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

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January 2025

Abstract

Policing reform advocates suggest that increased oversight will improve policing quality, but opponents fear that officers subjected to increased scrutiny will reduce crucial policing, compromising public safety. I examine the impact of a 2011 federal investigation into the Seattle Police Department on policing behavior and public safety. In response to the heightened scrutiny, officers immediately and significantly reduced stops, particularly traffic and suspicious-activity stops, in minority neighborhoods. Despite these stop reductions, I find no detectable effect on public safety, implying that the forgone stops offered minimal crime-reducing benefits beyond police presence. Federal oversight may lower the social costs of policing.

1 Introduction

Recent high-profile incidents of police violence, particularly those involving Black men, have renewed a long-standing public discourse on police reform in the United States (Cox, Cunningham and Ortega, 2024). Police–civilian interactions such as stops, arrests, or uses of force can harm the well-being of both the civilians directly involved and surrounding communities (Ang, 2021; Finlay, Mueller-Smith and Street, 2023). Similarly, police violence can erode public trust, undermine the legitimacy of law enforcement, and jeopardize civilian engagement with governmental and political institutions, especially in minority communities (Ang and Tebes, 2021; Owens and Ba, 2021). While ample evidence shows the crime-deterrent value of increased police presence (Di Tella and Schargrodsky, 2004; Draca, Machin and Witt, 2011; Evans and Owens, 2007), it remains an open question how policymakers can minimize the potential harms of policing without sacrificing its deterrence value.

Oversight of the police is an active area of policy discussion.[1] Proponents argue that increased oversight will improve policing quality. Opponents, however, fear that officers subjected to increased scrutiny will reduce essential policing activities, potentially compromising public safety. The existing oversight literature generally supports the concern that when oversight reduces policing activity, crime likely increases (Ba and Rivera, 2024; Devi and Fryer Jr, 2020; Premkumar, 2019; Prendergast, 2021; Shi, 2009). However, recent work suggests that oversight does not always reduce policing activity (Ba and Rivera, 2024; Devi and Fryer Jr, 2020). Still, it is unclear whether oversight can reduce policing activity without simultaneously increasing crime. A key aspect of this debate is that, while we know that having more police officers reduces crime, we know relatively little about which specific policing activities reduce crime and which can be scaled back without negative consequences (Cho, Gonçalves and Weisburst, 2024).

In this paper, I examine the impact of a 2011 federal investigation into the Seattle Police Department (SPD) on policing behavior and public safety. Such "Pattern or Practice" investigations are the primary means for the federal government to provide external oversight of law enforcement agencies and combat unconstitutional policing.[2] Using novel granular SPD administrative data, I analyze the effect of a federal investigation that occurred before the Black Lives Matter movement in a setting without significant public outrage. I differentiate between phases of the investigation to isolate the impact of increased scrutiny from

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[1]For example, oversight is one of the six pillars of 21$^{\text{st}}$ Century Policing (President's Task Force on 21st Century Policing, 2015).

[2]Since the passage of the 1994 Violent Crime Control and Law Enforcement Act, the Department of Justice (DOJ) has conducted over 70 investigations into local police for unconstitutional behavior, often after high-profile incidents of police misconduct. Recent reform proposals also aim to expand the DOJ's investigative authority and establish a grant program for state attorneys general to conduct similar investigations.

the investigation from effects driven by any public outrage related to the events preceding the investigation or reforms implemented after the investigation. I show that, in response to heightened scrutiny from the investigation, officers reduced weekly stops by 24% with no detectable effect on reported serious crime.[3] I also demonstrate that stop reductions were chiefly among traffic and suspicious-activity stops, which may be key to understanding why these reductions did not increase crime. These findings highlight variation in the crime-reducing efficacy of different policing activities, and suggest that oversight that reduces low-efficacy activities is unlikely to increase crime and may lower the social cost of policing, potentially benefiting minority communities.[4]

Estimating the causal relationship between policing activity and crime often is difficult because researchers frequently lack data on policing activity, and changes in local policing activity often are determined by changes in local crime. My study examines the Seattle Police Department, one of the largest police departments in the United States. Although my study examines a single police department and leverages across-neighborhood variation in treatment, my setting offers two key advantages. First, detailed administrative data from the SPD allow me to track changes in policing activities by time and location, overcoming the typical data challenges in this area and allowing me to link changes in different policing activities to changes in public safety. Second, I address identification challenges by exploiting the timing of a federal investigation into the SPD. The investigation increased perceived scrutiny on police officers, but did not explicitly mandate changes in policing behavior or affect departmental staffing. While investigations are primarily retrospective–Department of Justice (DOJ) investigators review administrative records, training materials, and policies, and may also conduct interviews and ride-alongs–my discussions with officers suggest that the investigation increased the perceived level of scrutiny on their actions.[5] Officers may alter their behavior due to the increased perceived costs of mistakes or increased psychic costs (e.g., lower morale).

I first present a stylized model to illustrate how federal oversight affects the allocation of effort among policing activities with varying productivity and how changes might affect serious crime. The model yields two predictions that guide my empirical analyses. First,

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[3]While I observe decreases in other crimes, such as prostitution, I cannot distinguish between changes in criminal activity and changes in officer or civilian reporting for these crime types. Therefore, I focus on serious (i.e., Part I index) crimes–murder, aggravated assault, forcible rape, robbery, car theft, burglary, larceny, and arson–that are more costly to society and less likely to be affected by changes in officer or civilian reporting behavior due to the investigation.

[4]A natural question is whether increased oversight also decreased officer misconduct, as shown in Ba and Rivera (2024). Unfortunately, officer complaint data for Seattle are not available during my study period.

[5]See United States Department of Justice Civil Rights Division (2017) and United States Department of Justice (2015) for more information on typical investigations.

the model predicts that if federal oversight increases the perceived cost of misconduct from stops, stops will decrease. Second, it shows that the impact of stop reductions on crime is ambiguous: while fewer stops may increase crime, greater focus on other policing activities, such as patrolling, may decrease crime. The net impact hinges on the magnitude of stop reductions and the relative crime-reducing productivity of stops compared to other activities.

In my empirical analysis, I implement difference-in-differences models using across neighborhood variation and synthetic control models to assess changes in policing behavior and crime. In some cases where I only have data from Seattle, I implement interrupted time series models to analyze outcomes. My analysis of policing behavior focuses on officer-initiated (OI) stops, where officers use their discretion to respond to incidents observed during patrols. OI stops make up nearly 40% of all officer-involved dispatches in Seattle, similar to the share from calls for service or 911 calls. I show that, during the federal investigation, SPD officers reduced OI stops by 24%. Arrests from stops and the stop arrest rate also significantly decreased.[6] Compared to non-minority neighborhoods, minority neighborhoods experienced larger reductions in stops but smaller decreases in stop arrest rates. This finding suggests that forgone stops in minority neighborhoods were less likely to result in arrests, potentially indicating less efficient stops. After the investigation concluded, stops rebounded but remained below pre-period levels in minority neighborhoods.

Next, using two complementary approaches which leverage both neighborhood heterogeneity in the change in policing intensity and a cross-city design, I find consistent evidence that these stop reductions did not endanger public safety. I find no detectable impact on serious crime in Seattle, and my estimates can rule out more than 2-3% increases. These results are robust to focusing instead on fatal car crashes or 911 calls, which may address concerns about systematic underreporting of crimes (Ang et al., 2024). I also present suggestive evidence from survey data that the investigation did not affect criminal behavior, civilian crime reporting, or short-term confidence in the police.

To explore mechanisms, I examine heterogeneity in which officers reduced stops and which stop types were reduced. Consistent with prior work showing that Black and female officers engage in fewer stops (Ba et al., 2021; Hoekstra and Sloan, 2022), I find that officers who are White, male, less-experienced, or had high arrest rates in the pre-period (adjusted for dispatch characteristics) reduced stops more than their peers during the investigation, largely due to differences in base rates. Crucially, I also find that, despite comprising 55% of pre-period stops, traffic and suspicious-activity stops account for over 90% of stop reductions

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[6]In October 2011, Seattle introduced the Law Enforcement Assisted Diversion (LEAD) program to redirect individuals suspected of minor drug and prostitution offenses to social and legal services instead of prosecution and incarceration (Collins, Lonczak and Clifasefi, 2017). Because diversion occurs post-arrest, the program's impact on my analysis likely is modest.

during the investigation, suggesting that officers pulled back among stops where they had greater discretion or which might have lower crime-reducing potential (Wu and Lum, 2020), possibly as a precaution to avoid scrutiny. Another precaution that officers may take to safeguard themselves against negative attention is to improve the documentation of their activities. I explore this hypothesis using novel police report data and find that officers produced arrest reports that were 42 words or 26% longer after the investigation.

My study contributes to the literature on how oversight affects police behavior by addressing the open question of whether oversight can reduce policing without increasing crime.[7] In a related study, Ba and Rivera (2024) argue that existing studies conflate the effects of oversight and public outrage, and show that oversight, absent public outrage, can reduce misconduct without impacting policing quantity or crime. In contrast, I find that federal oversight, absent public outrage, can reduce policing activities, such as stops and arrests, without increasing serious crime. Unlike prior single-city studies on federal oversight, my study provides insights into the impacts of federal investigations conducted with little public outrage, and in a city that is not predominantly Black.[8] My results also add to a broader literature demonstrating how changes in officers' work environment influence policing.[9]

I also add to a growing literature on the consequences of reduced low-level police enforcement. Cho, Gonçalves and Weisburst (2024) show that reduced arrest activity for up to two months, after an officer's death, does not increase crime. Whether reductions sustained over a longer period or across different types of policing activity would have similar impacts remains an open question. Using a different source of variation, I show that sustained reductions in discretionary stops and arrests over 11 months do not increase serious crime. Discretionary stops make up a significant portion of officer effort and the largest share of police–civilian interactions.[10] Stop reductions also represent reduced enforcement at an early stage of the criminal justice system, potentially minimizing unnecessary and costly interactions between civilians and later stages of the criminal-legal system.

Several studies also examine interventions targeting specific stop types (Rushin and Ed-

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[7]Devi and Fryer Jr (2020) find that federal investigations preceded by viral incidents decreased the quantity of policing and increased crime. By contrast, the authors argue that investigations not preceded by viral incidents had no impact on policing quantity but decreased crime, suggesting improved effectiveness.

[8]Seattle is a predominantly white city, but it has a sizable and diverse minority population, including roughly equal shares of Black, Hispanic, Asian, and Native American residents.

[9]See Mas (2006); McCrary (2007); Heaton (2010); Long (2019); Cheng and Long (2022); Rozema and Schanzenbach (2023); Cox, Cunningham and Ortega (2024); Dube, MacArthur and Shah (2024); and Rubalcaba, Ortega and Dantzler (2024).

[10]While consequential, arrests are relatively rare (Linn, 2009; Lum and Vovak, 2018; Rackstraw, 2023; Wu and Lum, 2020). Officers make over 10 million arrests each year, but traffic stops are the most common police–civilian interaction, occurring more than 20 million times annually (McCann, 2023; Tapp and Davis, November 2022). Furthermore, based on the SPD data, traffic stops account for about 18% of all stops.

wards, 2021). Tebes and Fagan (2022) show that the near elimination of investigative stops in New York City did not increase serious crime.[11] Similarly, Parker, Ross and Ross (2024) show that reductions in traffic and pretextual stops among minority motorists did not significantly affect public safety. Whereas these studies exploit interventions aimed at reducing specific stop types, my study relies on changes in policing behavior in response to a federal investigation, demonstrating that federal oversight–an already prominent mechanism for improving policing–can offer benefits comparable to these targeted measures. Moreover, that the reductions in my setting are primarily among stop types that other studies indicate may have low crime-reducing benefit, suggests that officers may be sophisticated enough to adjust effort without endangering public safety when appropriately incentivized.

The paper proceeds as follows. Section 2 provides institutional background. Section 3 presents a framework of policing. Section 4 describes the data. Sections 5 and 6 present my primary findings. Section 7 explores potential mechanisms. Section 8 examines other relevant public safety measures, and Section 9 concludes.

2 The SPD's Federal Investigation

In this section, I provide background information on federal investigations and the investigation into the SPD. In 1994, the U.S. Congress passed the Violent Crime Control and Law Enforcement Act, granting the federal government unprecedented authority to investigate and litigate cases against law enforcement agencies that exhibit a pattern or practice of unlawful policing behavior (Center for American Progress, 2021). These investigations, conducted by the Special Litigation Section of the Civil Rights Division within the DOJ, are key to the federal government's efforts to combat unconstitutional policing practices.

In a typical investigation, the Civil Rights Division staff first decides whether to investigate a law enforcement agency. While many agencies are often eligible for investigation, the division prioritizes resources based on factors such as whether a particular investigation can inform standards in other agencies facing similar issues. Once an investigation is initiated, DOJ officials promptly meet with local law enforcement official to discuss the investigation's basis and scope (United States Department of Justice Civil Rights Division, 2017). The length of the investigation varies based on factors like agency size and the scope and complexity of the investigation. During an investigation, the DOJ, often with help from external experts, conducts a comprehensive retrospective review of an agency's policies, practices,

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[11]While the New York Police Department (NYPD) widely adopted "broken windows" policing, which advocates for aggressive enforcement of low-level offenses to deter more serious crimes, in the 1990s (Corman and Mocan, 2002; Zimring, 2011), the SPD is not known to subscribe to this policing philosophy (Lum and Vovak, 2018). As a result, my findings may be more relevant for moderate police departments in the U.S.

training, data handling, accountability systems, and community engagement. Officials may also observe training sessions, ride along with officers, and interview command staff, officers, and community members (United States Department of Justice, 2015).[12] The primary objective of the investigation is to uncover systemic problems contributing to unconstitutional behavior and create a plan to address these issues.

If the investigation yields sufficient evidence, the division issues a findings letter. If not, it closes the case. After the findings announcement, if the agency is willing to cooperate, the DOJ begins confidential negotiations over a reform agreement. If an agreement is reached, reforms are overseen by a federally-appointed independent monitor through a court-enforceable consent decree. In challenging cases, the Civil Rights Division may exercise its authority under the 1994 act to file civil lawsuits for court-ordered reforms.

The SPD Investigation. Appendix Figure A1 shows the investigation timeline. On December 3, 2010, the ACLU of Washington and other community organizations filed a complaint against the SPD with the U.S. Attorney's Office and the DOJ requesting an investigation into the SPD (ACLU of Washington, n.d.). The complaint alleged several examples of excessive force by SPD officers, particularly against people of color. For example, on August 30, 2010, an SPD officer killed 50-year-old, Native American woodcarver John T. Williams (ACLU of Washington, 2010; NPR, 2016; Seattle Times Staff, 2018). The complaint alleged that Williams was crossing the street in a crosswalk, holding a piece of wood and a woodcarving knife, when the officer stopped his car, got out, and yelled at Williams to drop the knife. About 5 seconds after stopping his car, the officer had shot and killed Williams (NPR, 2016).

In **February 2011**, DOJ representatives met with Seattle Mayor Michael McGinn, other community and city leaders, and SPD personnel and union members to discuss structural challenges facing the SPD (United States Department of Justice, 2011). It is standard practice for DOJ officials to meet with department representatives shortly after the decision to investigate is made (United States Department of Justice, 2015). Finally, on March 31, 2011, in a joint press conference with U.S. Attorney for the Western District of Washington Jenny Durkan, Assistant Attorney General for the Civil Rights Division of the DOJ Thomas Perez announced a federal investigation into the SPD for excessive use of force and racial bias in policing. U.S. Attorney Durkan noted that the investigation would not focus on charging officers for their roles in past episodes, but would lead to formal changes in departmental policies if federal laws had been violated (Yardley, 2011). In response to the federal investi-

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[12]See Donnelly and Salvatore (2019); United States Department of Justice Civil Rights Division (2017) for an overview of the standard federal investigation process.

gation into the SPD, Seattle Chief of Police John Diaz welcomed the inquiry and encouraged the DOJ to make its investigation "as wide as possible."[13]

On **December 16, 2011**, the DOJ concluded the investigation, finding that the SPD had engaged in a practice of using excessive force. While the DOJ did not conclude that the SPD had engaged in a practice of discriminatory policing, the findings letter raised concerns that some of the SPD's policies and practices could result in unlawful discriminatory policing.[14] After the investigation's conclusion, the SPD began negotiating a reform agreement to address the investigation's findings with the federal government. On July 27, 2012, the SPD entered into a consent decree with the federal government. The consent decree, which is still in effect as of October 2024, required the SPD to implement reforms under the supervision of a federally-appointed monitor. 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. The city was also required to establish a community police commission that would work with the court-appointed monitor to provide recommendations and oversight on the implementation of the settlement agreement.[15]

3   A Model of Policing

Before turning to 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 perceived cost of misconduct from stops, then stops should decrease. Second, assuming there are alternative productive policing activities, such as patrolling, 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. I provide formal model derivations in Appendix C.

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[13]In an interview with the *Seattle Times*, he said, "I'm just looking at this as a way of getting a free audit from the Department of Justice" (Yardley, 2011).

[14]The findings letter noted that SPD officers confused "casual, social interactions and investigative detentions" (United States Department of Justice, 2011) and 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).

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

3.1   Set-up

A police captain manages a department. Her jurisdiction includes N neighborhoods. The number of officers assigned to each neighborhood is fixed. Officers can engage in stops, S_n; other productive policing practices, such as patrolling the neighborhood, G_n; or unproductive activities such as leisure. The police captain chooses the levels of S_n and G_n to maximize a neighborhood-specific objective function.[16] To simplify the analysis, I present the framework for a single neighborhood and normalize the department to include a single officer. The police captain's objective function, $V(\cdot)$, decreases in realized crime, R, and the costs she incurs to enforce policing activity. Realized crime is defined as the exogenous crime level, Θ, minus any crime reductions from policing activities, S and G. I assume that S exhibits diminishing marginal returns to crime reduction so that $\gamma < 1$, while G exhibits constant or diminishing marginal returns so that $\rho \leq 1$. I use the parameter A to capture the relative productivity of S to G for reducing crime (i.e., I normalize the productivity of G to 1). The police captain incurs costs to enforce her desired level of policing. Stops, S, are costless to enforce, but police misconduct, m, arises from stops with probability δ_m. When misconduct occurs, the police captain incurs costs, c_m, which includes the costs to the officers and the department. Therefore, the expected cost of misconduct is $c_m \delta_m$. The police captain must pay linear costs, c_g, to enforce other productive policing, G; otherwise, the officer might not engage in the desired level of G and instead engage in leisure. I further assume that the police captain's objective function is concave in realized crime so that $\tau > 1$:

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

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

3.2   The Police Captain's Decision

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

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

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

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[16]The model implies that the police captain can induce these actions from officers through monitoring and other incentives, which I have omitted to concentrate on the trade-off between different policing activities.

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

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

Increasing the perceived 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.[17] Some degree of substitutability between different policing activities is consistent with prior studies on the relative effectiveness of various forms of problem- and community-oriented policing (Gonzalez and Komisarow, 2020; Owens, 2020; Weisburd and Telep, 2014).

Proposition 2. *The impact of an increase in the perceived 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 stop reductions on crime is ambiguous because of two opposing forces. While the reduction in stops is likely to increase crime, substitution to other productive policing activities (e.g., patrolling) may decrease crime. The net impact on crime depends on the relative magnitude of the lost productivity from decreasing stops and the gained productivity from increasing other productive policing practices. If the lost productivity from decreasing stops dominates, for example, if stops are highly productive (i.e., A is sufficiently large) or the change in stops is sufficiently large, then crime increases. However, if the gained productivity from increasing other policing activities dominates, then crime decreases. Finally, in the knife-edge case, the two are roughly equal and crime is unchanged.

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

4 Data

My study draws on multiple data sources, which I discuss below. My analysis primarily relies on administrative data obtained through a research agreement with the SPD. The SPD data cover the department's geographical jurisdiction from June 2009 to December 2013, spanning 20 months before notification of the investigation to two years after its conclusion.

Seattle Municipal Court Data. I use misdemeanor case records from the Seattle Municipal Court, which include case type, charges, a defendant identifier, and charge disposition. I use the charge disposition field to identify guilty findings. The court data also contain a unique incident identifier that allows me to link to the SPD Computer-Aided Dispatch data.

SPD Computer-Aided Dispatch Data. Virtually all modern police departments in the United States use computer-aided dispatch (CAD) systems to assist 911 call-takers and dispatchers. The CAD system manages the high volume of requests for police services, collects information from callers, monitors real-time patrol unit availability, and dispatches appropriate resources. Dispatches include community-initiated dispatches (e.g., 911 and other telephone calls) and officer-initiated dispatches (e.g., officer-initiated stops). I focus my analysis on officer-initiated stops and 911 calls, which together constitute 78% of weekly dispatch activity in Seattle between June 2009 and December 2013.[18] Officer-initiated (OI) stops refer to instances in which individual officers assign themselves to respond to incidents observed while patrolling. In contrast, 911 calls are requests for assistance from the public, often related to emergencies. While OI stops are primarily at the discretion of the officer, 911 calls are conditionally randomly assigned based on officer proximity, availability, and the required resources needed for the call.[19]

For each dispatch, I observe the officer(s) dispatched, date, time, location (beat, sector, and approximate coordinates), priority code, initial case type, and final disposition.[20] The initial case type is a brief text description of the initial reason for the dispatch, assigned by the officer for OI stops or by the dispatcher for 911 calls. The final disposition describes the outcome of the dispatch including, for example, whether an arrest was made. I use the initial case type description field to categorize OI stops and 911 calls for my analysis.[21] Specifically,

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

[19]I test the conditionally random assignment of 911 calls in Appendix Table B1.

[20]The data do not include officer shift assignments, so I construct shifts based on the watch hours used in the department: the first watch runs from 3:00 to 11:00 AM, the second from 11:00 AM to 7:00 PM, and the third watch runs from 7:00 PM to 3:00 AM. The priority code is an ordinal ranking from 1 to 9 that describes the urgency of the dispatch as assigned by the officer for OI stops or the dispatcher for 911 calls.

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

I classify OI stops into four distinct categories: premise check stops, suspicious-activity stops, traffic stops, and other types of stops. Premise check stops involve inspecting specific locations to address potential issues or ensure site security. Suspicious-activity stops are similar to stop-and-frisk stops and are initiated when officers have a reasonable suspicion of potential criminal activity. Traffic stops involve stopping vehicles in response to potential law violations. Combined, premise check, suspicious-activity, and traffic stops constitute nearly 62% of all officer-initiated stops. Table 1 shows summary statistics for weekly dispatches. Weekly OI stops, on average, comprise about 841 premise check stops, 1,135 suspicious-activity stops, and 787 traffic stops. The average weekly arrest rate for officer-initiated stops (the stop arrest rate) is about 87 arrests per 1,000 stops.

SPD 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.[22] I focus my analysis on serious crimes. Serious crimes are the eight Part I index crimes that are tracked nationally by the FB. They represent the most serious violent and property offenses: homicide, rape, robbery, aggravated assault, car theft, burglary, larceny, and arson. I focus on serious crimes because they are particularly costly to victims and to society (Bhatt et al., 2024; Chalfin, 2015; Cho, Gonçalves and Weisburst, 2024; Tebes and Fagan, 2022), and they are more reliably observed and measured than lower-level crimes (Devi and Fryer Jr, 2020).

My analysis uses four crime measures. Car thefts serve as my primary measure because most insurance companies require a police report to process claims, and reporting a stolen vehicle is vital for mitigating liability if the car is used in a crime, providing a reliable gauge of criminal activity while minimizing bias from changes in civilian reporting behavior.[23] I also include violent crimes and other property crimes, as well as the social cost of index crimes in US$1,000s, as additional outcomes to broaden my analysis. I calculate the social cost of index crimes using the estimates presented in Bhatt et al. (2024), which I deflate to 2009 dollars.[24] Table 1 provides summary statistics for weekly reported crimes. On average, there are approximately 74 car thefts, 581 other property crimes, 66 violent crimes, and 649 non-index crimes each week during my sample period. The average weekly social cost of

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

[23]Bhatt et al. (2024) show that car thefts have the second-highest reporting rate after homicides. While homicides are often used in the literature to credibly measure crime, given their relative rarity and the geographic granularity at which my analysis is conducted, homicides are not a feasible primary outcome.

[24]The social cost of each index crime in 2009 dollars is presented in Appendix Table B2.

index crimes, measured in US$1,000s, is approximately 8,170.

SPD Officer Data. The SPD data also include unique officer identifiers 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 2011. Officer demographic summary statistics are reported in Table 1. Eight percent of the officers in my analysis are Black, 5% are Hispanic, and the overwhelming percentage are White (70%). About 13% of officers are female. The average officer has about 14 years of experience as of 2009.

I also create three measured officer traits (based on officer fixed effects) using the computer-aided dispatch data and municipal court records. The OI arrest officer fixed effect captures an officer's arrest propensity in OI stops, conditional on dispatch characteristics. I estimate these fixed effects using OLS regressions on officer–dispatch-level data containing all OI stops between June 2009 and January 2011. The 911 arrest officer fixed effect captures an officer's arrest propensity in 911 calls, conditional on dispatch characteristics. I estimate these fixed effects using OLS regressions on officer–dispatch-level data containing all 911 calls between June 2009 and January 2011. The conviction officer fixed effect captures the likelihood that misdemeanor charges associated with an officer lead to a guilty finding, conditional on dispatch and charge characteristics. I estimate these fixed effects using OLS regressions on officer–charge-level data containing all charges filed between June 2009 and January 2011. I use the estimated fixed effects to create three indicator variables set to 1 for officers with values above the median for each trait.[25] I use these traits in addition to officer demographic variables to explore heterogeneous officer responses to the federal investigation.

Other Data. I use information on the racial composition of the residential population in each census block group in Seattle from the 2014 American Community Survey (ACS) five-year estimates. I define a minority neighborhood as a census block group where share of non-Hispanic White residents is less than 50%. Defining neighborhoods as census block groups also helps to mitigate potential concerns about different population sizes across neighborhoods. Under this definition, one-fourth of Seattle neighborhoods are classified as minority neighborhoods. Summary statistics for the racial composition of neighborhoods are presented in Table 1 and Appendix Figure A2 illustrates the racial composition of neighborhoods in Seattle. I also employ crime data from the FBI UCR Program to compare

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[25]See Appendix E for more information on the construction of these measured officer traits and Appendix Table B3 for the correlation between officer traits.

changes in crime in Seattle to jurisdictions 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).[26] I restrict the dataset to core local police departments serving jurisdictions with 150,000 to 750,000 residents as of 2010, which have consistently reported monthly crime data for at least 10 years between 2005 and 2015, and I exclude all agencies, except the SPD, that were under federal investigation before 2015.[27] The final data set includes the SPD and 82 control agencies.[28] Finally, I complement my analyses with data on fatal crashes from the National Highway Traffic Safety Administration, data on crime victimization and reporting from the National Crime Victimization Survey's MSA Public-Use data files, and data on confidence in the police from Gallup's Confidence in Institutions survey.

5 Effect on Weekly Policing Activity

In this section, I present my main results for the effect of the federal investigation on policing activity. Consistent with the model's prediction, I show that when the federal investigation increases the perceived cost of misconduct, stops decrease.[29] Because the ACLU complaint alleged that officers engaged in racially-biased policing, it is reasonable to hypothesize that the investigation might be more salient in minority neighborhoods, leading to differential response between minority and non-minority neighborhoods.[30] I show that minority neighborhoods experienced larger decreases in stops than non-minority neighborhoods.

5.1 Aggregate Policing Activity

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

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[26]I use the data cleaned and formatted by Jacob Kaplan. The FBI UCR program is a voluntary, nationwide initiative, which, as of 2014, covers about 98% of the U.S. population.

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

[28]I provide the list of donor agencies in Appendix Table B4.

[29]The model assumes no significant change in weekly patrol officers, which I show in Appendix Figure A3.

[30]One way to capture this possibility in my model is through a neighborhood-specific perceived cost of misconduct, $c_{m,n}$. Non-minority neighborhoods may have higher perceived costs of misconduct if, for example, residents are more politically organized. Conversely, minority neighborhoods may have lower perceived costs of misconduct if residents are considered more likely to engage in criminal activity, and stops are seen as more necessary. This rationale is consistent with Chen et al. (2024)'s finding that officers spend more time patrolling in neighborhoods with larger non-White populations. If the federal investigation sets a sufficiently high, neighborhood-agnostic perceived cost of misconduct, $\bar{c}_m$, making misconduct equally costly regardless of location, then I would expect minority neighborhoods, which had lower initial c_m, to experience larger decreases in stops because the gap between initial c_m and $\bar{c}_m$ is larger.

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2009 to December 2013 for Seattle as a whole. This framework compares average weekly outcomes in the 20 months preceding notification about the investigation (June 2009 to January 2011) with average weekly outcomes in the subsequent months, extending to 2 years after the investigation's conclusion. I use the following OLS specification:

$$Y_t = \beta_0 + \beta_1 \text{Investigation}_t + \beta_2 \text{Post-Investigation}_t + \tau_t + \epsilon_t, \tag{5.1}$$

where Y_t is the number of OI stops, arrests from stops, or the OI stop arrest rate (arrests per 1,000 OI stops) in week t. Investigation$_t$ is an indicator for the weeks between notification in February 2011 and the findings report on December 16, 2011. Post-Investigation$_t$ is an indicator for the weeks after the findings report through December 2013. τ_t represents week-of-the-year fixed effects to adjust for seasonality, and ϵ_t is the error term. The identifying assumption is that, without the investigation, outcomes would have been similar to outcomes during the pre-period. Unless noted otherwise, standard errors throughout my study are calculated using the Newey-West method (Newey and West, 1987) with three lags (L=3).[31]

5.1.1 Results

Figure 1 shows seasonally adjusted weekly stops (Panel A) and arrests from stops (Panel B). The shaded area in all figures represents the investigation period. The figure shows a decrease in both OI stops and arrests during the investigation period. After the investigation, stops increased but remained below pre-period levels. In contrast, arrests remained low.

Table 2 reports the regression results from estimating Equation 5.1 and confirms the visual evidence in Figure 1. During the investigation period, weekly OI stops fell by about 1,259 or approximately 24% and weekly arrests from stops decreased by 146 or about 28%. The estimate for the weekly OI arrest rate suggests a decrease of about 6 arrests per 1,000 stops or about 6%. During the post-investigation period, average weekly stops, arrests from stops, and the stop arrest rate remained significantly lower than pre-period levels. As a robustness, I rerun my analysis including a linear time trend. These results are presented in Appendix Table B5 and are qualitatively similar to those from my main specification.

5.2 Policing Activity by Neighborhood Race

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

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[31]This is the default number of lags specified by the `feols` package in `R`.

the following specification:

$$Y_{n,t} = \gamma_1 \text{Minority}_n \times \text{Investigation}_t + \gamma_2 \text{Minority}_n \times \text{Post-Investigation}_t$$
$$+ \gamma_3 \text{Investigation}_t + \gamma_4 \text{Post-Investigation}_t + \eta_n + \tau_t + \epsilon_{n,t}, \qquad (5.2)$$

where $Y_{n,t}$ is the number of OI stops, the number of arrests, or the OI stop arrest rate (arrests per 1,000 stops) in neighborhood n in week t; Minority_n is an indicator for whether the share of non-Hispanic White residents in neighborhood n is less than 50%; 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. The identifying assumption is that, without the investigation, minority and non-minority neighborhoods would have common trends in outcomes, and there are no visible differences in the pre-trends across neighborhoods.

5.2.1 Results

Figure 2 seasonally adjusted weekly stops (Panel A) and arrests from stops (Panel B) for minority and non-minority neighborhoods. Consistent with Chen et al. (2024), minority neighborhoods had more OI stops and arrests than non-minority neighborhoods in the pre-period. During the investigation, minority neighborhoods experienced a larger decrease in stops and arrests than non-minority neighborhoods. After the investigation, stops steadily increased in both neighborhood types, but stops remained below pre-period levels in minority neighborhoods. Unlike stops, arrests remained low in both minority and non-minority neighborhoods after the investigation ended.

Table 3 reports results from estimating Equation 5.2. I find both larger decreases in stops and smaller decreases in the stop arrest rate in minority neighborhoods, suggesting that forgone stops were less likely than average to result in arrest. During the investigation, OI stops in minority neighborhoods decreased by an additional 1.5 stops per week or about 17%. Weekly arrests from stops in minority neighborhoods decreased by an additional 0.2 arrests or about 22%. Yet, the stop arrest rate in minority neighborhoods was about 8 arrests per 1,000 stops more or 14% higher than in non-minority neighborhoods. In the post-investigation period, weekly stops and arrests were significantly lower in minority neighborhoods, but the effect on the stop arrest rate, while negative, is not statistically significant.

As a robustness check, I re-estimate Equation 5.2 with calendar-week fixed effects to flexibly control for time and cluster standard errors at the neighborhood level.[32] These

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[32] I also test whether estimated differences in stop reductions between minority and non-minority neighborhoods reflect equiproportionate changes from pre-period means by adding controls for each neighborhood's pre-period average weekly number of OI stops interacted with each time period indicator to Equation 5.2. If the estimated effects are entirely explained by differences in pre-period stop levels, then these effects should

results are reported in Appendix Table B6 and are qualitatively similar to my baseline results. I find significantly larger decreases in stops in minority neighborhoods, but the effect on arrests is no longer statistically significant and the effect on the stop arrest rate is only marginally significant. Both Newey-West and clustered standard errors are sandwich covariance estimators (Zeileis, Köll and Graham, 2020). However, Newey-West is specifically robust to serial correlation, which is relevant because errors within neighborhoods are likely correlated over time. For consistency in my analysis, I use Newey-West standard errors in my preferred specification, while also showing that the results are robust to using clustered standard errors.

6 Effect on Serious Crime

Given the 24% decrease in weekly OI stops during the 9-month long federal investigation in Seattle, a crucial question arises: did the sustained decline in OI stops impact serious crime rates? I employ two complementary approaches to address this question. First, I compare crime rates between minority and non-minority neighborhoods in Seattle. If the OI stop reductions increased crime, I might expect minority neighborhoods, which saw larger stop reductions, to experience larger crime increases.[33] In the second approach, I compare crime rates in Seattle to crime rates in jurisdictions whose police departments were not subject to federal investigations. Similarly, if the reduction in OI stops in Seattle led to increased crime, I would anticipate that crime in Seattle would be higher than crime in control jurisdictions.

6.1 Crime Effects Using Within-Seattle Variation

I examine whether minority and non-minority neighborhoods experienced differential changes in crime by re-estimating Equation 5.2 with weekly car thefts, violent crimes, property crimes, and the social cost of index crimes in US$1,000s as my dependent variables.[34] Figure 3 shows seasonally adjusted weekly car thefts (Panel A), violent crimes (Panel B), property crimes

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no longer remain statistically significant after I include these additional controls. I report these results in Appendix Table B7. The estimates suggest that my effects are not entirely driven by pre-period level differences in weekly outcomes. Throughout my analysis, I focus on the unadjusted level changes in stops, as these are most likely to be relevant for understanding changes in crime.

[33]I would detect impacts on crime if the marginal return to stops in minority neighborhoods is the same or higher than the marginal return in non-minority neighborhoods.

[34]As previously mentioned, car thefts are my primary measure because they reliably gauge criminal activity and help minimize bias from changes in civilian reporting. Insurance companies typically require a police report for car theft claims to be processed, and reporting a stolen vehicle is crucial to avoid liability if the vehicle is used in a crime. Additionally, car thefts are highly responsive to police presence (Chalfin and McCrary, 2017; Di Tella and Schargrodsky, 2004), making them an important test of my hypothesis that reductions in policing activity in my setting were not due to decreased police presence.

(Panel C), and the social cost of index crimes in US\$1,000s (Panel D) for minority and non-minority neighborhoods. The figures do not show noticeable differences in crime changes across neighborhoods during the investigation, although the series are noisy.

Table 4 shows regression results. In line with the visual evidence in Figure 3, I do not find significant differences between serious crime in minority and non-minority neighborhoods. The exception is violent crimes for which I find significantly fewer violent crimes in minority compared to non-nonminority neighborhoods. The estimates for remaining crime types are generally small, however the standard errors are sufficiently large that I cannot rule out meaningful increases among some crimes. The implied 95% confidence interval for car thefts cannot rule out up to a 17% increase. For property crimes, the 95% confidence interval cannot rule out up to a 4.7% increase, while the 95% confidence interval for the social cost of index crimes cannot rule out up to a 25% increase in costs.[35]

6.2 Crime Effects Using Across-US Variation

If the reduction in OI stops in Seattle led to increased crime, I would anticipate that crime in Seattle would be higher than crime in control jurisdictions whose police departments did not undergo federal investigations. I utilize data from the FBI UCR program to construct a balanced agency-by-month panel from June 2009 to 2013 that includes the SPD and 82 control agencies without federal investigations and employ the synthetic control (SC) method (Abadie, Diamond and Hainmueller, 2010), implemented through the `synthdid` package (Arkhangelsky et al., 2021) in `R`, to estimate the impact of the investigation on serious crime in Seattle. The SC method offers a valid approach to estimating effects and conducting inference in settings with a single treated unit and multiple control units (Abadie, 2021). The SC method constructs a counterfactual for Seattle by reweighting control units so that the weighted average outcomes of these units match the pre-treatment outcomes of the treated unit as closely as possible in terms of pre-treatment levels and time trends. The synthetic control estimator captures the average causal effect of a treatment, $\hat{\beta}^{sc}$:

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

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[35] In Appendix Table B8, I calculate implied estimates for crimes per 1,000 OI stops averted based on the estimates in Tables 3 and 4 and report 95% credible intervals from performing Bayesian bootstrapping across neighborhoods with 1,000 replications (Rubin, 1981). I include estimates for each time period of my analysis, as well as an estimate for *Post*, which combines time periods after notification about the investigation. The 95% credible intervals on *Post* can rule out more than 2 additional car thefts and any increase in violent crimes per 1,000 stops averted. However, the 95% credible intervals for property crimes and social costs of index crime cannot rule out up to 72 additional property crimes and 4,465 in additional social cost of index crimes per 1,000 stops averted.

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

6.2.1 Results

Figure 4 shows monthly crimes per 100,000 residents in Seattle compared to its synthetic control counterfactual. Despite being seasonally adjusted, the crime series are noisy. Nevertheless, the figure provides compelling visual evidence that there were no detectable increases in serious crime in Seattle up to 2 years after the investigation's end.

I present the estimated effects in Table 5.[36] For car thefts, the estimated effect is -0.03. The estimate is not statistically different from zero and is modest relative to the pre-period monthly average of 48 car thefts per 100,000 residents. Furthermore, the implied 95% confidence interval rules out more than a 2.4% increase in car thefts during the 34 months after notification about the investigation. For violent crimes, the estimated effect is 0.04 and is small relative to the pre-period monthly average of 50 violent crimes per 100,000 residents. The implied 95% confidence interval rules out more than a 2.5% increase in violent crimes. My estimate for property crimes can rule out more than a 1.3% increase. Finally, the estimated effect on the social cost of index crimes in US\$1,000s is $-\$9.13$, which is small relative to pre-investigation monthly average social cost of \$5,755 per 100,000 residents. However, the implied 95% confidence interval cannot rule out up to a 10% increase in social cost during the 34 months after notification about the investigation.

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. The SDID method reweights control units to roughly match the pre-treatment trends of the treated unit, allowing for constant differences between treated and control units. Standard errors are calculated using the placebo method with 500 replications. I present the results

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[36]For consistency, I focus on the crime outcomes used throughout my analysis. I also report separate SC estimates for monthly homicides per 100,000 residents in Appendix Figure A5 and Appendix Table B10. The estimate is negative and not statistically significant. However, the implied 95% confidence interval cannot rule out up to a 37% increase in homicides.

in Appendix Figure A4 and Appendix Table B9. The estimates are qualitatively similar to those from the SC method but are less precise. This may be in part due to the relatively poorer pre-period fit of the SDID counterfactual. While SDID can offer benefits relative to SC, the unit and time weights imply that counterfactual should follow parallel pre-trends (Arkhangelsky et al., 2021)—visual inspection suggests that parallel trends do not hold in my setting so I use the SC method as my preferred estimation procedure.

7 Mechanisms

Why would a 24% reduction in stops over a 9-month period have no detectable effect on serious crime in my context, when the leading hypothesis for why federal investigations may increase serious crime is due to an abrupt decline in policing quantity (Devi and Fryer Jr, 2020; Nix et al., 2024)? I leverage the granularity of the SPD data to explore possible explanations, informed by the model in Section 3. I consider the relative crime-reducing productivity of stops compared with other policing activity (e.g., patrolling) by exploring whether stop reductions were concentrated among certain officers or certain stop types.

7.1 Do Different Officers Pullback Differently?

A growing strand of the economics literature demonstrates that individual officer traits shape policing behavior. Black and female officers make fewer stops, make fewer arrests, and use force less often than their peers (Ba et al., 2021; Hoekstra and Sloan, 2022). If officers with these traits perceive higher costs of misconduct (c_m) associated with their stops, then I posit that these traits may also impact responses to the federal investigation and the impact of stop reductions on crime.[37] Accordingly, I examine whether officer traits predict differential responses to the federal investigation using the officer traits described in Section 4.

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

$$
\begin{aligned}
Y_{j,t} =& \alpha_1 \text{Officer Trait}_j \times \text{Investigation}_t + \alpha_2 \text{Officer Trait}_j \times \text{Post-Investigation}_t \\
& + \theta_j + \tau_{j,t} + \nu_{j,t},
\end{aligned}
\tag{7.1}
$$

where $Y_{j,t}$ is the number of OI stops by officer j in week t, and Officer Trait$_j$ is one of the following officer traits: officer race (Black and Hispanic vs. other), sex (female vs. male), experience (high vs. low), misdemeanor conviction rate (high vs. low), OI stop arrest rates

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[37]Specifically, when the federal investigation establishes a new officer-agnostic $\bar{c_m}$, officers with higher initial c_m values in the pre-period may reduce their stops less than their peers with lower initial c_m values.

(high vs. low), or 911 call arrest rates (high vs. low).[38] 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.[39] Because I lack officer assignment data for my analysis, I define an officer's home sector as the sector in which most of her OI stops occur in the pre-period, following Hoekstra and Sloan (2022). By including home-sector-by-calendar-week fixed effects in the specification, I test for the role of officer traits beyond effects of their assigned location.

Table 6 reports the results from estimating Equation 7.1. Columns 1 and 2 present estimates for the regression with race as the officer trait in the difference-in-differences specification. Each subsequent column presents estimates for a different officer trait: Column 3 reports sex, Column 4 reports experience, and Columns 5 through 7 report conviction fixed effects, OI arrest fixed effects, and 911 call fixed effects, respectively. Positive (negative) coefficients indicate smaller (larger) decreases in OI stops compared to the control group. I find that Black, female, and more experienced officers decreased OI stops less than their peers. Black officers reduced their stops by 0.47 fewer stops than their non-Hispanic non-Black peers. Similarly, female officers decreased stops by 0.44 fewer stops than their male peers and high-experience officers decreased stops by 1.92 fewer stops than their more junior colleagues. On the other hand, I find that officers with high arrest fixed effects reduced stops more than their peers. Officers with high OI arrest fixed effects reduced stops by 0.80 more stops than their peers with low OI arrest fixed effects, and officers with high 911 arrest fixed effects reduced stops by 0.74 more stops than their peers. I do not find significant differences between officers with high versus low conviction fixed effects.[40]

7.2 Does Pullback Differ Across Stop Types?

For simplicity, my model assumes that a single parameter A captures the relative productivity of stops (i.e., all stop types are equally productive) in reducing serious crime. However, different types of stops may be differentially productive at reducing serious crime. For in-

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[38] Please see Section 4 and Appendix E for more details on these officer traits.

[39] I do not argue that these officer traits capture independent officer characteristics, so I do not include them collectively in the same regression. Moreover, it may not be appropriate to include them all in one regression. For example, one would not include both arrest fixed effect measures in the same regression due to potential collinearity.

[40] I test whether estimated differences in stop reductions across officers reflect equiproportionate changes from pre-period means by adding controls for each officer's pre-period average weekly number of OI stops interacted with each time period indicator to Equation 7.1. I report these results in Appendix Table B11. The estimate for officers with high pre-period OI arrest fixed effects remains significant at the 1% level after adding these controls. The estimate for high-experience officers is marginally significant, which implies an equally proportional response across the remaining officer traits. This finding is consistent with the visual evidence in Appendix Figure A6, which shows normalized seasonally adjusted OI stops for the different officer traits. I normalize each series by dividing by the pre-period mean so that these figures reflect proportional changes in weekly OI stops.

stance, stops related to potential traffic violations may not be highly productive at reducing serious crime or may be just as effective as officer presence, i.e., officers patrolling the neighborhood without making these stops. In this section, I examine which types of stops officers changed in response to the federal investigation. One potential response to the federal investigation could involve reducing all stop types proportionally. An alternative strategy, supported by discussions with SPD staff, suggests that officers might reduce stops where they have greater discretion or where effort is potentially less productive, either because the stop is unlikely to deter crime or because the benefits of the stop are unlikely to outweigh the costs of potential misconduct. Stops related to traffic violations or suspicious activity are prime candidates for such reductions, especially because many of these stops may not be initiated in response to observing actual crimes.[41] I show that when the federal investigation increases the perceived cost of misconduct and the expected cost of stops, stops for traffic violations and suspicious activity are disproportionately affected.

Appendix Figure A7 shows seasonally adjusted weekly stops (Panel A) and arrests from stops (Panel B) by stop type. The figure shows a significant decrease in traffic and suspicious-activity stops and arrests during the investigation period. Other stops also decreased during the investigation period, but premise check stops were unaffected. I observe a spike in the number of stops in the "other" category during the post-investigation period, likely reflecting changes in how stops were classified in response to the DOJ's findings.[42]

Table 7 presents regression results for the change in each of the four OI stop-type categories. I show that the majority (94%) of the decrease in OI stops during the investigation is driven by decreases in traffic and suspicious-activity stops. Specifically, during the investigation, average weekly traffic stops decreased by 497, or about 43%, and suspicious-activity stops decreased by 689 or about 41%. Arrests from both stop types also significantly decreased–34% for traffic stops and 38% for suspicious-activity stops. Despite the reduction in arrests, the stop arrest rate for traffic stops significantly increased during the investigation, suggesting a potential increase in stop quality. While the estimate for suspicious-activity stops is also positive, it is not statistically significant. Different patterns emerge for the remaining stop types. The estimate for weekly premise check (other) stops during the investigation is positive (negative) but not statistically significant. In stark contrast to traffic and suspicious-activity stops, I find significant decreases in the stop arrest rate for premise check and "other" stops during the investigation, though the estimates for premise check stops are only marginally significant.

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

[42]The DOJ concerns raised specific concerns about SPD officers' use of investigative detention stops.

7.2.1 Police Report Lengths

OI stop reductions during the investigation were concentrated among traffic and suspicious-activity stops. I argue that officers reduced stops where they had greater discretion or which may have lower crime-reducing potential because these stops were more difficult to justify with the increased perceived cost of misconduct from the investigation. In response to increased scrutiny, officers may also produce more detailed records as a way to safeguard themselves against complaints or other unwanted attention, a hypothesis supported by my discussions with SPD staff. As I do not have complaints data for the period of my analysis, I examine the length of police reports as an additional measure of officer activity. Despite their importance in the criminal justice system, few studies in economics have examined police reports (Campbell and Redpath, 2023). Officers write reports to document incidents to which they have responded. A crime report is an officer's official record of reported criminal activity occurring in her department's jurisdiction. Such reports are ubiquitous across U.S. police departments and link police departments to later stages of the criminal justice system.

Figure 5 shows monthly average police report length by the discovery source of crimes (the full sample, crimes discovered via OI stops, crimes discovered through 911 calls, and crimes discovered through other sources). To account for potential changes in the composition of crimes resulting in arrest over time, I hold crime type composition fixed at pre-period levels.[43] I show that, in cases where an arrest was made, report length increased around the time the department was notified about the investigation, remained consistently high during the investigation and post-investigation periods. The increase in report length occurs slightly before February 2011, which could reflect anticipation by officers or the fact that there is often a gap between the incident date and the date the report is written. The figure uses the incident date, which is the only date available in my dataset. A natural question is whether the increases in report length reflect selection, i.e., as officers engage less, forgone police reports would have been for less-serious incidents and therefore would have been shorter. Consider police reports for crimes discovered from OI stops. If increased report length were entirely selection driven, I would expect that as officers make fewer stops and arrests during the investigation period, as I show in Figure 1, average report length would steadily increase. Instead, Panel A of Figure 5 shows that report length is relatively flat over this period. Moreover, despite stops and arrests being relatively stable during the post-investigation period, I show further increases in report length, suggesting that observed increases in report length are unlikely to be entirely selection driven.

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[43]Results are qualitatively similar when examining the raw unadjusted series, which I show in Appendix Figure A8. However, a concern might be that as arrests decrease, only the most serious offenses result in arrest so reports become mechanically longer.

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

$$Y_{r,t,l} = \lambda_0 \text{Arrest}_r + \lambda_1 \text{Arrest}_r \times \text{Investigation}_t + \lambda_2 \text{Arrest}_r \times \text{Post-Investigation}_t$$
$$+ \lambda_3 \text{Investigation}_t + \lambda_4 \text{Post-Investigation}_t + X_{r,t,l} + \epsilon_{r,t,l}, \tag{7.2}$$

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

I estimate Equation 7.2 on the full sample of crime reports as well as subsamples based on how crimes were discovered and present the results in Appendix Table B12. The results are consistent with the visual evidence depicted in Figure 5. During the investigation, police reports for crimes in which arrests were made were on average 42 words longer or about 26%. I find similar effects across different discovery sources. These findings suggest that officers may have been putting more effort into report writing following news of the investigation, a conclusion consistent with my discussions with SPD personnel.

8   The Effect on Other Public Safety Measures

Given the concentration of stop reductions during the investigation among traffic and suspicious-activity stops, I explore other relevant public safety measures. Reduced traffic enforcement could lead to more reckless driving and an increase in fatal car crashes, so I examine fatal crashes in Seattle. In addition, if residents valued some share of OI stops, they might call 911 for assistance when these stops are reduced in response to the investigation, so I examine 911 calls, which represent a community response that is related to but distinct from crime. While some crimes are reported through 911 calls, a large share of 911 calls are not related to crimes. Nonetheless, elevated 911 call volume might indicate a community in distress. Examining 911 calls can also help to mitigate concerns that the estimated null effects on crime may be due to systematic underreporting following the investigation.[44]

I use the synthetic control method and data from the NHTSA to examine whether monthly fatal crashes in Seattle changed differently than in cities whose police departments were not subject to federal investigations. I show these results in Appendix Figure A9 and

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[44]For example, Ang et al. (2024) show that police violence, like the events preceding the SPD investigation, can reduce residents' trust in the police and their willingness to make 911 calls.

Appendix Table B13. I do not find evidence of significant changes in monthly fatal crashes. The synthetic control estimate is not statistically significant and is generally small compared to the pre-period mean of 2 fatal crashes per month. However, the implied 95% confidence interval cannot rule out up to a 8.3% increase in fatal crashes.

Weekly 911 Calls.   In Appendix Figure A10, I show seasonally adjusted weekly 911 calls and OI stops. Notably, unlike OI stops, 911 calls exhibit an overall increasing trend over time, and it is difficult to discern whether this upward trend results from the investigation.[45] Although not conclusive, it is informative that 911 calls remained steady during the investigation period while OI stops were decreasing. Furthermore, in the post-investigation period, 911 call volume increased even as OI stops were rebounding.

To formally assess whether 911 calls increased due to the investigation, I compare the growth in 911 calls across different call types. I classify 911 calls into seven distinct categories using the initial case type field (assigned by the dispatcher at the time of dispatch): disturbance, domestic violence, suicide, suspicious-activity, theft, traffic, and other.[46] I compare 911 calls in other categories to 911 calls for in-progress or recently occurred suicides or suicide attempts, a category that I hypothesize is unlikely to be affected by the federal investigation, changes in stops or arrests, or changes in civilian reporting behavior. I estimate separate regressions using Equation 5.1 with the log of weekly 911 calls of each type as my outcome variable. Appendix Figure A11 shows the log of weekly 911 calls for the different call types. I find no visual evidence that the different 911 call types are changing differently over my study period. The results in Appendix Table B14 also support this conclusion. My estimates suggest that 911 call volume for most call types increased at similar rates.[47]

Other Community Responses.   Null effects on crime in Seattle could also reflect changes in civilian reporting, although this seems unlikely given overall increases in all 911 call types. In Appendix Figure A12, I provide suggestive evidence that criminal activity and civilian reporting did not noticeably change during the investigation using data from the National Crime Victimization Survey (NCVS) MSA Public-Use files and Gallup's Confidence in Institutions survey. These survey data are available annually and at coarser geographical levels than the data used elsewhere in my study–at the metropolitan statistical area (MSA) level for the NCVS and at the state level for Gallup. Nonetheless, these represent the

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[45]The increase may also be in part due to population growth in Seattle around this time. Notably, the population in Seattle began to rise significantly starting in 2011, largely driven by the tech industry and companies like Amazon (Emerald City Journal, 2011; Reifman, 2015).

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

[47]The two exceptions are 911 calls related to disturbances and theft, which experienced lower increases than other 911 call types during the investigation period.

best available data for assessing other changes in community behavior. I do not find visual evidence of an increase in crime victimizations, in nonreporting of victimizations to the police, or in nonreporting due to mistrust of the police. I also show that the annual share of Gallup respondents reporting high confidence in the police does not change differently in Washington than in control states.

9   Conclusion

This paper documents significant reductions in officer-initiated stops, particularly in minority neighborhoods, during a federal investigation into the Seattle Police Department. Despite the substantial decrease in stops, I find no significant impact on public safety, suggesting that the forgone stops had little crime-reducing benefit relative to police presence. These findings suggest that federal oversight can reduce stops, at a given level of police presence, without compromising public safety, which may disproportionally benefit minority communities.

My study examines a single setting, leveraging across-neighborhood variation in treatment instead of policy variation. A key concern with case studies is whether the results are generalizable. The SPD's experience might be atypical; factors such as ongoing union contract negotiations and a history of frequent changes in police leadership may have heightened sensitivity to the investigation and fluctuations in crime rates in Seattle. While I cannot rigorously assess these concerns, the evidence indicates that the Seattle case study could offer useful insights. First, the DOJ's decision to investigate a department often rests on the findings being able to inform standards for other jurisdictions facing similar challenges. Thus, lessons from Seattle may be relevant for other moderate U.S. police departments. Additionally, Seattle's federal investigation may offer insights into the typical impacts of federal investigations without riots or significant public outrage, which are less represented in the literature. One potential policy implication is that proactive DOJ audits of police departments, rather than reactive investigations triggered by high-profile incidents, could improve policing without compromising public safety.

Previous research on federal oversight suggests that significant reductions in policing quantity likely increase crime. My study highlights that the extent and type of policing reductions are both crucial for determining the impact on crime. In Seattle, a 24% reduction in officer-initiated stops and a 28% decrease in resulting arrests did not increase serious crime. Although the stop reductions documented in my study were generally smaller than those in Devi and Fryer Jr (2020)—possibly due to less public outrage in Seattle—the arrest reductions were similar to those in Shi (2009) and occurred over a longer time frame. Devi and Fryer Jr (2020) document significant increases in crime following a 46% reduction in officer-

initiated stops in St. Louis, and a 54% and 90% reduction in police-civilian interactions in Riverside and Chicago, respectively. Similarly, Shi (2009) finds increases in felony crimes after a 22-44% reduction in misdemeanor arrests in Cincinnati. My findings highlight the need to understand not only the extent of the pullback in policing but also which specific activities are reduced, including how police presence changes, in order to assess potential impacts on crime. My results are consistent with work by Cho, Gonçalves and Weisburst (2024), Tebes and Fagan (2022), and Parker, Ross and Ross (2024).

The lack of increased serious crime despite stop reductions in my setting may also suggest that there is a threshold of policing reduction (potentially police department specific) below which crime rates are likely unaffected, and documenting the extent of pullback in Seattle is an important contribution toward a more complete understanding of the impacts of reduced policing on crime. My study also highlights variation in the crime-reducing efficacy of different policing activities and suggests that oversight reducing low-efficacy policing is unlikely to increase crime and may lower the social cost of policing. Furthermore, given the growing challenges of recruitment and retention in policing, more efficient allocation of scarce policing resources may serve as a viable second best to adding more officers to police departments (Bureau of Justice Assistance and Office of Community Oriented Policing Services, 2023; Smith, 2016; The Associated Press, 2023). Federal investigations can provide useful variation in policing activity to explore the effectiveness of different policing strategies in reducing serious crime. These investigations also remain important policy levers to ensure constitutional policing in the United States. Further research is needed to assess how these investigations impact policing and what oversight-induced changes in policing activity can teach us about effective crime reduction.

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Figures & Tables

Figure 1: Weekly Officer-Initiated Activity
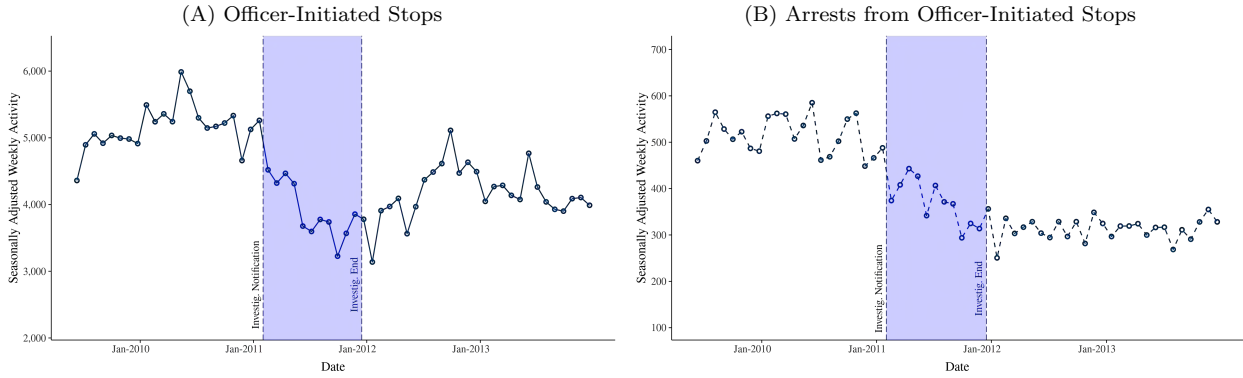
(A) Officer-Initiated Stops	(B) Arrests from Officer-Initiated Stops

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

Figure 2: Weekly Officer-Initiated Activity by Neighborhood Race
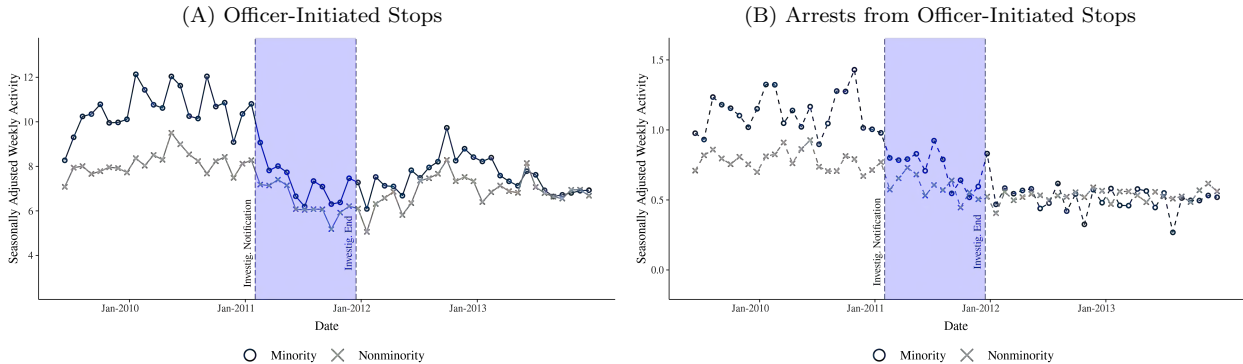
(A) Officer-Initiated Stops	(B) Arrests from Officer-Initiated Stops

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 nonminority neighborhoods. Minority neighborhoods are defined as census block groups with less than 50 percent non-Hispanic White residents. To seasonally adjust values, I residualize by week-of-the-year fixed effects and add back the mean of the fixed effects.

Figure 3: Weekly Within-Seattle Crimes by Neighborhood Race
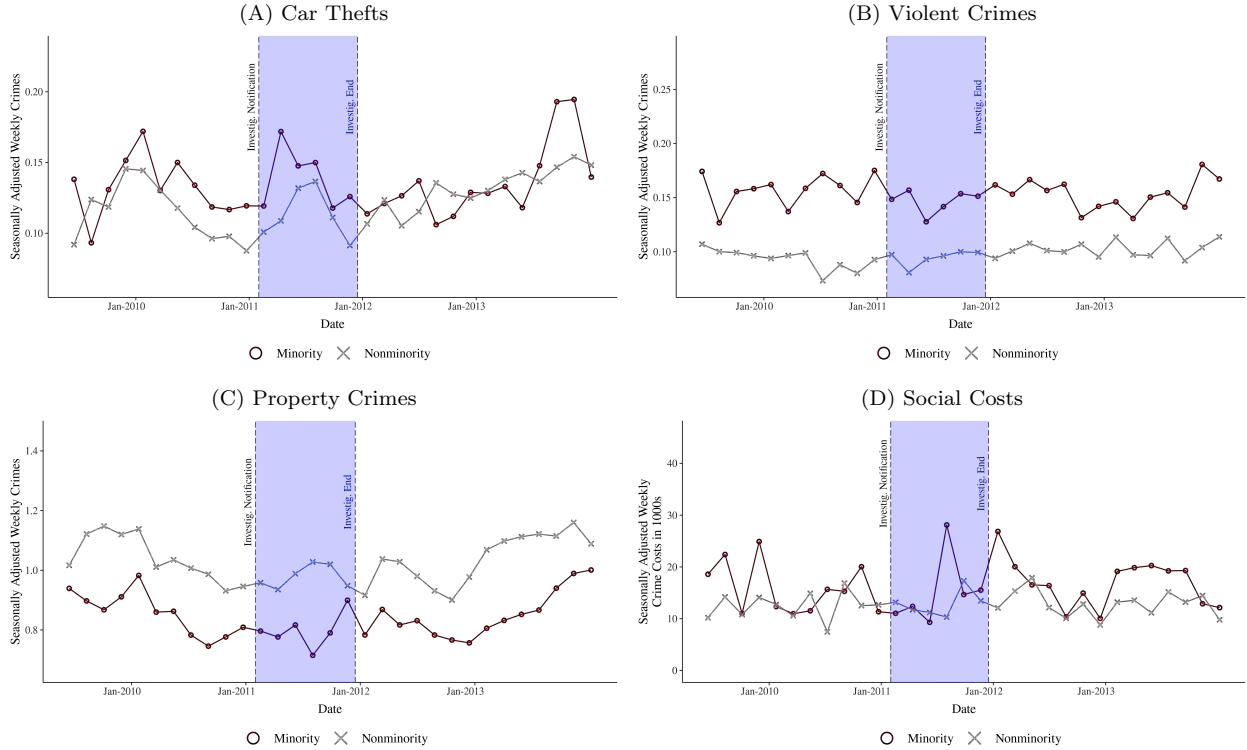
Notes: This figure plots seasonally adjusted weekly crimes for minority and nonminority neighborhoods from June 2009 to December 2013. Panel A reports car thefts, Panel B reports violent crimes, Panel C reports property crimes, and Panel D reports the social cost of index crimes in US$1,000s. To seasonally adjust values, I residualize by week-of-the-year fixed effects and add back the mean of the fixed effects.

Figure 4: Monthly Crimes per 100,000 Residents

(A) Car Thefts

(B) Violent Crimes

(C) Property Crimes

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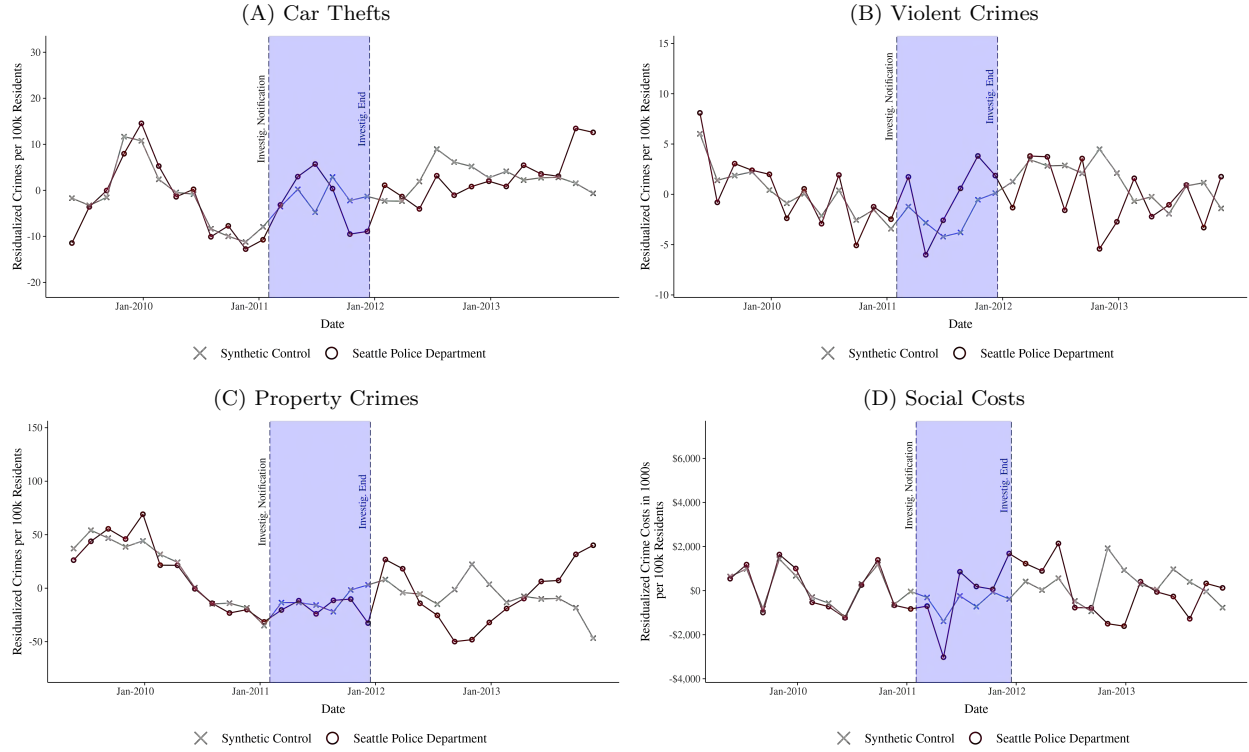
Notes: This figure plots monthly crimes per 100,000 residents, residualized by month-of-the-year fixed effects, in Seattle compared to its synthetic control counterfactual from June 2009 to December 2013. Panel A reports car thefts, Panel B reports violent crimes, Panel C reports property crimes, and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

Figure 5: Police Report Length by Crime Discovery Source

(A) Full Sample

(B) Officer-Initiated Stops

(C) 911 Calls

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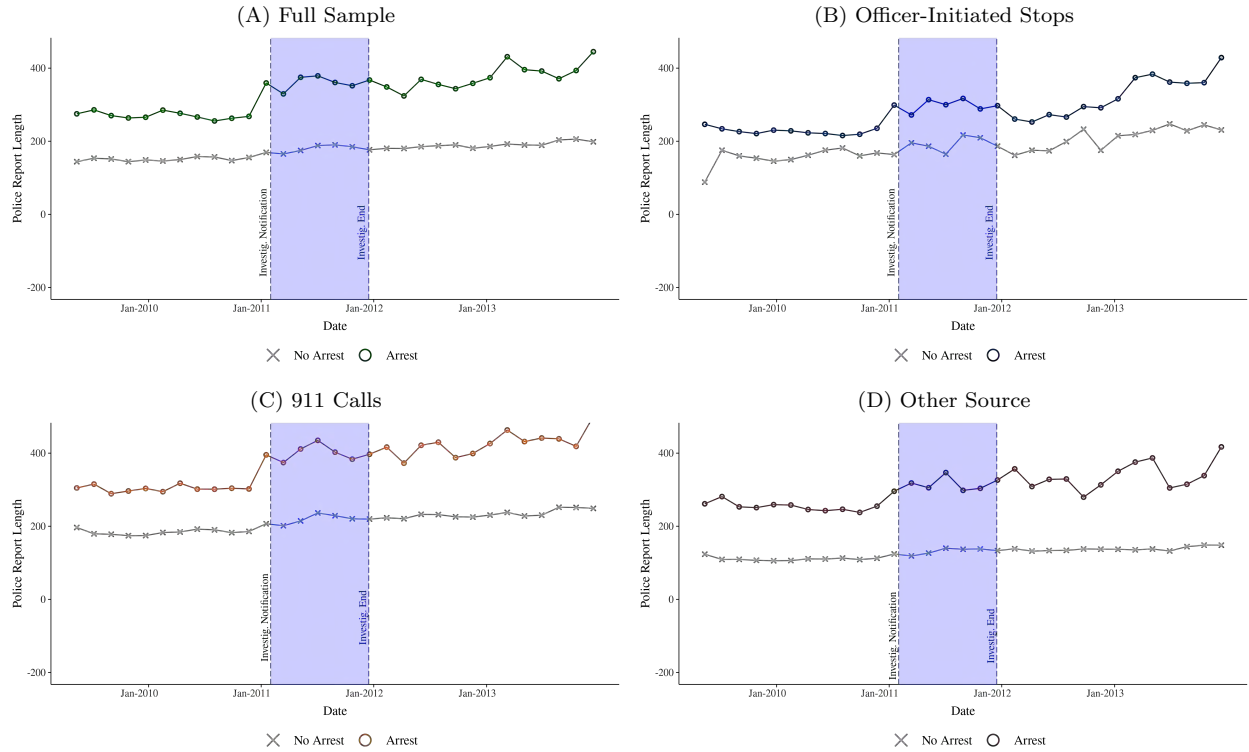
Notes: This figure plots the length, in words, of police reports from June 2009 to December 2013. Panel A shows report length for all crimes. Panel B shows report length for crimes discovered through officer-initiated (OI) stops, while Panel C and Panel D show report length for crimes discovered via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

Table 1: Summary Statistics

		Mean	Std Dev	N
A. Weekly Dispatch Characteristics				
Dispatch Source	OI Stops	4,502.54	820.23	239
	911 Calls	4,507.44	622.91	239
	Other	2,758.14	540.59	239
OI Stop Type	Premise Check	841.18	169.42	239
	Suspicious-Activity	1,135.00	489.96	239
	Traffic	787.38	324.63	239
	Other	1,738.97	510.83	239
Arrests per 1000 OI Stops		87.32	16.53	239
B. Weekly Reported Crime Characteristics				
Crime Type	Car Theft	73.88	14.93	239
	Property	581.16	62.88	239
	Violent	65.89	11.15	239
	Non-index	648.50	64.81	239
Social Cost in 1000s		8,169.67	3,462.49	239
Report Length		187.29	23.66	239
C. Officer Characteristics				
Black		0.08		1,098
Hispanic		0.05		1,098
White		0.70		1,098
Other Race		0.16		1,098
Female		0.13		1,098
Experience in 2009		13.85	9.01	1,098
D. Neighborhood (CBG) Characteristics				
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 based on data from June 2009 to December 2013. Social costs represent the social cost of index crimes in US$1,000s calculated using cost estimates from Bhatt et al. (2024) deflated to 2009 dollars. Minority neighborhoods are census block groups with less than 50 percent non-Hispanic White residents.

Table 2: Effect on Weekly Officer-Initiated Activity

	OI Stops (1)	OI Arrests (2)	OI Arrest Rate (3)
Investigation	-1,258.97***	-146.14***	-6.17**
	(129.95)	(13.96)	(2.60)
Post-Investigation	-1,013.92***	-205.68***	-24.50***
	(105.94)	(9.10)	(1.92)
Pre-period mean	5,173.78	513.68	99.32
Observations	239	239	239
Week-of-Year FEs	X	X	X

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

Table 3: Effect on Weekly Officer-Initiated Activity by Neighborhood Race

	OI Stops (1)	OI Arrests (2)	OI Arrest Rate (3)
Minority × Investigation	-1.48***	-0.19***	8.28**
	(0.24)	(0.05)	(3.71)
Minority × Post-Investigation	-1.77***	-0.36***	-4.22
	(0.23)	(0.04)	(2.78)
Investigation	-1.77***	-0.20***	-4.87***
	(0.11)	(0.02)	(1.65)
Post-Investigation	-1.28***	-0.26***	-10.33***
	(0.11)	(0.02)	(1.34)
Pre-period mean	8.74	0.87	57.79
Observations	141,488	141,488	87,991
Neighborhood FEs	X	X	X
Week-of-Year FEs	X	X	X

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

Table 4: Effect on Weekly Crimes by Neighborhood Race

	Car Theft (1)	Violent (2)	Property (3)	Social Costs (4)
Minority × Investigation	0.007	-0.014**	0.002	-0.869
	(0.007)	(0.007)	(0.023)	(2.671)
Minority × Post-Investigation	-0.013**	-0.014**	-0.012	0.658
	(0.005)	(0.005)	(0.018)	(2.270)
Investigation	0.000	0.002	-0.063***	0.271
	(0.003)	(0.003)	(0.012)	(1.284)
Post-Investigation	0.016***	0.009***	-0.004	0.596
	(0.003)	(0.002)	(0.010)	(0.955)
Pre-period mean	0.12	0.11	1.00	13.45
Observations	141,488	141,488	141,488	141,488
Neighborhood FEs	X	X	X	X
Week-of-Year FEs	X	X	X	X

Notes: This table reports the results of estimating Equation 5.2 on a balanced neighborhood weekly panel spanning June 2009 to December 2013. The unit of observation is a neighbor calendar week. Minority neighborhoods are census block groups with less than 50 percent non-Hispanic White residents. Column 1 reports the results for weekly car thefts, Column 2 for weekly violent crimes, Column 3 for weekly property crimes (excluding car thefts), and Column 4 for weekly social cost of index crimes in US$1,000s. The social cost of index crimes are calculated using cost estimates from Bhatt et al. (2024) deflated to 2009 dollars. Newey-West standard errors (L=3) are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Effect on Monthly Crimes per 100,000 Residents

	Car Theft (1)	Violent (2)	Property (3)	Social Costs (4)
Seattle x Post	-0.03	0.04	0.20	-9.13
	(0.60)	(0.62)	(2.67)	(300.66)
Pre-period mean	48.29	50.13	426.37	5,755.40

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

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

	Black vs. Other (1)	Hispanic vs. Other (2)	Female vs. Male (3)	High vs. Low Experience (4)	High vs. Low Conviction FE (5)	High vs. Low OI Arrest FE (6)	High vs. Low 911 Arrest FE (7)
			Estimates for Different Officer Traits				
Officer Trait × Investigation	0.47***	-0.19	0.44***	1.92***	-0.11	-0.80***	-0.74***
	(0.11)	(0.20)	(0.10)	(0.07)	(0.08)	(0.08)	(0.08)
Officer Trait × Post-Investigation	0.99***	-0.02	-0.06	2.17***	0.09	-0.28***	-0.92***
	(0.09)	(0.17)	(0.09)	(0.06)	(0.07)	(0.07)	(0.07)
Pre-period mean	3.66	5.30	4.13	2.15	5.26	5.53	6.16
Observations	258,351	258,351	258,351	258,351	258,351	258,351	258,351
Officer FEs	X	X	X	X	X	X	X
Home-Sector-Calendar-Week FEs	X	X	X	X	X	X	X

Notes: This table reports the results of estimating Equation 7.1 on an unbalanced officer weekly panel spanning June 2009 to December 2013 separately for each officer trait described in Section 4. The unit of observation is an officer calendar week, and the outcome variable is weekly officer-initiated (OI) stops. Columns 1 and 2 presents the results for race. Column 3 reports for sex, Column 4 for experience, Column 5 for conviction fixed effects, Column 6 for OI arrest fixed effects, and Column 7 for 911 arrest fixed effects. Newey-West standard errors (L=3) are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Effect on Weekly Officer-Initiated Activity by Stop Type

Panel A: Traffic and Suspicious-Activity Stops

	Traffic			Suspicious Activity		
	Stops	Arrests	Arrest Rate	Stops	Arrests	Arrest Rate
	(1)	(2)	(3)	(4)	(5)	(6)
Investigation	-496.54***	-24.07***	8.78***	-686.97***	-85.76***	5.39
	(33.34)	(2.67)	(3.34)	(34.10)	(6.52)	(4.57)
Post-Investigation	-643.69***	-39.88***	0.31	-986.38***	-128.67***	4.30
	(27.58)	(2.35)	(3.68)	(34.36)	(5.36)	(3.33)
Pre-period mean	1,163.79	71.10	61.40	1,690.07	227.32	134.99
Observations	239	239	239	239	239	239

Panel B: Premise Check and Other Stops

	Premise Check			Other		
	Stops	Arrests	Arrest Rate	Stops	Arrests	Arrest Rate
	(1)	(2)	(3)	(4)	(5)	(6)
Investigation	6.76	-1.65*	-2.00*	-82.23	-34.67***	-17.19***
	(33.91)	(0.93)	(1.04)	(67.77)	(8.43)	(4.68)
Post-Investigation	7.07	-2.94***	-3.46***	609.09***	-34.19***	-51.46***
	(35.49)	(0.60)	(0.73)	(88.41)	(5.67)	(4.45)
Pre-period mean	826.32	6.22	7.47	1,493.60	209.03	139.48
Observations	239	239	239	239	239	239

Notes: This table reports the results of estimating Equation 5.1 on weekly time series spanning June 2009 to December 2013 separately for each stop type. The unit of observation is a calendar week. Panel A reports the estimates traffic and suspicious-activity stops, while Panel B reports the estimates for premise check and other stops. For each stop category, I report estimates for weekly officer-initiated (OI) stops, weekly arrests from OI stops, and the weekly OI arrest rate, which I define as the number of arrests per 1,000 OI stops. All regressions include week-of-the-year fixed effects to adjust for seasonality. Newey-West standard errors (L=3) are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

APPENDIX FOR ONLINE PUBLICATION

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Figure A1: Timeline of the SPD's Federal Investigation
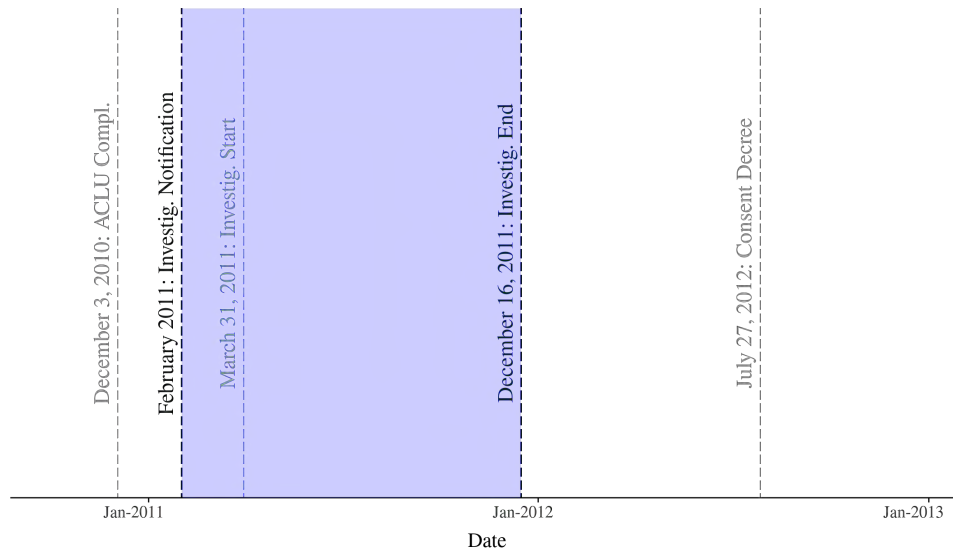
Notes: This figure shows the timeline for the federal investigation into the SPD. For more information, refer to Section 2.

Figure A2: Neighborhood Racial Composition in Seattle
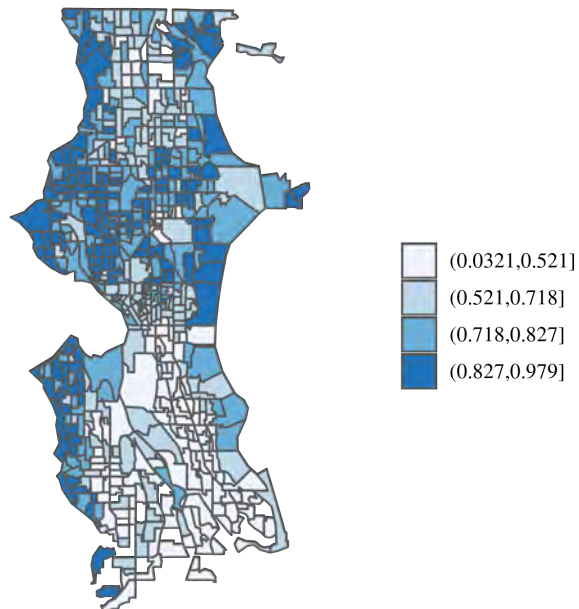
Notes: This figure plots the share of non-Hispanic White residents across neighborhoods in Seattle. Neighborhoods are defined as census block groups.

Figure A3: Weekly Number of Officers Observed in Dispatch Data
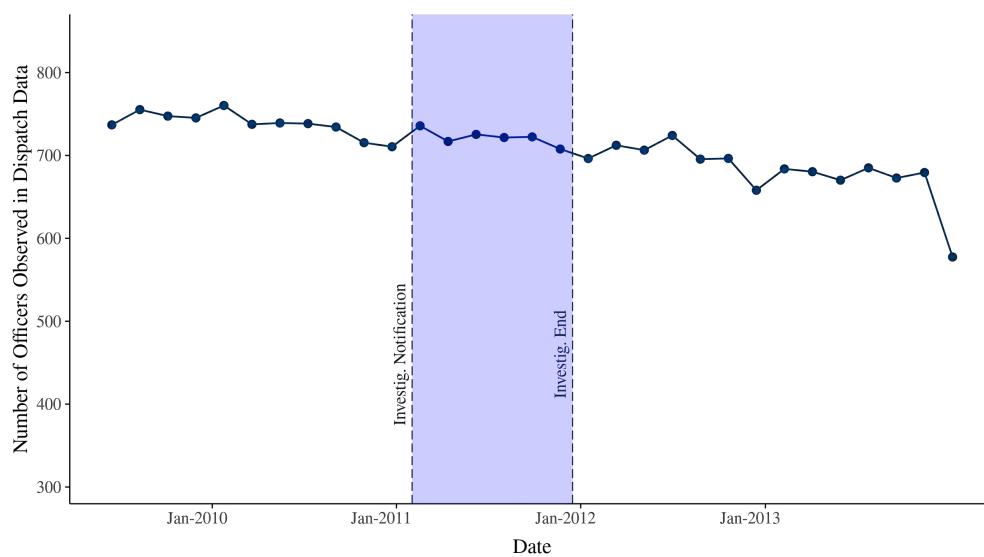
Notes: This figure plots the weekly number of officers observed in the computer-aided dispatch (CAD) data from June 2009 to December 2013.

Figure A4: Monthly Crimes per 100,000 Residents, Synthetic DID

(A) Car Thefts   (B) Violent Crimes

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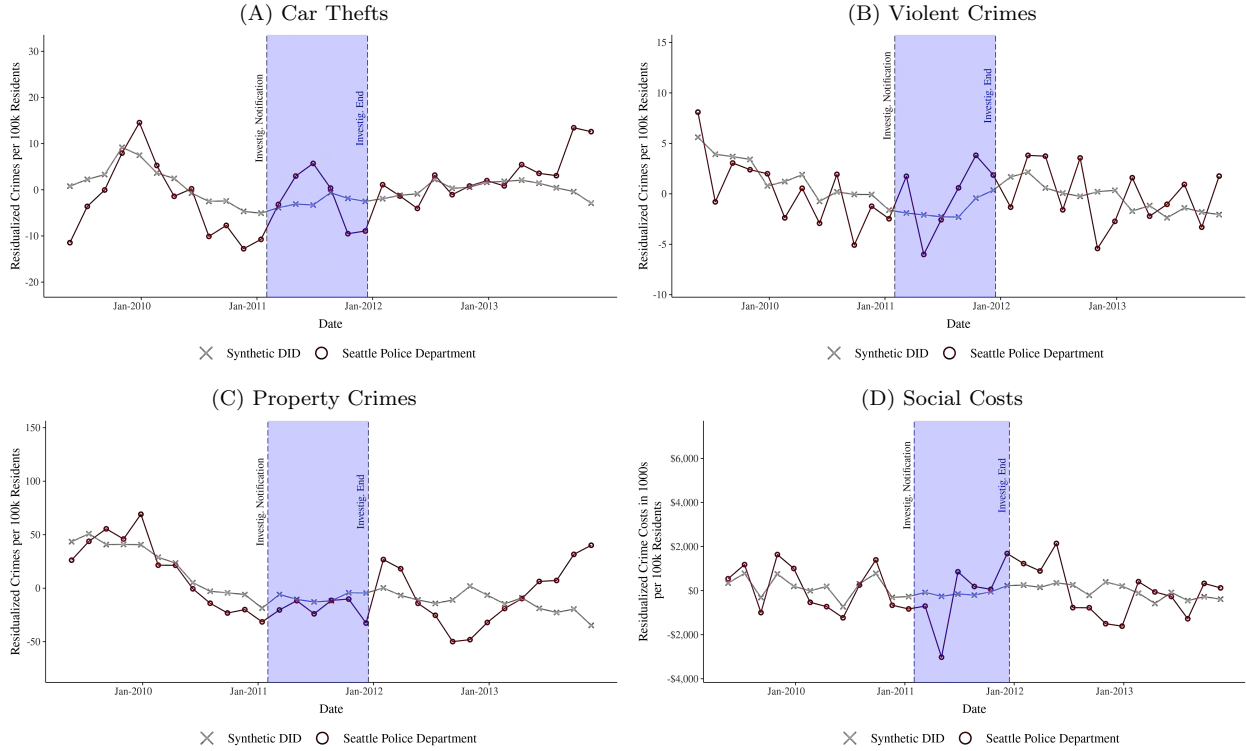
Notes: These figures plots monthly crimes per 100,000 residents, residualized by month-of-the-year fixed effects, in Seattle compared to its synthetic difference-in-differences counterfactual. Panel A reports car thefts, Panel B reports violent crimes, Panel C reports property crimes, and Panel D reports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

Figure A5: Monthly Homicides per 100,000 Residents

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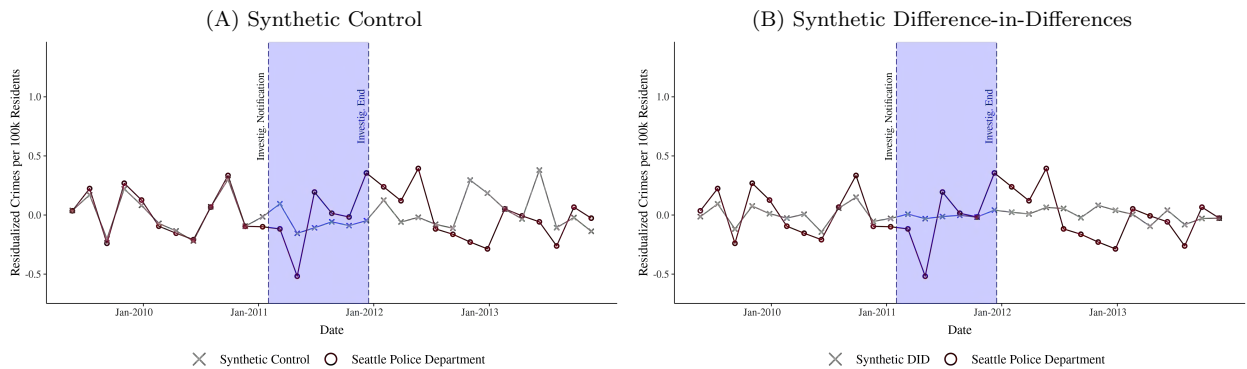
Notes: These figures plots monthly homicides per 100 thousand residents, residualized by month-of-the-year fixed effects, in Seattle compared to its synthetic control counterfactual in Panel A and its synthetic difference-in-differences counterfactual in Panel B.

Figure A6: Weekly Officer-Initiated Stops by Officer Traits
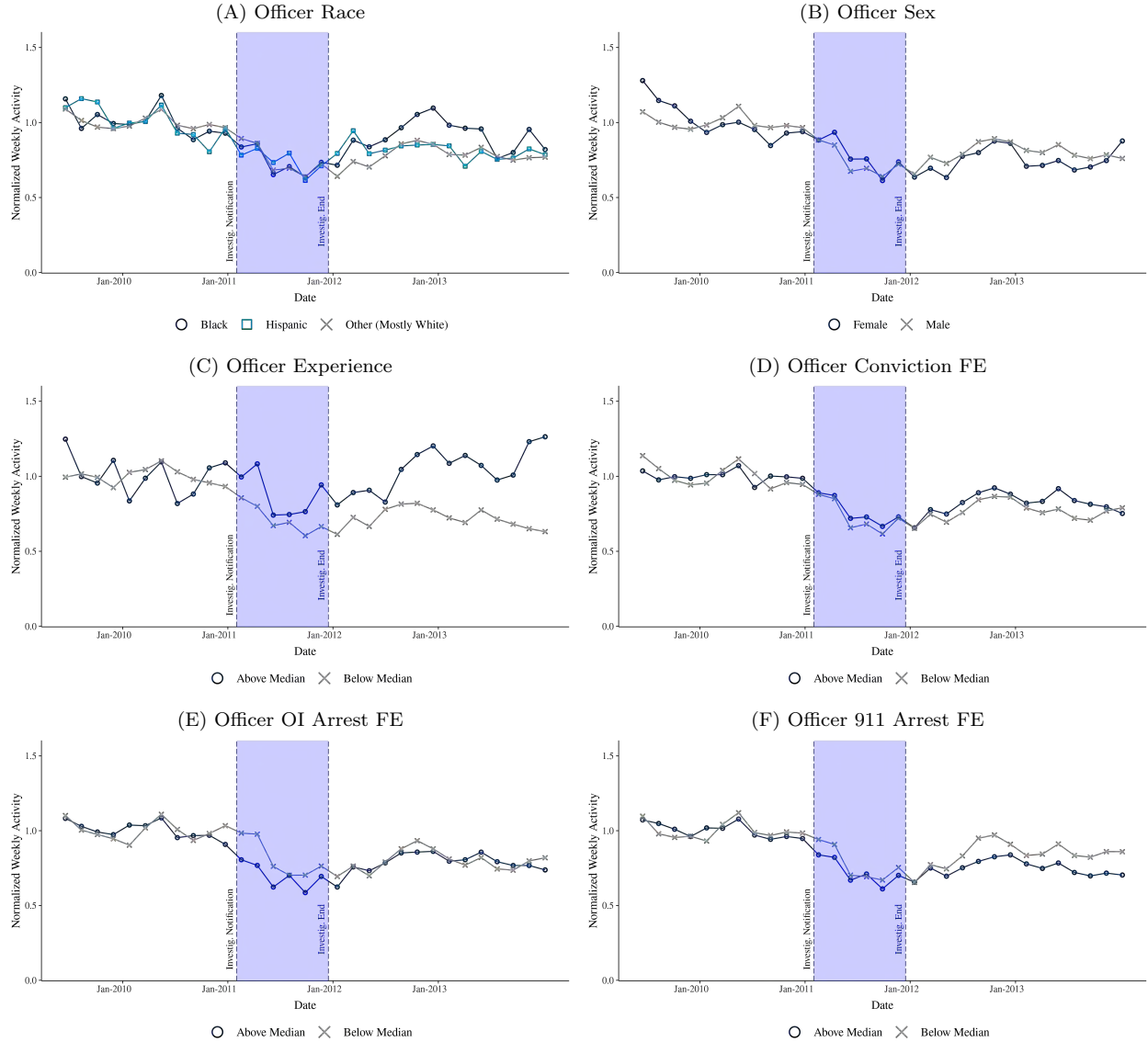
Notes: This figure plots normalized weekly officer-initiated (OI) stops from June 2009 to December 2013 for each officer trait described in Section 4. I normalize each series by its pre-period mean (e.g., the series for Black officers is divided by average weekly OI stops among Black officers in the pre-period). To seasonally adjust values, I residualize by week-of-the-year fixed effects and add back the mean of the fixed effects.

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Figure A7: Weekly Officer-Initiated Activity by Stop Type
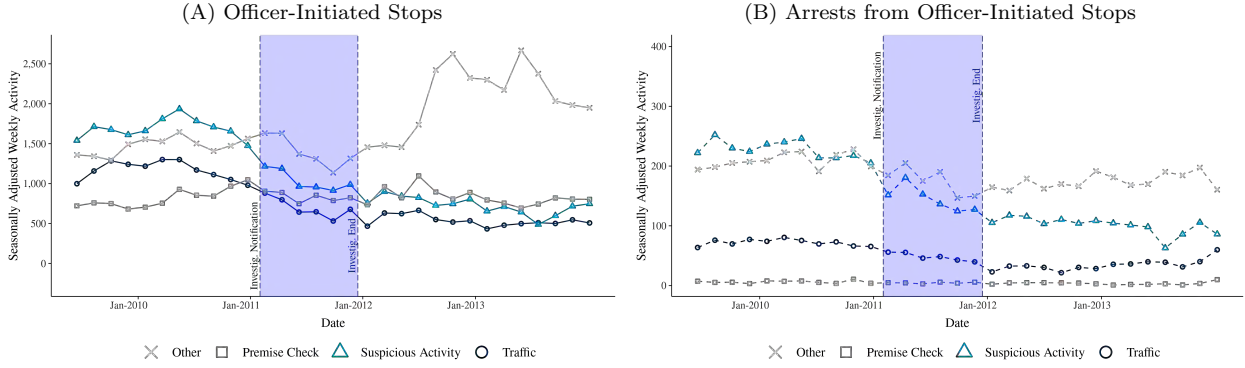
(A) Officer-Initiated Stops

(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 A8: Unadjusted Police Report Length by Crime Discovery Source

(A) Officer-Initiated Stops
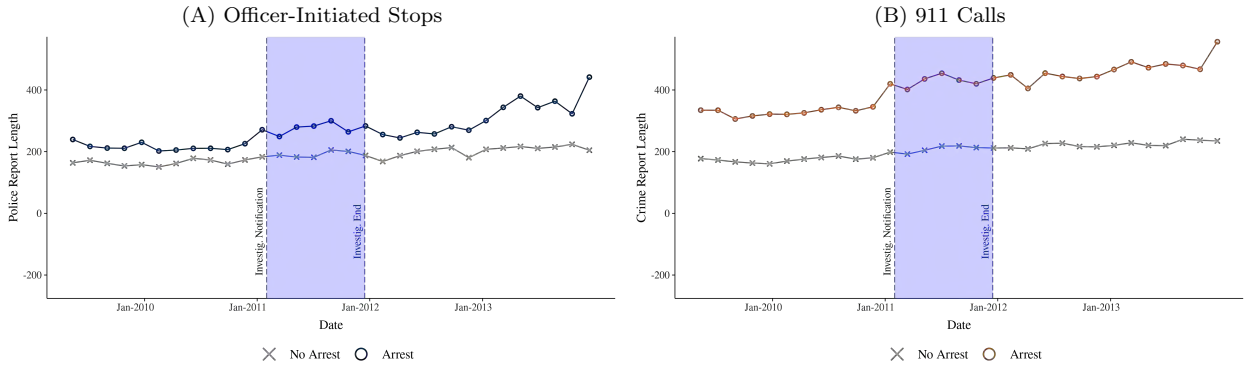
(B) 911 Calls

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

Figure A9: Monthly Fatal Crashes

(A) Synthetic Control

(B) Synthetic Difference-in-Differences
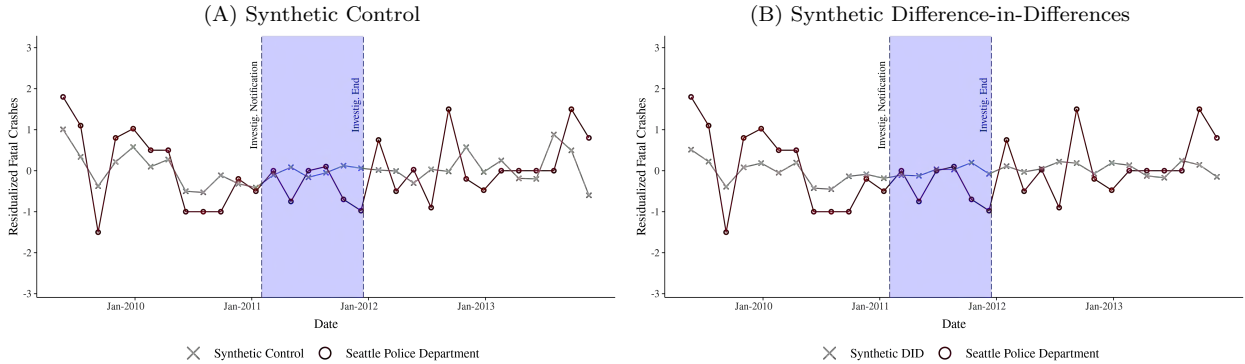

Notes: These figures plots monthly fatal crashes, residualized by month-of-the-year fixed effects, in Seattle compared to its synthetic control counterfactual (Panel A) and its synthetic difference-in-differences counterfactual (Panel B).

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Figure A10: Weekly 911 Calls and Officer-Initiated Stops
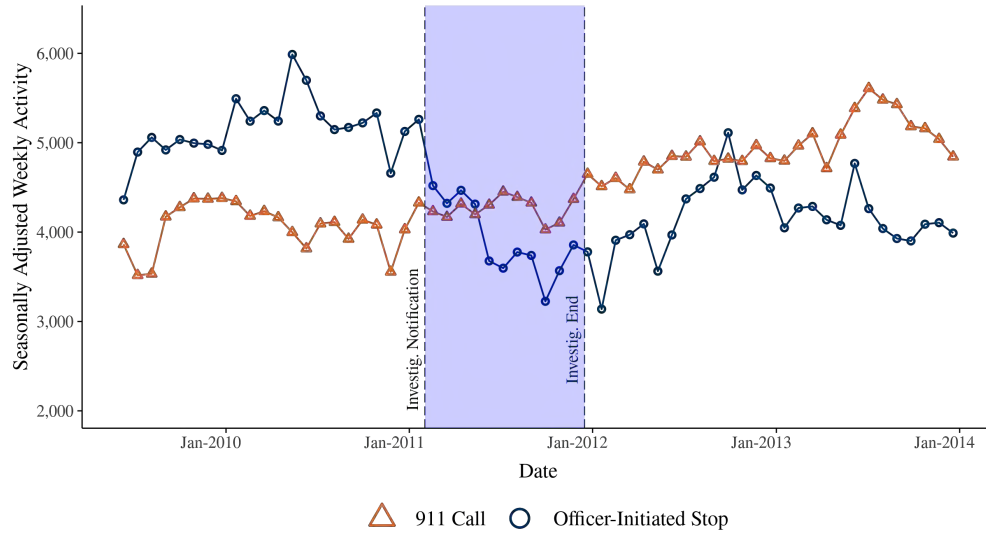
Notes: This figure plots seasonally adjusted weekly 911 calls and officer-initiated stops from June 2009 to December 2013. To seasonally adjust values, I residualize by week-of-the-year fixed effects and add back the mean of the fixed effects.

Figure A11: Log Weekly 911 Calls by Call Type

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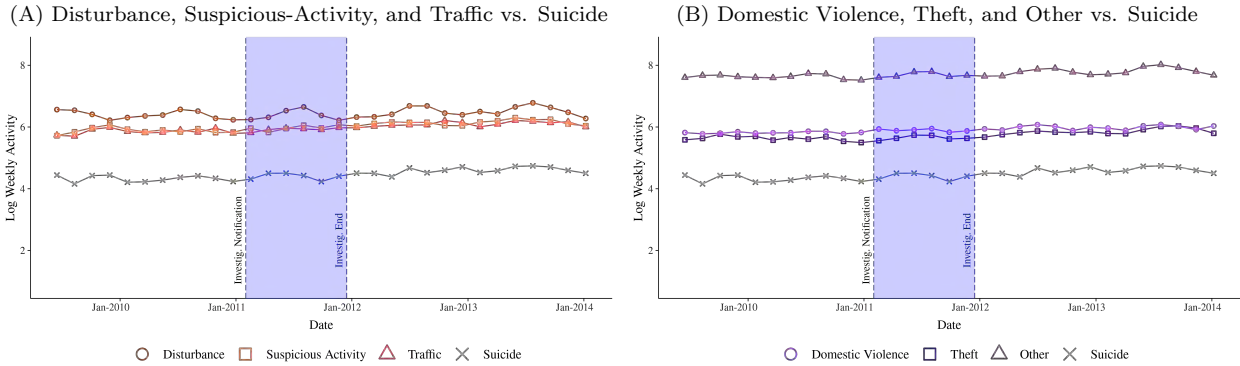
Notes: This figure plots log weekly 911 calls from June 2009 to December 2013 for the different 911 call types. Panel A reports 911 calls for disturbance, suspicious activity, and traffic. Panel B reports 911 calls for domestic violence, theft, and other. I include 911 calls for in-progress or recently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

Figure A12: Other Community Responses

Panel 1: Victimization and Nonreporting over Time

(A) Civilian Victimization Rate

(B) Victimization Nonreporting Rate

$\times$ Control MSAs $\triangledown$ Seattle MSA

$\times$ Control MSAs $\triangledown$ Seattle MSA
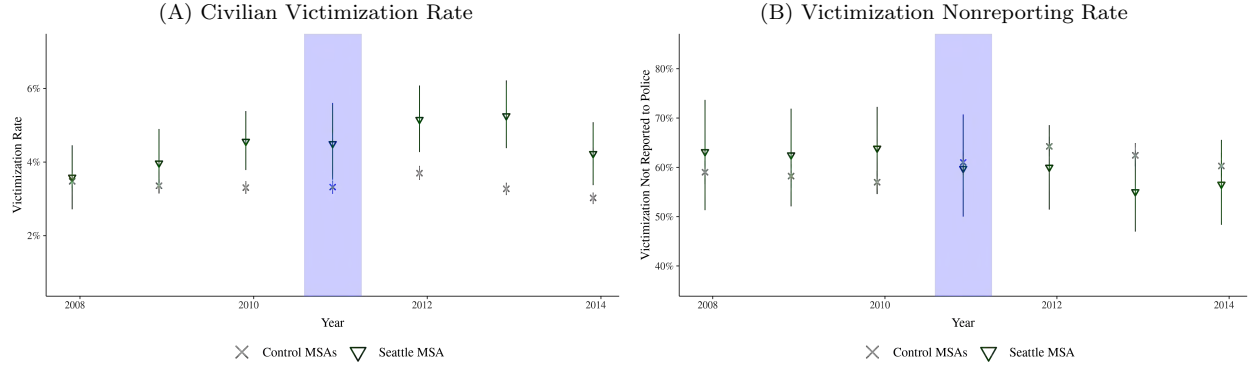
Panel 2: Perceptions of the Police over Time

(A) Nonreporting Due to Police Mistrust

(B) Confidence in Police

$\times$ Control MSAs $\triangledown$ Seattle MSA

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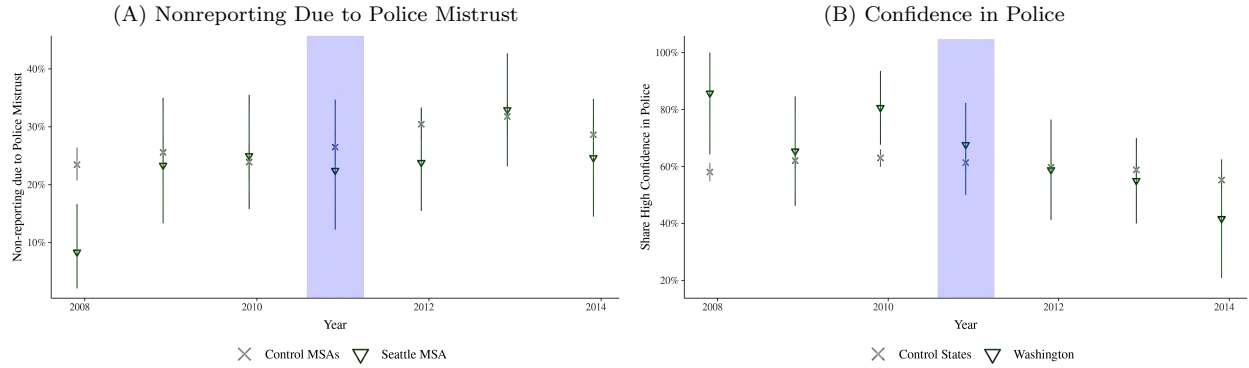
Notes: Panel 1A shows annual victimization rates from the National Crime Victimization Survey (NCVS). Panel 1B shows annual rates of nonreporting of victimizations to the police. Panel 2A shows annual rates of nonreporting attributed to mistrust of the police. Finally, Panel 2B shows annual shares of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

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B   Supplemental Tables

Table B1: Randomization of 911 Calls

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

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

Table B2: Social Cost of Index Crimes in 2009 Dollars

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

Notes: This table reports the social cost of each index crime type from Bhatt et al. (2024) deflated to 2009 dollars.

Table B3: Correlation between Officer Traits

	(1) Black	(2) Hispanic	(3) Female	(4) High 911 Arrest FE	(5) High OI Arrest FE	(6) High Conviction FE	(7) High Experience
Black	1.000						
Hispanic	-0.050	1.000					
Female	-0.028	-0.069	1.000				
High 911 Arrest FE	-0.065	-0.033	-0.083	1.000			
High OI Arrest FE	0.009	-0.067	0.016	0.391	1.000		
High Conviction FE	-0.038	0.001	0.000	0.024	0.149	1.000	
High Experience	-0.042	0.113	-0.058	-0.168	-0.196	-0.095	1.000

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

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Table B4: Synthetic Control Donor Pool with Assigned Weights

Agency	Car Thefts	Violent	Property	Social Costs
Santa Ana Police Department, CA	0.12	0.01	0.00	0.00
Norfolk Police Department, VA	0.07	0.00	0.04	0.00
St. Louis (City) Police Dept, MO	0.07	0.00	0.00	0.02
Kansas City Police Department, MO	0.05	0.00	0.00	0.00
Rochester Police Department, NY	0.04	0.01	0.00	0.01
Springfield Police Department, MA	0.04	0.01	0.00	0.00
San Bernardino Police Department, CA	0.04	0.01	0.00	0.02
Laredo Police Department, TX	0.03	0.01	0.02	0.01
Oklahoma City Police Department, OK	0.03	0.00	0.02	0.02
Baton Rouge Police Department, LA	0.03	0.03	0.04	0.03
Memphis Police Department, TN	0.03	0.02	0.00	0.01
Anaheim Police Department, CA	0.02	0.02	0.00	0.01
Virginia Beach Police Department, VA	0.02	0.01	0.03	0.02
El Paso Police Department, TX	0.02	0.00	0.02	0.01
Salinas Police Department, CA	0.02	0.01	0.03	0.02
Amarillo Police Department, TX	0.02	0.01	0.06	0.01
Anchorage Police Department, AK	0.02	0.00	0.02	0.01
Glendale Police Department, CA	0.02	0.02	0.01	0.01
Lubbock Police Department, TX	0.02	0.00	0.00	0.01
Shreveport Police Department, LA	0.02	0.00	0.00	0.00
Springfield Police Dept, MO	0.02	0.01	0.00	0.00
Sioux Falls Police Department, SD	0.02	0.02	0.00	0.00
Madison Police Department, WI	0.02	0.02	0.00	0.01
Ontario Police Department, CA	0.02	0.01	0.01	0.02
Modesto Police Department, CA	0.02	0.02	0.00	0.01
Durham Police Department, NC	0.02	0.01	0.00	0.02
Salt Lake City Police Department, UT	0.01	0.02	0.05	0.00
Boston Police Department, MA	0.01	0.00	0.02	0.02
Fort Wayne Police, IN	0.01	0.01	0.00	0.02
Colorado Springs Police Department, CO	0.01	0.01	0.00	0.00
Greensboro Police Department, NC	0.01	0.00	0.02	0.02
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	0.01
Oakland Police Department, CA	0.00	0.00	0.01	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	0.01	0.00	0.01
Santa Rosa Police Department, CA	0.00	0.01	0.00	0.01
Oxnard Police Department, CA	0.00	0.03	0.02	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	0.06	0.02
Atlanta Police Department, GA	0.00	0.00	0.00	0.00
Des Moines Police Department, IA	0.00	0.00	0.00	0.01
Continued on next page				

Table B4 – continued from previous page

Agency	Car Thefts	Violent	Property	Social Costs
Boise Police Department, ID	0.00	0.01	0.00	0.01
Rockford Police Dept, IL	0.00	0.01	0.07	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
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
Omaha Police Dept, NE	0.00	0.03	0.00	0.00
Lincoln Police Dept, NE	0.00	0.02	0.00	0.01
Fayetteville Police Department, NC	0.00	0.00	0.04	0.00
Charlotte - Mecklenburg Police Department, NC	0.00	0.01	0.00	0.01
Las Vegas Metro Police Department, NV	0.00	0.01	0.02	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	0.01	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
Nashville Metro Police Department, TN	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	0.01	0.01
Corpus Christi Police Department, TX	0.00	0.00	0.00	0.01
Arlington Police Department, TX	0.00	0.00	0.01	0.00
Fort Worth Police Department, TX	0.00	0.01	0.01	0.01
Newport News Police Department, VA	0.00	0.02	0.00	0.00
Richmond Police Department, VA	0.00	0.01	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	0.01	0.06	0.00

Table B5: Effect on Weekly Officer-Initiated Activity

	OI Stops (1)	OI Arrests (2)	OI Arrest Rate (3)
Investigation	-1,449.62***	-139.73***	-0.83
	(185.44)	(18.94)	(3.59)
Post-Investigation	-1,416.72***	-192.13***	-13.23**
	(323.05)	(26.42)	(5.77)
Linear Trend	2.80	-0.09	-0.08**
	(1.82)	(0.16)	(0.03)
Pre-period mean	5,173.78	513.68	99.32
Observations	239	239	239
Week-of-Year FEs	X	X	X

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

Table B6: Effect on Weekly OI Stops by Neighborhood Race with Full Time Fixed Effects and Clustered Standard Errors

	OI Stops (1)	OI Arrests (2)	OI Arrest Rate (3)
Minority × Investigation	-1.48**	-0.19	8.37*
	(0.75)	(0.13)	(4.30)
Minority × Post-Investigation	-1.77**	-0.36**	-4.20
	(0.82)	(0.16)	(3.95)
Pre-period mean	8.74	0.87	57.79
Observations	141,488	141,488	87,991
Neighborhood FEs	X	X	X
Calendar-Week FEs	X	X	X

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

Table B7: Testing for Equally Proportional Effect on Weekly Officer-Initiated Activity by Neighborhood Race

	OI Stops (1)	OI Arrests (2)	OI Arrest Rate (3)
Minority × Investigation	-1.11***	-0.13**	15.38***
	(0.24)	(0.05)	(3.78)
Minority × Post-Investigation	-2.03***	-0.27***	4.51
	(0.27)	(0.04)	(2.81)
Investigation	-0.51***	-0.06*	18.93***
	(0.18)	(0.03)	(2.42)
Post-Investigation	-2.15***	-0.06**	19.22***
	(0.25)	(0.02)	(2.05)
Pre-period mean × Investigation	-0.15***	-0.18***	-0.44***
	(0.03)	(0.05)	(0.04)
Pre-period mean × Post-Investigation	0.11***	-0.26***	-0.54***
	(0.04)	(0.04)	(0.03)
Pre-period mean	8.74	0.87	57.79
Observations	141,488	141,488	87,940
Neighborhood FEs	X	X	X
Week-of-Year FEs	X	X	X

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

Table B8: Implied Weekly Crimes per 1,000 Officer-Initiated Stops Averted

	Car Thefts	Violent	Property	Social Costs
Minority x Investigation	4.58	-9.35	1.53	-587.12
	[-8.25, 31.46]	[-52.26, -0.13]	[-94.52, 133.33]	[-7,279.26, 5,270.37]
Minority x Post-Investigation	-7.49	-7.82	-7.04	371.87
	[-51.31, -0.81]	[-38.19, 0.52]	[-70.44, 73.36]	[-3,079.99, 6,086.23]
Minority x Post	-4.28	-8.23	-4.76	116.55
	[-25.54, 2.10]	[-35.11, -0.39]	[-66.89, 71.86]	[-3,166.36, 4,464.52]

Notes: This table reports the implied estimates for crimes per 1,000 officer-initiated stops averted in minority neighborhoods based on the estimates in Tables 3 and 4. Minority neighborhoods are census block groups with less than 50 percent non-Hispanic White residents. Social costs represent the social cost of index crimes in US$1,000s calculated using cost estimates from Bhatt et al. (2024) deflated to 2009 dollars. In square brackets, I report the 95% credible interval, which I construct by performing Bayesian bootstrapping across neighborhoods with 1,000 replications (Rubin, 1981). I also report estimates for all time periods after notification about the investigation combined in "Post."

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Table B9: 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	5.43	1.65	5.60	152.11
	(8.58)	(5.67)	(35.63)	(1,291.55)
Pre-period mean	48.29	50.13	426.37	5,755.40

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

Table B10: Estimates for the Effect on Monthly Homicides per 100k Residents

	SC (1)	SDID (2)
Seattle x Post	-0.01	0.00
	(0.06)	(0.22)
Pre-period mean	0.29	0.29

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 B11: Testing for Equally Proportional Effect on Weekly Officer-Initiated Stops by Officer Traits

	Black vs. Other (1)	Hispanic vs. Other (2)	Female vs. Male (3)	High vs. Low Experience (4)	High vs. Low Conviction FE (5)	High vs. Low OI Arrest FE (6)	High vs. Low 911 Arrest FE (7)
Officer Trait × Investigation	0.04	-0.12	0.12	0.17*	0.10	-0.45***	-0.00
	(0.10)	(0.19)	(0.10)	(0.09)	(0.07)	(0.07)	(0.07)
Officer Trait × Post-Investigation	0.51***	0.04	-0.41***	0.29***	0.31***	0.12*	-0.12*
	(0.09)	(0.16)	(0.08)	(0.08)	(0.06)	(0.06)	(0.06)
Pre-period mean × Investigation	-0.32***	-0.32***	-0.32***	-0.32***	-0.32***	-0.32***	-0.32***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Pre-period mean × Post-Investigation	-0.35***	-0.35***	-0.35***	-0.34***	-0.35***	-0.35***	-0.35***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Pre-period mean	3.66	5.30	4.13	2.15	5.26	5.53	6.16
Observations	258,351	258,351	258,351	258,351	258,351	258,351	258,351
Officer FEs	X	X	X	X	X	X	X
Home-Sector-Calendar-Week FEs	X	X	X	X	X	X	X

Notes: This table reports the results of estimating Equation 7.1 on an unbalanced officer weekly panel spanning June 2009 to December 2013 separately for each officer trait described in Section 4. I additionally include controls for each officer's average weekly stops during the pre-period. The unit of observation is an officer calendar week, and the outcome variable is weekly officer-initiated (OI) stops. Column 1 and 2 report the results for officer race. Column 3 reports for officer sex, Column 4 for officer experience, Column 5 for officer conviction fixed effects, Column 6 for officer OI arrest fixed effects, and Column 7 for 911 arrest fixed effects. Newey-West standard errors (L=3) are reported in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table B12: Effect on Police Report Length

| | Report Length | | | |
| | Full Sample | 911 Calls | OI Stops | Other Source |
	(1)	(2)	(3)	(4)
Arrest	117.01***	118.27***	77.26***	127.26***
	(4.01)	(3.94)	(7.63)	(6.00)
Arrest × Investigation	41.88***	51.52***	34.06***	37.38***
	(4.17)	(5.53)	(8.15)	(7.79)
Arrest × Post-Investigation	61.69***	68.61***	46.35***	52.36***
	(6.31)	(5.69)	(13.07)	(8.38)
Investigation	28.39***	36.16***	23.91***	22.17***
	(1.57)	(2.26)	(5.87)	(1.43)
Post-Investigation	40.77***	50.33***	38.92***	28.45***
	(1.36)	(1.41)	(5.75)	(1.40)
Pre-period mean	159.97	205.92	198.23	118.71
Observations	327,164	121,810	46,617	158,737
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 7.2 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 B13: Estimates for the Effect on Monthly Fatal Crashes

	SC (1)	SDID (2)
Seattle x Post	0.01	-0.01
	(0.08)	(0.33)
Pre-period mean	2.00	2.00

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

Table B14: Effect on Log 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)
Investigation	0.09*	-0.00	0.08***	0.08**	0.09***	0.01	0.06***
	(0.05)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.02)
Post-Investigation	0.28***	0.13***	0.26***	0.26***	0.17***	0.24***	0.19***
	(0.03)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)
Observations	239	239	239	239	239	239	239
Week-of-Year FEs	X	X	X	X	X	X	X

Notes: This table reports the results of estimating Equation 5.1 on weekly time series spanning June 2009 to December 2013 separately for each 911 call type. The unit of observation is a calendar week, and the outcome variable is log weekly 911 calls of each type. Newey-West standard errors (L=3) are reported in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01.

C   Model Derivations

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

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

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

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

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

Second-Order Conditions.

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

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

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

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

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:

$$V_{Sc_m} = -\delta_m$$
$$V_{Gc_m} = 0$$

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

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

The comparative statics are as follows:

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

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.

D   Officer-Initiated Stop and 911 Call Classification

D.1   Officer-Initiated Stops

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

Premise Check: Premise Check Officer Initiated Onview Only.

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

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

Other: Abandoned Vehicle; Abduction No Known Kidnapping; Acc Hit And Run No Injuries Includes Ip Jo; Acc Non Injury Blocking; Acc Report Non Inj Non Blkg Or After Fact Inj; Acc Unk Injuries; Acc With Injuries Includes Hit And Run; Alarm Audible Automobile Unocc Anti Theft; Alarm Comm Hold Up Panic Except Banks; Alarm Comm Inc Bank Atm Schools Bsn; Alarm Comm Silent Aud Burg Incl Banks; Alarm Duress Panic Bus Taxi Car Prsn Not Dv; Alarm Residential Burglary Silent Audible; Animal Dangerous; Animal Injured Dead Hazard Other; Animal Ip Jo Bite; Animal Ip Jo Dangerous; Animal Report Bite; Arson Ip Jo; Arson Report; Aslt Dv; Aslt Ip Jo Dv; Aslt Ip Jo Person Shot Or Shot At; Aslt Ip Jo With Or W O Wpns No Shootings; Aslt Molested Adult Groped Fondled Etc; Aslt Person Shot Or Shot At; Aslt With Or W O Weapons No Shootings; Assigned Duty Centurylink Stadium; Assigned Duty Community School Special Event; Assigned Duty Court; Assigned Duty Detail By Supervisor; Assigned Duty Foot Beat From Assigned Car; Assigned Duty Hospital Guard; Assigned Duty In Service Training; Assigned Duty Meet W Supervisor Out Of Svc; Assigned Duty Other Escort; Assigned Duty Reports; Assigned Duty Seattle Center Event; Assigned Duty Stakeout; Assigned Duty Station Duty Clerk Mail Etc; Assigned Duty Transport Evidence Equipment; Assist Other Agency Emergency Service; Assist Other Agency Routine Service; Assist Public No Welfare Chk Or Dv Order Service; Assist Spd Routine Service; Assist Spd Urgent Service; Auto Recovery; Auto Theft Ip Jo Vehicle Plates Tabs; Auto Theft Loss Plates And Or Tab; Auto Theft Veh Theft Or Theft Recovery; Bias Racial Political Sexual Motivation; Bomb Threats Ip Jo; Burg Comm Burglary Includes Schools; Burg Ip Jo Comm Burg Includes Schools; Burg Ip Jo Res Incl Unocc Structures; Burg Res Incl Unocc Structures On Prop; Burn Reckless Burning; Carjacking Ip Jo Robbery; Child Aband Abused Molested Neglected; Child Ip Jo Aband Abuse Molest Neglect; Child Ip Jo Luring; Child Luring; Custodial Interference Dv; Demonstrations; Detox Pickup Fire Police Standing By; Detox Request For; Dist Dv No Aslt; Dist Ip Jo Dv Dist No Aslt; Disturbance Miscellaneous Other; Doa Casualty Dead Body; Down Check For Person Down; Dui Driving Under Influence; E R T Hostage; Elementary School Visit; Escape Ip Jo Prisoner; Explosion Ip Jo; Explosion With Significant Delay; Fight Ip Jo With Weapons; Fight Ip Physical No Weapons; Fight Jo Physical No Weapons; Fight Verbal Oral No Weapons; Fireworks Nuisance No Hazard; Follow Up; Foot Eluding Police; Found Person; Fraud Forgery Bunco Scams Id Theft Etc; Fraud Fraud Including Bunco; Gambling; Gas Maintenance Wash Garage; Haras No Bias Threats Or Maliciousness; Harbor Water Debris Navigational Hazards; Harbor Water Emergencies; Haz Imminent Thrt To Phys Safety No Haz Mat; Haz Potential Thrt To Phys Safety No Hazmat; Hospital Guard Assignment; Hzmat Haz Materials Leaks Spills Or Found; Illegal Dumping; Informational Broadcasts; Infrastructure Checks; Injured Ip Jo Person Industrial Accident; Injured Person Industrial Accident; Juvenile Runaway;

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Juvenile Runaway Pickup; Lewd Exposing Flashing; License Inspections Check For; Liquor Violations Adult; Liquor Violations Business; Liquor Violations Minor; Littering; Mental Person Or Pickup Transport; Missing Adult; Missing Alzheimer Endangered Elderly; Missing Child; Mvc Non Injury Blocking; Narcotics Found; Narcotics Violation Of Soda Order; Narcotics Violations Loiter Use Sell Nars; Narcotics Warrant Service; No Answer When Called; Noise Animal Includes Barking Dogs; Noise Dist General Const Resid Ball Play; Noise Disturbance Party Etc; Nuisance Mischief; Open Building Door Etc; Order Assist Dv Vic W Srvc Of Court Order; Order Ip Violation Of Dv Court Order; Order Service Of Dv Court Order; Order Violating Dv Court Order; Order Violation Of Court Order Non Dv; Out At Range; Out Of Car No Reason Given; Out To Precinct Station; Overdose Drug Related Casualty; Panhandling Aggressive; Parking Violation Except Abandoned Car; Parks Violations Cites Includes Exclusions; Peace Standby To Assure No Court Ordr Svc; Pedestrian Violations; Phone Obscene Or Nuisance Phone Calls; Power Out Poles And Transformers; Prisoner Escort Busy Code; Property Damage; Property Found; Property Lost Or Missing; Prowler; Prowler Ip Jo; Purse Snatch Ip Jo Robbery; Purse Snatch Robbery; Pursuit Foot Or Vehicle; Rape; Rape Ip Jo; Request To Watch; Robbery Includes Strong Arm; Robbery Ip Jo Includes Strong Arm; Service Welfare Check; Sex In Public Place View Incl Masturbation; Sex Offender Failure To Register; Sfd Assist On Fire Or Medic Response; Shoplift Theft; Shots Delay Includes Heard No Assault; Shots Ip Jo Includes Heard No Assault; Sick Person; Sleeper Aboard Bus Commuter Train; Stadium Event Assignment; Suicide Ip Jo Suicidal Person And Attempts; Suicide Suicidal Person And Attempts; Swat Critical Incident Logs And Callouts; Test Call Only; Theft Does Not Include Shoplift Or Svcs; Theft Of Services; Threats Dv No Assault; Threats Incls In Person By Phone In Writing; Tracking Alarm; Trees Down Obstructing Public Prop No Haz; Trespass; Tru Acc Hit And Run; Tru Commercial Burglary; Tru Forgery Chks Bunco Scams Id Theft; Tru Theft; Undercover Ops Caution Includes Stakeouts; Unknown Ani Ali Landline Includes Open Line; Unknown Complaint Of Unknown Nature; Vice Pornography; Vice Prostitution; Vice Violation Of Soap Order; Warrant Felony Pickup; Warrant Misd Warrant Pickup; Warrant Pickup From Other Agency; Warrant Search Caution Excl Narcotics; Water Floods Broken Mains Hydrants No Haz; Weapn Gun Deadly Wpn No Thrts Aslt Dist; Weapn Ip Jo Gun Deadly Wpn No Thrt Aslt Dist; Wires Down Phone Electrical Etc.

D.2   911 Calls

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

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

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

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

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

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

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

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

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

E Construction of Measured Officer Traits

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

The first trait is the OI arrest fixed effect, which captures an officer's arrest propensity in OI stops conditional on stop characteristics. To construct this measure, I use an officer–dispatch-level data set containing all OI stops between June 2009 and January 2011 to estimate the following ordinary least squares

specification:

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

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

The second measure I construct is the 911 call arrest fixed effect, which captures an officer's arrest propensity in 911 calls conditional on call characteristics. To construct this measure, I use an officer–dispatch-level data set containing all 911 call dispatches between June 2009 and January 2011 to estimate the following ordinary least squares specification:

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

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 of dispatch characteristics including 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 January 2011. I then estimate the following ordinary least squares specification:

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

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