]	[-60.41, 1.26]	[-74.37, 89.43]	[-3,611.39, 7,675.52]
Minority x Post	-2.18	-5.76	0.84	-297.85
	[-19.54, 5.49]	[-25.03, 3.45]	[-57.46, 109.61]	[-4,114.10, 4,493.76]

Notes: This table reports the implied estimates for crimes per 1,000 officer-initiated stops averted in minority neighborhoods based on the estimates in Tables 4 and 9. Minority neighborhoods are census block groups with less than 50 percent non-Hispanic White residents. Social costs represent the social cost of index crimes in US$1,000s calculated using cost estimates from Bhatt et al. (2023) deflated to 2009 dollars. In square brackets, I report the 95% credible interval, which I construct by performing Bayesian bootstrapping across neighborhoods with 1,000 replications (Rubin, 1981). I also report estimates for all time periods after the ACLU complaint combined in "Post."

Table 11: Synthetic Control Estimates for the Effect on Monthly Crimes per 100,000 Residents

	Car Theft	Violent	Property	Social Costs
	(1)	(2)	(3)	(4)
Seattle x Post	-0.05	0.14	0.16	-5.64
	(0.55)	(0.63)	(1.71)	(275.53)
Pre-period mean	49.41	50.45	430.33	5,910.07

Notes: This table reports the estimates for monthly crimes per 100,000 residents in Seattle compared to its synthetic control counterfactual. Standard errors calculated using the placebo method with 500 replications are reported in parentheses.

Table 12: Effect on Log Weekly 911 Calls by Call Type

	Ln(Suicide)	Ln(Disturbance)	Ln(Traffic)	Ln(Suspicious Activity)	Ln(Domestic Violence)	Ln(Theft)	Ln(Other)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Complaint	-0.01	-0.05*	-0.00	-0.00	0.09*	-0.07**	0.02
	(0.05)	(0.03)	(0.03)	(0.05)	(0.05)	(0.03)	(0.03)
Investigation	0.09*	0.01	0.08***	0.09**	0.07***	0.02	0.06***
	(0.05)	(0.02)	(0.03)	(0.04)	(0.02)	(0.03)	(0.02)
Post-Investigation	0.16***	0.03*	0.20***	0.23***	0.16***	0.16***	0.13***
	(0.05)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.02)
Consent Decree	0.31***	0.16***	0.28***	0.27***	0.17***	0.25***	0.21***
	(0.04)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)
Observations	239	239	239	239	239	239	239
Week-of-Year FEs	X	X	X	X	X	X	X

Notes: This table reports the results of estimating Equation 5.1 on weekly time series spanning June 2009 to December 2013 separately for each 911 call type. The unit of observation is a calendar week, and the outcome variable is log weekly 911 calls of each type. Newey-West standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 13: Estimates for the Effect on Monthly Fatal Crashes

	SC	SDID
	(1)	(2)
Seattle x Post	0.00	-0.15
	(0.06)	(0.37)
Pre-period mean	2.06	2.06

Notes: This table reports the estimates for monthly fatal crashes in Seattle compared to its synthetic control counterfactual in Column 1 and its synthetic difference-in-differences counterfactual in Column 2. Standard errors calculated using the placebo method with 500 replications are reported in parentheses.

Appendices

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A Model Derivations

The police captain chooses S^* and G^* to maximize

$$V = -c_m \delta_m S - c_g G - \beta \left(\Theta - A S^{\gamma} - G^{\rho}\right)^{\tau}.$$

First-Order Conditions. Taking derivatives with respect to S and G, respectively, yields the following first-order conditions, which implicitly define S^* and G^*:

$$\begin{aligned} V_S(S^*, G^*) &= -c_m \delta_m + \beta \tau A \gamma (S^*)^{\gamma - 1} \left(R^*\right)^{\tau - 1} = 0 \\ V_G(S^*, G^*) &= -c_g + \beta \tau \rho (G^*)^{\rho - 1} \left(R^*\right)^{\tau - 1} = 0 \end{aligned}$$

where $R^* = \Theta - A(S^*)^{\gamma} - (G^*)^{\rho}$.

Second-Order Conditions.

$$\begin{aligned} V_{SG} &= -\beta \tau (\tau - 1) A \gamma \rho (S^*)^{\gamma - 1} (G^*)^{\rho - 1} \left(R^*\right)^{\tau - 2} \\ V_{SS} &= \beta \tau A \gamma (\gamma - 1)(S^*)^{\gamma - 2} \left(R^*\right)^{\tau - 1} \\ &\quad - \beta \tau (\tau - 1) A^2 \gamma^2 (S^*)^{2\gamma - 2} \left(R^*\right)^{\tau - 2} \\ V_{GG} &= \beta \tau \rho (\rho - 1)(G^*)^{\rho - 2} (R^*)^{\tau - 1} \\ &\quad - \beta \tau (\tau - 1) \rho^2 (G^*)^{2\rho - 2} \left(R^*\right)^{\tau - 2}. \end{aligned}$$

The conditions $V_{SS} < 0$ and $V_{GG} < 0$ are satisfied as long as one of these holds: (1) $\tau > 1$ and $\gamma \leq 1$ and $\rho \leq 1$ or (2) $\tau = 1$ and $\gamma < 1$ and $\rho < 1$.

The condition $V_{SS} V_{GG} > V_{SG}^2$ is satisfied if

$$\begin{aligned} \Big(\beta \tau A \gamma (\gamma - 1)(S^*)^{\gamma - 2} \left(R^*\right)^{\tau - 1} - \beta \tau (\tau - 1) A^2 \gamma^2 (S^*)^{2\gamma - 2} \left(R^*\right)^{\tau - 2}\Big) \Big(\beta \tau \rho (\rho - 1)(G^*)^{\rho - 2} (R^*)^{\tau - 1} - \\ \beta \tau (\tau - 1) \rho^2 (G^*)^{2\rho - 2} \left(R^*\right)^{\tau - 2}\Big) > \beta^2 \tau^2 \left(\tau - 1\right)^2 A^2 \gamma^2 \rho^2 (S^*)^{2\gamma - 2} (G^*)^{2\rho - 2} (R^*)^{2\tau - 4}, \end{aligned}$$

which is satisfied if one of the following holds: (1) $\tau > 1$ and either $\gamma < 1$ and $\rho \leq 1$ or $\gamma \leq 1$ and $\rho < 1$ or (2) $\tau = 1$ and $\gamma < 1$ and $\rho < 1$.

Comparative Statics. The federal investigation increases the cost of misconduct, c_m. I apply the implicit function theorem to determine how S^* and G^* respond to changes in c_m:

$$\begin{aligned} V_{Sc_m} &= -\delta_m \\ V_{Gc_m} &= 0 \end{aligned}$$

.

Totally differentiating the first-order conditions with respect to c_m yields

$$V_{Sc_m} + V_{SS}\frac{\partial S^*}{\partial c_m} + V_{SG}\frac{\partial G^*}{\partial c_m} = 0$$
$$V_{Gc_m} + V_{SG}\frac{\partial S^*}{\partial c_m} + V_{GG}\frac{\partial G^*}{\partial c_m} = 0.$$

The comparative statics are as follows:

$$\frac{\partial S^*}{\partial c_m} = \frac{-V_{Sc_m}V_{GG}}{V_{SS}V_{GG} - V_{SG}^2} < 0$$

and

$$\frac{\partial G^*}{\partial c_m} = -\frac{V_{SG}}{V_{GG}}\frac{\partial S^*}{\partial c_m} > 0.$$

I use these comparative statistics and the equations for realized crime to assess responses to changes in c_m. I obtain the following comparative static:

$$\frac{\partial R^*}{\partial c_m} = (\rho(G^*)^{\rho-1}\frac{V_{SG}}{V_{GG}} - A\gamma(S^*)^{\gamma-1})\frac{\partial S^*}{\partial c_m}.$$

This expression is equal to zero if $\rho(G^*)^{\rho-1}\frac{V_{SG}}{V_{GG}} = A\gamma(S^*)^{\gamma-1}$, that is, if the lost productivity from S is equal to the gained productivity from G.

B Officer-Initiated Stop and 911 Call Type Classification

B.1 Officer-Initiated Stops

I use the initial case type description field in the computer-aided dispatch data to classify all officer-initiated (OI) stops into four categories: traffic, suspicious activity, premises check, and other. Below I list the text descriptions included in each category of OI stops.

Premises Check: Premise Check Officer Initiated Onview Only.

Suspicious Activity: Suspicious Package; Suspicious Person Vehicle Or Incident; Suspicious Stop Officer Initiated Onview; Tru Suspicious Circumstances.

Traffic: Traffic Assist Motorist; Traffic Blocking Roadway; Traffic Blocking Traffic; Traffic Bo Signals And Down Signs; Traffic Moving Violation; Traffic Pursuit Officer Initiated Onview; Traffic Road Rage; Traffic Stop Officer Initiated Onview.

Other: Abandoned Vehicle; Abduction No Known Kidnapping; Acc Hit And Run No Injuries Includes Ip Jo; Acc Non Injury Blocking; Acc Report Non Inj Non Blkg Or After Fact Inj; Acc Unk Injuries; Acc With Injuries Includes Hit And Run; Alarm Audible Automobile Unocc Anti Theft; Alarm Comm Hold Up Panic Except Banks; Alarm Comm Inc Bank Atm Schools Bsn; Alarm Comm Silent Aud Burg Incl Banks; Alarm Duress Panic Bus Taxi Car Prsn Not Dv; Alarm Residential Burglary Silent Audible; Animal Dangerous; Animal Injured Dead Hazard Other; Animal Ip Jo Bite; Animal Ip Jo Dangerous; Animal

Report Bite; Arson Ip Jo; Arson Report; Aslt Dv; Aslt Ip Jo Dv; Aslt Ip Jo Person Shot Or Shot At; Aslt Ip Jo With Or W O Wpns No Shootings; Aslt Molested Adult Groped Fondled Etc; Aslt Person Shot Or Shot At; Aslt With Or W O Weapons No Shootings; Assigned Duty Centurylink Stadium; Assigned Duty Community School Special Event; Assigned Duty Court; Assigned Duty Detail By Supervisor; Assigned Duty Foot Beat From Assigned Car; Assigned Duty Hospital Guard; Assigned Duty In Service Training; Assigned Duty Meet W Supervisor Out Of Svc; Assigned Duty Other Escort; Assigned Duty Reports; Assigned Duty Seattle Center Event; Assigned Duty Stakeout; Assigned Duty Station Duty Clerk Mail Etc; Assigned Duty Transport Evidence Equipment; Assist Other Agency Emergency Service; Assist Other Agency Routine Service; Assist Public No Welfare Chk Or Dv Order Service; Assist Spd Routine Service; Assist Spd Urgent Service; Auto Recovery; Auto Theft Ip Jo Vehicle Plates Tabs; Auto Theft Loss Plates And Or Tab; Auto Theft Veh Theft Or Theft Recovery; Bias Racial Political Sexual Motivation; Bomb Threats Ip Jo; Burg Comm Burglary Includes Schools; Burg Ip Jo Comm Burg Includes Schools; Burg Ip Jo Res Incl Unocc Structures; Burg Res Incl Unocc Structures On Prop; Burn Reckless Burning; Carjacking Ip Jo Robbery; Child Aband Abused Molested Neglected; Child Ip Jo Aband Abuse Molest Neglect; Child Ip Jo Luring; Child Luring; Custodial Interference Dv; Demonstrations; Detox Pickup Fire Police Standing By; Detox Request For; Dist Dv No Aslt; Dist Ip Jo Dv Dist No Aslt; Disturbance Miscellaneous Other; Doa Casualty Dead Body; Down Check For Person Down; Dui Driving Under Influence; E R T Hostage; Elementary School Visit; Escape Ip Jo Prisoner; Explosion Ip Jo; Explosion With Significant Delay; Fight Ip Jo With Weapons; Fight Ip Physical No Weapons; Fight Jo Physical No Weapons; Fight Verbal Oral No Weapons; Fireworks Nuisance No Hazard; Follow Up; Foot Eluding Police; Found Person; Fraud Forgery Bunco Scams Id Theft Etc; Fraud Fraud Including Bunco; Gambling; Gas Maintenance Wash Garage; Haras No Bias Threats Or Maliciousness; Harbor Water Debris Navigational Hazards; Harbor Water Emergencies; Haz Imminent Thrt To Phys Safety No Haz Mat; Haz Potential Thrt To Phys Safety No Hazmat; Hospital Guard Assignment; Hzmat Haz Materials Leaks Spills Or Found; Illegal Dumping; Informational Broadcasts; Infrastructure Checks; Injured Ip Jo Person Industrial Accident; Injured Person Industrial Accident; Juvenile Runaway; Juvenile Runaway Pickup; Lewd Exposing Flashing; License Inspections Check For; Liquor Violations Adult; Liquor Violations Business; Liquor Violations Minor; Littering; Mental Person Or Pickup Transport; Missing Adult; Missing Alzheimer Endangered Elderly; Missing Child; Mvc Non Injury Blocking; Narcotics Found; Narcotics Violation Of Soda Order; Narcotics Violations Loiter Use Sell Nars; Narcotics Warrant Service; No Answer When Called; Noise Animal Includes Barking Dogs; Noise Dist General Const Resid Ball Play; Noise Disturbance Party Etc; Nuisance Mischief; Open Building Door Etc; Order Assist Dv Vic W Srvc Of Court Order; Order Ip Violation Of Dv Court Order; Order Service Of Dv Court Order; Order Violating Dv Court Order; Order Violation Of Court Order Non Dv; Out At Range; Out Of Car No Reason Given; Out To Precinct Station; Overdose Drug Related Casualty; Panhandling Aggressive; Parking Violation Except Abandoned Car; Parks Violations Cites Includes Exclusions; Peace Standby To Assure No Court Ordr Svc; Pedestrian Violations; Phone Obscene Or Nuisance Phone Calls; Power Out Poles And Transformers; Prisoner Escort Busy Code; Property Damage; Property Found; Property Lost Or Missing; Prowler; Prowler Ip Jo; Purse Snatch Ip Jo Robbery; Purse Snatch Robbery; Pursuit Foot Or Vehicle; Rape; Rape Ip Jo; Request To

Watch; Robbery Includes Strong Arm; Robbery Ip Jo Includes Strong Arm; Service Welfare Check; Sex In Public Place View Incl Masturbation; Sex Offender Failure To Register; Sfd Assist On Fire Or Medic Response; Shoplift Theft; Shots Delay Includes Heard No Assault; Shots Ip Jo Includes Heard No Assault; Sick Person; Sleeper Aboard Bus Commuter Train; Stadium Event Assignment; Suicide Ip Jo Suicidal Person And Attempts; Suicide Suicidal Person And Attempts; Swat Critical Incident Logs And Callouts; Test Call Only; Theft Does Not Include Shoplift Or Svcs; Theft Of Services; Threats Dv No Assault; Threats Incls In Person By Phone In Writing; Tracking Alarm; Trees Down Obstructing Public Prop No Haz; Trespass; Tru Acc Hit And Run; Tru Commercial Burglary; Tru Forgery Chks Bunco Scams Id Theft; Tru Theft; Undercover Ops Caution Includes Stakeouts; Unknown Ani Ali Landline Includes Open Line; Unknown Complaint Of Unknown Nature; Vice Pornography; Vice Prostitution; Vice Violation Of Soap Order; Warrant Felony Pickup; Warrant Misd Warrant Pickup; Warrant Pickup From Other Agency; Warrant Search Caution Excl Narcotics; Water Floods Broken Mains Hydrants No Haz; Weapn Gun Deadly Wpn No Thrts Aslt Dist; Weapn Ip Jo Gun Deadly Wpn No Thrt Aslt Dist; Wires Down Phone Electrical Etc.

B.2 911 Calls

I similarly use the initial case type field to classify all 911 calls into seven categories: disturbance, domestic violence, suicide, suspicious activity, traffic, theft, and other. Below I list the text descriptions included in each category of 911 calls.

Disturbance: Noise Dist General Const Resid Ball Play; Disturbance Miscellaneous Other; Nuisance Mischief; Noise Disturbance Party Etc; Phone Obscene Or Nuisance Phone Calls; Fireworks Nuisance No Hazard; Tru Obscene Or Nuisance Phone Calls; Tru Disturbance.

Domestic Violence: Dist Dv No Aslt; Dist Ip Jo Dv Dist No Aslt; Aslt Dv; Aslt Ip Jo Dv; Threats Dv No Assault; Custodial Interference Dv.

Suicide: Suicide Ip Jo Suicidal Person And Attempts; Suicide Suicidal Person And Attempts.

Suspicious Activity: Suspicious Person Vehicle Or Incident; Suspicious Package; Tru Suspicious Circumstances.

Theft: Shoplift Theft; Theft Does Not Include Shoplift Or Svcs; Tru Theft; Secondary Theft Not Shoplift Or Services; Theft Of Services; Tru Shoplift; Tru Theft Of Services.

Traffic: Acc Non Injury Blocking; Acc With Injuries Includes Hit And Run; Traffic Blocking Roadway; Acc Report Non Inj Non Blkg Or After Fact Inj; Acc Hit And Run No Injuries Includes Ip Jo; Tru Acc Hit And Run; Traffic Bo Signals And Down Signs; Traffic Assist Motorist; Traffic Moving Violation; Acc Unk Injuries; Traffic Road Rage; Tru Road Rage; Traffic Stop Officer Initiated Onview.

Other: Narcotics Violations Loiter Use Sell Nars; Doa Casualty Dead Body; Auto Recovery; Unknown Ani Ali Pay Phns Incl Open Line; Burg Comm Burglary Includes Schools; Fight Verbal Oral No Weapons; Burg Res Incl Unocc Structures On Prop; Unknown Ani Ali Wrls Phns Incl Open Line; Assist Public No Welfare Chk Or Dv Order Service; Follow Up; Fraud Forgery Bunco Scams Id Theft Etc; Auto Theft Veh Theft Or Theft Recovery; Trespass; Aslt Ip Jo With Or W O Wpns No Shootings; Missing Adult; Purse Snatch Ip Jo

Robbery; Aslt With Or W O Weapons No Shootings; Purse Snatch Robbery; Peace Standby To Assure No Court Ordr Svc; Lewd Exposing Flashing; Missing Alzheimer Endangered Elderly; Property Damage; Down Check For Person Down; Rape; Liquor Violations Adult; Tru Residential Burglary; Child Aband Abused Molested Neglected; Found Person; Fight Ip Physical No Weapons; Robbery Includes Strong Arm; Assist Other Agency Routine Service; Arson Report; Unknown Ani Ali Landline Includes Open Line; Alarm Comm Silent Aud Burg Incl Banks; Threats Incls In Person By Phone In Writing; Fight With Weapons; Fight Jo Physical No Weapons; Haz Imminent Thrt To Phys Safety No Haz Mat; Prowler Ip Jo; Haras No Bias Threats Or Maliciousness; Aslt Molested Adult Groped Fondled Etc; Shots Delay Includes Heard No Assault; Open Building Door Etc; Robbery Ip Jo Includes Strong Arm; Mental Person Or Pickup Transport; Property Found; Service Welfare Check; Secondary Property Damage Destruction; Shots Ip Jo Includes Heard No Assault; Sex In Public Place View Incl Masturbation; Unknown Complaint Of Unknown Nature; Child Ip Jo Aband Abuse Molest Neglect; Fight Ip Jo With Weapons; Wires Down Phone Electrical Etc; Burg Ip Jo Res Incl Unocc Structures; Dui Driving Under Influence; Vice Prostitution; Juvenile Runaway; Alarm Residential Burglary Silent Audible; Animal Dangerous; Sick Person; Noise Animal Includes Barking Dogs; Auto Theft Ip Jo Vehicle Plates Tabs; Injured Ip Jo Person Industrial Accident; Alarm Audible Automobile Unocc Anti Theft; Informational Broadcasts; Tru Property Destruction Damage; Weapn Gun Deadly Wpn No Thrts Aslt Dist; Trees Down Obstructing Public Prop No Haz; Explosion Ip Jo; Gambling; Explosion With Significant Delay; Assist Other Agency Emergency Service; Parking Violation Except Abandoned Car; Illegal Dumping; Liquor Violations Minor; Order Violating Dv Court Order; Demonstrations; Narcotics Found; Tru Harassment; Auto Theft Loss Plates And Or Tab; Missing Child; Injured Person Industrial Accident; Sfd Assist On Fire Or Medic Response; Weapn Ip Jo Gun Deadly Wpn No Thrt Aslt Dist; Animal Injured Dead Hazard Other; Juvenile Runaway Pickup; Prowler; Order Ip Violation Of Dv Court Order; Animal Ip Jo Dangerous; Panhandling Aggressive; Property Lost Or Missing; Carjacking Ip Jo Robbery; Alarm Duress Panic Bus Taxi Car Prsn Not Dv; Harbor Water Emergencies; Arson Ip Jo; Burg Ip Jo Comm Burg Includes Schools; Haz Potential Thrt To Phys Safety No Hazmat; Sleeper Aboard Bus Commuter Train; Tru Forgery Chks Bunco Scams Id Theft; Tru Threats; Animal Report Bite; Warrant Misd Warrant Pickup; Child Ip Jo Luring; Bias Racial Political Sexual Motivation; Rape Ip Jo; Tru Commercial Burglary; Power Out Poles And Transformers; Order Service Of Dv Court Order; Bomb Threats; Warrant Felony Pickup; Warrant Pickup From Other Agency; Child Luring; Order Violation Of Court Order Non Dv; Parks Violations Cites Includes Exclusions; Littering; Escape Ip Jo Prisoner; Animal Ip Jo Bite; Overdose Drug Related Casualty; Warrant Search Caution Excl Narcotics; Bomb Threats Ip Jo; Assist Spd Routine Service; Detox Request For; Aslt Ip Jo Person Shot Or Shot At; Tru Aslt Molested Adult Groped Fondled Etc; License Inspections Check For; Abduction No Known Kidnapping; Bias Ip Jo Racial Political Sexual Motivation; Water Floods Broken Mains Hydrants No Haz; Order Assist Dv Vic W Srvc Of Court Order; Aslt Person Shot Or Shot At; Alarm Residential Silent Aud Panic Duress; Help The Officer; Vice Violation Of Soap Order; Harbor Water Debris Navigational Hazards; Hzmat Haz Materials Leaks Spills Or Found; Burn Reckless Burning; Alarm Atm Machine Free Standing; Liquor Violations Business; Alarm Comm Hold Up Panic Except Banks; Carjacking Robbery; Request To Watch; Abandoned Vehicle; Abduction Ip Jo Unk Kidnapping; Escape Prisoner;

Assist Spd Urgent Service; Awol Adult Or Juvenile; Tru Aslt With Or W O Wpns No Shootings; Hazard Ip Jo Mudslides; Bulletin Violent Offender; Narcotics Warrant Service; Tru Robbery; Pursuit Foot Or Vehicle; Alarm Bank Hold Up; Vice Pornography; Secondary Property Lost Or Missing; Premise Check Officer Initiated Onview Only; Secondary Forgery Bunco Scams Id Theft; Tru Follow Up; Infrastructure Checks; Rescue Of Person; Pedestrian Violations; Tru Illegal Dumping; Secondary Follow Up; Tru Lewd Conduct.

C Construction of Measured Officer Traits

I use officer–event-level data to construct three measured officer traits, which I use in my analysis of heterogeneous officer responses to the federal investigation. I describe each of the traits below and their construction.

The first trait is the OI arrest fixed effect, which captures an officer's arrest propensity in OI stops conditional on stop characteristics. To construct this measure, I use an officer–dispatch-level data set containing all OI stops between June 2009 and the investigation launch to estimate the following ordinary least squares specification:

$$Arrest_{i,j,t} = X_{i,j,t} + \omega_j + \epsilon_{i,j,t}, \tag{C.1}$$

where $Arrest_{i,j,t}$ is an indicator for whether OI stop i involving officer j at time t resulted in an arrest. $X_{i,j,t}$ is a vector containing location, year, and call priority fixed effects; ω_j is officer fixed effects; and $\epsilon_{i,j,t}$ is the error term. The OI arrest fixed effect encompasses at least two aspects of an officer's arrest decision-making: selection about which stops to make and decisions about how to proceed conditional on making a stop. For instance, officers may have a high OI arrest fixed effects if they are highly selective or cautious about making stops, opting to make stops for more serious things that are likely to result in arrest. Alternatively, an officer who engages aggressively conditional on the decision to make a stop, regardless of stop selectivity, may also have a high OI arrest fixed effect. Unfortunately, I am unable to distinguish between these margins in my analysis. I use the estimated fixed effects to create an indicator set to 1 for officers with values above the median.

The second measure I construct is the 911 call arrest fixed effect, which captures an officer's arrest propensity in 911 calls conditional on call characteristics. To construct this measure, I use an officer–dispatch-level data set containing all 911 call dispatches between June 2009 and the investigation launch to estimate the following ordinary least squares specification:

$$Arrest_{i,j,t} = X_{i,j,t} + \eta_j + \mu_{i,j,t}, \tag{C.2}$$

where $Arrest_{i,j,t}$ is an indicator for whether 911 call i involving officer j at time t resulted in an arrest. $X_{i,j,t}$ is a vector containing location, year, and call priority fixed effects; η_j is officer fixed effects; and $\mu_{i,j,t}$ is the error term. Because 911 calls are conditionally randomly assigned, the 911 call arrest fixed effect theoretically removes the selection margin featured in the OI arrest fixed effect and should instead capture an officer's arrest inclination conditional on being dispatched. Officers who are involved in more 911 calls that result in arrest will have higher 911 arrest fixed effects. I use the estimated fixed effects to create an indicator

set to 1 for officers with values above the median.

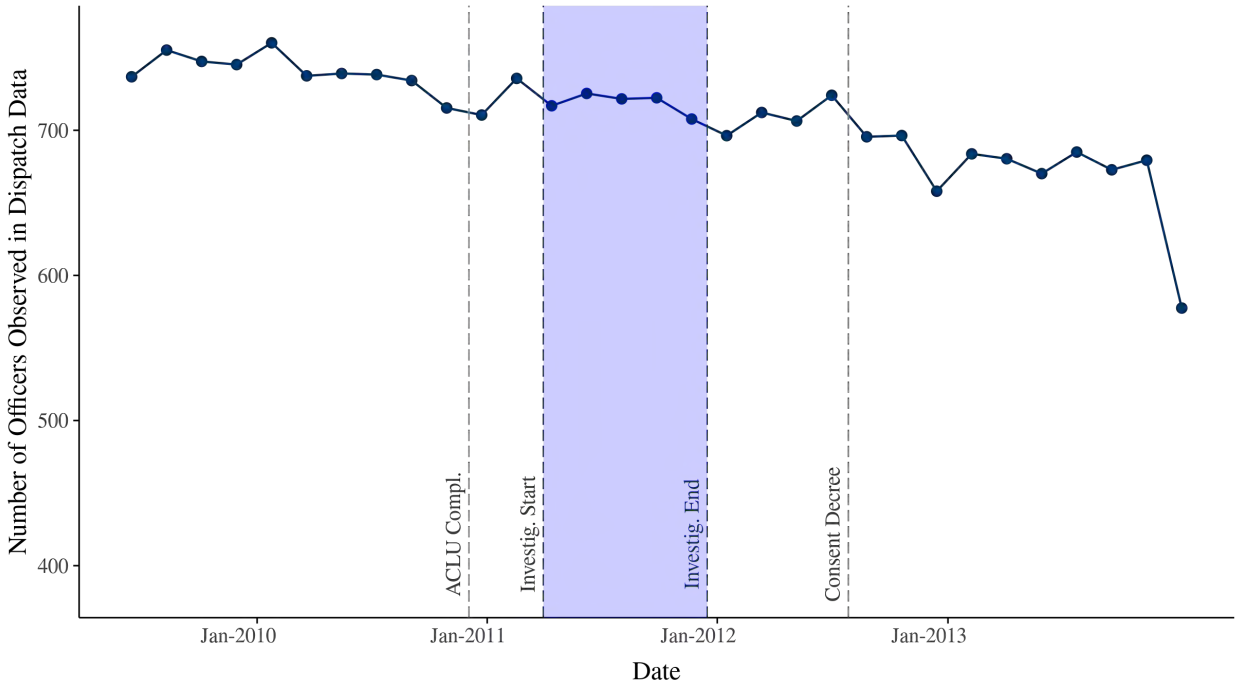
The third measure I construct is the conviction fixed effect, which captures the likelihood that misdemeanor charges associated with an officer lead to a guilty finding. I construct this measure by linking SPD and Seattle Municipal Court records to create an officer–charge-level data set containing all charges filed between June 2009 and the investigation launch. I then estimate the following ordinary least squares specification:

$$Conviction_{i,j,t} = X_{i,j,t} + \theta_j + \sigma_{i,j,t}, \tag{C.3}$$

where $Conviction_{i,j,t}$ is an indicator for whether charge i involving officer j at time t resulted in a guilty finding. $X_{i,j,t}$ is a vector containing controls for case and dispatch type as well as location, year, and call priority fixed effects; θ_j is officer fixed effects; and $\sigma_{i,j,t}$ is the error term. Similar to the OI arrest fixed effect, the conviction fixed effect captures at least two facets of an officer's job, and I am not able to distinguish between them. For example, officers with a high conviction fixed effect may be more selective in arrests or they may be better at documentation, improving the evidentiary basis for conviction. I use the estimated fixed effects to create an indicator set to 1 for officers with values above the median.

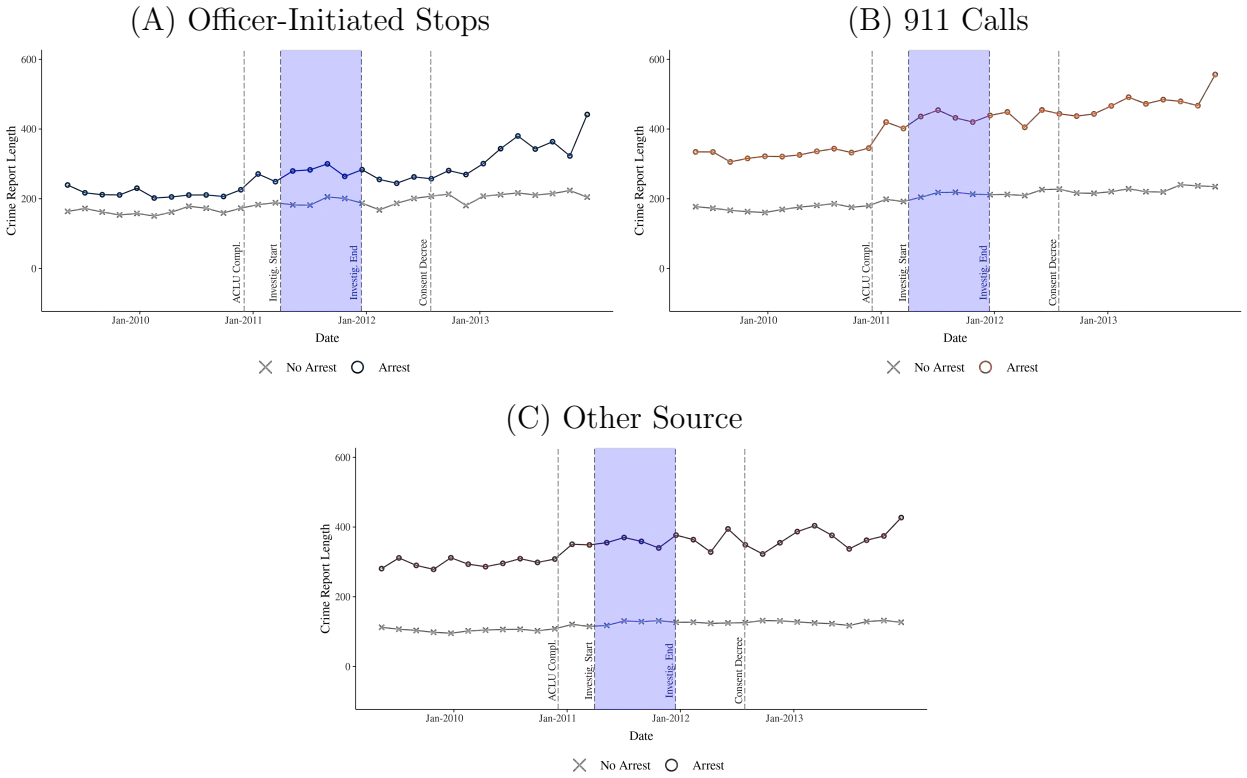
D Appendix Figures

Figure D1: Weekly Number of Officers Observed in Dispatch Data



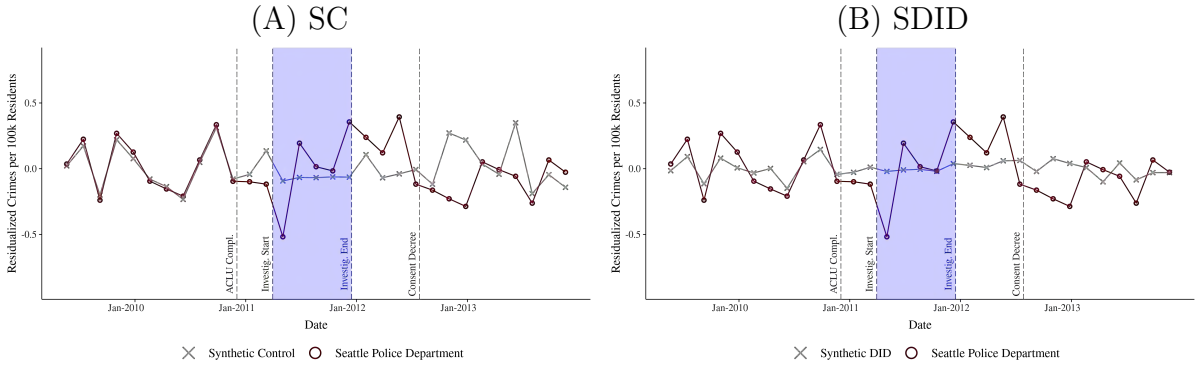
Notes: This figure plots the weekly number of officers observed in the computer-aided dispatch (CAD) data from June 2009 to December 2013.

Figure D2: Unadjusted Police Report Length by Crime Discovery Source



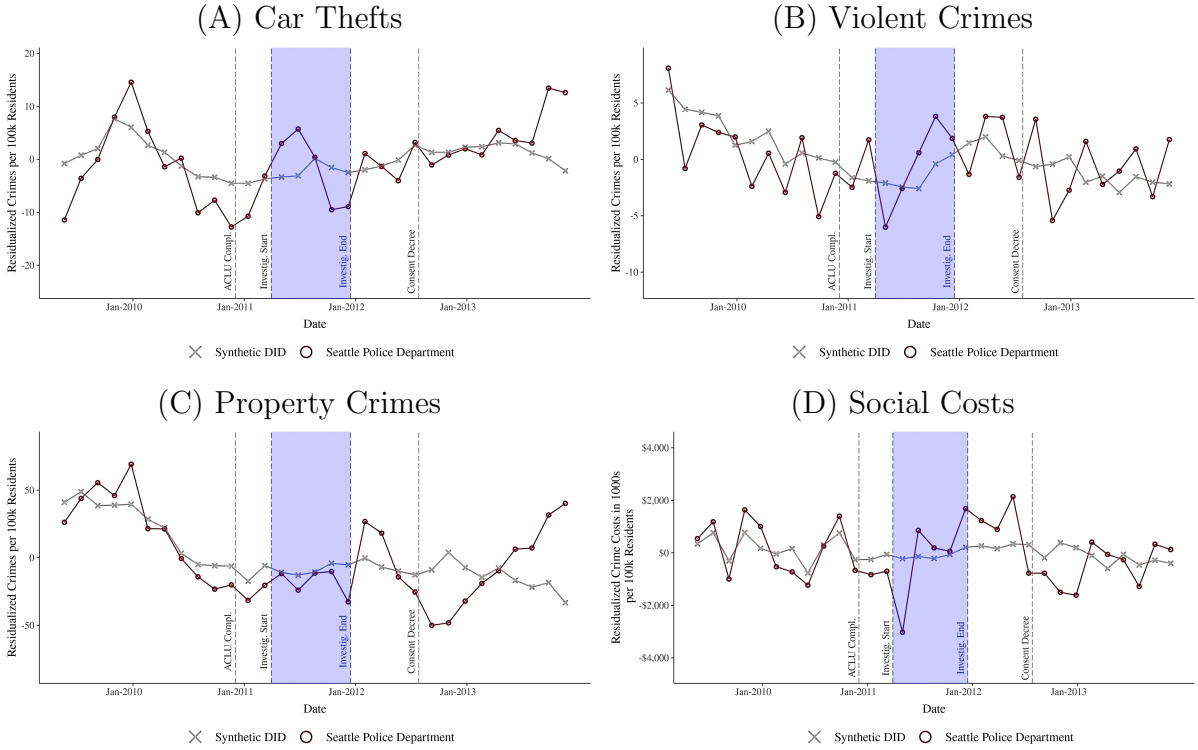
Notes: This figure plots the length, in words, of police reports from June 2009 to December 2013. Panel A shows report length for crimes discovered through officer-initiated (OI) stops. Panel B shows report length for crimes discovered via 911 calls, and Panel C shows report length for crimes discovered through other sources.

Figure D3: Monthly Homicides per 100,000 Residents



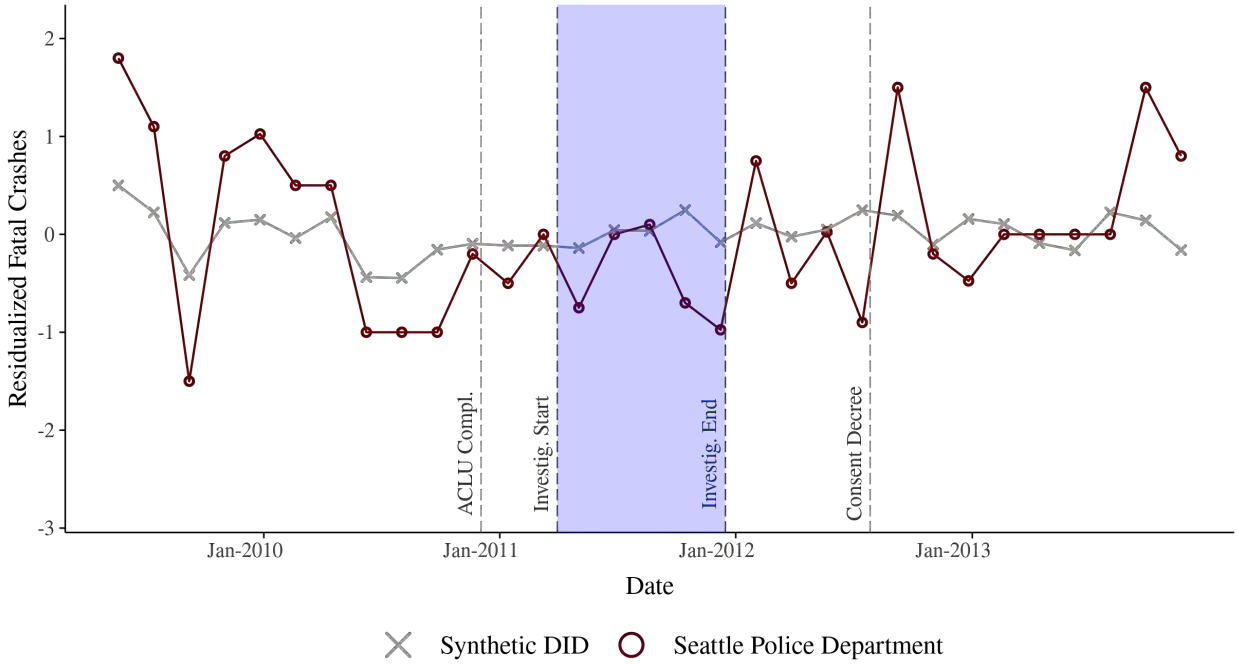
Notes: These figures plots monthly homicides per 100 thousand residents, residualized by month-of-the-year fixed effects, in Seattle compared to its synthetic control counterfactual in Panel A and its synthetic difference-in-differences counterfactual in Panel B.

Figure D4: Monthly Crimes per 100,000 Residents, Synthetic DID



Notes: These figures plots monthly crimes per 100,000 residents, residualized by month-of-the-year fixed effects, in Seattle compared to its synthetic difference-in-differences counterfactual. Panel A reports car thefts, Panel B reports violent crimes, Panel C reports property crimes, and Panel D reports the social cost of index crimes in US$1,000s.

Figure D5: Monthly Fatal Crashes, Synthetic DID



Notes: This figure plots monthly fatal crashes, residualized by month-of-the-year fixed effects, in Seattle compared to its synthetic difference-in-differences counterfactual from June 2009 to December 2013.

E Appendix Tables

Table E1: Randomization of 911 Calls

	White	Black	Hispanic	Female
	(1)	(2)	(3)	(4)
Share Minority	0.01	0.01	-0.00	-0.02
	(0.02)	(0.01)	(0.01)	(0.01)
Per Capita Inc	-0.01	0.01*	-0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)
Share Unemployed	0.03	0.01	-0.03*	-0.01
	(0.04)	(0.03)	(0.02)	(0.03)
Share Less Than HS	-0.08*	-0.02	-0.01	0.01
	(0.04)	(0.02)	(0.01)	(0.03)
Observations	2,812,579	2,812,579	2,812,579	2,812,579
F-test, p-value	1	1	1	1
Beat-Week-of-Year FEs	X	X	X	X
Beat-Shift FEs	X	X	X	X

Notes: This table reports the results from testing the conditionally random assignment of officers to 911 calls. The columns report estimates from an OLS regressions of officer race and sex on the variables listed in the rows. Standard errors clustered at the officer level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E2: Social Cost of Index Crimes in 2009 Dollars

Crime Type	Social Costs in 1000s
Murder	$5,162.88
Rape	$154.89
Arson	$61.95
Aggravated Assault	$56.79
Robbery	$23.75
Car Theft	$9.29
Burglary	$5.16
Larceny	$2.89

Notes: This table reports the social cost of each index crime type from Bhatt et al. (2023) deflated to 2009 dollars.

Table E3: Correlation between Officer Traits

	Black	Hispanic	Female	High 911 Arrest FE	High OI Arrest FE	High Conviction FE	High Experience
Black	1.000						
Hispanic	-0.050	1.000					
Female	-0.028	-0.069	1.000				
High 911 Arrest FE	-0.040	-0.028	-0.051	1.000			
High OI Arrest FE	0.002	-0.055	0.007	0.386	1.000		
High Conviction FE	-0.037	-0.019	-0.005	-0.002	0.122	1.000	
High Experience	-0.042	0.113	-0.058	-0.163	-0.189	-0.071	1.000

Notes: This table reports the correlation coefficients between officer traits. For more information on each trait, please refer to Section 4 and Appendix C.

Table E4: Synthetic Control Donor Pool with Assigned Weights

Agency	Car Thefts	Violent	Property	Social Costs
Santa Ana Police Department, CA	0.12	0.02	0.00	0.00
Norfolk Police Department, VA	0.07	0.01	0.04	0.00
St. Louis (City) Police Dept, MO	0.05	0.00	0.00	0.02
Kansas City Police Department, MO	0.05	0.00	0.00	0.00
Springfield Police Department, MA	0.04	0.02	0.00	0.00
San Bernardino Police Department, CA	0.04	0.01	0.00	0.02
Rochester Police Department, NY	0.04	0.01	0.00	0.01
Springfield Police Dept, MO	0.04	0.01	0.00	0.00
Oklahoma City Police Department, OK	0.03	0.00	0.03	0.02
Salinas Police Department, CA	0.03	0.00	0.03	0.01
Anaheim Police Department, CA	0.03	0.01	0.01	0.01
Amarillo Police Department, TX	0.03	0.01	0.07	0.01
Ontario Police Department, CA	0.03	0.01	0.01	0.02
Omaha Police Dept, NE	0.02	0.03	0.00	0.00
Baton Rouge Police Department, LA	0.02	0.03	0.05	0.03
Laredo Police Department, TX	0.02	0.01	0.02	0.00
El Paso Police Department, TX	0.02	0.00	0.03	0.01
Virginia Beach Police Department, VA	0.02	0.01	0.02	0.02
Glendale Police Department, CA	0.02	0.02	0.02	0.01
Modesto Police Department, CA	0.02	0.02	0.00	0.01
Memphis Police Department, TN	0.02	0.02	0.00	0.01
Sioux Falls Police Department, SD	0.02	0.02	0.00	0.00
Lubbock Police Department, TX	0.02	0.00	0.00	0.01
Anchorage Police Department, AK	0.02	0.00	0.02	0.01
Nashville Metro Police Department, TN	0.02	0.00	0.00	0.01
Madison Police Department, WI	0.02	0.02	0.00	0.01
Salt Lake City Police Department, UT	0.02	0.02	0.05	0.01
Fort Wayne Police, IN	0.02	0.01	0.00	0.02
Colorado Springs Police Department, CO	0.01	0.01	0.00	0.00
Little Rock Police Department, AR	0.00	0.00	0.00	0.01
Mesa Police Department, AZ	0.00	0.02	0.00	0.01
Scottsdale Police Dept, AZ	0.00	0.02	0.01	0.01
Tempe Police Department, AZ	0.00	0.02	0.00	0.02
Tucson Police Department, AZ	0.00	0.02	0.00	0.02
Oakland Police Department, CA	0.00	0.00	0.02	0.00
Fresno Police Department, CA	0.00	0.01	0.00	0.01
Long Beach Police Department, CA	0.00	0.02	0.00	0.01
Irvine Police Department, CA	0.00	0.02	0.00	0.01
Sacramento Police Department, CA	0.00	0.01	0.00	0.01
Victorville Pd, CA	0.00	0.01	0.00	0.02
Stockton Police Department, CA	0.00	0.00	0.00	0.01

Continued on next page

Table E4 – continued from previous page

Agency	Car Thefts	Violent	Property	Social Costs
Santa Rosa Police Department, CA	0.00	0.01	0.00	0.01
Oxnard Police Department, CA	0.00	0.03	0.03	0.01
Aurora Police Department, CO	0.00	0.01	0.00	0.01
Denver Police Department, CO	0.00	0.02	0.00	0.02
Columbus Police Department, GA	0.00	0.02	0.05	0.02
Atlanta Police Department, GA	0.00	0.00	0.00	0.01
Des Moines Police Department, IA	0.00	0.01	0.00	0.01
Boise Police Department, ID	0.00	0.01	0.00	0.01
Rockford Police Dept, IL	0.00	0.01	0.06	0.03
Wichita Police Department, KS	0.00	0.00	0.02	0.01
Lexington Division Of Police, KY	0.00	0.00	0.00	0.01
Louisville Metro Police Department, KY	0.00	0.01	0.00	0.01
Shreveport Police Department, LA	0.00	0.00	0.00	0.00
Boston Police Department, MA	0.00	0.00	0.01	0.02
Worcester Police Department, MA	0.00	0.02	0.00	0.01
Grand Rapids Police Department, MI	0.00	0.00	0.00	0.00
Jackson Police Department, MS	0.00	0.00	0.00	0.00
Lincoln Police Dept, NE	0.00	0.02	0.00	0.01
Fayetteville Police Department, NC	0.00	0.01	0.04	0.00
Durham Police Department, NC	0.00	0.02	0.00	0.01
Greensboro Police Department, NC	0.00	0.00	0.01	0.02
Charlotte - Mecklenburg Police Department, NC	0.00	0.01	0.00	0.01
Las Vegas Metro Police Department, NV	0.00	0.01	0.03	0.01
Reno Police Department, NV	0.00	0.03	0.06	0.01
Toledo Police Department, OH	0.00	0.03	0.02	0.01
Akron Police Department, OH	0.00	0.02	0.00	0.02
Eugene Police Department, OR	0.00	0.00	0.06	0.01
Salem Police Department, OR	0.00	0.00	0.00	0.01
Chattanooga Police Department, TN	0.00	0.01	0.00	0.02
Knoxville Police Department, TN	0.00	0.01	0.00	0.02
Brownsville Police Department, TX	0.00	0.00	0.00	0.01
Plano Police Department, TX	0.00	0.01	0.01	0.00
Irving Police Department, TX	0.00	0.02	0.00	0.01
Corpus Christi Police Department, TX	0.00	0.00	0.00	0.01
Arlington Police Department, TX	0.00	0.00	0.00	0.00
Fort Worth Police Department, TX	0.00	0.01	0.01	0.00
Newport News Police Department, VA	0.00	0.03	0.00	0.00
Richmond Police Department, VA	0.00	0.02	0.01	0.00
Vancouver Police Department, WA	0.00	0.01	0.00	0.02
Spokane Police Department, WA	0.00	0.00	0.00	0.02
Milwaukee Police Department, WI	0.00	0.00	0.05	0.00

Table E5: Poisson Specification for the Effect on Weekly OI Stops by Stop Type

Panel A: Traffic and Suspicious Activity Stops

	Traffic		Suspicious Activity	
	Stops	Arrests	Stops	Arrests
	(1)	(2)	(3)	(4)
Complaint	-0.28***	-0.24***	-0.26***	-0.29***
	(0.04)	(0.05)	(0.06)	(0.08)
Investigation	-0.63***	-0.47***	-0.55***	-0.48***
	(0.04)	(0.05)	(0.03)	(0.05)
Post-Investigation	-0.70***	-0.97***	-0.73***	-0.75***
	(0.05)	(0.07)	(0.05)	(0.05)
Consent Decree	-0.86***	-0.78***	-0.93***	-0.87***
	(0.03)	(0.05)	(0.03)	(0.04)
Observations	239	239	239	239

Panel B: Premises Check and Other Stops

	Premises Check		Other	
	Stops	Arrests	Stops	Arrests
	(1)	(2)	(3)	(4)
Complaint	0.26***	-0.37*	0.06*	-0.10**
	(0.07)	(0.20)	(0.03)	(0.05)
Investigation	0.02	-0.37*	-0.07***	-0.20***
	(0.04)	(0.21)	(0.03)	(0.05)
Post-Investigation	0.12**	-0.61***	-0.06*	-0.26***
	(0.06)	(0.20)	(0.03)	(0.04)
Consent Decree	0.02	-0.70***	0.45***	-0.16***
	(0.04)	(0.16)	(0.03)	(0.03)
Observations	239	238	239	239

Notes: This table reports the results of estimating Equation 5.1 on weekly time series spanning June 2009 to December 2013 separately for each stop type using Poisson models. The unit of observation is a calendar week, and outcome variables are weekly OI stops and weekly arrests from OI stops. Panel A reports the estimates traffic and suspicious activity stops, while Panel B reports the estimates for premises check and other stops. All regressions include week-of-the-year fixed effects to adjust for seasonality. Newey-West standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E6: Effect on Weekly OI Stops by Neighborhood Race with Full Time Fixed Effects and Clustered Standard Errors

	OI Stops	OI Arrests	OI Arrest Rate
	(1)	(2)	(3)
Minority × Complaint	-0.56	-0.09	5.03
	(0.62)	(0.08)	(5.13)
Minority × Investigation	-1.61*	-0.22	9.62**
	(0.83)	(0.14)	(4.77)
Minority × Post-Investigation	-1.58*	-0.32**	1.64
	(0.82)	(0.15)	(5.09)
Minority × Consent Decree	-1.87*	-0.39**	-5.65
	(0.96)	(0.16)	(4.44)
Pre-period mean	8.83	0.88	57.98
Observations	141,488	141,488	87,991
Adjusted R^2	0.82	0.57	0.05
Neighborhood FEs	X	X	X
Calendar-Week FEs	X	X	X

Notes: This table reports the results of estimating Equation 5.2 on a balanced neighborhood weekly panel spanning June 2009 to December 2013. I replace week-of-the-year fixed effects with calendar-week fixed effects to flexibly control for time effects. The unit of observation is a neighbor calendar week. Column 1 reports the estimates for the weekly number of officer-initiated (OI) stops, Column 2 reports for the number of arrests from weekly OI stops, and Column 3 reports for the OI arrest rate, which I define as the number of arrest per 1000 OI stops. Standard errors clustered at the neighborhood level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E7: Estimates for the Effect on Monthly Homicides per 100k Residents

	SC	SDID
	(1)	(2)
Seattle x Post	-0.01	-0.01
	(0.06)	(0.23)
Pre-period mean	0.31	0.31

Notes: This table reports the estimates on monthly homicides per 100 thousand residents, residualized by month-of-the-year fixed effects, comparing Seattle to its synthetic control counterfactual in Column 1 and its synthetic difference-in-differences counterfactual in Column 2. Standard errors calculated using the placebo method with 500 replications are reported in parentheses.

Table E8: Synthetic Difference-in-Differences Estimates for the Effect on Monthly Crimes per 100k Residents

	Car Theft	Violent	Property	Social Costs
	(1)	(2)	(3)	(4)
Seattle x Post	5.22	4.50	-4.16	116.19
	(9.60)	(6.26)	(35.62)	(1,410.69)
Pre-period mean	49.41	50.45	430.33	5,910.07

Notes: This table reports the estimates for monthly crimes per 100 thousand residents in Seattle compared to its synthetic difference-in-differences counterfactual. Standard errors calculated using the placebo method with 500 replications are reported in parentheses.