

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

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Abstract

Policing reform advocates propose increased oversight to improve policing quality and reduce officer misconduct, but opponents fear that increased scrutiny of officers may increase crime and harm public safety if it reduces policing activities. I examine a 2011 federal investigation into the Seattle Police Department, focusing on policing responses and the impact on serious (FBI index) crime. In response to heightened scrutiny from the investigation, officers significantly reduced stops, particularly traffic and suspicious activity stops. During the investigation, stop reductions were larger in minority neighborhoods and proportionally lower among officers with lower pre-period arrest rates. After the investigation ended, stops rebounded but remained below pre-period levels in minority neighborhoods. The sudden reduction in policing intensity allows me to assess the impact of stops on serious crime. 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 federal investigations. These estimates can rule out large, but not modest, crime increases. These findings suggest that federal oversight can reduce costly policing, particularly in minority neighborhoods, without significantly increasing serious crime.

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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. Policing reform advocates suggest that increased oversight of law enforcement will improve policing quality, reduce officer misconduct, and build civilian trust. However, opponents are concerned that crime will rise and public safety will be compromised if officers are subject to increased scrutiny (Ba and Rivera, Forthcoming; Prendergast, 2021). Since a 1994 act granted it the authority to do so, the Department of Justice 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 government to provide external oversight of law enforcement agencies and combat unconstitutional policing practices.¹ However, the looming concern is whether these investigations may lead to increased crime as a result of reduced policing activity, as recent studies have suggested (Devi and Fryer Jr, 2020; Shi, 2009). In this paper, I examine how a 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 detailed, 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 practices. 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 detailed administrative data from the SPD, which allows me to track shifts in different policing activities. 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. 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 influenced by police presence and productivity, because both increase the likelihood of de-

¹Recent legislative proposals for police reform, including The George Floyd Justice in Policing Act, 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.

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

tection 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). However, our understanding of which policing activities are crucial for crime reduction and which can be safely reduced without adverse effects remains limited. 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-civilians interactions, such as stops and potential subsequent actions like arrests or the use of force, can reduce the physical, psychological, and overall 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 affect serious crime rates. This model yields two key 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 because of the opposing forces at play. 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.

In my empirical analysis, I use detailed administrative SPD data 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, often 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 crime reports. I include the length of crime 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 provide 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, which I define as the number of arrests per 1000 officer-initiated stops. I uncover important neighborhood and officer heterogeneity in these reductions. Minority neighborhoods, which I define as census block groups with less than a 50 percent share of non-Hispanic White residents, experienced larger decreases in officer-initiated stops but smaller decreases in stop arrest rates relative to non-minority neighborhoods. Both larger decreases in stops and smaller decreases in the arrest rate of stops in minority neighborhoods suggest stop prioritization by officers, as foregone 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. These findings suggest that federal police oversight is potentially beneficial to minority neighborhoods.

Examining officer-level responses, I find that Black officers, female officers, more experienced officers, and officers with lower arrest rates (adjusted for dispatch characteristics) reduced stops less than their peers during the investigation. However, only differences among officers with lower stop arrest rates remain statistically significant after adjusting for an officer's pre-period level of stops. When I examine the length of crime reports, I find that after the ACLU complaint 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 costs 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 investigations. Despite this sudden reduction in officer-initiated stops, there is no detectable impact on the incidence or social costs 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. This reduction in stops has the potential to mitigate potentially costly interactions between civilians and the police, as well as interactions between civilians and later stages

of the criminal justice system. 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 of other measures of public safety, including 911 calls and fatal crashes. Because 911 calls increased over the time period in question, I examine changes in call volume by call type, using calls for an in-progress or recently occurred suicide or suicide attempt, 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. However, there are two exceptions: calls for disturbances and domestic violence, which experienced lower rate increases during the periods after the investigation concluded. It is important to note that while some crimes are reported via 911 calls, 911 call volume does not necessarily reflect crime rates. Elevated 911 call volume might indicate a community in distress due to high crime rates, but it could also signal a community's willingness to seek assistance through 911 or their trust in the police (Ang et al., 2021). In this case, it is possible that the lower rates of increase in 911 calls for disturbance and domestic violence are a strategic community response to the findings from the investigation. Next, I 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. Lastly, I examine fatal crashes and find no significant differences between fatal crashes in Seattle and 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 a literature examining 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. 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 decrease arrest activity and, when preceded by viral incidents, result in increases in crime due to reduced officer effort. My study differs from previous studies on federal investigations because I differentiate between behavior during the active investigation phase, when scrutiny and officer uncertainty are highest, and behavior after the investigation's conclusion, allowing me to carefully isolate the impact of federal oversight. Furthermore, the granularity of my data allows me to examine broader policing activities such as stops—which may not necessarily result from observing actual crimes—rather than only arrests. I demonstrate that it is possible to reduce stops and arrests on the margin without significantly increasing serious crime.

I build upon Ba and Rivera (Forthcoming), who analyze Chicago Police Department data and show that oversight, without public scandals or outrage, reduces misconduct without affecting either officer effort or crime. My findings indicate that even though federal oversight is commonly prompted by policing scandal, it can reduce officer effort on the margin and still not affect serious crime. In another related recent study, one of the few other economics studies directly examining stops, Tebes and Fagan (2022) find significant reductions in stop-and-frisk stops in New York City without increased serious crime. These reductions occurred following a 2012 federal lawsuit that severely restricted pedestrian stops as an investigative technique for NYPD officers. My study differs from that of Tebes and Fagan (2022) in two key ways. First, the stop reductions in my case stem from heightened scrutiny from a federal investigation, not a deliberate policy reform. Second, while the NYPD is known for its aggressive adoption of “broken windows” policing, characterized by the strict enforcement of minor offenses to deter major crimes, the SPD is not known to subscribe to this policing philosophy (Lum and Vovak, 2018).³ Therefore, this study demonstrates that it is possible to 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.

I also illuminate our understanding of which types of settings and communities benefit when the police have additional oversight. Research on which communities benefit from federal oversight on the police is sparse. In a case study of the 2001 Cincinnati riots and the federal investigation into the Cincinnati Police Department, Shi (2009) shows that increased scrutiny reduced low-level arrests, particularly in heavily African American neighborhoods, and increased felony crime rates. I show that federal oversight of the SPD resulted in significant reductions in officer-initiated stops and arrests, especially in minority neighborhoods, both during and after the investigation’s conclusion. This suggests that solely focusing on arrests provides an incomplete picture of the benefits of oversight. Furthermore, I do not find any significant increases in serious crime rates in Seattle.

My study also contributes to the literature on community responses to policing activity and the potential benefits of reduced low-level offense enforcement. 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.⁴ My study demonstrates

³The “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 support for broken windows policing has been mixed (Chalfin and McCrary, 2017; Harcourt, 2005; Weisburd et al., 2015).

⁴In a similar vein, Agan, Doleac and Harvey (2023) find that foregoing prosecution of nonviolent misde-

that stops and arrests can be significantly reduced without significantly affecting serious crimes or civilian reporting. Finally, my study also 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 institutional background on my context, while Section 3 presents a framework for understanding 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 pertaining to 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 exhibited 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 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 Rodney King beating by Los Angeles Police Department officers (United 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 or racial profiling. The Department of Justice can initiate investigations based on information gathered from various sources, including news reports, public complaints, or complaints from non-profit organizations like the ACLU. The Special Litigation Section of the Civil Rights Division within the Department of Justice conducts these investigations and can also file civil lawsuits against law enforcement agencies for unlawful conduct.

meanor offenses significantly reduces future criminal activity over the next two years.

A Typical Investigation. For 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 the 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, political leaders, police unions, 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, the scope of the investigation, or its complexity. During an investigation, the Department of Justice, 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. They observe training sessions, ride along with officers, and interview command staff, officers, and community members to gather additional insights ([United States Department of Justice, 2015](#)). The aim is to uncover systemic problems contributing to unconstitutional behavior and create a plan to address these issues. 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 isn't always transparent to the agencies under investigation, occasionally causing tension 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 Department of Justice engages in 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 bi-lateral 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.

SPD's Investigation. Figure 1 shows the timeline for the federal investigation into the SPD. On December 3, 2010, the Washington state ACLU and other community organizations filed a complaint against the SPD with the U.S. Attorney's office and the Department

of Justice 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 particularly 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, Department of Justice representatives met with Seattle Mayor Michael McGinn, other community and city leaders, as well as 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 Department of Justice 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 news of the decision to launch a federal investigation into the SPD, Seattle Chief of Police John Diaz welcomed the inquiry and encouraged the Department of Justice to make their investigation “as wide as possible.” During an interview with the Seattle Times, he expressed, “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 Department of Justice announced its findings that the SPD had engaged in a pattern or practice of using excessive force, concluding the investigation. In addition, while the Department of Justice 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 Department of Justice 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. During this time, the parties negotiated about the reforms needed 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 reforms to the SPD’s policies and practices, which would be supervised by 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, which would work collaboratively with the court-appointed monitor to provide recommendations and oversight on the implementation of the settlement agreement.⁵

3 A Multitask 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 principal, for instance, the chief of police, manages a department of police officers. Her department jurisdiction includes N neighborhoods. The number of officers assigned to each neighborhood is fixed. Officers can engage in stops, S_n , other productive policing including observing and being present in the neighborhood, G_n , or unproductive activity such as leisure. The principal chooses the levels of S_n and G_n to maximize a neighborhood-specific objective function. The model implies that the principal 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

⁵For 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 department to include a single officer. The principal's objective function, $V(\cdot)$, decreases in the level of 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 exhibit diminishing marginal returns to crime reduction so that $\gamma < 1$ while G exhibit 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 principal also incurs costs to enforce her desired level of policing. Stops, S , are costless for the principal to observe and enforce, but police misconduct, m , arises from stops with probability δ_m . When misconduct occurs, the principal 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 principal must pay linear costs c_g , to monitor other productive policing, G ; otherwise, the officer might not to engage in the desired level of G and instead engage in leisure. I further assume that principal's objective function is concave in realized crime so that $\tau > 1$:

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

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

3.2 The Principal's Decision

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

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

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

These first order conditions imply that the principal sets the marginal productivity of each policing activity equal to its marginal cost. They provide a system of two equations that relates parameters to the principal's choices of S^* and G^* . 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 principal'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.⁶ Some degree of substitutability between policing activity is consistent with work 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 the two opposing forces. While the reduction in stops is likely to increase crime, substitution to other forms of productive policing 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 forms of productivity policing. 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 forms of policing dominates, then crime decreases. Finally, in the knife-edge case, the two are roughly equal and crime remains unchanged.

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, and the Gallup Confidence in Institutions Survey. I discuss each in further 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

⁶This 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 $\rho < 1$.

geographical jurisdiction from June 2009 to December 2013. This timeframe includes 18 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, offense date, 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 (CAD) 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 helps manage 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 2013.⁷

Officer-initiated (OI) stops refer to incidents in which an officer observes something while patrolling and assigns themselves to respond. In contrast, 911 calls are requests for assistance from the community, often related to emergencies. While officer-initiated 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.⁸ For each dispatch, I observe the officer(s) dispatched, date, time, location (beat, sector, and approximate coordinates), priority code, initial and final case type, and final disposition.⁹ The initial and final case types are short text descriptions of the nature of the call. Initial case type is assigned at dispatch by the officer for officer-initiated stops or the dispatcher for 911 calls, and describes

⁷The remaining dispatches primarily originate from other telephone calls, such as 311.

⁸I test the conditionally random assignment of 911 calls in Appendix Table E1.

⁹The 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 to 11 AM, the second from 11 AM to 7 PM, and the third watch runs from 7 PM to 3 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 officer-initiated stops or the dispatcher for 911 calls. The highest priority calls are assigned to 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. Non-emergency 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.

the initial reason for the dispatch. The final case type may ultimately differ from the initial case type depending on how a dispatch progresses. Finally, 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 officer-initiated stops and 911 calls for my analysis.¹⁰ Specifically, I classify officer-initiated stops into four mutually exclusive categories: premises check stops, suspicious activity stops, traffic-related stops, and other types of stops. Premises check stops involve inspecting specific locations 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, there are approximately 4,503 officer-initiated stops per week and 4,507 911 calls. I also provide a breakdown of officer-initiated stops by stop type. On average, there are about 841 premises check stops per week, 1,135 suspicious activity stops, and 787 traffic stops. The average arrest rate for officer-initiated stops (the stop arrest rate) is about 87 arrests per 1000 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.¹¹ I focus my analysis on serious (index) crimes. 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). 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, robbery, and aggravated assault, while property index crimes include car theft, burglary, larceny, and arson.

My analysis uses four crime measures. 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. Because most insurance companies require a police report for

¹⁰For additional information on the classification on officer-initiated stops and 911 calls, refer to Appendix C.

¹¹As a condition of the research partnership, the department removed protected information (such as names and addresses) from all officer narratives and replace that information with the word “redacted”, using the NLTK library in a Python script, which the department shared with me.

car theft claims to be processed, car thefts have high reporting rates to the police.¹² I also include violent crimes and other property crimes, as well as the social cost of all index crimes, as additional outcomes to broaden my analysis. I calculate the social cost of all index crimes using the estimates presented in [Bhatt et al. \(2023\)](#), which I deflate to 2009 dollars.¹³

Table 1 provides summary statistics for weekly reported crimes. The table also includes a breakdown of crime types: there are approximately 74 car thefts per week, about 581 other property crimes, and around 66 violent crimes. There are about 649 non-index crimes per week. The average weekly cost of index crimes, measured in 1000s, is approximately 8,170, with a standard deviation of 3,462. The average length of a crime report is approximately 187 words.

Officer Data. Finally, I use officer data provided by the SPD, which includes 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. The officers in my analysis are 8 percent Black, 5 percent Hispanic, and overwhelmingly 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 using the computer-aided dispatch data and municipal court records, which I use 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 in officer-initiated (OI) stops conditional on stop characteristics. I estimate these fixed effects using ordinary least squares regressions on officer-dispatch level data containing all officer-initiated 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 ordinary least squares 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

¹²[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.

¹³The social cost of each index crime in 2009 dollars is presented in Appendix Table E2.

estimate these officer fixed effects using ordinary least squares regressions on officer-charge level data containing all charges filed between June 2009 and the investigation launch. Finally, I use each estimated fixed effects to create an indicator set to 1 for officers with values above the median for that trait. I include detailed information on the construction of these measured officer traits in Appendix B 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, augmenting administrative SPD data with demographic information from the 2014 American Community Survey (ACS) five-year estimates for each census block group in Seattle. In particular, 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 comprise 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 cities 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).¹⁴ The 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 population in the United States. I impose the following restrictions on the data. First, I restrict 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 agencies that have consistently reported monthly crime data for at least 10 years between 2005

¹⁴I use the version of this data set that has been cleaned and formatted by Jacob Kaplan.

and 2015. Finally, I exclude any agency, except the SPD, that was subject to a federal investigation by 2015.¹⁵ The final data set includes the SPD and 82 comparator agencies.¹⁶

Other. Finally, I complement my analyses with data on fatal crashes from the National Highway Traffic Safety Administration (NHTSA), 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 1000 OI stops, and the length of police reports in words. In the sections that follow, I analyze the outcomes related to officer-initiated activity. Consistent with the model’s prediction, I show that when the federal investigation increases the cost of misconduct, stops decrease.¹⁷ 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 officer-initiated stops in response to the federal investigation, I implement a standard interrupted time series (ITS) design on weekly data for Seattle as a whole spanning June 2009 to December 2013. 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 ordinary least squares specification:

$$Y_t = \beta_0 + \beta_1 \text{Complaint}_t + \beta_2 \text{Investigation}_t \\ + \beta_3 \text{Post-Investigation}_t + \beta_4 \text{Consent Decree}_t + \tau_t + \epsilon_t \quad (5.1)$$

¹⁵For a list of departments with investigations, refer to Devi and Fryer Jr (2020).

¹⁶I provide the list of donor agencies in Appendix Table E4.

¹⁷My model assumes no significant changes in the number of officers, which I demonstrate in Appendix Figure D1.

where Y_t is the number of officer-initiated (OI) stops, the number of arrests, or the OI stop arrest rate (arrests per 1000 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 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 are 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. The identifying assumption is that, without the investigation, outcomes would have followed a trajectory similar to that observed during the pre-period.

5.1.1 Results

Figure 3 shows the seasonally adjusted weekly officer-initiated (OI) activity. Panel A plots weekly stops, while Panel B shows weekly arrests from stops. The shaded area in the figure represents the federal investigation period. The figure shows a decrease in both officer-initiated stops and arrests in response to the federal investigation. After the end of the investigation, the period during which the SPD was negotiating the reform agreement with the federal government, stops increased to near pre-period officer-initiated stop levels, and stops stayed relatively stable after the consent decree is in place. In contrast, arrests from officer-initiated stops stayed low after the investigation ended and remained low for the rest of the period.

Table 2 reports the regression results from estimating Equation 5.1 and confirms the visual evidence in Figure 3. I find a significant decrease in officer-initiated activity during the investigation period. The estimate suggests that weekly officer-initiated 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. During the active investigation period, 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 1000 stops, or about 6 percent of the pre-period mean. After the investigation has ended but before the consent decree began, 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 approximately 16 percent

of 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 1000 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. Collectively, these findings suggest that during the first 17 months under the consent decree, officers made more stops compared to the investigation period and nearly as many as in pre-period, however, 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 due to the nature of the stops not warranting 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 that all stop types are equally productive in reducing serious crime (i.e., the parameter A is constant across stop types). However, different 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 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 department personnel, suggests that officers might target stops where they have greater discretion. Officers might prioritize stops 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 represent prime candidates for such reductions, especially because many of these stops may not be initiated in response to observing actual crimes.¹⁸ 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.

5.2.1 Empirical Strategy: Interrupted Time Series

I examine the change in different types of officer-initiated stops by estimating Equation 5.1 separately for the four different stop type categories: traffic stops, suspicious activity, premises

¹⁸Stops for victimless crimes, which 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.

checks, and all other stops.¹⁹ Standard errors are computed using the Newey-West method.

5.2.2 Results

Figure 4 shows seasonally adjusted weekly officer-initiated activity by stop type. Panel A plots weekly stops, while Panel B shows weekly arrests from stops. The shaded area in the figure represents the federal investigation period. This figure shows a significant decrease in both traffic and suspicious activity stops. Other stops also decrease during the investigation period, but premises check stops seem unaffected. After the investigation ends, I observe a notable spike in the number of stops in the “other” category. This is a substitution from other categories, likely in response to the issues raised about investigative detention stops in the Department of Justice’s findings letter. In Panel B, I show that arrests from all officer-initiated stop types also fall during the investigation period.

Table 3 presents regression results for the change in officer-initiated stops for each of the four stop type categories. I show that the majority of the decrease in officer-initiated 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: 30.1) 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 1000 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 1000 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. The weekly stop arrest rate for other stops also fell by about 17 arrests per 1000 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 1000 stops

¹⁹For additional information on how these stop types are defined, refer to Section 4.

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 ACLU 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 in my model is through a neighborhood-specific cost of misconduct, $c_{m,n}$. Some neighborhoods may have higher perceived cost of misconduct if, for example, residents are more politically organized or engaged. Conversely, in some neighborhoods, there may be a lower perceived cost of misconduct if residents are considered more likely to engage in criminal activity, as stops may be seen as more necessary. Residents in minority neighborhoods may be less likely to complain, for example, because of mistrust in the police or the political process, and may also be less likely to have their complaints taken seriously. If the federal investigation sets a sufficiently high neighborhood-agnostic cost of misconduct, 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 c_m^- is larger. In this section, I analyze whether there was differential response based on neighborhood racial composition.

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 using the following specification:

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

where $Y_{n,t}$ is the number of officer-initiated stops, the number of arrests, or the OI stop arrest rate (arrests per 1000 stops) in neighborhood n , which I define by a census block group, 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.

Are Estimated Effects Equiproportional?

An important consideration is whether the estimated differences in stop reductions between minority and non-minority neighborhoods might reflect equiproportionate changes from pre-period means. In this scenario, if minority neighborhoods had more officer-initiated stops in the pre-period, they may appear to experience larger reductions in stops, even if the reductions are proportional to those in non-minority neighborhoods. To test for equal proportional responses between minority and non-minority neighborhoods, I include controls for each neighborhood's pre-period average weekly number of officer-initiated stops interacted with each time period indicator in 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 6 reproduces Figure 3 broken out for minority and non-minority neighborhoods. Panel A plots officer-initiated stops, while Panel B shows arrests from officer-initiated stops. Minority neighborhoods had more officer-initiated stops than non-minority neighborhoods in the pre-period. During the investigation, both minority and non-minority neighborhoods experienced decreases in weekly officer-initiated stops, but minority neighborhoods experienced a larger decrease in stops. After the investigation, stops steadily increased in both neighborhood types. In non-minority neighborhoods, officer-initiated stops return to approximately pre-period levels, but stops remain below pre-period levels in minority neighborhoods. Similarly, there were more arrests in minority neighborhoods than non-minority neighborhoods during the pre-period. Both minority and non-minority neighborhoods experienced decreases in weekly arrests from officer-initiated stops during the investigation, though minority neighborhoods again experienced larger decreases. Arrests remained low in both minority and non-minority neighborhoods during the period after the investigation ended.

Table 6 reports regression results from estimating Equation 5.2. These results are consistent with Figure 6. During the investigation, officer-initiated 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. Arrests from officer-initiated 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. Weekly arrests from officer-initiated stops decreased by an additional 0.2 (standard error: 0.05) arrests per week or about 16 percent higher in minority neighborhoods compared to non-minority neighborhoods. This estimate is significant at the 5 percent level. Finding both larger decreases in officer-initiated stops and smaller decreases in the arrest rate of remaining stops in minority neighborhoods suggests that foregone stops were less likely than average to result in arrest. 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 are 1.9 (standard error: 0.26) stops lower or about 21 percent of the pre-period mean. This estimate is statistically significant at the 1 percent level. Both the number of arrests and the arrest rate are significantly lower during the consent decree period. Arrests from officer-initiated stops decreased in minority neighborhoods by an additional 0.39 (standard error: 0.04) arrests per week or about 44 percent of the pre-period mean. This estimate is statistically significant at the 1 percent level. The arrest rate for officer-initiated stops was about 6 arrests per 1000 stops fewer in minority neighborhoods compared to non-minority neighborhoods or 10 percent of the pre-period mean. However, this estimate is only marginally significant.

In Table 7, I report estimated effects when I additionally include interaction terms between each neighborhood's pre-period average weekly number of officer-initiated 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 Columns 1 to 3 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 where I re-estimate Equation 5.2 replacing week-of-the-year fixed effects with calendar week fixed effects to flexibly control for time and clustering standard errors at the neighborhood level. These results are reported in Appendix Table E6 and are qualitatively similar to the results in Table 6.

5.4 Effect on Weekly OI Stops by Officer Traits

A growing body of 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, use force less than their peers (Ba et al., 2021; Hoekstra and Sloan, 2022; Shi, 2009). If these findings reflect officers with these traits perceiving higher costs (c_m

in my model) associated with their policing activities, then I posit that these traits may also impact officers' response to oversight from a federal investigation. Specifically, when the federal investigation establishes a new 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. I use the officer traits described in Section 4.

5.4.1 Empirical Strategy: Difference-in-Differences

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

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

where $Y_{j,t}$ is the number of officer-initiated stops by officer j in week t , 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), officer-initiated stop arrest rates (high vs. low) or 911 call arrest rates (high vs. low).²⁰ I do not argue that these 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 concerns. 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. Because I lack officer assignment data for my analysis, I define an officer's home sector as the sector in which most of her officer-initiated stops occur in the pre-period, following Hoekstra and Sloan (2022). Furthermore, by including home-sector-by-calendar-week fixed effects in the specification, 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 the 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

²⁰Please refer to Section 4 for more details on these officer traits.

in proportion to their peers. To test for equal proportional responses across officer traits, I include controls for each officer's pre-period average weekly number of officer-initiated stops interacted with each time period indicator in Equation 5.3. If the estimated effects indicating 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 5 shows normalized seasonally adjusted officer-initiated stops for the different officer traits. I normalize each series by dividing by the pre-period mean so these figures reflect proportional changes in weekly officer-initiated 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 their officer-initiated stops adjusted for stop characteristics (i.e., the OI arrest fixed effects).

Table 4 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. Specifically, Column 3 reports to sex, Column 4 reports to experience, while Columns 5 through 7 report conviction fixed effect, OI arrest fixed effect, and 911 call fixed effect, respectively. In this table, positive coefficients indicate smaller decreases in officer-initiated stops compared with the control group, while negative coefficients indicate larger decreases in comparison to the control group. Consistent with the visual evidence in Figure 5, I find that Black, female, and more experienced officers decreased officer-initiated 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 officer 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. I find that 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. These estimates are all 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 in Table 5.

Only the estimate for officers with high OI arrest fixed effects remains significant. This implies an equal proportional response across the remaining officer traits, which is consistent with the visual evidence in Figure 5.

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 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 from complaints or other unwanted attention. 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 (from officer-initiated stops, through the 911 call system, or 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.²¹ Across the three 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 do less, foregone police reports are for less serious incidents and would have been shorter. If we consider the case of police reports for crimes discovered from officer-initiated stops, then if increased report length was 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 monotonically 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 after the consent decree began, I show another increase in report length, suggesting that observed increases in report length are unlikely to be entirely

²¹The takeaways are qualitatively when examining the unadjusted series, which I show in Appendix Figure D2.

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, and $\epsilon_{r,t,l}$ is the error term. Standard errors are clustered at the beat level.

5.5.1 Results

Table 8 reports 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 than they were in the pre-period or about 29 percent of the pre-period mean. I find similar effects when I examine crimes by discovery source. The police reports for crimes discovered from officer-initiated stops were 33 (standard error: 8.4) words or about 17 percent longer. For crimes discovered from 911 calls, reports were 57 (standard error: 6.2) words or about 28 percent longer. Lastly, crime reports for crimes discovered from other sources were 39 words or about 33 percent longer. 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. Finally, 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 officer-initiated stops during the federal investigation. Given this evidence, a crucial question arises: did the decline in officer-initiated (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. In the first approach, I compare crime rates between Seattle neighborhoods that experienced larger versus smaller reductions in officer-initiated stops. If the reductions in officer-initiated stops increased crime, I might expect neighborhoods that experienced 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 officer-initiated stops in Seattle led to increased crime, I would anticipate that crime in Seattle would change differently than crime in these other jurisdictions.

6.2 Crime Effects using within-Seattle Variation

To begin my examination of crime effects, I exploit variation across Seattle neighborhoods in their exposure to decreased officer-initiated stops during the investigation. In Section 5, I showed that minority neighborhoods experienced larger decreases in weekly officer-initiated stops than non-minority neighborhoods. I exploit this findings to examine whether minority and non-minority 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 costs of index crimes in 1000s in neighborhood n in week t . Standard errors are calculated using the Newey-West method.

6.2.2 Results

Figure 8 shows seasonally adjusted weekly crimes for minority and non-minority neighborhoods. Panel A shows car thefts, Panels B shows violent crimes, Panel C shows property

crime, and Panel D shows the social costs of index crimes in 1000s. 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 evidence that crimes are increasing differentially in minority compared with non-minority 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 2.5 percent increase in violent crimes. While the implied 95% confidence interval for property crimes can rule out an increase more than 5.7 percent, the 95% confidence interval for the social costs of index crimes cannot rule out up to 42 percent increase in costs.

In Table 10, I calculate implied crimes per 1000 officer-initiated stops averted based on the estimates in Tables 6 and 9. I also report the 95% credible interval from performing Bayesian bootstrap across neighborhoods with 1000 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 costs of index crimes for every 1000 stops averted. The estimates on *Post* imply 2 fewer car thefts, 6 fewer violent crimes, 1 additional property crime, and 298 lower social costs of index crimes for every 1000 stops averted. The 95% credible intervals can rule out more than 5 additional car thefts and 4 additional violent crimes per 1000 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 costs of index crimes.

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 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 synthetic control method offers a valid approach to estimating effects and conducting inference in settings with a single treated unit and multiple control units. Using the FBI UCR

data, I construct a balanced agency-by-month panel from June 2009 to 2013 that includes Seattle and 82 control agencies without federal investigations. The synthetic control 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, $\hat{\beta}^{sc}$:

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

where y_{ct} is monthly car thefts, violent crimes, property crimes, and social costs of index crimes per 100 thousand residents for agency c in month t , μ is a constant, γ_t are time fixed effects, W_{ct} 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 synthetic control 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 in crime changes 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 crimes per 100 thousand 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 figures provide 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 synthetic control effects in Table 11.²² For car thefts, the estimated effect is -0.05 (standard error: 0.55). The estimate is not statistically different from zero and is modest relative to the pre-investigation monthly average of 49.4 car thefts per 100 thousand 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 es-

²²For consistency, I focus on the crime outcomes used throughout my analysis, but I report separate synthetic control estimates for monthly homicides per 100 thousand 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.

timate is not statistically different from zero and is small relative to the pre-investigation monthly average of 50.5 violent crimes per 100 thousand 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-investigation monthly average of 430 property crimes per 100 thousand 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 costs of index crimes in 1000s 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 costs of \$5,910 per 100 thousand residents in Seattle. However, the implied 95% confidence interval cannot rule out up to a 9 percent increase in social costs 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 synthetic control method, the synthetic difference-in-differences 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 synthetic difference-in-differences method in Appendix Figure D4 and Appendix Table E8. These estimates, while more imprecise, are qualitatively similar to those obtained using the synthetic control method.

6.4 Other Measures of Community Well-being

In the previous section, I showed that despite the significant decrease in officer-initiated stops in Seattle, there was no detectable difference between crime changes in neighborhoods that experienced larger versus smaller decreases in officer-initiated stops or between crime changes in Seattle compared with 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. Some crimes are reported through 911 calls, but a large share of 911 calls are not related to crimes. For example, some 911 calls by residents could substitute for some

officer-initiated stops. If residents valued some share of officer-initiated 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 or their willingness to make 911 calls. Next, I examine fatal crashes in Seattle compared to control in Seattle and control cities whose police departments were not subject to federal investigations. Lastly, I 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.

6.4.1 Effect on Weekly 911 Calls

I begin my analysis of the effect on 911 calls with Figure 10. This figure shows seasonally adjusted weekly 911 calls spanning from June 2009 to December 2013. Additionally, I include seasonally adjusted officer-initiated stops, as previously presented in Figure 3, in this figure for comparison. The shaded area on the figure corresponds to the investigation period. Notably, unlike officer-initiated stops, 911 calls exhibit an overall increasing trend over time, and it is challenging to discern whether this upward trend results from the investigation. While not conclusive evidence, it is nonetheless informative that 911 calls remained relatively steady during the investigation while officer-initiated stops were decreasing. Furthermore, after the end of the investigation, 911 call volume increased even as officer-initiated stops increased and continued to increase even after officer-initiated stops return to near pre-period levels.

To formally assess whether the increases in 911 calls resulted from the investigation, I compare the growth in calls among different 911 call types. I classify 911 calls into seven mutually exclusive categories using the initial case type field: disturbance, domestic violence, suicide, suspicious activity, theft, traffic, and other.²³ I use 911 calls for an in-progress or recently occurred suicide or suicide attempt for comparison. I hypothesize that these suicide-related calls are 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 log outcomes. Standard errors are computed using the Newey-West method.

²³For additional information on the classification of 911 call types, please refer to Appendix C.

6.4.2 Results

Figure 11 shows the log of weekly 911 calls for the different 911 call types. Panel A includes 911 calls for disturbance, suspicious activity, and traffic, while Panel B includes 911 calls for theft, domestic violence, and all other matters. I include 911 calls for an in-progress or recently occurred suicide or suicide attempt 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 the log of 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 911 call types increased at similar rates during and after the investigation period. The two exceptions are 911 calls for disturbances during the post-investigation and consent decree periods, and 911 calls for 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 Department of Justice'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, including for less urgent matters such as disturbances. In this section, I provide further evidence supporting limited changes to civilian behavior during the federal investigation into the SPD. Specifically, 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 units 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 only available at the state level. Nonetheless, these are the best data available to me for assessing other changes in community behavior. I present summaries of these data in Figure 13.

Figure 13 presents measures derived from both NCVS and Gallup data. When using NCVS data, I compare the MSA encompassing Seattle with MSAs that do not include jurisdictions with federal investigations, while I compare Washington state with states that do not include jurisdictions with federal investigations when using Gallup data. In Panel 1A, I show the annual victimization rates for respondents to the NCVS. In Panel 1B, I show the annual non-reporting rates to the police for these victimizations. I do not find visual

evidence of an increase in crime victimizations or an increase in unreported victimizations to the police.

In Panel 2A, I present annual rate of non-reporting attributed to mistrust in the police. Here again, I do not find visual evidence that mistrust in the police is changing differently in the Seattle MSA than comparator MSAs during the investigation period. Finally, in Panel 2B, I show the annual share of Gallup poll 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 other states.

6.4.4 Effect on Monthly Fatal Crashes

Lastly, I use the synthetic control method and data from the National Highway Traffic Safety Administration to examine whether fatal crashes in Seattle changed differently than cities without police departments that were 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 less pronounced among Black, female, and more experienced officers, as well as among officers with lower arrest fixed effects in pre-period dispatches. Following the investigation's conclusion, officer-initiated stops steadily increased but remained slightly lower than pre-period levels in minority neighborhoods. Despite the substantial decrease in officer-initiated stops, no significant increases in serious crime were detected in neighborhoods that experienced larger reductions in stops. Furthermore, there were no noticeable differences in serious crime increases between Seattle and comparator jurisdictions. There is also 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 of the police may be beneficial for minority communities and that it may be possible to reduce certain

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 police departments following federal investigations. Several factors could contribute to these differences, including the magnitude of officer pullback and the specific types of activities that officers are pulling back on in investigated jurisdictions. My study demonstrates that a decrease in policing activity is not necessarily linked to an increase in crime. A roughly 26 percent reduction in officer-initiated 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 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 changes in policing activity during federal investigations can teach us about their effectiveness in reducing serious crimes.

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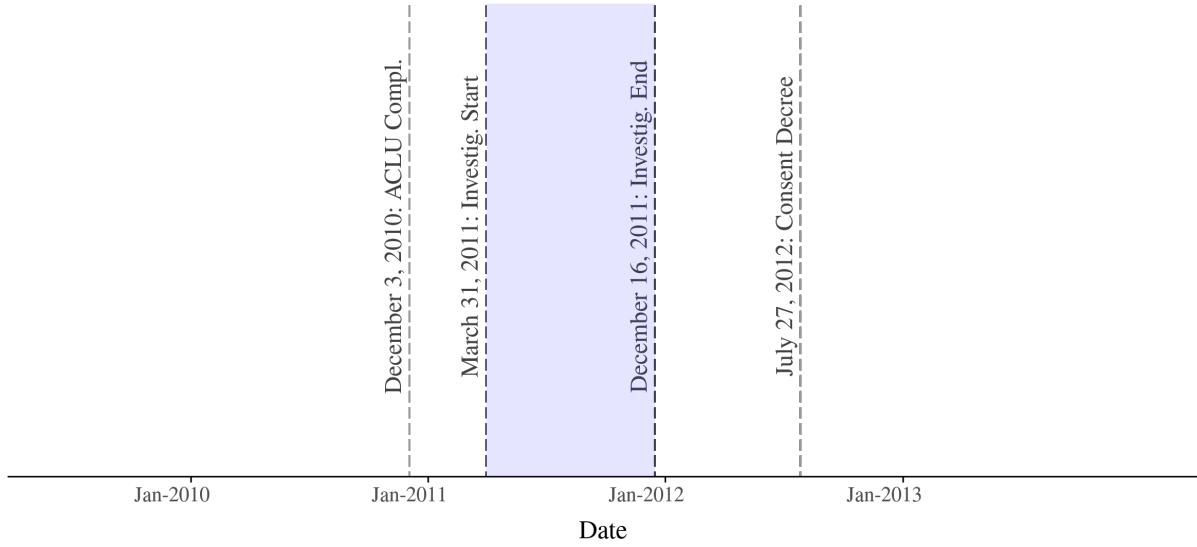
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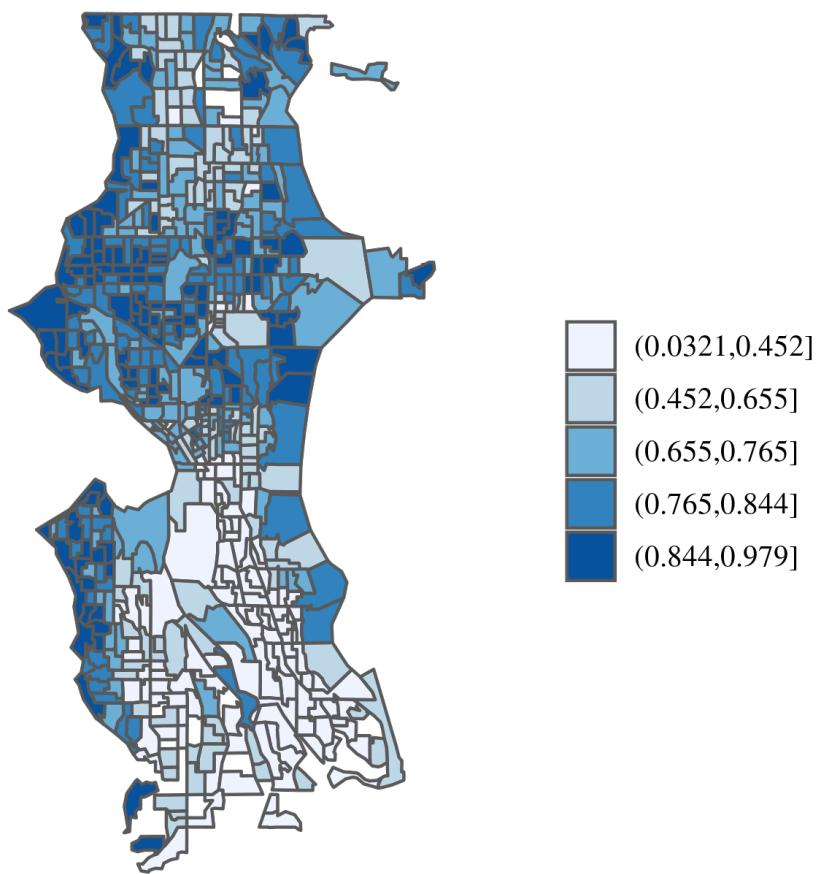
8 Figures

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.

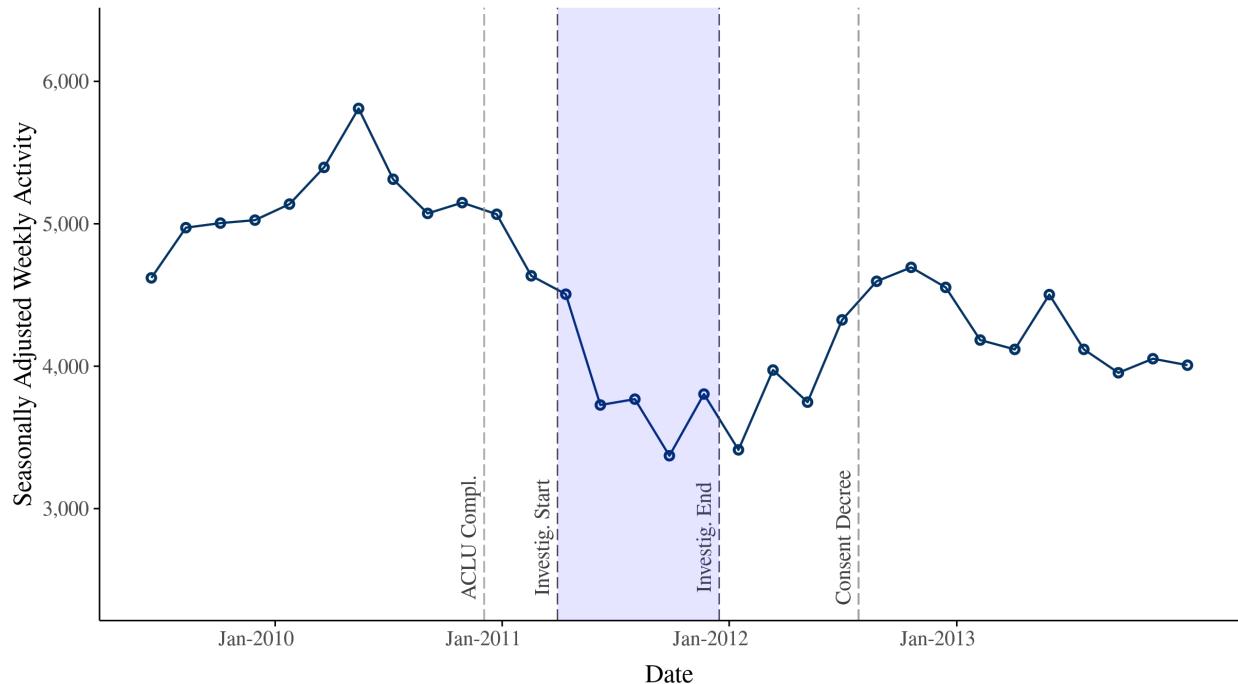
Figure 2: Neighborhood Racial Composition in Seattle



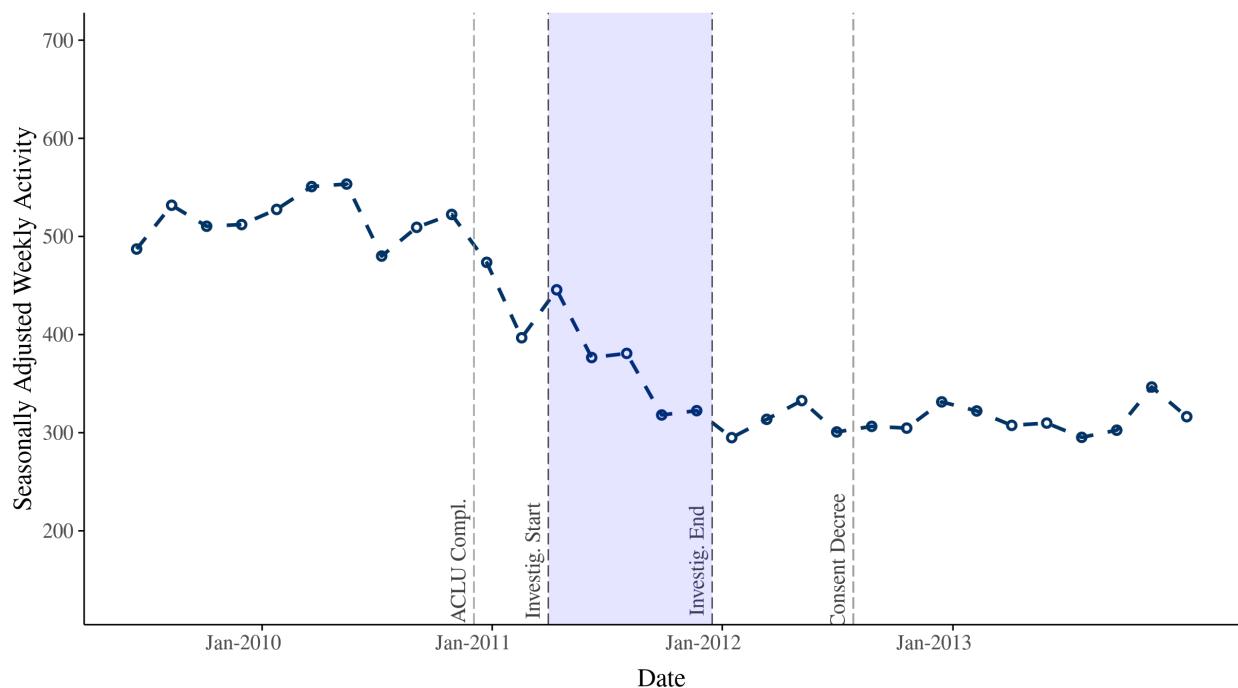
Notes: This figure plots the share of non-Hispanic White residents across neighborhoods in Seattle. Neighborhoods are defined as census block groups.

Figure 3: Weekly Officer-Initiated (OI) Activity

(A) Officer-Initiated Stops



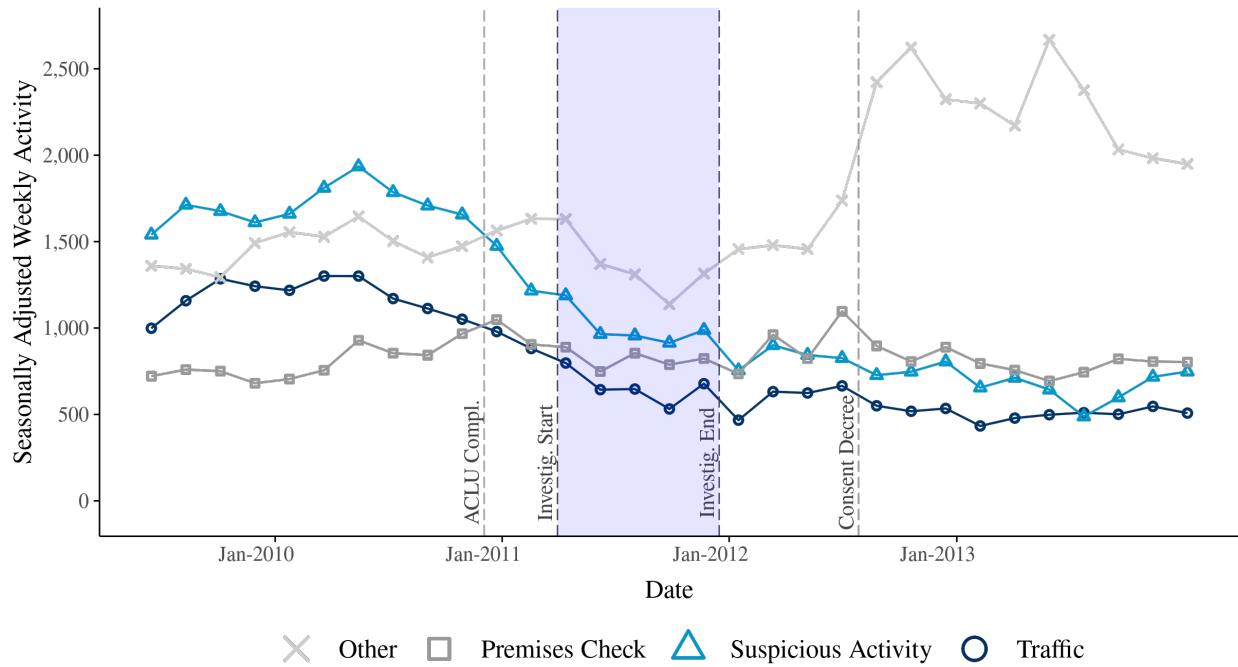
(B) Arrests from Officer-Initiated Stops



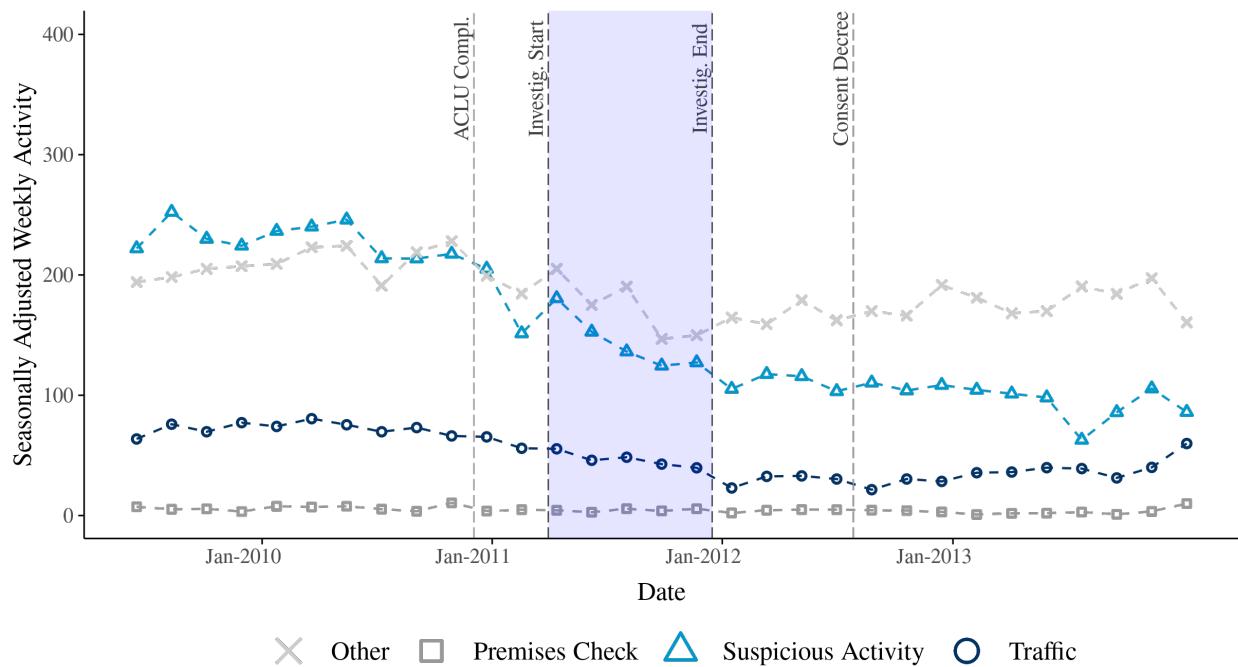
Notes: This figure plots seasonally adjusted weekly officer-initiated (OI) stops (Panel A) and arrests from OI stops (Panel B) from June 2009 to December 2013. To seasonally adjust values, I residualize by week-of-the-year fixed effects and add back the mean of the fixed effects.

Figure 4: Weekly OI Activity by Stop Type

(A) Officer-Initiated Stops

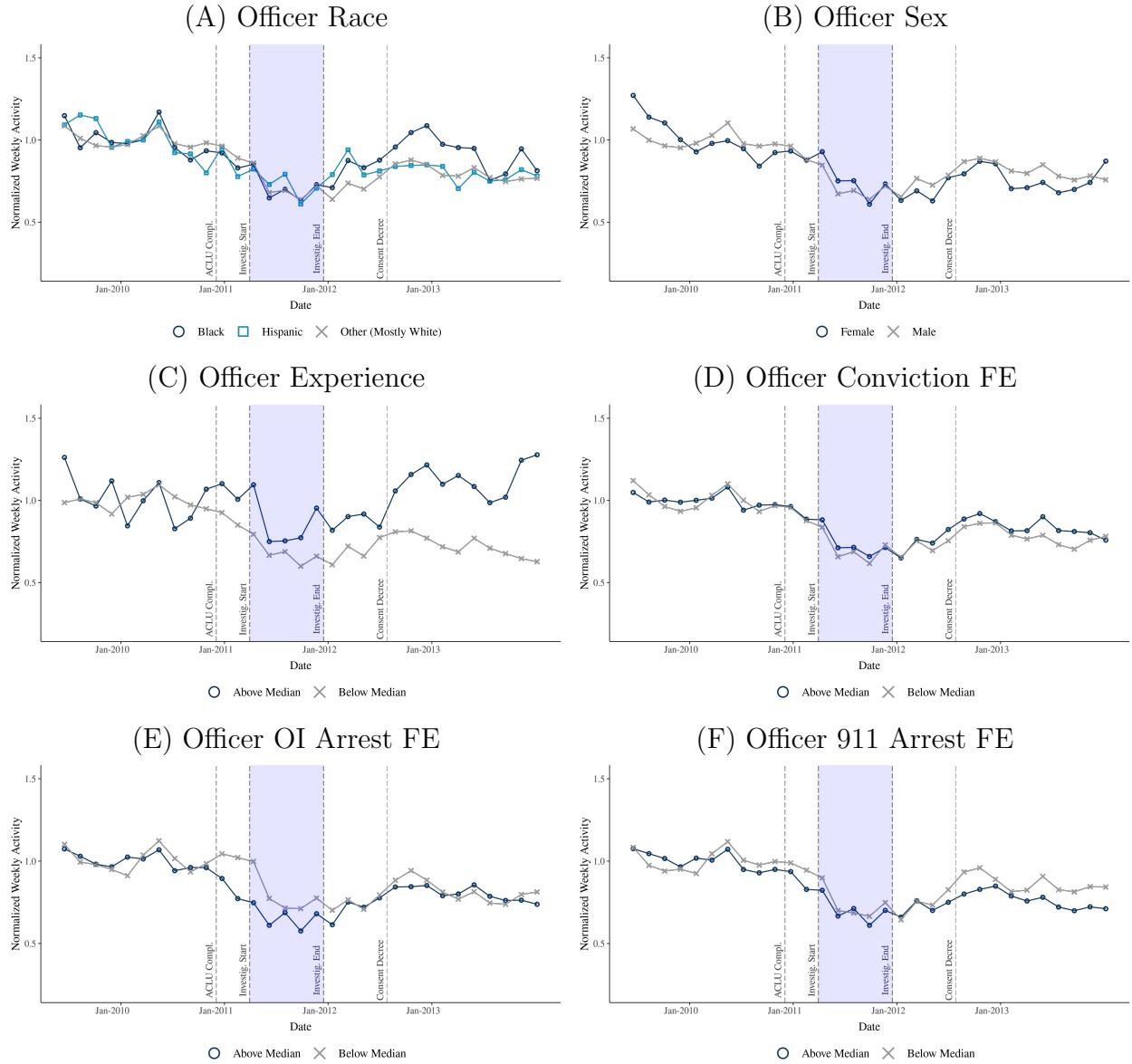


(B) Arrests from Officer-Initiated Stops



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 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.

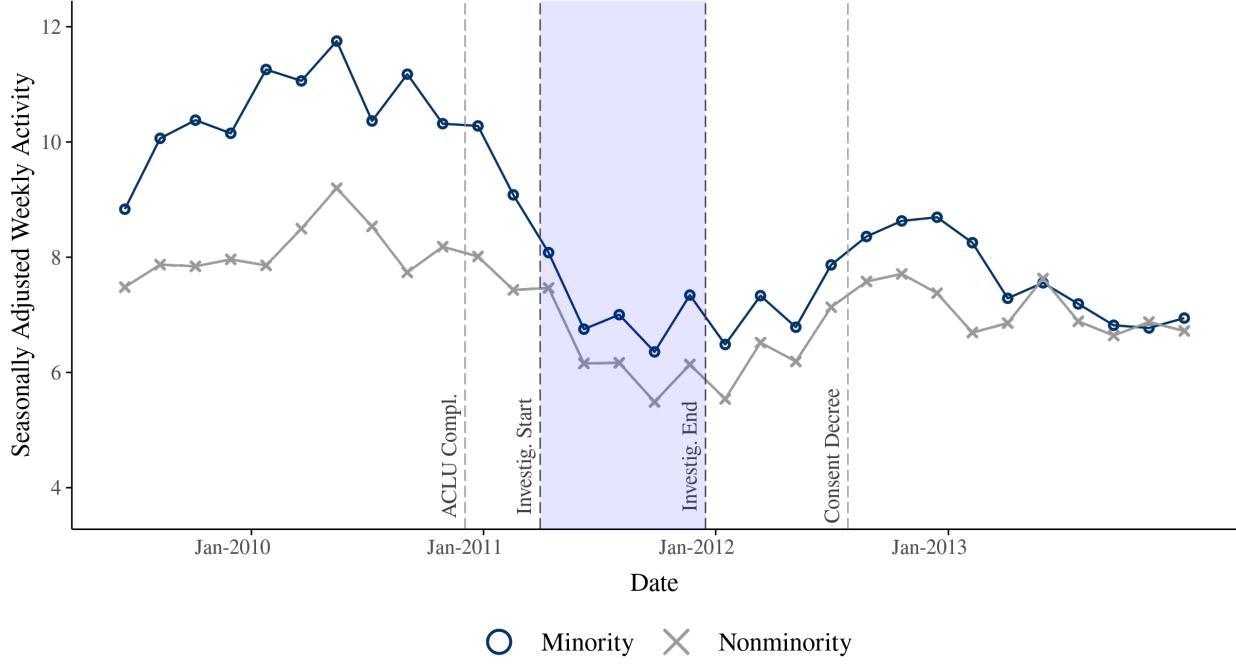
Figure 5: Weekly OI Stops by Officer Traits



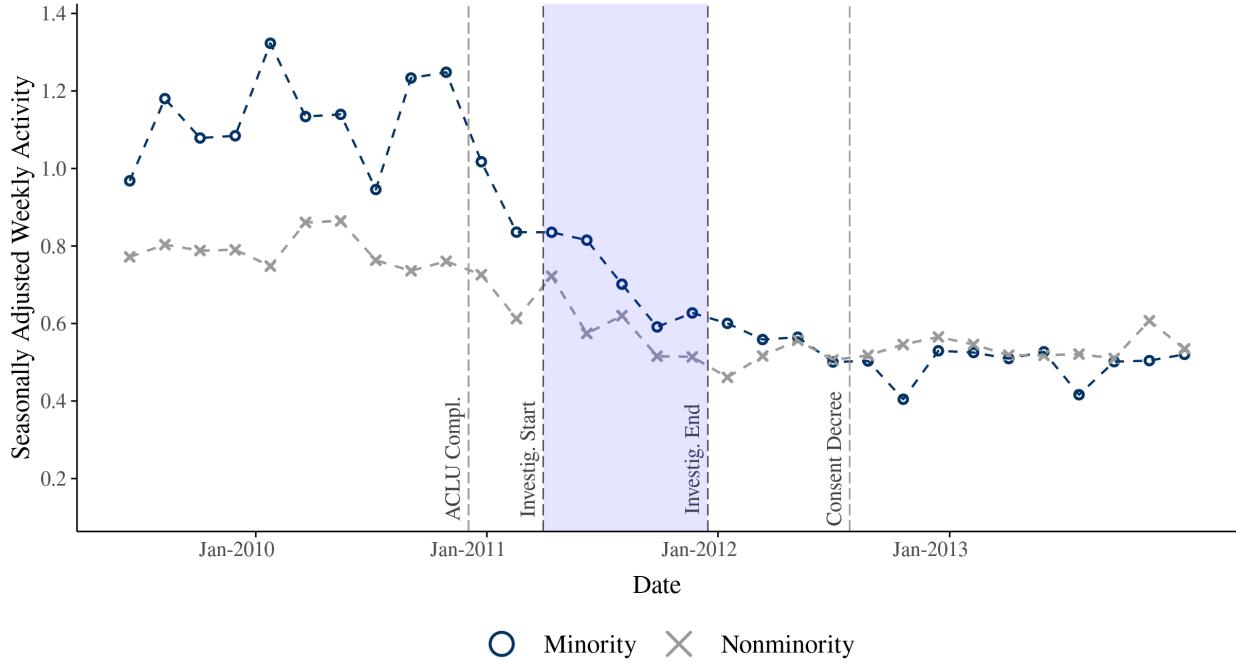
Notes: This figure plots normalized weekly officer-initiated (OI) stops from June 2009 to December 2013 for each officer trait described in Section 4. I normalize each series by its pre-period mean (e.g., the series for Black officers is divided by average weekly OI stops among Black officers in the pre-period).

Figure 6: Weekly OI Activity by Neighborhood Race

(A) Officer-Initiated Stops

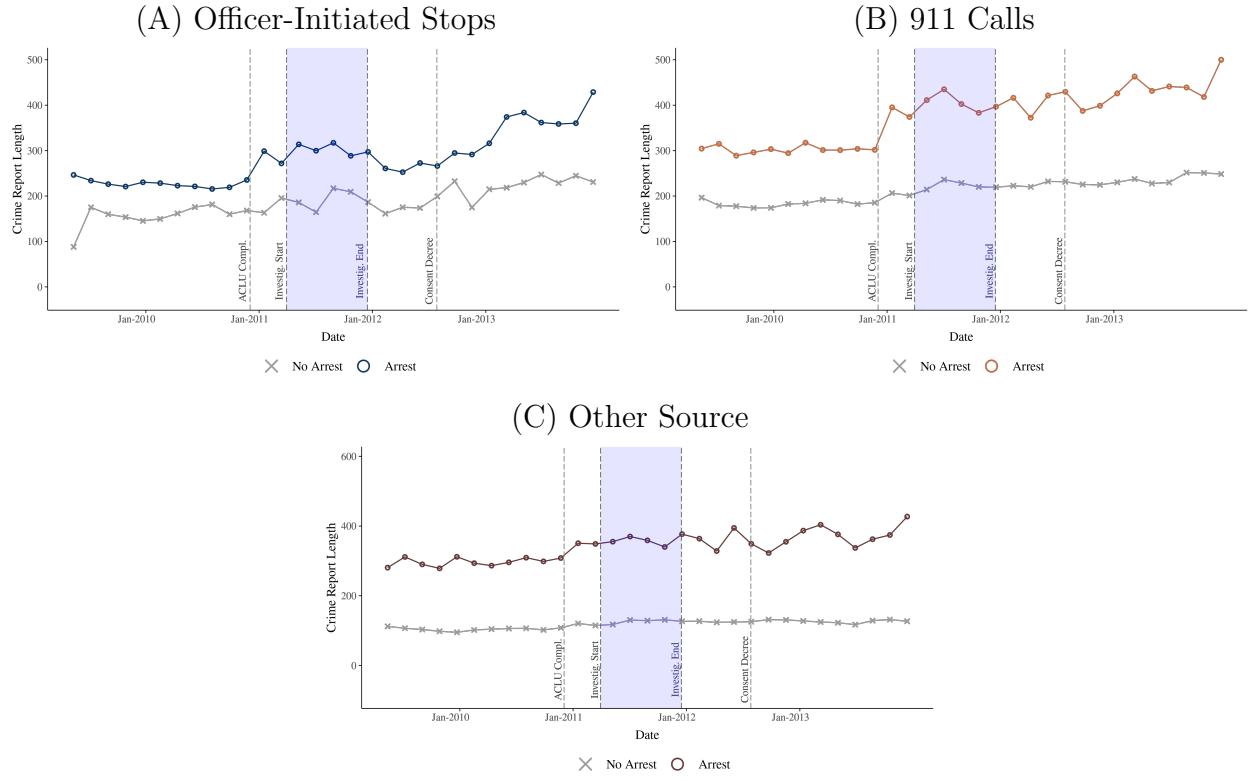


(B) Arrests from Officer-Initiated Stops



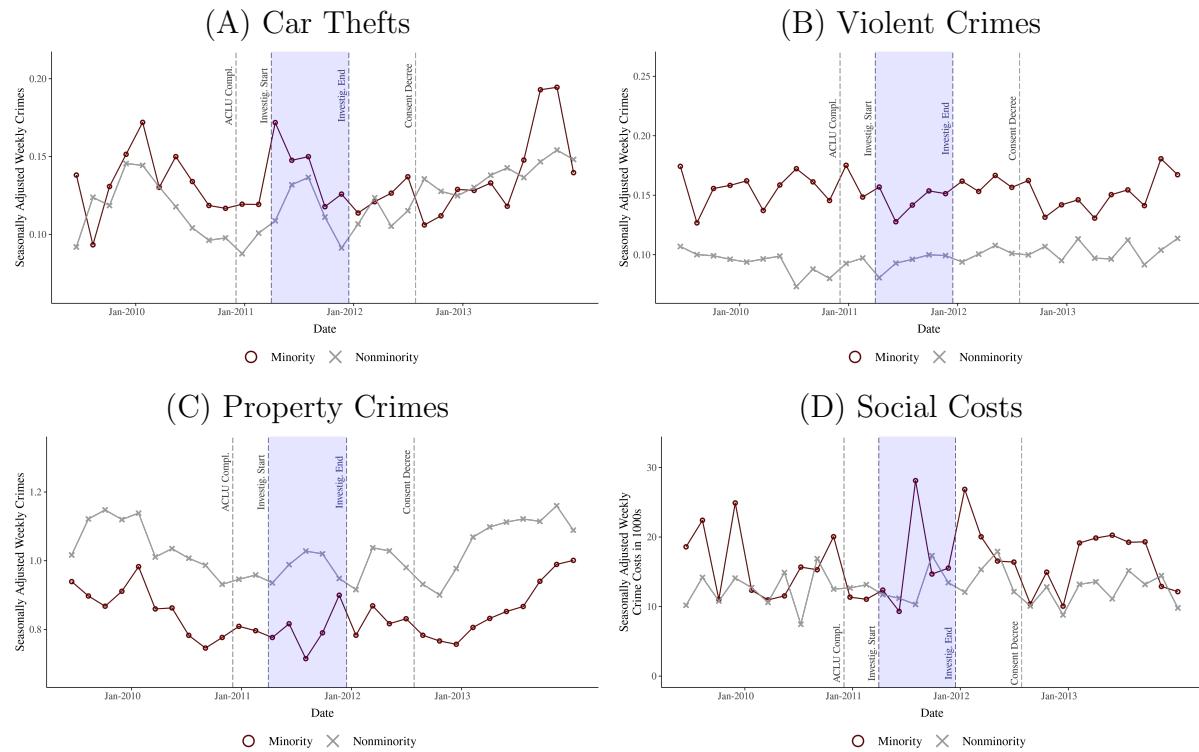
Notes: This figure plots seasonally adjusted weekly officer-initiated (OI) stops (Panel A) and arrests from OI stops (Panel B) from June 2009 to December 2013 for minority and non-minority neighborhoods. Minority neighborhoods are defined as census block groups with less than 50 percent non-Hispanic 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 7: Police Report Length by Crime Discovery Source



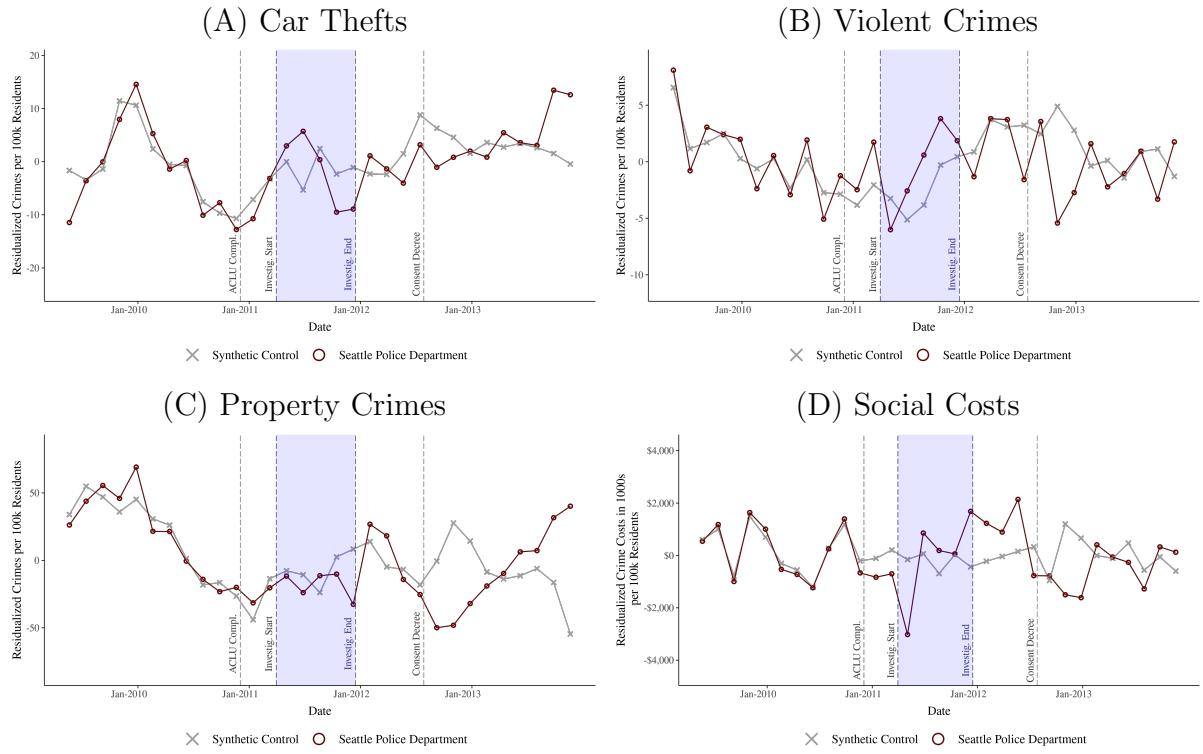
Notes: This figure plots the length, in words, of police reports from June 2009 to December 2013. Panel A shows report length for crimes discovered through officer-initiated (OI) stops. Panel B shows report length for crimes discovered via 911 calls, and Panel C shows report length for crimes discovered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

Figure 8: Weekly within-Seattle Crimes by Neighborhood Race



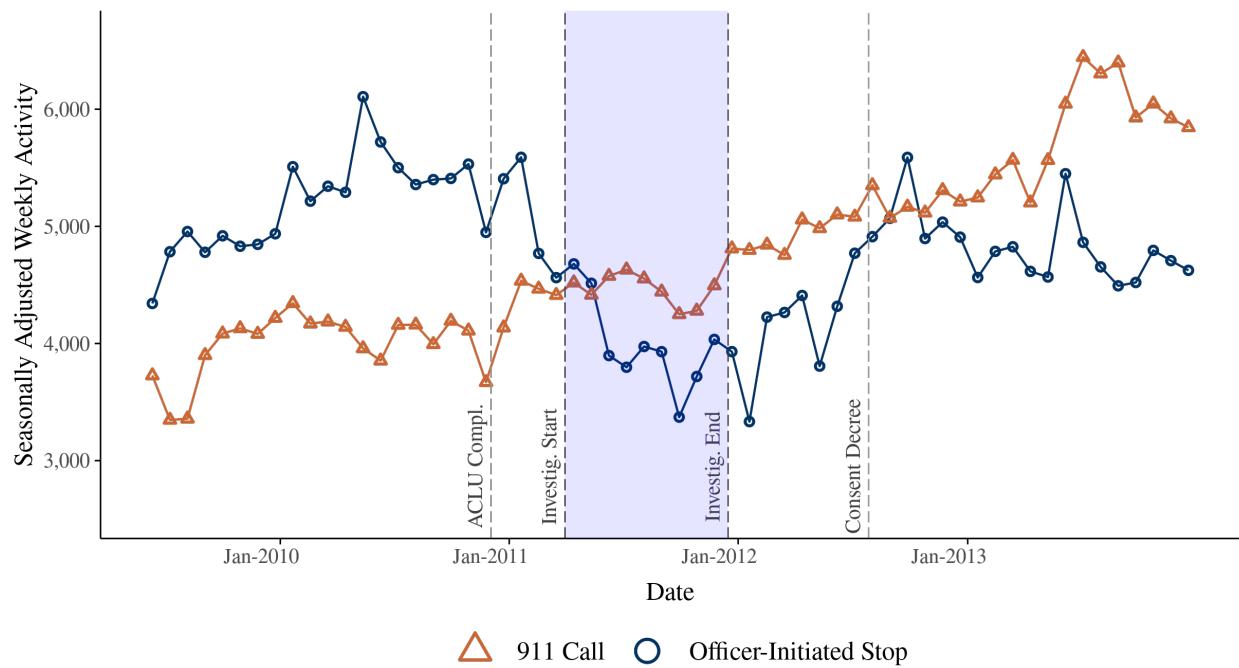
Notes: This figure plots seasonally adjusted weekly crimes for minority and non-minority neighborhoods from June 2009 to December 2013. Panel A reports car thefts, Panel B reports violent crimes, Panel C reports property crimes, and Panel D reports the social costs of index crimes in 1000s. To seasonally 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 100k Residents, Synthetic Control



Notes: This figure plots monthly crimes per 100 thousand 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 costs of index crimes in 1000s.

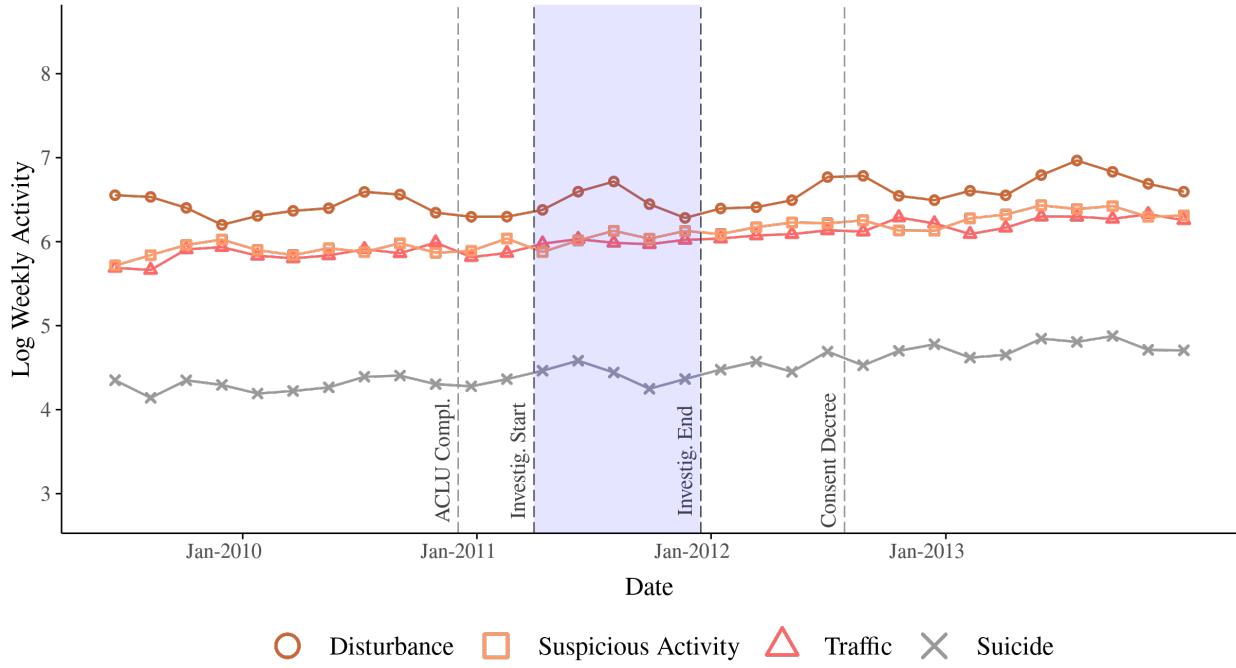
Figure 10: Weekly 911 Calls vs. OI Stops



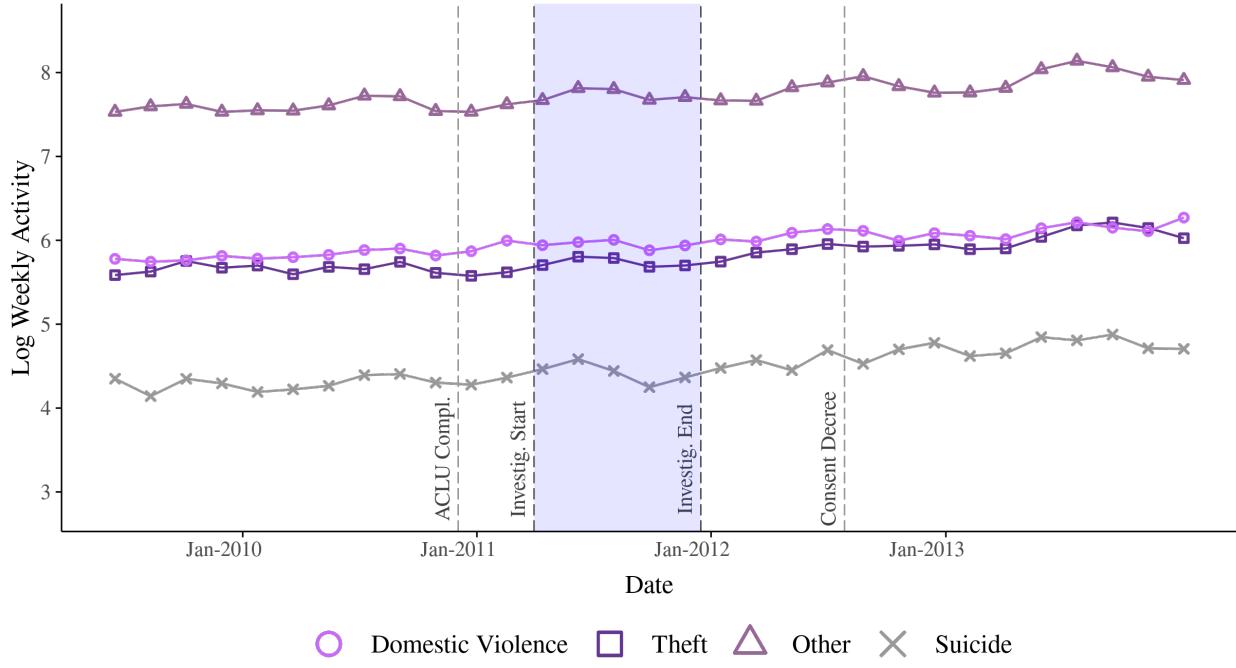
Notes: This figure plots seasonally adjusted weekly 911 calls and officer-initiated (OI) stops from June 2009 to December 2013. To seasonally adjust values, I residualize by week-of-the-year fixed effects and add back the mean of the fixed effects.

Figure 11: Weekly 911 Calls by Call Type

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

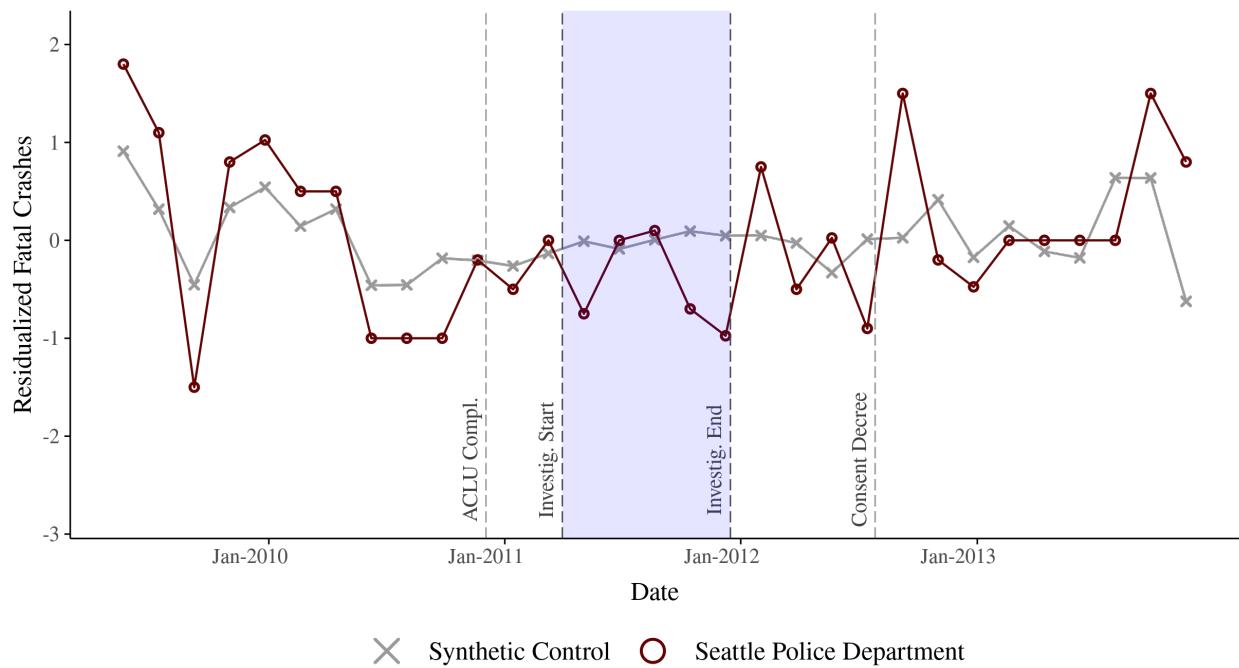


(B) Domestic Violence, Theft, and Other vs. Suicide Calls



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 suicide in both panels for comparison. To seasonally adjust values, I residualize by week-of-the-year fixed effects and add back the mean of the fixed effects.

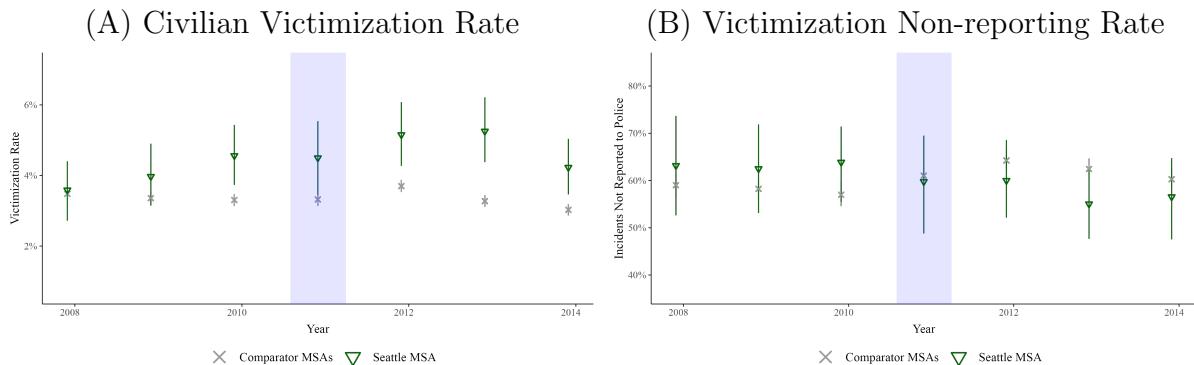
Figure 12: Monthly Fatal Crashes, Synthetic Control



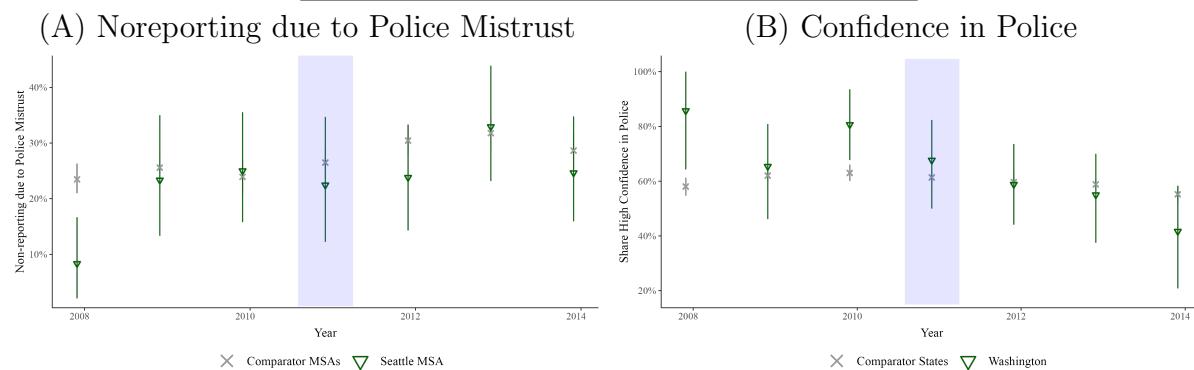
Notes: This figure plots monthly fatal crashes, residualized by month-of-the-year fixed effects, in Seattle compared to its synthetic control counterfactual from June 2009 to December 2013.

Figure 13: Other Community Responses

Panel 1: Victimization and Non-reporting over Time



Panel 2: Perceptions of the Police over Time



Notes: Panel 1A shows annual victimization rates from the National Crime Victimization Survey (NCVS). Panel 1B shows annual non-reporting rates to the police for these victimizations. Panel 2A shows annual rates of non-reporting attributed to mistrust in the police. Finally, Panel 2B shows the annual share of Gallup poll 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 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
<u>A. Weekly Dispatch Characteristics</u>				
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
<u>B. Weekly Reported Crime Characteristics</u>				
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 Costs in 1000s		8,169.67	3,462.49	239
Report Length		187.29	23.66	239
<u>C. Officer Characteristics</u>				
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
<u>D. Neighborhood (CBG) Characteristics</u>				
Non-minority	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 calculated using data from June 2009 to December 2013. Social costs in 1000s includes the social costs of all index crimes calculated using the 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 OI Activity

	OI Stops (1)	OI Arrests (2)	OI Arrest Rate (3)
Complaint	-334.54 (209.49)	-94.68*** (22.24)	-12.23*** (2.55)
Investigation	-1,373.81*** (117.66)	-154.01*** (15.15)	-5.59** (2.83)
Post-Investigation	-1,469.30*** (146.92)	-221.78*** (13.09)	-18.92*** (2.60)
Consent Decree	-857.94*** (99.32)	-208.18*** (9.30)	-27.88*** (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 weekly OI arrest rate, which I define as the number of arrests per 1000 OI stops. Newey-West standard errors are reported in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 3: Effect on Weekly OI Activity by Stop Type

Panel A: Traffic and Suspicious Activity Stops

	Traffic			Suspicious Activity		
	Stops (1)	Arrests (2)	Arrest Rate (3)	Stops (4)	Arrests (5)	Arrest Rate (6)
Complaint	-274.33*** (47.21)	-13.79*** (3.37)	0.01 (4.48)	-405.46*** (78.70)	-57.91*** (14.46)	-2.57 (6.52)
Investigation	-545.68*** (30.15)	-26.66*** (2.79)	9.79** (4.07)	-731.12*** (33.10)	-87.46*** (7.33)	8.40* (4.68)
Post-Investigation	-604.87*** (44.01)	-47.36*** (2.81)	-18.06*** (3.42)	-902.35*** (48.79)	-123.77*** (7.30)	-4.31 (4.39)
Consent Decree	-681.71*** (25.29)	-38.86*** (2.43)	5.84 (3.86)	-1,044.98*** (32.28)	-134.16*** (5.86)	7.22** (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 (1)	Arrests (2)	Arrest Rate (3)	Stops (4)	Arrests (5)	Arrest Rate (6)
Complaint	218.51*** (62.12)	-2.00* (1.05)	-4.02*** (1.40)	126.75** (55.73)	-20.97** (9.87)	-19.94*** (5.05)
Investigation	17.62 (35.40)	-2.00* (1.03)	-2.45** (1.10)	-114.63** (45.42)	-37.90*** (9.29)	-17.26*** (5.03)
Post-Investigation	107.71* (55.79)	-2.92*** (0.82)	-4.06*** (0.97)	-69.78 (58.23)	-47.72*** (7.23)	-24.00*** (4.63)
Consent Decree	19.74 (31.64)	-3.31*** (0.70)	-4.02*** (0.81)	849.01*** (65.94)	-31.86*** (6.07)	-62.64*** (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 officer-initiated 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 weekly OI arrest rate, which I define as the number of arrests per 1000 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 OI 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	OI Arrest FE	High vs. Low 911 Arrest FE
		(1)	(2)	(3)	(4)	(5)	(6)	(7)
Officer Trait × Complaint		0.10 (0.16)	-0.44 (0.27)	0.07 (0.14)	0.00*** (0.11)	-0.12 (0.11)	-0.99*** (0.11)	-0.53*** (0.12)
Officer Trait × Investigation		0.54*** (0.12)	-0.12 (0.22)	0.48*** (0.11)	2.15*** (0.08)	-0.13 (0.08)	-0.92*** (0.08)	-0.64*** (0.08)
Officer Trait × Post-Investigation		0.92*** (0.13)	0.57** (0.28)	-0.09 (0.12)	2.30*** (0.09)	-0.17* (0.09)	-0.56*** (0.09)	-0.63*** (0.09)
Officer Trait × Consent Decree		1.01*** (0.11)	-0.27 (0.18)	-0.06 (0.10)	2.23*** (0.07)	0.09 (0.07)	-0.21*** (0.07)	-0.84*** (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 FE _s	X		X	X	X	X	X	X
Home-Sector-Calendar-Week FE _s	X	X	X	X	X	X	X	X

Notes: This table reports the results of estimating Equation 5.3 on an unbalanced weekly officer 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 effect, Column 6 for OI arrest fixed effect, and Column 7 for 911 arrest fixed effect. Newey-West standard errors are reported in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 5: Testing for Equal Proportional Effect on Weekly OI Stops by Officer Traits

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

Notes: This table reports the results of estimating Equation 5.3 on an unbalanced weekly officer 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 effect, Column 6 for officer OI arrest fixed effect, and Column 7 for 911 arrest fixed effect. Newey-West standard errors are reported in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 6: Effect on Weekly OI Stops by Neighborhood Race

	OI Stops (1)	OI Arrests (2)	OI Arrest Rate (3)
Minority × Complaint	-0.56 (0.36)	-0.09 (0.07)	5.18 (4.98)
Minority × Investigation	-1.61*** (0.26)	-0.22*** (0.05)	9.48** (4.11)
Minority × Post-Investigation	-1.58*** (0.29)	-0.32*** (0.05)	1.58 (4.24)
Minority × Consent Decree	-1.87*** (0.26)	-0.39*** (0.04)	-5.67* (3.10)
Complaint	-0.43*** (0.16)	-0.14*** (0.03)	-7.18*** (2.53)
Investigation	-1.93*** (0.12)	-0.21*** (0.02)	-4.47** (1.79)
Post-Investigation	-2.10*** (0.17)	-0.30*** (0.03)	-15.38*** (2.05)
Consent Decree	-0.00*** (0.14)	-0.26*** (0.02)	-9.37*** (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 weekly neighborhood 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 weekly OI arrest rate, which I define as the number of arrests per 1000 OI stops. Newey-West standard errors are reported in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 7: Testing for Equal Proportional Effect on Weekly OI Activity by Neighborhood Race

	OI Stops (1)	OI Arrests (2)	OI Arrest Rate (3)
Minority × Complaint	-0.49 (0.37)	-0.07 (0.07)	13.14*** (5.05)
Minority × Investigation	-1.15*** (0.26)	-0.15** (0.06)	16.40*** (4.18)
Minority × Post-Investigation	-1.12*** (0.31)	-0.20*** (0.05)	10.22** (4.28)
Minority × Consent Decree	-2.35*** (0.30)	-0.30*** (0.05)	3.35 (3.13)
Investigation	-0.36* (0.19)	-0.03 (0.03)	20.48*** (2.54)
Complaint	-0.19 (0.23)	-0.09** (0.04)	20.51*** (3.52)
Post-Investigation	-0.54** (0.27)	-0.03 (0.03)	13.87*** (2.80)
Consent Decree	-2.64*** (0.25)	-0.06** (0.03)	22.76*** (2.29)
Pre-period mean × Complaint	-0.03 (0.03)	-0.07 (0.06)	-0.52*** (0.05)
Pre-period mean × Investigation	-0.19*** (0.03)	-0.22*** (0.06)	-0.46*** (0.04)
Pre-period mean × Post-Investigation	-0.19*** (0.04)	-0.34*** (0.04)	-0.54*** (0.04)
Pre-period mean × Consent Decree	0.20*** (0.04)	-0.25*** (0.04)	-0.59*** (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 weekly neighborhood 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 weekly OI arrest rate, which I define as the number of arrests per 1000 OI stops. 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 (1)	911 Calls (2)	OI Stops (3)	Other Source (4)
Arrest	113.24*** (3.91)	113.89*** (3.96)	74.89*** (7.50)	124.05*** (6.43)
Arrest × Complaint	41.56*** (5.83)	44.55*** (7.26)	36.11*** (8.79)	41.99*** (13.89)
Arrest × Investigation	46.05*** (4.54)	57.29*** (6.21)	33.27*** (8.41)	39.32*** (8.48)
Arrest × Post-Investigation	38.75*** (4.28)	52.22*** (5.76)	16.43** (7.46)	46.21*** (10.16)
Arrest × Consent Decree	77.12*** (8.99)	81.91*** (6.96)	61.44*** (17.82)	60.88*** (10.13)
Complaint	15.32*** (1.51)	22.20*** (2.60)	13.19* (6.86)	11.06*** (1.65)
Investigation	30.29*** (1.77)	38.86*** (2.53)	27.37*** (6.45)	24.17*** (1.68)
Post-Investigation	33.84*** (1.38)	42.31*** (2.10)	24.57*** (6.58)	26.39*** (1.52)
Consent Decree	44.23*** (1.54)	54.83*** (1.60)	46.57*** (6.32)	29.00*** (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 (1)	Violent (2)	Property (3)	Social Costs (4)
Minority × Complaint	0.010 (0.009)	0.008 (0.009)	0.032 (0.031)	-5.152** (2.470)
Minority × Investigation	0.011 (0.007)	-0.011 (0.007)	0.009 (0.025)	-0.320 (3.085)
Minority × Post-Investigation	-0.006 (0.008)	-0.001 (0.008)	0.012 (0.025)	0.489 (4.127)
Minority × Consent Decree	-0.013** (0.006)	-0.015** (0.006)	-0.013 (0.020)	0.110 (2.486)
Complaint	-0.029*** (0.005)	0.002 (0.004)	-0.117*** (0.018)	0.389 (1.966)
Investigation	0.001 (0.004)	0.001 (0.003)	-0.065*** (0.013)	0.241 (1.399)
Post-Investigation	-0.010** (0.004)	0.010*** (0.004)	-0.070*** (0.015)	3.211* (1.758)
Consent Decree	0.019*** (0.003)	0.009*** (0.003)	-0.000 (0.011)	-0.239 (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 ?? on a balanced weekly neighborhood panel spanning June 2009 to December 2013. The unit of observation is a neighbor calendar week. Column 1 reports the results for weekly car thefts, Columns for weekly violent crimes, Column 3 for weekly property crimes (excluding car thefts), and Column 4 for weekly social costs of index crimes in 1000s. Newey-West standard errors are reported in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 10: Implied Weekly Crimes per 1000 Officer-Initiated Stops Averted

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

Notes: This table reports the implied estimates for crimes per 1000 officer-initiated stops averted in minority neighborhoods based on the estimates in Tables 6 and 9. I report the 95% credible interval in square brackets, which I construct by performing Bayesian bootstrap across neighborhoods with 1000 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 100k Residents

	Car Theft (1)	Violent (2)	Property (3)	Social Costs (4)
Seattle x Post	-0.05 (0.55)	0.14 (0.63)	0.16 (1.71)	-5.64 (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 thousand 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 Weekly 911 Calls by Call Type

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

Week-of-Year FEs

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 the log of weekly 911 calls of that 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 Fatal Crashes

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

Notes: This table reports the estimates on 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 principal chooses S^* and G^* to maximize:

$$V = -c_m \delta_m S - c_g G - \beta (\Theta - AS^\gamma - G^\rho)^\tau$$

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

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

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

Second Order Conditions.

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

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

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

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:

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

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 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 officer-initiated (OI) stops conditional on stop characteristics. To construct this measure, I use an officer-dispatch level data set containing all officer-initiated stops between June 2009 and the investigation launch to estimate the following ordinary least squares specification:

$$Arrest_{i,j,t} = X_{i,j,t} + \omega_j + \epsilon_{i,j,t} \quad (B.1)$$

where $Arrest_{i,j,t}$ is an indicator for whether officer-initiated 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, an officer may have a high OI arrest fixed effect 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:

$$Arrest_{i,j,t} = X_{i,j,t} + \eta_j + \mu_{i,j,t} \quad (\text{B.2})$$

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:

$$Conviction_{i,j,t} = X_{i,j,t} + \theta_j + \sigma_{i,j,t} \quad (\text{B.3})$$

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, high conviction fixed effect officers 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.

C Officer-Initiated Stop and 911 Call Type Classification

C.1 Officer-Initiated Stops

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

Premises Check: Premise Check Officer Initiated Onview Only.

Suspicious Activity: Suspicious Package; Suspicious Person Vehicle Or Incident; Sus-

picious 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; Car-jacking 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 Srv 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.

C.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.

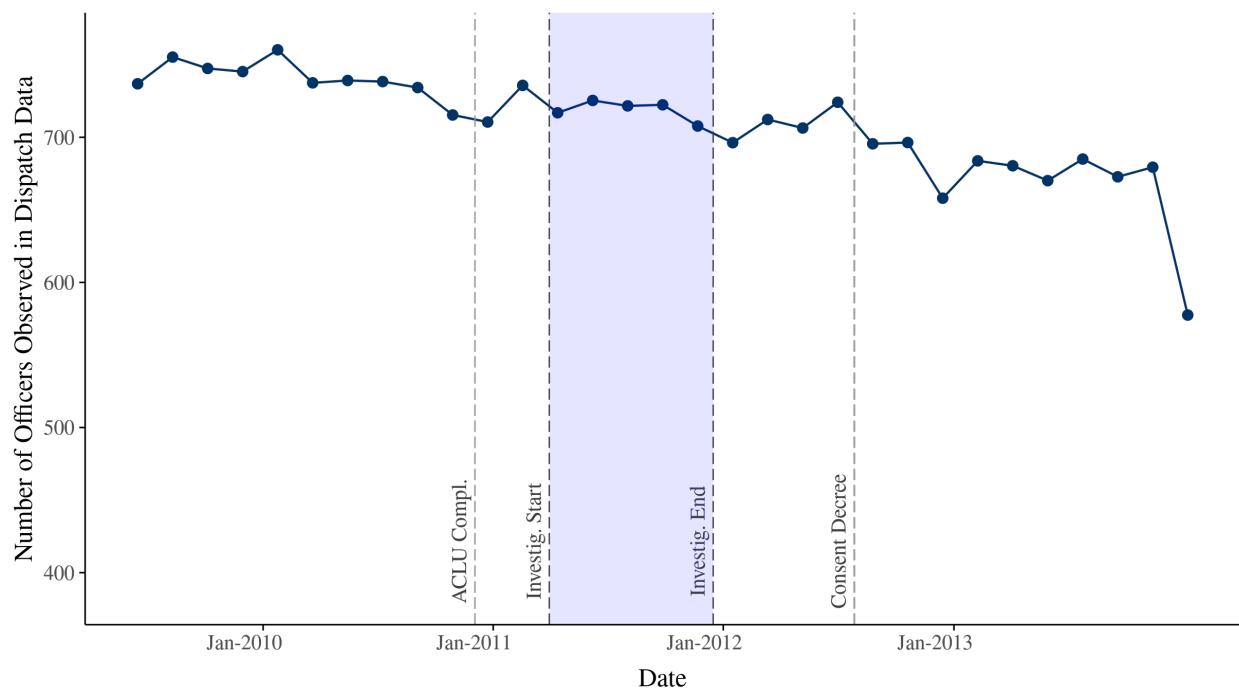
Traffic: Acc Non Injury Blocking; Acc With Injuries Includes Hit And Run; Traffic Blocking Roadway; Acc Report Non Inj Non Blkg Or After Fact Inj; Acc Hit And Run No Injuries Includes Ip Jo; Tru Acc Hit And Run; Traffic Bo Signals And Down Signs; Traffic Assist Motorist; Traffic Moving Violation; Acc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

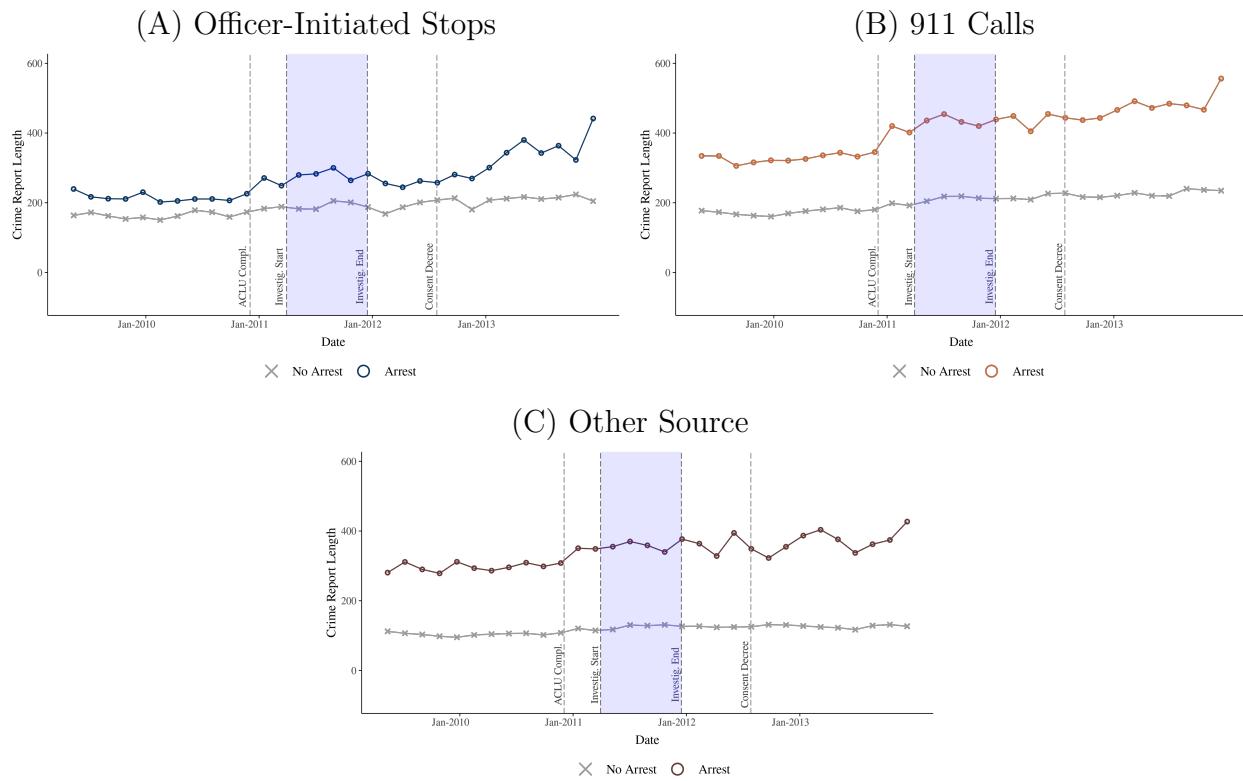
D Appendix Figures

Figure D1: Weekly Number of Officers Observed in Dispatch Data



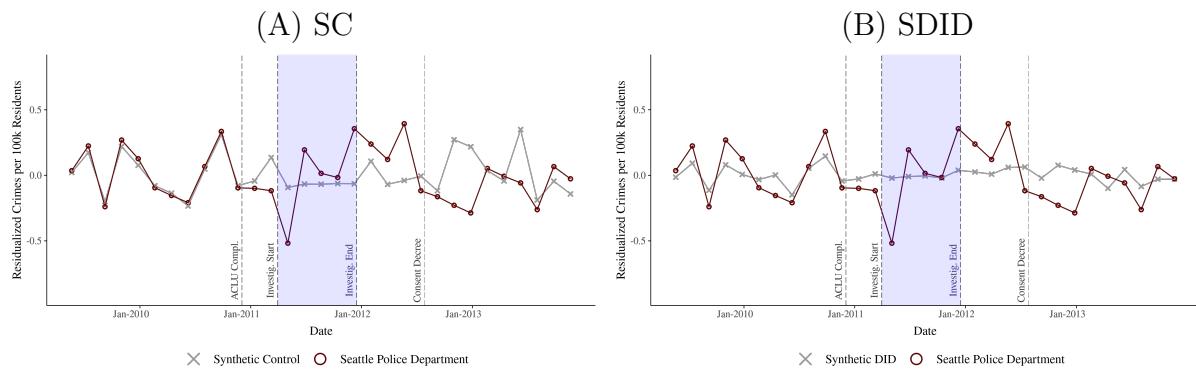
Notes: This figure plots the weekly number of officers observed in the computer-aided dispatch (CAD) data from June 2009 to December 2013.

Figure D2: Unadjusted Police Report Length by Crime Discovery Source



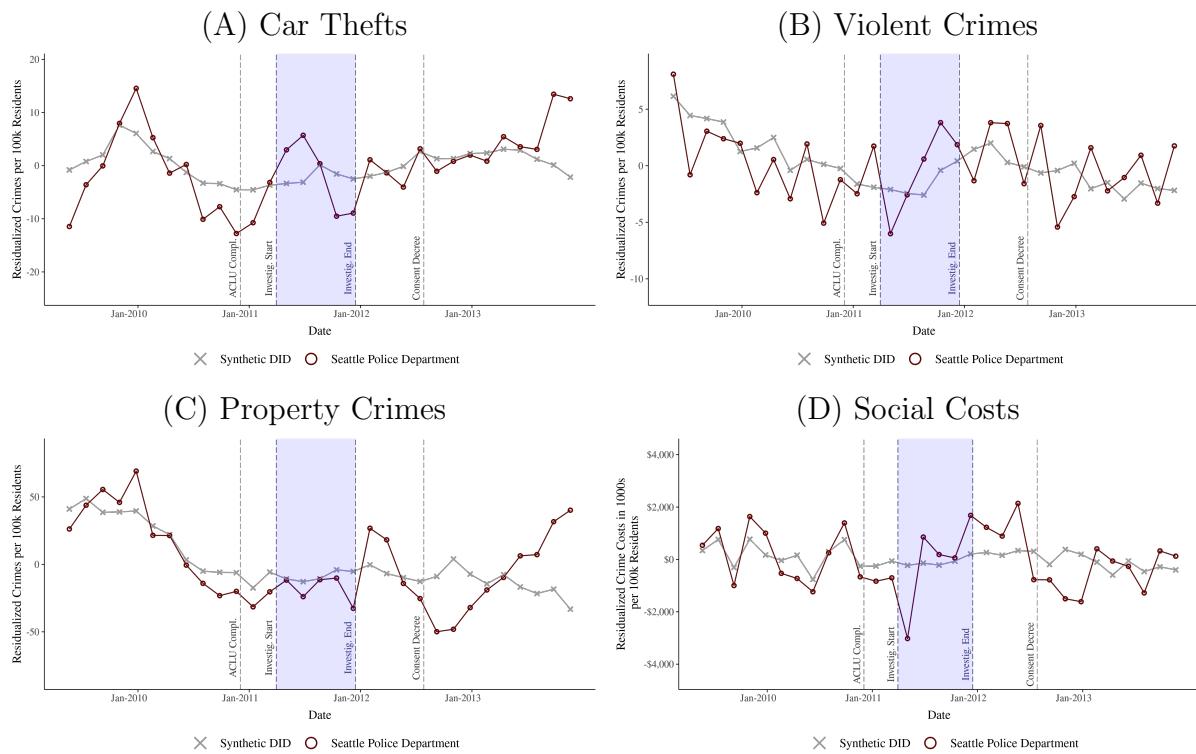
Notes: This figure plots the length, in words, of police reports from June 2009 to December 2013. Panel A shows report length for crimes discovered through officer-initiated (OI) stops. Panel B shows report length for crimes discovered via 911 calls, and Panel C shows report length for crimes discovered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

Figure D3: Monthly Homicides per 100k Residents



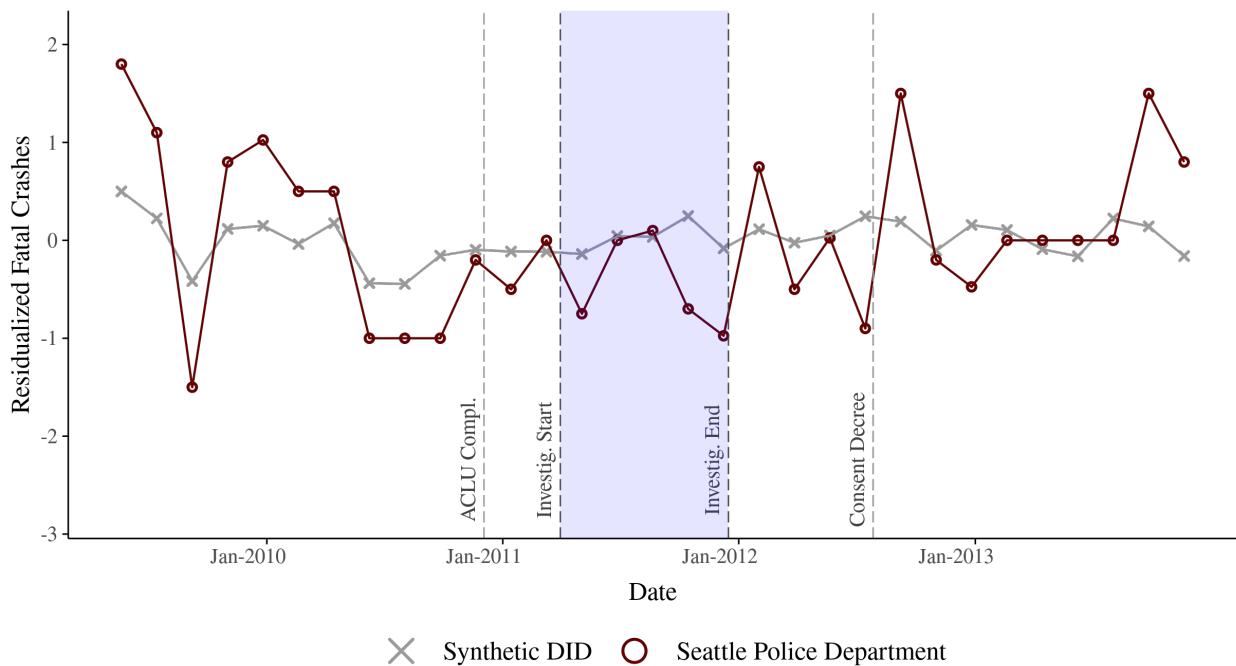
Notes: These figures plots monthly homicides per 100 thousand residents, residualized by month-of-the-year fixed effects, in Seattle compared to its synthetic control counterfactual in Panel A and compared to its synthetic difference-in-differences counterfactual in Panel B.

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



Notes: These figures plots monthly crimes per 100 thousand residents, residualized by month-of-the-year fixed effects, in Seattle compared to its synthetic difference-in-differences counterfactual. Panel A reports for car thefts, Panel B reports for violent crimes, Panel C for property crimes, and Panel D reports for the social costs of these crimes in 1000s.

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 (1)	Black (2)	Hispanic (3)	Female (4)
Share Minority	0.01 (0.02)	0.01 (0.01)	-0.00 (0.01)	-0.02 (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 (0.04)	0.01 (0.03)	-0.03* (0.02)	-0.01 (0.03)
Share Less Than HS	-0.08* (0.04)	-0.02 (0.02)	-0.01 (0.01)	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 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 Costs 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 estimated social costs 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 between officer traits. For more information on each trait, please refer to Section 4 and Appendix B.

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

Panel B: Premises Check and Other Stops				
	Premises Check		Other	
	Stops (1)	Arrests (2)	Stops (3)	Arrests (4)
Complaint	0.26*** (0.07)	-0.37* (0.20)	0.06* (0.03)	-0.10** (0.05)
Investigation	0.02 (0.04)	-0.37* (0.21)	-0.07*** (0.03)	-0.20*** (0.05)
Post-Investigation	0.12** (0.06)	-0.61*** (0.20)	-0.06* (0.03)	-0.26*** (0.04)
Consent Decree	0.02 (0.04)	-0.70*** (0.16)	0.45*** (0.03)	-0.16*** (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 of the four officer-initiated stop types 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 Clustered Standard Errors

	OI Stops (1)	OI Arrests (2)	OI Arrest Rate (3)
Minority × Complaint	-0.56 (0.62)	-0.09 (0.08)	5.03 (5.13)
Minority × Investigation	-1.61* (0.83)	-0.22 (0.14)	9.62** (4.77)
Minority × Post-Investigation	-1.58* (0.82)	-0.32** (0.15)	1.64 (5.09)
Minority × Consent Decree	-1.87* (0.96)	-0.39** (0.16)	-5.65 (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 weekly neighborhood 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 (1)	SDID (2)
Seattle x Post	-0.01 (0.06)	-0.01 (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 (1)	Violent (2)	Property (3)	Social Costs (4)
Seattle x Post	5.22 (9.60)	4.50 (6.26)	-4.16 (35.62)	116.19 (1,410.69)
Pre-period mean	49.41	50.45	430.33	5,910.07

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