Data for Change

A Statistical Analysis of Police Stops, Searches, Handcuffings, and Arrests in Oakland, Calif., 2013-2014

Stanford | **SPARQ**
Social Psychological Answers to Real-world Questions

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EXECUTIVE SUMMARY

Law enforcement agencies across the United States are facing claims that they discriminate against community members of color. Inquiries into these claims typically take one of two approaches: either attack the agency for intentional racism, or deny the presence of racial disparities altogether. Yet neither of these approaches has yielded adequate progress toward many agencies' stated mission of serving their communities with fairness and respect.

Taking a different approach, the City of Oakland engaged our team of Stanford social psychologists to examine relations between the Oakland Police Department (OPD) and the Oakland community, and then to develop evidence-based remedies for any racial disparities we might find. Since May 2014, our team has undertaken five research initiatives. We describe our research methods, findings, and recommendations in *Strategies for Change: Research Initiatives and Recommendations to Improve Police-Community Relations in Oakland, Calif.* We provide a technical report of our main research initiative, a thorough analysis of OPD stop reports, in *Data for Change: A Statistical Analysis of Police Stops, Searches, Handcuffings, and Arrests in Oakland, Calif., 2013-2014.*

Across our research programs, we indeed uncovered evidence that OPD officers treat people of different races differently. At the same time, we found little evidence that this disparate treatment arose from overt bias or purposeful discrimination. Instead, our research suggests that many subtle and unexamined cultural norms, beliefs, and practices sustain disparate treatment. Our findings also suggest 50 evidence-based actions that agencies can take to change department cultures and strengthen police-community ties.

Below, we highlight some of our research initiatives, findings, and recommendations for improving police-community relations in Oakland and other parts of the United States.

The 5 Research Initiatives

- Statistical analyses of "stop data" from 28,119 forms that 510 OPD officers filed after stopping drivers and pedestrians in Oakland, Calif., between April 1, 2013, and April 30, 2014 (for a summary, see Chapter 1 of *Strategies for Change;* for the technical report, see *Data for Change*);
- Development of computational tools to analyze linguistic data from body-worn cameras (BWCs) and, using these tools, analyses of some 157,000 words spoken by OPD officers during 380 stops in April 2014 (see Chapter 2 of *Strategies for Change*);
- Development of computational tools to analyze written narratives from police stop data forms, and, using these tools as well as human experts, analyses of some 1,000 OPD officer narratives from April 2014 (see Chapter 3 of *Strategies for Change*);
- Two surveys of 416 Oakland community members regarding their attitudes toward and experiences with OPD officers (see Chapter 4 of *Strategies for Change*);
- To mitigate racial disparities, development and evaluation of implicit bias and procedural justice training modules with some 700 OPD officers (see Chapter 5 of *Strategies for Change*).

Key Findings

- OPD officers stopped, searched, handcuffed, and arrested more African Americans than Whites, a finding that remained significant even after we controlled for neighborhood crime rates and demographics; officer race, gender, and experience; and other factors that shape police actions;
- Some 60% of OPD stops were of African Americans, who make up 28% of Oakland's population;
- Of OPD officers making at least one stop during the 13-month period of study:
  - Only 20% stopped a White person, while 96% stopped an African American person;
  - Only 26% handcuffed a White person, while 72% handcuffed an African American person (excluding arrests);
  - Only 23% conducted a discretionary search of a White person, while 65% conducted a discretionary search of an African American person.
- When OPD officers could identify the person's race before a stop, they were much more likely to stop an African American, as compared to when officers could not identify the person's race;
- With African Americans, OPD officers used more severe legal language (e.g., mentioned *probation, parole,* and *arrest*) and offered fewer explanations for the stop than with Whites;
- In police-initiated interactions, African American and Hispanic Oakland residents felt more disrespected and misunderstood than did White and Asian Oakland residents.

Select Recommendations

- Our findings suggest the OPD has a culture where officers stop, search, handcuff, and arrest more African Americans than Whites. We suspect many other law enforcement agencies have similar cultures. In *Strategies for Change*, we therefore recommend that the OPD and other agencies regularly review their policies, practices, and procedures for evidence of disparate impact.
- As our findings reveal that less-experienced officers show more racial disparities, better training of new officers could likely reduce disparate treatment. To this end, *Strategies for Change* presents several recommendations to improve officer training.
- Although the OPD collects copious amounts of data, few measures track the OPD's relations with its community. In *Strategies for Change*, we therefore recommend several actions that the OPD and other law enforcement agencies can take to measure what matters most.
- More broadly, we observe that many law enforcement agencies view data as evidence to be used for punishment, rather than as feedback to be used for improvement. Consequently, these agencies are reluctant to collect and use data. In *Strategies for Change*, we recommend more than a dozen actions that the OPD and other law enforcement agencies can take to leverage their data for learning and change.

Chapter 1 | STOP DATA OVERVIEW AND DEFINITION OF TERMS

Our task

In May 2014, the City of Oakland contracted with our team of Stanford University researchers to assist the Oakland Police Department (OPD) in complying with a federal order to collect and analyze data on OPD officers' self-initiated stops[1] of pedestrians and vehicles by race. Our task was to analyze the reports that OPD officers completed after every stop they initiated between April 1, 2013, and April 30, 2014. These reports are called Field Interview/Stop Data Reports (FI/SDR), and the information they contain is called *stop data.*

We present our independent, detailed, and rigorous assessment of these stop data in the current document, *Data for Change: A Statistical Analysis of Police Stops, Searches, Handcuffings, and Arrests in Oakland, Calif., 2013-2014.* In addition, we summarize the findings of this stop data analysis, discuss four other research initiatives, and list 50 recommendations for reform in a second document, *Strategies for Change: Research Initiatives and Recommendations to Improve Police-Community Relations in Oakland, Calif.*

Our approach

Analysts usually take one of two approaches to police stop data. The first approach is to lay out the evidence for racial disparities in stops, and then conclude that the police are racists who are deliberately targeting people of color. This approach intends to shake law enforcement agencies into changing their ways. Instead, it usually incites so much police resistance that meaningful reform becomes difficult, if not impossible.

The second approach is the opposite of the first: Analysts find no evidence for racial disparities in stops. These analysts often use bloated statistical models so chock-full of covariates (i.e., control variables) that any evidence of disparate treatment disappears. For instance, their reports often conclude that African Americans are more likely to commit crime than are other groups, and so police are just going where the crime is. Everything is as it should be. There is nothing to see here. Yet the daily experiences of communities of color suggest otherwise, and their frustration with these null-finding reports harms relations with police.

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[1] For a stop to be included in this data set, an officer must have been required to complete a Field Interview/Stop Data Report (FI/SDR). In other words, the stop must have been self-initiated and have involved one or more members of the community who were detained, arrested, or subjected to a search or the request to be searched. Casual encounters in which officers talked to community members, but the community member remained free to leave at any time, are not captured here.

In our stop data analysis, we take a third approach—a problem-solving approach–that concludes with neither attack nor denial. We report some real and significant racial disparities in OPD stops, searches, handcuffings, and arrests, even after accounting for crime rates, demographics, and other factors that influence policing activity. The OPD acknowledges these disparities and is eager to address them. To this end, we have conducted our analyses in a manner that allows the OPD to make evidence-based changes in their policies, practices, and procedures. For example, using statistical models, we have isolated the conditions under which racial disparities are greatest and least. Simply knowing where, when, and how racial disparities are likely to emerge gives the agency direction on how to lessen them. This approach has yielded dozens of tactics that the OPD and other law enforcement agencies can undertake to reduce racial disparities. In other words, our approach both acknowledges existing racial disparities in policing and gives police the tools they need to mitigate and perhaps even eliminate these disparities.

Soon, a new California assembly bill (AB 953) will require law enforcement agencies across the state to collect the sort of stop data we have analyzed here. Yet to date, many law enforcement agencies are not sure how to use their data to make change, as they lack a common model for addressing racial disparities in a productive way. Here we offer a model of how policing agencies can use data to solve problems, instead of using data to attack or deny.

As researchers, we can apply a problem-solving approach only when law enforcement agencies value, trust, and understand this approach. The OPD is such an agency. The OPD leadership has given us unprecedented access to the data on which our work relies. They understand that our findings may be unfavorable at times, yet they are poised to address any racial disparities that come to light. Because of their progressive position, we now understand more about improving police-community relations than ever before. On this issue, the OPD has contributed greatly to the Oakland community, many other communities, and the law enforcement industry as a whole.

Overview of the data

During this 13-month time period, 28,119 stops were recorded by 510 sworn OPD officers. Each of these officers made an average of 55 stops during the 13-month period under examination (median number of stops = 35, Interquartile Range[2] = [9 ; 82]). It is thus worth keeping in mind throughout this report that the median number of documented stops for a given officer was only one stop every

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[2] The Interquartile Range (IQR) is a summary of a distribution based on dividing a data set into quartiles, or four equal parts. The IQR lies between the first (25%) and the third (75%) quartiles—it thus literally describes the values occupied by the 50% of observations located in the middle of the distribution. The IQR is a useful measure of central tendency as well as of the variability in a sample or population that is not too affected by outliers, and does not make implicit assumptions about the shape of this distribution (for example, compared to the standard deviation).

10 days. Keeping in mind that one stop involving multiple individuals can generate many stop reports (see below), the number of effective stops is likely to be even smaller. It is thus fair to say that the actions examined in this report represent a very small fraction of an officer's professional activities.[3] Of the officers who made stops during the time period, 456 (89%) are men and 54 (11%) are women. In terms of officer race, 43% are White, 22% are Hispanic, 17% are African American, 14% are Asian, and 4% are listed as "Other." Only 37 (7%) out of these 510 officers were Oakland residents at the time that we acquired this data (the fall of 2014), while the vast majority of officers, 93%, did not reside in Oakland. The average age of officers at the time the stops were made was 37 years (Median age = 37 years, IQR = [31 ; 43]). The average years of experience on the force at the OPD was 9 years (Median = 7 years, IQR = [3 ; 14]).

Type of stops

The majority of stops, 69%, were vehicle stops (see Table 1.1). Another quarter of these stops were pedestrian stops. The remainder of the stops fell into the categories of bicycle stops, 4%, and stops recorded as "other," 2%. This category of "other" stops is intended to be a catchall for stops that cannot be categorized as vehicle, pedestrian, or bicycle stops. Based on a sample of such stops that the OPD reviewed, examples of stops for which the officer selected "other" include compliance checks at the residence of registered sex offenders, going to the residences of wanted persons or persons on probation/parole, and investigating suspicious parked vehicles.

Table 1.1. Stops by type

	Percentage of All Stops	Raw Number
Vehicle	69.2%	19,468
Pedestrian	24.9%	6,995
Bicycle	3.8%	1,081
Other	2.0%	575

Who gets stopped?

Three-quarters of all stops were of men, while one-quarter of stops were of women (Table 1.2).

[3] The majority of an officer's regular duties include responding to calls for service as relayed by the dispatcher (e.g., 911 and other emergency calls) and investigating crimes. Less frequently, officers also work special events like protests, parades, and sporting events.

Table 1.2. Stops by gender of person stopped

Gender	Percentage of All Stops	Raw Number
Male	74.8%	21,042
Female	25.1%	7,069

Examining the stops by age (Table 1.3) reveals that members of the 18- to 29-year-old age group were most represented among those stopped. Of the total stops, nearly 12,000, or 42%, were of 18- to 29-year-olds. Stops of persons aged 17 or younger were the least common; only 3% of all stops involved a juvenile.

Table 1.3. Stops by age group of person stopped

Age Group	Percentage of All Stops	Raw Number
17 years or younger	2.8%	801
18 to 29 years	42.3%	11,904
30 to 39 years	22.2%	6,229
40 years or older	32.7%	9,185

African Americans were the racial group most often stopped (Table 1.4). Sixty percent of stops, or nearly 17,000 stops, were of African Americans. Stops of African Americans were made at a rate of more than three times that of the next most common group, Hispanics. Nearly 5,000 stops, or 18% of total stops, were of Hispanics. There were 3,661 stops of Whites, which comprised 13% of total stops. Stops of Asians and of people categorized as Other were the least frequent, 7% and 3%, respectively.

Note that the race of the person stopped is determined by the officer's perception, and does not necessarily reflect the self-identification of the person who was stopped. On the stop data form, officers indicate what they thought the race of the person was using the following options: W-White; A-Asian; B-Black; H-Hispanic; I-Native American; P-Pacific Islander; M-Middle Eastern; O-Other. We retained the White, Black, Asian, and Hispanic categories as the officer originally entered them. To avoid small counts, we combined racial groups who were very rarely stopped (e.g., Native Americans and Pacific Islanders) and people who were listed as Other by the officer into the "Other" category.

Table 1.4. Stops by race of person stopped

Race	Percentage of All Stops	Raw Number
African American	59.8%	16,818
Hispanic	17.5%	4,933
White	13.0%	3,661
Asian	6.5%	1,827
Other	3.1%	880

Why are people stopped?

The legal basis of a stop can fall into one of five categories: traffic violation, probable cause, reasonable suspicion, consensual encounter, or because the person stopped was on probation/parole. A traffic violation was the most common reason for a stop: 18,100 stops, or 64% of stops. Nearly 6,000 stops were made because of probable cause. Together, reasonable suspicion and consensual encounters accounted for 3,667 stops, or 13%. Finally, probation/parole was the least common reason for a stop being made: Only 493 stops, or just under 2% of all stops, were made because of probation/parole (Table 1.5).

Table 1.5. Stops by reason for encounter

Reason for Encounter	Percentage of All Stops	Raw Number
Traffic Violation	64.4%	18,100
Probable Cause	20.8%	5,854
Reasonable Suspicion	8.7%	2,453
Consensual Encounter	4.3%	1,214
Probation/Parole	1.8%	493

To be captured in this data set, a stop must have been self-initiated by the officer and officers must have completed an FI/SDR. From here on out, we will refer to this form simply as the “stop data form.” Note that self-initiated stops comprise just one small part of what police officers typically do on the job. The majority of an officer’s regular duties include responding to calls for service as relayed by the dispatcher (e.g., 911 and other emergency calls) and investigating ongoing crimes and reported crimes. Less frequently, officers also work special events like protests. Parades and sporting events are typically staffed by officers working overtime. Nonetheless, because self-initiated stops reflect a relatively higher level of discretion than responding to a call, scientists commonly examine these data to determine whether or not there is a pattern of racial disparities. Past research has found that discretion allows for treatment of people to vary as a function of other factors, such as race. In contrast, when behavior is heavily constrained by the situation (e.g., officers' being called to the scene of a homicide), procedures are more standardized and there may be fewer possibilities for variation by race or other factors.

When are officers required to fill out a stop data form?

Members of the OPD are required to complete a stop data form for all self-initiated encounters that involve one or more persons subject to detention,[4] arrest, search, or request to search. According to the OPD's Report Writing Manual,[5] self-initiated encounters are "encounters that are not related to any radio dispatched call for service, citizen flag-down, or encounters conducted pursuant to the service of a search warrant" (p. 1). In other words, the *officer* makes the decision to begin an interaction with a member of the community. This decision may be based on an officer directly viewing or having reason to suspect a law has been violated and/or may be based on a larger enforcement strategy or more general investigatory purpose.

The manual gives numerous examples of when a stop data form is required and when it is not required. For instance, if an officer were to engage in casual conversation with a member of the community while walking the beat, a stop data form would generally not be required. If "while conversing with [the community member] the officer asks if he/she can produce identification," a stop data form is still not required, "provided the officer simply asks and does not demand or coerce his/her identification."[6] If, however, an officer were to ask a member of the community if he/she is on probation or parole, a stop data form would be required. Another example in the manual specifies that if during an operation, "an undercover officer asks a uniformed officer to stop a vehicle for a vehicle code violation," a stop data form is required and must be completed by the officer who executed the stop. An officer is required to fill out a stop data form if he or she conducts a search or makes a request to conduct a search. Even during a consensual encounter, when the person remains free to leave at any time, a stop data form is required if an officer at any time asks for a person's consent to search. This requirement exists regardless of whether the person consents to the search and whether the search is actually conducted.[7]

The Report Writing Manual also provides guidelines for whether and for whom a stop data form is required in the case that multiple people are involved in the same self-initiated stop. For example, a

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[4] Detention is "a temporary seizure of a person to determine if the person seized is involved in criminal activity. The seizure must be supported by a reasonable suspicion to believe criminal activity may be afoot and the person seized is possibly involved with that criminal activity. Unlike consensual encounters, a person subject to a detention is not free to leave" (Special Order NO. 9101, effective March 1, 2013, p. 1).

[5] Oakland Police Department. (May 2013). Report writing manual.

[6] Oakland Police Department. (May 2013). Report writing manual, p. 1.

[7] See Chapter 1 of this report for definitions of the reasons for stops and for definitions of the types of searches.

stop data form "is not needed for a passenger(s) of a vehicle who is/are merely detained for officer safety reasons and the interaction is not intrusive."[8] For instance, if an officer stops a vehicle due to a broken taillight and encounters three occupants in the car and only speaks with the driver (and collects the driver's information), but otherwise does not interact with the passengers, a stop data form is not required for the passengers (but *is* required for the driver).[9] If during that same vehicle stop, the officer discovered that the driver is on probation or parole and asked all of the occupants of the car to exit the vehicle while the officer searches it (but otherwise there is "no interaction between the officer and the passengers"), a stop data form is still not required for the passengers.[10] Simply asking passengers for identification does not require that a stop data form be completed for those individuals. However, asking one or more passengers if he/she is on probation or parole, has a criminal history, or if he/she has anything illegal on his/her person all require the completion of a stop data form for that person.[11] Regardless of the type of stop (e.g., vehicle, pedestrian), guidelines state that when "multiple people are detained during a self-initiated stop, each person shall be listed as the primary 'Subject' on a separate [stop data form]."[12] In other words, the same incident of one vehicle with three occupants being stopped may require and result in the completion of up to three separate stop data forms, depending on which occupants were detained, searched, and/or arrested. In such cases, the data from each completed form will be treated as a separate stop (although in fact they were not independent events).

How do officers fill out each section of the stop data form?

General information:

Officers are instructed to provide general information about the stop including where and when it occurred (see Figure 1.1 and Appendix A for a sample stop data form). For stop category, officers are to select "self-initiated." To track the stop, and any additional outcomes that may have resulted from the stop, up to three tracking numbers are attached to the stop. An incident number (the letters LOP followed by 12 digits) is automatically generated by the dispatch system when officers advise dispatchers that a stop was made. Officers also request an RD-number (an 8-digit report number) if the stop results in the completion of a crime report or an arrest. Finally, a citation number (7 digits) is generated in the event a citation was issued during the stop. Officers enter the date and time of the stop. Time is recorded in military time (e.g., 0015 for 12:15 AM; 1458 for 2:58 PM). The location of the stop is recorded manually by the officers. Officers are instructed to fill in the exact street address or block number and street name, as well as the police beat in which the stop

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[8] Oakland Police Department. (May 2013). Report writing manual, p. 3.

[9] For details, see Example 1 on p. 3, Oakland Police Department. (May 2013). Report writing manual.

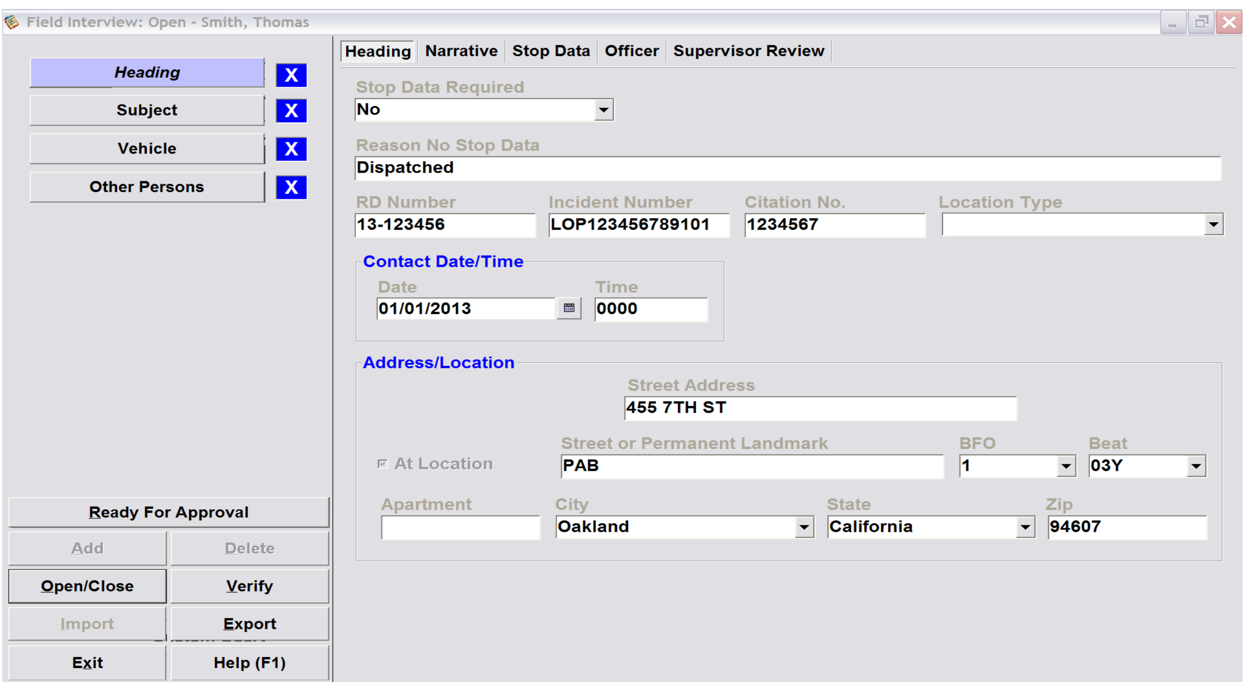
[10] See Example 4 on p. 3, Oakland Police Department. (May 2013). Report writing manual.

[11] See p. 3, Oakland Police Department. (May 2013). Report writing manual.

[12] See p. 10, Oakland Police Department. (May 2013). Report writing manual.

occurred. Officers are also required to indicate whether or not they were on a special assignment at the time of the stop (yes/no; see Figure 1.2). Working a special assignment essentially means that an officer's normal assignment has been modified so that he/she can provide assistance with a specific department need and/or enforcement strategy. While working a special assignment, officers may continue to make self-initiated stops, which may or may not be related to the specific special assignment. If officers respond yes to being on a special assignment at the time of the stop, they are to additionally select one of the following special assignment types: narcotics, prostitution, cruising, violence suppression, special event, and other. Lastly, the officer indicates his/her own role as the primary officer, and thus the officer who is responsible for and required to complete the stop data form, and provides his/her name and serial number (an internal employee tracking number). Should a second cover officer be present during the stop, his/her name and serial number are also recorded in the stop data form.[13] Further, there is a section to include the name and serial number of a supervisor. This supervisor is the person who reviews and approves of the completed form and is *not* the primary officer's commanding officer.

Figure 1.1. General information section of stop data form



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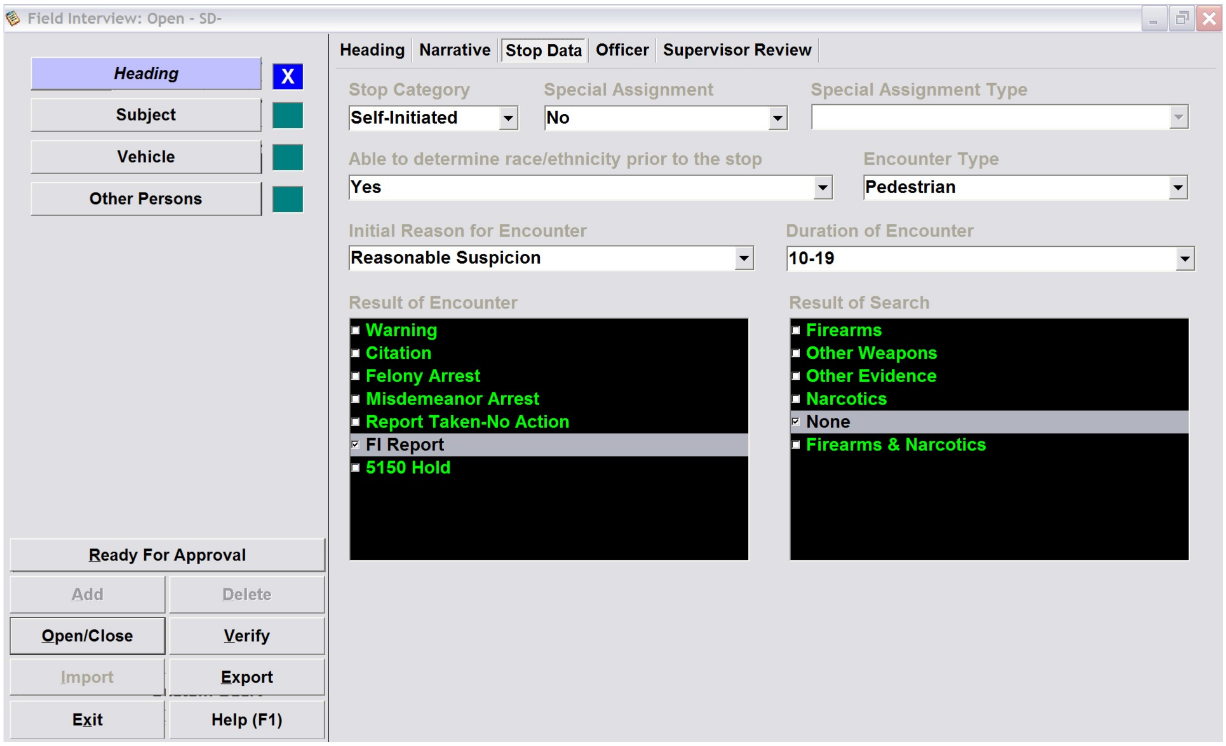
[13] Cover officers typically are not required to complete a separate stop data form. However, if there are additional detentions that occur after the initial stop has been made, the cover officer may end up assisting with the completion of stop data forms. For example, the cover officer may end up observing narcotics on the rear passenger of vehicle. In that case, the cover officer may end up completing a separate stop data form if the officers decide to split up the paperwork.

Figure 1.2. Section of stop data form containing information about encounter type and initial reason for encounter



Race/ethnicity determination question:

The stop data form asks officers to answer the following question: "Could you determine the race/ethnicity of the individual(s) prior to the stop?" (see Figure 1.2). The officer selects either yes or no from the drop-down menu. There is no additional training on what this question means or specifically how to interpret it, nor does it ask what race/ethnicity the officer believed the individual to be in the case where he/she acknowledges having been incorrect. The Report Writing Manual simply instructs officers to "Select your answer to the question."[14]

Encounter type:

Officers indicate whether the subject was encountered, or stopped, in a vehicle, as a pedestrian (e.g., on foot), on a bicycle, or other (see Figure 1.2). Some examples of "other" types of stops include compliance checks at the residence of registered sex offenders, going to the residences of wanted persons or persons on probation/parole, and investigating suspicious parked vehicles.

[14] See p. 8, Oakland Police Department. (May 2013). Report writing manual.

Initial reason for encounter:

Officers articulate the basis for the stop (see Figure 1.2). The form contains a drop-down menu with the following selections: consensual encounter, reasonable suspicion, probable cause, traffic violation, and probation/parole. Each term is defined exactly as follows in the Oakland Police Department Report Writing Manual:[15]

> **Consensual Encounter** – A police encounter in which officers do not exert any authority or use any force, and the subject voluntarily agrees to stop and answer questions or otherwise assist officers in their investigation. Because these encounters are, by definition, consensual, a subject may refuse to talk with officers, refuse to identify himself/herself, or otherwise refuse to cooperate. Officers shall select "Consensual Encounter" in the event that the encounter begins as consensual, but is elevated to a detention because the person is determined to be on probation/parole.
>
> **Reasonable Suspicion** – A seizure supported by a reasonable suspicion to believe criminal activity may be afoot and the person seized is possibly involved with that criminal activity. Unlike consensual encounters, a person subject to detention is not free to leave.
>
> **Probable Cause** – Probable cause to arrest exists when the totality of the circumstances or "total atmosphere" of the case would cause a person of ordinary care and prudence to entertain an honest and strong suspicion that the person to be arrested is guilty of a crime.
>
> **Traffic Violation** – Any traffic related violation of the Vehicle or Oakland Municipal Codes involving a pedestrian, bicyclist, or motor vehicle.
>
> **Probation/Parole** – Any initial seizure due to the status of a probationer/parolee.

Narrative:

The stop data form contains a section in which officers can explain in their own words "the reason(s) for the contact, indicating your actions and those of the subject."[16] The report-writing manual instructs officers to "Be as complete and concise as possible." [16]In particular, officers are instructed that it is important to articulate what the original basis for the contact was and, in the event that the subject was detained, what reasonable suspicion existed to justify the detention, or seizure. Furthermore, "If a pat-search was conducted, articulate the reasonable suspicion that caused you to believe the subject was armed or dangerous."[16]

Result of encounter:

Officers indicate what action they took and how the stop ended (see Figure 1.2). They are to choose only one option from the following list: warning, citation, felony arrest, misdemeanor arrest, report taken-no action, and Field Interview (FI) Report. The primary purpose of an FI Report is to

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[15] Oakland Police Department. (May 2013). Report writing manual, pp. 8-9.

[16] Oakland Police Department. (May 2013). Report writing manual, p. 5.

document the legal basis for a stop and search (if applicable). In some cases, when officers select "no action taken," the stop is part of larger investigation and officers are attempting to identify individuals for whom they might possibly take action at a later time. To help think about what these possible results of the encounter mean, consider that the Officer of Inspector General typically combines "Warning" and "FI Report" into a "non-consequential outcome" category.

Person encountered:
The stop data form contains a section concerning demographic information about the community member who was the subject of the stop.

Race. The form instructs officers to indicate the subject's race using the following race codes: W-White; A-Asian; B-Black; H-Hispanic; I-Native American; P-Pacific Islander; M-Middle Eastern; O-Other. Note that officers are instructed to choose only one race. In the Report Writing Manual Insert R-2 (effective Jan 15, 2010), officers are given additional information about how to answer this question:

> Indicate what you believed was the race of the person stopped at the time you made the decision to make the stop… If you are unable to see the driver or cannot ascertain the race at the time you made the decision to make the stop, indicate what you believe to be the race of the person after you made the stop… **Members shall not question person(s) regarding their race to make this decision.**[17] (emphasis in original)

Gender. Officers indicate whether the subject is male or female.

Age group. Officers indicate the subject's age using the following age group codes: A-Under 18; B-18-29; C-30-39; D-Over 40.

Oakland resident. Officers indicate whether or not the subject is a resident of Oakland (yes or no).

During the course of most stops, officers ask the person stopped for ID, usually a driver's license, which contains the gender, age, and address of the person. When the subject of the stop does not have his or her ID, officers inquire about the person's age and residency, but they are instructed not to ask about the person's race or gender.

Whether search was conducted and type of search:
Alongside demographic information about the subject, officers indicate whether or not the stopped community member was subjected to a search (yes or no; see Appendix A). A search can be made of a person and/or of property. Searches of persons can include patting down the person's body and examinations of the contents of clothing (pulling back garments to see what is concealed beneath,

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[17] Oakland Police Department. (May 2013). Report writing manual, p. 2.

looking in pockets, etc.) and/or any containers in the person's possession. In extreme cases, as in a strip search, a person may be required to remove or arrange some or all of his or her clothing to allow visual inspection of his or her body. In the context of self-initiated stops, searches of property typically involve the search of a vehicle. Officers are not asked on the stop data form whether a search was of the person or of property, but officers typically include this information in the narrative. In the event that a search was conducted, officers are required to indicate what type of search it was from the following selections:

- **Consent Search** – "An officer may search a person after obtaining the person's consent."[18] Police officers are instructed to "advise individuals of their right to refuse a consent search."[19] Recall that OPD policy requires a stop data form to be completed for every consent search conducted.[20]
- **Probable Cause (PC) Search** – Most probable cause searches are of vehicles. An officer may conduct a search of a vehicle without a warrant "if there is probable cause to believe the vehicle contains contraband or other evidence of a crime. Probable cause exists if officers are aware of facts establishing a 'fair probability' that evidence of a crime is located in the vehicle."[21,22]
    - Note that probable cause gives an officer the authority to specifically search areas of the vehicle where the suspected category of evidence may reasonably be concealed (e.g., one is not justified in searching for a stolen television set in a glove compartment; see Training Bulletin I-O.1, 1998 for more information).
    - Officers can also conduct a probable cause search of people. For example, if an officer sees a hypodermic needle in someone's shirt pocket, there is justification to search the person based on the likelihood that the person may have injectable drugs. Similarly, if a person is observed smoking marijuana,

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[18] Oakland Police Departmental Training Bulletin I-O.02, *The legal aspects of searching persons* (revised April 2, 2013), p. 2.

[19] See p. 3 of Oakland Police Departmental General Order (DGO) M-19, *Prohibitions regarding racial profiling and other bias-based policing*, 11/15/04.

[20] DGO M-19, 11/15/04, p. 5.

[21] See p. 3 of Training Bulletin I-O.1, 1998.

[22] For example, if an officer sees contraband in plain sight inside a car, he or she has probable cause to search the vehicle, including the trunk, for additional contraband or weapons. The odor of marijuana in a vehicle similarly gives officers probable cause to search the vehicle for contraband (see TB I-O1, 1998 for more examples).

there is reason to believe the person may be carrying more marijuana and the officer can search him/her.[23]

- **Probation/Parole Search** – "An officer may search a person pursuant to the subject's parole or probation search clause."[24] Officers are allowed to ask someone if he or she is on probation/parole, but they are encouraged to first build rapport with the community member. "Officers' primary motivation to conduct a parole or probation search shall serve as a legitimate law enforcement or rehabilitative interest" and shall not be "arbitrary; capricious; or harassing,"[25]
    - According to the Training Bulletin: There are three requirements to invoke a search clause of a parolee or probationer:
        - 1) *Knowledge of search condition* – which is basically confirmation that the person is on probation or parole, either because the community member confirms his/her status either voluntarily or in response to the officer asking and/or the officer independently verifies the status by looking up the person. In the narrative, officers are asked to indicate how probation/parole status was determined and how it was verified (either by looking it up using an accessible computer terminal or by checking it over the service channel/dispatch);
        - 2) *Rehabilitative or law enforcement motivation* – in other words, to make sure the person is "adhering to the appropriate legal guidelines set forth" and as "an accountability mechanism to prevent any future criminal behavior"; and
        - 3) *Is of reasonable scope and intensity* – meaning generally that officers should search only in those areas where they believe contraband could be hidden.
- **Incident to Arrest Search** – When a legal arrest is made and the arrest is "custodial," meaning the arrested person will be transported to jail (or in some cases to a hospital detox facility), officers are authorized to conduct a "full body" search of

[23] Alameda County District Attorney. (Winter 2015). *Point of view on probable cause to search.*
[24] Oakland Police Departmental Training Bulletin I-O.02, *The legal aspects of searching persons* (revised April 2, 2013), p. 2. See also, Oakland Police Departmental Training Bulletin I-O.4. (November 23, 2011). *Legal aspects of searching persons on parole and probation.*
[25] Samson v. California, 547 U.S. 843, 2006; see OPD Training Bulletin I-O.4, p. 1.

the person and a search of his/her vehicle. "The purpose of a search incident to arrest is to locate and secure any weapons that might be used against officers and to prevent the arrestee or others from concealing or destroying evidence."[26] The search must be "contemporaneous" with the arrest, which means "the search must take place at the same general time and the same general location as the arrest."[27] Note that "it is the custodial nature of the arrest and not the charge for which a person is arrested which justifies the search" (p. 4).[27]

- A "full body" search "means a pat-down and thorough examination of the arrestee's clothing and containers in his or her possession. When multiple garments (jacket, pants, etc.) are worn, the outer garment may be removed for examination and to allow for examination of garments below the outer layer" (p. 2).
- "Other searches which are incident to an arrest (i.e., containers, vehicles, surrounding areas) are also permissible" (p. 5). Officers may search the "entire passenger compartment, including the glove box and consoles" as well as all containers (e.g., boxes, bags) and clothing found within the car. However, the trunk may not be searched.[28]

- **Inventory Search** – When a vehicle is towed, officers may search the vehicle to

take inventory of the car and its contents in order "to secure any valuable property located within the vehicle and to guard against false claims that property in the vehicle was lost, stolen, or damaged."[29] More specifically, inventory searches of vehicles are justified when all of the following conditions are met: [29]

- It is reasonable to tow the car,[30]
- The decision to tow the car is made in good faith,[31]

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[26] Oakland Police Department. (Sept. 25, 1998). *Training bulletin I-O.1: Vehicle searches*, p. 7.

[27] Oakland Police Department. (April 2, 2013). *Departmental Training Bulletin I-O.02, The legal aspects of searching persons*, p. 4.

[28] For more information, see: Oakland Police Department. (Sept. 25, 1998). *Training Bulletin I-O.1: Vehicle searches*, p. 7.

[29] TB I-O.1 *Vehicle searches* (September 25, 1998), p. 1.

[30] According to Oakland Police Department. (Sept. 25, 1998). *Training Bulletin I-O.1: Vehicle searches*, vehicle inventory searches typically occur after a driver has been cited or arrested or when a vehicle has been in a traffic accident or is a hazard. The Training Bulletin further states "It is reasonable to tow a car when the situation fulfills criteria specified in the California Vehicle Code and/or when removal of the vehicle is reasonably necessary to protect the car or its contents" (p. 1).

[31] In this context, "in good faith" means, for example, that an officer does not decide to tow the car solely in order to secure justification to search it. The Training Bulletin on Vehicle Searches further states: "As a practical matter, 'good

- The decision to conduct an inventory search is made in good faith.[32]

- **Weapons Search** – When police detain a person during a stop, officers "may conduct a pat-down or limited weapons search … but only for weapons, of the person's outer clothing, and if you [the officer] have specific facts which make you feel in danger."[33] This type of weapons search falls into what the OPD would refer to as a "cursory" search to protect the officer's safety in a potentially dangerous situation and *not* to uncover evidence. Officers do not automatically have the authority "to conduct a general, full, exploratory search of the suspect."

Also note that in the narrative section of the stop data form, officers are specifically instructed to articulate the specific justification for any search they conducted.[34]

Result of search:
Officers must indicate what, if anything, was recovered during any search that was conducted (see Figure 1.2). They can select from the following options: none, firearms, other weapons, other evidence, narcotics, and firearms and narcotics. Firearms refers to various types of guns, including handguns, rifles, and semi-automatic weapons. Other weapons include knives and objects that "are not constructed for the purpose of inflicting bodily injury, but [are] reasonably capable of doing so," such as screwdrivers, bats, and razor blades.[35] Other evidence is a general category used when the other recovery categories do not apply and the discovery of such evidence confirms or supports reasonable suspicion or probable cause. Other evidence can include, for example, money, gang indicia, and drug paraphernalia (e.g., pipes, scales). Narcotics recoveries can include marijuana, heroin, cocaine, ecstasy, and methamphetamine.[36]

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faith' is usually found if the decision to tow is based on Department regulations or standard procedure which requires or permits such action" (OPD Training Bulletin I-01: *Vehicle searches,* p. 1).

[32] In this context, according to the Training Bulletin on Vehicle Searches, "'good faith' can be established by showing the decision to conduct the search is based on Department regulations or established routine that requires or permits such action for the purpose of protecting the vehicle and its contents" (p. 2). Further, inventory searches are to be limited to areas in which "valuable or dangerous items are commonly found," such as in the glove box, trunk, or underneath the seats (p. 2). As such, officers are routinely not authorized to rip up carpeting on vehicle floors or remove door panels during inventory searches since personal property is usually not kept in those locations. There are further guidelines for how and when to search containers found in the vehicle and whether or not to return any containers to the driver. See Oakland Police Departmental Training Bulletin I-O.1, *Vehicle searches* (September 25, 1998), p. 2.

[33] Oakland Police Departmental Training Bulletin I-O.02, *The legal aspects of searching persons* (revised April 2, 2013), p. 3.

[34] Oakland Police Department. (May 2013). Report writing manual, p. 5.

[35] Review of cursory searches presentation, May 2015, p. 8.

[36] OPD Report Writing Manual T-16. (November, 9, 1992). Narcotic and dangerous drugs.

Handcuffed:

Finally, alongside demographic information about the subject of the stop, officers indicate whether or not the person was handcuffed (yes or no) during the course of the stop.

Chapter 2 | METHODOLOGY

In this stop data report, our aim is to understand whether or not race influenced the rate at which people of different racial groups were stopped by the Oakland Police Department and whether or not race affected the course of a given stop. The first question could take the form of "Were African Americans more likely to be stopped because of their race?" The second question focuses on what we will call *post-stop outcomes*. More specifically, once the stop was made, was the subject of the stop more or less likely to be handcuffed, searched, and/or arrested as a function of his or her race? We focus on these three post-stop outcomes because they are of interest to many stakeholders. Members of the community often complain about being handcuffed at high rates, even in cases in which the stop does not ultimately end in an arrest. Similarly, many members of the public are concerned about the frequency of searches being conducted, especially when the majority of searches overall (more than 70%) lead to no recovery. The OPD in particular is interested in what it can do as an organization to improve search recovery rates. As for arrests, our goal was to examine what is arguably the most severe outcome of a stop.

A police stop is a complex interaction between two people: the officer and the community member. Each individual brings something to the interaction, but is also, to some extent, bound by the other person's behavior. Each person is continuously acting and reacting to the other person. A police stop, then, can be thought of as a complex and dynamic system in which each actor reacts in real time to cues emitted by the other actor in the interaction. It thus can be difficult to confidently assign causality when analyzing stop outcomes. To take a few examples, if we observed a systematic effect of officer gender, it could be a mistake to attribute it only to officer behavior. Imagine we found, for example, that female officers handcuffed community members less often. The more obvious interpretation is that for whatever reason female officers may be hesitant or reluctant to use handcuffs and therefore they use handcuffs less often. It could also be the case, however, that maybe female officers appear less threatening to community members, which causes the community members to not behave in ways that can lead to the kind of escalation that may require handcuffing. Thus, in this example, it is not actually the officer's gender but the reaction in the civilian that is elicited by the officer's gender that triggered the need for handcuffing. Similar problems of causality emerge in the study of parenting, such as in cases in which some children's traits can trigger certain behaviors in parents, which in turn are causal in shaping children's development. Similarly, when we look for examples of quality of life indices predicting stop outcomes it is important to take into account that these environmental factors may affect both actors going into the interaction, and that both then react to each other's initial attitude, creating complex dynamics that can spiral out of control without being directly attributable to either actor.

On the surface, the question of how race shapes policing decisions may seem fairly straightforward. It seems plausible enough that one could examine raw counts of the number of stops that were made of each racial group and compare them to each other. Similarly, one could examine the number of times each post-stop outcome occurred as a function of the race of the person stopped. Stop data reports produced by the Oakland Police Department typically examine this type of raw data.[37] Such reports are produced as part of the OPD Stop Data program, which is designed to promote transparency and allow "the Department to assess effectiveness and identify potentially biased behaviors."[38] This method, clearly, is considered to have some merit.

The question of whether or not race plays a *causal role* in policing decisions, however, is quickly complicated by the fact that we cannot know for certain what the officer was thinking when he or she made the decision to pull someone over or conduct a search. Even if the stop data form had a question that read, "Did you decide to make the stop *because of* the community member's race?" this problem would still not be solved. There is a strong prescriptive norm, or societal consensus or demand, that people *should be* egalitarian and *should not* express racial prejudice or discriminate by using race as the basis of their decisions.[39] The fear of being seen as a racist, and even of the disciplinary or legal consequences of profiling, would likely compel most officers, to simply answer "no" to this question regardless of whether race did play a factor in their decisions.[40] Furthermore, psychologists have found that, in many cases, people are surprisingly inaccurate at knowing and

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[37] Stop data reports produced and published by the Oakland Police Department are available at this link: http://www2.oaklandnet.com/government/o/OPD/a/PublicReports/index.htm#stop

[38] OPD Stop Data Analysis Report (September 2014), p. 1.

[39] Crandall, C. S., Eshleman, A., & O'Brien, L. (2002). Social norms and the expression and suppression of prejudice: The struggle for internalization. *Journal of Personality and Social Psychology*, *82*(3), 359-378; Biernat, M., Vescio, T. K., Theno, S. A., & Crandall, C. S. (1996). Values and prejudice: Toward understanding the impact of American values on outgroup attitudes. In Clive Seligman, James M. Olson, and Mark P. Zanna (Eds.), *The Psychology of Values: The Ontario Symposium on Personality and Social Psychology*, Vol. 8, Hillsdale, NJ: Lawrence Erlbaum Associates, 153-189; Schwartz, S. H. (1999). A theory of cultural values and some implications for work. *Applied Psychology*, *48*(1), 23-47; De Tocqueville, A. (1835/2003). *Democracy in America*. Washington, DC: Regnery Publishing; Pew Research Center, August, 2015, "Across Racial Lines, More Say Nation Needs to Make Changes to Achieve Racial Equality." Washington, DC: Pew Research Center.

[40] Indeed, social psychological research has shown that there are a variety of reasons individuals might want to respond without prejudice. For some people, the motivation to respond without prejudice is internal: they avoid acting in prejudiced ways and relying on stereotypes because doing so would violate their own personal values and concept of themselves as egalitarian. For others, the motivation to respond without prejudice is external: they try to appear nonprejudiced in order to avoid the negative reactions and disapproval of others. See Plant, E. A., & Devine, P. G. (1998). Internal and external motivation to respond without prejudice. *Journal of Personality and Social Psychology, 75*(3), 811-832. Further, another powerful motivating force is to fit in with others and to conform to social norms about when it is and is not acceptable to express prejudice and discriminate and about which groups it is and is not acceptable to express prejudice against. See Crandall, C. S., Eshleman, A., & O'Brien, L. (2002). Social norms and the expression and suppression of prejudice: The struggle for internalization. *Journal of Personality and Social Psychology, 82*(3), 359-378. To avoid appearing racist, people will go out of their way to avoid mentioning or acknowledging race, even when this avoidance is counterproductive. See Apfelbaum, E. P., Sommers, S. R., & Norton, M. I. (2008). Seeing race and seeming racist? Evaluating strategic colorblindness in social interaction. *Journal of Personality and Social Psychology, 95*(4), 918-932.

articulating the reasons for their behavior, judgments, and decisions.[41] Especially in the case of race, bias is often not explicit. Rather, much of racial bias tends to be *implicit*, or a bias that people are not even aware that they have.[42] Implicit bias can come from repeated exposure throughout one's lifetime to subtle cultural cues that are transmitted to us, for example, when we watch television, interact with our parents, or more generally observe the stereotypical ways in which different groups are commonly depicted, treated, and talked or thought about.[43]

Thus, to the extent that a given officer's true motivations and attitudes are *unknowable*, we argue that posing the question of whether or not particular officers are "biased" is not the most fruitful way to begin an investigation of how race may influence police stops. Indeed, this question of individual-level bias can be counterproductive and something of a nonstarter. Even in the absence of biased or racist individuals, institutions themselves can be biased by having policies and structures

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[41] de Camp Wilson, T., & Nisbett, R. E. (1978). The accuracy of verbal reports about the effects of stimuli on evaluations and behavior. *Social Psychology*, 118-131; Gavanski, I., & Hoffman, C. (1987). Awareness of influences on one's own judgments: The roles of covariation detection and attention to the judgment process. *Journal of Personality and Social Psychology,52*(3), 453-463; Kraut, R. E., & Lewis, S. H. (1982). Person perception and self-awareness: Knowledge of influences on one's own judgments. *Journal of Personality and Social Psychology,42*(3), 448-460; Nisbett, R. E., & Bellows, N. (1977). Verbal reports about causal influences on social judgments: Private access versus public theories. *Journal of Personality and Social Psychology,35*(9), 613-624; Nisbett, R. E., & Wilson, T. D. (1977). Telling more than we can know: Verbal reports on mental processes. *Psychological Review,84*(3), 231-259.

[42] Correll, J., Park, B., Judd, C. M., & Wittenbrink, B. (2002). The police officer's dilemma: Using ethnicity to disambiguate potentially threatening individuals. *Journal of Personality and Social Psychology, 83*, 1314-1329; Devine, P. G. (1989). Stereotypes and prejudice: Their automatic and controlled components. *Journal of Personality and Social Psychology, 56*(1), 5-18; Dovidio, J. F., Kawakami, K., Johnson, C., Johnson, B., & Howard, A. (1997). On the nature of prejudice: Automatic and controlled processes. *Journal of Experimental Social Psychology, 33*(5), 510-540; Eberhardt, J. L., Goff, P. A., Purdie, V., & Davies, P. G. (2004). Seeing Black: Race, crime, and visual processing. *Journal of Personality and Social Psychology, 87*, 876-893; Greenwald, A. G., & Banaji, M. R. (1995). Implicit social cognition: Attitudes, self-esteem, and stereotypes. *Psychological Review, 102*(1), 4-27; Greenwald, A. G., McGhee, D. E., & Schwartz, J. L. (1998). Measuring individual differences in implicit cognition: The implicit association test. *Journal of Personality and Social Psychology, 74*(6), 1464-1480; Payne, B. K. (2001). Prejudice and perception: The role of automatic and controlled processes in misperceiving a weapon. *Journal of Personality and Social Psychology, 81*(2), 181-192. For a discussion of implicit bias in the criminal justice system, see Banks, R. R., Eberhardt, J. L., & Ross, L. (2006). Discrimination and implicit bias in a racially unequal society. *California Law Review, 94*(4), 1169-1190.

[43] Hetey, R. C., & Eberhardt, J. L. (2014). Racial disparities in incarceration increase acceptance of punitive policies. *Psychological Science, 25*(10), 1949-1954; Lowery, B. S., Hardin, C. D., & Sinclair, S. (2001). Social influence effects on automatic racial prejudice. *Journal of Personality and Social Psychology, 81*(5), 842-855; Sinclair, S., Dunn, E., & Lowery, B. (2005). The relationship between parental racial attitudes and children's implicit prejudice. *Journal of Experimental Social Psychology, 41*(3), 283-289; Weisbuch, M., Pauker, K., & Ambady, N. (2009). The subtle transmission of race bias via televised nonverbal behavior. *Science, 326*(5960), 1711-1714; Weisbuch, M., & Pauker, K. (2011). The nonverbal transmission of intergroup bias: A model of bias contagion with implications for social policy. *Social Issues and Policy Review, 5*(1), 257-291.

in place that harm some people and favor others, even in unintended and unanticipated ways.[44] Institutional disparities can perpetuate themselves: Simply being exposed to evidence of inequality can cause people to become more supportive of the very policies that produce that inequality.[45] It can be tempting to look for a window into the hearts and minds of individuals (e.g., police officers), but when researchers and practitioners focus instead on institutions (e.g., the criminal justice system, a specific law enforcement agency), they are in a better position to measure and evaluate the consequences of policies and practices and identify whether some groups disproportionately bear the burden of any negative outcomes. In the academic, legal, and policy arenas (and beyond), this disproportionate burden is referred to as *disparate outcomes* or *disparate impacts*. The hunt for bias can also unfortunately lead to name calling and defensiveness, and can thus become counterproductive. Here, we acknowledge that the issue of individual officer bias is inherently unanswerable. As researchers, we can never know what was inside a particular police officer's head. Instead, in this report, we focus on and examine what we can know: whether or not there are systematic differences in outcomes of stops for different groups, controlling for as many factors as possible that could legitimately justify such differences; and whether or not police officers' decisions to make stops and to handcuff, search, and arrest have disparate impacts on people of color in the community.

What is a benchmark?

Of all OPD stops that were made from April 1, 2013, through April 30, 2014, 60% were of African Americans. Stops of African Americans were made at a rate of more than three times that of Hispanics, the next most common racial group that was stopped. Sixty percent may sound like a large percentage; however, we cannot know if 60% is high or low or begin to understand the role of race until we have some figure to which to compare this number, or some larger context in which to consider it. What *should* the number be? At what rate would we expect African Americans in the City of Oakland to be stopped? To begin to answer this question, we need some point of reference, or benchmark. Thought of another way, we cannot know whether race played a significant role until we have first accounted for *other factors* that might plausibly explain why we would expect to

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[44] Bonilla-Silva, E. (2006). *Racism without racists: Color-blind racism and the persistence of racial inequality in the United States.* Lanham, MD: Rowman & Littlefield.

[45] Hetey, R. C., & Eberhardt, J. L. (2014). Racial disparities in incarceration increase acceptance of punitive policies. *Psychological Science, 25*(10), 1949-1954; Peffley, M., & Hurwitz, J. (2007). Persuasion and resistance: Race and the death penalty in America. *American Journal of Political Science, 51*(4), 996-1012. For a larger discussion of the underlying motivation to perpetuate the status quo, see also Kay, A. C., Gaucher, D., Peach, J. M., Laurin, K., Friesen, J., Zanna, M. P., & Spencer, S. J. (2009). Inequality, discrimination, and the power of the status quo: Direct evidence for a motivation to see the way things are as the way they should be. *Journal of Personality and Social Psychology, 97*(3), 421-434.

see racial differences.[46] As a salient example of such other factors, let us consider crime rate. When a claim of racial profiling is made against a police department, perhaps the most common rebuttal is that the police are simply going where the crime is, presumably acting under the theory that increased police presence reduces crime; there is evidence that police patrol does, in fact, deter crime.[47] To the extent that high-crime areas have higher concentrations of African Americans and to the extent that it is an effective policing strategy to stop more people in these high-crime areas, then that means that, indirectly, more African Americans will be stopped. In this scenario, race is not the driving factor; crime is. To the extent that race and crime actually predict each other,[48] one might observe apparent racial differences in stop rates that in actuality have nothing to do with race at all and are really accounted for by crime rate in the neighborhood.

There is no consensus in the academic literature about which benchmarks or other factors are the most appropriate to take into account.[49] In fact, selecting benchmarks to include in statistical

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[46] Ayres & Borowsky, 2008; Analysis Group, Inc. (2006), Pedestrian and motor vehicle post-stop data analysis report, prepared for City of Los Angeles.

[47] Di Tella, R., & Schargrodsky, E. (2004). Do police reduce crime? Estimates using the allocation of police forces after a terrorist attack. *American Economic Review, 94*(1), 115-133; Koper, C. S. (1995). Just enough police presence: Reducing crime and disorderly behavior by optimizing patrol time in crime hot spots. *Justice Quarterly, 12*(4), 649-672; Sherman, L. W. (1990). Police crackdowns: Initial and residual deterrence. *Crime and Justice*, 1-48; Sherman, L. W., & Weisburd, D. (1995). General deterrent effects of police patrol in crime “hot spots”: A randomized, controlled trial. *Justice Quarterly, 12*(4), 625-648.

[48] See Snyder, H. N., & Mulako-Wangota, J. Bureau of Justice Statistics. Violent crime index trend tables by race. Generated using the Arrest Data Analysis Tool at www.bjs.gov. (2012).

[49] There is no established procedure for how to select benchmarks. Some researchers begin with population data and compare the rates of stops made of African Americans to the African American population share (usually measured by census data; e.g., Bailey, et al., Plaintiffs v. City of Philadelphia, et al., Defendants, *Plaintiffs’ fifth report to court and monitor on stop and frisk practices* (2015), C.A. No. 10-5952, In the United States District Court for the Eastern District of Pennsylvania; New York Civil Liberties Union (2012), *Stop-and-frisk 2012: NYCLU briefing*). Other researchers, particularly those analyzing vehicle stops, focus instead on measures of driving behavior, such as traffic data about involvement in motor vehicle collisions (e.g., Lovrich, et al., 2007). Still other research teams, such as Analysis Group (2006) in its treatment of the LAPD’s stop data, include more than a dozen control variables that include characteristics of the officer who made the stop, in addition to multiple measures of crime rate, and other variables about the location of the stop, including the neighborhood’s economic well-being and stability (Analysis Group, Inc. 2006, Pedestrian and motor vehicle post-stop data analysis report, prepared for City of Los Angeles). There is also no established procedure for what to do with benchmarks in an analysis once they have been selected. Some researchers include benchmark variables as control variables in large statistical models. The Analysis Group (2006) in its treatment of the LAPD data on vehicle and pedestrian stops did this. Other researchers adopt a criterion for specifying what they will consider a significant race difference. For example, in an analysis of data resulting from vehicle stops made by the Washington State Patrol, Lovrich, Gaffney, Mosher, Pratt, and Pickerill (2007) decided that a racial difference would not be considered “substantively significant as long as the percentage of those contacted in any particular racial group is not more than **five percentage points** greater than the percentage of the group in the benchmark comparison” (p. 5; original emphasis; Lovrich, et al., 2007).

analyses can be in and of itself a fairly contentious issue.[50] In the next section, we will review which benchmarks have been most commonly used in analyses of stop data and policing decisions. As you will see, no benchmark is perfect and each comes with its own set of strengths and weaknesses.

What benchmarks have been used in past research?

Population demographics:
Suppose we were analyzing the stop data for the police department of a major American city and found that 75% of all stops were of African Americans. With this information alone, it is impossible to tell whether or not this police department might be engaging in policing practices that disproportionately affect the African American community. As one benchmark, we might want to know something about the local population. *Whom the police stop is necessarily limited to the universe of people the police could stop*. Should we learn that our hypothetical police department were in Ann Arbor, Michigan, which according to the 2010 Census is only 8% African American, then we might conclude that the police department was stopping African Americans at a very high rate given the demographics of the residents. If, however, the police department were a few miles away in Detroit, Michigan, which is 83% African American, then the rate at which the police department was stopping African Americans would more closely mirror the demographics of the residents. If anything, we might conclude that the police department was stopping fewer African Americans than we would expect given the population demographics.

As in this example, one place many researchers start is with an examination of local population demographics. Usually researchers acquire relevant census data about what share of the total population is made up of those who fall into different demographic categories. By breaking down stops and census information at the level of census tract (the geographical subdivision used by the Census, typically between 1,000 and 8,000 inhabitants), researchers can control for demographic factors as they vary within a city. The racial demographics of an area is usually the central focus, but other types of information are collected as well to the extent that these other demographic variables are also suspected of changing the likelihood at which people are at risk of being stopped. For instance, the police stop younger people more than they stop older people. Unemployed people might similarly be stopped more often to the extent that they might be out driving during the day when many people are at work. Researchers usually take into account or compare how many people

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[50] As we have said and will discuss in more detail, many researchers rely on census data about the racial demographics of the neighborhoods where stops were made to establish a sense of who may be at risk of being stopped by police in the first place. Grogger and Ridgeway (2006) are critical of the use of such data when trying to estimate risk for being pulled over in a vehicle. To get around the need for any external benchmarks, they instead created an approach to test for possible race differences that they call the "veil of darkness." Another controversy surrounds what the appropriate use of benchmarks is.

are stopped by race to the share of that racial group in the population.[51] Indeed, an analysis done by the New York Civil Liberties Union (NYCLU) of the New York Police Department's (NYPD) stop-and-frisk rates in 2012[52] appealed to population demographics. In the data highlights section of the NYCLU report, the authors noted that:

> Young black and Latino men were the targets of a hugely disproportionate number of stops. Though they account for only 4.7% of the city's population, black and Latino males between the ages of 14 and 24 accounted for 40.6% of stops in 2012. The number of stops of young black men neared the entire city population of young black men (133,119 as compared to 158,406).[53]

The NYCLU report further used demographic data to take on the argument that so many stops are of African American and Latino community members because they happen to live in high-crime precincts that are predominately African American and Latino. The report noted that even in most of the 10 precincts with the lowest percentages of African American and Latino residents in the entire city (comprising between 8% and 14% of the population) more than 70% of stops were of African Americans and Latinos.[54]

Though comparing the rates at which certain racial groups are stopped to the rates at which those groups are present in the general population makes intuitive sense, this approach has limitations. First, in some cases, census data may systematically undercount undocumented residents and migrant workers, an issue that has been noted as a significant problem when trying to obtain accurate information about the percentage of Hispanics who reside in a given area.[55] Second, most of the data on racial demographics include all residents of a particular area, regardless of their age or other characteristics. A particular census tract might be 50% African American, for example, but a significant portion of those African American residents might be small children or the elderly, who are statistically less likely to be stopped by police compared to 18- to 30-year-olds. To the extent that racial minority groups tend to have higher birth rates, resulting in a population that skews younger than majority groups,[56] using Census data that include all residents regardless of age would make for

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[51] See, for instance, Bailey, et al., Plaintiffs v. City of Philadelphia, et al., Defendants, *Plaintiffs' fifth report to court and monitor on stop and frisk practices* (2015), C.A. No. 10-5952, In the United States District Court for the Eastern District of Pennsylvania; Analysis Group, Inc. (2006), Pedestrian and motor vehicle post-stop data analysis report, prepared for City of Los Angeles.

[52] New York Civil Liberties Union (2012), Stop-and-frisk 2012: NYCLU briefing.

[53] New York Civil Liberties Union (2012), p. 2.

[54] New York Civil Liberties Union, p. 6.

[55] Nicholas P. Lovrich et al. (2007), Results of the monitoring of WSP traffic stops for biased policing: Analysis of WSP stop, citation, search and use of force data and results of the use of observational studies for denominator assessment, Report to the Washington State Patrol relating to: National Highway Traffic Safety Administration (NHTSA) Grant-Funded Study on Racial Profiling Phenomena in Washington State OGRD # 107828.

[56] Cohn, D. (2014). Are minority births the majority yet? Retrieved May 21, 2016, from http://www.pewresearch.org/fact-tank/2014/06/04/are-minority-births-the-majority-yet/

a conservative test of the role of race in police decision-making. Relatedly, population demographics often do not take into account how many residents have driver's licenses or otherwise drive regularly, which is of particular importance in areas in which the majority of police stops are vehicle stops. Driving behavior, then, may be another important variable, which we will return to shortly. Another limitation of population demographics is that people routinely venture away from where they live, e.g., to go to work, school, or church or to go shopping. People are not always stopped where they live.[57] Heavily commercial areas, for example, might not even have a sizeable population of residents, but they do attract people who inhabit those spaces and therefore can be stopped there. People are not supposed to be stopped by police merely because they are physically present somewhere, but rather because they are suspected of breaking some law. Even if most people in a given city are of one race, if that group commits traffic violations or commits crimes at low rates, then that group should not be stopped often. For this reason, the utility of population demographics may be overestimated. Overall, the major limitation of relying on population information is that the demographics of residents may not reflect the demographics of those who actually could be stopped.

Crime rate:

Another common approach is to rely on data about crime. When using crime rate as a benchmark, researchers often include the rates of violent crime, property crime, or both.[58] To the extent that self-initiated police stops are part of a larger enforcement strategy to prevent crime, the distribution of stops throughout a city should mirror the distribution of crime. In fact, members of the law enforcement community often say that they are simply "going where the crime is." Therefore, to the extent that self-initiated stops and other policing strategies are concentrated in high-crime areas that happen to be predominately African American,[59] then it makes sense that African Americans will, indirectly, be stopped more frequently overall because they are more likely to be physically present in areas where a larger proportion of stops are made. One limitation to using crime rate data as a benchmark, of course, is that simply being in a high-crime area does not, on its own, provide justification to make a stop. Indeed, living, working, or otherwise traveling in a high-crime area

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Cohn, D. (2014). Falloff in births slows shift to a majority-minority youth population. Retrieved May 21, 2016, from http://www.pewresearch.org/fact-tank/2014/06/26/falloff-in-births-slows-shift-to-a-majority-minority-youth-population/

[57] For more on this point, see Ridgeway, G. (2009). Cincinnati Police Department traffic stops: Applying RAND's framework to analyze racial disparities. Santa Monica, CA: RAND Corporation.

[58] See Nutter, M. A., & Ramsey, C. H. (2011). Making Philadelphia a Safer City: 2011 Progress Report on the Crime Fighting Strategy and Five-Year Plan; Geoffrey P. Alpert et al., 2006, Pedestrian and Motor Vehicle Stop Data Analysis Report, Analysis Group, Inc. (2006); Ayres, I., & Borowsky, J. (2008), A Study of Racially Disparate Outcomes in the Los Angeles Police Department, Prepared for the ACLU of Southern California.

[59] Past research has shown that high crime, on the one hand, and segregation and poverty in African American communities, on the other hand, tend to go together. Racial segregation concentrates poverty, which, in turn, concentrates crime and violence. Massey, D. S. (1995). Getting away with murder: Segregation and violent crime in urban America. *University of Pennsylvania Law Review, 143*(5), 1203-1232.

does not, on its own, predict one's likelihood of being directly involved in criminal activity.[60] In the case of *Brown v. Texas*, the U.S. Supreme Court ruled that it is unconstitutional to stop someone based solely on the fact that he or she is in a high-crime area.[61] Note also that crime rates may not be as accurate as one might think. Quillian and Pager, both sociologists, explain that crime rate data, which usually comprise crimes reported to police by the public, may systematically undercount the true numbers of crimes because a fear or mistrust of the police may keep members of the community from filing reports.[62] Other reasons people may not report crime to police: They feel that less serious crimes are too trivial to report, the police would not be interested or would not be able to do anything in response, and they can privately deal with the incident themselves.[63] Quillian and Pager argue that people's perceptions of crime are colored by the racial demographics of the neighborhood. The researchers found that even after controlling for a host of factors including two measures of crime (the crime rate collected by police and a separate measure based on people's self-reported victimization of crime), the percentage of young Black men[64] living in the neighborhood predicted residents' views of how much crime there was in the neighborhood. It is an open empirical question how susceptible police officers might be to the influence of stereotypes equating African American men and crime in determining what they consider to be a high-crime neighborhood and, more consequently, how they make policing decisions when in those areas.

What about the argument that a difference in crime rates by race could justify racial disparities in stops? Consider a federal court case regarding the New York City Police Department's (NYPD) stop

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[60] Kochel, T. R. (2010). Constructing hot spots policing: Unexamined consequences for disadvantaged populations and for police legitimacy. *Criminal Justice Policy Review*.

[61] Brown v. Texas (443 U.S. 47 1979). Note that in Illinois v. Wardlow (528 U.S. 119, 2000), the U.S. Supreme Court considered the character of the neighborhood to be a legitimate factor in finding reasonable suspicion to stop someone, although the Court ruled that it cannot be the sole justification for a stop. The Court allowed that an officer needs only two factors to establish reasonable suspicion under the Fourth Amendment: being in a high-crime area and unprovoked flight from police. Ferguson and Bernache (2008) describe how the same behavior, apparently running from police, means two different things depending on whether the neighborhood is considered to be a "low-crime" or "high-crime" area. In a low-crime area, the police do not automatically have justification for making a stop, whereas in a high-crime area, the police do. The authors argue that this difference has implications for residents of high-crime areas: "'High-crime areas' are a fact of constitutional law: individuals in those areas have different Fourth Amendment protections than they would in other locations in the same town, city, or state." (p. 1589). See Ferguson, A. G., & Bernache, D. (2008). The "high-crime area" question: Requiring verifiable and quantifiable evidence for fourth amendment reasonable suspicion analysis. *American University Law Review, 57*, 1587-1644.

[62] Quillian, L., & Pager, D. (2001). Black neighbors, higher crime? The role of racial stereotypes in evaluations of neighborhood crime. *American Journal of Sociology, 107*(3), 717-767.

[63] Tarling, R., & Morris, K. (2010). Reporting crime to the police. *British Journal of Criminology*, *50*(3), 474-490.

[64] Defined as being between the ages of 12 and 29 years old. Note the researchers also controlled for the total percentage of young men (of all races) who fell into this age range, so their effects cannot be dismissed as being driven by the presence of young men more generally.

and frisk program[65] that received a great deal of attention. In 2013, a federal judge ruled that stop, question, and frisk tactics were unconstitutional because the NYPD violated the rights of racial minorities by subjecting them to high numbers of stops and searches.[66] During the trial, New York Police Commissioner Raymond Kelly did a television interview with the ABC program "Nightline," during which he defended the legitimacy of these policies by appealing to the crime rate by race:

> About 70% to 75% of the people described as committing violent crimes—assault, robbery, shootings, grand larceny—are described as being African-American… The percentage of people who are stopped is 53% African-American, so really, African-Americans are being under-stopped in relation to the percentage of people being described as being the perpetrators of violent crime.[67]

Commissioner Kelly argued that because African Americans commit more violent crimes, then it follows that African Americans should be stopped more often, presumably on the suspicion that they are more likely to be violent criminals. Legal scholar David Cole explains that, indeed, many criminologists have concluded that Blacks, men, and younger people do in fact commit crime at a higher per capita rate than Whites, women, and older people. "Thus, all other things being equal, it is rational to be more suspicious of a young black man than an elderly white woman. But that it may be rational does not make it right."[68] Cole describes how the correlation between race and crime remains a stereotype to which most African Americans do not conform. Even if African Americans are actually more criminal in their behavior, he reasons, only about 2% of African Americans are arrested each year for committing any crime. Thus, the vast majority of African Americans are not charged with crimes. A police department that relies exclusively on race in making the decision to stop people, then, is likely to stop many more innocent people than guilty people. In addition to being less than effective, using "an individual's race as a direct proxy for that individual's criminality is legally problematic under current prohibitions against racial profiling."[69] In the State of California, California Penal Code Section 13519.4(e) prohibits racial profiling by law enforcement. According to the OPD's Departmental General Order M-19, racial profiling can be defined as:

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[65] Floyd v. City of New York (2013), Opinion and order, 08 Civ. 1034 (SAS); Upheld Floyd v. City of New York, (2014), United States Court of Appeals, Second Circuit.

[66] Floyd v. City of New York (2013), Opinion and order, 08 Civ. 1034 (SAS); Upheld Floyd v. City of New York, (2014), States Court of Appeals, Second Circuit.

[67] But note that, according to the 2010 Census, the population of New York City is 25.5% Black or African American alone, meaning people who identify as one race (Black or African American) and no other race (this figure does not take into account Hispanic origin). Therefore, African Americans were overrepresented among those stopped relative to their general share of the population.

[68] Cole, D. (1999). *No equal justice: Race and class in the American criminal justice system.* New York: The New Press, p. 42.

[69] Ayres, I., & Borowsky, J. (2008), A study of racially disparate outcomes in the Los Angeles Police Department, Prepared for the ACLU of Southern California, p. 4.

> The use of race, ethnicity, or national origin in determining reasonable suspicion, probable cause or the focus or scope of any police action that directly or indirectly imposes on the freedoms or free movement of any person, unless the use of race, ethnicity, or national origin is used as part of a specific suspect description.[70]

***Driving behavior**:*
In many jurisdictions, the majority of stops are vehicle stops that are made because of traffic violations. Consider, for example, various state highway patrol agencies that are responsible for enforcing traffic laws on a large stretch of interstate highways and freeways. Since the primary purpose of state highway patrol agencies is to provide traffic enforcement and keep roads safe,[71] then crime rates are less relevant. Highway and freeway drivers may also be more likely to live far away from where they are stopped.[72] In fact, they may not even live in the same state. Population demographics, then, may also be of less use. Accordingly, researchers who conducted an analysis of traffic stops made by the Washington State Patrol argued that driving behavior is the more appropriate benchmark.[73] Their measures of driving behavior included contacts initiated as a result of calls for service and vehicle assists, as well as contacts initiated as a result of radar patrols (e.g., drivers who were identified as speeding via radar and thus the Trooper was "blind" to the identity of the driver), and collision data. In particular, the researchers argue that collision data coded by race is the most effective benchmark of driver quantity and quality that provides "a reliable and cost-effective indicator of driver population demographics."[74]

The problem is that many agencies do not collect this information or have the resources to store and make such information searchable or user-friendly. In addition, because many traffic stops are made due to alleged equipment failure (e.g., broken taillights), it is unclear to what extent actual driving behavior (e.g., speeding, reckless driving) would provide an accurate base rate for the likelihood of being pulled over. The rates at which members of different racial groups are stopped and cited due to equipment failure might have more to do with the year, make, and model of the cars they tend to drive than with how they drive. Additionally, people who tend to drive older vehicles, and/or

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[70] Oakland Police, Departmental General Order M-19 (November 15, 2004), p. 1
[71] See, for example, Missouri Revised Statutes, (August 28, 2015), Primary purpose of highway patrol, Chapter 43, Highway Patrol, State, Section 43.025.1, which states "the primary purpose of highway patrol is to enforce the traffic laws and promote safety upon the highways" and California Highway Patrol, (2016), which states that the CHP was created to "provide uniform traffic law enforcement throughout the state" and that "assuring the safe, convenient and efficient transportation of people and goods on our highway system is still our primary purpose." Retrieved from https://www.chp.ca.gov/home/about-us
[72] Nicholas P. Lovrich et al. (2007). Results of the monitoring of WSP traffic stops for biased policing: Analysis of WSP stop, citation, search and use of force data and results of the use of observational studies for denominator assessment, Report to the Washington State Patrol relating to: National Highway Traffic Safety Administration (NHTSA) Grant-Funded Study on Racial Profiling Phenomena in Washington State OGRD # 107828.
[73] Lovrich, et al., (2007).
[74] Lovrich, et al., (2007), p. 2; see also p. 12 for details on collision data.

people who are unable to afford maintenance of said vehicle (e.g., unable to afford the timely replacement of a headlight or taillight) may be more likely to be stopped for equipment violations. Socioeconomic status, then, may be a useful benchmark. Another possibility is that getting pulled over is driven, in part, by how attention-grabbing a vehicle is. People have a commonsense notion that certain types of cars lead to more traffic tickets. For instance, a Google search of "red cars get more tickets" yields more than 29 million results. Perhaps different racial groups, and certainly different socioeconomic groups, are more or less likely to drive "flashy" or attention-grabbing cars. Another issue is that vehicle stops may sometimes be made in service of law enforcement purposes other than the enforcement of traffic laws. Pretext stops are legal and "objectively valid" traffic stops, that is, stops based on genuine traffic infractions, wherein a separate motivation for the stop is to "search for evidence of an unrelated offense."[75] In these so-called "pretext" stops, an officer's goal may be to gather intelligence or engage in some other enforcement strategy and the actual traffic violation is the legal justification to make the stop.[76] Although we cannot know whether a traffic violation stop is or is not a pretext stop, driving behavior may not always be a useful benchmark if such stops do occur. Therefore, in all of these cases, it is debatable whether driving behavior is an informative benchmark to use.

Internal benchmarks:

Rather than obtaining external data (e.g., census data) with which to compare an agency's stop data, some researchers have used internal benchmarks that they constructed from within the stop data itself. In an analysis of Cincinnati Police Department traffic stops done on behalf of the RAND Corporation, Ridgeway (2009) constructed an internal benchmark for each officer:

> This method selects an officer, identifies stops that other officers made at the same time and in the same neighborhoods, and compares the racial distributions of the stopped drivers. Since the officers are patrolling the same area at the same times, the racial distributions should be the same (assuming that the officers are on the same assignment).[77]

This basic approach has been adopted in many cities as part of Risk Management and "early-warning systems" designed to identify problem officers.[78] The OPD uses this approach, as does the

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[75] Gamrath, C. G. & Johnston, I. D. (1997). *The law of pretext stops since Whren v United States, Illinois Bar Journal, 85*(488). See also Epp, C. R., Maynard-Moody, S., & Haider-Markel (2014). *Pulled over: How police stops define race and citizenship.* Chicago: University of Chicago Press.

[76] In Whren v. United States, 517 U.S. 806 (1996), the Supreme Court ruled that if and when police officers have probable cause for a traffic stop, a pretextual motive does not invalidate the stop. They held that "the temporary detention of a motorist upon probable cause to believe that he has violated the traffic laws does not violate the Fourth Amendment's prohibition against unreasonable seizures, even if a reasonable officer would not have stopped the motorist absent some additional law enforcement objective (pp. 809-819)."

[77] Ridgeway, G. (2009). Cincinnati Police Department traffic stops: Applying RAND's framework to analyze racial disparities. Santa Monica, CA: RAND Corporation, p. 23.

City of Los Angeles.[79] When it comes to an analysis of stop data, however, different officers may be on different assignments, especially in smaller police forces with limited personnel. We might expect that officers working different assignments would have different patterns of stopping people by race. This method also relies on researchers having access to accurate data about what assignment each officer was working during any given stop throughout the course of the entire time period from which the stop data have been collected.

Time of day:

Another benchmarking method that has been used to test for the existence of racial bias in policing decisions is known as the veil-of-darkness method.[80] As described by Ridgeway (2009), this method compares the breakdown of stops by race made during the day to the breakdown of stops by race made at night. The basic logic is as follows: If a police department were targeting African American drivers to stop, evidence of this practice should be most apparent during daylight when the race of a driver is presumably most visible to the officer. If race is not discernible at night, then police are simply less able to racially profile at night. Ridgeway cautions that an "overly simplistic implementation of this analysis" would simply compare the percentage of African American drivers stopped during the day to the percentage of African American drivers stopped at night.[81] A number of researchers do simply take into account whether a stop was made during the day or at night,[82] although they usually do so alongside other benchmarks and control variables. This is considered "overly simplistic" because other variables are likely to vary as a function of time of day. For instance, if African Americans were simply less likely to drive at night than during the day relative to members of other racial groups, then we would expect a lower percentage of African Americans to be stopped at night, but this would prove nothing about suspected racial profiling. To combat this problem of variables that are linked or confounded, Ridgeway proposes using Daylight Saving Time as a natural experiment. The pattern of stops by race can be observed on one Monday, when it is still light out at 6:30 PM, and can be compared to the pattern of stops by race on the following Monday, when it is dark out at 6:30 PM (because clocks have been set back one hour). Everything

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[78] Ridgeway, G. (2009); see also Ridgeway, G., & MacDonald, J. M. (2009). Doubly robust internal benchmarking and false discovery rates for detecting racial bias in police stops, *Journal of the American Statistical Association, 104*(486), 661-668.

[79] Birotte, 2007.

[80] Grogger. J. & Ridgeway, G. (2006), Testing for racial profiling in traffic stops from behind a veil of darkness, *Journal of the American Statistical Association, Applications and Case Studies, 101*, 475; Ridgeway. G. (2009).

[81] Grogger. J., & Ridgeway, G. (2006), p. 12.

[82] See, for instance, Nicholas P. Lovrich et al. (2007), Results of the monitoring of WSP traffic stops for biased policing: Analysis of WSP stop, citation, search and use of force data and results of the use of observational studies for denominator assessment, Report to the Washington State Patrol relating to: National Highway Traffic Safety Administration (NHTSA) Grant-Funded Study on Racial Profiling Phenomena in Washington State OGRD # 107828; Analysis Group, Inc. (2006), Pedestrian and motor vehicle post-stop data analysis report, prepared for City of Los Angeles.

else has been held constant, except for the visibility of race. This type of analysis tends to only include stops made during the inter-twilight hours, usually from approximately 6:00 PM to 8:00 PM, because they could be made during daylight one week and at night the next week.[83] This approach, then, significantly reduces the number of data points, which may reduce the statistical power to such an extent as to even preclude the possibility of detecting statistically significant racial bias. This limitation may or may not be acceptable to interested parties whose goal is to make policy recommendations based on the results of a given analysis.

A reliance on time of day as a benchmark also tends to work better for vehicle stops than for pedestrian or bicycle stops, in which the officer is more likely to be physically close to the subject of the stop—thereby making race more apparent—when the decision to make the stop is made. The subject of the stop is likely to be in plain sight as opposed to being enclosed and occluded by a large, usually moving, vehicle. Another limitation of time of day as a benchmark is that it is debatable to what extent the race of a driver is truly obscured at night and to what extent it is clearly visible during the day. Especially on city streets, as opposed to a dimly lit highway or freeway, streetlights may make it entirely possible to see the race of the driver at night, especially when an officer has the opportunity to first closely follow a car. Relatedly, many vehicles have tinted windshields and windows that can make it impossible to determine the race of the driver, even during broad daylight. Another limitation to the argument that nighttime lighting conditions completely blind an officer to race is that the make and model of the car itself, or any number of other factors (e.g., demographic makeup of the location) may act as a proxy for the race of the driver.

What role do benchmarks play in statistical models?

"Controlling for" vs. interactions: Understanding the role of additional variables

Benchmarks are a class of factors that theoretically should be of importance in establishing a baseline, or base rate, of the outcome in question. In this research, the outcomes of interest are the rates at which members of different racial groups in Oakland are stopped, handcuffed, searched, and/or arrested by the OPD. When building statistical models to test for the existence of significant racial disproportionality, we need to translate our theoretical benchmarks into concrete, operationalized, and measurable variables. For example, to include a crime benchmark in our model, we could count the number of times a given criminal offense was committed within a neighborhood (or census tract), and divide that by the number of residents in that neighborhood. For instance, if we count the number of murders that took place and divide that number by the number of residents in that same location, we have a concrete murder rate to serve as our crime variable. We can then include that variable in our model. When we "take into account" or "control for" the effect of a variable, we call that variable a "covariate."

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[83] Grogger, J., & Ridgeway, G. (2006), p. 886.

Those unaccustomed to reading academic or social scientific research are likely not familiar with the fact that including more variables in complex statistical analyses can change the underlying statistics so much as to lead to very different, or even contradictory, conclusions. Many of us have seen a description of scientific or social scientific research in the newspaper or on a popular website and read that researchers "controlled for" or "accounted for" some information or set of variables. What does "controlling for" actually mean?

As an example, let us step outside the world of policing and consider a body of research that led the World Health Organization to announce in October 2015 that processed meats cause cancer and that red meat is "probably carcinogenic." A study of over 170,000 men and women led by doctors at Harvard University found that eating 3 ounces of red meat a day can significantly increase the risk of dying early from cardiovascular disease and cancer.[84] When trying to explain variable Y (the risk of death) as a function of variable X (red meat consumption), social scientists often use statistical techniques to evaluate the influence of other variables that may also play a role in explaining Y, but which do not speak directly to the effect of X. For instance, people are at risk of dying for a whole host of reasons, many of which likely have nothing to do with eating meat. Further, individuals can vary widely in their overall risk of premature death. It is customary to talk about this challenge as wanting to *account for the variance* (or variability) in Y. The challenge of statistical analysis is to look at the variability in Y—the fact that Y is not always uniformly the same—and make sense of it by accounting for this variability with *explanatory* variables. Explanatory variables are the factors that scientists set out to explore in order to attempt to establish some pattern of cause and effect or association. What is left over (not explained by the statistical model) is referred to interchangeably as error variance, residuals, or simply noise. In this context, it is customary to add other factors or variables to the model so that instead of just explaining Y (the risk of death) with X (red meat consumption), social scientists may choose to include other predictors such as Z (family history of disease, for example). Indeed, in our meat consumption example, the researchers did include other predictors like age, level of physical activity, and body mass index. By controlling for these factors, the scientists were able to isolate and establish the role of meat consumption in predicting the risk of death. It is useful to distinguish between at least three different scenarios or reasons for including additional variables because each can lead to distinct conclusions:

1) **Reducing error variance**. One use for including additional variables is simply as "covariates." They might explain some of the variance in Y that is unrelated to X, but by being in the model they explain some of the variance in Y otherwise unaccounted for, and thus reduce the error variance. To go back to our example, including covariates might serve to explain some of the

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[84] Pan, A., Sun, Q., Bernstein, A. M., Schulze, M. B., Manson, J. E., Stampfer, M. J., ... & Hu, F. B. (2012). Red meat consumption and mortality: Results from 2 prospective cohort studies. *Archives of Internal Medicine, 172*(7), 555-563.

individual variance in the risk of dying that is unrelated to red meat consumption. The inclusion of covariates in this model serves to explain some of the "leftover" variance in risk of death, and therefore reduce the error variance, or "noise." Reducing error variance leads to more robust, or stable and reliable, models and potentially to more confidence in the estimates of the role of X. Quantitatively, because including additional variables reduces the error variance, these additional variables can also reduce standard errors (increasing the statistical accuracy of an estimate) and thus increase the statistical significance of a finding.

Returning to our example, if the team of researchers set out to understand how meat consumption affected one's risk of death and they only considered those two variables, the resulting statistical models would be very noisy. The models would be noisy because they were based on incomplete information that failed to take into account any of the hundreds, maybe thousands, of other variables that could affect one's chance of dying. One can easily imagine all sorts of genetic, environmental, demographic, and lifestyle factors that could predict a person's likelihood of dying prematurely. It is nearly impossible to quantify the strength of the relationship between meat consumption and the risk of death without accounting for some of the other factors associated with the risk of death. Alone in a statistical model, it is highly doubtful that meat consumption would emerge as a meaningful or statistically significant predictor of premature death, even though we know from the scientific research that it is an important factor. Whereas X may not have seemed like a reliable predictor of Y, the picture could change when we control for, or take into account the effect of, Z. By doing so, X may become a significant predictor when we have controlled for Z.[85] Assume Z is family history of disease. Cardiovascular disease and cancer tend to run in families. This genetic link can be very strong in some cases. Once the research team statistically accounted for family history in their analyses, and family history thus soaked up some of the variance in risk of death, then their models could better begin to estimate the true role that meat consumption played. In this case X and Z are assumed to be mostly uncorrelated, that is, having a family history of disease has nothing at all to do with red meat (although, of course, in real life this is probably untrue, as knowledge of a family history of disease may lead some to change their eating habits). This is the first meaning of "controlling for"—creating a stronger model by accounting for some of the unexplained variance.

2) **Ruling out spurious associations**. Statisticians like to say that "correlation is not causation." While there are various possible meanings of this statement, the core point is that the observed association between two variables, X and Y, could result from the fact that a third variable, Z, is actually causing both. In that case, controlling for Z would not serve the purpose of accounting for error variance, but instead to make sure we are not drawing the incorrect conclusion that there is an association between X and Y. Returning to our example, let us assume Z is fast food

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[85] We are leaving out the case of statistical suppression here for simplicity and are only addressing the function of reducing error variance.

consumption. It could be the case that eating fast food causes both higher meat consumption and a higher risk of death, perhaps because of the increased consumption of sodium and fat that comes from eating items such as French fries. Without taking fast food consumption into account, it could appear that meat consumption and premature death were linked, but this association was spurious. Fast food consumption actually caused both higher meat consumption and higher risk of death, and the meat consumption and risk of death are not directly linked. A special case of spurious association is called *mediation*. Here, let us assume that Z is body mass index, a proxy for whether or not someone is overweight. It is easy to imagine that being overweight is related to both eating red meat and premature risk of death. It could appear that eating red meat causes an increase in the risk of early death (in a simple model where X predicts Y), when really it is the case that eating red meat increases the likelihood of being overweight and, in turn, being overweight increases the likelihood of premature death. In other words, X no longer predicts Y once Z is included in the model. Alternatively, because the effect of X on Y is theorized to happen *through* Z (this is known as a *mediator model*), we might also find that controlling for Z reduces the relationship between X and Y in the model, potentially making the relationship between X and Y statistically non-significant. In contrast to **(1)** above, it is assumed here that X and Z are actually correlated or associated with each other. This is the second meaning of "controlling for"—providing a more accurate model by ruling out some spurious effects in the explained variance.

3) Showing that the effect depends on a third variable. Finally, included variables can also play a more complex role. Social scientists have a penchant for responding "it depends" when asked if one variable affects or causes another. While this may be frustrating to the layperson and may seem at first like hedging, in reality most academics are relying on their training in a series of statistical models that focus on what is called *interaction* or *moderation*. Testing for an interaction means exploring whether the role of X on Y *depends on* Z. Some disciplines use the phrase "Difference in differences" (DiD) to illustrate that the effect of one variable on a second variable depends on the value of yet a third variable. That is, Z affects the relationship of X on Y, so that it can be of one kind (positive, negative, null) for some values of Z, and of a different kind for different values of Z.[86]

Returning to our meat consumption example, imagine that Z is now one's level of physical activity. It makes intuitive sense that the effect of X (red meat consumption) on Y (the risk of death)

[86] Note that a positive relationship between two variables simply means that two variables tend to move together in the same direction: as one variable increases, so does the other one (e.g., the more educated someone is, the more money he or she tends to make) or as one variable decreases, so does the other one (the fewer the number of traffic accidents one gets into, the lower his or her car insurance premium). A negative relationship between two variables means that the variables move in opposite directions: as one variable increases, the other one decreases (e.g., the more educated someone is, the fewer children he or she tends to have) and vice versa. A null relationship means there is no relationship between the two variables (e.g., there is no link between how educated someone is and what time they go to bed).

might depend on how much exercise a person gets. For someone with a very low level of physical activity, we could imagine there might be a strong positive relationship between X and Y: For every extra ounce of red meat a sedentary person eats, his or her risk of premature death goes up by a significant percentage. However, for someone who exercises a great deal, there might be no relationship between meat consumption and risk of premature death because the person is able to effectively "burn off" the fat in the meat before it has an effect and/or his or her heart functions so well that there is no added risk of cardiovascular damage (we make no claims here; this is imagined for the sake of argument, and not based on any scientific findings). Here a social scientist would go from the type of *additive model* described in **(1)** and **(2)** above (e.g., Y ~ X + Z) to an *interactive model* (e.g., Y ~ X * Z) where the role of one predictor on X can change as a function of the value of the other predictor in the model. Another simple model of a moderation or interaction would be if the effect of meat consumption on mortality were found to depend on sex, or age, or the amount of fiber consumption. Typically, a social scientist would test such a model by adding an interactive term to the model, and if a significant interaction is revealed, she would make sense of this interaction by drilling down into the various possible values of the moderator (Z) and testing the role of X on Y at each of these levels (e.g., someone who gets no exercise, a little exercise, a moderate level of exercise, or a great deal of exercise). These effects that are conditional on the various levels of a third variable are sometimes referred to as "simple effects."

An example: Policing by night:
We will now consider another example, in the domain of policing, to illustrate the above distinctions. Let us assume that the police of the fictitious city Smallville, USA, are trying to evaluate if men are more likely to be handcuffed than are women. Here is how the three cases detailed above might play out:

1) **Reducing error variance**. In this first case, it seems at first blush that we cannot conclude that there is a difference between men and women in handcuffing rate. While there does seem to be an apparent difference in the raw data, this difference does not seem statistically significant in the regression analysis because there is still so much variance left unexplained that the coefficients are unreliable. Perhaps individuals are much more likely to be handcuffed at night than during the day. Once the variable of whether the stop happened at night versus day is entered in the model, it "soaks up" much of the error variance, and we now find that male versus female is also a significant predictor. Accounting for the role of time of day makes the effect of gender more apparent, and thus more significant.

2) **Ruling out spurious associations**. In **(1)** above, we assumed that day versus night created a lot of unexplained variance, but we did not make any assumptions about the link or association between time of day and the gender of the person being stopped. Let us assume a

different situation in which there does seem at first to be a link between gender and handcuffing rates, but upon further exploration it turns out that men are actually more likely to be out at night, and those who are stopped at night are more likely to be handcuffed. Controlling for night versus day may actually make the gender difference on handcuffing rates go away, because the gender differences can actually be explained by the fact that night stops are more likely to involve men. Gender is not the driving force; time of day is. Accounting for the role of night, then, makes the effect of gender less significant.

3) **Showing that the effect depends on a third variable**. Imagine yet a third case. Let's say that upon further exploration, what is apparent instead is that the difference in handcuffing rates between men and women is entirely due to day stops. The difference vanishes for night stops. What we have in this case is an interaction between gender and time of day. Like **(1)**, this step could reveal an effect that was hidden away in the first analysis, but whereas in **(1)** it was a matter of eliminating noise to expose an effect that happens across the board, here, what is revealed is that the *effect happens only in a subset of the data.* After showing a significant interaction between gender and time of day, the next step for the researcher would be to document the effect of gender during the day and the effect of gender at night to understand where this interaction comes from.

Legitimate or illegitimate benchmark?

Now that we know what a benchmark is and we are familiar with three roles that such additional variables can play in statistical models, let us return to a discussion of why selecting benchmarks can be controversial. In our overview of the different benchmarks used in past analyses we saw that each benchmark comes with its own set of strengths and limitations. For example, data about population demographics are intuitive and easy to obtain, given that the Census Bureau makes a wealth of information publicly available. However, just because a person lives somewhere does not mean that he or she is necessarily likely to be stopped by police.

In the use of benchmarks, what should the goal of researchers be? One possible goal is to isolate, measure, and include variables that *legitimately* explain police decision-making, but may also happen to be confounded with race so that any spurious patterns of racial disparity can be appropriately explained. This role of additional variables, you will remember, is "ruling out spurious associations" **(2)** in our list of possible cases that we discussed in the previous section. If, as we mentioned earlier, there happen to be more African Americans in high-crime areas and high-crime areas tend to be policed more heavily, then we would expect that, indirectly, there would be more stops of African Americans. By not taking the crime rate into account, a researcher might mistakenly conclude that this reflects that race was a factor when deciding whom to stop. If, however, crime were truly the driving force, then when the researcher accounted for crime rate in the analyses, the gap between the rates at which African Americans and Whites were stopped would be "explained" and would no longer be of any statistical significance. In essence, the race gap was an artifact that would now go away because neighborhood crime was in fact the reason for the police's enforcement strategy. Alongside these statistical decisions, there is a more theoretical decision to be made about which variables are *legitimate* or *justifiable* factors for police to consider and to act on when making decisions about their enforcement strategy. Most people would probably agree that neighborhood crime rate is one such legitimate factor for additional police monitoring, even if when acting on it the police may place some extra burden on the African American community to the extent that they are more likely to reside in high-crime areas.

Another goal researchers may have is to try to account for as much of the variance in police decision-making as possible. This goal would be analogous to the first case we described above, "reducing error variance" **(1)**. In line with this goal, researchers may want to include *any and all* variables that may be at all relevant in influencing police decision-making in order to account for variance and reduce noise in their models. This approach may be informed by a legal understanding of bias and discrimination that is focused more on specific evidence of biased or race-contingent decisions by police officers rather than biased or disparate racial impacts or outcomes.[87] In order to prove the existence of bias in legal arenas, for example, one must usually rule out other possible explanations first. But by including many variables in a statistical model, racial disparities may be inappropriately explained.

This latter approach of simply trying to reduce variance has been criticized. In a reanalysis of the Los Angeles Police Department's (LAPD) stop data prepared for the ACLU of Southern California, Ayres and Borowsky took issue with the previous data analysis conducted by Analysis Group, Inc. They contend that Analysis Group "inappropriately limited their analysis to an unduly cramped definition of 'racially biased policing'" (p. 3). Indeed, Analysis Group sought out as many potential explanations as possible for the racial disparities uncovered in their post-stop analyses before contending with the possibility that the LAPD might have engaged in racial profiling (note that they did not analyze stops by race, which was another point of criticism by Ayres and Borowsky). Analysis Group used as benchmarks and controlled for a relatively long list of variables, including the number of complaints and major commendations received by the stopping officer. The Analysis Group justified including officer complaints in their models because it is a "police behavioral indicator" (p. 17) and commendations because they "may indicate the demeanor and experience of officers" (p. 18). Despite their extensive use of control variables, the Analysis Group expressed concerns throughout their report about "omitted variable" bias, whereby racial disparities detected in their analysis might be explained by variables they did not or could not include in their analyses.

[87] See Ayres & Borowsky, 2008, for a discussion of this issue.

Analysis Group posited that the racial disparities they did uncover might be explained by these unquantifiable or unavailable omitted variables (e.g., vehicle condition, differences in driving behavior, whether the victim of a crime requested that the police make an arrest, etc.).[88]

In response, Ayres and Borowsky argued that the real problem is not omitted variables, but rather including too many variables, or what they refer to as "included variable" bias.[89] They argued that it is inappropriate to include control variables that "would not plausibly justify a racial disparity in outcomes," even though these variables do statistically contribute to the observed disparity and thus reduce it when controlled for.[90] Specifically, Ayres and Borowsky take issue with controlling for some officer-related variables as an attempt to explain racial disparities. The problem of included variable bias, according to Ayres and Borowsky, is twofold: It provides illegitimate explanations for racial disparities, and also causes statistical models to "understate the true size of the unjustified racial impact."[91] Consider Ayres and Borowsky's argument against controlling for whether officers have received complaints and commendations. If there is racial disparity in stops and searches, and if officers who stop and search a large number of African Americans also have more complaints lodged against them, controlling for complaints might very well statistically reduce the apparent disparity—but this reduction would be problematic, because the number of complaints could in fact be tied to racially disparate treatment itself, insofar as complaints may be received as a result of such experiences. Therefore, number of complaints is not simply a nuisance variable that needs to be explained away. It is this distinction between covariates that need to be explained away and ones that may be part of the problem that makes Ayres and Borowsky raise the issue of "included variables."

Indeed, a difficulty faced by any researcher conducting analyses of stop data is to determine which predictors are legitimate to explain racial disparities in policing decisions. In effect, this captures some of the heated debates in policing about racial profiling at the statistical level. If we find, for example, that racial disparities in the stop rate disappear once we control for poverty, then poverty can be considered a proximal cause for the greater amount of stops. But does that necessarily mean that the greater number of stops in poor neighborhoods is unproblematic? At the extreme, as exemplified by Ayres and Borowsky's concern about included variable bias, the variables used to explain the problem may in fact be part of the problem. Explaining racial disparities by controlling for officers' past records of complaints, for example, may be circular to the extent that problem officers who engage in disparate treatment of racial minorities are those who are most likely to get complaints. In turn, these complaints are included and being controlled for in a model testing for

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[88] Analysis Group, Inc., pp. 4-5, 33.

[89] Ayres & Borowsky, 2008, p. 13.

[90] Ayres & Borowsky, p. 13. For a discussion of the roles that included variables can play in a statistical analysis.

[91] Ayres & Borowsky, p. 3.

racial disparities when the complaints themselves may be proxies for those disparities. By controlling for complaints, then, the racial disparities are less likely to be statistically significant.

Even when there is a more direct link between a potential control variable and the variable being predicted (in our case the race of the person being stopped or subjected to various post-stop outcomes, etc.), including them in statistical analyses may still be questionable. Even if a police department could establish that one racial or ethnic population is statistically more involved in recorded crimes, it would still be unconstitutional for police to use race or ethnicity as the sole basis for determining reasonable suspicion or probable cause or decisions about how to treat a particular person.[92] The difficulty, then, is to separate factors on the one hand that may lead community members to behave in ways that may be considered more noteworthy or "suspicious" by police, and which might legitimately justify higher stop rates, and factors on the other hand that instead change an officer's threshold for making decisions about when to make a stop, conduct a search, or use handcuffs.

[92] See, for instance, U.S. Const. amend. XIV, sec. 1; OPD Departmental General Order (M-19, Effective November 15, 2004); CA Penal Code Section 13510-13519.15.

Our benchmarks

Overview:
The benchmarks we considered including in our analyses can be broadly classified as:

- Encounter variables
- Officer variables
- Census tract variables

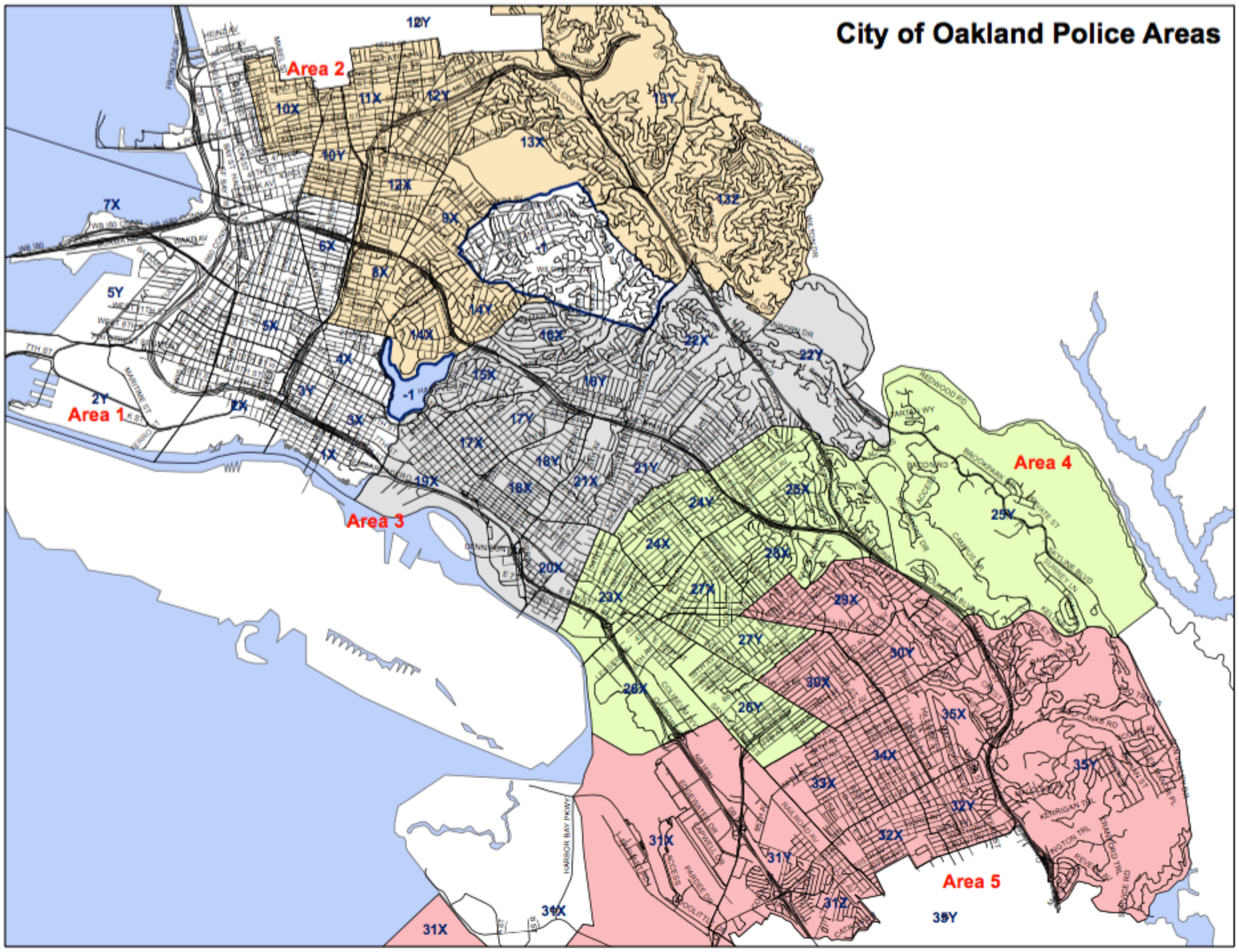
By *encounter variables*, we mean information that pertains to where, why, when, and how each stop was made, in addition to who was stopped (e.g., demographic characteristics, other than race, of the person stopped). By *officer variables*, we mean information that pertains to characteristics of the member of the OPD who made the stop (e.g., the race and gender of the officer, years of experience). Finally, by *census tract variables*, we mean information that pertains to the characteristics of the location, namely the census tract, in which the stop took place (e.g., crime rate, demographic characteristics).

Encounter variables:
Where the stop took place:
For each stop, officers are required to enter the Oakland police beat in which the stop was made. There are 64 unique police beats in Oakland and we observed that at least one stop was made in each of these 64 beats.

Oakland policing area: Using the OPD police beats, we identified in which of the five policing areas in Oakland each stop was made.[93] See Figure 2.1 below and Appendix B for maps of each of the five areas. In many of our models, we include the area in which the stop was made. If we examined policing decisions collapsed across the entire department, we might miss important variation that exists between the five policing areas of Oakland. Therefore, rather than controlling for the area in which each stop was made, we decided to examine instead the interaction between race and area. In other words, we *explore how the effect of race on policing decisions varied as a function of where the stop was made*. Because the most stops were made in Area 1, we consider Area 1 our reference group and we compare stops made in all other areas to stops made in Area 1.

[93] Beats 01X through 07X are located in Area 1. Beats 08X through 14Y are in Area 2. Beats 15X through 22Y are in Area 3. Beats 23X through 28Y are in Area 4. Finally, beats 29X through 35Y are in Area 5.

Figure 2.1. A map of the correspondence between OPD police beats and police areas



<u>Why the stop took place:</u>

Initial reason for encounter: As you will recall from the overview of the stop data form, officers are required to articulate the basis for the stop. The five possible reasons are: consensual encounter, reasonable suspicion, probable cause, traffic violation, and probation/parole (see Chapter 1 of this report for definitions of these terms). Sometimes we control for the initial reason for the stop in our analyses and sometimes we treat it as a moderator so that we can examine how outcomes of stops vary as a function of why the stop was made in the first place. Because traffic violation was the most common reason for the encounter, we considered it the baseline to which we compared all other reasons for the stop being made.

<u>When the stop took place:</u>

Using the date and time of the stop that the officer entered in the stop data form, we coded two variables pertaining to when the stop was made.

Time of day: We coded whether the stop occurred during the day or at night. Stops that were made between 7:00 AM and 6:59 PM were considered daytime stops and were assigned a value of 0. Stops that were made between 7:00 PM and 6:59 AM were considered nighttime stops and were assigned a value of 1. Daytime stops were considered the reference group when we controlled for time of day in our analyses.

Day of the week: Given the date on which the stop occurred we coded whether the stop happened during the week, Monday through Thursday, which was assigned a value of 0, or during the weekend, Friday through Sunday, which was assigned a value of 1. Weekday stops were considered the baseline against which we compared weekend stops when we included day of the week in our analyses as a covariate.

<u>How the stop took place:</u>

Type of encounter: On the stop data form, officers indicate whether the subject was stopped in a vehicle, as a pedestrian (e.g., on foot), on a bicycle, or "other." Because vehicle stops were the most common, we considered it our reference group against which we compared all other types of stops when we controlled for encounter type in our analyses. In other analyses, we treat type of encounter as a moderator variable and we examine how the degree of any race differences may vary as a function of the type of stop.

<u>Who was stopped:</u>

Community member gender: We controlled for whether the subject of the stop was male or female. We had this information for all but 8 of the 28,119 stops. Stops of males were considered the baseline.

Community member age: We controlled for the age group of the subject of the stop. The age of those stopped could fall into the following four categories: 17 or younger, 18-29 years old, 30-39 years old, 40 or older. Because stops of 18-to-29-year-olds were most frequent we considered this group the baseline.

Community member race known prior to stop: Recall that the stop data form asks officers to complete the following item: Could you determine the race/ethnicity of the individual(s) prior to the stop? In select analyses, we compared differences in stop rates by race as a function of whether or not the officer could determine the person's race prior to making the stop.

Officer Variables:

The stop data form identifies the primary officer who made the stop. The OPD provided this information in the form of the employee ID number (an internal personnel tracking number used by the Department in addition to the serial number that is captured on the stop data form). We therefore were able to track which officer made which stops during the 13-month time period under investigation, without identifying the actual officer by name. In the hope of trying to account for the variance in how each of the 510 different officers made their policing decisions, the research team obtained information about each officer who made a stop in our data set. This information was obtained using the OPD's personnel records. Officer names were *not* part of our analyses. We used employee ID number to match officers in the stop data with their personnel information in order to preserve officer anonymity. We included many of the following officer variables in our analyses.

Officer race: Using OPD personnel records we coded the race of the primary officer for each stop (White, African American, Hispanic, Asian, or Other). In some analyses we controlled for officer race and in other analyses we specifically explored the influence of officer race. Because officers were most likely to be White (43% of officers who made stops were White), White officers were treated as the baseline against which officers of all other races were compared.

Officer gender: We coded whether the primary officer was male or female. In some analyses we controlled for officer gender and in other analyses we explored the influence of officer gender. Because nearly 90% of officers were male, male officers were treated as the baseline.

Officer age: Using the officer's date of birth, we calculated how old the officer was, in years, on the date each stop was made. When we began these analyses in the fall of 2014, the average officer was 36.38 years old (SD = 8.00 years). In some analyses we controlled for officer age and in other analyses we specifically explored the influence of officer age.

Officer experience: Using the hire date on which each officer officially joined the OPD, we calculated how many months of experience each officer had on the date each stop was made. When we first began these analyses (the fall of 2014), the average officer had been with the Oakland Police Department for approximately 10 years (SD = 7.5 years) at that time. The range of experience was from approximately six months (for those who were recruits and were first hired during our time period) to nearly 36 years. In some analyses we controlled for officer experience and in other analyses we explored the influence of officer seniority.

Officer on special assignment: The stop data form asks whether the officer was on special assignment at the time of the stop (yes or no; see Chapter 1 for a description of what it means to be on special assignment). If officers indicate that they had been on special assignment, they are to select one of the following special assignment types: narcotics, prostitution, cruising, violence suppression, special event, or other. Because officers who were not on special assignment made the majority of stops, non-special assignment was considered the baseline against which all other types of special assignment were compared when we controlled for special assignment in our analyses. In other analyses, we consider special assignment a moderator variable and examine how any race differences in outcomes vary as a function of the officer's special assignment at the time the stop was made.

Officer typical assignment and squad: We were interested in whether or not the assignment influences his or her policing decisions when it comes to stops and post-stop outcomes. Further, officers tend not to work these assignments in isolation and are instead usually part of a squad composed of other officers who have similar goals and tasks. These squads, we hypothesized, might have their own unique culture, norms, and way of approaching policing under the supervision and direction of their commanding officers. Capturing these squad-level differences might provide rich insight. However, we were unable to obtain this information (see Appendix C for more information).

Individual Officer: Finally, in some analyses designed as robustness checks, we assigned a unique code to each officer for inclusion as a covariate, and treated this as a fixed effect. This fixed effect covariate allowed us to capture all of the statistical variability between individual officers, without us knowing what underlying factors (e.g., officer age, race, years with OPD) were driving those differences. We recognize the limits of observed covariates (things that we can measure and count with regard to a particular officer), but still wanted a way to know how much the officers differ from each other statistically. In this way, using this "blind" fixed effect covariate is a more "conservative" test because it controls for more of the inter-officer variance than would likely be possible if we only included factors we could directly measure. To the extent that we are less interested in the explanatory value of any particular officer-level covariate, but simply want to be exhaustive in making sure the variability is not the product of potentially unobserved or unobservable officer factors, we thought this was the most thorough approach.[94] Typically, to establish the robustness of our results, we present the results of models with observable officer covariates alongside the models with these fixed effects.

[94] Recall our discussion of omitted variable bias earlier in this chapter.

Census Tract Variables:

In the stop data form, officers are instructed to record the full address (or block number and street name) of the location of each stop. We used this information to geocode the location of the stop and thereby identify the census tract in which each stop was made. We were able to do this for 99% of the stops.[95] Stops were made in 113 unique census tracts within the borders of the City of Oakland. Knowing in which census tract each stop was made allowed us to attach demographic and other information about the neighborhood in which the stop was made.

Crime rate:

We obtained Oakland crime rate data from the City of Oakland.[96] In line with previous work, we used lagged crime data from the year before our time period began to avoid the possibility that "crime levels could be influenced by current policing policies."[97] Put another way, we did not want the crime rate we used in our analyses to be correlated with the stops that we were examining. Thus, we obtained crime rate data from April 1, 2012, through March 31, 2013. This data set contained 115,748 incidents. The public reported the majority of these crimes to the police. This is especially true of crimes that have a specific victim, such as aggravated assault, robbery, and burglary. Other types of crime, such as the possession and carrying of weapons, drug possession and

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[95] Though officers are supposed to enter the full address, in 187 cases (0.7%), the address was left blank, was in an unusable format, or was incorrect (e.g., officers mistakenly entered a summary of the stop in the address field, location was listed as an intersection that does not exist). Given that we had the addresses for 99.3% of all stops that were made in our time period of interest, we had a team of Stanford University affiliated personnel geocode the addresses of the stops using ArcGIS software. The team identified the census tract in which each stop was made. Of the stops with usable addresses, 165 stops were made outside of Oakland (mainly in Berkeley, San Leandro, Hayward, and other nearby cities). We did not include the census tract information for these non-Oakland stops.

[96] To validate that the raw crime data that we obtained from the City of Oakland would match raw crime data used by the OPD, we obtained a data set about crime for the period 4/1/13 to 4/30/14 directly from the OPD and it contained the same number of incidents as the data we received from the City of Oakland. This strengthened our confidence in the crime data provided by the City of Oakland, even if the data we use in our analysis is actually from the previous year. On another note, we used the raw crime data with which we were provided. Official crime rate data goes through an auditing process and crime statistics can be affected by late reporting, the reclassification or unfounding of crimes, and/or the process of geocoding the location of the crimes. Therefore, the numbers we were working with likely do not match exactly the official monthly reports reported directly to the FBI through the Uniform Crime Reporting (UCR) program. For example, we counted 8,998 violent crime incidents for 2013 in the raw data set provided to us, whereas the City of Oakland public report lists 7,551 (see http://www2.oaklandnet.com/oakca1/groups/police/documents/webcontent/oak044795.pdf), and the FBI public website lists 7,984. For our purposes, these absolute numbers are of little import as what matters is the variability between census tracts, which is unlikely to be biased by such reporting discrepancies.

[97] Bailey, et al., Plaintiffs v. City of Philadelphia, et al., Defendants, *Plaintiffs' fifth report to court and monitor on stop and frisk practices* (2015), C.A. No. 10-5952, p. 29.

sales, and prostitution and commercialized vice are more directly tied to patterns of law enforcement activity.

The raw data contained, among other information, the statute code of the alleged crime, as well as the City of Oakland's internal coding that corresponded to categories of crime featured in the Federal Bureau of Investigation's (FBI's) Uniform Crime Reporting (UCR) standards, and the location of the reported incident. Rather than including all possible reported crimes in our measure of crime, we chose to follow the precedent set by a number of other researchers and base our crime rate on the FBI's UCR standards. To standardize the reporting of crime rates across jurisdictions, the FBI's UCR Program specifies a select number of crimes to focus on when calculating violent crime rates and property crimes rates.[98] The data set contained 27,713 relevant property crimes and 9,055 relevant violent crimes. In addition, we identified 1,793 narcotics crimes. Using the location of the incident, we identified the census tract in which each of these alleged crimes occurred.[99]

Violent crime rate: Following the UCR procedure, we calculated the total raw number of violent crimes per census tract by adding up the total number of the following four crimes: 1) murder and non-negligent manslaughter, 2) forcible rape, 3) robbery, and 4) aggravated assault.[100] We divided the resulting number by the total population residing within each tract and multiplied by 10,000 to find the rate of violent crime per 10,000 people within each census tract.

Property crime rate: Also following the UCR procedure, we calculated the total raw number of property crimes per census tract by adding up the total number of the following four

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[98] For more information about the UCR program, see the following website, which is part of the official site of the U.S. Department of Justice: http://www.ucrdatatool.gov/

[99] In the raw crime data, we had the address of the reported incident, the OPD beat in which the incident occurred, and the X and Y coordinates. Using the X and Y coordinates, we had a team of Stanford University affiliated personnel geocode the addresses of the reported crimes using ArcGIS software. We had the team identify the census tract in which each alleged crime occurred. Of the 38,561 relevant crimes, the geocoding team was able to identify the census tract for 37,915 of the crimes (98.3%). Of these incidents, the reported location of 116 of them were outside of the City of Oakland (0.3%). In total, we included 37,799 crimes in our calculated crime rates.

[100] Murder and nonnegligent manslaughter, both of which are considered criminal homicide, are defined by the FBI as "the willful (nonnegligent) killing of one human being by another." Other types of deaths and justifiable homicides are excluded. Forcible rape is defined as "The carnal knowledge of a female forcibly and against her will. Rapes by force and attempts or assaults to rape, regardless of the age of the victim, are included. Statutory offenses (no force used—victim under age of consent) are excluded." Robbery is defined as "The taking or attempting to take anything of value from the care, custody, or control of a person or persons by force or threat of force or violence and/or by putting the victim in fear." Aggravated assault is defined as "An unlawful attack by one person upon another for the purpose of inflicting severe or aggravated bodily injury. This type of assault usually is accompanied by the use of a weapon or by means likely to produce death or great bodily harm. Simple assaults are excluded."
All definitions can be found at: https://www.fbi.gov/about-us/cjis/ucr/crime-in-the-u.s/2011/crime-in-the-u.s.-2011/offense-definitions

crimes: 1) burglary, 2) larceny-theft, 3) motor vehicle theft, and 4) arson.[101] We divided the resulting number by the total population residing within each tract and multiplied by 10,000 to find the rate of property crime per 10,000 people within each census tract.

Narcotics crime rate: We also calculated the number of all drug-related crimes per census tract. These crimes fell into 5 categories: 1) possession of marijuana, 2) possession of opium or cocaine, 3) possession of other drugs, 4) sale or manufacturing of marijuana, and 5) sale or manufacturing of other drugs. We divided the resulting number by the total population residing within each tract and multiplied by 10,000 to find the rate of narcotics crime per 10,000 people within each census tract.

Population demographics:

To learn more about the residents of the neighborhoods where stops were made, we relied on information collected by the United States Census Bureau. In order to obtain more detailed, comprehensive, and up-to-date information, we used information from the 2013 (5-year estimate) American Community Survey (ACS) rather than the 2010 Census.[102] Considered part of the decennial census, the ACS is a replacement for the "long form" that was traditionally sent to a percentage of households across the United States once every ten years as part of the official census. Like the decennial census, with which most people are familiar, the accurate completion of all ACS questions is legally mandatory. Each year, the Census Bureau selects approximately 3.5 million addresses (or about 1 in 38 U.S. households) to participate in the survey. Based on these responses,

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[101] Burglary (breaking or entering) is defined as "The unlawful entry of a structure to commit a felony or a theft. Attempted forcible entry is included." Larceny-theft (except motor vehicle theft) is defined as "The unlawful taking, carrying, leading, or riding away of property from the possession or constructive possession of another. Examples are thefts of bicycles, motor vehicle parts and accessories, shoplifting, pocket-picking, or the stealing of any property or article that is not taken by force and violence or by fraud. Attempted larcenies are included. Embezzlement, confidence games, forgery, check fraud, etc., are excluded." Motor vehicle theft is defined as "The theft or attempted theft of a motor vehicle. A motor vehicle is self-propelled and runs on land surface and not on rails. Motorboats, construction equipment, airplanes, and farming equipment are specifically excluded from this category." Arson is defined as "Any willful or malicious burning or attempt to burn, with or without intent to defraud, a dwelling house, public building, motor vehicle or aircraft, personal property of another, etc." All definitions can be found at: https://www.fbi.gov/about-us/cjis/ucr/crime-in-the-u.s/2011/crime-in-the-u.s.-2011/offense-definitions

[102] We used Social Explorer to access this information from the Census Bureau. According to the US Census Bureau's website, the ACS is: "an ongoing survey that provides vital information on a yearly basis about our nation and its people. Information from the survey generates data that help determine how more than $400 billion in federal and state funds are distributed each year. Through the ACS, we know more about jobs and occupations, educational attainment, veterans, whether people own or rent their home, and other topics. Public officials, planners, and entrepreneurs use this information to assess the past and plan the future." For more information, see: http://www.census.gov/programs-surveys/acs/about.html

estimates are generated for the entire United States population on a wide range of variables including employment and educational attainment, income, public assistance received, racial and ethnic demographics, language spoken at home, marital and family relationships, commute to work, citizenship status, and information about housing units, home ownership, property values, and rent.[103]

In line with past literature,[104] we obtained a number of demographic variables about the residents in each Oakland census tract from the Census Bureau. See Appendix D for more information about our rationale for which variables we chose to include in our analyses and for more information about how the US Census Bureau collects and defines these variables. We collected:

- **Total population in each census tract**
- **Land area (in square miles)**
- **Population density (per square mile)**
- **Percentage of the total population that is Black (non-Hispanic)[105]**
- **Percentage of the total population that is Hispanic[106]**
- **Percentage of the total population that is 24 years of age or younger**
- **Percentage of the population aged 16 years and older who are in the civilian labor force and unemployed**
- **Percentage of families living in poverty[107]**

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[103] For more information about how the ACS works, please see http://www.census.gov/programs-surveys/acs/about/how-the-acs-works.html.

[104] Ayres and Borowsky (2008); Analysis Group, Inc. (2006).

[105] Note that the federal government, and as such the United States Census Bureau, does not consider Hispanic or Latino to be a race, but rather an origin. According to the Census Bureau, "Origin can be viewed as the heritage, nationality group, lineage, or country of birth of the person or the person's parents or ancestors before their arrival in the United States" (see http://quickfacts.census.gov/qfd/meta/long_RHI725214.htm). Therefore, one could identify as White, for example, in the race question on the census and then go on to classify himself/herself in one of the specific Spanish, Hispanic, or Latino categories listed in the census questionnaire. To get around the issue of potentially double counting individuals who identify as African American and also as Hispanic, we chose to use counts of those who indicated they were Black and non-Hispanic for our percentage African American variable. Similarly, we used counts of those who indicated they were White and non-Hispanic for our percentage White variable.

[106] Because the Census Bureau does not consider Hispanic or Latino to be a race, but rather an origin, those who indicate that they are Hispanic are supposed to first choose their race. Therefore, the people who fall into this Hispanic category may be of any race (e.g., White, Black, Hispanic).

[107] Note that, according to the Census Bureau, a family "consists of a household and one or more other people living in the same household who are related to the householder by birth, marriage, or adoption." See http://www.census.gov/prod/cen2010/briefs/c2010br-14.pdf

- **Percentage of all housing units that are occupied by the owner**
- **Percentage of the population 15 years of age and older who are divorced**

In some analyses, we included a number of these variables as covariates. In other analyses, however, we specifically explored the influence of these variables.

Urban disorder and decay:

Claims to the Oakland Public Works Department: We wanted to include in our analyses a proxy for how "run down" a neighborhood is, which has been considered an important factor in policing. The now well known "broken windows" theory of policing posits that signs of urban disorder and decay, such as litter, graffiti, and broken windows and street lights, signal to would-be criminals that incivility and petty crime are tolerated in an area, which in turn increases crime.[108] Cleaning up neighborhoods, painting over graffiti, and fixing broken windows and street light can be an important enforcement strategy for tackling crime.[109] Much has been written about cities that experimented with broken windows policing in the 1990s, most famously New York City under Commissioner William Bratton.[110]

We obtained a data set from the City of Oakland, which records requests/claims made to the Department of Public Works. This data set is referred to as the Quality of Life data set or QOL.

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108 Kelling, G. L., & Wilson, J. Q. (March, 1982). Broken windows: The police and neighborhood safety. *The Atlantic Monthly*.

109 Braga, A. A., & Bond, B. J. (2008). Policing crime and disorder hot spots: A randomized controlled trial. *Criminology, 46*(3), 577-607; Sampson, R. J., & Cohen, J. (1988). Deterrent effects of the police on crime: A replication and theoretical extension. *Law and Society Review*, 163-189. For a self-proclaimed "insiders' view" of broken windows policing, see the following article written by George Kelling, one of the authors of what is considered the most classic article on broken windows policing, and William Bratton, the Commissioner of the New York Police Department when New York City began its experiment with this enforcement strategy: Kelling, G. L., & Bratton, W. J. (1998). Declining crime rates: Insiders' views of the New York City story. *Journal of Criminal Law and Criminology, 88*(4), 1217-1232. There have also been a number of critiques of broken windows policing. Opponents tend to argue that arresting and citing people for minor offenses accomplishes the exact opposite of what it sets out to do by hurting the poor through unneeded arrests and undermining the relationship between the police and the public. See Cole, D. (1999). *No equal justice: Race and class in the American criminal justice system*. New York: The New Press; Meares, T. (2015) Broken windows, neighborhoods, and the legitimacy of law enforcement or why I fell in and out of love with Zimbardo. *Journal of Research in Crime and Delinquency, 52*, 609-625.

110 For example, see Zimring, F. E. (2011). *The city that became safe: New York's lessons for urban crime and its control*. New York: Oxford University Press; Kelling, G. L., & Bratton, W. J. (1998). Declining crime rates: Insiders' views of the New York City story. *Journal of Criminal Law and Criminology*, *88*(4), 1217-1232. Overall, the evidence is mixed and academics are generally undecided about how effective the policy was in reducing crime.

Sometimes residents make these claims through Oakland's "SeeClickFix" program.[111] Like our crime rate and calls for service control variables, we used lagged data from April 1, 2012, through March 31, 2013, to prevent the possibility that our control variables could directly influence policing strategy and thereby affect the outcomes we were analyzing. The vast majority of the 20,217 claims made during this time were in regard to illegal dumping, littering, graffiti, and the need for street cleaning. A team of Stanford University affiliated personnel geocoded the addresses of these claims using ArcGIS software. The team identified the census tract for each claim in order to calculate the total number of claims per census tract. To create a rate of quality of life complaints, our measure of urban decay, we took these raw counts and divided them by 10,000. We sometimes included this QOL rate in our analyses as a covariate and sometimes as a predictor.

Economic activity:

Business Tax Registration Certificates: In our analyses, we wanted to include a proxy for economic activity and begin to establish which neighborhoods had more commercial (versus residential) areas. We obtained a data set from the City of Oakland that contained all of the business tax registration certificates issued by the City. We used data that were current as of August 2015 because we could not obtain this information going back to the time period in which the stops were made (or the year before the stops, given our preference for lagged control variables). This data set contained 26,440 businesses. A team of Stanford University affiliated personnel geocoded the addresses of these businesses using ArcGIS software. The team identified the census tract for each business, and thus we were able to calculate the total number of businesses per census tract. We then calculated the total number of businesses per square mile, which we included in our analyses.

Drivers and driving behavior:

Because the majority of the stops made during the 13-month time period under investigation were vehicle stops made because of traffic violations, we wanted information about drivers and driving behavior to include in our analyses. From the California Department of Motor Vehicles (DMV), we tried to obtain data about the number of drivers in the City of Oakland as a whole and per neighborhood, ideally broken down by race, as a way to approximate who might be eligible to be stopped in a vehicle. Additionally, we attempted to obtain traffic collision reports collected by the Oakland Police Department as a proxy for the quality of driving. We wanted a measure of driver quality to help approximate a potential base rate for the number of alleged traffic

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[111] SeeClickFix is an interactive issue submission portal, accessed via the internet or cellular phone application, that allows residents of communities such as Oakland to submit notifications to public officials regarding "neighborhood concerns like potholes and light outages." For more information, please see http://en.seeclickfix.com and http://en.seeclickfix.com/oakland.

violations (e.g., more bad drivers might translate into more stops for traffic violations). We acknowledge these measures are imperfect because, for example, not everyone who drives has (or has ever had) a valid driver's license and not all traffic violations are caused by the manner in which the driver operates the vehicle (e.g., equipment violations).

Ultimately, we were unable to acquire this information and/or it was not in a usable format that we could feasibly include in our analyses (see Appendix C for more information).

Blind covariate for neighborhood:

Individual census tract: Finally, in some robustness checks, we included as a fixed effect covariate a code that was assigned to each census tract. This "blind" covariate allowed us to capture all of the statistical variability between individual census tracts, without us knowing what underlying factors (e.g., racial demographics, unemployment rate, poverty) were driving those differences. For more information about this approach, see the discussion of our officer fixed effect covariate in Chapter 2.

Overview of analyses

In each of the chapters that are to follow, we will focus on one specific outcome. In Chapter 3, we analyze the decision to make a stop. We aggregate the stops at the level of the census tract in order to examine what factors predict the total number of stops made across different neighborhoods. We also examine the difference between the rate at which members of a particular racial or ethnic group were stopped compared to the rate at which we would expect them to be stopped based on their share in the general population and other variables. Next, we focus on the three post-stop outcomes of interest. In all of our analyses of post-stop outcomes, the stop is the unit of analysis. In Chapter 4, we provide background information necessary to understand the post-stop outcomes presented in the subsequent chapters. In Chapter 5, we focus on race differences in the likelihood of being handcuffed, when no arrest is ultimately made. In Chapter 6, we focus on high-discretion searches and recoveries and, finally, in Chapter 7, we focus on race differences in the likelihood of arrest. In Chapter 8, we switch gears and examine the influence of officer-level variables (e.g., officer gender) on the rate of stops and stop outcomes. In that chapter, the officer is the unit of analysis. In Chapter 9, we discuss how to interpret these analyses and results, and spell out our recommendations for the Oakland Police Department moving forward.

Chapter 3 | ANALYSIS OF STOPS

Core Findings	
	• ***Regardless of the percentage of African Americans living in a neighborhood, African Americans are overrepresented among those stopped.*** • ***When both the crime rate and the racial makeup of a neighborhood are included, crime rate drives the total number of stops made in a neighborhood, not the race of the people who live there.*** • ***When officers report being able to identify the race of the person before stopping them, the person stopped is much more likely to be African American.***

Overview of the analyses presented in this chapter

One of the hardest questions faced by police departments and social scientists analyzing stop data is that of *benchmarking*. Essentially, how many stops of people of different races would we expect the police to make?

Would we expect a group's proportion of stops to match their share in the general population of the city (or neighborhood) where they were stopped? This is a natural starting place, yet not without pitfalls. If a group is a minority in the general population yet constitutes a majority (or simply a much larger proportion) of stops, this warrants scrutiny, but is not necessarily evidence of a significant racial disproportionality. **First**, it is possible that members of this group display more behavior justifying a stop (e.g., traffic violations, criminal behavior), such that a pattern of stops justified on a case-to-case basis sums to the overrepresentation of that group. **Second**, it is possible that membership in that group is associated with other demographic characteristics associated with being in the pool of people to be stopped (e.g., being of driving age), or social characteristics likely to lead to a stop (e.g., poverty leading to faulty equipment or lapsed registration). **Third**, if a group happens to be overrepresented in neighborhoods that warrant a higher police presence (e.g., high-crime neighborhoods), then they could also be overrepresented simply because they are more likely to be witnessed by an officer if they commit a traffic violation or other behavior that would justifiably lead to a stop.

While it is impossible for us to rule out the first explanation (that the behavior of members of certain groups warrants more stops), we can statistically address some of the second and third explanations by controlling for neighborhood characteristics gleaned from United States Census data and by assessing whether the representation of various groups among those stopped accurately reflects the demographic makeup of the neighborhoods where more stops are made.

Should people of different races be stopped in proportion to the crime rate in the neighborhoods where they live? Not necessarily. Living in a high-crime area does not automatically make someone a criminal. Some could make the argument that people who live in high-crime areas, while certainly being deserving of police protection, are victimized enough and do not deserve the additional burden of the police routinely stopping them because others around them commit crime.[112] Consider also the fact that many of the stops made in Oakland, and in other major cities, are vehicle stops for a traffic violation. It is debatable whether crime rate more generally is relevant. It is an open question whether the fact that a neighborhood has been the scene of recent murders, rapes, aggravated assaults, and robberies (the four violent crimes we include in our violent crime rate as per the FBI's Uniform Crime Reporting program) would predict the number of equipment or moving violations that are typically the reason vehicle stops are made. A full 86% of all vehicle stops during the 13-month analysis period were made because of a traffic violation.

Would we expect people of different races to be stopped in proportion to the amount of crime that other members of their group commit? Some have argued that the ultimate benchmark for establishing who is eligible to be stopped by police is the crime rate by race.[113] But is it fair or justified, much less legal or constitutional, to stop someone based on their race and any accompanying presumption of criminality? As discussed in Chapter 2,[114] doing so would constitute

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[112] Legal scholar David Cole, in his book *No Equal Justice* (1999), explains that: "Because we live in segregated communities, most crime is intraracial; the more black crime there is, the more black victims there are. But at the same time, the more law enforcement resources we direct toward protecting the black community from crime, the more often black citizens, especially those living in the inner city, will find their friends, relatives, and neighbors behind bars" (p. 5). Strict enforcement strategies, then, may come at a very high cost to African American communities, which Cole would describe as "communities that have been doubly ravaged by crime and the criminal justice system" (p. 13). Cole, D. (1999). *No equal justice: Race and class in the American criminal justice system.* New York: The New Press.

[113] For example, see Analysis Group, Inc. (2006), Pedestrian and motor vehicle post-stop data analysis report, prepared for City of Los Angeles.

[114] See Chapter 2 of this report for a discussion of racial profiling and the use of crime rate by race as a possible (and problematic) benchmark.

racial profiling. Once again, according to OPD Departmental General Order (M-19, Effective November 15, 2004), racial profiling is defined as:

> "The use of race, ethnicity, or national origin in determining reasonable suspicion, probable cause or the focus or scope of any police action that directly or indirectly imposes on the freedoms or free movement of any person, unless the use of race, ethnicity, or national origin is used as part of a specific suspect description."[115]

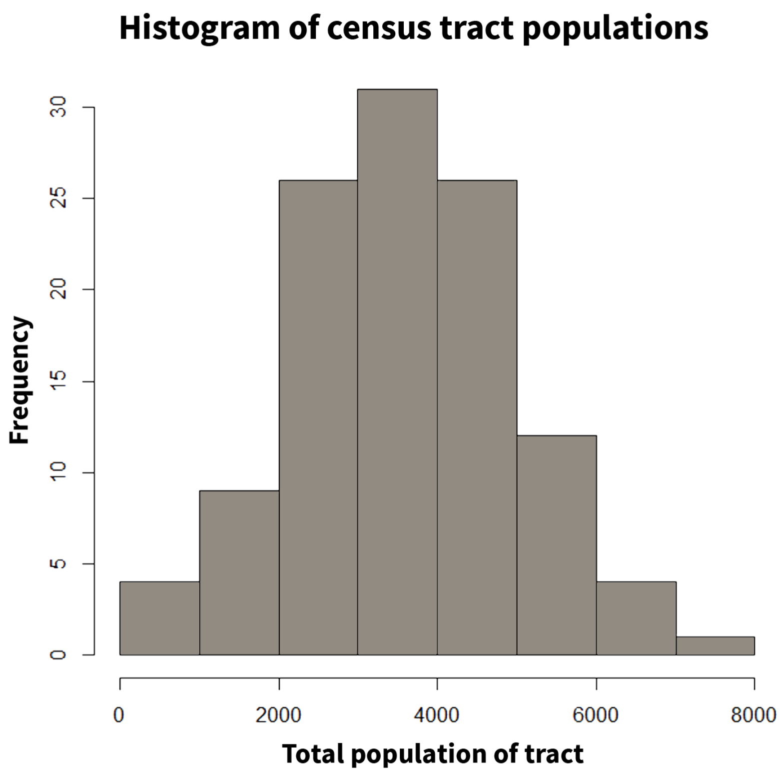
Crime rate by race, then, would also not be an ideal benchmark. Additionally, we did not have access to these data.

For all of these reasons and more, the benchmark issue is particularly thorny in an analysis of the police's decision to make a stop because it is unclear who makes up the pool of people that are eligible to be stopped. As we mentioned in the methodology chapter, some researchers have considered the issue of selecting benchmarks so problematic that they avoided analyzing stops altogether.[116] We decided to analyze the decision to make a stop since being the subject of any post-stop outcome of interest is contingent on having been stopped. For the analyses presented in this chapter, we aggregate the stops at the level of the census tract in which the stops were made and examine the difference between the observed rate at which members of different racial groups were actually stopped compared to the rate at which we would expect them to be stopped as a function of their share in the general population, while also taking into account other factors like crime rate and the size of the tract.

[115] OPD Departmental General Order (M-19, Effective November 15, 2004).
[116] See, for instance, Analysis Group, Inc. (2006).

Description of Oakland's census tracts

Figure 3.1. Histogram of tract populations



Stops within our data set took place within the City of Oakland's 113 unique census tracts, the boundaries of which are defined by the United States Census Bureau (see Appendix B for a map of census tracts overlaid with OPD policing areas). These census tracts have an average population of 3,513 people (Median population = 3,496, IQR[117] = 2,544 – 4,334), which were somewhat normally distributed (see Figure 3.1 for a histogram of tract populations). Note that all of the following analyses include a total of 27,767 stops (99% of original 28,119 stops) because these were only the stops that had useable addresses for which we could identify the census tract in which the stop was made.[118]

There were a few notable outliers, which we note here for completeness and retained in our analyses. The following 5 tracts had particularly small populations (under 1,000 inhabitants):

- 9819 (43 inhabitants) – Harbor
- 9820 (105 inhabitants) – Harbor
- 9832 (416 inhabitants) – Harbor to Jack London Square

[117] The InterQuartile Range (IQR) is the location of the middle 50% of the distribution, and is bounded by the first quartile (25% percentile) and third quartile (75%). In this example it means half of the tract populations fell between 2,544 and 4,334. It is a good way to capture the variability in a sample or population that is not too affected by outliers, and without making implicit assumptions about the shape of this distribution (as does, for example, the standard deviation).

[118] We were able to identify census tracts for stops that contained an address in the address field of the stop data form by geocoding the addresses using ArcGIS software. About 1% of stops did not contain usable addresses, and we were thus unable to identify the census tract in which the stop occurred. Therefore, these stops are not included in our analyses.

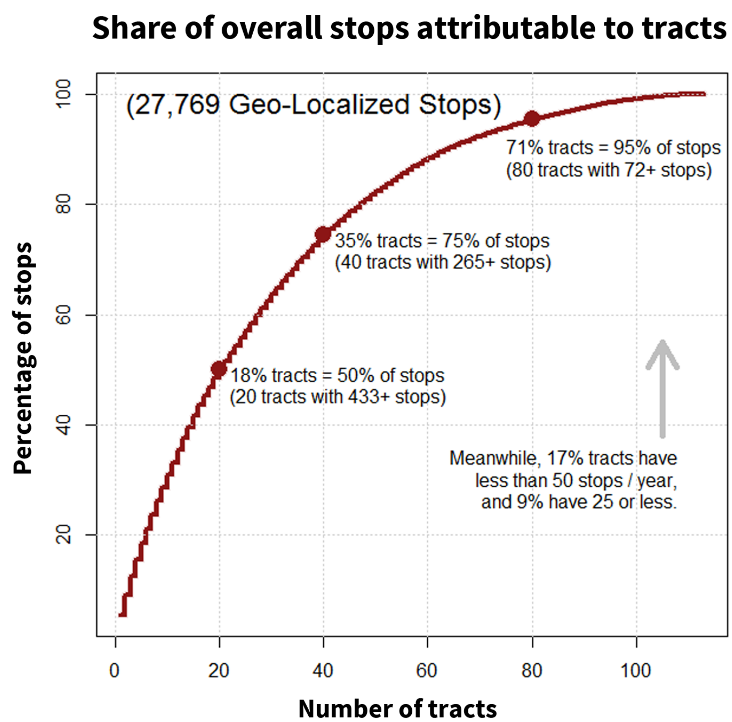
- 4026 (955 inhabitants) – Just West of OPD, Lowell Park and MLK School

Meanwhile, this tract had a noticeably larger population than most (over 7,000 inhabitants):

- 4087 (7,285 inhabitants) – Concordia Park

The average number of stops made per tract was 246 (Median number of stops = 161, IQR = 65 – 355). Furthermore, stops were not evenly distributed among tracts. The bulk of stops occurred in just a handful of tracts, while very few stops occurred in other tracts. In the graph below we see the non-uniform distribution of the stops across the census tracts: Fully half of stops took place in just 18% of the tracts, 75% of stops took place in 35% of the tracts, and 95% of stops took place in 71% of the tracts.

Figure 3.2. Share of overall stops attributable to tracts



Turning to an examination of the total number of stops made by tract, we found some noteworthy outliers in which fewer than 25 stops were made. Most of these low-stop areas are in northeast Oakland, especially east of Highway 13:

- 4045.01 (2 stops) – Piedmont/Montclair
- 4080 (8 stops) – Holy Names University

- 4100 (8 stops) – Zoo + Golf Course
- 4001 (9 stops) – Claremont Canyon (UCB)
- 4047 (10 stops) – Oakmore
- 4046 (11 stops) – Montclair / Chabot
- 4043 (18 stops) – Broadway Terrace / Temescal
- 4035.02 (22 stops) – Oakland Ave. North of Lake Merritt

This tract stood out as one where noticeably more stops were made than the rest:

- 4096 (1,523 stops) – Between Olive & International, 94th and 82nd Ave.

What neighborhood characteristics predict the number of stops?

As we mentioned, the number of stops made per census tract was skewed such that there were many tracts in which very few stops were made, whereas there were relatively few tracts in which many stops were made (see the left panel of Figure 3.3). This is problematic in terms of conducting statistical analyses because many statistical tools assume that the data approximate more of a normal (Gaussian) distribution—the symmetrical bell-shaped curve that we associate with many statistical distributions. To correct this underlying problem in the distribution of stops per tract, we can take the logarithm (the function *log[x]*) of number of stops to create a more normal distribution (see the right panel of Figure 3.3). We used the log-transformation of the number of stops as our dependent variable (that which we were predicting) in our models because of this greater statistical robustness. Log-transformation does not affect the validity of the models, and only means that once the models are computed, any prediction they make would need to be back-transformed (by using the exponential function, e^x) to generate predictions.

Figure 3.3. Histogram of raw distribution of stops by tract (left panel). Histogram of log transformed distribution of stops by tract (right panel)

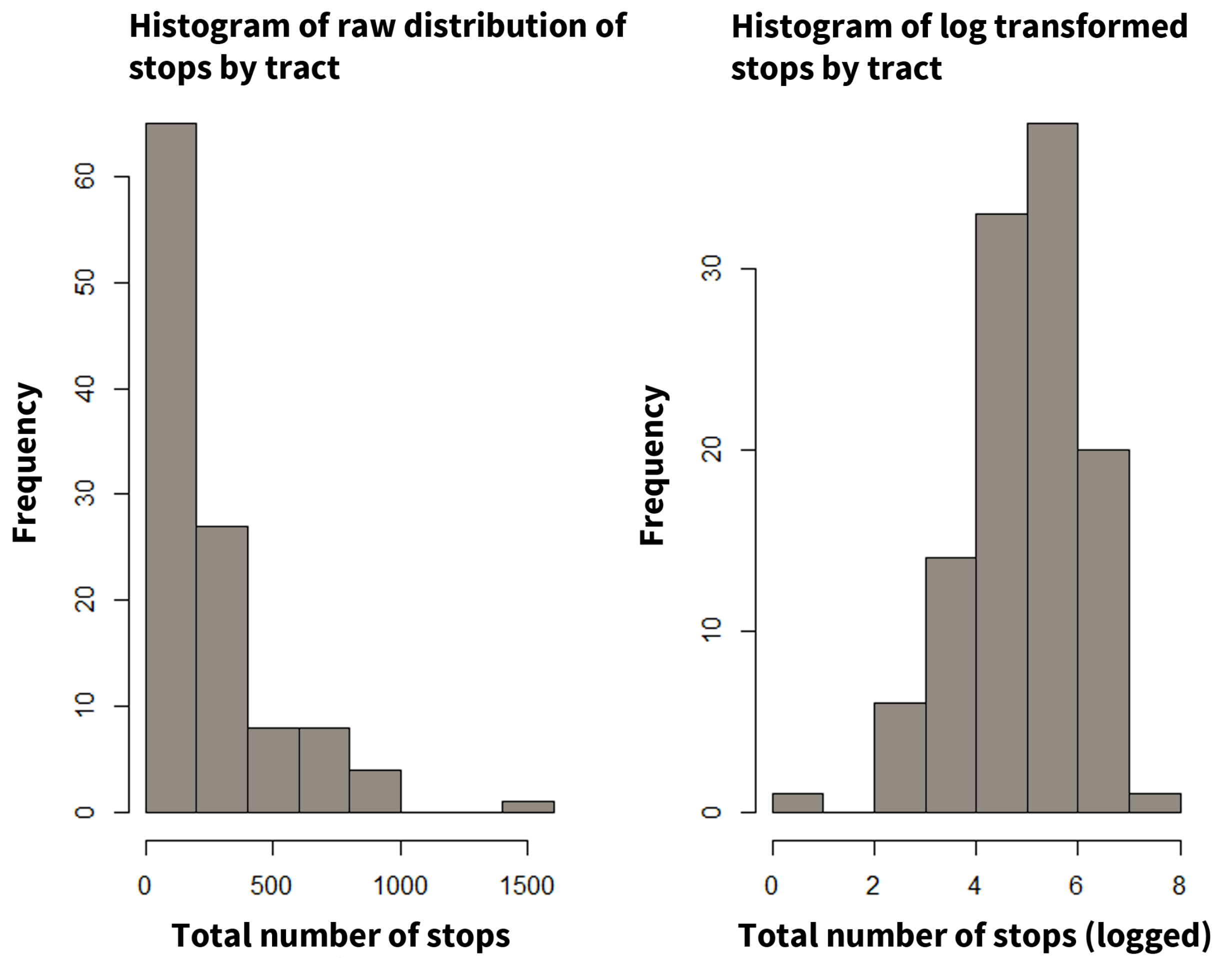


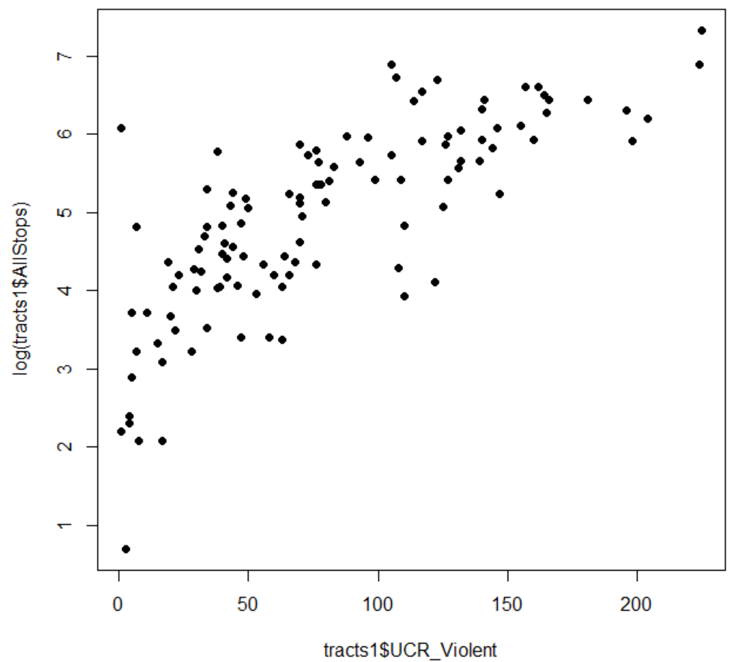
Table 3.1. Pearson zero-order r correlation coefficient between number of stops made in census tract and characteristics of that tract

	Raw	Logged
UCR_Violent	.76	.77
UCR_property	.45	.47
UCR_narcotics	.61	.59
PopDensity	.12	.21
TotalPop24YrsorYounger_percent	.28	.28
BlackAloneNotHisp_percent	.10	.25
Hispanic_percent	.46	.49
Percent15yrsOrOlder_Divorced	-.09	.01
TotalUnemployed_percent	.36	.44
Incomein2013belowpovertylevel_percent	.44	.56
Owneroccupiedhousingunits_percent	-.45	-.63
BusinessCount_persqmile	.31	.27
QOL_per10000	.20	.30

Note: All correlation coefficients with absolute values above .19 are significant p = .05, two-tailed. All correlations based on 113 tracts.

Table 3.1 shows the correlations between the total number of stops made in a census tract and demographic characteristics of that tract, namely crime rates (violent, property, and narcotics), racial and age demographics, economic factors (like unemployment, poverty, and commercial activity), and urban decay. We see that the variance in the number of stops in a given census tract is significantly correlated with many characteristics of that area. Many of these predictors, however, are themselves inter-correlated (e.g., race and poverty, etc.). Strong predictors of the number of stops were the recorded presence of crime and variables associated with economic disadvantage. The scatter plot below (Figure 3.4), for example, shows a clear linear relationship between the prevalence of violent crime and the number of stops (logged). From a statistical point of view, this is an extremely strong prediction, with 59% of the variance (the squared zero-order correlation coefficient, or .77 squared) in number of stops per tract explained simply by the prevalence of violent crime in those tracts.

Figure 3.4. Scatter plot of the association between violent crime rate and log transformed stops by tract



We also see in the table of correlation coefficients (Table 3.1) that the percentage of African American residents significantly predicts the number of stops, $r = .25$, though not very strongly. The presence of Hispanic residents was a stronger predictor, $r = .49$. A natural next question, of course, is whether areas that have a stronger concentration of African Americans or Hispanics continue to be the target of more stops once we control for other factors, like crime and economic disadvantage. In other words, does the racial make-up of a neighborhood directly affect the number of stops in that neighborhood, or is race merely acting as a proxy for crime and poverty? This is an important question because to the extent that the number of stops reflects police presence, it amounts to asking whether police are more present in primarily minority neighborhoods, once one accounts for other factors like crime rate. Multiple regression analysis enables us to gauge the impact of each predictor controlling for the other predictors in the model.

Table 3.2. Results of regression models predicting log-transformed stops as a function of characteristics of the census tract in which the stop was made

	Dependent variable:			
	log(AllStops)			
	(1)	(2)	(3)	(4)
BlackAloneNotHisp_percent	1.646** (0.581)		0.155 (0.485)	0.266 (0.514)
Hispanic_percent	3.178*** (0.528)		-0.316 (0.575)	0.407 (0.595)
UCR_Violent		0.014*** (0.002)	0.015*** (0.002)	0.010*** (0.002)
UCR_property		0.001 (0.001)	0.001 (0.001)	0.001 (0.001)
UCR_narcotics		0.011* (0.005)	0.010 (0.005)	0.004 (0.005)
TotalPop24YrsorYounger_percent				0.367 (1.156)
TotalUnemployed_percent				1.203 (1.527)
Percent15yrsOrOlder_Divorced				1.275 (1.959)
Incomein2013belowpovertylevel_percent				-0.186 (0.843)
Owneroccupiedhousingunits_percent				-1.421*** (0.402)
BusinessCount_persqmile				0.0001* (0.0001)
QOL_per10000				0.0003*** (0.0001)
Constant	3.750*** (0.214)	3.448*** (0.156)	3.436*** (0.202)	3.503*** (0.426)
Observations	113	113	113	113
R^2	0.296	0.616	0.618	0.767
Adjusted R^2	0.283	0.605	0.600	0.740
Residual Std. Error	1.043 (df = 110)	0.774 (df = 109)	0.779 (df = 107)	0.629 (df = 100)
F Statistic	23.106*** (df = 2; 110)	58.247*** (df = 3; 109)	34.568*** (df = 5; 107)	27.496*** (df = 12; 100)

*p < .05; **p < .01; ***p < 0.001

Table 3.2 shows the results of 4 regression models that build on each other in a stepwise fashion and include more and more characteristics of the neighborhood in which the stops were made. We see that even though in Model (1) it seems that the percentage of African Americans and Hispanics does matter for the overall number of stops in a tract, including crime rate as we do in Model (3), renders the underlying racial demographics of a neighborhood non-significant as a predictor, suggesting that the effect of racial make-up in Model (1) can be accounted for by crime rates. As we see in Model (2), the OPD seems to be stopping more people in high-crime areas. Taken together, the violent, property, and narcotics crime rates alone explain 62% of the variance in the number of stops (as denoted by the squared multiple correlation coefficient or R^2 at the bottom of the table).

Naturally, questions of "endogeneity" (the chicken/egg problem) are likely to arise here: the order of causality could be reversed if crimes recorded include arrests made during stops. If this were the case, then the apparent high crime rate could be the result of a large police presence and an increased number of stops, not the other way around, and thus it would be erroneous to conclude that crime rates guide the amount of stops. But note that the strongest effect is for violent crime, which includes murder, rape, robberies, and aggravated assault, all crimes unlikely to result from a stop or to prompt a stop if witnessed by an officer. In Model (3), we see that the proportion of the variance that is accounted for (R^2) does not increase at all (62% vs. 62%) when the underlying racial demographics of the tract are also included in the model alongside crime rates. In other words, crime rate seems to trump race: Any effect of racial make-up as a predictor of the number of stops seems to be a result of crime rate. There is little evidence that African American or Hispanic neighborhoods are directly targeted for stops independent of crime rates in those neighborhoods.

Finally, when the remaining tract-level predictors in our model are included simultaneously in Model (4), 77% of the variance is explained, and we see that violent crime rate is still a significant predictor, as well as quality of life calls. The proportion of owner-occupied homes is a negative significant predictor, suggesting that the fewer the homeowners (an index of economic well-being of the neighborhood), the more stops there were. And not surprisingly given the results of Model (3), the racial make-up of a neighborhood was not in itself a factor in predicting the number of stops.

The main take-away here is that we found no evidence that the OPD was specifically targeting African American or Hispanic neighborhoods once crime rate was taken into account. We did find evidence that the OPD specifically targeted high-crime areas, especially pertaining to violent crimes such as murder, rape, and aggravated assault—and this association explained any association between number of stops and the racial demographics of the census tract in which they occurred.

As a simulation, we used our statistical model to compute the number of stops to be expected in each tract based on crime rate alone. Then, we assumed that within each tract the distribution of the stops reflected the racial breakdown of that tract. For example, in Tract 4010 we actually observed 449 stops in the data, 311 of which were stops of African Americans, or 69% of the total stops in that tract. African Americans make up 43% of that tract. In our simulation, based on crime rate alone, according to Model (2), we would expect 493 stops in Tract 4010, and because 43% of those living in the tract are African American, we would expect 209 stops (or 43% of the stops) to be of African Americans—instead of the 311 we observed.

When we ran this simulation for all census tracts, we found that even allowing for more stops in high-crime areas, we would still predict only 7,773 stops of African Americans if the racial breakdown of those stops reflected the racial breakdown of the local population where stops were made out of the total 26,242 predicted stops (with 30% of the stops being of African Americans). In reality, we observed a total of 27,767 stops[119] and 16,582 of the stops were of African Americans, or 60% of the stops. This suggests that something other than greater police presence and the resulting higher number of stops in high-crime-rate neighborhoods is driving the high numbers of African American stops. The oft-repeated explanation that police simply target high-crime areas, and those areas happen to be predominantly African American, does not seem to be enough to explain the prevalence of African American stops in its entirety. We elaborate on this point below.

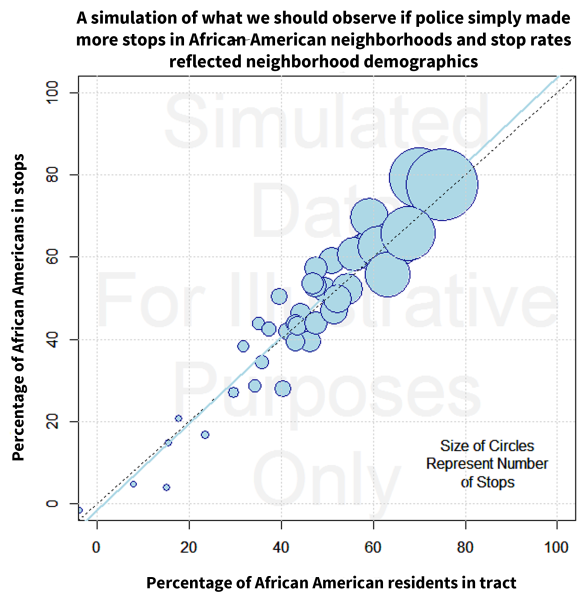
"Overrepresentation" analysis

Could the overrepresentation of African Americans among stops at the city level simply reflect the expected proportion of African Americans according to the census, but with a legitimate overrepresentation of African American neighborhoods because of high crime rates? In other words, police could be sent more routinely to primarily African American areas (potentially because of crime rates), and while there, the racial breakdown of stops would simply reflect the demographic make-up of the neighborhood, with no need to assume disproportionality or any overrepresentation of African Americans at the neighborhood level. Versions of this narrative are often offered as an explanation for apparent racial disparities and to counter concerns about race-based policing.

The figure below (Figure 3.5) presents completely hypothetical data that would support such a narrative. Each "bubble" represents a tract, and the size of each bubble is a function of the number of stops made in that tract, with larger bubbles denoting more stops. The X-axis, or horizontal axis, here captures the percentage of African Americans in the population of that census tract, while the Y-axis, or vertical axis, captures the percentage of African Americans among the stops recorded in that tract. If the percentage of African American stops in a tract (e.g., 40%) perfectly reflected the percentage of African Americans in that tract (i.e., 40%), then the bubble for that tract would fall on the diagonal (the dotted line in the figure). In such a scenario, most bubbles would fall close to the diagonal, wherein the racial breakdown of stops simply reflects the demographic make-up of the areas where they occur. However, by virtue of stops occurring more often in predominantly African American areas (the larger bubbles to the right), potentially for legitimate reasons like higher violent crime rates, it appears as though African Americans are overrepresented in stops across the city, despite fair representation at the level of each tract.

[119] Remember, we are including only those stops we were able to geocode; hence the slightly lower number than the original 28,119 stops.

Figure 3.5. A simulation of the percentage of African American stops that would be observed if police stop rates reflected neighborhood demographics



Instead, what the scatterplots of the actual data shown below (Figure 3.6) clearly illustrate is that in virtually every tract, African Americans were overrepresented compared to their share of the tract population. In the scatterplot in the upper-left corner of Figure 3.6, which plots the percentage of African American stops by percentage of African Americans in the tract, we see that nearly all of the dots are markedly above the diagonal. This means that African Americans were overrepresented among the stops relative to the number of African Americans who lived in that tract. In contrast, the two other scatterplots in Figure 3.6 (the middle and right panels) show that Hispanics and Whites were underrepresented among those stopped relative to their share of the population of the tract. Note that these three graphs are not independent of each other. What is illustrated in the graphs for Whites and Hispanics is to some extent a consequence of the graph for African Americans. If some groups are stopped more than would be expected in terms of shares of stops, then other groups must necessarily be stopped less frequently than would be expected. In other words, not all groups can be overrepresented. The low percentages of Whites and Hispanics who were stopped may be a byproduct of the high percentages of African Americans who were stopped, or vice versa. The three plots in Figure 3.7 are robustness checks that reproduce the left panel of Figure 3.6, showing the association between the number of African American stops made in a census tract and the percentage of African Americans who live in the tract, but now as a function of (from left to right) number of stops, violent crime rate, and population in the tract.

Figure 3.6. Scatterplots showing the association between the percentage of stops by race made in a census tract and the percentage of the racial group in question that lives in the tract

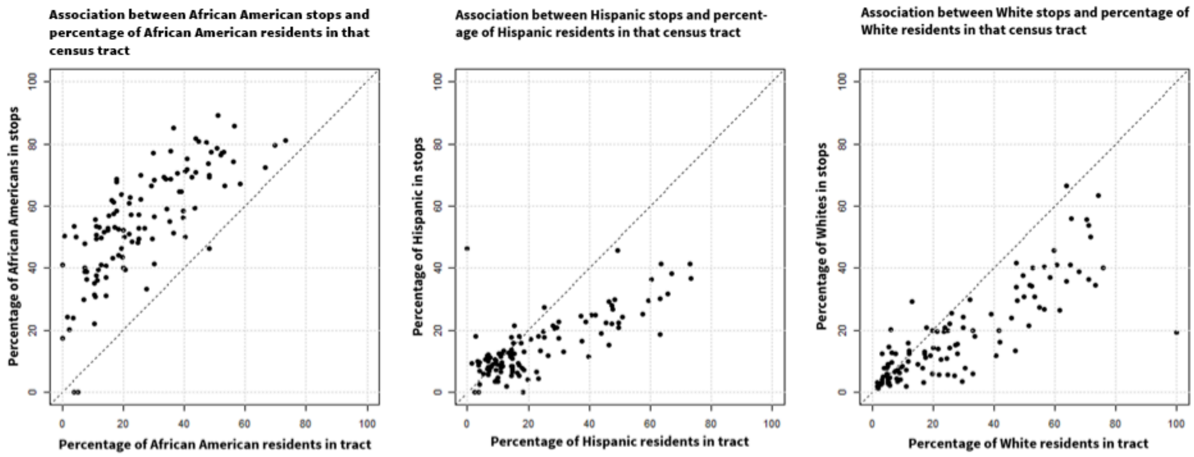
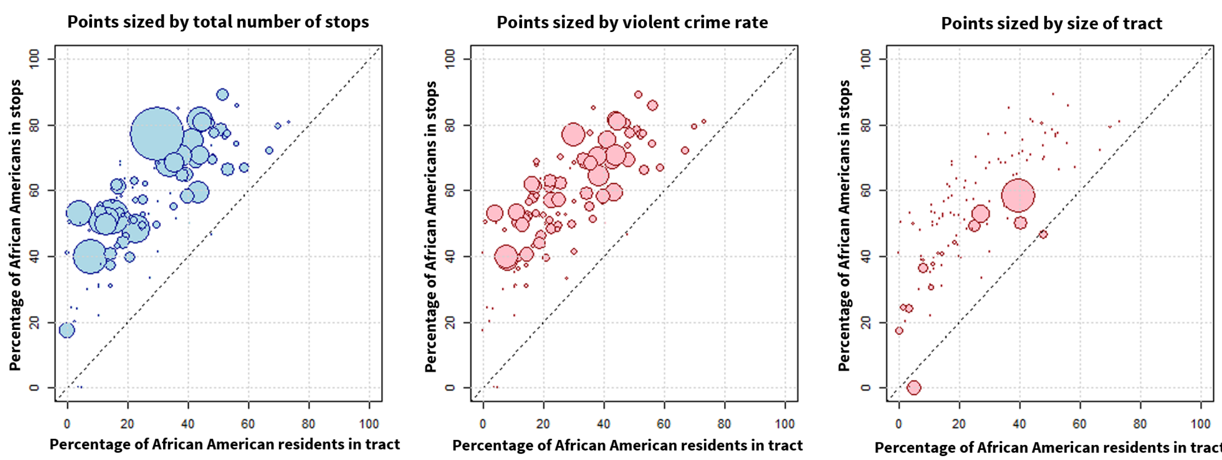


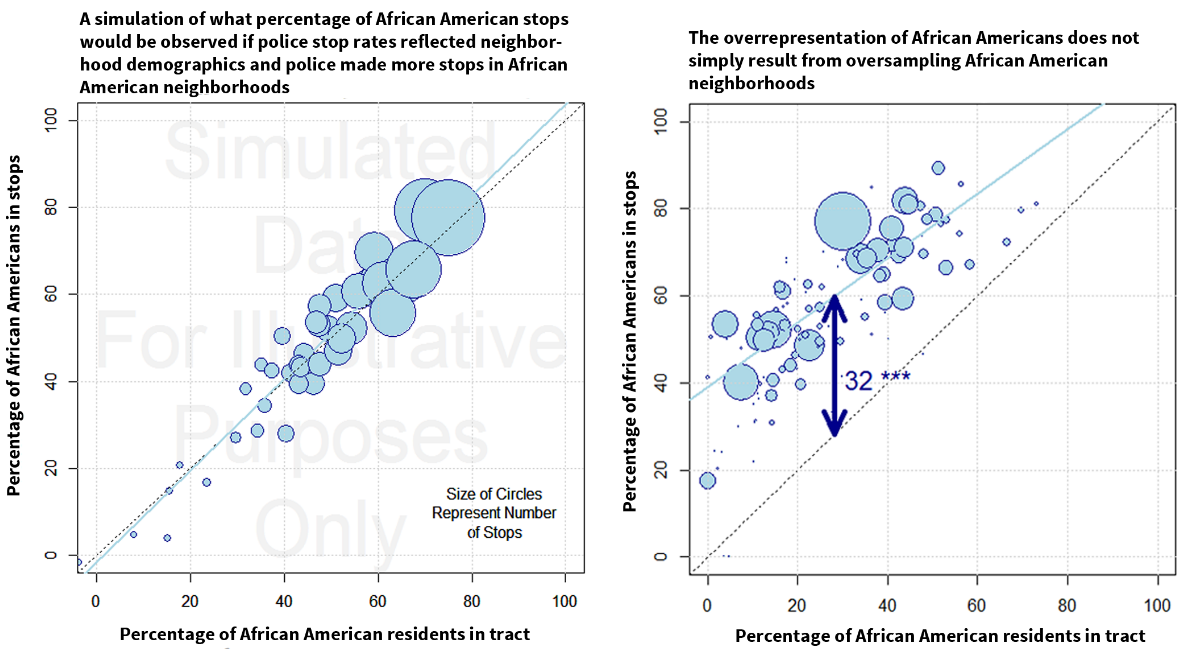
Figure 3.7. Scatterplots showing the association between the number of African American stops made in a census tract and the percentage of African Americans who live in the tract, shown as a function of: the total number of stops made (left panel), the violent crime rate (middle panel), and the size of the tract (right panel)



As mentioned in the methodology chapter, some researchers have dismissed population demographics as an inherently problematic benchmark. While census demographics may indeed be problematic as an absolute benchmark, these data show that it would be unwise to ignore them altogether in analyzing the proportion of African American stops. The percentage of African Americans living in a tract explains 64% of the variance in the percentage of stops that were of African Americans. However, the issue goes beyond explaining why some tracts have higher rates of African American stops than other tracts (the inter-tract variance, explained by the regression "slope"), to the independent question of whether rates of African American stops match demographic breakdowns, or if they are higher or lower than one would expect if stops simply reflected the demographics of those living in the area (more akin to the regression "intercept").

Readers will remember that a regression line is defined by its "slope" ("rise over run," or how much Y increases for each unit increase in X) and its "intercept" (the predicted value when X = 0, or the intersection between the regression line and the vertical or Y axis). When looking at a graph like the ones below, the slope captures whether there is a relationship between the two variables (here as one increases, the other one does too). Independent of the slope, the intercept captures how "high" or "low" the regression line is drawn on the figure. Thus here a high slope would mean a steep line and the fact that the more African Americans live in a neighborhood, the greater the proportion of African Americans among stops; a high intercept would indicate how likely African Americans are to be stopped above and beyond the neighborhood make-up: in the simulation in the left-hand panel the intercept would be close to 0, reflecting that the rate of African American stops directly reflects the percentage of African Americans in that neighborhood. An intercept that deviates from zero suggests a systematic overrepresentation of African Americans in the stops relative to their representation among residents (in practice we computed this average difference at the weighted mean of X, not at X=0, but the logic is the same).

Figure 3.8. A simulation of what percentage of African American stops would be observed if police stop rates reflected neighborhood demographics and police made more stops in African American neighborhoods (left panel). A scatterplot showing the association between the number of African American stops made in a census tract and the actual percentage of African Americans who live in the tract (right panel). Each blue dot represents a census tract, and the size of each dot is proportional to the number of stops in that census tract



On the left panel of Figure 3.8, we reproduced the hypothetical graph already shown displaying what the data would look like if officers simply made more stops in predominantly African American areas and those stops reflected the racial demographics of the area. The right panel of Figure 3.8 shows the actual data. Regardless of the proportion of African Americans in a census tract, African Americans were overrepresented among those stopped. Again, each bubble represents a census tract and the size of the bubble captures the number of stops made in that tract (i.e., large bubbles represent tracts with a lot of stops, small dots represent tracts with few stops). The light blue regression line is weighted by the number of stops, meaning that we have allowed larger bubbles (representing tracts with more stops) to exert more influence on the regression line, thus providing a model that fits the observed data better than if we simply averaged across all tracts and gave each tract the same weight regardless of the number of stops. The main point here is that the overall representation of African Americans in the stops is not simply the result of oversampling African American neighborhoods (e.g., if police were targeting high-crime areas that just happened to be mostly African American), which would yield data that look like the figure on the left. Instead, we find that at the mean percentage of African Americans in the tracts (weighted by the number of stops) there is a significant 32% gap between the census percentage and the stop percentage.[120] The percentage of African Americans who reside in the census tract matters a great deal in predicting the between-tract variability in the percentage of African Americans who were stopped (it explains 64% of that variance), but it does not tell the whole story behind the racial disproportionality in the stop rates, as indicated by the 32% gap.

Does the officer's prior determination of race predict the number of stops?

Finally, recall from Chapter 1 that the stop data form asks officers to answer the following question: "Could you determine the race/ethnicity of the individual(s) prior to the stop?" The officer indicates either "Yes" or "No."[121] We used the information about whether the race was known or unknown to the officer to predict the degree of the racial disproportionality in the stops. In this case, the bubbles and regression lines in the figure below are weighted by the total number of stops in a given census tract in which the race of the person stopped was known or unknown.

We find (see Figure 3.9) that when the officer reported that the race of the person was not known, the regression line is closer to the diagonal, and the gap is around 23%. When the officer reported that the race was known, however, the gap is 39%, an increase of 70%. A simple paired *t*-test reveals a significant difference in the percentages of African Americans stopped among these two types of stops, $t(111) = 11.98$, $p < .001$. Among stops made in which the race of the person was unknown, 48% of those stopped were African American. In contrast, among stops made in which the race of the person was known, 62% of those stopped were African American.[122]

These data may support multiple interpretations. On the one hand, it could be that race is used as a cue prior to the stop, and the use of this cue affects the decision to stop, such that identifying someone as African American makes the officer more inclined to make a stop. The 70% increase in stops of African Americans when officers indicate that they knew the race of the person, across all census tracts, suggests that race may be used in this manner. On the other hand, when police officers indicated that they did not know the race, African Americans are still stopped more frequently than

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[120] Note that 32% is not exactly the intercept (i.e., where the regression line intersects the vertical [Y] axis) because we intentionally computed it and tested it at the mean of census percentage of African Americans, weighted by number of stops. The actual intercept would be larger but less relevant, as it captures the predicted percentage of African Americans stopped in a census tract with no African Americans (at the Y axis, when X = 0).

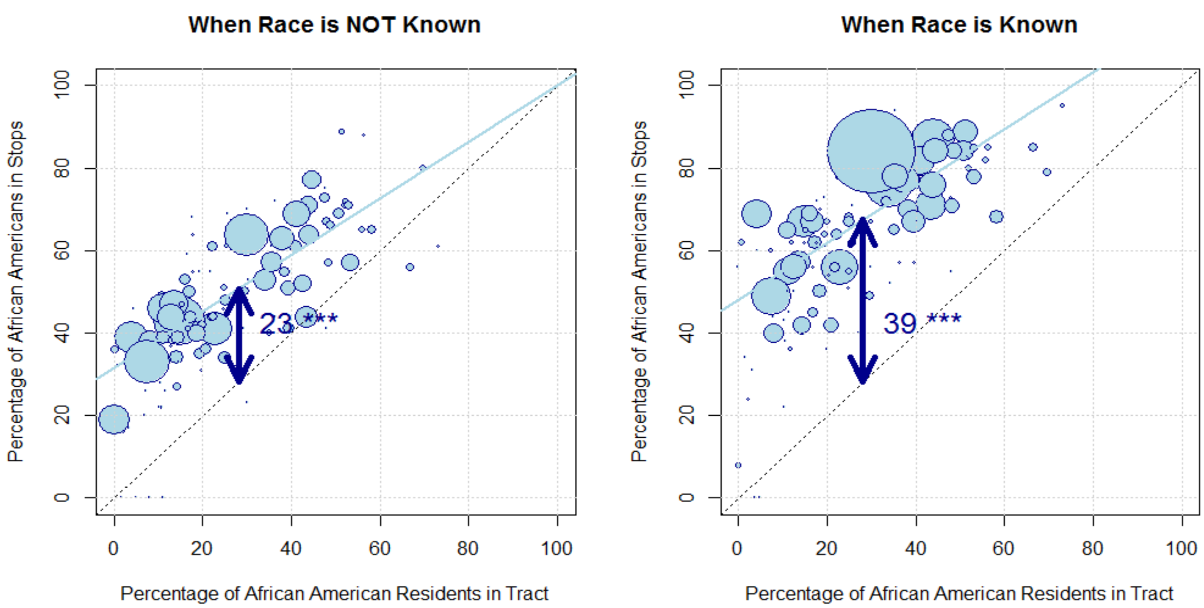
[121] See Chapter 1 for a description of this question on the stop data form.

[122] Note that the distinction between "race known" and "race unknown" is self-reported by officers after completion of the stop and is not randomly assigned. It could be the case that African American features are easier to recognize compared to other racial/ethnic groups, and so officers are more likely to identify the race of the person, and thus select "race known" when the person stopped is African American. As a result, it may be the case that officers are more likely to select "race known" for a stop of African Americans. Thus, these results should be interpreted with caution.

would be expected given population demographics (the percentage of African American residents within each census tract). This may support the argument that factors other than race, such as driving behavior, systematically cue officers to make more stops of African Americans than would be expected.

Note that a more benign interpretation of this finding is also possible. The distinction between "race known" and "race unknown" is reported by officers after the completion of the stop. It is possible that officers find it easier to make out if a person is African American than whether he or she is, for example, White or Hispanic. Thus, it is possible that what we are observing is not more African American stops when race was visible a priori (which may suggest race-based policing) but instead that when officers stopped a Black person (for whatever *legitimate* reason) it was easier for them to determine a priori his or her race, and they are therefore more likely to select "race known" when the person is African American compared to other racial groups, resulting in more African American stops in the "race known" category.

Figure 3.9. A scatterplot showing the association between the number of African American stops made in a census tract and the percentage of African Americans who live in the tract as a function of whether race is not known by the officer prior to the stop (left panel) or is known prior to the stop (right panel)



Chapter 4 | UNDERSTANDING THE POST-STOP OUTCOME ANALYSES

In this chapter, we provide the reader with some background information needed to fully understand the analyses of post-stop outcomes described in the upcoming chapters. In Chapter 5, we explore handcuffing. In Chapter 6, we examine searches and recoveries. In Chapter 7, we explore arrests. As a brief roadmap, this chapter will cover 1) how we conducted the statistical analyses of the post-stop outcomes, 2) how we will present the results of those analyses, and 3) some basic information about the raw numbers of people who were stopped, handcuffed, searched, and arrested in order to give the reader a quick snapshot of the OPD's activity.

How do we statistically test whether or not there are race differences in the post-stop outcomes?

The type of model we built to test whether or not there were statistically significant race differences in the likelihood of being subjected to one of the three post-stop outcomes is known as a *logistic regression*. Logistic regressions are appropriate for the data at hand because the outcomes we are focusing on are dichotomous, meaning that they can fall into only one of two categories. More concretely, either someone was handcuffed or she wasn't. Either someone was searched or she wasn't. Either someone was arrested or she wasn't. The data to be explained take the form of "yes" and "no," and we are trying to figure out what predicts whether or not someone fell into the "yes" category. This task is different from trying to predict how two variables that can take on a wide range of numerical values rise and fall together (e.g., the relationship in the previous section between the percentage of African Americans living in a neighborhood and the percentage of African American stops in that neighborhood).

In any given model, we will have an independent variable, a dependent variable, and covariates. In addition, some models include a moderator variable.

An **independent variable,** also known as the variable of interest, is the factor that we are specifically interested in establishing what its effect is. For our purposes, **this independent variable is the race of the person stopped.** More specifically, because African Americans are the racial group for whom we observe police treatment that appears to diverge most from other groups, we tend to focus on African Americans because, statistically, there is potentially more of a gap to be explained. Race does not exist in the abstract. Rather, we are interested in how different groups are treated *relative to each other*. Because White Americans are the majority racial group in this country numerically speaking[123] and in terms of representation in positions of power, we treat Whites as the baseline against which all other racial groups are compared.[124]

The dependent variable is the outcome we are trying to predict. In Chapter 5, the dependent variable is the likelihood of being handcuffed. In Chapter 6, the dependent variable is the likelihood of being subjected to a search (and later in that chapter, the likelihood of being found with contraband). In Chapter 7, the dependent variable is the likelihood of being arrested.

Covariates are the factors that we are controlling for in our analyses.[125] These are factors that we think could legitimately explain apparent differences in treatment by race. Returning to our often-repeated example, it could be the case that African Americans are simply more likely to be physically present in high-crime areas and police may stop more people in these high-crime areas as part of a targeted enforcement strategy. If this were the case, we would expect, then, that indirectly more African Americans would be stopped. In a statistical analysis, once we account for the high crime of the area, the link between race and an increased likelihood of being stopped should go away and be reduced to statistical non-significance.

Moderators are factors that affect the relationship between the independent and dependent variables. More specifically, we will explore whether the effect of race on stop outcomes **depends** on, for example, whether the stop was a vehicle or pedestrian stop. The underlying logic is that we are examining whether there is a "difference in differences" by race.[126] The effect of race may not be uniform across our data set, and may be more or less pronounced depending on some other factor. Remember that when we treat a variable as a moderator instead of as a covariate, we are not

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[123] Whites still make up a majority of the population in the United States. As of 2014, Whites (non-Hispanic) made up 62.1% of the total population. See: U.S. Census Bureau. (2016). QuickFacts from the U.S. Census Bureau. Retrieved from: https://www.census.gov/quickfacts/table/PST045215/00

[124] It is consistent with previous work in the social sciences to treat Whites as the baseline. For example, when discussing disease rates by race, scholars often talk about how *African Americans have higher rates of disease than Whites,* who are considered the baseline. Racism and discrimination are seen as additional burdens to people of color that negatively affect health, relative to what it might be otherwise. See: Williams, D. R. (1999). Race, socioeconomic status, and health: The added effects of racism and discrimination. *Annals of the New York Academy of Sciences*, *896*(1), 173-188. Similarly, in discussing the academic achievement gap between Whites and African Americans, many scholars point to additional burdens that African American students face in the classroom (such as stereotype threat) that depress their academic performance relative to what it might otherwise be (i.e., the same as Whites). See Steele, C. M., & Aronson, J. (1995). Stereotype threat and the intellectual test performance of African Americans. *Journal of Personality and Social Psychology*, *69*(5), 797-811.

[125] See Chapter 2 for a discussion of covariates.

[126] See discussion of moderation in Chapter 2 of this report.

necessarily indicating that the variable is a legitimate reason why we might expect race differences. Rather, we are trying to shed light on a variable that in practice may affect policing decisions, and thus could serve as the basis for making a policy recommendation that might lessen the degree of racial disparities.

How will the results of the post-stop outcome analyses be presented?

For ease of comprehension, we focus the post-stop outcome chapters on likelihood tables that present the probability that a community member, once stopped, was handcuffed, searched, or arrested, as a function of race, area, and other factors. We present significance tests comparing rates for Whites compared to other groups (primarily African Americans and Hispanics), either in the raw data (no covariates), or in models controlling for a number of demographic and other factors (with covariates). The full logistic regression equations on which these prediction tables are based are presented in Appendices H through J.

When we say that something is "statistically significant," we are referring to a *p*-value of less than .05, a standard across much of the social sciences. A widespread convention in the social sciences is to test models by relying on "Null Hypothesis Significance Testing" (NHST). The somewhat backward logic of NHST is to posit first that there is no effect to be explained (e.g., there is no difference in handcuffing rates between Blacks and Whites), also called the "Null Hypothesis." Allowing for the fact that even in the absence of any real structural difference, we would not expect the observed rates to be exactly the same because of naturally occurring variability or noise, we then compute whether any difference observed in the data is within the range that we would expect by chance alone if the Null Hypothesis were true, or whether the difference is so large that the Null Hypothesis is unlikely to be true. By convention, "unlikely" is set at 5%, such that if there is a 5% or lower probability that we would observe this difference if the Null were true ($p < .05$), we "reject" the Null and posit instead that there is a meaningful, "statistically significant," difference in rates. Because differences occur between any two samples simply as a result of natural variance, social scientists dismiss any observed difference that is not statistically significant as potentially unreliable, unlikely to obtain again in subsequent measures, and potentially resulting from too small a sample. In the analyses we present, we similarly invite readers to pay attention only to differences marked as significant, typically with an asterisk (* $p < .05$).

For each analysis, we present two likelihood tables. The first likelihood table shows the raw likelihoods. These raw likelihoods are very similar to frequency tables that people are used to seeing. These tables present, for example, what percentage of African Americans, compared to Whites, who were stopped in Area 1 were handcuffed. In the second likelihood table, however, we show the likelihoods by race and area after they have been adjusted to control for our covariates. In other words, we are trying to establish what the likelihood of being subjected to the various post-stop outcomes would be by race and area if all else were equal. Do we still see a race difference after we have accounted for all of our covariates and crime and poverty, for example, have been held constant? If, for example, African American community members are handcuffed more often because they find themselves more often in high-crime areas, and officers tend to handcuff more in high-crime areas, then controlling for the effect of crime rate on handcuffing should shave off this difference, and we should no longer see a race difference—to the extent that crime rate was the only factor explaining it.

More technically, to create these adjusted likelihood tables, we calculated a "control value" for each covariate (see Table 4.1). For continuous variables (e.g., those variables that take on fully numerical properties, such as the percentage of the total population that is Hispanic or the number of businesses in a given tract), we set the control value at the average value for the City of Oakland for tract-level variables (e.g., crime rate) or the average value among officers in our data set for officer-level variable (e.g., age). Note that these values are therefore *not* the average values in the stop data set. Using the average across the stop data would overrepresent tracts and officers with more stops. For categorical variables (e.g., those variables that refer to whether something is one kind or another, such as whether the officer who made the stop was male or female or whether the stop was a vehicle or pedestrian stop), the control value was the most frequent category. We then mathematically fixed, or held constant, all of the covariates at their control value by plugging these values into our models, such that *only the race of the person stopped and the area in which the stop was made were allowed to vary*.

In our more detailed analyses, we present only the likelihoods for Whites, African Americans, and Hispanics, because there were too few stops of those identified as Asian or Other to allow for the generation of reliable estimates within each area (but see Appendices H - J for the full results, including results for Asians and "Others" in the regression tables). As we have said before, stops of Whites are considered the baseline. Stops of African Americans and stops of Hispanics are compared to stops of Whites. The asterisks (*) in these likelihood tables indicate whether or not stops of African Americans and stops of Hispanics were statistically different ($p < .05$) from stops of Whites in the likelihood of involving handcuffing, searching, or an arrest.

As we go forward, recall that 60% of all stops were of African Americans. In comparison, 13% of stops were of Whites and 18% were of Hispanics. In raw numbers, 16,818 stops were of African Americans, compared to 3,661 stops of Whites and 4,933 stops of Hispanics. It is worth pausing here for a moment to note the implications of this underlying difference, whether or not it is justified or legitimate, for various communities. Assuming, for example, that police officers are perfectly calibrated so that anyone stopped, regardless of race, is as likely to ultimately receive a citation or be arrested (X%), then note that the impact on the African American community is much larger because 16,818*(1-X%) were stopped when there was no clear outcome or consequence, compared to only 3,661*(1-X%) for Whites.

In each post-stop outcome chapter, we explore the role of type of encounter (see Table 4.2). Vehicle stops were most common (69% of stops). Broken down by race, 54% of vehicle stops were stops of African Americans, compared to 20%, or 3,939, of Hispanics, and 14%, or 2,822, of Whites. Overall, there were 6,995 pedestrian stops, or 25% of the data. By race, there were 5,193 African American pedestrian stops, 660 White pedestrian stops and 770 Hispanic pedestrian stops. Based purely on the overall stop rate by race, we would expect approximately 60% of pedestrian stops to be of African Americans. Instead, we observed that 74% of all pedestrian stops were of African Americans.

We also explore the role of reason for encounter (see Table 4.3). Again, based purely on the stop rate by race we would expect approximately 60% of African American stops to be made for each of the 5 reasons. Instead, we observe that African Americans made up 71% of probable cause stops, 74% of reasonable suspicion stops, 70% of consensual encounters, and 77% of probation/parole stops. On the flip side, African Americans made up 53% of those stopped for traffic violations, or 9,654 stops of African Americans in raw numbers. Between 3.5 and 14 times as many African American stops as White stops happened for each reason. In ascending order: 3.5 times as many African Americans as Whites were stopped because of traffic violations (9,654 African American stops vs. 2761 White stops). 7 times as many African Americans as Whites were stopped as part of consensual encounters (844 vs. 120). 7 times as many African Americans as Whites were stopped because of probable cause (4,129 vs. 579), 10 times as many African Americans as Whites were stopped because of reasonable suspicion (1,808 vs. 175), and nearly 15 times as many African Americans as Whites were stopped because of probation/parole (383 vs. 26).

Finally, we examine the moderating role of whether or not the officer was on special assignment at the time of the stop (see Table 4.4). Among non-special-assignment stops, 64% or 9,700 stops were of African Americans, 10% or 1,531 stops were of Whites, and 17% or 2,594 were of Hispanics. Among violence suppression stops, 57% or 4,007 stops were African Americans, 17% or 1,169 stops were of Whites, and 15% or 1,080 stops were of Hispanics. Lastly, among other types of special assignment, 52% or 3,111 stops were of African Americans, 16% or 961 stops were of Whites, and 21% or 1,259 stops were of Hispanics.

Table 4.1. Control values of all covariates

Description of Covariate	Control Value (Most Common Frequency for Referent Categories)	
Encounter Covariates		
Sex of community member	Male	(75 %)
Age of community member	18-29	(42 %)
Reason for the encounter	Traffic Violation	(64 %)
Type of encounter	Vehicle	(69 %)
Week or weekend	Mon-Thu	(61 %)
Time of day	7am-7pm	(54 %)
Census Tract Covariates		
% Pop Hispanic	26 %	
% Pop Black (non-Hispanic)	27 %	
% Pop Younger than 25	31 %	
% Pop>15 Divorced	10 %	
% Pop>16 Unemployed	13 %	
% Housing units owned by owner	37 %	
% Poverty level	17 %	
Violent crime rate (/10,000)	226	
Property crime rate (/10,000)	682	
Narcotics crime rate (/10,000)	45	
Population density	7,100	
Businesses per square mile	471	
Quality of life calls (/10,000)	504	
Officer Covariates		
Officer age at time of stop	37.0	
Officer experience at time of stop	9.4	
Officer gender	Male	(89 %)
Officer race	White	(43 %)
Special assignment	None	(54 %)

Note: All subsequent analysis presented "with covariates" set these variables at these control values. Control values for census tract covariates are set at Oakland averages, while control values for officers are set at the average for officers in the data set. Categorical predictors use the most frequent category as the reference category.

Breakdown of moderator variables

In the coming chapters, we will be presenting predicted proportions of members of various racial groups who were or would be handcuffed, searched, and arrested, as a function of area and of various moderators: type of encounter, reason for encounter, and special assignment. Our focus will be on the percentage predicted by logistic regression equations (corresponding to raw observed values for the model without covariates, and controlling for covariates by setting them at their control value in the model with covariates). These numbers are all contingent on being stopped, and on finding oneself in that category (e.g., vehicle stop, traffic violation, no special assignment). As we already said, for ease of presentation we present percentages in the prediction tables, not the actual raw frequencies that we observed or predicted. However, not including the raw numbers can obfuscate the racial breakdown within each of the moderator categories, and therefore obscure the human toll. For example, while these analyses test whether African Americans were more likely to be arrested contingent on being stopped for probable cause than Whites stopped for the same reason, these analyses do not compare whether African Americans and Whites were more or less likely to be stopped for probable cause in the first place. While we made this analytical choice because, as for stops, it is very difficult to determine the proper denominator/benchmark to use in these calculations (perhaps African Americans do exhibit more evidence indicative of probable cause), readers may want to keep these numbers in mind as they look at the percentages reported in the following sections.

To remedy this, over the next several pages, we present tables that show *the raw numbers of each racial group who fell into a particular category of stop* (for example, the number of African Americans stopped in Area 1 in vehicles, or the number of Whites stopped in Area 5 because of reasonable suspicion). Tables 4.2, 4.3, and 4.4 present these raw numbers of stops for 100% of our data set (28,119 stops). Later, in each chapter, we present these numbers with exclusions that match the analyses we conducted.

Table 4.2. Frequency of observed stops by area, race, and type of encounter

	White	Afr Am	Asian	Hispanic	Other
Overall	3,661	16,818	1,827	4,933	880
%	.13	.60	.06	.18	.03
Vehicle Stops					
	White	Afr Am	Asian	Hispanic	Other
Area 1	999	2,647	456	666	224
Area 2	1,003	1,278	302	230	142
Area 3	373	1,557	487	668	137
Area 4	257	1,848	176	1,237	118
Area 5	190	3,145	79	1,138	111
Total	2,822	10,475	1,500	3,939	732
%	.14	.54	.08	.20	.04
Pedestrian Stops					
	White	Afr Am	Asian	Hispanic	Other
Area 1	231	1,590	43	112	21
Area 2	179	656	13	39	12
Area 3	99	633	161	176	34
Area 4	90	631	20	228	22
Area 5	61	1,683	20	215	26
Total	660	5,193	257	770	115
%	.09	.74	.04	.11	.02
Bicycle Stops					
	White	Afr Am	Asian	Hispanic	Other
Area 1	57	275	2	20	3
Area 2	35	164	3	14	2
Area 3	21	51	10	22	2
Area 4	8	89	2	32	2
Area 5	6	215	2	44	0
Total	127	794	19	132	9
%	.12	.73	.02	.12	.01
Other Stops					
	White	Afr Am	Asian	Hispanic	Other
Area 1	17	77	5	4	7
Area 2	4	31	4	3	5
Area 3	11	56	36	25	4
Area 4	8	41	3	20	2
Area 5	12	151	3	40	6
Total	52	356	51	92	24
%	.09	.62	.09	.16	.04

Table 4.3. Frequency of observed stops by area, race, and reason for encounter

Traffic Violation					
	White	Afr Am	Asian	Hispanic	Other
Area 1	1,065	2,665	438	662	223
Area 2	961	1,304	285	211	129
Area 3	337	1,260	436	585	118
Area 4	227	1,694	165	1,138	102
Area 5	171	2,731	76	1,026	91
Total	2,761	9,654	1,400	3,622	663
%	.15	.53	.08	.20	.04
Probable Cause					
	White	Afr Am	Asian	Hispanic	Other
Area 1	165	1,150	42	93	20
Area 2	187	526	29	51	26
Area 3	108	589	148	183	37
Area 4	69	510	23	223	27
Area 5	50	1,354	15	208	21
Total	579	4,129	257	758	131
%	.10	.71	.04	.14	.03

Reasonable Suspicion					
	White	Afr Am	Asian	Hispanic	Other
Area 1	35	447	9	26	2
Area 2	44	182	6	16	4
Area 3	35	274	72	79	10
Area 4	37	256	7	95	10
Area 5	24	649	8	106	20
Total	175	1,808	102	322	46
%	.07	.74	.04	.13	.02
Consensual Encounter					
	White	Afr Am	Asian	Hispanic	Other
Area 1	32	219	15	19	10
Area 2	22	82	2	6	1
Area 3	20	145	26	30	7
Area 4	25	104	5	43	5
Area 5	21	294	5	67	9
Total	120	844	53	165	32
%	.10	.70	.04	.14	.03

Probation/Parole					
	White	Afr Am	Asian	Hispanic	Other
Area 1	7	108	2	2	0
Area 2	7	35	0	2	1
Area 3	4	29	12	14	5
Area 4	5	45	1	18	0
Area 5	3	166	0	30	2
Total	26	383	15	66	8
%	.05	.77	.03	.13	.02

Table 4.4. Frequency of observed stops by area, race, and special assignment

		No Special Assignment			
	White	Afr Am	Asian	Hispanic	Other
Area 1	491	2,674	216	397	95
Area 2	418	947	100	98	52
Area 3	261	1,244	367	455	89
Area 4	206	1,643	92	817	86
Area 5	155	3,192	64	827	89
Total	1,531	9,700	839	2,594	411
%	.10	.64	.06	.17	.03
		Violence Suppression			
	White	Afr Am	Asian	Hispanic	Other
Area 1	499	1,154	172	211	108
Area 2	489	783	153	114	64
Area 3	79	435	137	177	46
Area 4	63	515	51	309	24
Area 5	39	1,120	17	269	20
Total	1,169	4,007	530	1,080	262
%	.17	.57	.08	.15	.04
		Other (Cruising, etc.)			
	White	Afr Am	Asian	Hispanic	Other
Area 1	314	761	118	194	52
Area 2	314	399	69	74	45
Area 3	164	618	190	259	42
Area 4	94	451	58	391	34
Area 5	75	882	23	341	34
Total	961	3,111	458	1,259	207
%	.16	.52	.08	.21	.03

Chapter 5 | ANALYSIS OF HANDCUFFING

Core Findings	
	• ***Excluding arrests, African American men were handcuffed in 1 out of every 4 stops vs. 1 in every 15 stops for White men.*** • ***Even controlling for multiple covariates like neighborhood crime rate, African Americans were still significantly more likely to be handcuffed (excluding arrests) than Whites in 4 out of 5 of Oakland's policing areas.*** • ***The African American-White race difference in handcuffing was especially pronounced for vehicle stops and stops made because of traffic violations.***

In this chapter, we test the question of whether or not there is a statistically significant difference in the likelihood that stops of members of different races involved handcuffing. Our main focus is the difference in handcuffing rates between stops of Whites and stops of African Americans. Looking at the raw numbers, Whites were handcuffed in 12.5% of stops, compared to 34.6% for African Americans and 21.5% for Hispanics. Because arrests tend to trigger automatic handcuffing,[127] we excluded all stops that resulted in an arrest (4,099 stops or 15% of the data set—which excludes 3,949 instances of handcuffing, or 51% of all cases of handcuffing). When excluding arrests, the overall rate of handcuffing was 5.7% for Whites, 21.0% for African Americans, and 12.1% for Hispanics. Just looking at men, we find a rate of 6.8% for Whites, 24.7% for African Americans, and 13.9% for Hispanics. Thus, excluding individuals who were ultimately arrested, 1 in 4 Black men stopped by OPD was handcuffed compared to 1 in 15 White men.

Are there racial disparities in handcuffing rates?

To dig deeper statistically into these apparent discrepancies, we started out by conducting a simple logistic regression analysis predicting whether or not stops of Whites significantly differed in the

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[127] Oakland Police Department. (October 30, 1998). *Training bulletin III-B.7: Handcuffs, when to use.* Note that our data set includes 150 stops of people who were arrested that did not involve handcuffing. This could be due to errors in how the data was recorded or may reflect some circumstances that made it unnecessary or impossible to use handcuffs.

likelihood of handcuffing when no arrest was ultimately made compared to stops of other racial groups. We included an interaction term between the race of the person stopped and policing area so we could examine how the effect of race varied as a function of the location in which the stop was made. Table 5.1 shows that, without controlling for other covariates, stops of African Americans were significantly more likely to include handcuffing of the person stopped than were stops of Whites across all areas of Oakland.

Table 5.1. Likelihood of being handcuffed contingent on being stopped, excluding arrests, broken down by area and race, without covariates

Handcuffing: No Covariates								
	White	Afr Am	Af/W	Asian	As/W	Hispan	His/W	Other
Area 1	.05	.18	* * *	.03	+	.05		.02
Area 2	.03	.13	* * *	.02		.06	+	.04
Area 3	.09	.17	* * *	.12		.11		.08
Area 4	.08	.21	* * *	.06		.12	+	.10
Area 5	.13	.29	* * *	.08		.19	*	.17

Note: + $p < .10$ / * $p < .05$ / ** $p < .01$ / *** $p < .001$

Table 5.2. Raw frequency of handcuffing, excluding stops that resulted in an arrest, by area and race

Handcuffing						
		White	Afr Am	Asian	Hispanic	Other
Area 1	Handcuffed	63	696	15	36	6
	Not Handcuffed	1,170	3,117	481	726	238
Area 2	Handcuffed	37	236	7	15	7
	Not Handcuffed	1,137	1,623	309	225	150
Area 3	Handcuffed	40	319	72	89	13
	Not Handcuffed	399	1,547	522	688	144
Area 4	Handcuffed	24	445	11	155	12
	Not Handcuffed	283	1,682	171	1,172	109
Area 5	Handcuffed	29	1,194	7	236	22
	Not Handcuffed	201	2,919	82	1,005	104
Overall	Handcuffed	193	2890	112	531	60
	Not Handcuffed	3,190	10,888	1,565	3,846	745

Stops of African Americans were two to four times as likely to involve handcuffing as stops of Whites. In the area with the least handcuffing, Area 2, stops of Whites involved handcuffing 3% of the time compared to 13% of the time for stops of African Americans. Similarly, in the area with the most handcuffing, Area 5, stops of African Americans were more than twice as likely to involve handcuffing as stops of Whites (29% compared to 13%, respectively). Put another way, in Area 5, OPD officers handcuffed the community member in more than 1 out of every 4 stops of African Americans—and these data exclude stops that resulted in an arrest, so this difference cannot be attributed to (potentially justified) differences in arrest rates.

Turning to the comparison between stops of Whites and stops of Hispanics, we see a statistically significant difference in the rate of handcuffing in 1 out of the 5 policing areas. In Area 5, 19% of stops of Hispanics involved handcuffing compared to 13% of stops of Whites. Among the remaining areas, in Areas 2 and 4, stops of Hispanics were marginally more likely ($p < .10$) to include handcuffing than stops of Whites. In Areas 1 and 3, however, there was no difference between the likelihood of handcuffing between Hispanic and White stops. We found no statistical difference between the likelihood that Whites and Asians or Whites and Others were handcuffed.

Controlling for the covariates (see Table 4.1 for a list of the included covariates), we see in Table 5.3 that the disparity in handcuffing rates between Whites and African Americans remains in 4 of 5 areas, but is no longer significant in Area 3. As we described in the methodology chapter, the likelihood of African American stops involving handcuffing in each of the 5 areas decreased when the benchmarks were taken into account. This is because factors pertaining to the characteristics of the encounter, the census tract in which the stop was made, and the officer who made the stop explain some of the variation in handcuffing.

Table 5.3. Likelihood of being handcuffed contingent on being stopped, excluding arrests, broken down by area and race, with covariates

Handcuffing: With All Covariates								
	White	Afr Am	Af/W	Asian	As/W	Hispan	His/W	Other
Area 1	.05	.12	* * *	.05		.04		.03
Area 2	.04	.11	* * *	.04		.05		.05
Area 3	.10	.13		.10		.08		.06
Area 4	.07	.16	* * *	.06		.09		.07
Area 5	.10	.17	* *	.05		.12		.10

Note: + p < .10 / * p < .05 / ** p < .01 / *** p < .001

We observe that controlling for these factors, stops of African Americans were still significantly more likely to involve handcuffing than stops of Whites in 4 out of the 5 areas. However, when controlling for the same factors, Hispanics were no more likely than Whites to be handcuffed in any of the 5 areas. In addition, note that the variation between handcuffing rates between areas largely disappears, as we would expect, once the underlying differences between them are taken into account.

The moderating role of type of encounter on handcuffing rates

Next, we examined whether the type of encounter (e.g., whether the person stopped was driving, walking, or riding a bicycle) influenced the race differences in the likelihood that police stops would involve handcuffing (see Table 5.4). For these analyses, we again excluded stops that resulted in arrests. Because of their small numbers, we also excluded stops of Asians and others. 6,581 stops, or 23% of the data set, were eliminated due to these exclusions. Furthermore, we do not present stops classified as "Other" in Table 5.4 (i.e., stops that were neither vehicle, pedestrian, nor bicycle stops) because of their small numbers (1.6% of the total data) and the fact that these stops are unusual.[128] See Table 5.2 for the raw frequency of handcuffing by area, race, and type of encounter.

<u>Vehicle stops</u>: Beginning with the most common type of encounter, vehicle stops, we found significant differences between stops of Whites and stops of African Americans in the likelihood of handcuffing in all 5 of Oakland's policing areas. The left panel of Table 5.4 shows that the raw disparity in handcuffing of African Americans compared to Whites ranged from more than twice as likely to more than 5 times as likely. Across the 5 areas, vehicle stops of Whites involved an officer using handcuffs in between 1% and 7% of the stops. Across the five areas, OPD officers handcuffed the community member in nearly 1 out of every 10 vehicle stops of African Americans, on the low end, and in nearly 1 out of every 4 vehicle stops of African Americans on the high end.

The raw numbers perhaps better illustrate the difference in handcuffing among vehicle stops (see Table 5.2). Switching our focus from the five policing areas of Oakland to the entire City for a moment, we observed that across all of Oakland during a 13-month time period, excluding stops that resulted in an arrest, 3% of White vehicle stops, or 72 vehicle stops of a White person involved handcuffing. In contrast, 16% of African American vehicle stops, or 1,466 stops of African Americans involved handcuffing—twenty times more than the number of Whites handcuffed.

When the likelihoods are adjusted to include the covariates, the race difference in handcuffing between vehicle stops of Whites and vehicle stops of African Americans remains statistically significant in all 5 of the areas. All else being equal, vehicle stops of African Americans are still 75%

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[128] See Chapter 1 for a description and examples of "Other" types of stops.

more likely to involve handcuffing in the area with the smallest disparity (Area 3) and 500% more likely to involve handcuffing in the area with the largest disparity (Area 2) than vehicle stops of Whites.

Comparing vehicle stops of Whites to vehicle stops of Hispanics, the raw likelihoods reveal a significant race gap in 2 of the 5 areas (Areas 4 and 5) and a marginal race gap in a third area (Area 2). The right panel of Table 5.4, however, illustrates that these differences disappear once the covariates are taken into account.

<u>Pedestrian stops</u>: In the left panel of Table 5.4, notice that the raw likelihoods of handcuffing among White pedestrian stops range from 16% of stops in Area 1 to 34% of stops in Area 5. For African American pedestrian stops, these likelihoods range from 22% in Area 2 to 41% in Area 5. Among Hispanic pedestrian stops, the likelihood of handcuffing ranged from 16% in Area 1 to 47% in Area 5. In 3 of the 5 areas, there was no significant difference in rates of handcuffing between White and African American pedestrian stops. However, in 2 of the 5 areas, Areas 1 and 4, the race difference between White and African American stops in handcuffing was statistically significant. In these two areas, pedestrian stops of African Americans involved handcuffing twice as often as pedestrian stops of Whites. In Area 4, the difference between White and Hispanic stops was statistically significant. Pedestrian stops of Hispanics in Area 4 involved handcuffing nearly twice as often as pedestrian stops of Whites (39% and 21%, respectively). When taking covariates into account, there was no significant difference in rates of handcuffing during White and Hispanic pedestrian stops.

Adjusting the likelihood of pedestrian handcuffing to take the covariates into account, we found that 2 of the 3 significant race gaps disappeared; meaning that in 4 of 5 areas, there was no significant difference between rates of handcuffing during White and African American pedestrian stops. Area 1 is notable in that there remains a significant difference in handcuffing between pedestrian stops of African Americans and Whites.

Widening our focus to the City of Oakland as a whole, there were large raw differences in the frequency of handcuffing by race among pedestrian stops in which no arrest was made. Excluding stops that resulted in an arrest, 20% of White pedestrian stops led to handcuffing, compared to 34% of similar African American stops. In raw numbers, this means that 99 White pedestrian stops involved handcuffing compared to 1,175 similar stops of African Americans.

<u>Bicycle stops</u>: The raw likelihoods of handcuffing among White bicycle stops ranged from 9% of stops in Area 1 to 29% of stops in Area 4. For African American bicycle stops, these likelihoods ranged from 14% in Area 2 to 34% in Area 5. Among Hispanic bicycle stops, the likelihood of handcuffing ranged from 8% in Area 2 to 17% in Area 5. In 4 of 5 areas, there was no significant difference between rates of handcuffing of African Americans compared to Whites during bicycle

stops. However, there was a significant race difference between the handcuffing rates among bicycle stops of Whites and stops of African Americans in Area 1 (9% and 24%, respectively). Adjusting the likelihood rates to take on the average or most likely values for each of the covariates did little to change the results and the race difference in Area 1 remained statistically significant, while there remained no significant difference in rates of handcuffing during bicycle stops in Areas 2 through 5.

Table 5.4. Likelihood of being handcuffed contingent on being stopped, excluding arrests, broken down by area, race, and type of encounter, without covariates (left panel), and controlling for covariates (right panel)

	Handcuffing: No Covariates					Handcuffing: With All Covariates				
	Vehicle Stops (15,820 / 73%)					Vehicle Stops (15,820 / 73%)				
	White	Afr Am	Afr/W	Hisp	His/W	White	Afr Am	Afr/W	Hisp	His/W
Area 1	.02	.11	***	.03		.04	.12	***	.03	
Area 2	.01	.09	***	.02	+	.02	.12	***	.03	
Area 3	.05	.14	***	.06		.08	.14	**	.06	
Area 4	.04	.16	***	.08	*	.05	.16	***	.08	
Area 5	.07	.23	***	.15	**	.08	.20	***	.12	
	Pedestrian Stops (4,462 / 21%)					Pedestrian Stops (4,462 / 21%)				
	White	Afr Am	Afr/W	Hisp	His/W	White	Afr Am	Afr/W	Hisp	His/W
Area 1	.16	.34	***	.16		.11	.16	*	.08	
Area 2	.17	.22		.28		.11	.13		.15	
Area 3	.27	.25		.33		.18	.14		.16	
Area 4	.21	.40	**	.39	*	.13	.18		.17	
Area 5	.34	.41		.47		.19	.16		.19	
	Bicycle Stops (906 / 4%)					Bicycle Stops (906 / 4%)				
	White	Afr Am	Afr/W	Hisp	His/W	White	Afr Am	Afr/W	Hisp	His/W
Area 1	.09	.24	*	.11		.06	.22	*	.06	
Area 2	.11	.14		.08		.15	.16		.09	
Area 3	.10	.17		.09		.12	.15		.06	
Area 4	.29	.19		.10		.36	.19		.09	
Area 5	.20	.34		.17		.20	.27		.10	

Note: + p < .10 / * p < .05 / ** p < .01 / *** p < .001

Table 5.5. Raw frequency of handcuffing, excluding arrests, by area, race, and type of stop

		White	Afr Am	Asian	Hispanic	Other
Vehicle Stop: Handcuffed						
Area 1	Handcuffed	24	261	8	19	4
	Not Handcuffed	959	2,189	445	637	217
Area 2	Handcuffed	8	102	3	5	2
	Not Handcuffed	980	1,097	295	220	138
Area 3	Handcuffed	18	193	20	39	7
	Not Handcuffed	331	1,177	442	580	124
Area 4	Handcuffed	9	268	4	93	11
	Not Handcuffed	226	1,370	162	1,047	98
Area 5	Handcuffed	13	642	4	157	8
	Not Handcuffed	163	2,112	69	881	92
Overall	Handcuffed	72	1,466	39	313	32
	Not Handcuffed	2,659	7,945	1,413	3,365	669
Pedestrian Stop: Handcuffed						
Area 1	Handcuffed	30	360	7	13	2
	Not Handcuffed	154	706	29	70	14
Area 2	Handcuffed	25	107	3	9	3
	Not Handcuffed	122	384	10	23	7
Area 3	Handcuffed	17	102	40	39	6
	Not Handcuffed	46	307	64	79	16
Area 4	Handcuffed	13	159	7	57	1
	Not Handcuffed	48	243	6	90	7
Area 5	Handcuffed	14	447	2	64	11
	Not Handcuffed	27	635	11	72	10
Overall	Handcuffed	99	1175	59	182	23
	Not Handcuffed	397	2,275	120	334	54

		White	Afr Am	Asian	Hispanic	Other
Bicycle Stop: Handcuffed						
Area 1	Handcuffed	5	56	0	2	0
	Not Handcuffed	48	182	2	17	3
Area 2	Handcuffed	4	20	0	1	0
	Not Handcuffed	31	127	3	11	2
Area 3	Handcuffed	2	8	1	2	0
	Not Handcuffed	18	39	7	20	1
Area 4	Handcuffed	2	12	0	3	0
	Not Handcuffed	5	51	2	26	2
Area 5	Handcuffed	1	59	0	6	0
	Not Handcuffed	4	115	2	29	0
Overall	Handcuffed	14	155	1	14	0
	Not Handcuffed	106	514	16	103	8
Other Stop: Handcuffed						
Area 1	Handcuffed	4	19	0	2	0
	Not Handcuffed	9	40	5	2	4
Area 2	Handcuffed	0	7	1	0	2
	Not Handcuffed	4	15	1	1	3
Area 3	Handcuffed	3	16	11	9	0
	Not Handcuffed	4	24	9	9	3
Area 4	Handcuffed	0	6	0	2	0
	Not Handcuffed	4	18	1	9	2
Area 5	Handcuffed	1	46	1	9	3
	Not Handcuffed	7	57	0	23	2
Overall	Handcuffed	8	94	13	22	5
	Not Handcuffed	28	154	16	44	14

The moderating role of reason for encounter on handcuffing rates

Do the race differences in the likelihood of being handcuffed vary as a function of the reason the stop was made? Recall from the overview of the stop data form that officers can select one of five reasons that they initially decided to make the stop: (1) traffic violation, (2) probable cause, (3) reasonable suspicion, (4) consensual encounter, and (5) probation/parole. Because of the small numbers of consensual encounters (951 stops or 4% of the data set) and probation/parole stops (314 stops or 1% of the data set), we are not showing those results here. These small frequencies are likely to yield cell counts of zero once we break down the likelihoods by area, race, and reason, which can cause less reliable estimates. Also, officers are not ordinarily required to complete a stop data form/field interview report for consensual encounters, unless those encounters lead to a search or a detention. Therefore, the consensual encounters that are recorded in our data set are qualitatively different from the larger body of consensual stops that members of the OPD routinely make (and which we have no data about). As in the previous analysis of handcuffing, here we also excluded stops that ended in an arrest and stops of Asians and Others (excludes 6,581 stops or 23% of the data). In total, this analysis included 20,273 stops (see Table 5.6).

Traffic violation: We begin with the most common reason for making a stop. Among White stops made because of traffic violations, the raw likelihoods of being handcuffed ranged from 1% in Area 2 to 5% in Area 5. These numbers are in line with what one might expect given that these were stops made primarily for equipment violations, such as broken taillights, and moving violations, like speeding and failure to signal before making a turn or changing lanes. For African American stops made because of traffic violations, these raw likelihoods range from 7% in Area 2 to 19% in Area 5. These two sets of raw likelihoods are non-overlapping. The *lowest* percentage of traffic violation stops of African Americans that involved handcuffing is 9% (Area 1), four percentage points above the *highest* percentage of traffic violation stops of Whites that involved handcuffing, which is 5% (Area 5). Examining the degree of this race difference, we observe that, compared to stops of Whites for traffic violations, stops of African Americans for traffic violations involved handcuffing between 3 and 7 times more often. In Area 1, 3% of stops of Whites involved handcuffing compared to 9% of stops of African Americans. In Area 5, 5% of stops of Whites involved handcuffing compared to 19% of stops of African Americans. Across all 5 areas, the difference between the likelihood of an African American community member being handcuffed compared to a White community member being handcuffed is statistically significant, with African Americans being significantly more likely to be handcuffed during a traffic violation stop.

When we examine the raw frequencies of handcuffing among stops made for traffic violations (see Table 5.7), we are able to understand the magnitude of these race differences. In Area 5, the area with the greatest likelihood of handcuffing for both Whites and African Americans, a White person was handcuffed on 9 occasions, compared to 484 cases of an African American being handcuffed. Overall, across the entire City of Oakland, there were 64 instances of a White person stopped for a traffic violation being handcuffed when no arrest was made. For similar stops of African Americans, 1,162 stops resulted in handcuffing, or 18 times more than Whites.

When we add the covariates to the model, this pattern of racial differences in handcuffing remains significant. All else being equal, the rates at which stops of African Americans for traffic violations would involve handcuffing remains between 2 and 6 times the rates of White stops.

Comparing stops made of Hispanics and of Whites for traffic violations, there is a significant difference in Area 5 (5% of White stops involved handcuffing compared to 11% of Hispanic stops), which is no longer significant once the covariates are controlled for.

Probable cause: Overall, the likelihood tables reveal that stops made because of probable cause tend to have higher rates of handcuffing. To illustrate this point and to give a sense of the overall frequency of handcuffing, in the total data set, not controlling for anything and for a moment not dropping arrests, stops made because of traffic violations involved handcuffing 12% of the time (9% dropping arrests), whereas stops made because of probable cause involved handcuffing 60% of the time (34% dropping arrests). Again, recall that the present analysis excludes stops that ended in an arrest. Therefore, it is not the case that stops made because of probable cause were simply more likely to lead to an arrest and that fact alone is driving these higher rates of handcuffing.

Among White stops for probable cause, the raw likelihoods of being handcuffed ranged from 11% in Area 2 to 31% in Area 3. Among African American stops for probable cause, the likelihood of being handcuffed ranged from 19% on the low end to 51% on the high end. Among African American stops, the likelihood of being handcuffed ranged from 17% to 43%. In Areas 1 and 2, but not Areas 3 to 5, there was a significant difference between the likelihood that White and African American probable cause stops involved handcuffing. In contrast, we found no statistically significant differences between the likelihood that Hispanic stops for probable cause involved handcuffing compared to White stops.

Turning to the raw frequencies (see Table 5.7), overall across all of Oakland there were 75 instances of White probable cause stops that involved handcuffing when no arrest was made, compared to 896 African American stops that involved handcuffing, or 12 times more than Whites. Next, we examine the raw frequencies in Areas 1 and 2, the areas where we observed significant race differences in the raw likelihoods of being handcuffed. In Area 1, there were 24 instances in which a White person stopped for probable cause was handcuffed compared to 252 instances for African Americans, or 10 times more than Whites. In Area 2, the frequencies were 17 instances of handcuffing for Whites, and 72 instances of handcuffing for African Americans.

When the model is adjusted to account for our covariates, the difference between handcuffing in White and African American probable cause stops remains statistically significant in 1 of 5 areas. In Area 1, when all else is presumed to be equal, Whites were expected to be handcuffed 25% of the time compared to 41% of the time for African Americans. When controlling for covariates in the remaining areas, Areas 2 through 5, there is no significant difference in handcuffing between White and African American probable cause stops.

Reasonable suspicion: In the raw data, we see that in 3 of the areas African American stops involved handcuffing about twice as often as White stops. Overall, there was a significant difference in handcuffing between White and African American reasonable suspicion stops in 2 areas, Areas 2 and 4. Comparing raw White and Hispanic reasonable suspicion stops, there are significant differences in two areas, Areas 4 and 5. In Area 4, the OPD handcuffed a Hispanic person stopped for reasonable suspicion in 45% of stops compared to in 17% of similar White stops. In Area 5, the OPD handcuffed a Hispanic person in 65% of stops for reasonable suspicion compared to in 40% of similar White stops. When the model is adjusted to take covariates into account, however, all observed race differences become statistically nonsignificant, suggesting that one or more of our covariates, and not race, was driving the rate of handcuffing among reasonable suspicion stops.

Table 5.6. Likelihood of being handcuffed contingent on being stopped, excluding arrests, broken down by area, race, and reason for encounter, without covariates (left panel), and controlling for covariates (right panel)

Handcuffing: No Covariates						**Handcuffing: With All Covariates**					
Traffic Violation (15,312 / 71%)						**Traffic Violation (15,312 / 71%)**					
	White	Afr Am	Afr/W	Hisp	His/W		White	Afr Am	Afr/W	Hisp	His/W
Area 1	.03	.09	***	.02		Area 1	.05	.12	***	.03	
Area 2	.01	.07	***	.01		Area 2	.02	.12	***	.02	
Area 3	.03	.11	***	.05		Area 3	.07	.15	**	.07	
Area 4	.03	.14	***	.06	+	Area 4	.04	.17	***	.08	
Area 5	.05	.19	***	.11	*	Area 5	.07	.21	***	.10	
Probable Cause (3,188 / 15%)						**Probable Cause (3,188 / 15%)**					
	White	Afr Am	Afr/W	Hisp	His/W		White	Afr Am	Afr/W	Hisp	His/W
Area 1	.19	.36	***	.17		Area 1	.25	.41	**	.21	
Area 2	.11	.19	*	.19		Area 2	.22	.29		.31	
Area 3	.31	.32		.29		Area 3	.42	.39		.33	
Area 4	.30	.43		.39		Area 4	.42	.46		.40	
Area 5	.30	.51	+	.43		Area 5	.39	.49		.35	
Reasonable Suspicion (1,773 / 8%)						**Reasonable Suspicion (1,773 / 8%)**					
	White	Afr Am	Afr/W	Hisp	His/W		White	Afr Am	Afr/W	Hisp	His/W
Area 1	.19	.37	+	.30		Area 1	.20	.38		.29	
Area 2	.22	.42	*	.27		Area 2	.29	.47		.37	
Area 3	.33	.29		.27		Area 3	.46	.36		.29	
Area 4	.17	.42	*	.45	*	Area 4	.26	.41		.42	
Area 5	.40	.46		.65	*	Area 5	.40	.41		.59	

Note: + p < .10 / * p < .05 / ** p < .01 / *** p < .001

Table 5.7. Handcuffing frequency, excluding arrests, by area, race, and reason for encounter

		White	Afr Am	Asian	Hispanic	Other
Traffic Violation Stops: Handcuffed						
Area 1	Handcuffed	29	236	5	16	3
	Not Handcuffed	1,021	2,309	432	638	218
Area 2	Handcuffed	9	89	2	2	2
	Not Handcuffed	946	1,161	283	204	126
Area 3	Handcuffed	11	134	16	28	4
	Not Handcuffed	322	1,069	402	533	111
Area 4	Handcuffed	6	219	5	65	5
	Not Handcuffed	215	1,354	156	1,028	95
Area 5	Handcuffed	9	484	2	106	4
	Not Handcuffed	156	2,048	68	865	83
Overall	Handcuffed	64	1,162	30	217	18
	Not Handcuffed	2,660	7,941	1,341	3,268	633
Probable Cause Stops: Handcuffed						
Area 1	Handcuffed	24	252	3	12	3
	Not Handcuffed	104	440	32	58	11
Area 2	Handcuffed	17	72	4	8	4
	Not Handcuffed	143	311	22	34	20
Area 3	Handcuffed	17	93	27	35	7
	Not Handcuffed	37	195	62	86	16
Area 4	Handcuffed	10	110	3	44	5
	Not Handcuffed	23	143	7	68	5
Area 5	Handcuffed	7	369	4	45	7
	Not Handcuffed	16	356	5	59	7
Overall	Handcuffed	75	896	41	144	26
	Not Handcuffed	323	1,445	128	305	59

Reasonable Suspicion Stops: Handcuffed						
		White	Afr Am	Asian	Hispanic	Other
Area 1	Handcuffed	5	123	5	6	0
	Not Handcuffed	21	207	3	14	1
Area 2	Handcuffed	8	58	1	4	0
	Not Handcuffed	29	80	2	11	4
Area 3	Handcuffed	10	65	21	16	1
	Not Handcuffed	20	161	40	44	8
Area 4	Handcuffed	5	77	2	33	2
	Not Handcuffed	24	108	3	40	5
Area 5	Handcuffed	8	231	0	54	9
	Not Handcuffed	12	270	6	29	7
Overall	Handcuffed	36	554	29	113	12
	Not Handcuffed	106	826	54	138	25

The moderating role of special assignment on handcuffing rates

In this section, we explore whether the fact that some officers were on special assignment at the time of the stop influenced the overall pattern of racial disparities in the likelihood of handcuffing between African Americans and Whites that we found (see Table 5.8).[129] Here, we compare stops made by officers who were not on special assignment at the time of the stop, stops made by officers who were working violence suppression at the time of the stop, and stops made by officers on other assignments (which include "Other," "Prostitution," "Narcotics," "Cruising," and "Special Event," in order of decreasing frequency in the total data set). As in the previous analyses of handcuffing, we exclude stops that ended in an arrest and stops of Asians and Others (excludes 6,581 stops or 23% of the data).

No special assignment: We begin with the majority of stops, in which the officer was not on any type of special assignment at the time the stop was made (54% of stops). Among these stops, we found that, in raw numbers, stops of African Americans made by officers not on special assignment involved handcuffing between 1.7 and 3.7 times as often as similar stops of Whites. Among White non-special-assignment stops, officers handcuffed the person in between 6% and 14% of stops. For African American non-special-assignment stops, the likelihood of handcuffing ranged from 19% to 30% of stops. OPD officers not on special assignment handcuffed 1 out of every 5 African Americans stopped in Areas 2 and 3 and nearly 1 out of every 3 African Americans in Area 5. Furthermore, the gap between the handcuffing rates of African Americans and Whites was statistically significant

[129] See Chapter 1 of this report for a description of what it means to work special assignment and how this information is captured on the stop data form.

across all five of Oakland's policing areas. The difference in handcuffing between African Americans and Whites remained statistically significant in 4 of the 5 areas (Areas 1, 2, 4, and 5) once the model was adjusted to take the covariates into account.

Widening our scope and examining the raw frequencies across all of Oakland (see Table 5.9), we observed that 120 non-special-assignment stops of Whites involved handcuffing compared to 1,938 stops of African Americans, or 16 times more than Whites. In Area 5, which had the most handcuffing, there were 20 instances of a White person stopped by a non-special-assignment officer compared to 786 instances of African Americans being handcuffed, or 39 times more than Whites.

In contrast, we found no significant Hispanic-White difference in handcuffing rates in the raw data. Once the covariates were taken into account, there remains no significant difference in 4 of 5 areas. However, in Area 3, Hispanics would actually be significantly less likely to be handcuffed by non-special-assignment officers than Whites.

Violence suppression: Next, we considered stops made by officers who were working violence suppression at the time of the stop (26% of stops). African Americans were handcuffed between 3 and 4 times as often as Whites in Areas 1, 2, 3 and 5. In Area 1, for example, officers working violence suppression used handcuffs in 3% of White stops compared to 12% of African American stops. In these 4 areas, the difference in handcuffing rates between Whites and African Americans is statistically significant. In Area 4, not a single White person stopped by an officer working violence suppression was handcuffed. In contrast, 20% of similar African American stops resulted in handcuffing. While the raw difference in Area 4 is striking (0 stops of Whites and 89 stops of African Americans), it fails to meet the threshold for statistical significance, in part, because the absence of any Whites who were handcuffed introduces noise and causes the model to create less statistically reliable estimates, making it harder to find conclusive results for that cell.[130] Once we control for the covariates, there is a significant difference between African American and White rates of handcuffing in Areas 1 and 2, but not in Areas 3, 4, or 5.

Across all of Oakland, 31 total stops of a White person made by an officer working violence suppression involved handcuffing when no arrest was made, compared to 585 cases of African Americans that involved handcuffing, or 19 times more than Whites. Let's examine the raw frequencies in Areas 1 and 2, the areas in which the race difference in handcuffing persisted after

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[130] By virtue of its underlying mathematical structure, logistic regression places a lot of uncertainty on proportion differences when one of the observed proportions is 0%, so the lack of significance here for this relatively large proportion difference is to be expected and should be interpreted with caution. Conversely, cases such as this one, when one group shows 0% and the other shows a sizeable proportion (20%), may speak for themselves and be less in need of statistical testing than subtler quantitative differences.

controlling for covariates. In Area 1, an officer working violence suppression handcuffed a White person on 15 occasions, compared to 121 occasions for African Americans, or 8 times more than Whites. In Area 2, 10 stops of Whites involved handcuffing compared to 56 stops of African Americans, or more than 5 times as often.

Turning to the comparison between stops of Hispanics and Whites made by officers working violence suppression, we observe no statistically significant differences either in the raw data or once we have taken into account our covariates.

Other assignment: Finally, we examine the handcuffing rates among stops made by officers who were working other types of special assignment at the time of the stop. These other assignments include "Other" (89% of this category among non-arrest stops), "Prostitution" (1%), "Narcotics" (4%), "Cruising" (4%), and "Special Event" (2%).

Looking at the raw likelihoods, stops of Whites involved handcuffing between 1% of the time in Area 2 on the low end and 10% of the time in Area 5 on the high end. By contrast, stops of African Americans involved an officer using handcuffs between 9% of the time and 22% of the time. Overall, African Americans were between 1.4 and 9 times more likely to be handcuffed by officers working other types of special assignment than were Whites stopped in the same policing area. This difference is statistically significant in all areas except in Area 4. When taking the covariates into account, and holding all else constant, the racial disparity between African Americans and Whites remains significant in Areas 1 and 2. It is worth reviewing the list of covariates so that the reader is reminded of how many factors we controlled for. The consistent pattern of African Americans being handcuffed by the OPD more often than Whites cannot be explained by the violent, property, or narcotics crime rates of the area or the underlying demographic characteristics of the neighborhood in which the stop was made. It is also not the case that these handcuffing rates were driven by other characteristics of the stops, such as the person stopped being more likely to be a man, compared to a woman, or these stops being made more often at night for African Americans or during the day for Whites. And again, it is worth repeating, none of these stops led to an arrest.

Examining the raw frequencies in Table 5.9 reveals that overall, across Oakland, officers working other types of special assignment handcuffed the person stopped in 42 cases for White stops, and 369 cases for African Americans, or 9 times more than Whites. In Area 1, one of the areas where the disparities persist when controlling for covariates, 20 stops of Whites, compared to 101 stops of African Americans, involved handcuffing. In Area 2, the other area with a significant difference after controlling for covariates, these figures were 3 and 31.

The same kind of stops of Hispanics involved handcuffing at similar rates as Whites, which ranged between 4% of the time and 12% of the time. We found no significant differences between Whites and Hispanics in handcuffing by officers working other types of special assignment.

Table 5.8. Likelihood of being handcuffed contingent on being stopped, excluding arrests, broken down by area, race, and special assignment, without covariates (left panel), and controlling for covariates (right panel)

Handcuffing: No Covariates						Handcuffing: With All Covariates					
No Special Assignment (11,567 / 54%)						**No Special Assignment (11,567 / 54%)**					
	White	Afr Am	Afr/W	Hisp	His/W		White	Afr Am	Afr/W	Hisp	His/W
Area 1	.06	.22	* * *	.05		Area 1	.05	.11	* * *	.03	
Area 2	.06	.19	* * *	.12	+	Area 2	.06	.12	* *	.10	
Area 3	.12	.20	*	.13		Area 3	.13	.14		.08	*
Area 4	.10	.24	* * *	.15		Area 4	.08	.16	* *	.09	
Area 5	.14	.30	* * *	.21		Area 5	.11	.17	*	.12	
Violence Suppression (5,600 / 26%)						**Violence Suppression (5,600 / 26%)**					
	White	Afr Am	Afr/W	Hisp	His/W		White	Afr Am	Afr/W	Hisp	His/W
Area 1	.03	.12	* * *	.04		Area 1	.04	.10	* * *	.04	
Area 2	.02	.08	* * *	.02		Area 2	.03	.06	*	.02	
Area 3	.04	.15	*	.11	+	Area 3	.03	.10	+	.07	
Area 4	.00	.20		.09		Area 4	.00	.16		.08	
Area 5	.10	.31	*	.23		Area 5	.06	.18	+	.13	
Other Assignment (4,371 / 20%)						**Other Assignment (4,371 / 20%)**					
	White	Afr Am	Afr/W	Hisp	His/W		White	Afr Am	Afr/W	Hisp	His/W
Area 1	.07	.16	* * *	.05		Area 1	.06	.11	*	.04	
Area 2	.01	.09	* * *	.04	+	Area 2	.01	.07	* * *	.03	
Area 3	.06	.13	*	.10		Area 3	.07	.10		.07	
Area 4	.08	.11		.07		Area 4	.07	.09		.06	
Area 5	.10	.22	*	.12		Area 5	.08	.13		.07	

Note: + $p < .10$ / * $p < .05$ / ** $p < .01$ / *** $p < .001$

Table 5.9. Frequency of handcuffing, excluding arrests by area, race, and special assignment

		White	Afr Am	Asian	Hispanic	Other
No Special Assignment: Handcuffed						
Area 1	Handcuffed	28	474	5	18	4
	Not Handcuffed	418	1,672	204	351	86
Area 2	Handcuffed	24	149	4	10	6
	Not Handcuffed	358	631	93	75	42
Area 3	Handcuffed	30	209	39	49	9
	Not Handcuffed	211	859	261	337	70
Area 4	Handcuffed	18	320	9	104	11
	Not Handcuffed	155	1,027	73	595	64
Area 5	Handcuffed	20	786	4	148	13
	Not Handcuffed	118	1,801	52	572	64
Overall	Handcuffed	120	1,938	61	329	43
	Not Handcuffed	1,260	5,990	683	1,930	326
Violence Suppression: Handcuffed						
Area 1	Handcuffed	15	121	3	8	1
	Not Handcuffed	480	925	169	197	106
Area 2	Handcuffed	10	56	1	2	0
	Not Handcuffed	473	670	152	109	64
Area 3	Handcuffed	3	57	12	18	2
	Not Handcuffed	73	333	120	152	39
Area 4	Handcuffed	0	89	2	27	0
	Not Handcuffed	59	349	47	265	21
Area 5	Handcuffed	3	260	2	52	4
	Not Handcuffed	26	592	11	176	14
Overall	Handcuffed	31	583	20	107	7
	Not Handcuffed	1,111	2,869	499	899	244

Other Assignment: Handcuffed						
		White	Afr Am	Asian	Hispanic	Other
Area 1	Handcuffed	20	101	7	10	1
	Not Handcuffed	272	520	108	178	46
Area 2	Handcuffed	3	31	2	3	1
	Not Handcuffed	306	322	64	71	44
Area 3	Handcuffed	7	53	21	22	2
	Not Handcuffed	115	355	141	199	35
Area 4	Handcuffed	6	36	0	24	1
	Not Handcuffed	69	306	51	312	24
Area 5	Handcuffed	6	148	1	36	5
	Not Handcuffed	57	526	19	257	26
Overall	Handcuffed	42	369	31	95	10
	Not Handcuffed	819	2,029	383	1,017	175

Chapter 6 | ANALYSIS OF SEARCHES AND RECOVERY

Core Findings	
	• ***Excluding incident to arrest, inventory, and probation/parole searches, Black men were searched in 1 out of 5 stops, vs. 1 out of 20 stops for White men.*** • ***Even after controlling for a host of factors, including the crime rate and the racial demographics of the neighborhood where the stop was made, African Americans were still significantly more likely than Whites to be the subject of such high-discretion searches in 3 of Oakland's 5 policing areas.*** • ***The African American-White race difference was especially pronounced for vehicle stops, stops made because of traffic violations, and stops made by officers working special assignments, other than violence suppression.*** • ***We found no race differences in search recovery rates.***

In this chapter, we explore whether stops were more likely to include a search as a function of the race of the person stopped. We further explore whether or not any contraband was recovered and if the likelihood of recovery was associated with race. Search and search recovery rates are of great interest to members of the public and the OPD alike. The OPD conducted a total of 8,975 searches out of the 28,119 stops that were made between April 1, 2013, and April 30, 2014, an effective overall search rate of 32%. Recall from the stop data form overview chapter that a search can be made of a person and/or of property. OPD officers can conduct 1 of 6 distinct kinds of searches (see Chapter 1 for full definitions and policies related to each search type).

Looking at the raw numbers, Whites were searched in 13.7% of all stops, compared to 40.2% for African Americans and 26.0% for Hispanics. When we drop searches in which the officer had less discretion and the search was more or less automatic, Whites were searched in 4.6% of stops, compared to 17.6% for African Americans and 12.7% for Hispanics. Shortly, we will discuss in detail why we chose to exclude these low-discretion searches. Just looking at males (and continuing to drop low-discretion searches), White males were searched in 5.5% of stops, compared to 20.4% for African American males and 14.4% for Hispanic males. The OPD searched 1 in 5 Black men—even though we eliminated more obvious reasons for conducting a search, such as an arrest being made—compared to only 1 in 20 White men.

To briefly review, the 6 kinds of searches[131] are:

- **Consent Search** – An officer can search a person after he or she has consented to be searched.
- **Probable Cause (PC) Search** – An officer may conduct a search of a vehicle or of a person if there is probable cause to believe there is contraband or evidence of a crime.
- **Probation/Parole Search** – An officer may search a person who is confirmed as being on probation or parole.
- **Incident to Arrest Search** – When an arrest is made and the arrested person will be transported, an officer is authorized to conduct a search to locate any weapons and prevent the person from concealing or destroying evidence.
- **Inventory Search** – When a vehicle is towed, officers can search the vehicle to take inventory of the car and its contents.
- **Weapons Search** – An officer may conduct a cursory weapons search if the officer has reason to fear for his or her safety.

We were most interested in cases in which the officer could exercise some degree of discretion in opting whether or not to conduct a search. Past literature has shown that having discretion tends to bring out differences in treatment as a function of race.[132] In certain circumstances, namely when an arrest is made or when a car is towed, searches are essentially mandatory.[133] Therefore, the decision about whether or not to conduct a search is largely taken out of the officer's hands and is instead dictated by the situation. Indeed, both Analysis Group (2006) and Ayres and Borowsky (2008) chose to focus exclusively on higher-discretion searches in their analyses of LAPD stop data.[134] Internal reports produced by the OPD similarly exclude incident to arrest searches when calculating search rates (i.e., whether or not a stop involved a search) and search recovery rates (i.e., whether or not a search uncovered any contraband). Following these standards, we excluded all incident to arrest

[131] For more detailed information about these search types, please refer to Chapter 1 of this report.

[132] Alexander, M. (2010). *The new Jim Crow: Mass incarceration in the age of colorblindness.* New York: The New Press; Banks, R. R. (2003). Beyond profiling: Race, policing, and the drug war. *Stanford Law Review*, 571-603; Cole, D. (1999). *No equal justice: Race and class in the American criminal justice system.* New York: The New Press; Glaser, J. (2015). *Suspect race: Causes and consequences of racial profiling.* New York: Oxford University Press; Sommers, S. R., & Norton, M. I. (2008). Race and jury selection: Psychological perspectives on the peremptory challenge debate. *American Psychologist, 63*(6), 527-539.

[133] Oakland Police Departmental Training Bulletin I-O.02, *The legal aspects of searching persons* (revised April 2, 2013); Oakland Police Department. (Sept. 25, 1998). *Training bulletin I-O.1: Vehicle searches*, p. 7.

[134] Analysis Group, Inc. (2006), Pedestrian and motor vehicle post-stop data analysis report, prepared for City of Los Angeles; Ayres and Borowsky (2008).

(3,160 searches or 35% of all searches) as well as inventory searches (120 searches or 1.3% of searches) from our analyses.

An argument sometimes presented to justify a discrepancy in search rates between Whites and African Americans is that African Americans are more likely to be on probation or parole. Indeed, when a person is confirmed to be on probation or parole and has a relevant search clause, officers automatically have the authority to conduct a search, granted that "the motive for the search is rehabilitative in nature and that the search is of reasonable scope and intensity and the decision to search is not arbitrary, capricious, or of a harassing nature" (see Chapter 1 for the full policy regarding probation/parole searches). Because the law is clear and all parties involved understand that officers can conduct the search, we did not feel there was much room for individual officers to weigh in and truly make the decision about whether or not to conduct a probation/parole search. From our perspective, the question of whether or not an officer has the legal authority and *can* conduct a search is distinct from whether or not an officer *should* exercise that authority and actually conduct the search.

Another potentially thorny issue concerning probation/parole revolves around the completeness of the data on which we are relying. Note that we do not know what proportion of residents of Oakland or people who routinely travel in and around Oakland are on probation/parole. This would require probation and criminal records from Alameda County to which we did not have access. Nor do we know if an individual stopped is on probation/parole unless that is recorded as the reason for the stop or the reason for the search. It is possible that officers have consensual encounters with people on probation/parole, which do not require the completion of a stop data form. To make it into our data set—by having a stop data form accompany the encounter—the person must be detained, arrested, and/or subjected to a search or request to search. Therefore, it is possible that in the majority of cases the only way the research team would know whether or not someone stopped was on probation/parole is because they were searched. Thus our sample of probationers/parolees would be skewed from the outset.[135]

Therefore, to rule out the possibility that any racial differences in search rates result from the fact that more African Americans are found to be on probation/parole, we also exclude from our data set for this analysis all stops that were prompted by the fact that the community member was

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[135] Note that the OPD Report Writing Manual stipulates that officers are to select "Consensual Encounter" as the reason for the stop if it begins as consensual and is later elevated to a detention, request to search or search conducted, or arrest. Therefore, if a consensual encounter is elevated to a detention because that person is determined to be on probation or parole, the original reason for the stop will be listed as "Consensual Encounter." Because of this policy, we may be undercounting the number of stops that involve a person who is on probation or parole to the extent that they might be included in other categories in our data set. In addition, remember that we do not have data on the total number of people in Oakland who were on probation or parole during the stop data period of analysis to use as a benchmark.

known to be on probation/parole, as well as all stops that involved a probation/parole search. In summary, we excluded stops triggered by probation/parole as the reason for the stop, as well as stops involving a search for probation/parole, incident to arrest, or inventory.

The remaining three types of searches, consent searches, weapons searches, and probable cause searches, fit our definition of discretionary searches. Consent searches, almost by definition, are subjective to the extent that there is not a legal mandate for compelling the search and the subject is free to say no. In practice, whether the subject understands his or her right to refuse is another issue altogether. Weapons searches are cursory (limited in scope) searches that are conducted out of the officer's concern about his or her own safety. Whether or not an officer feels in danger certainly has some subjective dimension to it. Remember that the suspected weapon in question may not always be a firearm, but a screwdriver, bat, or a razor blade. Finally, different officers may have different thresholds for what constitutes probable cause. Overall, between the exclusions of incident to arrest searches, inventory searches, and probation/parole searches, plus stops made because the person was on probation/parole, we excluded 6,071 stops or 22% of the total data.

As we did in the last chapter, we present the search results in the form of two likelihood tables. The first likelihood table shows the raw likelihoods of being searched. In the second likelihood table, we show the likelihoods of being searched by race and area after they have been adjusted to control for our covariates. In other words, we are trying to establish what the likelihood of being subjected to a search would be by race and area if all else were equal. Because searches, of course, have their own outcome, namely whether or not any contraband was recovered, after we have presented all of our analyses on search rate, we present the results of analyses predicting search recovery rates by race.

Are there racial disparities in high-discretion search rates?

Table 6.1. Likelihood of being searched contingent on being stopped, excluding incident to arrest, inventory, and probation/parole stops and searches, broken down by area and race, without covariates

High-Discretion Search: No Covariates								
	White	Afr Am	Af/W	Asian	As/W	Hispan	His/W	Other
Area 1	.03	.12	* * *	.03		.04	*	.02
Area 2	.02	.10	* * *	.02		.06	* *	.03
Area 3	.09	.17	* * *	.15	* *	.13	*	.06
Area 4	.09	.17	* *	.02	* *	.12		.08
Area 5	.13	.27	* * *	.08		.20	*	.18

Note: + p < .10 / * p < .05 / ** p < .01 / *** p < .001

Table 6.2. Raw frequency of searches, excluding incident to arrest, inventory, and probation/parole stops and searches, by race and area

High-Discretion Searches						
		White	**Afr Am**	**Asian**	**Hispanic**	**Other**
Area 1	Searched	32	398	13	33	4
	Not Searched	1,171	2,911	473	709	235
Area 2	Searched	28	168	6	15	5
	Not Searched	1,134	1,544	311	248	149
Area 3	Searched	38	293	86	97	9
	Not Searched	387	1,427	503	646	143
Area 4	Searched	27	312	4	151	9
	Not Searched	269	1,575	175	1,100	108
Area 5	Searched	29	961	7	232	22
	Not Searched	191	2,547	80	934	99
Overall	Searched	154	2,132	116	528	49
	Not Searched	3,152	10,004	1,542	3,637	734

Examining the raw search likelihoods presented in Table 6.1, we see that Whites were searched between 2% of the time on the low end in Area 2 and 13% of the time on the high end in Area 5. These rates jump for African Americans who were searched in between 10% and 27% of stops. The rate of searching for Asians resembled the White rates and ranged from 2% to 15% of stops. The rates for Others were similar to the rates for Whites and Asians and ranged from between 2% and 18% of stops (though the numbers are smaller and thus subject to fluctuate more wildly). The search rates for Hispanics were generally higher than for Whites, but lower than they were for African Americans and varied from 4% to 20% of stops included in the analysis.

Across all 5 Oakland policing areas, officers were statistically more likely to conduct a high-discretion search during a stop of an African American person than during a stop of a White person (see Table 6.2 for raw frequencies of high-discretion searches). African American searches were between nearly 2 and 5 times more likely than White searches across Oakland. Remember that this analysis is restricted to high-discretion searches. None of these searches was triggered by an arrest or by the subject of the stop being on probation or parole. The argument, then, that the higher search rate of African Americans compared to Whites is being driven by different arrest or probation/parole rates cannot explain these differences. In Area 1, for example, Whites were searched in 3% of White stops, while African Americans were searched in 12% of African American stops.

Similarly, we found that officers were statistically more likely to conduct a high-discretion search during Hispanic stops than during White stops in 4 of the 5 areas. The results were mixed for Asians, who were subjected to a high-discretion search more often than were Whites in Area 3, but less often in Area 4. In Area 4, stops of Asians involved a high-discretion search less than one-quarter as often as stops of Whites.

Table 6.3. Likelihood of being searched contingent on being stopped, excluding incident to arrest, inventory, and probation/parole stops and searches, broken down by area and race, with covariates

High-Discretion Search: With All Covariates								
	White	Afr Am	Afr/W	Asian	As/W	Hispanic	His/W	Other
Area 1	.04	.10	* * *	.06		.05		.03
Area 2	.04	.09	* * *	.03		.06		.04
Area 3	.11	.13		.13		.11		.04
Area 4	.09	.11		.02	* *	.09		.05
Area 5	.09	.15	*	.04	+	.12		.09

Note: + p < .10 / * p < .05 / ** p < .01 / *** p < .001

When we control for covariates (see Table 6.3), we find that the pattern of officers conducting more high-discretion searches during stops of African Americans than during stops of Whites remains statistically significant in 3 of the 5 areas.

Finally, the Hispanic-White gap in the rate of high-discretion searching statistically disappears once the covariates are added to the model. In Area 4, stops of Asians remain significantly less likely than stops of Whites to involve a high-discretion search.

The moderating role of type of encounter in search rates

In this section, we examine whether the type of encounter influenced the degree of race differences in the likelihood that OPD officers would conduct a high-discretion search during a stop. As in the last section, for these analyses, we again excluded low-discretion searches and stops made because of probation/parole. Because of their small numbers, we also excluded stops of Asians and Others (2,441 or 9% of the total data) because any estimates produced would be less reliable. Thus, 19,607 stops (or 70% of the total data set) were retained in these analyses. The percentages in Table 6.4 refer to this specific subset of the data. Also, although included in the models, we do not present the results for "Other" types of stops (e.g., those stops that were not vehicle, pedestrian, or bicycle stops; 1.5% of the subsetted data).

<u>Vehicle stops</u>: We begin with the most common type of encounter, vehicle stops. Among vehicle stops, we found significant differences between stops of Whites and stops of African Americans in the likelihood that a high-discretion search was conducted across all 5 of Oakland's policing areas. The left panel of Table 6.4 shows that the raw disparity in the likelihood of a high-discretion search being conducted during an African American stop compared to during a White stop ranged from twice as likely to 18 times as likely. Across all 5 areas, the raw African American-White disparity in high-discretion searches was statistically significant. For example, within Area 5, 6% of White vehicle stops involved a high-discretion search being conducted compared to 21% of African American vehicle stops, or more than 1 out of every 5.

Examining the raw frequencies presented in Table 6.5, we observe that overall across Oakland, the OPD conducted 46 high-discretion searches of Whites during vehicle stops, compared to 1,056 high-discretion searches of African Americans, or 23 times more than Whites. The area with the fewest number of African American searches made during a vehicle stop (Area 2, 80 searches) is more than the total number of White searches conducted during vehicle stops across the entire City (46 searches).

The disparities within each area are large. In Area 1, 7 White discretionary searches were made during vehicle stops, compared to 143 similar African American searches, or 20 times more than Whites. In Area 2, the number of times an OPD officer conducted a high-discretion search of a White person during a vehicle stop during the 13-month period can be counted on one hand (4 times). In contrast, the raw count for similar African Americans searches in Area 2 is 80. In Area 3, the OPD conducted 11 White searches compared to 152 African American searches, or 14 times more than Whites. In Area 4, the OPD conducted 14 high-discretion searches of Whites pulled over in a vehicle, compared to 181 high-discretion searches of African Americans made during vehicle stops, or 13 times more than Whites. Finally, in Area 5, officers conducted 10 White high-discretion searches during vehicle stops compared to 500 similar African American searches, or 50 times more than Whites.

When the likelihoods of a high-discretion search being conducted are adjusted to include the covariates, the race difference between searches during vehicle stops of Whites and vehicle stops of African Americans remains statistically significant in 4 of the 5 areas. Holding our covariates constant, vehicle stops of African Americans, compared to vehicle stops of Whites, would still be twice as likely to involve a high-discretion search in the areas with the smallest absolute (and statistically significant) disparity (Areas 3 and 5) and 11 times more likely to involve a high-discretion search in the area with the largest disparity (Area 2).

Comparing vehicle stops of Whites to vehicle stops of Hispanics, the raw likelihoods reveal a significant race gap in 3 of the 5 areas (Areas 1, 3, and 5). In these 3 areas, officers were overall about twice as likely to conduct a high-discretion search during stops of Hispanics compared to during stops of Whites. These 3 differences, however, are reduced to statistical nonsignificance once the covariates are taken into account.

Pedestrian stops: Notice in the left panel of Table 6.4 that high-discretion searches were conducted in between 12% and 41% of White pedestrian stops on the low and high ends in Areas 1 and 5, respectively. These rates were somewhat elevated for African American pedestrian stops. In 2 of the 5 areas, we found a statistically significant African American-White gap in the likelihood that a high-discretion search was conducted. Whereas a high-discretion search was conducted in 12% of White pedestrian stops in Area 1, this figure was more than doubled (27%) for African American stops. Similarly, in Area 4, a high-discretion search was conducted in 22% of White pedestrian stops compared to in 36% of African American stops. Note that in Area 3, stops of White pedestrians were in fact more likely to involve a high-discretion search than were stops of African American pedestrians (39% and 30%, respectively), though this raw difference was not significant.

Once the covariates were included in the model, the race gap remained significant in Area 1. The Area 4 raw African American-White gap disappeared, and instead a statistically significant gap in Area 3 emerged such that Whites would be *more likely* to be the subject of a high-discretion search than African Americans. If all else were equal, pedestrian stops of Whites in Area 3 would involve a search in 29% of cases compared to pedestrian stops of African Americans, which would involve a search in 14% of cases.

Turning again to the raw counts of high-discretion searches by race and area and type of encounter (see Table 6.5), we see that overall 98 searches of Whites were conducted during pedestrian stops compared to 913 searches of African American pedestrians, or 9 times more than Whites. In Area 1, officers conducted 21 searches of White pedestrians, compared to 221 searches of African American pedestrians. In Area 3, where Whites were proportionally more likely to be the subject of a high-discretion search than African Americans, in raw numbers, there were nearly 5 times as many high-discretion searches conducted of African American pedestrians (116) as similar searches of Whites (24).

Like the African American-White gap in pedestrian searching, pedestrian stops of Hispanics involved significantly more discretionary searching than pedestrian stops of Whites in 2 of the 5 areas. In Area 2, a high-discretion search was conducted in 17% of White pedestrian stops compared to in 39% of Hispanic stops. In Area 4, Hispanics were 1.7 more times likely to be subjected to a high-discretion search than Whites.

Adjusting the likelihood of pedestrian discretionary searching to take the covariates into account, neither instance of the race difference between Hispanic and White high-discretion searches remained significant.

Bicycle stops: Finally, among bicycle stops we found no evidence of any significant racial differences between either African Americans and Whites or Hispanics and Whites in the likelihood that a high-discretion was conducted, with or without covariates.

Table 6.4. Likelihood of being searched contingent on being stopped, excluding incident to arrest, inventory, and probation/parole stops and searches, by area, race, and type of encounter, without covariates (left panel), and controlling for covariates (right panel)

	High-Discretion Searches: No Covariates					High-Discretion Searches: With All Covariates				
	Vehicle Stops (14,797 / 75%)					**Vehicle Stops (14,797 / 75%)**				
	White	Afr Am	Afr/W	Hisp	His/W	White	Afr Am	Afr/W	Hisp	His/W
Area 1	.01	.06	* * *	.02	* *	.02	.10	* * *	.04	+
Area 2	.004	.07	* * *	.01		.01	.11	* * *	.02	
Area 3	.03	.12	* * *	.07	*	.06	.14	* *	.08	
Area 4	.06	.12	* *	.09		.08	.13	+	.09	
Area 5	.06	.21	* * *	.16	* *	.07	.18	* *	.12	+
	Pedestrian Stops (3,778 / 19%)					**Pedestrian Stops (3,778 / 19%)**				
	White	Afr Am	Afr/W	Hisp	His/W	White	Afr Am	Afr/W	Hisp	His/W
Area 1	.12	.27	* * *	.17		.08	.13	*	.09	
Area 2	.17	.19		.39	*	.10	.10		.19	+
Area 3	.39	.30		.43		.29	.14	* *	.21	
Area 4	.22	.36	*	.38	*	.12	.12		.12	
Area 5	.41	.42		.54		.19	.12		.16	
	Bicycle Stops (729 / 4%)					**Bicycle Stops (729 / 4%)**				
	White	Afr Am	Afr/W	Hisp	His/W	White	Afr Am	Afr/W	Hisp	His/W
Area 1	.02	.07		.18	+	.01	.08	+	.13	+
Area 2	.00	.03		.09		.00	.03		.11	
Area 3	.11	.11		.11		.14	.11		.10	
Area 4	.20	.04		.04		.33	.04	+	.03	+
Area 5	.20	.19		.16		.13	.14		.09	

Table 6.5. Frequency of searches by area, race, and type of encounter

		White	Afr Am	Asian	Hispanic	Other
Vehicle Searches						
Area 1	Searched	7	143	5	15	3
	Not Searched	963	2,105	442	630	214
Area 2	Searched	4	80	1	3	0
	Not Searched	983	1,075	297	220	139
Area 3	Searched	11	152	21	39	3
	Not Searched	983	1,111	436	560	125
Area 4	Searched	14	181	1	100	8
	Not Searched	220	1,295	163	993	98
Area 5	Searched	10	500	1	156	7
	Not Searched	158	1,902	69	837	90
Overall	Searched	46	1,056	29	313	21
	Not Searched	2,654	7,488	1,407	3,240	666
Pedestrian Searches						
Area 1	Searched	21	221	8	13	0
	Not Searched	154	604	25	64	15
Area 2	Searched	24	82	3	11	4
	Not Searched	116	344	10	17	5
Area 3	Searched	24	116	41	47	5
	Not Searched	37	266	56	62	14
Area 4	Searched	12	119	3	46	1
	Not Searched	43	215	9	76	6
Area 5	Searched	17	375	5	64	13
	Not Searched	24	510	9	54	6
Overall	Searched	98	913	60	181	23
	Not Searched	374	1,939	109	273	46

		Bicycle Searches				
Area 1	Searched	1	14	0	3	0
	Not Searched	46	173	2	14	3
Area 2	Searched	0	3	0	1	0
	Not Searched	32	110	3	10	2
Area 3	Searched	2	4	0	2	0
	Not Searched	16	32	8	17	1
Area 4	Searched	1	2	0	1	0
	Not Searched	4	50	2	24	2
Area 5	Searched	1	25	0	5	0
	Not Searched	4	105	2	27	0
Overall	Searched	5	48	0	12	0
	Not Searched	102	470	17	92	8
		Other Searches				
Area 1	Searched	3	20	0	2	1
	Not Searched	8	29	4	1	3
Area 2	Searched	0	3	2	0	1
	Not Searched	3	15	1	1	3
Area 3	Searched	1	21	24	9	1
	Not Searched	4	18	3	7	3
Area 4	Searched	0	10	0	4	0
	Not Searched	2	15	1	7	2
Area 5	Searched	1	61	1	7	2
	Not Searched	5	30	0	16	3
Overall	Searched	5	115	27	22	5
	Not Searched	22	107	9	32	14

The moderating role of reason for encounter in search rates

In this section, we ask whether the race differences in the likelihood of being the subject of a high-discretion search vary as a function of the reason for the stop (see Table 6.6). As we did previously, because of the small numbers of consensual encounters (951 stops or 4% of the data set) and probation/parole stops (314 stops or 1% of the data set), we did not present predictions for these categories in the table, though they were included in our analysis. We continue to exclude the low-discretion searches and stops of Asians and Others. In total, our analysis included 19,607 stops and percentages in the tables refer to this subset of the data and not to the total data set.

<u>Traffic violation</u>: Among White stops made because of traffic violations, the raw likelihoods of being the subject of a high-discretion search ranged from .1% in Area 2 to 4% in Area 5. For African American stops made because of traffic violations, these raw likelihoods rise to between 4% in Area 1 to 14% in Area 5. For Hispanic stops made because of a traffic violation, the discretionary search rate is somewhere in between the rates of African American and White stops and ranges from .5% in Area 2 to 11% in Area 5. Stops of African Americans made for traffic violations involve a high-discretionary search between 3 and 50 times as often as stops of Whites. Indeed, we found that in all 5 policing areas, the African American-White gap in discretionary searching was statistically significant.

In Table 6.7, the raw frequencies reveal that across all of Oakland, officers conducted 28 discretionary searches of Whites during stops made because of a traffic violation compared to 672 similar searches of African Americans, or 24 times more than Whites. In Area 1, 8 White searches were conducted compared to 93 African American searches, or nearly 12 times more than Whites. In Area 2, the OPD conducted a single search of a White person stopped for a traffic violation. In contrast, 57 searches of African Americans were conducted. In Area 3, there were 5 White searches and 89 African American searches, or 18 times more than Whites. In Area 4, the figures are 7 and 123, respectively. In Area 5, officers searched a White person stopped for a traffic violation 7 times compared to 310 times when the person stopped was African American, or 44 times more than Whites.

When we add the covariates to the model, this pattern of racial differences in the rate of discretionary searches between African Americans and Whites remains robust. All else being equal, the rate at which officers would conduct a discretionary search during a stop of an African American person for a traffic violation would remain between 2 and 37 times more likely than during a stop of a similarly situated White person.

Similarly, in 3 of the 5 areas (Areas 1, 3, and 5) Hispanics were significantly more likely to be searched than Whites. The Hispanic-White disparity meant searches were 2 to 3 times more frequent for Hispanics stopped for a suspected traffic violation than for Whites. Adjusting for covariates, however, reduces the difference between Hispanic and White stops to nonsignificance.

Probable cause: We found two significant differences in the likelihood that African Americans and Whites were subjected to a high-discretion search in Areas 1 and 4. In Area 1, a White person stopped because of probable cause was searched in 13% of stops compared to in 31% of stops of an African American person. Likewise, in Area 4, a White person stopped because of probable cause was searched in 26% of stops compared to in 47% of stops of an African American person. In Areas 2 and 5, we find that African Americans stopped for probable cause were marginally more likely to be subjected to a high-discretion search.

Table 6.7 shows that the OPD conducted a total of 65 high-discretion searches of Whites stopped because of probable cause across the entire City of Oakland. In contrast, the OPD conducted 826 searches of similarly situated African Americans, or 13 times more than Whites. In Area 1, the area with the most statistically robust race gap, the OPD conducted 16 searches of Whites compared to 178 searches of African Americans, or 11 times more than Whites.

Once the model was adjusted to take the covariates into account, these race differences disappear except for the African American-White gap in Area 1. All else being equal, including crime rate and underlying neighborhood, officer, and stop characteristics, African Americans would still be nearly twice as likely to be searched by an OPD officer for weapons or as part of a consent or probable cause search compared to a White person in Area 1. Controlling for covariates, there is no significant race difference between the rate of probable cause searches for African Americans compared to Whites in Areas 2 through 5.

We found no significant differences in discretionary searches for Hispanics and Whites stopped for probable cause.

Reasonable suspicion: In the raw data, we see that in 3 of the 5 areas, Areas 1, 2, and 5, African American stops for reasonable suspicion involved searching between 2 and 3 times as often as White stops did. These differences are statistically significant. In Area 1, a White person stopped because of reasonable suspicion was searched in 12% of stops compared to in 35% of stops of an African American person. In Areas 2 and 5, a White person stopped because of reasonable suspicion was searched in 26% and 24% of stops compared to in 47% and 51% of stops of an African American person.

When the model is adjusted to take into account the covariates, however, these differences are no longer significant in any area, suggesting that the covariates, and not race, were driving the rate of high-discretion searching among reasonable suspicion stops.

Likewise, we observed 2 significant Hispanic-White differences in the raw data in Areas 4 and 5. In these areas, Hispanic stops made because of reasonable suspicion involved a high-discretion search between 2.3 and 2.8 times as often as White stops did. The Hispanic-White difference in Area 5 remained significant when all else is equal. In Area 5, even after crime rates, demographic characteristics of the neighborhood in which the stop was made, and other features of the officer and the stop are held constant, a Hispanic person stopped because of reasonable suspicion would be the subject of a high-discretion search in 61% of stops compared to in only 27% of stops of a White person, more than twice as often as a similarly situated White person.

Widening our scope and examining the raw frequencies in Table 6.7 across all of Oakland shows that, overall, among reasonable suspicion stops, 32 discretionary searches were conducted of Whites, compared to 497 searches of African Americans, or 16 times more than Whites. For Hispanics, 110 discretionary searches were conducted.

Table 6.6. Likelihood of being searched contingent on being stopped, excluding incident to arrest, inventory, and probation/parole stops and searches, by area, race, and reason for encounter, without covariates (left panel), and controlling for covariates (right panel)

	High-Discretion Search: No Covariates					High-Discretion Search: With All Covariates				
	Traffic Violation (14,274 / 73%)					**Traffic Violation (14,274 / 73%)**				
	White	Afr Am	Afr/W	Hisp	His/W	White	Afr Am	Afr/W	Hisp	His/W
Area 1	.01	.04	* * *	.02	*	.02	.09	* * *	.05	
Area 2	.001	.05	* * *	.005		.003	.11	* * *	.01	
Area 3	.02	.08	* * *	.05	*	.05	.16	* *	.10	
Area 4	.03	.09	* *	.06		.06	.12	+	.09	
Area 5	.04	.14	* *	.11	*	.07	.17	* *	.11	
	Probable Cause (2,945 / 15%)					**Probable Cause (2,945 / 15%)**				
	White	Afr Am	Afr/W	Hisp	His/W	White	Afr Am	Afr/W	Hisp	His/W
Area 1	.13	.31	* * *	.18		.26	.47	* *	.33	
Area 2	.08	.13	+	.16		.23	.29		.33	
Area 3	.35	.39		.41		.57	.57		.56	
Area 4	.26	.47	*	.46	+	.42	.54		.49	
Area 5	.40	.57	+	.56		.48	.59		.51	
	Reasonable Suspicion (1,553 / 8%)					**Reasonable Suspicion (1,553 / 8%)**				
	White	Afr Am	Afr/W	Hisp	His/W	White	Afr Am	Afr/W	Hisp	His/W
Area 1	.12	.35	*	.25		.21	.48	+	.35	
Area 2	.26	.47	*	.33		.43	.59		.53	
Area 3	.32	.33		.29		.58	.50		.41	
Area 4	.21	.38	+	.48	*	.39	.39		.49	
Area 5	.24	.51	*	.67	* *	.27	.48		.61	*

Note: + p < .10 / * p < .05 / ** p < .01 / *** p < .001

Table 6.7. Raw frequency of searches by area, race, and reason for encounter

		White	Afr Am	Asian	Hispanic	Other
Traffic Violation Stops: Searches						
Area 1	Searched	8	93	3	15	2
	Not Searched	1,025	2,230	429	629	215
Area 2	Searched	1	57	1	1	0
	Not Searched	950	1,129	283	203	127
Area 3	Searched	5	89	12	28	0
	Not Searched	316	1,018	401	515	112
Area 4	Searched	7	123	1	62	4
	Not Searched	209	1,288	158	983	94
Area 5	Searched	7	310	2	105	4
	Not Searched	152	1,891	67	825	81
Overall	Searched	28	672	19	211	10
	Not Searched	2,652	7,556	1,338	3,155	629
Probable Cause Stops: Searches						
Area 1	Searched	16	178	2	12	1
	Not Searched	106	404	33	56	12
Area 2	Searched	12	46	4	6	3
	Not Searched	143	298	23	32	19
Area 3	Searched	19	110	45	50	9
	Not Searched	36	171	51	72	16
Area 4	Searched	8	114	1	49	4
	Not Searched	23	130	9	58	4
Area 5	Searched	10	378	2	60	6
	Not Searched	15	285	6	48	6
Overall	Searched	65	826	54	177	23
	Not Searched	323	1,288	122	266	57
Reasonable Suspicion Stops: Searches						
Area 1	Searched	3	93	3	4	0
	Not Searched	21	170	3	12	2
Area 2	Searched	9	55	1	5	1
	Not Searched	26	62	3	10	3
Area 3	Searched	10	70	22	16	0
	Not Searched	21	142	37	40	9
Area 4	Searched	6	59	2	33	1
	Not Searched	23	97	3	36	6
Area 5	Searched	4	220	2	52	10
	Not Searched	13	215	4	26	6
Overall	Searched	32	497	30	110	12
	Not Searched	104	686	50	124	26

The moderating role of special assignment in search rates

When it comes to race differences in the likelihood of being the subject of a high-discretion search, does it matter whether or not the officer was on special assignment at the time the stop was made? We examine the degree of racial disparity between Whites on the one hand and African Americans and Hispanics on the other hand as a function of whether, at the time of the stop, the officer: 1) was not on special assignment at the time of the stop, 2) was working violence suppression, or 3) was on another type of special assignment at the time of the stop (which include "Other," "Prostitution," "Narcotics," "Cruising," and "Special Event," in order of decreasing frequency in the data set). As in the previous moderator analyses of high-discretion searches, in this analysis we continue to exclude what we consider low-discretion searches (i.e., incident to arrest, inventory, and probation/parole searches, as well as stops made because of probation/parole status) and we exclude stops of Asians and Others. In total, this analysis included 19,607 stops. Percentages in the tables refer specifically to this subset of the data and not to the entire data set.

<u>No special assignment</u>: Among stops in which the officer was not on any type of special assignment when the stop was made, we found that African Americans were significantly more likely to be searched for weapons, probable cause, or because they consented than Whites were (see left panel of Table 6.8). In 4 of the 5 areas, these differences were statistically significant. Among White non-special-assignment stops, officers chose to search the person in between 5% and 16% of stops. For African American non-special-assignment stops, the likelihood of being searched rose to between 16% and 28% of stops. In Area 3, the area with the smallest (significant) degree of African American-White disparity, Whites were searched in 12% of non-special-assignment stops, compared to African Americans who were searched in 20% of similar stops, a difference of 67%. In Area 1, the area with the most extreme degree of (significant) African American-White disparity, Whites were searched in 5% of non-special-assignment stops, compared to African Americans, who were searched in 16% of similar stops, a difference of 223%.

In terms of raw frequencies (see Table 6.9), officers who were not on special assignment conducted 115 high-discretion searches of Whites across all of Oakland over the 13-month period. In comparison, non-special-assignment officers conducted 1,449 high-discretion searches of African Americans, or 13 times more than Whites. In Area 1, where the race difference proved to be the most statistically robust, officers conducted 21 searches of Whites and 291 searches of African Americans.

Once the model is adjusted to statistically control for the covariates (see right panel of Table 6.8), we find that there is no significant race difference between high-discretion searches of African Americans and Whites in Areas 2 through 5. However, the African American-White disparity in high-discretion searches remains significant in Area 1, where, all else being equal, African

Americans stopped by officers not on special assignment would be searched more than twice as often as similarly situated Whites.

In contrast, we found no evidence of a significant disparity between the high-discretion search rates of Whites and Hispanics in either the raw or adjusted data.

Violence suppression: Next, we considered stops made by officers who were working violence suppression at the time of the stop. In Areas 1 and 4, we found a significant racial disparity in the discretionary search rates between African Americans and Whites. In these two areas, African Americans were searched between 5 and 6 times as frequently as were Whites.

Examining Table 6.9, we see that across Oakland, officers working violence suppression conducted 18 high-discretion searches of Whites compared to 3,770 searches of African Americans, or 209 times more than Whites. In Area 1, where again the race difference in search rate was most robust, officers working violence suppression conducted 6 searches of Whites and 43 searches of African Americans.

In the right panel of Table 6.8 we see that when controlling for numerous factors, the racial disparity in Area 1 remained statistically significant. As was the case for officers not working special assignment at the time of the stop, we again found no evidence of disparity between the search rates of Whites and Hispanics in either the raw or adjusted data.

Other assignment: Finally, we examined how often stops made by officers working all other types of special assignment involved a high-discretion search. Looking at the raw likelihoods, we see that stops of Whites involved discretionary searching between 1% of the time in Area 2 on the low end and 6% of the time in Areas 3 and 4 on the high end. The same kind of stops of African Americans involved discretionary searching at higher rates, which ranged between 7% of the time and 24% of the time. Statistically, the racial disparities in high-discretion search rates between African Americans and Whites were significant in 3 of the 5 areas, Areas 1, 2, and 5. In these 3 areas, stops of African Americans involved a discretionary search between 5 and 8 times more often than stops of Whites.

When adjusting the model for covariates, the pattern of racial disparities between African Americans and Whites in discretionary searching by officers on other types of special assignment remains significant in the same 3 areas as we observed when examining the raw data. If all of our observable covariates were held constant at the average or most common value, African Americans would still be searched between 4 and 13 times more often than Whites would be in Areas 1, 2, and 5.

Similarly situated Hispanic stops involved searches at a rate in between Whites and African Americans. Officers working another type of special assignment at the time of the stop searched Hispanics in between 4% and 12% of stops. The disparity between Hispanics and Whites was significant in Area 2. In Area 2, stops of Hispanics involved a discretionary search 6 times as often as stops of Whites. All else being equal, Hispanics would be searched 4 times as often as Whites.

In terms of raw numbers of searches, officers working other types of special assignment, including prostitution, narcotics, and cruising, conducted 21 searches of Whites across Oakland. In comparison, they conducted 313 searches of African Americans, or 15 times the number of Whites. The search rate was also higher for Hispanics. Officers working other types of special assignment conducted 86 searches of Hispanics. In Areas 1, 2, and 5, we observed significant racial disparities in the high-discretion search rate of officers working other types of special assignment. In Area 1, officers on other types of special assignment conducted 5 searches of Whites compared to 64 searches of African Americans. In Area 2, officers on other types of special assignment conducted 2 White searches, compared to 22 African American searches and 4 Hispanic searches. Lastly, in Area 5, officers on other types of special assignment conducted 2 searches of Whites compared to 148 searches of African Americans.

Table 6.8. Likelihood of being searched contingent on being stopped, excluding incident to arrest, inventory, and probation/parole stops and searches, by area, race, and special assignment, without covariates (left panel), and controlling for covariates (right panel)

	High-Discretion Search: No Covariates					**High-Discretion Search: With All Covariates**				
	No Special Assignment (10,293 / 52%)					**No Special Assignment (10,293 / 52%)**				
	White	Afr Am	Afr/W	Hisp	His/W	White	Afr Am	Afr/W	Hisp	His/W
Area 1	.05	.16	* * *	.06		.04	.09	* *	.06	
Area 2	.07	.17	* * *	.10		.07	.11	+	.07	
Area 3	.12	.20	* *	.17		.14	.15		.12	
Area 4	.13	.19	+	.16		.10	.10		.10	
Area 5	.16	.28	* *	.21		.10	.14		.11	
	Violence Suppression (5,144 / 26%)					**Violence Suppression (5,144 / 26%)**				
	White	Afr Am	Afr/W	Hisp	His/W	White	Afr Am	Afr/W	Hisp	His/W
Area 1	.01	.05	* *	.02		.02	.06	*	.02	
Area 2	.00	.04		.03		.00	.04		.03	
Area 3	.05	.12	+	.10		.05	.09		.07	
Area 4	.03	.17	*	.08		.04	.13	+	.07	
Area 5	.20	.28		.27		.13	.16		.15	
	Other Assignment (4,170 / 21%)					**Other Assignment (4,170 / 21%)**				
	White	Afr Am	Afr/W	Hisp	His/W	White	Afr Am	Afr/W	Hisp	His/W
Area 1	.02	.11	* * *	.04		.02	.09	* * *	.03	
Area 2	.01	.07	* *	.06	*	.01	.05	* *	.04	*
Area 3	.06	.13	+	.09		.08	.10		.07	
Area 4	.06	.09		.07		.05	.08		.05	
Area 5	.03	.24	* *	.12	+	.01	.13	*	.07	

Note: + p < .10 / * p < .05 / ** p < .01 / *** p < .001

Table 6.9. Raw frequency of searches by area, race, and special assignment

		White	Afr Am	Asian	Hispanic	Other
No Special Assignment: Searches						
Area 1	Searched	21	291	7	22	3
	Not Searched	410	1,511	198	334	85
Area 2	Searched	26	119	4	8	3
	Not Searched	349	574	93	74	42
Area 3	Searched	26	199	45	61	4
	Not Searched	196	774	246	307	69
Area 4	Searched	21	220	4	106	8
	Not Searched	144	965	76	543	62
Area 5	Searched	21	620	3	142	13
	Not Searched	110	1,566	50	533	58
Overall	Searched	115	1,449	63	339	31
	Not Searched	1,209	5,390	663	1,791	316
Violence Suppression: Searches						
Area 1	Searched	6	43	0	4	1
	Not Searched	481	893	171	198	105
Area 2	Searched	0	27	0	3	0
	Not Searched	477	654	153	106	64
Area 3	Searched	4	44	10	16	2
	Not Searched	72	310	118	143	39
Area 4	Searched	2	63	0	22	0
	Not Searched	57	318	48	253	21
Area 5	Searched	6	193	2	58	5
	Not Searched	24	507	11	160	14
Overall	Searched	18	370	12	103	8
	Not Searched	1,111	2,682	501	860	243
Other Assignment: Searches						
Area 1	Searched	5	64	6	7	0
	Not Searched	280	507	104	177	45
Area 2	Searched	2	22	2	4	2
	Not Searched	308	316	65	68	43
Area 3	Searched	8	50	31	20	3
	Not Searched	119	343	139	196	35
Area 4	Searched	4	29	0	23	1
	Not Searched	68	292	51	304	25
Area 5	Searched	2	148	2	32	4
	Not Searched	57	474	19	241	27
Overall	Searched	21	313	41	86	10
	Not Searched	832	1,932	378	986	175

Are there racial disparities in search recovery rates?

Before we get to the results, let us first say a few words about how the analysis of search recovery rates is different from the analyses that we have conducted so far (and will return to again in the next chapter). The reader will recall from the methodology chapter that the crux of the benchmark issue is "eligibility." In other words, the challenge for researchers is to establish which people are actually likely to be stopped by police and therefore have the potential to be subjected to the various post-stop outcomes. As we discussed in Chapter 4, not all members of the general population of a city have an equal likelihood of being stopped by the police. For all intents and purposes, for example, small children may effectively have a 0% chance of being stopped by police.

This issue of eligibility and benchmarks largely goes away when examining search recovery rates. In contrast with other outcomes, analyses of the results of searches need not control for other variables. The reason why control variables are no longer needed is because the entire universe, or pool, of people of interest has already been clearly defined: those who have been searched. Once an officer has made the decision to search someone, then other factors, such as those pertaining to the underlying characteristics or demographics of the neighborhood, largely become irrelevant and should not shape whether or not the officer finds something. In the words of Ayres and Borowsky (2008): "In sharp contrast to disparate-treatment testing, an outcome-regression testing for unjustified disparate racial impacts in searching decisions need only include controls for the race of the people who are stopped. Under the null hypothesis there should be no observable variables that systematically affect the probability of success once the police have made an individualized assessment so as to equalize this very probability." By virtue of deciding to search the person, the officer has effectively determined that there is a good chance that something will be recovered (otherwise, there would be no basis to conduct the search in the first place).

The reader is by now familiar with our focus on the role of officer discretion and as such it will probably come as no surprise that, again, we will exclude searches that were dictated by policy and not decided by the officer. As in our analyses of whether or not a search was conducted in the first place, we will continue to exclude incident to arrest searches and inventory searches (i.e., searches conducted when an arrest is made or a vehicle is towed). In the case of analyzing recoveries, it is less clear whether probation/parole searches are discretionary. In the last section on search rates, we argued that probation/parole searches were not discretionary because officers more or less automatically have the authority to conduct searches of probationers and parolees. Because we could potentially observe very high search rates of probationers and parolees without it necessarily reflecting anything about the given officer who made the stop (e.g., because he or she had the authority and was free to act on it) we left those searches out. Another reason to leave these searches out is that we had no information about the different rates of probation/parole status among different races in the population or in the data set. In the case of recoveries, however, the probationer/parolee case is more complicated because the officer could also choose not to exercise his/her authority to conduct the search if the officer felt reasonably certain that there would be nothing to recover. We acknowledge that there is ambiguity surrounding probation/parole searches and thus we analyzed the data both ways by including probation/parole searches within the category of high-discretion searches and by excluding them. Before we make any exclusions and focus on officer discretion, we will first present tables containing the raw likelihoods of whether or not contraband was found for all 8,975 searches in the data set to satisfy readers who may be curious. Because of the special interest of the law enforcement community in recovering weapons in particular, in the tables that follow we distinguish the discovery of a weapon (grouping the categories “Firearms,” “Firearms and Narcotics,” and “Other Weapons”) from the discovery of other contraband (grouping “Narcotics” and “Other Evidence”). We repeat the regression analysis to determine the likelihood of finding a weapon and then the likelihood of finding any contraband.

Table 6.10 shows the raw data broken down by type of search. Overall, 28% of all searches conducted by OPD officers led to the recovery of contraband. Looking across the rows of the raw data, the reader will notice that search recovery rates are stable and do not vary as a function of the race of the person stopped. This is the case for all 6 types of searches that are recorded by the OPD on the stop data form.

Table 6.11 presents the same data, but grouped into discretionary and non-discretionary searches. Presented another way, namely grouped into non-discretionary and discretionary searches (with and without probation/parole searches), we still see no significant differences in search recovery rates by race.

In the regression table (Table 6.12), note that none of the coefficients is significant, illustrating that the race of the community member who was searched was not a statistically robust factor when predicting whether or not something would be recovered during the course of the search. This statistically echoes the impression one gets from looking at the previous two tables, in which rates of recovery remained relatively similar across racial groups. We found no evidence from the recovery rate analyses that African Americans were unfairly targeted more than Whites in searches. Though we found that African Americans were significantly more likely to be searched than Whites, this overall higher rate of searching did not result in a lower recovery rate as one would expect if officers were using lower standards to search African Americans, as other researchers have found in other cities.[136]

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[136] For instance, see Ayres and Borowsky (2008).

Recovery rates that do not differ by race can be hard to interpret.[137] Stanford law professor Ralph Richard Banks describes the inherent ambiguities of evidence, like search rates and recovery rates, intended to either prove or disprove that police decision-making is discriminatory. He writes: "Whereas lower hit rates for minorities than for Whites would suggest irrational discrimination, equal hit rates are equally consistent with either no discrimination or rational discrimination."[138] We would caution against automatically concluding that because the recovery rates were the same across race the higher search rates of African Americans that we uncovered are necessarily justified. Not only are equal recovery rates inherently ambiguous but also, until recently, there were inconsistencies in what OPD officers counted as recoveries. To address these inconsistencies, in 2015 the OPD reviewed and revised some of its policies around search and recovery procedures. For example, they addressed inconsistencies in what officers counted as a recovery (e.g., should an object like a screwdriver get counted as a recovery if the officer takes the item but then gives it back?). Additionally, the OPD revised its practices for how to count recoveries in cases where multiple people are stopped at the same time (is a knife found on the floor of a car attributable to all of the passengers or to none if everyone denies possession?).

[137] Banks, R. R. (2003). Beyond profiling: Race, policing, and the drug war. *Stanford Law Review*, 571-603.

[138] *Ibid*, p. 585. In this case, "irrational discrimination" can be taken to mean that relying on race is unjustified based on crime rates/drug usage rates and/or it will lead to less effective policing. In contrast "rational discrimination" can be taken to mean that relying on race may help officers do their jobs, in which case "officers will have a powerful incentive to use racial profiling, no matter what the rules say" (p. 588).

Table 6.10. Search recovery rates by reason for search and race (raw data)

	White		Afr American		Asian		Hispanic		Other	
		(%)		(%)		(%)		(%)		(%)
Incident to Arrest (3,160 / 35%)										
Nothing	155	(.68)	1,428	(.62)	79	(.68)	306	(.71)	44	(.67)
Other	67	(.29)	765	(.33)	34	(.29)	102	(.24)	19	(.20)
Weapon	7	(.03)	123	(.05)	4	(.03)	24	(.06)	3	(.05)
Probation / Parole (2,688 / 30%)										
Nothing	88	(.78)	1,716	(.78)	38	(.76)	220	(.75)	18	(.72)
Other	22	(.19)	442	(.20)	9	(.18)	69	(.23)	7	(.28)
Weapon	3	(.03)	48	(.02)	3	(.06)	5	(.02)	0	(.00)
Weapons (1,601 / 18%)										
Nothing	78	(.91)	1,014	(.89)	63	(.86)	238	(.86)	23	(.82)
Other	4	(.05)	87	(.08)	6	(.08)	22	(.08)	4	(.14)
Weapon	4	(.05)	37	(.03)	4	(.05)	16	(.06)	1	(.04)
Probable Cause (1,135 / 13%)										
Nothing	21	(.50)	446	(.53)	17	(.55)	109	(.55)	9	(.45)
Other	20	(.48)	354	(.42)	11	(.35)	81	(.40)	9	(.45)
Weapon	1	(.02)	42	(.05)	3	(.10)	10	(.05)	2	(.10)
Inventory (120 / 1%)										
Nothing	6	(.86)	79	(.96)	2	(1.00)	24	(.92)	3	(1.00)
Other	1	(.14)	2	(.02)	0	(.00)	2	(.08)	0	(.00)
Weapon	0	(.00)	1	(.01)	0	(.00)	0	(.00)	0	(.00)
Consent (271 / 3%)										
Nothing	20	(.77)	150	(.86)	9	(.75)	47	(.84)	2	(1.00)
Other	4	(.15)	20	(.11)	2	(.17)	8	(.14)	0	(.00)
Weapon	2	(.08)	5	(.03)	1	(.08)	1	(.02)	0	(.00)

Note: Weapon is Firearms (63%), Other Weapons (25%), and Firearms & Narcotics (12%).
Other is Narcotics (59%) and Other Evidence (41%).

Table 6.11. Discretionary vs. nondiscretionary search outcomes by reason for search and race of community member (raw data)

	White		Afr American		Asian		Hispanic		Other	
		(%)		(%)		(%)		(%)		(%)
Non-Discretionary: Incident to Arrest + Inventory										
Nothing	161	(.68)	1,507	(.63)	81	(.68)	330	(.72)	47	(.68)
Other	68	(.29)	767	(.32)	34	(.29)	104	(.23)	19	(.28)
Weapon	7	(.03)	124	(.05)	4	(.03)	24	(.05)	3	(.04)
Discretionary: Weapons + Probable Cause + Consent										
Nothing	119	(.77)	1,610	(.75)	89	(.77)	394	(.74)	34	(.68)
Other	28	(.18)	461	(.21)	19	(.16)	111	(.21)	13	(.26)
Weapon	7	(.05)	84	(.04)	8	(.07)	27	(.05)	3	(.06)
Discretionary: Weapons + Probable Cause + Consent + Probation/Parole										
Nothing	207	(.78)	3,326	(.76)	127	(.77)	614	(.74)	52	(.69)
Other	50	(.19)	903	(.21)	28	(.17)	180	(.22)	20	(.27)
Weapon	10	(.04)	132	(.03)	11	(.07)	32	(.04)	3	(.04)

Note: These numbers regroup the numbers on the previous page. The last two tables are redundant. We provide them for the reader because of the uncertainty about where to classify Probation/Parole searches.

Table 6.12. Binomial log-linear regression models predicting the likelihood of finding contraband

	Dependent variable:							
	Finding a Weapon				Finding a Weapon, Narcotics, or Other Evidence			
	All	Incident to Arrest + Inventory	Weapons + P/C + Consent	Weapons + P/C + Consent +Prob/Parole	All	Incident to Arrest + Inventory	Weapons + P/C + Consent	Weapons + P/C + Consent +Prob/Parole
SDRace2Afr American	0.118	0.579	-0.160	-0.220	0.083	0.238	0.141	0.071
	(0.255)	(0.395)	(0.403)	(0.334)	(0.104)	(0.146)	(0.199)	(0.151)
SDRace2Asian	0.463	0.129	0.442	0.601	0.009	0.007	0.031	0.058
	(0.362)	(0.637)	(0.533)	(0.449)	(0.167)	(0.241)	(0.292)	(0.235)
SDRace2Hispanic	0.265	0.593	0.116	0.035	-0.018	-0.183	0.175	0.175
	(0.282)	(0.437)	(0.434)	(0.369)	(0.119)	(0.174)	(0.216)	(0.167)
SDRace2Other	0.218	0.397	0.293	0.068	0.214	0.005	0.470	0.423
	(0.485)	(0.704)	(0.710)	(0.672)	(0.206)	(0.294)	(0.359)	(0.290)
Constant	-3.353***	-3.488***	-3.045***	-3.246***	-1.003***	-0.764***	-1.224***	-1.238***
	(0.247)	(0.384)	(0.387)	(0.322)	(0.101)	(0.140)	(0.192)	(0.147)
Observations	8,975	3,280	3,007	5,695	8,975	3,280	3,007	5,695
Log Likelihood	-1,477.269	-643.534	-530.618	-822.807	-5,329.439	-2,118.776	-1,700.003	-3,139.183
Akaike Inf. Crit.	2,964.537	1,297.069	1,071.236	1,655.613	10,668.880	4,247.553	3,410.006	6,288.366

Note: ***p<0.01

What about probation/parole stops and searches?

In many of our analyses, we exclude stops or searches that were conducted because of probation/parole status. Our goal was to show that, if effects remain when these cases are excluded, our documented effects were not the result of race differences in rates of probation/parole. However, it is legitimate to still ask about the role of probation/parole in the experience of community members interacting with the Oakland Police Department. First, one big caveat: we are not able to study the causal role of probation/parole status. For example, we are unable to determine whether White parolees are less likely to be searched than African American parolees, or if parolees are in general more likely to be handcuffed than non-parolees. This is because officers do not record the probation/parole status of everyone they stop. We only know if probation/parole status *is the reason* for a stop and/or search. We also do not know how many people in the general population of Oakland are actually on probation/parole.

Exclusions: Losing the bulk of the phenomenon?

For high-discretion searches, we excluded people stopped for probation/parole, and people who were the subject of probation/parole, incident to arrest, and inventory searches. This retains 78% of all stops, but only 33% of all searches. Although it is important to exclude these stops to gain internal validity and some clarity on the causal processes, and to rule out explanations having to do with differences in arrest rates or probation/parole rates, it is also important to realize that doing so excludes the bulk of the phenomenon because the majority of searches (66%) result from an arrest, or a vehicle being towed and triggering an inventory search, or are probation/parole searches. Note that the Oakland Police Department commonly excludes incident to arrest searches when computing search and recovery rates in their own statistical reports.

Probation/parole as reason for the stop

Very few stops were based solely on the fact that the officer identified the community member as being on probation/parole: out of 28,119 stops, only 498 (1.8%) were recorded as such. Note that 90% of those probation/parole stops were of either African Americans (77%) or Hispanics (13%), as compared to 73% of traffic stops. Overall, only 2.3% of African American stops and 1.3% of Hispanic stops were due to probation/parole.

What is the outcome of probation/parole stops? In most cases (85%) these stops involve a search, and about one-third of cases (35%) result in an arrest. Of all the categories of stops, probation/parole stops yield the most productive searches: officers recovered contraband 30% of the time (compare this, for example, to 23% for stops made because of a traffic violation). They yield narcotics at the same rate as other types of stops (14%, vs. 14% for traffic violation stops), but a higher percentage of these searches lead to the discovery of weapons (6% vs. 3% for consensual encounter, or 3% for traffic violations), with the highest yield of firearms recovery of any type of stop (4%, vs. 2% for traffic violation or consensual encounter).

Probation/parole as reason for the search

Of the 32% of total stops that involve a search, probation/parole was the second most likely reason for a search (30%) after incident to arrest (35%). 93% of these searches were of either African Americans (82%) or Hispanics (11%). If we exclude individuals searched for incident to arrest, inventory, or probable cause (leaving 84% of stops), we find that 16% of African American stops and 7% of Hispanic stops involved a probation/parole search, compared to 3% for Whites.[139]

What is the outcome of probation/parole searches? Probation/parole searches tend to have a high yield at 23%. In comparison, a recovery is made in 6% of consent searches, 12% in weapons searches, whereas incident to arrest searches yielded a recovery in 36% of cases and probable cause searches yielded a recovery in 47%. Recoveries were primarily narcotics (13%) or other evidence (8%), whereas weapons recoveries were rarer (2%). The discovery of a weapon in this type of search was most common with Asian parolees (6%), but was rare with Whites (3%) and rarest for African American parolees (2%).

Overall, community members were handcuffed 83% of the time when there was a search (compared to 1.5% when there was no search; and of course an arrest would trigger a search). This was true of probation/parole searches too, in which the rate of handcuffing was 82% (compared to 30% for consensual searches, or 70% for a weapons search—which, remember, is justified in the name of officer safety).

20% of probation/parole searches were associated with an arrest.

There were 2,688 probation/parole searches. As mentioned before, 77% yielded nothing, but 8% of those individuals who were found with nothing were still arrested. Of the 2% that yielded any kind of weapon, 83% were arrested (95% were found with firearms). Of the 13% that were found with narcotics, 68% were arrested. Finally, of the 8% that carried "other evidence," 47% were arrested. Of course we do not know from the data whether the recovered contraband was the cause of the arrest

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[139] Recall that we do not know what proportion of residents of Oakland or people who routinely travel in and around Oakland are on probation/parole. This would require probation and criminal records from Alameda County to which we did not have access. Nor do we know if an individual stopped is on probation/parole unless that is recorded as the reason for the stop or the reason for the search.

in any of these cases. It is striking that in 62% of the probation/parole searches that yielded any kind of contraband there was an arrest, whereas when nothing was found there was an 8% arrest rate, which suggests that the recovery may have been the reason for the arrest. These probation/parole search recoveries and subsequent arrests constituted 9% (377) of all arrests (4,099) in the data set. Adding the probation/parole searches that yielded an arrest even in the absence of contraband constituted 13% of arrests in the data set.

How many officers conduct probation/parole stops and searches? On the one hand, we find that, during the 13-month period of analysis, 68% of officers never made a stop because of probation/parole—only 163 officers (32%) made this type of stop. On the other hand, we found that using probation/parole as a reason for a search was widespread, with two-thirds of officers (67%) having used this as a justification for a search at least once. 12% of officers in our data set never conducted a search, which means that of officers who conducted a search, 77% have used the probation/parole justification at some point.

Chapter 7 | ARRESTS

Core Findings	
	• ***Overall, more than 1 in 6 African American men stopped was arrested vs. only 1 in 14 White men stopped.*** • ***Even when controlling for other variables, African Americans were still significantly more likely than Whites to be arrested in 2 of Oakland's 5 policing areas.*** • ***The African American-White arrest gap was most pronounced for vehicle stops, stops made because of traffic violations, and stops made by officers working violence suppression.***

In this chapter, we pose the question of whether or not stops of African Americans and Hispanics, compared to stops of Whites, were more likely to end in an arrest. Being arrested during a routine officer-initiated stop is arguably the most severe possible outcome. The OPD records in its stop data whether a misdemeanor or felony arrest was made. We decided to collapse across arrest type and here we analyze the likelihood that a stop ended in any kind of arrest. In the raw data, stops of African Americans, compared to stops of non-African Americans, were significantly more likely to end in both felony arrest, $F = 428.6$, $p < .0005$, and misdemeanor arrest, $F = 22.3$, $p < .0005$. Therefore, we can be confident that collapsing across arrest type is not distorting the underlying pattern of racial disparities.

Looking at the total data set, a stop ultimately ended in an arrest 7.6% of the time for Whites, compared to 18.1% of the time for African Americans, and 11.3% of the time for Hispanics. Looking only at males, White males were arrested 7.3% of the time, compared to 18.5% for African American males, and 12.0% for Hispanic males.

As an additional note, the stop data form does not include information about why the arrest was made or what statute was allegedly broken. Presumably, officers can write about the reason for an arrest in the narrative section of the stop data form, but in many cases a crime report supplants the narrative. In these cases, the narrative section provides a reference to a separate arrest report, which was not included in the data we analyzed. Therefore, the research team does not know why any arrest was made. In the future, one could try to obtain this information (e.g., from arrest reports stored in the OPD's Records Management System) and code the reasons for arrest in terms of severity of the alleged crime for which African Americans and Hispanics relative to Whites are arrested. It may be the case that the generally higher arrest rates for African Americans and Hispanics is due to racial differences in criminal behavior, as many in law enforcement would argue.[140] We would expect then that members of racial minority groups, compared to Whites, would be arrested in greater numbers for offenses of similarly high severity, as "evidence" of higher levels of criminal activity. It could also be the case, however, that police officers have lower thresholds for deciding when the actions of an African American or Hispanic person, compared to a White person, become criminal. If this were true, then we would expect to see minority group members being arrested for greater numbers of minor or more subjective reasons, such as resisting an officer or disorderly conduct. Indeed, in an analysis conducted by *The New York Times* of traffic stops made in Greensboro, North Carolina, this is exactly what was found.[141]

Are there racial disparities in arrest rates?

Overall, among the 28,119 stops recorded in our 13-month period, 4,099 (15%) involved an arrest. Among White stops, an arrest was made in between 4% (Area 2) and 15% (Area 4) of stops. In comparing the White arrest rate to that of African Americans, we see that by area, the highest arrest rate for Whites is closer to the lowest arrest rate for African Americans. Of African American stops, an arrest was made in between 13% of stops on the low end (Area 2) and 21% of stops on the high end (Area 5). Turning to the raw numbers allows us to better grasp the magnitude of these percentage differences. For instance, in Area 5, there were 1,081 African American stops that resulted in an arrest compared to only 39 White stops that led to an arrest. In 4 of 5 areas, the African American-White race gap in arrests was statistically significant when not controlling for covariates. In these 4 areas, stops of African Americans ended in an arrest between 1.5 and 3.4 times as often as stops of Whites. In Area 1, the area with the most extreme African American-White

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[140] A report from the Bureau of Justice Statistics that examines homicide data from 1980 to 2008 finds that African Americans are disproportionately represented both as homicide offenders and homicide victims. In 2008, the offending rate for African Americans (24.7 offenders per 100,000) was 7 times higher than the rate for Whites (3.4 offenders per 100,000). In the same year, the victimization rate for African Americans (19.6 victims per 100,000) was 6 times the victimization rate for Whites (3.3 homicides per 100,000) (p. 11). Between 1980 and 2008, young adult African American males had the highest homicide offending rate compared to offenders in other racial and sex categories. Young African American males (14 to 24 years old) accounted for about 1% of the population from 1980 to 2008, but made up 17% to 35% of all homicide offenders, and in 2008 made up 27% of all homicide offenders (see figure 23b). Young African American males made up between 9% to 18% of offenders, and in 2008 made up 16% of victims. For more information, see Cooper, A., & Smith, E. L. (2011). *Homicide trends in the United States, 1980-2008.* Washington, DC: Bureau of Justice Statistics.

[141] LaFraniere, S., & Lehren, A. W. (Oct. 24, 2015). The disproportionate risks of driving while black: An examination of traffic stops and arrests in Greensboro, N.C., uncovered wide racial differences in measure after measure of police conduct. *The New York Times.*

disparity, a White person was arrested in 5% of stops compared to in 17% of stops of an African American person

Table 7.1. Likelihood of being arrested contingent on being stopped broken down by area and race, without covariates

Arrests: No Covariates								
	White	Afr Am	Af/W	Asian	As/W	Hispan	His/W	Other
Area 1	.05	.17	* * *	.02	* *	.05		.04
Area 2	.04	.13	* * *	.02	+	.06		.02
Area 3	.13	.19	* *	.14		.13		.11
Area 4	.15	.18		.09	*	.13		.16
Area 5	.14	.21	*	.14		.14		.12

Note: + p < .10 / * p < .05 / ** p < .01 / *** p < .001

Table 7.2. Raw frequency of arrests by race and area

Arrests						
		White	**Afr Am**	**Asian**	**Hispanic**	**Other**
Area 1	Arrested	71	776	40	10	11
	Not Arrested	1,233	3,813	762	496	244
Area 2	Arrested	47	270	16	6	4
	Not Arrested	1,174	1,859	270	316	157
Area 3	Arrested	65	431	114	100	20
	Not Arrested	439	1,866	777	594	157
Area 4	Arrested	56	482	190	19	23
	Not Arrested	307	2,127	1,327	182	121
Area 5	Arrested	39	1,081	196	15	17
	Not Arrested	230	4,113	1,241	89	126
Overall	Arrested	278	3,040	556	150	75
	Not Arrested	3,383	13,778	4,377	1,677	805

The arrest rates of Asians more closely resembled the arrest rates of Whites in 3 of 5 areas. However, in Area 1 and Area 4, without controlling for covariates, Asians were significantly less likely than Whites to be arrested. In Area 1, an Asian person was arrested in 2% of stops compared to 5% of stops of a White person. Similarly, in Area 4, a person was arrested in 9% of Asian stops compared to the 15% of White stops that resulted in an arrest. We observed no significant differences between White arrest rates and Hispanic and Other arrest rates.

Table 7.3. Likelihood of being arrested contingent on being stopped broken down by area and race, with covariates

Arrests: With All Covariates								
	White	Afr Am	Af/W	Asian	As/W	Hispan	His/W	Other
Area 1	.03	.05	* * *	.02	+	.03		.03
Area 2	.02	.05	* * *	.01		.03		.01
Area 3	.04	.05		.06	+	.04		.04
Area 4	.05	.06		.04		.05		.06
Area 5	.05	.05		.05		.05		.04

Note: + p < .10 / * p < .05 / ** p < .01 / *** p < .001

When the model is adjusted to control for covariates, the African American-White gap in arrests remains statistically significant in 2 of the original 4 areas, Areas 1 and 2 (see Table 7.3). In these areas, if all observable covariates were equal, stops of African Americans would still end in an arrest between 1.7 and 2.5 times as often as would stops of Whites. This gap cannot be explained by the area crime rate or a number of underlying characteristics of the neighborhood in question.

The moderating role of type of encounter in arrest rates

Does the type of encounter influence the race differences we observed in the likelihood that OPD officers made an arrest during a stop? As is now familiar, in this analysis and the arrest analyses going forward, we have excluded stops of Asians and Others (2,441 or 9% of the total data), because any estimates produced would be somewhat unreliable. The percentages in Table 7.4 refer to this specific subset of the data. Also, although included in the models, we do not present the results for "Other" types of stops (e.g., those stops that were not vehicle, pedestrian, or bicycle stops; 2% of the subsetted data).

Vehicle stops: Among vehicle stops, we found significant differences in the raw data between stops of Whites and stops of African Americans in the likelihood that an arrest was made across 4 of the 5 areas. As illustrated in the left panel of Table 7.4, the raw differences in the likelihood that an African American stop compared to a White stop ended in an arrest ranged from 1.7 times as likely to 6 times as likely. As just one example, whereas Whites stopped in a vehicle in Area 3 were arrested in 6% of stops, African Americans stopped in a vehicle were arrested in 12% of stops, a difference of 100%. Once covariates were taken into account, the African American-White race gap in arrest rates among vehicle stops remained statistically significant in 3 areas. If all else were equal, vehicle stops of African Americans in Areas 1, 2, and 3 would still be more likely to end in an arrest compared to vehicle stops of Whites.

In terms of the raw frequencies (see Table 7.4), we found that overall, the OPD made 91 arrests of Whites who had been stopped in a vehicle, compared to 1,064 arrests of similarly situated African Americans, or almost 12 times the number of Whites. In Areas 1, 2, and 3, we found significant racial disparities in the arrest rate among vehicle stops that were statistically robust when accounting for covariates. In Area 1, OPD officers arrested 16 Whites compared to 197 African Americans, or 12 times the number of Whites. In Area 2, the OPD made 15 arrests of Whites stopped in vehicles compared to 79 arrests of similarly situated African Americans. Lastly, in Area 3, 24 Whites were arrested compared to 187 African Americans, or 8 times the number of Whites. Examining the left panel of Table 7.4 shows that Hispanics and Whites stopped in vehicles experienced very similar rates of arrest. As such, we found no significant difference in the likelihood that a Hispanic person stopped in a vehicle was arrested compared to a similarly situated White person. This remained the case once we adjusted the model for covariates.

Pedestrian stops: The left panel of Table 7.4 illustrates that an arrest was made in between 18% of White pedestrian stops on the low end (Area 2) and 36% of White pedestrian stops on the high end (Area 3). The likelihood of arrest was significantly elevated for African American pedestrian stops in Areas 1 and 2 but was similar to that of Whites in the remaining areas. In Area 1, the location with the largest raw magnitude of difference, 1 in 5 White pedestrians was arrested compared to 1 in 3 African American pedestrians. Adjusting the likelihood of pedestrian arrests to take into account the covariates, however, we found that these two instances of African American-White difference in arrest rates were reduced to non-significance. We found no evidence that stops of Hispanics statistically differed from stops of Whites in the arrest rate. In raw numbers across all of Oakland (see Table 7.5), the OPD overall made 164 arrests of White pedestrians compared to 1,743 arrests of African American pedestrians, or more than 10 times the number of Whites.

Bicycle stops: Finally, among bicycle stops we found no evidence of any significant racial differences between either African Americans and Whites or Hispanics and Whites in the likelihood that an arrest was made.

Table 7.4. Likelihood of being arrested contingent on being stopped broken down by area, race, and type of encounter, without covariates, and controlling for covariates

Arrest: No Covariates						Arrest: With All Covariates					
Vehicle Stops (17,236 / 68%)						**Vehicle Stops (17,236 / 68%)**					
	White	Afr Am	Afr/W	Hisp	His/W		White	Afr Am	Afr/W	Hisp	His/W
Area 1	.02	.07	* * *	.02		Area 1	.02	.05	* * *	.01	
Area 2	.01	.06	* * *	.02		Area 2	.02	.05	* * *	.02	
Area 3	.06	.12	* *	.07		Area 3	.03	.05	*	.04	
Area 4	.09	.11		.08		Area 4	.05	.07		.05	
Area 5	.07	.12	*	.09		Area 5	.05	.06		.05	
Pedestrian Stops (6,623 / 26%)						**Pedestrian Stops (6,623 / 26%)**					
	White	Afr Am	Afr/W	Hisp	His/W		White	Afr Am	Afr/W	Hisp	His/W
Area 1	.20	.33	* * *	.26		Area 1	.06	.08	+	.08	
Area 2	.18	.25	*	.18		Area 2	.05	.07		.04	
Area 3	.36	.35		.33		Area 3	.08	.08		.08	
Area 4	.32	.36		.36		Area 4	.08	.08		.09	
Area 5	.33	.36		.37		Area 5	.08	.08		.08	
Bicycle Stops (1,053 / 4%)						**Bicycle Stops (1,053 / 4%)**					
	White	Afr Am	Afr/W	Hisp	His/W		White	Afr Am	Afr/W	Hisp	His/W
Area 1	.07	.13		.05		Area 1	.04	.08		.02	
Area 2	.00	.10		.14		Area 2	.00	.07		.10	
Area 3	.05	.08		.00		Area 3	.03	.05		.00	
Area 4	.12	.29		.09		Area 4	.13	.25		.05	
Area 5	.17	.19		.20		Area 5	.06	.08		.08	

Note: + p < .10 / * p < .05 / ** p < .01 / *** p < .001

Table 7.5. Raw frequency of arrests by area, race, and type of encounter

Vehicle Stops: Arrests						
		White	**Afr Am**	**Asian**	**Hispanic**	**Other**
Area 1	Arrested	16	197	3	10	3
	Not Arrested	983	2,450	453	656	221
Area 2	Arrested	15	79	4	5	2
	Not Arrested	988	1,199	298	225	140
Area 3	Arrested	24	187	25	49	6
	Not Arrested	349	1,370	462	619	131
Area 4	Arrested	22	210	10	97	9
	Not Arrested	235	1,638	166	1,140	109
Area 5	Arrested	14	391	6	100	11
	Not Arrested	176	2,754	73	1,038	100
Overall	Arrested	91	1,064	48	261	31
	Not Arrested	2,731	9,411	1,452	3,678	701

Pedestrian Stops: Arrests						
		White	**Afr Am**	**Asian**	**Hispanic**	**Other**
Area 1	Arrested	47	524	7	29	5
	Not Arrested	184	1,066	36	83	16
Area 2	Arrested	32	165	0	7	2
	Not Arrested	147	491	13	32	10
Area 3	Arrested	36	224	57	58	12
	Not Arrested	63	409	104	118	22
Area 4	Arrested	29	229	7	81	14
	Not Arrested	61	402	13	147	8
Area 5	Arrested	20	601	7	79	5
	Not Arrested	41	1,082	13	136	21
Overall	Arrested	164	1,743	78	254	38
	Not Arrested	496	3,450	179	516	77

Bicycle Stops: Arrests						
		White	Afr Am	Asian	Hispanic	Other
Area 1	Arrested	4	37	0	1	0
	Not Arrested	53	238	2	19	3
Area 2	Arrested	0	17	0	2	0
	Not Arrested	35	147	3	12	2
Area 3	Arrested	1	4	2	0	1
	Not Arrested	20	47	8	22	1
Area 4	Arrested	1	26	0	3	0
	Not Arrested	7	63	2	29	2
Area 5	Arrested	1	41	0	9	0
	Not Arrested	5	174	2	35	0
Overall	Arrested	7	125	2	15	1
	Not Arrested	120	669	17	117	8

Other Stops: Arrests						
		White	Afr Am	Asian	Hispanic	Other
Area 1	Arrested	4	18	0	0	3
	Not Arrested	13	59	5	4	4
Area 2	Arrested	0	9	2	2	0
	Not Arrested	4	22	2	1	5
Area 3	Arrested	4	16	16	7	1
	Not Arrested	7	40	20	18	3
Area 4	Arrested	4	17	2	9	0
	Not Arrested	4	24	1	11	2
Area 5	Arrested	4	48	2	8	1
	Not Arrested	8	103	1	32	5
Overall	Arrested	16	108	22	26	5
	Not Arrested	36	248	29	66	19

The moderating role of reason for encounter in arrest rates

Do the race differences observed in the likelihood of being arrested vary as a function of the reason for the stop? We continue to exclude stops of Asians and Others because of their small numbers. As we did in previous analyses that included reason for encounter as a moderator, we are not presenting the results for consensual encounters (951 stops or 4% of the data set) or probation/parole stops (314 stops or 1% of the data set) because of the small numbers.

Traffic violation: Among White stops made because of traffic violations, the raw likelihoods of being arrested ranged from 1% to 4% (see left panel of Table 7.6). For African American stops made because of traffic violations, these raw likelihoods were between 4% and 7%. For Hispanic stops made because of a traffic violation, the arrest rate was higher than for Whites but lower than for African Americans and ranged from 1% to 5%. Stops of African Americans made for traffic violations involved an arrest being made between 1.75 and 5 times as often as similar stops of Whites. Indeed, we found that in 4 of the 5 policing areas, the African American-White gap in arrest rate was statistically significant without controlling for covariates. Similarly, in 2 of the 5 areas (Areas 2 and 3), Hispanics were significantly more likely to be arrested than Whites. In these areas, Hispanics were arrested 2 to 4 times more often than Whites. When we add the covariates to the model (see right panel of Table 7.6), the pattern of racial differences in arrest between African Americans and Whites remains robust. All else being equal, the rate at which officers would arrest an African American person stopped for a traffic violation would remain between 2.5 and 5 times more likely than during a stop of a similarly situated White person in Areas 1 through 4. Adjusting for covariates leaves one statistically significant difference between Hispanic and White arrest rates, in Area 3.

Let us note the raw number of arrests made during stops for traffic violations in order to understand the magnitude of the racial disparities observed here. Overall, across all of Oakland during a 13-month period, officers made 37 arrests of Whites stopped for traffic violations, and 551 arrests of African Americans, or 15 times more than Whites. There were 137 arrests made of Hispanics. In Area 1, the OPD arrested 15 Whites stopped for a traffic violation compared to 120 African Americans. In Area 2, 6 White arrests were made compared to 54 African American arrests. In Area 3, the number of Whites stopped for a traffic violation who were ultimately arrested can be counted on one hand (4). In contrast, 57 African Americans and 24 Hispanics were arrested. In Area 4, 6 White stops made because of a traffic violation ended in an arrest compared to 121 similarly situated African American stops.

Probable cause: We found two significant differences in the likelihood that African Americans and Whites were arrested, in Areas 1 and 2. In Area 1, a White person stopped because of probable cause was arrested in 22% of stops compared to in 40% of stops of an African American person, a difference of 82%. Likewise, in Area 2, a White person stopped because of probable cause was arrested in 14% of stops compared to in 27% of stops of an African American person, a difference of 93%. We found one significant difference in arrest rates for Hispanics and Whites stopped for probable cause. In Area 3, a White person stopped because of probable cause was arrested in 50% of stops compared to in 34% of stops of a Hispanic person. Thus, Hispanics were 32% less likely to be arrested. Once the model was adjusted to take the covariates into account, the African American-White race gap remained significant in Areas 1 and 2. All else being equal, including crime rate and underlying neighborhood, officer, and stop characteristics, African Americans would still be between 44% and 68% more likely to be arrested than Whites. The Hispanic-White gap in arrest, however, was no longer significant once covariates were included in the model.

In raw counts (see Table 7.7), there were 181 total arrests of Whites stopped for probable cause across all of Oakland compared to 1,788 arrests of African Americans stopped for probable cause, or 10 times the number of Whites. In Area 1, 37 White arrests were made compared to 458 African American arrests. In Area 2, 27 White arrests were made compared to 143 African American arrests.

Reasonable suspicion: Finally, among stops made because of reasonable suspicion, we did not find any significant differences in the rates of arrest by race.

Table 7.6. Likelihood of being arrested contingent on being stopped broken down by area, race, and reason for encounter, without covariates (left panel), and controlling for covariates (right panel)

	Arrest: No Covariates					**Arrest: With All Covariates**				
	Traffic Violation (16,037 / 63%)					**Traffic Violation (16,037 / 63%)**				
	White	Afr Am	Afr/W	Hisp	His/W	White	Afr Am	Afr/W	Hisp	His/W
Area 1	.01	.05	* * *	.01		.02	.05	* *	.02	
Area 2	.01	.04	* * *	.02	*	.01	.05	* * *	.03	+
Area 3	.01	.05	* *	.04	*	.02	.06	*	.05	*
Area 4	.03	.07	*	.04		.03	.07	*	.04	
Area 5	.04	.07	+	.05		.04	.06		.05	
	Probable Cause (5,466 / 22%)					**Probable Cause (5,466 / 22%)**				
	White	Afr Am	Afr/W	Hisp	His/W	White	Afr Am	Afr/W	Hisp	His/W
Area 1	.22	.40	* * *	.25		.19	.32	* * *	.24	
Area 2	.14	.27	* * *	.18		.18	.26	*	.19	
Area 3	.50	.51		.34	* *	.33	.34		.23	+
Area 4	.52	.50		.50		.40	.38		.38	
Area 5	.54	.46		.50		.45	.34		.37	
	Reasonable Suspicion (2,305 / 9%)					**Reasonable Suspicion (2,305 / 9%)**				
	White	Afr Am	Afr/W	Hisp	His/W	White	Afr Am	Afr/W	Hisp	His/W
Area 1	.26	.26		.23		.19	.20		.18	
Area 2	.16	.24		.06		.15	.19		.07	
Area 3	.14	.18		.24		.16	.15		.20	
Area 4	.22	.28		.23		.19	.20		.17	
Area 5	.17	.23		.22		.11	.14		.15	

Note: + p < .10 / * p < .05 / ** p < .01 / *** p < .001

Table 7.7. Frequency of arrests by area, race, and reason for encounter

Traffic Violation Stops: Arrests						
		White	**Afr Am**	**Asian**	**Hispanic**	**Other**
Area 1	Arrested	15	120	1	8	2
	Not Arrested	1,050	2,545	437	654	221
Area 2	Arrested	6	54	0	5	1
	Not Arrested	955	1,250	285	206	128
Area 3	Arrested	4	57	18	24	3
	Not Arrested	333	1,203	418	561	115
Area 4	Arrested	6	121	4	45	2
	Not Arrested	221	1,573	161	1,093	100
Area 5	Arrested	6	199	6	55	4
	Not Arrested	165	2,532	70	971	87
Overall	Arrested	37	551	29	137	12
	Not Arrested	2,724	9,103	1,371	3,485	651

Probable Cause Stops: Arrests						
		White	**Afr Am**	**Asian**	**Hispanic**	**Other**
Area 1	Arrested	37	458	7	23	6
	Not Arrested	128	692	35	70	14
Area 2	Arrested	27	143	3	9	2
	Not Arrested	160	383	26	42	24
Area 3	Arrested	54	301	59	62	14
	Not Arrested	54	288	89	121	23
Area 4	Arrested	36	257	13	111	17
	Not Arrested	33	253	10	112	10
Area 5	Arrested	27	629	6	104	7
	Not Arrested	23	725	9	104	14
Overall	Arrested	181	1,788	88	309	46
	Not Arrested	398	2,341	169	449	85

Reasonable Suspicion Stops: Arrests						
		White	Afr Am	Asian	Hispanic	Other
Area 1	Arrested	9	117	1	6	1
	Not Arrested	26	330	8	20	1
Area 2	Arrested	7	44	3	1	0
	Not Arrested	37	138	3	15	4
Area 3	Arrested	5	48	11	19	1
	Not Arrested	30	226	61	60	9
Area 4	Arrested	8	71	2	22	3
	Not Arrested	29	185	5	73	7
Area 5	Arrested	4	148	2	23	4
	Not Arrested	20	501	6	83	16
Overall	Arrested	33	428	19	71	9
	Not Arrested	142	1,380	83	251	37

The moderating role of special assignment in arrest rates

Are race differences in the likelihood of being arrested moderated by whether or not the officer was working a special assignment at the time of the stop? We examine the degree of racial disparity between stops of Whites on the one hand and stops of African Americans and Hispanics on the other hand as a function of whether the officer who made the stop was not on special assignment at the time of the stop, was working violence suppression at the time of the stop, or was on another type of special assignment at the time of the stop (which include "Other," "Prostitution," "Narcotics," "Cruising," and "Special Event," in order of decreasing frequency in the total data set). We exclude stops of Asians and Others. Percentages in the tables refer specifically to shares of this subset of the data (i.e., stops of Whites, African Americans, and Hispanics) and not to the entire data set.

No special assignment: Among stops in which the officer was not on any type of special assignment when the stop was made, we found that African Americans were more likely to be arrested than Whites were (see left panel of Table 7.8). In 4 of the 5 areas, these differences were statistically significant. Among White non-special-assignment stops, officers arrested the person in between 8% and 11% of stops. For African American non-special-assignment stops, the likelihood of being arrested increased to between 14% and 20% of stops. Similarly, we found one statistically significant difference in the likelihood that Hispanics compared to Whites were arrested in Area 3. In this area, Whites were arrested in 8% of non-special-assignment stops, compared to Hispanics who were arrested in 15% of similar stops, a difference of 88%.

Table 7.9 shows that overall, officers not on special assignment arrested 151 Whites. In contrast, officers not on special assignment arrested 1,772 African Americans and 335 Hispanics.

Once the model is adjusted to control for the covariates, however, we find that all of the raw significant differences were reduced to statistical nonsignificance, suggesting that covariates, such as crime rate, were likely driving the apparent race differences.

Violence suppression: Next, we considered stops made by officers who were working violence suppression at the time that they made the stop. In Areas 1 and 2, we found a significant racial disparity in the arrest rates between African Americans and Whites. In these two areas, African Americans stopped by an officer working violence suppression were arrested 7 and 9 times more frequently than similarly situated Whites. In another two areas, Areas 3 and 4, the African American-White difference was marginal and African Americans were arrested 2.5 times more frequently than Whites were arrested, though again these differences were not significant. Likewise, in one area, Area 1, Hispanics stopped by an officer working violence suppression were 3 times more likely to be arrested than similar Whites were arrested.

In raw numbers, officers working violence suppression at the time of the stop arrested 27 Whites, 555 African Americans, and 74 Hispanics. In Area 1, violence suppression officers arrested 4 Whites, 108 African Americans, and 6 Hispanics. In Area 2, violence suppression officers arrested 6 Whites compared to 57 African Americans.

In the right panel of Table 7.8 we see that if all else were equal, stops of African Americans made by officers working violence suppression would still end in an arrest 3 to 5 times more likely than similar stops of Whites in Areas 1 and 2. These differences were not explained away by many of the common explanations for racial differences typically offered by law enforcement, including, perhaps most notably for arrests, the crime rate of the neighborhood in which the stop was made. The fact that we still found race differences in arrest rates suggests, as we discussed at the outset of this chapter, that deciding whether or not to make an arrest may be more discretionary than most members of the public would think. Once the covariates were included in the model, we found no significant difference in arrest rates between Whites and Hispanics.

Other assignment: Finally, we examined how often stops made by officers working other types of special assignment ended in an arrest. Looking at the raw likelihoods, we see that stops of Whites ended in an arrest between 2% of the time in Area 2 on the low end and 26% of the time in Area 3 on the high end. The same kinds of stops of African Americans were more likely to end in an arrest. African Americans were arrested in between 12% and 34% of stops. Statistically, the racial disparities in arrest rates between African Americans and Whites were significant in 3 of the 5 areas, Areas 1, 2, and 3. In these 3 areas, stops of African Americans involved an arrest between 1.3 and 6 times more often than stops of Whites. The disparity between Hispanics and Whites was significant in Area 3. In Area 3, stops of Hispanics ended in an arrest 15% of the time compared to 26% of the time for similar Whites, a reduction of 42%.

When taking the covariates into account, however, all of the racial disparities in arrest rates among stops made by officers working other types of special assignment were reduced to statistical nonsignificance. Nonetheless (see Table 7.9), officers working other types of special assignment made 100 arrests of Whites during the time period under examination. In contrast, they made 713 arrests of African Americans and 147 arrests of Hispanics.

Table 7.8. Likelihood of being arrested contingent on being stopped broken down by area, race, and special assignment, without covariates, and controlling for covariates

	Arrest: No Covariates					Arrest: With All Covariates				
	No Special Assignment (13,825 / 54%)					**No Special Assignment (13,825 / 54%)**				
	White	Afr Am	Afr/W	Hisp	His/W	White	Afr Am	Afr/W	Hisp	His/W
Area 1	.09	.20	* * *	.07		.03	.04		.03	
Area 2	.09	.18	* * *	.13		.04	.05	+	.06	
Area 3	.08	.14	* *	.15	* *	.03	.05		.06	+
Area 4	.16	.18		.14		.06	.06		.05	
Area 5	.11	.19	*	.13		.03	.04		.04	
	Violence Suppression (6,256 / 25%)					**Violence Suppression (6,256 / 25%)**				
	White	Afr Am	Afr/W	Hisp	His/W	White	Afr Am	Afr/W	Hisp	His/W
Area 1	.01	.09	* * *	.03	*	.01	.05	* * *	.02	
Area 2	.01	.07	* * *	.03		.01	.03	* *	.01	
Area 3	.04	.10	+	.04		.01	.04	+	.01	
Area 4	.06	.15	+	.06		.03	.07		.03	
Area 5	.26	.24		.15		.10	.07		.06	
	Other Assignment (5,331 / 21%)					**Other Assignment (5,331 / 21%)**				
	White	Afr Am	Afr/W	Hisp	His/W	White	Afr Am	Afr/W	Hisp	His/W
Area 1	.07	.18	* * *	.03	+	.03	.04		.03	
Area 2	.02	.12	* * *	.00		.04	.05	+	.06	
Area 3	.26	.34	*	.15	* *	.03	.05		.06	+
Area 4	.20	.24		.14		.06	.06		.05	
Area 5	.16	.24		.14		.03	.04		.04	

Note: + p < .10 / * p < .05 / ** p < .01 / *** p < .001

Table 7.9. Frequency of arrests by area, race, and special assignment

		White	Afr Am	Asian	Hispanic	Other
No Special Assignment: Arrests						
Area 1	Arrested	45	528	7	28	5
	Not Arrested	446	2,146	209	369	90
Area 2	Arrested	36	167	3	13	4
	Not Arrested	382	780	97	85	48
Area 3	Arrested	20	176	67	69	10
	Not Arrested	241	1,068	300	386	79
Area 4	Arrested	33	296	10	118	11
	Not Arrested	173	1,347	82	699	75
Area 5	Arrested	17	605	8	107	12
	Not Arrested	138	2,587	56	720	77
Overall	Arrested	151	1772	95	335	42
	Not Arrested	1,380	7,928	744	2,259	369

		White	Afr Am	Asian	Hispanic	Other
Violence Suppression: Arrests						
Area 1	Arrested	4	108	0	6	1
	Not Arrested	495	1,046	172	205	107
Area 2	Arrested	6	57	0	3	0
	Not Arrested	483	726	153	111	64
Area 3	Arrested	3	45	5	7	5
	Not Arrested	76	390	132	170	41
Area 4	Arrested	4	77	2	17	3
	Not Arrested	59	438	49	292	21
Area 5	Arrested	10	268	4	41	2
	Not Arrested	29	852	13	228	18
Overall	Arrested	27	555	11	74	11
	Not Arrested	1,142	3,452	519	1,006	251

Other Assignment: Arrests						
		White	Afr Am	Asian	Hispanic	Other
Area 1	Arrested	22	140	3	6	5
	Not Arrested	292	621	115	188	47
Area 2	Arrested	5	46	3	0	0
	Not Arrested	309	353	66	74	45
Area 3	Arrested	42	210	28	38	5
	Not Arrested	122	408	162	221	37
Area 4	Arrested	19	109	7	55	9
	Not Arrested	75	342	51	336	25
Area 5	Arrested	12	208	3	48	3
	Not Arrested	63	674	20	293	31
Overall	Arrested	100	713	44	147	22
	Not Arrested	861	2,398	414	1,112	185

Chapter 8 | OFFICER-LEVEL ANALYSES

Core Findings	***Overall:*** • ***Female officers make fewer stops.*** • ***More senior officers make fewer stops.*** • ***More senior officers search less, handcuff less, and arrest less (controlling for stops).*** ***Focusing on racial disparities in post-stop outcomes:*** • ***Officer seniority reduces the African American-White difference in handcuffing and arrests, but not searches.*** • ***Asian officers show less of an African American-White gap in searches.*** • ***African American officers show more of an African American-White gap in arrests.***

Up until this point, we have been analyzing 28,119 stops (or some subset of those stops). We have been counting and predicting the number of times that certain events, such as a search or an arrest, happened overall and as a function of race or other variables. It was a passive universe that we have been describing. The events were primary and the actors involved were in the background. In this chapter, we switch gears and the officer, as opposed to the stop, becomes the unit of analysis.

To focus on the officer, we reorganized the 28,119 stops into the actions of 510 individual officers who made at least one self-initiated stop during the 13-month time period of analysis. Rather than examining, for example, how many total searches were conducted, we can assign these searches to the individual officers who conducted them. Recall that on the stop data form, the primary officer who made the stop enters his or her employee ID, an internal Oakland Police Department (OPD) personnel tracking number. Using this tracking number, the research team can group unique stops by officer without knowing the identity of the officer and allowing our data set to remain anonymous.

The focus on the officer is an important component of the analysis of policing activity. We feel it is enlightening because it brings to life the experience of the people who make the decisions to stop members of the community. Focusing on the officer as the agent allows us to understand what characteristics officers who conduct a lot of searches, for example, might have in common. Most germane to our task at hand, we can also begin to pinpoint what factors are associated with officers who stop members of one racial group more often than they stop members of other groups.

Description of officers

A total of 510 officers were represented in the data set. Of these officers, 456 (89%) are men and 54 are women (11%). In terms of race, 43% of these officers are White, 22% are Hispanic, 17% are African American, 14% are Asian American, and 4% are listed as Other. 37 (7%) of officers who made at least one stop between April 1, 2013, and April 30, 2014, were listed as Oakland residents at the time that we collected this information (the fall of 2014), whereas the vast majority of officers, 93%, were not residents of Oakland. The average age of officers at the time they made the stops was 37 years (Median age = 37, Interquartile range (IQR) = [31 ; 43]). At the time the stops were made, the average years of experience on the force at the Oakland Police Department was nine years (Median years of experience = 7, IQR = [3 ; 14]).

Heterogeneity in officer activity

On average, officers made 55 stops during the 13 months of interest, or in other words only about one stop per week throughout the year (Median number of stops = 35, IQR = [9 ; 82]). Before going any further, it is important for the reader to recognize that we observed large variation in the level of activity of officers represented in the data set. Ignoring this could lead to erroneous interpretation of the data. In the four graphs below, we plot the overall share of stops, high-discretion searches, incidents of handcuffing (excluding arrests), and arrests that can be attributed to officers.

As you can see in Figure 8.1, the bulk of stops and post-stop activity is attributable to a small fraction of officers. In the graph in the top left panel, which represents stops, you will see that just 20% of the officers made 54% of all stops. These 100 members of the OPD made 96 or more stops each. Compare those numbers to the fact that approximately one-quarter of officers made only 10 stops or fewer during the entire 13 months. Continuing the trend, we see in the bottom left-hand corner panel that just 20% of officers were responsible for 67% of all instances of handcuffing. These 100 officers each handcuffed 11 or more people that they had stopped. In contrast, 22% of officers did not handcuff anyone during the entire 13 months. In the graph in the top right-hand corner, representing high-discretion searches (which exclude incident to arrest, inventory, probation/parole searches, as well as stops for probation/parole), we see that just 20% of officers conducted 70% of high-discretion searches. These 100 officers conducted nine or more searches each throughout the year. On the other side of the distribution, a full 28% of officers did not conduct a single high-discretion search during a self-initiated stop made between April 1, 2013, and April 30, 2014. Finally, in the graph in the lower right-hand corner, which represents arrests, we see that 20% officers made 70% of all arrests. These 100 officers each made 13 or more arrests during the period in question. Slightly more than one-quarter of officers, 27%, did not make any arrests. In sum, the bulk of the data set can be attributed to a small subset of officers.

Furthermore, making stops, handcuffing, conducting high-discretion searches, and making arrests are all very strongly correlated, as one might expect and as we see in the correlation table shown in Table 8.1. If these four activities are all part of normal policing strategy and they simply capture an officer's overall level of activity or productivity, then the strong correlations between them make sense. It is good to have in mind as one looks at the graphs in Figure 8.1 that the same officers are most likely on the same side of the distribution across the 4 graphs (e.g., the same officers who conduct many searches also make many arrests).

Figure 8.1. Share of overall stops attributable to officers

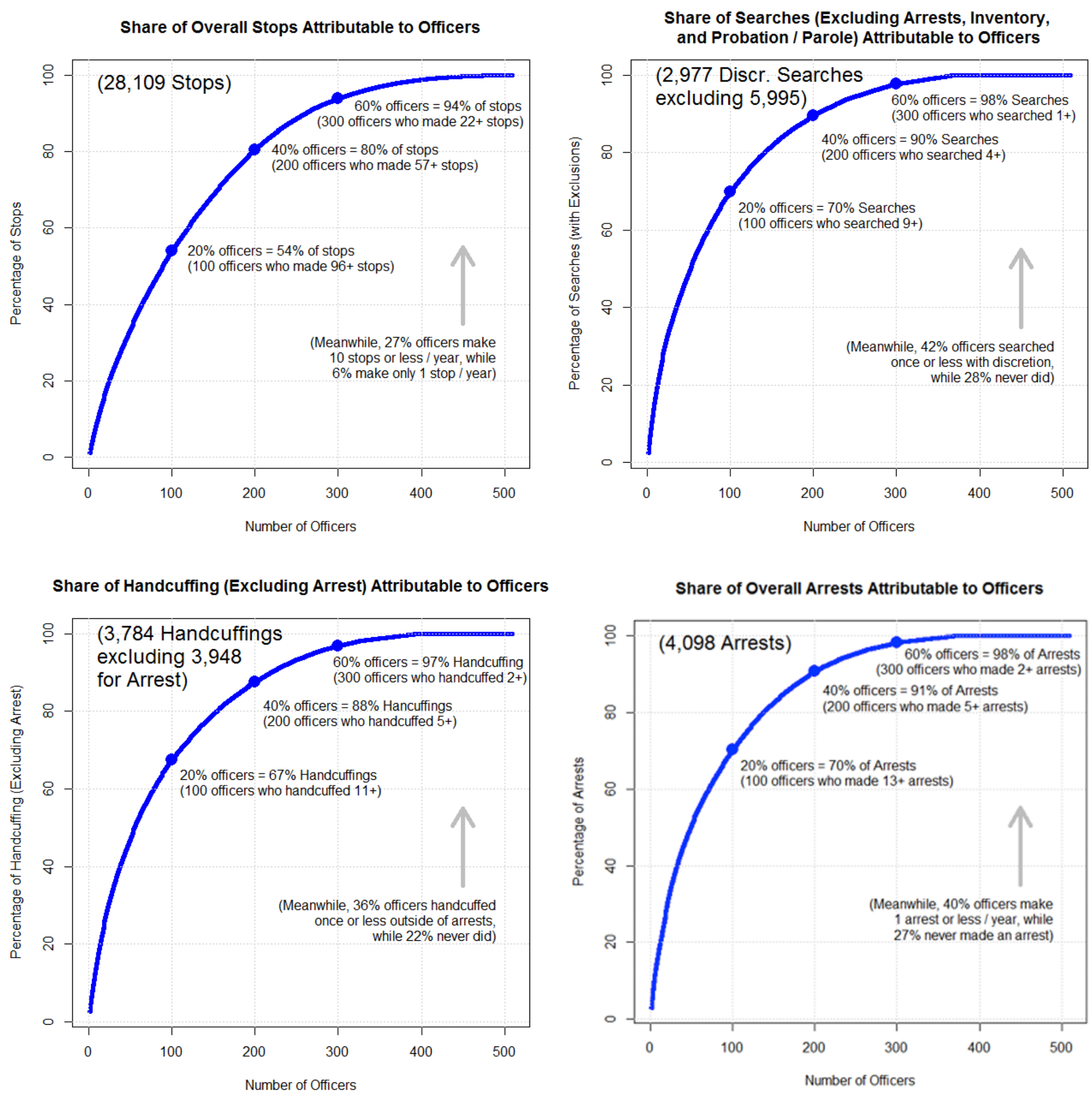

Table 8.1. Zero-order Pearson correlation coefficients between the totals of each activity for each officer

	All stops	All arrests	Searches	Searches subset	Handcuffed
All arrests	.64				
Searches	.70	.95			
Searches subset	.62	.82	.92		
Handcuffed	.69	.96	.99	.91	
Handcuffed subset	.68	.82	.95	.92	.95

Do officer demographics predict an officer's level of activity?

We conducted a regression analysis in which we predicted for each officer his or her total number of stops, high-discretion searches, instances of handcuffing in the absence of arrest, and arrests (see Table 8.2). We tested whether the officer's gender, race, residency in the City of Oakland, years of experience with the OPD, and/or age would predict an officer's activity. We found that a transformation of the data was necessary. Thus, we conducted the analysis using a log transformation on the behavioral frequencies (adding +.5 to the numbers to avoid log(0), which is not defined). This transformation also improved the multiple correlation coefficient (R^2) for all analyses, suggesting that these models are a better fit. Because the overall total number of stops predicted frequency of handcuffing, searching, and arrests, it was necessary to control for an officer's overall total number of stops when trying to predict officers' post-stop activity.

We begin with officer gender. We found that compared to male officers, female officers made significantly fewer stops. Note that by quickly glancing at Table 8.2, it appears that female officers were also significantly less likely than male officers to handcuff or to make arrests. Once officers' total number of stops were controlled for, however, these differences were reduced to nonsignificance, suggesting that, in general, female officers handcuffed people less and made fewer arrests not because they were less prone to engage in this type of policing activity, but because they overall made fewer stops and therefore necessarily had fewer instances in which they *could* handcuff or arrest people.

Next, we turn to officer race. We included in the model variables capturing whether an officer was African American, Asian, Hispanic, or of another race. White officers were considered the baseline. This is because White officers were in fact most represented in our data set (43% of all officers who made stops during the 13 months were White). We also made the decision to set White officers as the baseline group because many scholars and commentators discuss the need for more diversity among law enforcement, which usually translates into calls for people of color to join the ranks of mostly White police forces.[142] African American officers, Hispanic officers, and "Other" officers did

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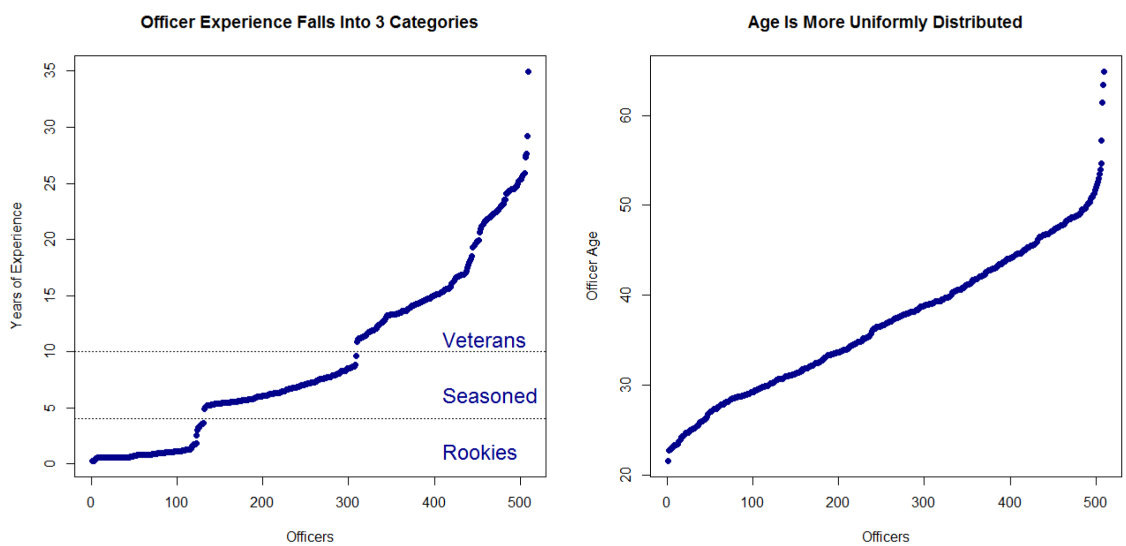
[142] Indeed, the majority of police officers in the United States are White. According to a report released by the Bureau of Justice Statistics (which is a part of the U.S. Department of Justice), 73% of the estimated 605,000 full-time employees who worked at more than 12,000 local police departments as of January 1, 2013, were White. The report did note that local police agencies have become more diverse over the past two decades. See: Reaves, B. A. (2015). Local police departments, 2013: Personnel, policies, and practices (No. NCJ 248677). US Department of Justice, Office of Justice Programs, Bureau of Justice Assistance, Washington, DC. A number of government task forces have made explicit calls to local police agencies to diversify their ranks. For example, recommendation 1.8 of the report put out by the President's Task Force on 21st Century Policing reads: "Law enforcement agencies should strive to create a workforce that contains a broad range of diversity including race, gender, language, life experience, and cultural background to improve understanding and effectiveness in dealing with all communities" (p. 16). See President's Task Force on 21st

not statistically differ from White officers in the rates at which they made stops, conducted high-discretion searches, handcuffed people who were not ultimately arrested, and made arrests. Asian officers, however, were somewhat (though not significantly) less likely than White officers to handcuff and arrest people after controlling for the total number of stops. Overall, Asian officers had similar absolute numbers of handcuffing and arrests as officers of all other races, but Asian officers seemed to stop people slightly more often (though not significantly so), which mathematically made the overall percentages smaller.

We found no effect on officer activity as a function of whether or not OPD officers lived in Oakland. Due to the small sample size of officers who were Oakland residents (7% of 510 officers), these results are inconclusive. Perhaps with larger numbers we would have been able to detect some trends. We caution our readers not to interpret this finding as any sort of "proof" that the calls some have made for officers to live in the places they work so that they have more at stake as members of the community themselves[143] are needed or unneeded.

Figure 8.2. Graphs showing the distribution of officer experience (left panel) and officer age (right panel)



Century Policing. 2015. *Final Report of the President's Task Force on 21st Century Policing.* Washington, DC: Office of Community Oriented Policing Services. See also: Principles of good policing: Avoiding violence between police and citizens. (2003). US Department of Justice, Community Relations Service, Washington, DC; Hailes Jr, E. A., & Manalili, J. (2000). Revisiting who is guarding the guardians? A report on police practices and civil rights in America. U.S. Commission on Civil Rights, Washington, DC.

[143] E.g., see Silver, N. (Aug. 20, 2014). Most police don't live in the cities they serve. *FiveThirtyEight.* Retrieved from http://fivethirtyeight.com/datalab/most-police-dont-live-in-the-cities-they-serve/; Meyer, J. P. (Jun 6, 2014). Meyer: Should cities have residency requirements for police officers? *Denver Post.* Retrieved from http://www.denverpost.com/2014/06/06/meyer-should-cities-have-residency-requirements-for-police-officers/

Because age and experience are highly correlated ($r = .78$) we will avoid putting them together in regression models when evaluating their effects. Because the regression coefficients computed in a regression model reflect the unique contribution of any predictor, inclusion of highly correlated ("collinear") predictors means that none of them may appear significant, because although they may actually all predict the outcome in parallel, none has significant *unique* contribution. When the emphasis is on controlling for these variables it thus makes sense to include them all. When assessing their (total) contribution, however, it is important to avoid redundancy among predictors. We categorized officer experience into "Rookies" (less than 4 years of experience), "Seasoned" (between 4 and 10 years of experience), and "Veterans" (more than 10 years of experience), because the distribution (see Figure 8.2), reveals big gaps in when officers were recruited to join the force that lead to three naturally occurring categories. Beside the fact that categorizing this variable sidesteps some distributional assumptions and facilitates interpretation, it is likely that the gaps in recruitment years create actual social groups. To the extent that social networks were built in the Academy or among officers with similar early assignments and a commonality of experience was developed, these groups are likely a social reality. Although there were actually more officers in the "Veterans" category, we selected the middle category, "Seasoned," as the reference group to ease interpretations. This enables us, as researchers, to be able to assess if there is a "Rookie" effect and a "Veterans" effect compared to officers who have served a more average number of years on the force.

Overall, as we see in Table 8.2, (1) rookies[144] stop marginally fewer people, which seems to explain the lower instances of handcuffing and searches, (2) rookies arrest significantly fewer people when controlling for the total number of stops, and (3) veteran officers stop fewer people, and handcuff, search, and arrest fewer people even controlling for number of stops.

The number of years officers have been with the OPD significantly predicted the number of self-initiated stops they made. We found that more senior members of the force made significantly fewer stops. As was the case for female officers, quickly looking at Table 8.2 might give the impression that more experienced officers were also significantly less likely than less experienced officers to search, handcuff, and arrest the people they stopped. As we saw previously, once officers' total number of stops were controlled for, however, these differences statistically disappeared, illustrating that more experienced officers were not less likely or less willing to conduct searches or handcuff people or make arrests, but because they overall made fewer stops and, consequently, had fewer opportunities to conduct as many searches, handcuff as many people, or make as many arrests.

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[144] Please note however that some of the people classified as "Rookies" were hired during the period under study, so they simply would have served fewer months, reducing the number of stops they could have possibly made.

Table 8.2. Regression table showing the prediction of log transformed officer activity (with regard to stops, high-discretion searches, handcuffing [with no arrest], and arrests) as a function of officer demographics

	Dependent variable:						
	All Outcome Variables Logged – log (x + .5)						
	Stops	Discretionary Searches		Handcuffing Excl. Arrest		Arrests	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Constant	3.861***	1.548***	-0.876***	1.860***	-0.773***	2.007***	-0.646***
	(0.119)	(0.115)	(0.153)	(0.113)	(0.139)	(0.119)	(0.152)
log(AllStops + 0.5)			0.628***		0.682***		0.687***
			(0.033)		(0.030)		(0.032)
GenderFemale	-0.460*	-0.127	0.162	-0.323+	-0.009	-0.461*	-0.144
	(0.190)	(0.183)	(0.140)	(0.180)	(0.126)	(0.189)	(0.138)
RaceAfr American	-0.051	-0.176	-0.144	-0.055	-0.020	-0.262	-0.227+
	(0.164)	(0.158)	(0.120)	(0.156)	(0.109)	(0.164)	(0.119)
RaceAsian	0.308+	0.003	-0.191	-0.017	-0.227+	-0.036	-0.248+
	(0.180)	(0.174)	(0.132)	(0.171)	(0.120)	(0.180)	(0.131)
RaceHispanic	-0.021	-0.049	-0.035	0.033	0.047	0.051	0.066
	(0.151)	(0.146)	(0.111)	(0.144)	(0.100)	(0.151)	(0.110)
RaceOther	-0.206	0.030	0.160	-0.019	0.122	-0.135	0.007
	(0.297)	(0.286)	(0.217)	(0.282)	(0.196)	(0.296)	(0.215)
OaklandResYes	0.186	0.176	0.059	0.159	0.032	0.240	0.112
	(0.227)	(0.219)	(0.166)	(0.215)	(0.150)	(0.226)	(0.164)
ExpcatRookie	-0.287+	-0.336*	-0.156	-0.323*	-0.127	-0.660***	-0.462***
	(0.151)	(0.145)	(0.111)	(0.143)	(0.100)	(0.151)	(0.110)
ExpcatVeteran	-1.242***	-1.077***	-0.297**	-1.314***	-0.467***	-1.494***	-0.640***
	(0.134)	(0.129)	(0.106)	(0.128)	(0.096)	(0.134)	(0.105)
Observations	510	510	510	510	510	510	510
R^2	0.178	0.134	0.502	0.194	0.610	0.219	0.589
Adjusted R^2	0.165	0.120	0.493	0.182	0.603	0.207	0.582
Residual Std. Error	1.295 (df = 501)	1.247 (df = 501)	0.947 (df = 500)	1.229 (df = 501)	0.856 (df = 500)	1.292 (df = 501)	0.938 (df = 500)
F Statistic	13.540*** (df = 8; 501)	9.675*** (df = 8; 501)	55.915*** (df = 9; 500)	15.112*** (df = 8; 501)	86.831*** (df = 9; 500)	17.583*** (df = 8; 501)	79.718*** (df = 9; 500)

Note: + p<.10; * p<.05; ** p<.01; *** p<.001

Do officer demographics predict racial differences in officer activity?

In some sense, all of the analyses presented thus far were a preamble to the primary question at hand: Do officers' demographic characteristics predict differences in their treatment of community members of different races? In this section, we focus on post-stop outcomes. Without information about which police beat individual officers were assigned to, it is impossible for us to interpret information about the racial breakdown of their stops. This issue, of course, is the same issue of eligibility that has come up time and again and that is the heart of the benchmark controversy. What would we expect the base rate of stops by race to be for any particular officer? Should officers stop members of all racial groups equally? In theory, maybe perfectly proportionate stopping would be desirable. Once we factor in that different officers work in different parts of Oakland, many of which have very different underlying demographic characteristics, crime rates, mixes of residential and commercial areas, etc., then this ideal quickly becomes more complicated. More concretely, suppose we find that some officers make one stop of an African American out of every 10 stops they make, whereas other officers make 9 stops of African Americans out of every 10 stops they make. Are those numbers evidence that the latter group of officers is necessarily biased, whereas the former group is egalitarian? The answer to that question is and should be a resounding no. We simply cannot know because, for starters, we do not know in which neighborhoods the officers were working or what their assignments were. We are unable to determine at the officer level what factors might lead officers' stop data to show an apparent pattern of racial disparity in the rates at which they stop African Americans as compared to Whites. We refer the reader to our stop analysis, described in Chapter 3, for an examination of what other factors might be driving racial disproportionality in stops.

As we explained in the methodology chapter, an examination of post-stop outcomes, however, suffers less from the benchmark problem since the pool of eligible people has already been defined and necessarily consists of those who have already been stopped by police.[145] Therefore, we can figure out if officers differ in their tendency to search, handcuff, and arrest African Americans (relative to Whites) and what officer characteristics predict that variability in treatment by race. We focus specifically on the difference between African Americans and Whites, in part, because this is the comparison that most stakeholders are primarily interested in and is the comparison of interest for the majority of social science research on differences in myriad outcomes by race. Furthermore, in the previous 3 chapters, we found that the most consistent patterns of racial disparities in post-stop were between Whites and African Americans. Differences between stops of Whites and Hispanics, in contrast, tended to be statistically non-significant.

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[145] Please note Ayres and Borowsky's (2008) critique of this point and their method for establishing rates of post-stop outcomes by race that are not contingent on first having been stopped.

For a given officer, which officer characteristics are associated with significant gaps in search rates, handcuffing rates, and arrest rates between Whites and African Americans? Our previous exploration of the substantial heterogeneity, or variation, in how active the officers are as a group suggests that an analysis of racial disparities in post-stop outcomes at the level of the officer will be constrained to a small subset. But note that when we compare how given officers treat White or African Americans once they are stopped, we are implicitly assuming that for each officer we have a sample of White stops and a sample of Black stops that can be compared side by side. This, however, is largely a fiction. Many officers never stop a White person, and very few officers actually stopped a sizeable number of both Black and White community members.

For example, of the original 510 officers:

- A full 104 (20%) never stopped a White person, 20 (4%) never stopped an African American, and 78 (15%) never stopped a Hispanic person during the 13 months of interest.
- As a result, only 399 officers (78%) have stopped both at least one White and one African American person.
- Only 190 officers (37%) stopped five or more White people and five or more African American people.
- Only 106 officers (21%) stopped 10 or more White and 10 or more African American people.

These numbers are further reduced when we exclude stops that led to an arrest (as is customary for our handcuffing analyses) or stops made because the person was on probation/parole, or stops that included an incident to arrest search, inventory search, or probation/parole search (as is customary for our high-discretion search analyses). If some officers stop only or primarily African American community members, this would obviously constrain the racial distribution of the people they ultimately search, handcuff, and/or arrest. The specific focus here, then, must be whether officers who stopped a similar sample of Whites and African Americans, presumably under similar conditions, *ultimately treated these community members differently as a function of their race.* As we have been doing all along, we again focus on the three outcomes of handcuffing (when no arrest was ultimately made), high-discretion searches (which exclude incident to arrest, inventory, and probation/parole searches, as well as stops made because of probation/parole), and arrests.

Before we get into the results of our examination of officer-level racial disparity in post-stop outcomes, let us first describe how we constructed the sub-sample of the data that we use in our analyses. Recall that the median number of stops for each officer in the full sample was 35. To be able to fully investigate whether there is any racial disparity in the treatment of people who have been stopped, the officer must have had sufficient opportunity to engage in the behaviors of interest (i.e., handcuffing, searching, and arresting). In other words, the officer must have stopped a sufficient number of African Americans and Whites. Consequently, we arbitrarily defined a "sufficient" number of stops as 10. We accordingly restricted our analysis to officers who made at least 10 stops of Whites and 10 stops of African Americans over the course of the 13-month period. This exclusion rule retains only 106 officers (21% of officers), but a full 12,369 of the stops (44% of the stops), though these figures include stops of Hispanics, Asians, and Others, which will effectively not be included in the analyses below, because they exclusively compare treatment of Whites and African Americans. This universe consists of 9,001 stops (32% of all total stops) made by officers who each made at least 10 stops of Whites and 10 stops of African Americans. Of these stops, 6,581 stops were of African Americans and 2,420 stops were of Whites. Note right from the beginning that this sample is more skewed than the full data set. Overall, 60% of all 28,119 stops were of African Americans. Of this subsample, 73% of stops were of African Americans. This shift is a byproduct of our exclusion rules.

Description of the officers included in this subset of the data

A total of 106 officers were represented in the subset of the data used to conduct the analyses below. Of these officers, 95 (90%) are men and 11 are women (10%). In terms of race, 40% are White, 17% are Hispanic, 19% are African American, 22% are Asian American, and 3% are listed as Other. Only 8 (8%) are listed as Oakland residents, while the remaining 92% are non-Oakland residents. The average age of these officers at the time the stops were made was 36 years (Median age = 34 years, IQR = [23 ; 43]). The average number of years of experience they had on the force at the Oakland Police Department was 8 years (Median experience = 6 years, IQR = [1 ; 14]). Notice that for all of these demographic variables, the officers contained in the subset look very much like those in the full data set, except that the average number of stops in the subset was (obviously) higher. At 117, the average number of stops in the subset was double the average number of stops (55) in the full set (Median number of stops in the subset = 96 (IQR = [73 ; 146]). These figures include stops of members of racial groups other than African Americans and Whites, which will effectively be excluded in the analyses below.

Table 8.3. Descriptives of how often officers (included in the subset of officers who made at least 10 stops of Whites and 10 stops of African Americans) engaged in handcuffing, searching, and arresting as a function of the race of the person who was stopped

	Whites			African American		
	Mean (SD)	Median	Never	Mean (SD)	Median	Never
Handcuffing (no arrest)	.06 (.10)	.02	51 (48%)	.13 (.11)	.12	11 (10%)
Search (discretionary)	.04 (.08)	.00	64 (60%)	.10 (.10)	.07	17 (16%)
Arrest	.08 (.13)	.11	53 (50%)	.12 (.11)	.09	15 (14%)
Number of stops	23 (15)	17		62 (42)	52	

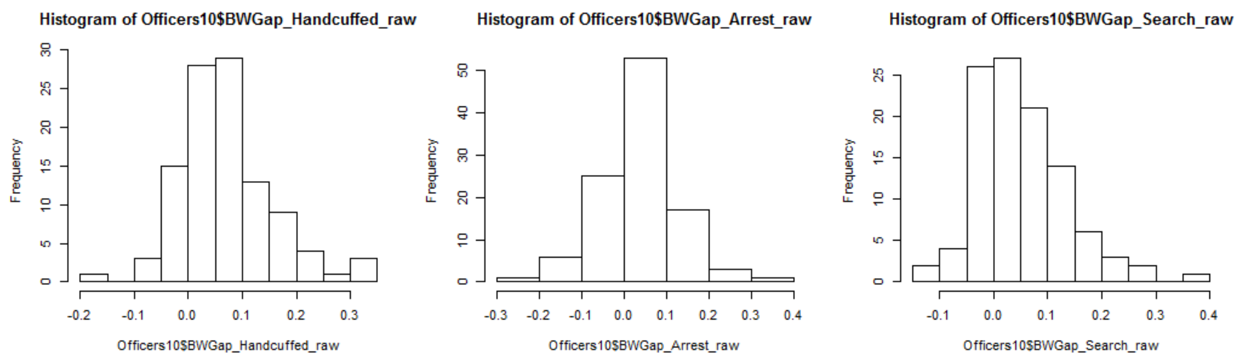
Note: The percentages above have different denominators. "Arrest" is relative to all stops of Whites and African Americans, respectively. But "Handcuffing" is relative to the number of stops for that race which did not result in an arrest being made for that officer. And "Search (Discretionary)" is relative to stops that were not caused by Probation/Parole as the reason for the stop, and that did not result in an Incident to Arrest, Inventory, or Probation/Parole search. Again, "Handcuffing" rates exclude any stop resulting in an arrest, but "Search" rates retain stops that resulted in an arrest if that arrest did not trigger an "Incident to Arrest" search.

To illustrate how to read Table 8.3, let us look at handcuffing for Whites. For each of the 106 officers in this subset we computed the percentage of their White stops that led to handcuffing in the absence of an arrest. Averaging these percentage scores across the 106 officers, we get .06, a standard deviation (SD, capturing variability) of .10. The median percentage was .02, suggesting that half of the officers (53) handcuffed fewer than 2% of Whites, while the other half handcuffed more than 2% of Whites. Indeed, the next column reports that 51 of these officers (48% of them) never handcuffed a White person—so their percentage would be 0%. The "Number of Stops" row is different, as these are the actual averages, SDs, and median numbers of stops of Whites and African Americans across the 106 officers in the subset.

Despite our best efforts to select a subset that purposely oversampled White stops, note in Table 8.3 that we have cause for concern about the statistical robustness of this data. We found that half of these active officers, in the context of self-initiated stops made over a 13-month period, never handcuffed a White person who was not under arrest, never discretionarily searched a White person, and never arrested a White person. Note that 48% of officers included in this subset never handcuffed a White person who was not under arrest, compared to only 10% of officers who never handcuffed an African American person who was not ultimately arrested. Likewise, 60% of subset officers never searched a White person for probable cause, for weapons, or after he or she gave consent (and probation/parole was not the reason for the stop), compared to only 16% of officers who never searched an African American person under those same conditions. Finally, 50% of subset officers never arrested a White person, compared to only 14% of officers who never arrested an African American person. Remember in interpreting this last result that we are including both felony and misdemeanor arrests here. Misdemeanor arrests can be triggered by relatively minor infractions like disorderly conduct.

To get around this limitation of the data, we created for each officer in this sample a difference score in the rate of handcuffing, searching, and arresting between African American and White community members. In other words, for handcuffing, we took the percentage of the officer's African American stops that involved handcuffing (excluding arrests) and subtracted out the percentage of the officer's White stops that involved handcuffing (excluding arrests). The difference between the treatment of African Americans and Whites served as our measure of racial disparity. We repeated this process for discretionary searches and for arrests. Rather than trying to predict the absolute number of times an officer had engaged in each type of behavior during stops of African Americans as compared to during stops of Whites, we instead tried to predict the relative difference in treatment of African Americans and Whites as a function of officer variables.

Figure 8.3. Histograms of difference scores capturing the African American-White gap in officers' rates of handcuffing (without arrest), high-discretion searching, and arrests



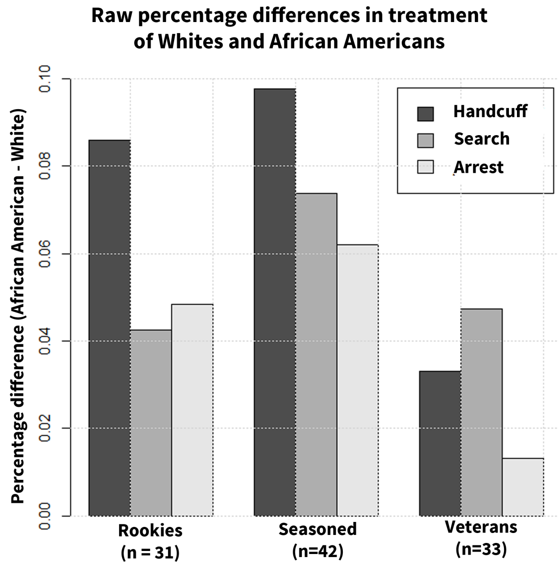
Note: To be included in this subset, officers must each have made at least 10 stops of Whites and 10 stops of African Americans.

We created graphs of each of these 3 differences scores. In Figure 8.3 above, we see that the difference scores are normally distributed and not skewed like the raw numbers were. Having established the suitability of our new dependent variables, we moved forward with testing whether or not officer demographics predict the African American-White gaps in each of our post-stop outcomes.

In Table 8.5, we show regression analyses predicting the race gap in post-stop outcomes, by officer, for the 106 active officers who stopped at least 10 African American and 10 White community members.

The bar plot seen in Figure 8.4 shows the average "race gap" scores that are modeled in Table 8.5 (see columns 3, 6, and 9), broken down by experience group, but not controlling for other variables in the regression model (raw scores). We see, for example, that the race gap in arrests all but disappears for veterans, and that the handcuffing gap is also drastically reduced for veterans, despite the fact that these are still active veterans, who stopped at least 10 White and 10 African American citizens. As Table 8.5 shows, this does not seem to be a result of a drastic change in denominators, because in this subset of active officers, even veterans stopped a sizeable number of White and African American community members.

Figure 8.4. Graphs showing the degree of African American-White difference in post-stop outcomes as a function of officer seniority



We found that African American officers (19% of this subset) show more of a race gap in arrest rates of Whites and African Americans, which appears to be driven by the fact that African American officers were less likely to arrest Whites. Veterans show less of a race gap in handcuffing and arrests, in both cases because they handcuffed and arrested African Americans significantly less (see Table 8.4 for more information about these officers).

Officer gender and officer residency were *not* associated with the size of the African American-White gap for any of the three post-stop outcomes (high-discretion searches, handcuffing, or arrests). Furthermore, we found that Hispanic officers, Asian officers, and officers whose race was categorized as "Other" did not show African American-White gaps in any post-stop outcome that were significantly different from that of White officers.

Table 8.4. Descriptives of demographics and how often officers (included in the subset of officers who made at least 10 stops of Whites and 10 stops of African Americans) made stops overall, White stops, and African American stops as a function of officer seniority

Snapshot of "Active" Officers (who stopped more than 10 African American and 10 White persons)					
		Rookies (Exp < 4 yrs)	Seasoned	Veterans (Exp > 10 yrs)	Overall
n		31	42	33	106
Gender	% Male	.77	.90	1.00	.90
Race	% White	.45	.36	.39	.40
Age	Mean	28	36	44	36
Stops	Mean	132	106	116	117
	Median	124	88	91	96
	IQR	90 - 168	70 - 129	63 - 145	73 – 146
White Stops	Mean	22	19	29	23
	Median	20	16	19	17
	IQR	14 - 28	12 - 22	14 - 37	13 – 27
African American Stops	Mean	74	61	52	62
	Median	67	49	38	52
	IQR	47 - 98	36 - 68	23 - 61	33 -85

Note: IQR = Interquartile Range, the location of 50% of the group between the 25% and the 75% quartiles. It is a rough indicator of variability that makes no assumptions about distribution.

Table 8.5. Regression table showing the prediction of post-stop activity as a function of officer demographics

	Dependent variable:								
	Handcuffing			Search			Arrest		
	White	Afr Am	Diff	White	Afr Am	Diff	White	Afr Am	Diff
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Constant	-.041	.128	.169*	-.124+	.017	.141+	-.015	.093	.108
	(.085)	(.087)	(.074)	(.067)	(.085)	(.073)	(.116)	(.094)	(.077)
Gender - Female	-.046	-.048	-.003	-.028	-.059+	-.031	.003	-.026	-.030
	(.033)	(.034)	(.029)	(.026)	(.033)	(.029)	(.045)	(.037)	(.030)
Race - Afr American	-.001	-.003	-.001	.013	.001	-.012	-.078*	-.006	.073**
	(.027)	(.028)	(.024)	(.022)	(.028)	(.024)	(.037)	(.030)	(.025)
Race - Asian	-.017	-.038	-.021	.009	-.027	-.036+	-.037	-.040	-.002
	(.025)	(.026)	(.022)	(.020)	(.025)	(.022)	(.034)	(.028)	(.023)
Race - Hispanic	.008	.026	.018	.011	.025	.015	-.011	.002	.013
	(.027)	(.028)	(.024)	(.021)	(.028)	(.024)	(.037)	(.030)	(.025)
Race - Other	-.028	-.034	-.005	-.005	-.005	.0004	-.026	.011	.037
	(.058)	(.059)	(.050)	(.045)	(.058)	(.050)	(.079)	(.064)	(.052)
Oakland Resident - Yes	-.041	-.060	-.019	-.038	-.035	.003	-.017	-.022	-.005
	(.036)	(.037)	(.032)	(.029)	(.037)	(.031)	(.050)	(.040)	(.033)
Experience - Rookie	-.017	-.016	.001	-.013	-.031	-.018	-.082*	-.073*	.009
	(.025)	(.026)	(.022)	(.020)	(.025)	(.022)	(.035)	(.028)	(.023)
Experience - Veteran	-.041+	-.104***	-.063**	-.045*	-.074**	-.029	-.049	-.100***	-.051*
	(.023)	(.023)	(.020)	(.018)	(.023)	(.019)	(.031)	(.025)	(.020)
log(Stops + 0.5)	.028	.012	-.016	.040**	.027	-.013	.034	.020	-.014
	(.018)	(.018)	(.016)	(.014)	(.018)	(.016)	(.025)	(.020)	(.016)
Observations	106	106	106	106	106	106	106	106	106
R^2	.096	.238	.138	.157	.171	.092	.119	.194	.151
Adjusted R^2	.012	.167	.058	.078	.093	.007	.036	.119	.072
Residual Std. Error (df = 96)	.096	.098	.083	.075	.096	.082	.130	.105	.086
F Statistic (df = 9; 96)	1.136	3.339**	1.713+	1.982*	2.197*	1.082	1.439	2.569*	1.900+

Note: + p<.10; * p<.05; ** p<.01; *** p<.001

Analysis of race differences at the level of the officer

We were struck by the fact that a majority of officers in our subset (which itself was designed to oversample officers who had stopped Whites) had simply not had the experience of treating Whites the way they routinely treated African Americans in the context of self-initiated stops made over the course of 13 months. In Table 8.6, we present the proportion of officers who **never** performed a given behavior during our time period of interest. These figures can also help us establish a sense of what behaviors are “normal” or “typical” or perhaps even considered “appropriate” among varying sets of sworn officers at the OPD. Examining Table 8.6, for example, we see that 77% of officers never searched a White person who was not arrested, the subject of an inventory search, or identified as being on probation/parole. This number remains high at 60% if we look exclusively at officers who made at least 10 stops of African Americans and 10 stops of Whites. Yet 65% of officers have conducted a discretionary search of an African American community member. Similarly, during the 13 months, 74% of officers never handcuffed a White person who was not arrested, but 72% of officers did handcuff an African American person who was not ultimately arrested. The fact that 20% of officers never stopped a White person might be explained by legitimate reasons; for instance, they may have been assigned to a predominantly African American neighborhood with fewer opportunities to stop a White person.

The possibilities for post-stop outcomes, of course, are necessarily limited to the people who have already been stopped. We present the “all officers” data for completeness, but of course in that case some of the low percentages come from the fact that some officers simply did not have the opportunity to search a White person because they never even stopped someone who was White. Subsetting the data to include only officers who made either at least five or 10 stops of both Whites and African Americans gets around this issue of a lack of opportunity, but it is still an issue we will return to because African Americans are so overrepresented in all officers’ stops. Even among the more active subset of officers who made at least 10 stops of African Americans and 10 stops of Whites, we see that a majority (60%) never discretionarily searched a White person, whereas 84% of these same officers had conducted at least one discretionary search of an African American person. Examining the rates of handcuffing in the absence of an arrest tells a similar story: 48% of these very active officers never handcuffed a White person they were not arresting, whereas 90% of these same officers did this with an African American person. Finally, we see the same pattern for arrests. Among officers who made at least 10 stops of Whites and 10 stops of African Americans, 50% of them never arrested a White person in the entire 13 months, but 86% of them had arrested an African American person.

Table 8.6. Table showing the proportion of officers who never engaged in various post-stop activities as a function of the race of the person stopped and the activity level of the officer

Proportion of officers who NEVER…	All Officers N = 510		Officers who made at least 5 stops of Whites and 5 stops of African Americans N = 190 (37%)		Officers who made at least 10 stops of Whites and 10 stops of African Americans N = 106 (21%)	
	White	Afr Am	White	Afr Am	White	Afr Am
Stopped	.20	.04	--	--	--	--
Searched	.56	.17	.28	.05	.28	.05
Discr. Search	.77	.35	.60	.15	.60	.16
Handcuffed	.58	.20	.31	.06	.29	.08
Handcuffed, no arrest	.74	.28	.54	.09	.48	.10
Arrested	.70	.33	.52	.14	.50	.14

In Table 8.7, we break this down by the proportion of officers in the subset who never engaged in various activities (with only 10% women in this subset, we did not look at gender). For every one of the 40 comparisons in this table, the "White" percentage is higher than the "African American" percentage. However you slice it (admittedly, some of the comparisons in the table are not orthogonal), officers are more likely during the 13 months to have never done the behavior to a White person than to an African American person. We see a pattern whereby rookie officers are still more likely to search, handcuff, and arrest across the board, while veterans are less so, which can of course result from the beat or area to which an officer is assigned. But at every level of experience we see that more officers never exhibited a given behavior toward a White person than toward an African American person. Race of the officer also seems to have an impact but we continue to see the difference in behavior toward Whites stopped and African Americans stopped across officers of all races. African American officers were especially likely during the 13 months to never search (45%/60%), handcuff (45%/50%), or arrest (70%) a White person, whereas they were between 85% and 100% likely to have done so with at least one African American person.

Table 8.7. Table showing the proportion of officers who never engaged in various post-stop activities as a function of the race of the person stopped and the seniority of the officer

		Percent of active officers who NEVER...							
		By experience of officer			By race of officer*				
		Rookies	Season.	Veterans	White	Afr Am	Asian	Hispan	Overall
n		31	42	33	42	20	23	18	106
Searched	White	.19	.19	.48	.21	.45	.22	.33	.28
	Afr. Amer.	.00	.02	.12	.05	.00	.09	.06	.05
Discr. search	White	.45	.57	.79	.60	.60	.43	.83	.60
	Afr. Amer.	.00	.17	.30	.12	.15	.13	.33	.16
Handcuffed	White	.19	.21	.48	.26	.45	.26	.22	.29
	Afr. Amer.	.03	.05	.15	.07	.05	.17	.00	.08
Hand. no arrest	White	.39	.45	.61	.52	.50	.35	.50	.48
	Afr. Amer.	.03	.05	.24	.12	.05	.17	.00	.10
Arrest	White	.48	.40	.64	.50	.70	.43	.39	.50
	Afr. Amer.	.06	.07	.30	.10	.15	.22	.17	.14

* Left out 3 officers listed as "Other."

One legitimate rejoinder to the analysis showing that for many officers handcuffing, or searching, or arresting a White person is something that simply isn't done is that it is just a consequence of numbers—that even using the criterion of at least 10 stops of each group, officers in this subset still stop on average three times as many African Americans as Whites. This is undeniably part of the pattern. We want to argue that this is part of the effect: regardless of the reason, the phenomenon is that half of these active officers never discretionarily searched a White person (60%), never handcuffed a White person when an arrest was not made (48%), or never arrested a White person during a routine stop (50%), whereas more than 84% of them have done so with an African American person.

Chapter 9 | CONCLUSIONS AND FUTURE DIRECTIONS

What did we find?

We found that Oakland Police Department (OPD) officers significantly overstopped African Americans, even after we statistically controlled for the percentage of African Americans living in a given neighborhood. This effect was more pronounced when officers indicated that they could determine the race of the person before making the stop. We also found significant race differences in post-stop outcomes. OPD officers were more likely to handcuff African Americans during a stop than they were Whites, even after excluding arrests from our analyses. OPD officers were also more likely to search African Americans, even after excluding searches that result from an incident to arrest, inventory, or probation/parole. Although officers were more likely to search African Americans, they were no more likely to find contraband on African Americans, as compared to community members of other races. OPD officers were also more likely to arrest African Americans. These differences in handcuffing, searching, and arresting remain even after controlling for a host of variables known to influence policing, including neighborhood crime rates.

The location of the stop also influenced the level of racial disparities, with race effects being stronger in some areas of the city than others. Race effects were also stronger for some officers than for others. The degree of racial disparities in handcuffing and arrests, for example, was less for more experienced officers than for less experienced officers.

Finally, we found that what officers typically do during the course of a stop differs by the race of the community member. For example, only 23% of OPD officers who made stops during our 13-month period discretionarily searched a White person, while 65% searched an African American person. Likewise, 26% of these officers handcuffed a White person who was not ultimately arrested, yet 72% handcuffed an African American person who was not ultimately arrested.

What do the findings mean?

We show clear race effects across stops, handcuffings, searches, and arrests, even when we take into account factors that are known to influence policing. Why do those race effects exist? In particular, how much does racial bias drive these effects? We know that many people associate African Americans with criminality. In fact, one of the strongest stereotypes of African Americans in American society is that they are dangerous, violent, aggressive—criminal. Might this stereotypic association cause differences in treatment by police?

This is certainly a possibility, one that some readers may automatically assume by looking at the sheer magnitude of the disparities we have found. It may then seem that the OPD's next step is to identify and screen out biased people who apply to be officers, and to punish biased officers who have managed to join the ranks.

But we believe that many other factors better explain the racial disparities we found here. Even if we showed a link between racial bias and disparate policing, we could not determine whether the bias caused the disparate treatment, or the disparate treatment caused the bias. Our behaviors not only reflect our attitudes and beliefs; but also *shape* our attitudes and beliefs.

How officers understand the expectations of their supervisors might also influence officers' decisions about whom to stop and how to treat the people they stop. That is, even when officers exhibit large racial disparities in whom they stop, these officers may not be acting on their own biases, but rather on what they believe their superiors expect.

In other words, racial disparities in stops and stop outcomes could emerge for any number of reasons. Yet even if racial disparities exist for a completely benign reason, they are still a serious problem because the OPD is falling short of its value of *equal treatment* under the law.

To be clear, though: our results do not suggest that OPD officers are "racists." Our mission is not to point fingers at specific individuals, but to explore an institution's effects on its communities, particularly its communities of color. Our exploration revealed that racial disparities in the OPD's activities are widespread and systemic.

What should the OPD do? Many people believe that law enforcement agencies just need to weed out a few bad apples (i.e., the "bad" or "racist" cops) and racial disparities will disappear. The social science research, however, simply does not support this belief. Much racial bias is implicit, meaning that people do not even know they have it. People often have egalitarian values and still unknowingly disadvantage some people and favor others. Implicit bias is transmitted by subtle cultural cues that are all around us. Removing a few bad apples would not address the fact that three-quarters of all OPD officers making stops in a 13-month period never handcuffed, searched, or arrested a White person, but the majority of officers performed these actions with African Americans.

These findings are not evidence of a few or even many bad apples, but of pervasive cultural norms—the unwritten rules of how to behave—about how to police people of different races. Focusing on individual officers, rather than on the culture as a whole, will likely allow racial disparities in policing to persist. Put another way, focusing on the individual officer may let law enforcement agencies, especially their leaders, off the hook too easily. Instead, to combat racial disparities in the treatment of community members, law enforce agencies must challenge the cultural beliefs, policies, practices, and norms that encourage disparate treatment. The leaders of law enforcement agencies are best positioned to examine these cultural features and change the problematic ones. The OPD leadership has begun to answer this call for cultural change.

APPENDICES

Appendix A: Examples of Stop Data Forms

Oakland Police Department
Stop-Data Collection Form

Could you determine the race/ethnicity of the individual(s) prior to the stop? Yes

STOP CATEGORY	DATE	TIME	INCIDENT NO.	RD NO.	CITATION NO.
Self-Initiated	01/01/2014	1458	LOP140101000692	14-000083	

SPECIAL ASSIGNMENT
No

SPECIFIC STREET LOCATION	BEAT
600 Blk Of E.18th St , Oakland, CA 94606	17X

ENCOUNTER TYPE	INITIAL REASON FOR ENCOUNTER	RESULT OF ENCOUNTER
Pedestrian	Consensual Encounter	FI Report

PERSON ENCOUNTERED / PRESENT AND SEARCH INFORMATION

Instructions:

1. Complete the boxes for race, gender, age, and if an Oakland resident for all individuals encountered.
   a. Use the following Race Codes: W - White; A - Asian; B - Black; H - Hispanic; I - Native American; P - Pacific Islander; M - Middle Eastern; O - Other
   b. Use the following Age Group Codes: A - Under 18; B - 18-29; C - 30-39; D - Over 40
2. Check the appropriate box whether a search was conducted and regarding the type of search for all individuals. (Check all that apply)

	Race Code	Gender	Age Group Code	Oakland Resident	Search Conducted	Consent	P/C	Prob./ Parole	Incident to Arrest	Inventory	Search Warrant	Weapons
P1	B	● M ○ F	B	●	○ Yes ● No	○	○	○	○	○	○	○
P2		○ M ○ F		○	○ Yes ○ No	○	○	○	○	○	○	○
P3		○ M ○ F		○	○ Yes ○ No	○	○	○	○	○	○	○
P4		○ M ○ F		○	○ Yes ○ No	○	○	○	○	○	○	○
P5		○ M ○ F		○	○ Yes ○ No	○	○	○	○	○	○	○
P6		○ M ○ F		○	○ Yes ○ No	○	○	○	○	○	○	○
P7		○ M ○ F		○	○ Yes ○ No	○	○	○	○	○	○	○

DURATION OF ENCOUNTER (In Minutes)	RESULT OF SEARCH
20-30	

PRIMARY OFFICER SERIAL NO.	COVER OFFICER SERIAL NO.

SUPERVISOR SERIAL NO.

TF-3232E

Oakland Police Department

455 - 7th Street
Oakland, CA 94607

Field Interview

RD #	CAD INCIDENT	LOCATION TYPE	CONTACT DATE	CONTACT TIME
14-000083	LOP140101000692		01 JAN 14	1458

LOCATION
600 Blk Of E.18th St , Oakland, CA 94606

PERSON #1	PERSONS NAME (LAST, FIRST, MIDDLE, SUFFIX)	SEX	RACE	D.O.B.	AGE
		M	B		26

ALIAS NAME (LAST, FIRST)	ROLE TYPE	ETHNICITY
	Subject	Not of Hispanic Origin

SSN	DL STATE	DL NUMBER	DL EXP	HEIGHT	WEIGHT	HAIR	EYES
	CA			6'0"	170	BLK	BRO

PLACE EMPLOYED/SCHOOL/UNION LOCAL NO./ETC.	EMPLOYER

ADDRESS	STREET NAME		
CITY		STATE	ZIP
Oakland		CA	94606

BUS ADDRESS	STREET NAME		
CITY		STATE	ZIP

CONTACT	HOME PHONE	CELL PHONE	BUS PHONE	EXT	PAGER	CODE

DETAILS: ☐ DRIVING ☐ PROBATION ☐ TRANSVESTITE ☐ PAROLE ☐ 647bPC

PFN/JFN	GANG NAME
BEX296	

CHARACTERISTICS	TYPE	DESCRIPTION
	Clothing	Grn hat, gray vest, whi pants
	Complexion	Dark
	Facial Hair	Unshaven

SMTI	SMTI TYPE	SMTI DESCRIPTION
	Tattoo	"Star" Tattoo Left Cheek On Face, "Flower" Right Cheek

Oakland Police Department

455 - 7th Street
Oakland, CA 94607

Field Interview

RD #	CAD INCIDENT	LOCATION TYPE	CONTACT DATE	CONTACT TIME
14-000083	LOP140101000692		01 JAN 14	1458

LOCATION

600 Blk Of E.18th St , Oakland, CA 94606

Narrative

Summary:

On 01 Jan 14 at approx. 1458 hrs, I (1L17) was wearing full police uniform and driving marked veh 1721. I have been in contact w/ a local merchant ( ) who has had issues w/ petty thefts and robberies on the 1800 - 1900 blk of Park Blvd. I was told by that a certain individual was seen on video (MB 20's, 6'0, 170LBS with long dreadlocks) being involved w/ the crimes. I also know that Investigator : needed FC's on a similar per a DB item from 30 Dec 13.

On today's date, I was on routine patrol on the 600 blk of E.18th St when I saw two MB's walking EB on the south sidewalk. I saw a MB that looked similar to the above description and decided to make consensual contact.

I made contact w/ Subj's and and advised them of my intentions to speak w/ them consensually. They both were receptive to my request and gave me his CDL and provided his personal info to Ofc.

I was able to determine that PFN # was and his Crims photo matched his person. was not active to Probation or Parole at this time.

vas positively ID'd by his CDL and I was able to confirm his PFN # ol was active to Probation (245(a) PC) w/ a full way search clause via Wants / Warrants. I decided to invoke robation status and searched his person w/ negative results.

Both and were not handcuffed and were cooperative w/ our requests.

Both subjects were 937C, FC'd, ID'd and released.

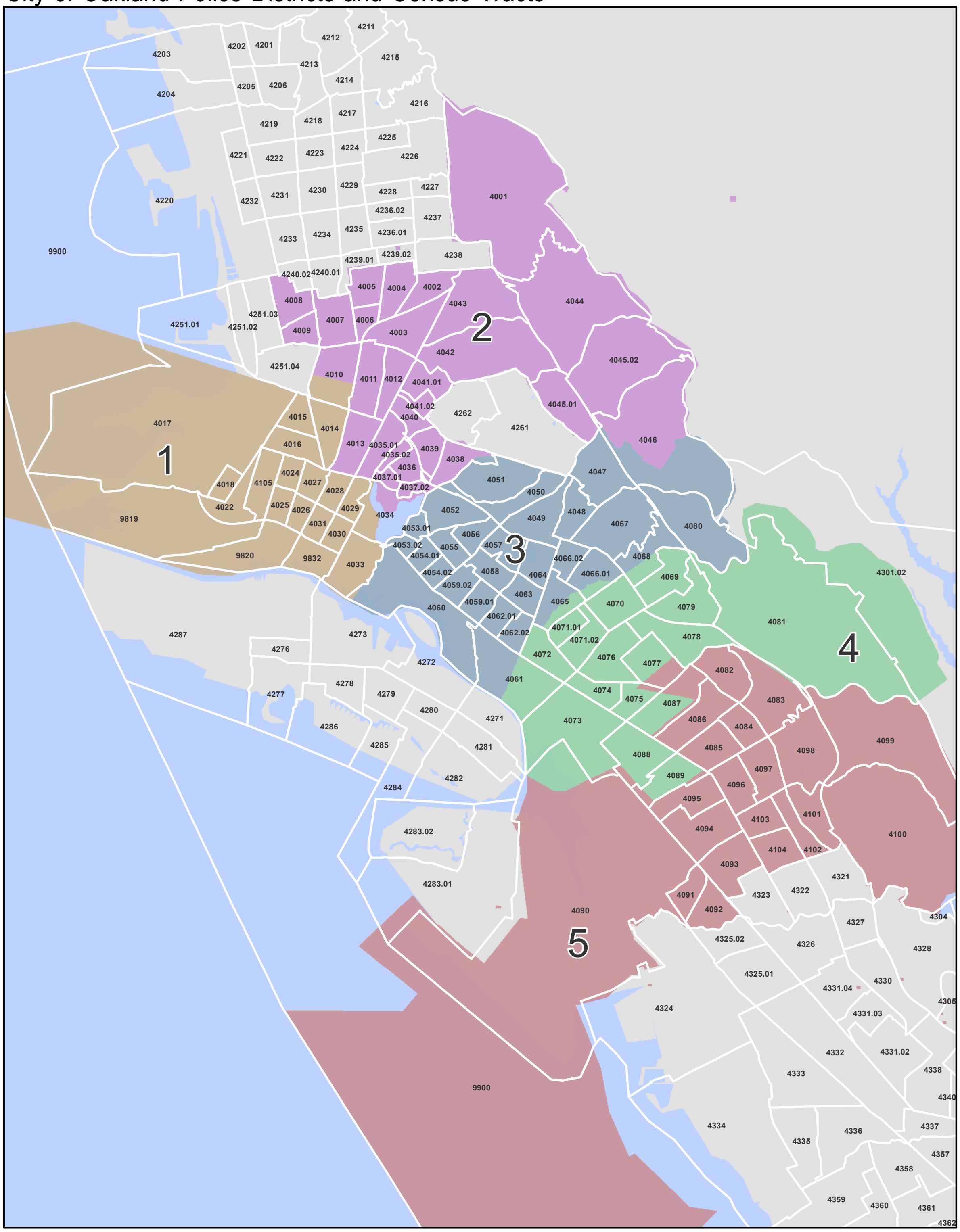
No Other Known Witnesses.

PDRD activated.

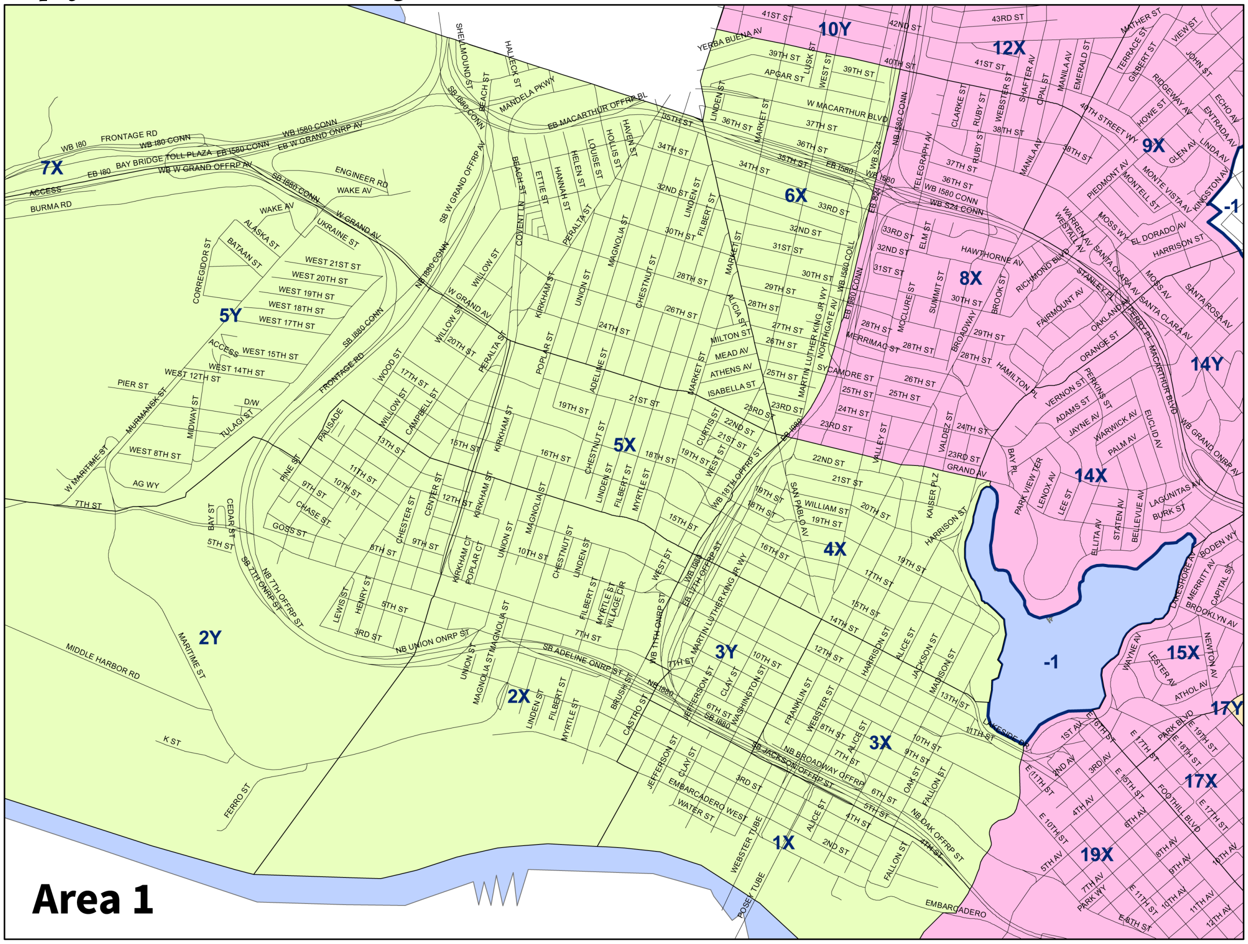
Appendix B: Maps of Oakland

Map of Oakland with Census Tracts and OPD Policing Areas (aka Districts) Overlaid

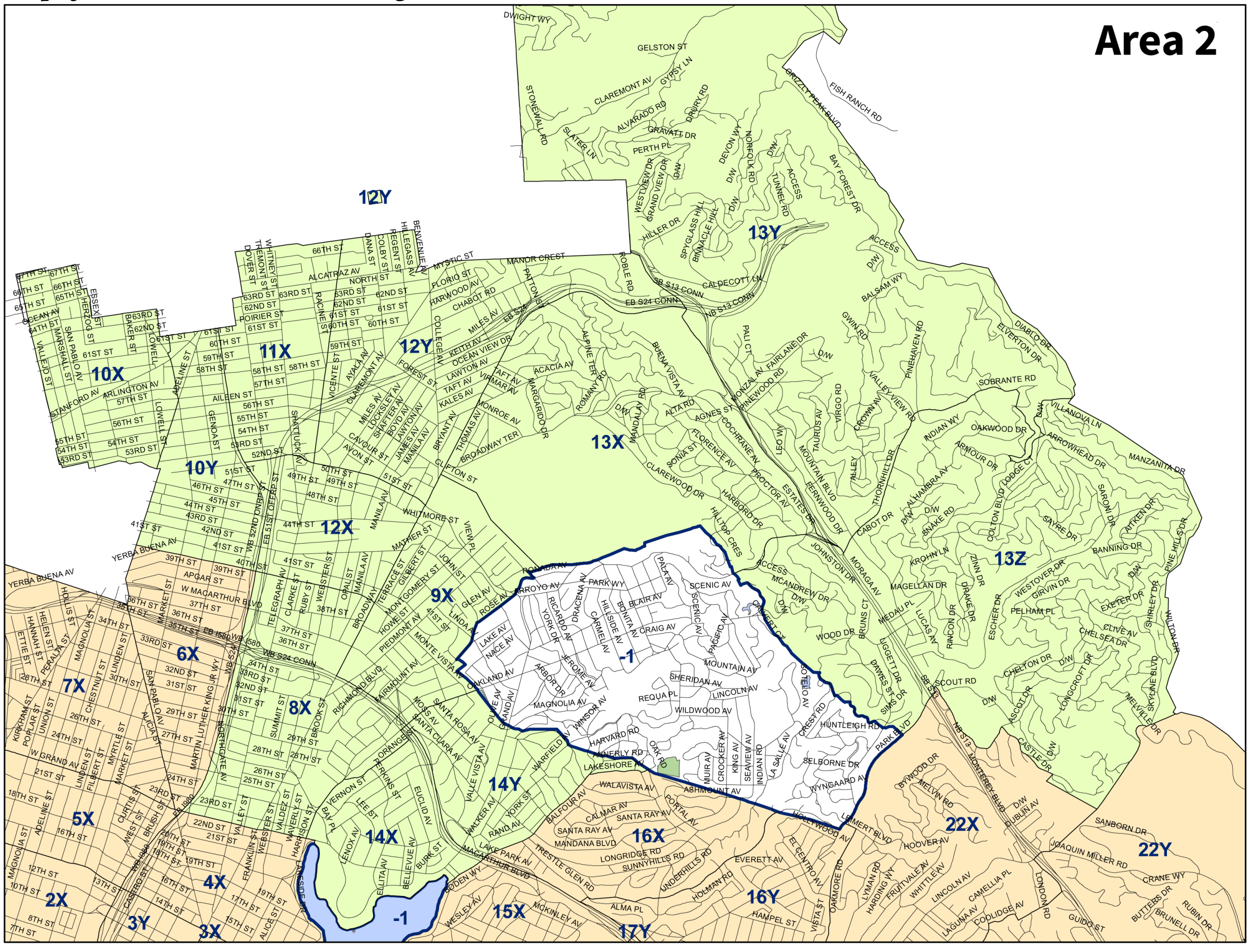
City of Oakland Police Districts and Census Tracts



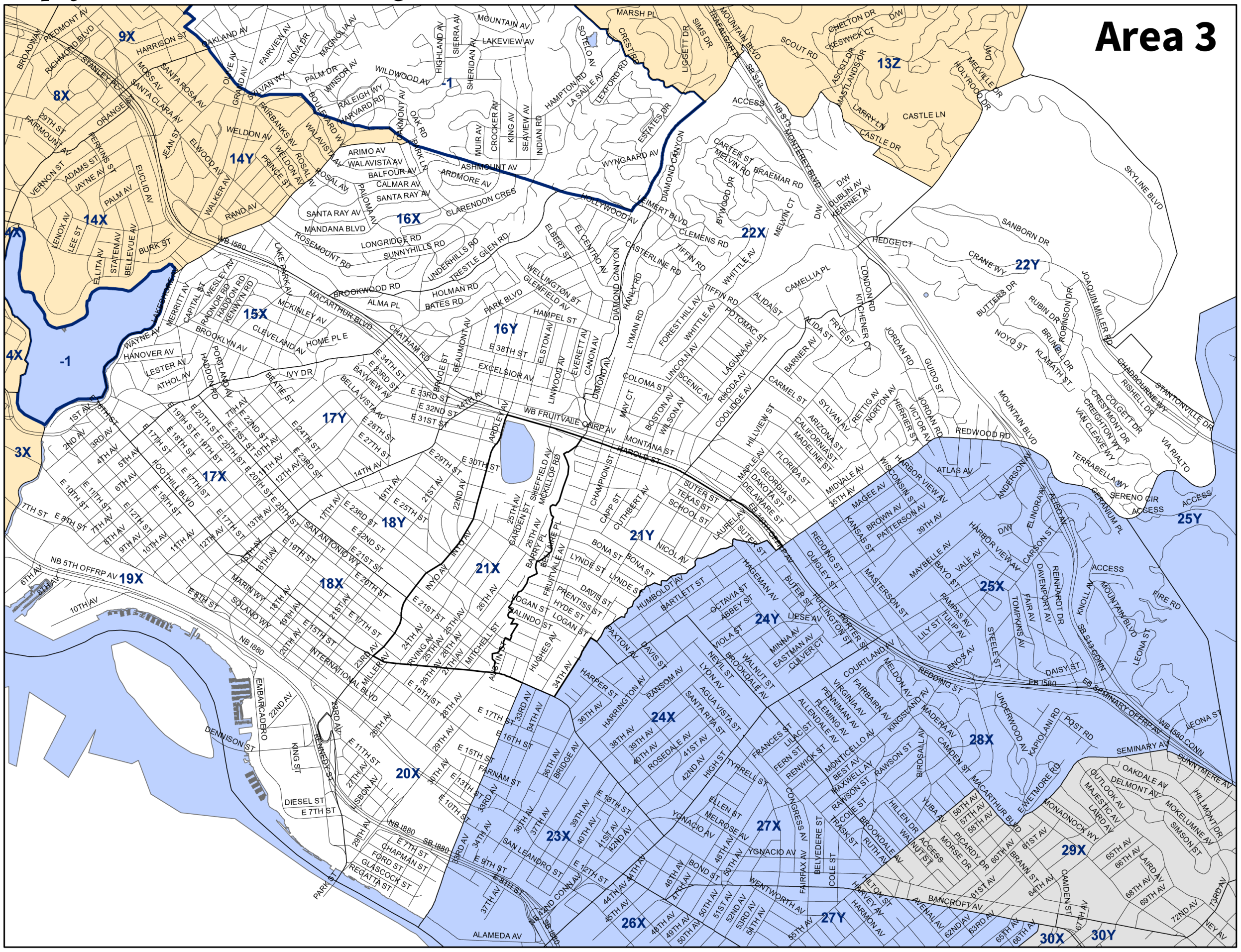
Map of Beats within OPD Policing Area 1



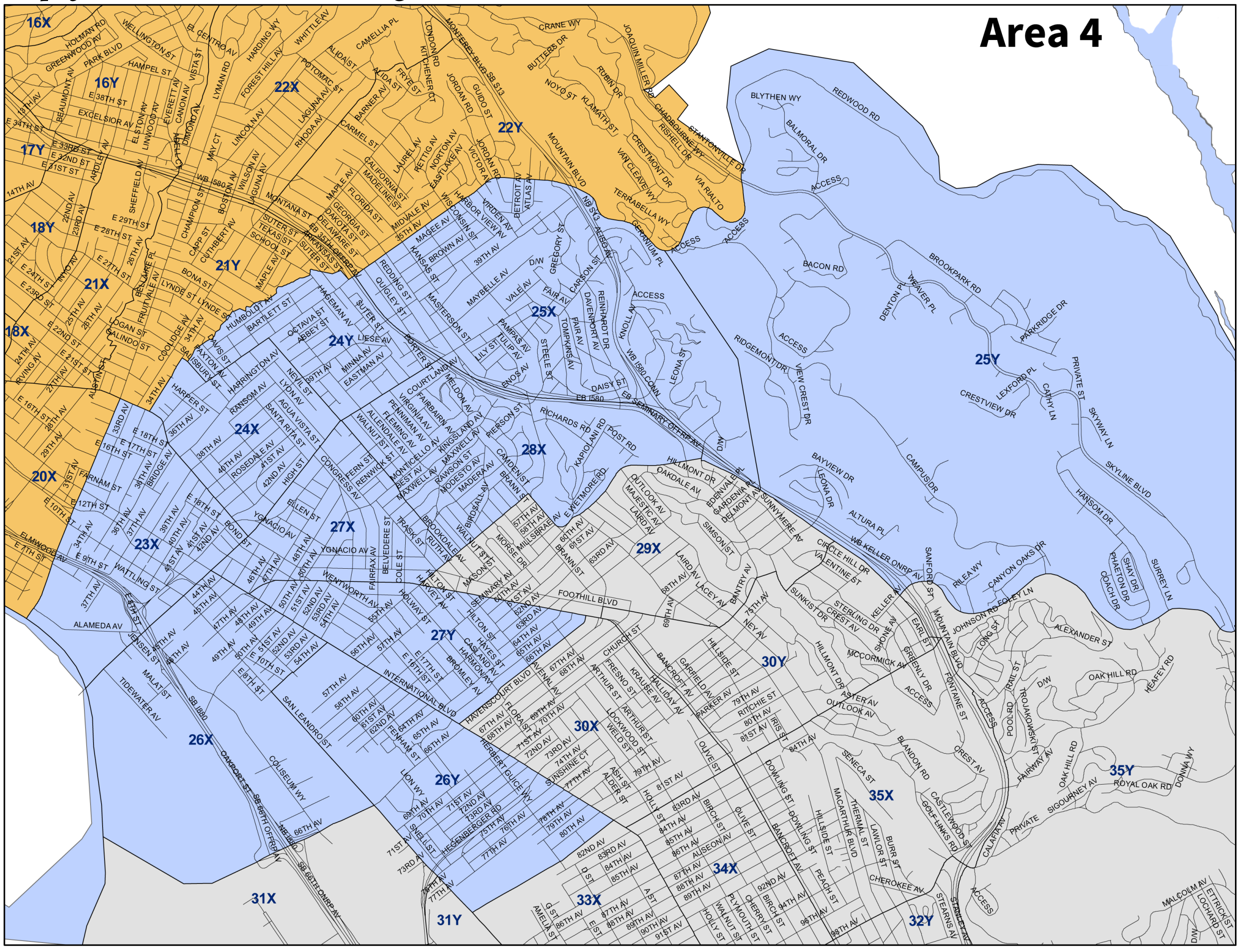
Map of Beats within OPD Policing Area 2



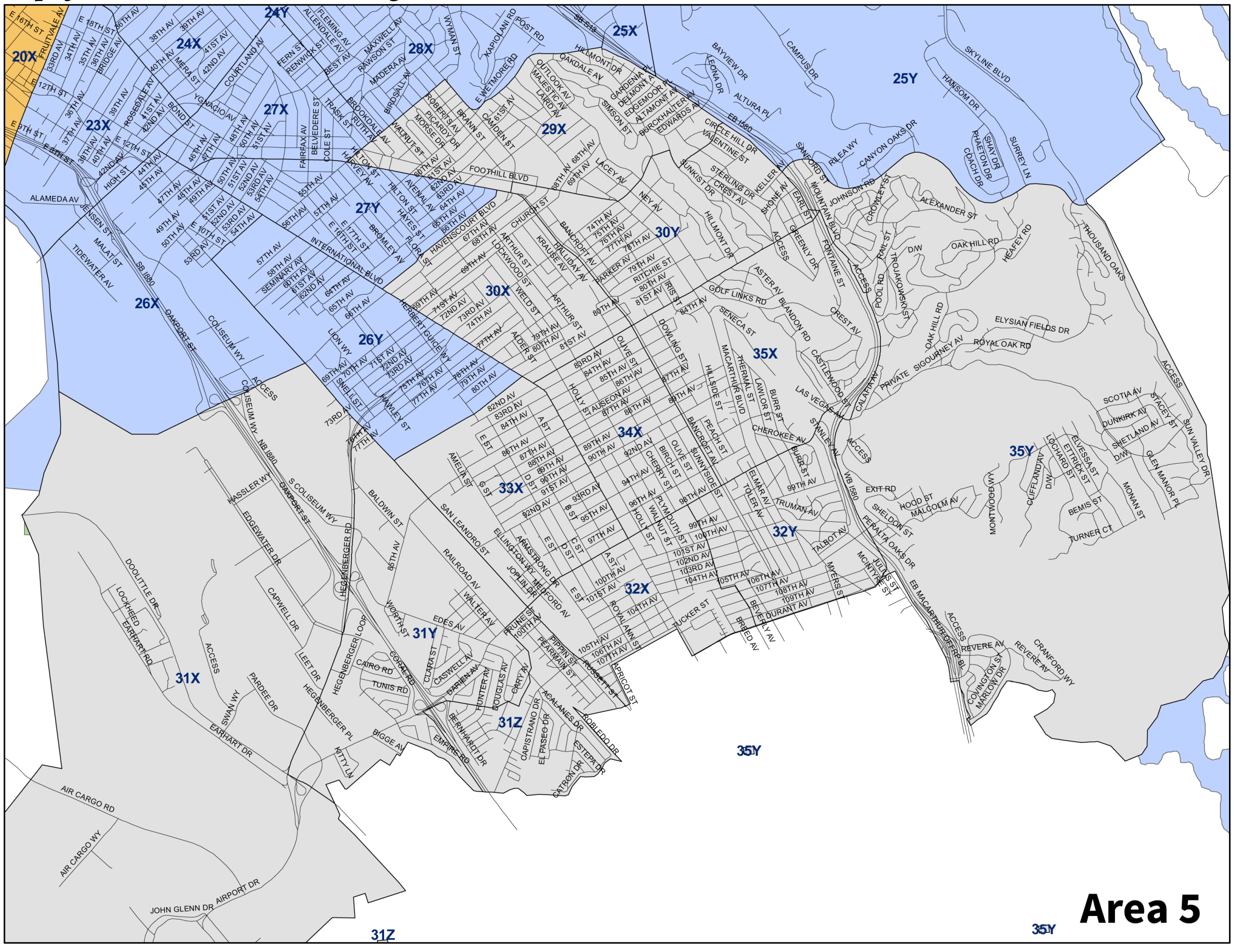
Map of Beats within OPD Policing Area 3



Map of Beats within OPD Policing Area 4



Map of Beats within OPD Policing Area 5



Appendix C: Information about Benchmarks and Variables We Would Have Included but Were Unable to Obtain

Typical officer assignment and squad:

We were interested in whether or not the assignment an officer is working influences his or her policing decisions when it comes to stops and post-stop outcomes. Furthermore, officers tend not to work these assignments in isolation and are usually part of a squad that is made up of other officers who have similar goals and tasks. These squads, we hypothesized, might have their own unique culture, norms, and way of approaching policing under the supervision and direction of their commanding officers. Capturing these squad-level differences might provide rich insight. We were unable, however, to obtain this information.

As such, we were extremely motivated to try and reconstruct from OPD records what assignment each officer was working on the day he or she made a particular stop. Further, from these assignments, we were also hoping to reconstruct which officers were working together as part of the same unit or squad. Having this information could allow us to analyze the stop data at the level of squad. Our plan was to empirically measure which was more influential in policing decisions and any resulting racial disparities: the individual officer making the stop, the squad of officers, or the commanding officer? The first set of obstacles came when we learned that the software the OPD uses to track its internal police rosters is not user-friendly and does not typically contain the level of precision that we as researchers are looking for to ensure accuracy in our analyses. Because of the software limitations, we were told that we would essentially need to try to manually look up, read through, and make sense of 510 officers' daily calendars for each of approximately 400 days on which stops were made. The time commitment needed to do this would be unfeasible for our team. We were driven to look for solutions that would allow us to ease this manual burden. We considered using a few time points scattered throughout the year to give us a "snapshot" of what officers were typically doing. Beginning to do this, we quickly ran into another obstacle when we realized that there is a lot of movement and lack of stability in which assignments officers work. Officers fill in for each other when their fellow officers have a day off or are on medical leave. Officers change assignments in response to special events, such as protests, parades, and athletic events. In many cases, when reading an officer's daily roster it was impossible to tell which of multiple assignments that were listed for that day the officer was actually working. One preliminary analysis we did showed that of the 510 officers, only 7 were working the same assignment on each of 5 days we selected across our 13-month time period. This high level of instability caused us to distrust any data we would be able to obtain by these less than satisfactory methods. We did not want to analyze inaccurate data, knowing that any conclusions based on these analyses would be highly suspect. As such, we were forced to abandon our plan to include officer assignment and squad as covariates and exploratory variables.

Driving behavior:

Because the majority of the stops made during the 13-month time period under investigation were vehicle stops made because of traffic violations, we wanted to include some measure of driving behavior in our analyses. The first place we thought to look was to the California Department of Motor Vehicles (DMV) to obtain some measure of how many people in the City of Oakland had valid driver's licenses, and ideally to obtain this information broken down by race. We learned from a representative of the California DMV that information about race is not collected. Furthermore, to identify how many licensed drivers reside specifically in Oakland, the DMV would have to look up their addresses, which is considered personally identifiable information and thus cannot be released. The best data we could obtain, then, was simply a count of the number of valid driver's licenses in each county in the State of California.

Another source of data we considered were traffic collision reports collected by the Oakland Police Department. These reports typically include detailed information about the drivers involved in traffic accidents, including race. The documentation typically also includes information about who was at fault and where the collision occurred. We learned, however, from Lieutenant David Elzey, the head of the Traffic Operations Section at the OPD that these reports were stored in a piecemeal way and were not organized in a database, though efforts were underway to archive them in a database. Given our inability to obtain data on traffic behavior, we were unable to include any proxy for driving behavior in our analyses.

Appendix D: Additional information about tract-level variables collected from the US Census Bureau

Rationale for Our Choice of Census Tract-Level Predictors

We based our choice of predictors for our initial models of post-stop outcomes on the Ayres and Borowsky 2008 ACLU report on LAPD, and more specifically on their "unrestricted" model. This model includes more predictors than the original Analysis Group Model (2006) in that it does not drop predictors that were not significant in the analysis (as in Percent Black). As the table below illustrates, we were able to find equivalent predictors for our post-stop models, with a few discrepancies: (a) we included the number of businesses per square mile, but did not separate retail businesses as they did, as we did not have that information; (b) we did not include number of stops (gang, incivility, or violent crime) as predictors, as we did not have that information—and also did not want to use officer behavior to predict officer behavior as it runs the risk of "included variable bias"; (c) we did not include the percentage of single parents;[146] (d) we added quality of life calls and crime rate for narcotics.

Demographic Predictors in Ayres & Borowsky's "Unrestricted" Model	Demographic Predictors in Our Model
Percent Hispanic	Percent Hispanic
Percent Black	Percent Black (Not Hispanic)
Percent under 24	Percent 24 or younger
Percent Owner Occupied	Percent Owner Occupied
Percent Divorced	Percent Divorced
Percent Unemployed	Percent Unemployed
Percent Poverty	Percent Poverty
Rate Violent Crime	Rate Violent Crime
Rate Property Crime	Rate Property Crime
Population Density	Population Density
Count of Businesses	Business Per Square Mile
Count of Retail Businesses	
Count of Gang Stops	
Count of Incivility Stops	
Count of Violent Crime Stops	
Percent Single Parent	
	Quality of Life Calls
	Rate Narcotics Crime

[146] Note however that a regression model shows that 80% of the variance in the percentage of single-parent households is explained by 5 predictors already in the model: poverty, 24 or younger, unemployed, owner occupied, and divorced. Thus is it unlikely that including or excluding this predictor makes much difference.

U.S. Census Bureau Definitions

We obtained a number of demographic variables about the residents in each Oakland census tract from the Census Bureau. In order to obtain more detailed, comprehensive, and up-to-date information, we used information from the 2013 (5-year estimate) American Community Survey (ACS) rather than the 2010 Census. We collected:

- **Total population in each census tract**
- **Land area (in square miles)**
- **Population density (per square mile)**
- **Percentage of the total population that is Black (non-Hispanic)**
- **Percentage of the total population that is Hispanic**
- **Percentage of the total population that is 24 years of age or younger**
- **Percentage of the population aged 16 years and older who are in the civilian labor force and unemployed**
- **Percentage of families living in poverty**
- **Percentage of all housing units that are occupied by the owner**
- **Percentage of the population 15 years of age and older who are divorced**

Here, we provide more information from the U.S. Census Bureau about the variables that require a little more explanation. Please note that the descriptions below largely come directly from the U.S. Census Bureau's Technical and Supplemental Documentation of the ACS 2013 (5 year). We accessed this documentation through the Social Explorer program.

Race and Hispanic Origin:

Note that the federal government, and as such the United States Census Bureau, does not consider Hispanic or Latino to be a race, but rather an origin. According to the Census Bureau, "Origin can be viewed as the heritage, nationality group, lineage, or country of birth of the person or the person's parents or ancestors before their arrival in the United States."[147] The Census Bureau specifies that people who identify their origin as Hispanic or Latino (or Spanish; all three terms are

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[147] United States Census Bureau. (2012). *About Hispanic origin.* Retrieved from http://www.census.gov/population/hispanic/about/ http://quickfacts.census.gov/qfd/meta/long_RHI725214.htm.

used interchangeably) may be of any race. Therefore, one could identify as White, for example, in the race question on the Census and then go on to classify oneself in a specific Spanish, Hispanic, or Latino category listed in the Census questionnaire. For our percentage Hispanic variable we used the Census' Hispanic or Latino origin question, which counts people who classified themselves in one of the specific Hispanic, Latino, or Spanish categories listed on the questionnaire ("Mexican," "Puerto Rican," or "Cuban") as well as those who indicated that they are "another Hispanic, Latino, or Spanish origin," which would include those whose origins are from Spain, the Spanish-speaking countries of Central or South American, or the Dominican Republic.

To get around the issue of potentially double counting individuals who identify as African American and also as Hispanic, we chose to use counts of those who indicated they were both Black alone (Black and no other race) ***and*** non-Hispanic (i.e., they answered "No" to the Hispanic or Latino origin question just described in the preceding paragraph) for our percentage African American variable.

The data on race were derived from answers to the question on race that was asked of all people. The U.S. Census Bureau collects race data in accordance with guidelines provided by the U.S. Office of Management and Budget (OMB), and these data are based on self-identification. The racial classifications used by the Census Bureau adhere to the October 30, 1997, *Federal Register* notice titled "Revisions to the Standards for the Classification of Federal Data on Race and Ethnicity" issued by OMB. These standards govern the categories used to collect and present federal data on race and ethnicity. OMB requires five minimum categories (White, Black or African American, American Indian or Alaska Native, Asian, and Native Hawaiian or Other Pacific Islander) for race. The race categories include a sixth category, "Some Other Race," added with OMB approval. In addition to the five race groups, OMB also states that respondents should be offered the option of selecting one or more races.

According to the OMB, Black or African American is defined as: A person having origins in any of the Black racial groups of Africa. It includes people who indicate their race as "Black, African Am., or Negro" or report entries such as African American, Kenyan, Nigerian, or Haitian.

Unemployment:

For our unemployment variable, we used census data recording the percentage of people in the civilian labor force who are 16 years of age and older and are unemployed. According to the Census Bureau, the unemployment rate represents the number of unemployed people as a percentage of the civilian labor force. For example, if the civilian labor force equals 100 people and 7 people are unemployed, then the unemployment rate would be 7 percent. "Civilian labor force" refers to people who are not in the military and thus people who are serving on active duty in the U.S. Armed Forces are excluded. Furthermore, not all people 16 years old and over are automatically considered part of the labor force. Those not in the labor force include students, homemakers, retired workers, seasonal workers interviewed in an off season who were not looking for work, institutionalized people, and people doing only incidental unpaid family work (less than 15 hours during the reference week).

Poverty:

For our poverty variable, we relied on census data about the total percentage of families who are living below the poverty line. Income was calculated using 2013 inflation-adjusted dollars. According to the Census Bureau, a family "consists of a household and one or more other people living in the same household who are related to the householder by birth, marriage, or adoption."[148]

According to the technical documentation: "The Census Bureau uses a set of dollar value thresholds that vary by family size and composition to determine who is in poverty. Further, poverty thresholds for people living alone or with nonrelatives (unrelated individuals) vary by age (under 65 years or 65 years and older). The poverty thresholds for two-person families also vary by the age of the householder. If a family's total income is less than the dollar value of the appropriate threshold, then that family and every individual in it are considered to be in poverty. Similarly, if an unrelated individual's total income is less than the appropriate threshold, then that individual is considered to be in poverty. To determine a person's poverty status, one compares the person's total family income in the last 12 months with the poverty threshold appropriate for that person's family size and composition. If the total income of that person's family is less than the threshold appropriate for that family, then the person is considered 'below the poverty level,' together with every member of his or her family. If a person is not living with anyone related by birth, marriage, or adoption, then the person's own income is compared with his or her poverty threshold. The total number of people

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[148] For more information, see Lofquist, D., Lugaila, T., O'Connell, & Feliz, S. (2012). *2010 census briefs: Households and families: 2010.* United States Census Bureau.

below the poverty level is the sum of people in families and the number of unrelated individuals with incomes in the last 12 months below the poverty threshold."

To give the reader a sense of what would be considered living in poverty, a family of three with one related child under the age of 18 who responded to the ACS in July 2013 who earned less than $22,886 in a 12-month period would qualify as living in poverty. To provide some context, we also obtained the median family income of every census tract in Oakland. The range was from $19,940 a year in the tract with the lowest income to $202,750 a year in the tract with the highest income. The average median family income across the City of Oakland was $69,824 (SD = $44,968; this is not weighted by the largest or most populated tracts, but is simply a mathematical average of all tracts). Remember that the poverty thresholds change depending on the number of people in the family and the age of the householder so it is hard to interpret these numbers just by glancing at them.

Appendix D: Statistical Note

All the statistical analyses described in this report were conducted using R.[149] In particular, the analysis of the post-stop outcomes reported in the binary logistic regression tables in the appendices make use of the glm() function, with family = binomial. Presentation of these tables in a format adequate for word processing was facilitated by the use of the Stargazer package.[150] The test of simple effects reported in the main body of the reports (where we test, for example, if there is a significant African American-White difference in Handcuffing in Area 1 for Vehicle Stops) was conducted using the Survey package.[151] The likelihood tables presented in the main report are actually based on the regression tables presented in the appendices ("No Covariates" and "All Covariates" regression equations). In the appendices we actually conduct the analyses three different ways: No Covariates, All Covariates, and Fixed Effects.

The "No Covariates" Analysis. In the first column is the analysis without covariates, which only includes Ethnicity of the citizen being stopped (SDRace2), the Area where the stop occurred (Area), and whatever moderator we are focusing on in this analysis (e.g., SpecialAssignment2). This model produced the likelihood estimates reported in the "No Covariates" tables in the main text. These predicted values are exactly equal to the raw numbers one would obtain if one simply tallied by hand the number of stops for a given race in a given area and at a given value of the moderator. For example, one can generate the likelihood of African Americans to be handcuffed in Area 1 during a vehicle stops from the table in Appendix I by adding the appropriate terms: -3.688 (intercept) + 1.561 (African American) = -2.127, and remembering that a logistic regression models log (p / (1-p)), such that he the predicted likelihood here would be exp(-2.127) / (1 + exp(-2.127)) = .1065. And indeed if we just count the number of African American vehicle stops in Area 1 that resulted in handcuffing without an arrest, we would find 261, out of 2,189 such stops, or .1065. Again, the "No Covariates" tables in the report (e.g., Table 5.1) were produced using these regression equations, but could have been produced with the raw data. What the regression enables us to do, which the raw data doesn't, is use the svycontrast() command in the Survey package (Lumley, 2014) to test the significance of a linear combination of regression coefficients, in effect testing a simple effect. In the example just given, this amounts to testing whether the African American-White difference in Area 1 (captured by coefficient 1.561) is significantly different from 0. This is what the asterisks in those tables correspond to. The other thing the regression tables enable us to do is actually test interaction

[149] R Core Team. (2015). R: A language and environment for statistical computing. R Foundation for Statistical Computing, Vienna, Austria. https://www.R-project.org/.
[150] Hlavac, M. (2015). Stargazer: Well-formatted regression and summary statistics tables. R package version 5.2. http://CRAN.R-project.org/package=stargazer
[151] Lumley, T. (2014). Survey: Analysis of complex survey samples. R package version 3.30.

terms. While we do not feature these prominently in the report, they are in the tables for readers who want to go deeper into the data. In the same way we test whether the African American-White difference in handcuffing rates in Area 1 vehicle stops is statistically significant by testing 1.561 against zero in our example, the interaction terms enable us to test "differences in differences," e.g., whether the African American-White gap in Area 1 is different for vehicle vs. pedestrian stops. While in the report we merely comment on these differences in the tables, the regression tables actually provide a former test. In the example just given, this would be tested by looking at the interaction term in the table in Appendix I (SDRace2Afr American:EncounterTypePedestrian), which is -.599, and statistically significant. This tells us that when we look at the table of likelihoods in Table 5.4, the African American-White gap for vehicle stops in Area 1 (11% vs. 2%) is statistically more significant than for pedestrian stops (34% vs. 16%), despite the latter being larger in absolute difference.[152]

To read these regression tables it is important to be aware of the "reference category," against which effects are computed. Thus if the reference category is "Male" for gender, the coefficient for "GenderFemale" is the difference between male and female. This is more important to know when the predictor takes more than 2 values. Thus the reference category for Type of Encounter was "Vehicle Stop," so the coefficient for pedestrian is the difference between pedestrian and vehicle stop, while the coefficient for bicycle is the difference between bicycle and vehicle stop. The reference category never gets a coefficient, and in this example, pedestrian stops are compared only to vehicle stops, never to bicycle stops.

The "All Covariates" and "Fixed Effects" Analyses. These analyses, presented in Columns 2 and 3 of the appendix tables, are essentially redundant rather than building on each other. We present both as a robustness check. The first one (Column 2), "All Covariates," includes in the analysis control variables at the level of the census tract (e.g., crime rate, population density) and at the officer level (e.g., race, gender). Those were entered to control for possible variability between tracts and officers captured by these known measures. Including covariates enable us to control for spurious associations, e.g., if census tracts with higher crime rates trigger more handcuffing and they also happen to be predominantly African American, it could give rise to a spurious correlation between race and handcuffing that is really due to the crime risk in that neighborhood. Controlling for crime rate solves this problem and should make such a spurious correlation go away. At the

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[152] Note that this is a pattern we see elsewhere in these tables: differences between small percentages are more statistically significant than differences (of the same magnitude) between larger percentages. While the absolute difference between 11% and 2% (9) is smaller than the difference between 34% and 16% (18), in the logit model log(.11/.89)-log(.02/.98) = 1.80 is much larger than log(.34/.66)-log(.16/.84) = .99. So the logit models that we are using to look at these percentage differences place less emphasis on absolute linear differences than on ratios: a jump from 2% to 11% is a fivefold increase, whereas an increase from 16% to 34% is only a twofold increase.

same time, there is only so much that can be captured by the available census data, and differences exist between neighborhoods that may not be captured by a questionnaire or recorded databases. For example, we can use recorded crime rates in a neighborhood as a proxy for the likelihood that a given citizen exhibits suspicious behavior (warranting a search) but these rates only give a very partial snapshot of the criminal activity in an area, are delayed since they come from the previous year, etc. For these reasons, and as a robustness check, we also include in the table a "Fixed Effects" analysis that is redundant with the "All Covariates" analysis, but instead of entering covariate measures at the census-tract and officer levels, we entered "dummy codes" capturing which census tract a stop took place in, and dummy codes capturing which officer made the stop. So because there were 113 unique census tract, there should be 112 new predictors in the analysis, each for one tract beyond the reference one, and because there were 510 officers, there should be 509 new dummy codes in the prediction model. For ease of presentation, and because they are of no interest, we do not present the additional 621 coefficients thus obtained—the table just indicates at the bottom which fixed effects (dummy codes) were included in the model. Note that we use the term "Fixed Effects," which is what such an approach is often called in econometrics (also sometimes called "indicator" variables), but this is not to contrast this approach strongly with "Random Effects" as would be done in some social sciences. Here the emphasis is on including dummy codes for Census Tracts and Officers that capture and therefore remove the variance attributable to these two sources of variation, without needing to hope that we captured the right covariates. While the likelihood tables in the main text are obtained from the "No Covariates" analyses, the "Fixed Effect" tables can be used to make sure the effect under study (here race) remains significant with this other way of controlling for extraneous variability. Thus in Appendix I we can see that the effect of race is significant in the "No Covariates" model, 1.561, $p < .001$, as well as in the "All Covariates" model, 1.136, $p < .001$, and that it remains significant in the "Fixed Effects" model, .895, $p < .001$. Given that these models include interactions with Area, and in this case with Type of Encounter, it is useful to remember that these effects just described correspond to the reference categories, i.e., Vehicle Stops in Area 1. One would then look at the regression table for interaction terms to determine if there is any reason to believe the race effect is different in other areas or other types of stops. In this example, there is the suggestion that the African American-White difference in handcuffing in vehicle stops is greater in Area 2, as there is a significant interaction between Race and Area 2 in all three equations. The pattern of two-way interactions in this example also suggest that the African American-White gap is significantly reduced for pedestrians and "other" types of stops. We also observe some significant three-way interactions which can be similarly interpreted. For ease of interpretation we refer readers to the likelihood tables in the main text, but the regression analyses tables are included for completeness, with the "Fixed Effects" regressions presented as robustness checks, even if they are not discussed in the main text.

A word of caution. In the "All Covariates" analysis, readers should refrain from putting too much stock in the significance level indicated by stars pertaining to Census Tract and Officer level variables. While it is perfectly legitimate to include these predictors in the analysis to control for these variables, the fact that these covariates are clustered within tracts and officers makes it harder to properly evaluate the standard error of these regression coefficients, and thus their significance should be taken with caution. To illustrate why this is a problem, imagine that one census tract had a very large number of stops (800) and also a large number of stores, and a lot of handcuffing; ignoring the fact that the number of stores is "clustered" within the tract, we could think we have 800 independent data points linking high rates of handcuffing and number of stores, when really they all come from that one census tract and the correlation could be due to another feature of that single tract. We encourage readers to treat tract-level and officer-level predictors in these models simply as covariates included to control for spurious association, but not to place stock in the specific coefficients and significance levels obtained for those. Instead we come back to these predictors when we present officer-level and census-tract-level analyses elsewhere in the report. Although it is possible to use statistical techniques that consider all levels in the same analysis while accounting for these clustering issues (e.g., hierarchical linear modeling, mixed models), we chose to study those different levels of analyses separately. A central motivation for this choice is that we found that the more complex models obfuscated some of the basic patterns in the data set—e.g., that most stops are conducted by few officers, that most stops occur in a subset of census tracts, that many officers never once behaved with White citizens the way they did with African American citizens, etc. We refer readers to the officer-level and tract-level analyses for more discussion of these patterns.

Appendix E: Descriptive Statistics by Census Tract: Census Covariates used in Regression Models

Census Tract	Hispanic (%)	African American (%)	Population 24 years or younger (%)	Divorced[153] (%)	Unemployed[154] (%)	Owner occupied housing[155] (%)	Below poverty level[156] (%)	Violent Crime Rate	Property Crime Rate	Narcotics Crime Rate	Population Density	Business Count[157]	Quality of Life[158]
4001	7.5	4.9	33.6	7.0	5.1	76.7	2.7	3.0	176.0	3.0	1,261.8	17.3	23.9
4002	11.0	2.1	17.6	7.7	2.7	64.4	0	97.7	766.5	0	8,460.6	991.3	210.9
4003	7.1	14.2	20.6	7.5	10.4	42.3	2.2	70.7	753.3	5.6	12,597	1,504.7	275.3
4004	10.9	14.2	17.8	9.9	5.6	40.6	2.4	113.2	503.4	7.2	15,281.8	1,048.1	467.2
4005	14.7	27.6	20.4	14.5	8.3	33.8	6.7	80.2	406.3	2.8	15,933.4	987.0	276.4
4006	13.0	39.6	22.5	5.3	12.7	38.1	2.8	230.6	490.8	41.4	14,704	791.7	620.9
4007	11.2	48.2	26.8	10.6	11.8	26.3	12.8	172.3	534.2	14.8	11,948.2	770.6	359.4
4008	18.2	35.5	19.8	15.5	14.7	26.7	2.3	214.9	543.8	39.1	11,440.6	788.9	244.2
4009	4.1	40.1	30.6	13.3	10.0	42.0	9.3	243.3	674.9	50.5	13,267.8	825	303.0
4010	23.3	42.5	21.7	14.6	9.0	32.7	18.3	309.6	687.0	59.9	11,236.3	677.8	1,094.5
4011	14.6	22.7	21.3	14.5	12.9	18.8	2.6	263.9	1,369.0	29.6	11,845.8	1,214.7	929.9
4012	11.5	19.4	15.5	12.8	12.0	41.7	3.3	443.8	1,156.4	44.8	9,532.8	865.4	464.2
4013	20.0	33.8	18.8	10.7	14.6	11.3	28.6	308.8	1,225.2	100.4	12,056.6	2,175.8	1,416.0
4014	22.6	43.8	41.6	9.5	20.1	15.3	42.7	361.8	755.8	244.2	14,871	765.5	1,624.4
4015	9.1	51.2	22.5	12.2	13.4	36.7	23.1	336.5	870.2	182.7	11,534.6	900	1,812.5
4016	14.5	50.8	36.3	12.7	16.1	16.1	23.2	465.9	1,154.0	487.0	7,691.7	936	2,805.7
4017	37.1	24.9	28.6	10.5	7.0	33.1	16.9	326.3	1,344.3	114.0	1,135.5	209.4	2,790.9
4018	17.0	52.2	46.4	8.2	22.9	13.6	42.5	378.8	582.4	179.9	18,313.1	850	421.4

[153] Percentage of the people 15 years or older who are divorced.
[154] Percentage of the people 16 years and older in the civilian labor force who are unemployed.
[155] Percentage of all housing units that are occupied by the owner.
[156] Percentage of all families living in poverty.
[157] Number of businesses per square mile.
[158] Quality of life complaints (e.g., litter, graffiti) per 10,000 people.

Appendix E: Descriptive Statistics by Census Tract: Census Covariates used in Regression Models

Census Tract	Hispanic (%)	African American (%)	Population 24 years or younger (%)	Divorced[153] (%)	Unemployed[154] (%)	Owner occupied housing[155] (%)	Below poverty level[156] (%)	Violent Crime Rate	Property Crime Rate	Narcotics Crime Rate	Population Density	Business Count[157]	Quality of Life[158]
4022	26.4	40.9	35.5	12.1	17.3	26.6	18.2	503.0	742.5	83.8	9,102.5	492.9	1,237.5
4024	5.2	73.1	36.1	10.0	39.7	11.6	35.2	132.2	328.9	35.5	23,826.9	661.5	428.9
4025	18.7	66.6	46.7	7.6	22.2	17.3	43.2	385.0	439.3	177.7	14,358.2	428.6	844.0
4026	3.9	24.9	11.6	6.0	11.0	15.4	21.9	450.3	816.8	125.7	7,760.4	725	994.8
4027	17.1	58.2	32.1	5.6	15.4	25.6	22.7	486.4	1,036.0	183.2	10,281.4	573.3	770.7
4028	9.4	43.4	15.3	15.7	18.2	8.2	38.2	545.3	1,885.7	118.3	19,791.2	2,720	758.9
4029	9.5	14.8	14.4	9.4	6.7	0	12.4	837.3	3,732.1	103.7	8,341.7	7,946.7	1,371.6
4030	1.4	3.9	12.3	7.4	13.5	16.8	8.3	684.1	1,672.3	42.2	17,359.7	7,642.9	857.3
4031	11.1	17.1	16.1	8.1	6.9	28.5	13.9	495.1	2,123.8	234.5	11,462.8	4,869.2	456.0
4033	7.7	7.3	19.0	8.6	5.6	22.1	20.0	108.9	730.2	12.4	11,299.3	1,380.6	396.0
4034	14.8	21.8	14.0	15.4	9.8	9.9	3.4	176.0	685.5	13.9	16,938.3	956	433.1
4035.01	9.6	29.6	18.4	13.6	7.2	12.1	27.9	193.9	1,110.6	12.0	16,230.9	1,246.2	904.7
4035.02	10.1	19.4	8.2	15.5	12.2	28.6	9.8	83.4	681.7	4.9	28,950.9	1,357.1	269.7
4036	10.9	30.2	15.4	13.5	9.7	15.7	12.9	108.4	625.3	6.9	28,774.7	1,266.7	193.8
4037.01	9.6	18.4	7.9	10.0	1.0	7.2	9.6	136.2	524.6	0	37,179.1	2,085.7	280.3
4037.02	6.7	10.8	12.7	13.0	6.8	25.0	3.7	172.7	1,020.8	15.2	25,395.2	3,687.5	624.7
4038	14.6	10.5	20	11.3	6.6	32.5	0	90.3	741.9	5.6	13,987.9	1,672	172.1
4039	6.9	19.9	19.0	15.6	5.9	32.0	1.9	70.3	527.2	5.0	20,826.6	1,978.9	155.7
4040	14.9	11.9	13.4	13.2	9.4	23.8	2.6	117.9	843.5	7.1	19,525.1	2,214.3	325.2
4041.01	3.6	0.7	16.1	13.3	6.9	36.9	3.3	142.2	778.8	10.7	12,695.7	2,209.1	128.0
4041.02	5.8	10.4	13.0	9.6	8.5	12.7	5.4	80.3	417.0	0	29,413.8	1,111.1	133.9
4042	7.3	10.7	31.9	6.4	7.1	71.8	1.6	57.2	443.4	2.9	3,318	152.4	57.2
4043	6.8	4.4	22.0	7.5	6.4	79.6	0	14.6	407.5	0	4,860.9	250.7	174.6
4044	7.8	3.3	24.7	7.3	2.9	78.9	0.5	13.8	336.1	0	3,176	185.5	247.1

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4045.01	2.4	3.7	26.9	3.2	8.9	81.9	2.0	16.5	274.4	0	4,256.9	95.3	54.9
4045.02	3.8	1.7	21.7	10.9	1.7	87.1	0.4	19.9	509.0	1.8	4,671.6	479.8	88.4
4046	2.9	8.0	24.6	9.5	3.5	91.6	0.5	8.8	502.6	4.4	2,403.7	127.5	134.5
4047	3.3	6.9	24.9	14.5	6.0	84.5	1.3	20.3	547.4	0	4,237.3	212.8	152.1
4048	10.1	25.8	26.6	14.5	6.4	55.3	2.4	211.2	684.6	14.6	11,317.7	758.3	240.3
4049	9.3	11.5	20.4	16.3	6.9	58.7	5.0	107.1	626.3	16.3	10,993.5	848.7	246.8
4050	9.1	7.4	19.1	9.1	5.9	58.9	1.4	68.5	613.8	0	10,715.3	619.4	166.9
4051	4.1	10.4	27.5	5.3	8.4	81.6	0.8	12.6	307.0	2.5	8,132	453.1	35.2
4052	10.9	11.1	18.9	12.4	6.4	24.6	3.9	146.3	682.2	8.6	14,090.1	651.5	320.6
4053.01	14.9	20.3	17.2	16.4	6.5	9.8	0	119.8	583.5	30.9	29,430.5	1,788.9	409.6
4053.02	18.6	24.6	9.8	16.6	14.0	2.2	28.2	195.9	673.5	12.2	31,359.4	1,287.5	253.1
4054.01	31.6	20.7	29.3	11.0	17.5	10.1	31.0	177.9	419.1	39.0	33,859	1,066.7	328.9
4054.02	17.5	20.0	32.3	5.6	20.2	12.2	26.6	226.3	462.8	54.9	26,474.5	636.4	291.4
4055	17.9	19.7	29.7	10.4	12.0	18.3	22.9	156.9	439.8	5.6	22,457.6	762.5	224.1
4056	14.5	30.0	26.3	10.4	14.7	20.7	14.7	193.7	611.9	9.2	19,643.5	894.1	249.1
4057	17.0	34.3	32.1	7.1	14.7	20.1	12.2	401.3	743.4	26.3	14,397.8	581.0	266.4
4058	14.4	16.7	38.2	5.7	14.2	40.5	31.9	252.6	336.8	23.4	21,331.5	480	248.0
4059.01	29.4	11.2	42.7	6.9	25.1	23.7	38.3	340.3	491.6	66.9	19,674.4	664.7	529.4
4059.02	28.7	13.6	38.0	8.7	11.3	25.9	31.4	332.4	524.8	43.7	18,231	657.9	501.5
4060	20.9	18.4	30.8	12.1	7.8	9.3	34.2	352.5	1,054.5	51.2	4,065	746.3	1,401.0
4061	67.0	8.0	34.5	9.3	15.1	28.1	13.1	452.5	1,237.8	68.8	6,337	749.3	1,233.4
4062.01	47.5	16.8	36.5	10.5	17.7	17.6	37.0	362.6	573.6	89.4	25,558.7	606.2	270.7
4062.02	59.4	11.0	37.3	4.2	12.0	12.6	33.0	362.0	393.7	31.7	29,189.7	873.3	407.2
4063	28.0	21.9	32.7	12.7	8.9	35.8	25.5	247.0	444.6	20.2	23,743.4	631.6	446.9

Appendix E: Descriptive Statistics by Census Tract: Census Covariates used in Regression Models

Census Tract	Hispanic (%)	African American (%)	Population 24 years or younger (%)	Divorced[153] (%)	Unemployed[154] (%)	Owner occupied housing[155] (%)	Below poverty level[156] (%)	Violent Crime Rate	Property Crime Rate	Narcotics Crime Rate	Population Density	Business Count[157]	Quality of Life[158]
4064	25.2	29.1	30.1	13.0	21.2	36.7	23.3	93.3	432.8	12.7	12,511.9	357.9	297.0
4065	42.0	22.5	34.9	8.6	19.8	27.4	18.3	242.9	535.3	18.2	21,041.7	634.5	302.3
4066.01	15.2	36.3	38.3	12.0	18.0	36	20.1	157.1	628.5	14.5	16,749.5	537.9	506.5
4066.02	30.0	17.6	29.8	12.9	5.0	30.6	17.3	298.2	1,099.7	55.9	13,402	687.5	778.2
4067	10.4	12.7	27.6	11.7	7.5	64.1	2.0	77.0	591.7	0	7,591.2	436.9	135.8
4068	16.7	17.7	32.8	12.0	6.8	62.1	5.9	115.3	713.9	19.2	10,337	494.3	159.3
4069	23.9	17.7	32.3	10.4	9.4	53.0	11.9	157.6	596.3	26.3	11,295.1	591.2	294.2
4070	25.3	25.5	32.5	10.5	12.9	39.9	21.9	230.3	681.7	22.1	16,301.6	733.3	436.6
4071.01	49.4	11.6	41.6	7.5	16.9	20.4	34.6	188.0	354.6	10.7	26,002	571.4	394.8
4071.02	48.2	15.2	35.5	7.5	18.2	39.0	27.2	166.4	534.2	9.4	16,726.7	461.5	201.5
4072	73.1	7.4	38.3	8.5	17.2	19.4	25.1	369.2	509.3	13.2	21,974.8	1,071.4	461.5
4073	63.7	14.4	38.0	3.0	10.3	31.6	17.6	500.5	1,333.6	107.3	3,055.4	755.4	1,383.6
4074	73.4	12.7	41.8	8.4	19.3	24.1	26.4	322.0	561.8	77.6	22,173.6	885	447.6
4075	40.8	39.0	46.0	13.0	23.3	21.5	41.6	348.1	496.2	106.9	19,106.3	652.6	523.6
4076	23.3	48.0	32.5	12.5	17.9	42.3	17.3	245.9	748.7	18.6	13,596.8	543.6	661.2
4077	11.9	52	25.1	15.6	15.4	67.6	8.9	122.0	609.8	34.1	11,559.1	580	258.5
4078	25.0	30.0	46.9	11.1	10.5	65.3	4.3	152.2	467.4	10.9	5,790.1	175	496.4
4079	8.6	14.7	27.3	11.9	10.5	65.9	1.6	120.0	931.6	72.0	5,891.9	304.8	883.6
4080	11.3	11.2	26.4	8	8.9	83.6	1.6	28.4	581.4	3.5	3,818.8	254.1	70.9
4081	6.6	27.1	18.5	8.4	4.9	79.6	1.8	52.6	450.0	3.1	1,907.8	87.6	352.6
4082	16.4	56.3	24.5	20.9	13.0	42.9	13.2	303.5	629.1	22.1	10,408.7	402.9	347.7
4083	14.4	47.3	32.2	12.9	16.0	63.2	11.0	185.4	552.2	21.9	8,537.6	361.0	239.2
4084	37.5	48.7	42.0	6.3	25.3	28.9	33.5	284.1	579.0	29.8	17,640.1	495.2	359.8
4085	43.9	41.0	47.6	9.8	13.5	30.4	28.1	307.1	505.5	84.3	16,639.2	365.6	597.3

Appendix E: Descriptive Statistics by Census Tract: Census Covariates used in Regression Models

Census Tract	Hispanic (%)	African American (%)	Population 24 years or younger (%)	Divorced[153] (%)	Unemployed[154] (%)	Owner occupied housing[155] (%)	Below poverty level[156] (%)	Violent Crime Rate	Property Crime Rate	Narcotics Crime Rate	Population Density	Business Count[157]	Quality of Life[158]
4086	45.6	43.7	40.1	9.9	16.9	32.6	39.2	331.0	592.7	47.3	14,455.3	536.6	314.1
4087	49.8	37.8	38.3	14.1	24.1	33.3	22.7	248.5	466.7	54.9	16,682.5	550	324.0
4088	47.5	38.2	47.7	8.6	20.1	20.3	36.8	316.9	544.2	113.6	13,436.5	314.9	374.5
4089	57.4	33.2	42.4	6.8	18.7	28.1	39.8	404.8	589.7	149.8	10,125.5	422.6	717.2
4090	49.7	39.4	46.3	6.6	16.3	45.8	18.7	327.5	2,723.9	138.9	632.3	128.5	724.4
4091	39.8	36.4	45.2	11.9	2.5	54.3	24.5	179.1	644.9	49.3	11,814.1	247.4	501.6
4092	38.7	47.8	44.8	7.0	15.7	40.9	20.7	205.8	359.4	29.4	10,963.6	225	663.2
4093	63.4	22.2	37.4	6.0	21.0	38.2	15.9	244.6	582.5	18.7	12,890.3	523.8	832.7
4094	60.3	25.0	52.0	7.0	15.9	31.9	31.1	307.3	649.9	46.4	9,346.7	377.1	1,074.3
4095	65.8	16.0	46.8	4.9	15.5	23.2	27.0	331.0	563.1	108.0	13,469.9	490.6	462.0
4096	63.3	29.9	51.2	8.0	19.7	23.6	28.6	410.6	547.4	147.8	18,988	448.3	366.8
4097	46.4	44.6	42.3	9.8	13.6	31.6	31.1	328.0	614.3	65.6	16,057.6	345.2	379.7
4098	14.0	69.6	32.5	14.7	15.3	50.5	13.1	185.6	689.4	30.3	3,819.8	182.6	950.8
4099	15.3	48.0	20.6	10.8	11.1	82.4	0	45.9	554.0	9.2	1,880.9	109.2	572.4
4100	15.5	40.3	27.7	6.5	6.8	93.4	2.8	52.2	383.7	6.1	1,243.8	43.5	214.9
4101	13.6	52.9	44.7	11.0	20.6	45.7	20.4	247.6	498.4	25.7	10,265.2	450	498.4
4102	30.1	55.9	34.7	6.5	20.3	43.4	15.7	206.9	452.3	41.4	14,956.1	487.0	620.8
4103	51.0	35.5	42.4	7.8	29.3	27.9	29.0	384.0	625.3	52.1	16,328.3	386.4	523.9
4104	46.5	35.0	36.9	9.8	17.2	63.6	16.6	235.1	522.4	14.2	15,434.9	622.2	308.7
4105	6.7	53.1	43.4	14.0	26.5	7.5	50.9	397.5	927.5	144.9	8,638.6	571.4	1,060.0
9819	0	0	0	20.9	0	0	0	232.6	3,953.5	0	29.7	20	3,255.8
9820	47.6	0	11.4	26.8	0	82.9	27.3	666.7	4,952.4	95.2	214.2	216.3	6,095.2
9832	7.0	16.6	11.8	10.6	3.2	38.9	0	1,057.7	8,581.7	288.5	2,182.7	2,252.6	4,903.8

Appendix E: Descriptive Statistics by Census Tract: Type of Stop

Census Tract	All Stops	Vehicle		Pedestrian		Bicycle		Other	
	Raw Number	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent
4001	9	5	55.6	2	22.2	0	0	2	22.2
4002	79	67	84.8	9	11.4	1	1.3	2	2.5
4003	321	274	85.4	40	12.5	6	1.9	1	0.3
4004	129	113	87.6	12	9.3	4	3.1	0	0
4005	72	60	83.3	9	12.5	1	1.4	2	2.8
4006	57	44	77.2	12	21.1	1	1.8	0	0
4007	180	142	78.9	36	20	2	1.1	0	0
4008	67	41	61.2	15	22.4	8	11.9	3	4.5
4009	52	20	38.5	21	40.4	8	15.4	3	5.8
4010	449	318	70.8	102	22.7	20	4.5	9	2.0
4011	832	635	76.3	131	15.7	61	7.3	5	0.6
4012	225	171	76	33	14.7	16	7.1	5	2.2
4013	812	379	46.7	343	42.2	81	10.0	9	1.1
4014	733	413	56.3	243	33.2	66	9.0	11	1.5
4015	355	123	34.6	190	53.5	35	9.9	7	2.0
4016	391	246	62.9	114	29.2	25	6.4	6	1.5
4017	265	207	78.1	46	17.4	4	1.5	8	3.0
4018	170	113	66.5	48	28.2	5	2.9	4	2.4
4022	351	239	68.1	81	23.1	24	6.8	7	2.0
4024	100	63	63	29	29	5	5	3	3
4025	213	159	74.6	43	20.2	4	1.9	7	3.3
4026	161	135	83.9	25	15.5	1	0.6	0	0
4027	282	174	61.7	94	33.3	11	3.9	3	1.1
4028	625	370	59.2	203	32.5	45	7.2	7	1.1
4029	988	764	77.3	184	18.6	32	3.2	8	0.8

Appendix E: Descriptive Statistics by Census Tract: Type of Stop

Census Tract	All Stops	Vehicle		Pedestrian		Bicycle		Other	
	Raw Number	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent
4030	735	496	67.5	212	28.8	23	3.1	4	0.5
4031	327	211	64.5	92	28.1	10	3.1	14	4.3
4033	96	55	57.3	38	39.6	2	2.1	1	1.0
4034	212	110	51.9	90	42.5	11	5.2	1	0.5
4035.01	222	174	78.4	37	16.7	9	4.1	2	0.9
4035.02	22	17	77.3	5	22.7	0	0	0	0
4036	30	21	70	9	30	0	0	0	0
4037.01	124	88	71.0	29	23.4	7	5.6	0	0
4037.02	200	145	72.5	47	23.5	8	4	0	0
4038	70	58	82.9	9	12.9	2	2.9	1	1.4
4039	25	14	56	9	36	0	0	2	8
4040	109	80	73.4	25	22.9	2	1.8	2	1.8
4041.01	125	74	59.2	48	38.4	3	2.4	0	0
4041.02	57	38	66.7	18	31.6	0	0	1	1.8
4042	39	36	92.3	3	7.7	0	0	0	0
4043	18	16	88.9	1	5.6	0	0	1	5.6
4044	25	19	76	5	20	0	0	1	4
4045.01	2	2	100	0	0	0	0	0	0
4045.02	41	26	63.4	13	31.7	0	0	2	4.9
4046	11	4	36.4	7	63.6	0	0	0	0
4047	10	7	70	3	30	0	0	0	0
4048	30	19	63.3	10	33.3	0	0	1	3.3
4049	58	46	79.3	10	17.2	1	1.7	1	1.7
4050	67	55	82.1	6	9.0	1	1.5	5	7.5
4051	41	36	87.8	4	9.8	1	2.4	0	0
4052	79	62	78.5	15	19.0	2	2.5	0	0

Appendix E: Descriptive Statistics by Census Tract: Type of Stop

Census Tract	All Stops	Vehicle		Pedestrian		Bicycle		Other	
	Raw Number	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent
4053.01	92	61	66.3	22	23.9	8	8.7	1	1.1
4053.02	85	63	74.1	17	20	3	3.5	2	2.4
4054.01	308	176	57.1	94	30.5	10	3.2	28	9.1
4054.02	189	150	79.4	29	15.3	10	5.3	0	0
4055	76	63	82.9	11	14.5	1	1.3	1	1.3
4056	29	19	65.5	9	31.0	0	0	1	3.4
4057	61	42	68.9	16	26.2	0	0	3	4.9
4058	73	49	67.1	22	30.1	0	0	2	2.7
4059.01	694	510	73.5	148	21.3	16	2.3	20	2.9
4059.02	616	430	69.8	147	23.9	14	2.3	25	4.1
4060	369	272	73.7	75	20.3	9	2.4	13	3.5
4061	495	386	78.0	92	18.6	11	2.2	6	1.2
4062.01	437	261	59.7	159	36.4	13	3.0	4	0.9
4062.02	374	247	66.0	114	30.5	9	2.4	4	1.1
4063	51	29	56.9	16	31.4	0	0	6	11.8
4064	33	27	81.8	6	18.2	0	0	0	0
4065	189	146	77.2	38	20.1	3	1.6	2	1.1
4066.01	76	53	69.7	22	28.9	0	0	1	1.3
4066.02	84	62	73.8	20	23.8	1	1.2	1	1.2
4067	56	48	85.7	7	12.5	1	1.8	0	0
4068	65	55	84.6	8	12.3	1	1.5	1	1.5
4069	67	51	76.1	15	22.4	0	0	1	1.5
4070	158	113	71.5	39	24.7	1	0.6	5	3.2
4071.01	101	78	77.2	17	16.8	3	3.0	3	3.0
4071.02	141	116	82.3	19	13.5	2	1.4	4	2.8
4072	977	728	74.5	211	21.6	28	2.9	10	1.0

Appendix E: Descriptive Statistics by Census Tract: Type of Stop

Census Tract	All Stops	Vehicle		Pedestrian		Bicycle		Other	
	Raw Number	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent
4073	373	292	78.3	65	17.4	13	3.5	3	0.8
4074	625	470	75.2	116	18.6	29	4.6	10	1.6
4075	395	307	77.7	66	16.7	12	3.0	10	2.5
4076	288	222	77.1	62	21.5	2	0.7	2	0.7
4077	157	121	77.1	28	17.8	2	1.3	6	3.8
4078	82	68	82.9	10	12.2	0	0	4	4.9
4079	55	34	61.8	18	32.7	0	0	3	5.5
4080	8	5	62.5	3	37.5	0	0	0	0
4081	34	28	82.4	6	17.6	0	0	0	0
4082	126	82	65.1	32	25.4	1	0.8	11	8.7
4083	283	215	76.0	56	19.8	5	1.8	7	2.5
4084	307	205	66.8	93	30.3	3	1.0	6	2.0
4085	662	438	66.2	185	27.9	28	4.2	11	1.7
4086	550	389	70.7	142	25.8	9	1.6	10	1.8
4087	628	477	76.0	121	19.3	14	2.2	16	2.5
4088	368	251	68.2	103	28.0	6	1.6	8	2.2
4089	226	158	69.9	55	24.3	11	4.9	2	0.9
4090	422	248	58.8	154	36.5	7	1.7	13	3.1
4091	87	48	55.2	35	40.2	3	3.4	1	1.1
4092	57	32	56.1	19	33.3	0	0	6	10.5
4093	263	148	56.3	93	35.4	8	3.0	14	5.3
4094	286	185	64.7	79	27.6	7	2.4	15	5.2
4095	336	205	61.0	97	28.9	17	5.1	17	5.1
4096	1,523	834	54.8	542	35.6	114	7.5	33	2.2
4097	531	399	75.1	103	19.4	16	3.0	13	2.4
4098	176	135	76.7	33	18.8	1	0.6	7	4.0

Appendix E: Descriptive Statistics by Census Tract: Type of Stop

Census Tract	All Stops	Vehicle		Pedestrian		Bicycle		Other	
	Raw Number	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent
4099	28	22	78.6	4	14.3	0	0	2	7.1
4100	8	5	62.5	3	37.5	0	0	0	0
4101	212	154	72.6	48	22.6	8	3.8	2	0.9
4102	167	128	76.6	34	20.4	2	1.2	3	1.8
4103	554	396	71.5	116	20.9	30	5.4	12	2.2
4104	225	156	69.3	52	23.1	8	3.6	9	4
4105	387	302	78.0	67	17.3	14	3.6	4	1.0
9819	433	427	98.6	6	1.4	0	0	0	0
9820	124	108	87.1	15	12.1	1	0.8	0	0
9832	192	119	62.0	60	31.2	7	3.6	6	3.1

Appendix E: Descriptive Statistics by Census Tract: Stops by Reason for Encounter

Census Tract	All Stops	Traffic Violation		Consensual Encounter		Probable Cause		Probation/Parole		Reasonable Suspicion	
	Raw Number	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent
4001	9	5	55.6	1	11.1	3	33.3	0	0	0	0
4002	79	65	82.3	0	0	13	16.5	0	0	1	1.3
4003	321	266	82.9	6	1.9	37	11.5	0	0	12	3.7
4004	129	101	78.3	0	0	25	19.4	0	0	3	2.3
4005	72	61	84.7	2	2.8	7	9.7	1	1.4	1	1.4
4006	57	38	66.7	1	1.8	9	15.8	1	1.8	8	14.0
4007	180	118	65.6	2	1.1	44	24.4	2	1.1	14	7.8
4008	67	46	68.7	1	1.5	8	11.9	2	3.0	10	14.9
4009	52	22	42.3	4	7.7	20	38.5	1	1.9	5	9.6
4010	449	310	69.0	11	2.4	75	16.7	11	2.4	42	9.4
4011	832	645	77.5	15	1.8	132	15.9	12	1.4	28	3.4
4012	225	160	71.1	4	1.8	54	24	0	0	7	3.1
4013	812	423	52.1	37	4.6	255	31.4	14	1.7	83	10.2
4014	733	420	57.3	32	4.4	192	26.2	34	4.6	55	7.5
4015	355	122	34.4	39	11.0	125	35.2	17	4.8	52	14.6
4016	391	225	57.5	31	7.9	79	20.2	11	2.8	45	11.5
4017	265	184	69.4	23	8.7	30	11.3	2	0.8	26	9.8
4018	170	105	61.8	8	4.7	36	21.2	7	4.1	14	8.2
4022	351	234	66.7	12	3.4	63	17.9	5	1.4	37	10.5
4024	100	45	45	6	6	26	26	3	3	20	20
4025	213	149	70.0	7	3.3	41	19.2	4	1.9	12	5.6
4026	161	137	85.1	2	1.2	17	10.6	0	0	5	3.1
4027	282	189	67.0	11	3.9	47	16.7	4	1.4	31	11.0
4028	625	393	62.9	21	3.4	159	25.4	6	1.0	46	7.4

Appendix E: Descriptive Statistics by Census Tract: Stops by Reason for Encounter

Census Tract	All Stops	Traffic Violation		Consensual Encounter		Probable Cause		Probation/Parole		Reasonable Suspicion	
	Raw Number	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent
4029	988	815	82.5	18	1.8	129	13.1	3	0.3	23	2.3
4030	735	515	70.1	21	2.9	162	22.0	3	0.4	34	4.6
4031	327	214	65.4	20	6.1	75	22.9	5	1.5	13	4.0
4033	96	49	51.0	5	5.2	32	33.3	0	0	10	10.4
4034	212	112	52.8	3	1.4	77	36.3	1	0.5	19	9.0
4035.01	222	169	76.1	7	3.2	28	12.6	3	1.4	15	6.8
4035.02	22	14	63.6	0	0	6	27.3	0	0	2	9.1
4036	30	19	63.3	5	16.7	3	10	2	6.7	1	3.3
4037.01	124	88	71.0	6	4.8	26	21.0	1	0.8	3	2.4
4037.02	200	147	73.5	6	3	34	17	1	0.5	12	6
4038	70	52	74.3	0	0	14	20	0	0	4	5.7
4039	25	14	56	4	16	5	20	0	0	2	8
4040	109	72	66.1	4	3.7	20	18.3	2	1.8	11	10.1
4041.01	125	62	49.6	6	4.8	38	30.4	3	2.4	16	12.8
4041.02	57	37	64.9	0	0	11	19.3	3	5.3	6	10.5
4042	39	33	84.6	0	0	4	10.3	0	0	2	5.1
4043	18	14	77.8	0	0	1	5.6	0	0	3	16.7
4044	25	19	76	0	0	4	16	0	0	2	8
4045.01	2	2	100	0	0	0	0	0	0	0	0
4045.02	41	25	61.0	1	2.4	8	19.5	0	0	7	17.1
4046	11	3	27.3	0	0	5	45.5	0	0	3	27.3
4047	10	4	40	3	30	2	20	0	0	1	10
4048	30	16	53.3	4	13.3	7	23.3	1	3.3	2	6.7
4049	58	44	75.9	3	5.2	7	12.1	0	0	4	6.9

Appendix E: Descriptive Statistics by Census Tract: Stops by Reason for Encounter

Census Tract	All Stops	Traffic Violation		Consensual Encounter		Probable Cause		Probation/Parole		Reasonable Suspicion	
	Raw Number	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent
4050	67	53	79.1	4	6.0	6	9.0	0	0	4	6.0
4051	41	35	85.4	2	4.9	4	9.8	0	0	0	0
4052	79	57	72.2	9	11.4	8	10.1	0	0	5	6.3
4053.01	92	63	68.5	7	7.6	13	14.1	0	0	9	9.8
4053.02	85	61	71.8	5	5.9	12	14.1	1	1.2	6	7.1
4054.01	308	158	51.3	15	4.9	91	29.5	13	4.2	31	10.1
4054.02	189	139	73.5	3	1.6	25	13.2	2	1.1	20	10.6
4055	76	59	77.6	7	9.2	10	13.2	0	0	0	0
4056	29	18	62.1	1	3.4	7	24.1	0	0	3	10.3
4057	61	33	54.1	5	8.2	14	23.0	3	4.9	6	9.8
4058	73	47	64.4	5	6.8	12	16.4	0	0	9	12.3
4059.01	694	425	61.2	29	4.2	164	23.6	13	1.9	63	9.1
4059.02	616	359	58.3	25	4.1	132	21.4	9	1.5	91	14.8
4060	369	234	63.4	18	4.9	89	24.1	8	2.2	20	5.4
4061	495	325	65.7	18	3.6	111	22.4	2	0.4	39	7.9
4062.01	437	193	44.2	16	3.7	160	36.6	6	1.4	62	14.2
4062.02	374	210	56.1	11	2.9	102	27.3	2	0.5	49	13.1
4063	51	15	29.4	6	11.8	24	47.1	1	2.0	5	9.8
4064	33	19	57.6	5	15.2	4	12.1	0	0	5	15.2
4065	189	118	62.4	8	4.2	49	25.9	1	0.5	13	6.9
4066.01	76	43	56.6	9	11.8	13	17.1	0	0	11	14.5
4066.02	84	52	61.9	4	4.8	15	17.9	2	2.4	11	13.1
4067	56	42	75	0	0	9	16.1	0	0	5	8.9
4068	65	48	73.8	1	1.5	10	15.4	0	0	6	9.2

Appendix E: Descriptive Statistics by Census Tract: Stops by Reason for Encounter

Census Tract	All Stops	Traffic Violation		Consensual Encounter		Probable Cause		Probation/Parole		Reasonable Suspicion	
	Raw Number	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent
4069	67	44	65.7	4	6.0	6	9.0	1	1.5	12	17.9
4070	158	111	70.3	3	1.9	36	22.8	3	1.9	5	3.2
4071.01	101	73	72.3	3	3.0	18	17.8	1	1.0	6	5.9
4071.02	141	106	75.2	3	2.1	24	17.0	1	0.7	7	5.0
4072	977	701	71.8	19	1.9	156	16.0	6	0.6	95	9.7
4073	373	253	67.8	19	5.1	68	18.2	7	1.9	26	7.0
4074	625	452	72.3	11	1.8	114	18.2	2	0.3	46	7.4
4075	395	297	75.2	21	5.3	49	12.4	13	3.3	15	3.8
4076	288	190	66.0	9	3.1	53	18.4	3	1.0	33	11.5
4077	157	117	74.5	10	6.4	15	9.6	6	3.8	9	5.7
4078	82	69	84.1	3	3.7	9	11.0	0	0	1	1.2
4079	55	27	49.1	5	9.1	14	25.5	2	3.6	7	12.7
4080	8	4	50	0	0	4	50	0	0	0	0
4081	34	7	20.6	21	61.8	4	11.8	0	0	2	5.9
4082	126	62	49.2	7	5.6	37	29.4	3	2.4	17	13.5
4083	283	195	68.9	12	4.2	50	17.7	5	1.8	21	7.4
4084	307	144	46.9	21	6.8	91	29.6	14	4.6	37	12.1
4085	662	376	56.8	32	4.8	170	25.7	8	1.2	76	11.5
4086	550	349	63.5	38	6.9	113	20.5	17	3.1	33	6
4087	628	436	69.4	22	3.5	102	16.2	13	2.1	55	8.8
4088	368	208	56.5	24	6.5	72	19.6	15	4.1	49	13.3
4089	226	144	63.7	11	4.9	51	22.6	6	2.7	14	6.2
4090	422	194	46.0	40	9.5	112	26.5	25	5.9	51	12.1
4091	87	31	35.6	11	12.6	24	27.6	3	3.4	18	20.7

Appendix E: Descriptive Statistics by Census Tract: Stops by Reason for Encounter

Census Tract	All Stops	Traffic Violation		Consensual Encounter		Probable Cause		Probation/Parole		Reasonable Suspicion	
	Raw Number	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent
4092	57	28	49.1	1	1.8	17	29.8	4	7.0	7	12.3
4093	263	123	46.8	15	5.7	71	27.0	7	2.7	47	17.9
4094	286	159	55.6	17	5.9	64	22.4	6	2.1	40	14.0
4095	336	189	56.2	17	5.1	91	27.1	3	0.9	36	10.7
4096	1,523	795	52.2	86	5.6	411	27.0	36	2.4	195	12.8
4097	531	347	65.3	23	4.3	85	16.0	16	3.0	60	11.3
4098	176	115	65.3	5	2.8	33	18.8	2	1.1	21	11.9
4099	28	22	78.6	1	3.6	2	7.1	1	3.6	2	7.1
4100	8	2	25	2	25	1	12.5	0	0	3	37.5
4101	212	132	62.3	9	4.2	41	19.3	2	0.9	28	13.2
4102	167	107	64.1	13	7.8	33	19.8	7	4.2	7	4.2
4103	554	375	67.7	16	2.9	98	17.7	15	2.7	50	9.0
4104	225	143	63.6	10	4.4	45	20	8	3.6	19	8.4
4105	387	297	76.7	11	2.8	54	14.0	1	0.3	24	6.2
9819	433	424	97.9	1	0.2	8	1.8	0	0	0	0
9820	124	109	87.9	3	2.4	8	6.5	0	0	4	3.2
9832	192	127	66.1	14	7.3	40	20.8	2	1.0	9	4.7

Appendix E: Descriptive Statistics by Census Tract: Stops by Age and Gender

Census Tract	All Stops	Male		Female		Under 17 years		18 to 29 years		30 to 39 years		Over 40 years	
	Raw Number	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent
4001	9	8	88.9	1	11.1	1	11.1	4	44.4	2	22.2	2	22.2
4002	79	56	70.9	23	29.1	0	0	17	21.5	20	25.3	42	53.2
4003	321	201	62.6	120	37.4	2	0.6	110	34.3	89	27.7	120	37.4
4004	129	72	55.8	57	44.2	0	0	47	36.4	27	20.9	55	42.6
4005	72	43	59.7	29	40.3	0	0	22	30.6	20	27.8	30	41.7
4006	57	37	64.9	20	35.1	3	5.3	18	31.6	10	17.5	26	45.6
4007	180	137	76.1	43	23.9	7	3.9	69	38.3	33	18.3	71	39.4
4008	67	51	76.1	16	23.9	9	13.4	28	41.8	12	17.9	18	26.9
4009	52	44	84.6	8	15.4	8	15.4	13	25	10	19.2	21	40.4
4010	449	326	72.6	123	27.4	8	1.8	164	36.5	97	21.6	180	40.1
4011	832	561	67.4	271	32.6	9	1.1	274	32.9	219	26.3	330	39.7
4012	225	151	67.1	74	32.9	3	1.3	75	33.3	69	30.7	78	34.7
4013	812	612	75.4	200	24.6	7	0.9	199	24.5	211	26.0	395	48.6
4014	733	562	76.7	170	23.2	15	2.0	234	31.9	179	24.4	305	41.6
4015	355	297	83.7	58	16.3	3	0.8	90	25.4	58	16.3	204	57.5
4016	391	287	73.4	104	26.6	8	2.0	131	33.5	116	29.7	136	34.8
4017	265	236	89.1	29	10.9	4	1.5	112	42.3	42	15.8	107	40.4
4018	170	131	77.1	39	22.9	4	2.4	70	41.2	34	20	62	36.5
4022	351	272	77.5	79	22.5	8	2.3	143	40.7	70	19.9	130	37.0
4024	100	80	80	20	20	12	12	30	30	24	24	34	34
4025	213	151	70.9	62	29.1	9	4.2	81	38.0	51	23.9	72	33.8
4026	161	109	67.7	52	32.3	3	1.9	52	32.3	45	28.0	61	37.9

Appendix E: Descriptive Statistics by Census Tract: Stops by Age and Gender

Census Tract	All Stops	Male		Female		Under 17 years		18 to 29 years		30 to 39 years		Over 40 years	
	Raw Number	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent
4027	282	211	74.8	71	25.2	4	1.4	91	32.3	69	24.5	118	41.8
4028	625	447	71.5	178	28.5	7	1.1	231	37.0	143	22.9	244	39.0
4029	988	694	70.2	294	29.8	14	1.4	397	40.2	272	27.5	305	30.9
4030	735	533	72.5	202	27.5	6	0.8	279	38.0	162	22.0	288	39.2
4031	327	234	71.6	93	28.4	2	0.6	118	36.1	83	25.4	124	37.9
4033	96	75	78.1	21	21.9	1	1.0	35	36.5	19	19.8	41	42.7
4034	212	163	76.9	49	23.1	3	1.4	67	31.6	51	24.1	91	42.9
4035.01	222	143	64.4	79	35.6	4	1.8	80	36.0	66	29.7	72	32.4
4035.02	22	18	81.8	4	18.2	1	4.5	14	63.6	3	13.6	4	18.2
4036	30	17	56.7	13	43.3	1	3.3	10	33.3	14	46.7	5	16.7
4037.01	124	94	75.8	30	24.2	2	1.6	39	31.5	30	24.2	53	42.7
4037.02	200	146	73	54	27	3	1.5	56	28	60	30	81	40.5
4038	70	49	70	21	30	1	1.4	19	27.1	13	18.6	37	52.9
4039	25	19	76	6	24	0	0	8	32	8	32	9	36
4040	109	66	60.6	43	39.4	1	0.9	28	25.7	30	27.5	50	45.9
4041.01	125	93	74.4	32	25.6	2	1.6	29	23.2	21	16.8	73	58.4
4041.02	57	41	71.9	16	28.1	0	0	6	10.5	11	19.3	40	70.2
4042	39	21	53.8	18	46.2	0	0	11	28.2	8	20.5	20	51.3
4043	18	13	72.2	4	22.2	0	0	4	22.2	5	27.8	9	50
4044	25	18	72	7	28	2	8	5	20	6	24	12	48
4045.01	2	1	50	1	50	0	0	0	0	0	0	2	100
4045.02	41	30	73.2	11	26.8	2	4.9	6	14.6	4	9.8	29	70.7

Appendix E: Descriptive Statistics by Census Tract: Stops by Age and Gender

Census Tract	All Stops	Male		Female		Under 17 years		18 to 29 years		30 to 39 years		Over 40 years	
	Raw Number	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent
4046	11	9	81.8	2	18.2	3	27.3	2	18.2	3	27.3	3	27.3
4047	10	6	60	4	40	1	10	4	40	1	10	4	40
4048	30	18	60	12	40	0	0	4	13.3	8	26.7	18	60
4049	58	41	70.7	17	29.3	2	3.4	16	27.6	13	22.4	27	46.6
4050	67	43	64.2	24	35.8	2	3.0	19	28.4	16	23.9	30	44.8
4051	41	22	53.7	19	46.3	1	2.4	8	19.5	4	9.8	28	68.3
4052	79	49	62.0	29	36.7	1	1.3	24	30.4	17	21.5	37	46.8
4053.01	92	65	70.7	27	29.3	0	0	33	35.9	33	35.9	26	28.3
4053.02	85	60	70.6	25	29.4	1	1.2	37	43.5	17	20	30	35.3
4054.01	308	239	77.6	69	22.4	4	1.3	98	31.8	73	23.7	133	43.2
4054.02	189	154	81.5	35	18.5	3	1.6	71	37.6	35	18.5	80	42.3
4055	76	57	75	19	25	1	1.3	28	36.8	21	27.6	26	34.2
4056	29	22	75.9	7	24.1	1	3.4	12	41.4	4	13.8	12	41.4
4057	61	45	73.8	16	26.2	1	1.6	28	45.9	13	21.3	19	31.1
4058	73	54	74.0	19	26.0	3	4.1	40	54.8	12	16.4	18	24.7
4059.01	694	492	70.9	202	29.1	18	2.6	369	53.2	137	19.7	170	24.5
4059.02	616	419	68.0	196	31.8	17	2.8	315	51.1	121	19.6	163	26.5
4060	369	278	75.3	90	24.4	7	1.9	147	39.8	97	26.3	118	32.0
4061	495	388	78.4	107	21.6	12	2.4	232	46.9	102	20.6	149	30.1
4062.01	437	302	69.1	135	30.9	19	4.3	242	55.4	77	17.6	99	22.7
4062.02	374	228	61.0	145	38.8	11	2.9	231	61.8	62	16.6	70	18.7
4063	51	33	64.7	18	35.3	4	7.8	25	49.0	12	23.5	10	19.6

Appendix E: Descriptive Statistics by Census Tract: Stops by Age and Gender

Census Tract	All Stops	Male		Female		Under 17 years		18 to 29 years		30 to 39 years		Over 40 years	
	Raw Number	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent
4064	33	26	78.8	7	21.2	2	6.1	15	45.5	11	33.3	5	15.2
4065	189	129	68.3	60	31.7	5	2.6	86	45.5	46	24.3	52	27.5
4066.01	76	57	75	19	25	1	1.3	40	52.6	14	18.4	21	27.6
4066.02	84	67	79.8	17	20.2	6	7.1	38	45.2	16	19.0	24	28.6
4067	56	41	73.2	15	26.8	2	3.6	24	42.9	13	23.2	17	30.4
4068	65	40	61.5	25	38.5	4	6.2	21	32.3	14	21.5	26	40
4069	67	46	68.7	21	31.3	1	1.5	34	50.7	12	17.9	20	29.9
4070	158	116	73.4	42	26.6	1	0.6	64	40.5	36	22.8	57	36.1
4071.01	101	75	74.3	26	25.7	6	5.9	61	60.4	15	14.9	19	18.8
4071.02	141	102	72.3	39	27.7	9	6.4	69	48.9	23	16.3	40	28.4
4072	977	711	72.8	266	27.2	27	2.8	489	50.1	219	22.4	242	24.8
4073	373	311	83.4	62	16.6	16	4.3	186	49.9	71	19.0	100	26.8
4074	625	490	78.4	135	21.6	23	3.7	284	45.4	137	21.9	181	29.0
4075	395	303	76.7	92	23.3	21	5.3	199	50.4	92	23.3	83	21.0
4076	288	217	75.3	71	24.7	16	5.6	162	56.2	56	19.4	54	18.8
4077	157	115	73.2	42	26.8	6	3.8	74	47.1	34	21.7	43	27.4
4078	82	55	67.1	27	32.9	4	4.9	35	42.7	18	22.0	25	30.5
4079	55	36	65.5	19	34.5	1	1.8	12	21.8	22	40	20	36.4
4080	8	6	75	2	25	0	0	4	50	1	12.5	3	37.5
4081	34	18	52.9	16	47.1	0	0	29	85.3	1	2.9	4	11.8
4082	126	99	78.6	27	21.4	6	4.8	68	54.0	26	20.6	26	20.6
4083	283	203	71.7	79	27.9	13	4.6	147	51.9	60	21.2	63	22.3

Appendix E: Descriptive Statistics by Census Tract: Stops by Age and Gender

Census Tract	All Stops	Male		Female		Under 17 years		18 to 29 years		30 to 39 years		Over 40 years	
	Raw Number	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent
4084	307	235	76.5	72	23.5	15	4.9	187	60.9	67	21.8	38	12.4
4085	662	518	78.2	144	21.8	39	5.9	314	47.4	146	22.1	163	24.6
4086	550	421	76.5	129	23.5	27	4.9	259	47.1	104	18.9	160	29.1
4087	628	486	77.4	142	22.6	37	5.9	310	49.4	127	20.2	154	24.5
4088	368	297	80.7	71	19.3	11	3.0	173	47.0	86	23.4	98	26.6
4089	226	185	81.9	41	18.1	4	1.8	104	46.0	54	23.9	64	28.3
4090	422	328	77.7	94	22.3	5	1.2	194	46.0	72	17.1	151	35.8
4091	87	66	75.9	21	24.1	1	1.1	21	24.1	12	13.8	53	60.9
4092	57	47	82.5	10	17.5	2	3.5	35	61.4	7	12.3	13	22.8
4093	263	213	81.0	50	19.0	7	2.7	117	44.5	54	20.5	85	32.3
4094	286	237	82.9	48	16.8	9	3.1	151	52.8	59	20.6	67	23.4
4095	336	268	79.8	68	20.2	15	4.5	161	47.9	69	20.5	91	27.1
4096	1,523	1,235	81.1	288	18.9	41	2.7	582	38.2	373	24.5	527	34.6
4097	531	429	80.8	102	19.2	35	6.6	311	58.6	93	17.5	92	17.3
4098	176	135	76.7	41	23.3	9	5.1	85	48.3	44	25	38	21.6
4099	28	22	78.6	6	21.4	0	0	12	42.9	7	25	9	32.1
4100	8	6	75	2	25	0	0	5	62.5	2	25	1	12.5
4101	212	164	77.4	48	22.6	5	2.4	101	47.6	57	26.9	49	23.1
4102	167	133	79.6	34	20.4	7	4.2	78	46.7	36	21.6	46	27.5
4103	554	421	76.0	133	24.0	25	4.5	236	42.6	122	22.0	171	30.9
4104	225	173	76.9	52	23.1	8	3.6	105	46.7	45	20	67	29.8
4105	387	280	72.4	107	27.6	11	2.8	151	39.0	83	21.4	142	36.7

Appendix E: Descriptive Statistics by Census Tract: Stops by Age and Gender

Census Tract	All Stops	Male		Female		Under 17 years		18 to 29 years		30 to 39 years		Over 40 years	
	Raw Number	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent
9819	433	398	91.9	35	8.1	10	2.3	279	64.4	57	13.2	87	20.1
9820	124	109	87.9	15	12.1	1	0.8	51	41.1	15	12.1	57	46.0
9832	192	139	72.4	53	27.6	2	1.0	74	38.5	38	19.8	78	40.6

Appendix E: Descriptive Statistics by Census Tract: Stops by Race

Census Tract	All Stops	White		African American		Hispanic		Asian		Other	
	Raw Number	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent
4001	9	6	66.7	0	0	1	11.1	0	0	2	22.2
4002	79	44	55.7	16	20.3	7	8.9	10	12.7	2	2.5
4003	321	132	41.1	119	37.1	22	6.9	33	10.3	15	4.7
4004	129	59	45.7	40	31.0	10	7.8	13	10.1	7	5.4
4005	72	30	41.7	24	33.3	6	8.3	9	12.5	3	4.2
4006	57	17	29.8	32	56.1	1	1.8	4	7.0	3	5.3
4007	180	28	15.6	126	70	15	8.3	8	4.4	3	1.7
4008	67	12	17.9	52	77.6	0	0	3	4.5	0	0
4009	52	7	13.5	37	71.2	5	9.6	1	1.9	2	3.8
4010	449	88	19.6	311	69.3	20	4.5	20	4.5	10	2.2
4011	832	247	29.7	403	48.4	71	8.5	77	9.3	34	4.1
4012	225	77	34.2	104	46.2	19	8.4	14	6.2	11	4.9
4013	812	159	19.6	557	68.6	33	4.1	36	4.4	27	3.3
4014	733	57	7.8	599	81.7	48	6.5	12	1.6	17	2.3
4015	355	19	5.4	317	89.3	12	3.4	5	1.4	2	0.6
4016	391	49	12.5	307	78.5	21	5.4	11	2.8	3	0.8
4017	265	37	14.0	131	49.4	65	24.5	23	8.7	9	3.4
4018	170	18	10.6	131	77.1	15	8.8	3	1.8	3	1.8
4022	351	39	11.1	252	71.8	42	12.0	10	2.8	8	2.3
4024	100	7	7	81	81	6	6	4	4	2	2
4025	213	27	12.7	154	72.3	15	7.0	12	5.6	5	2.3
4026	161	47	29.2	85	52.8	11	6.8	11	6.8	7	4.3
4027	282	56	19.9	189	67.0	15	5.3	16	5.7	6	2.1
4028	625	137	21.9	371	59.4	45	7.2	41	6.6	31	5.0

Appendix E: Descriptive Statistics by Census Tract: Stops by Race

Census Tract	All Stops	White		African American		Hispanic		Asian		Other	
	Raw Number	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent
4029	988	253	25.6	516	52.2	95	9.6	68	6.9	56	5.7
4030	735	149	20.3	392	53.3	68	9.3	95	12.9	31	4.2
4031	327	68	20.8	174	53.2	32	9.8	42	12.8	11	3.4
4033	96	19	19.8	46	47.9	8	8.3	18	18.8	5	5.2
4034	212	44	20.8	108	50.9	23	10.8	27	12.7	10	4.7
4035.01	222	54	24.3	110	49.5	20	9.0	29	13.1	9	4.1
4035.02	22	6	27.3	14	63.6	1	4.5	0	0	1	4.5
4036	30	6	20	17	56.7	2	6.7	5	16.7	0	0
4037.01	124	38	30.6	65	52.4	5	4.0	10	8.1	6	4.8
4037.02	200	53	26.5	111	55.5	15	7.5	15	7.5	6	3
4038	70	26	37.1	22	31.4	9	12.9	7	10	6	8.6
4039	25	10	40	10	40	3	12	2	8	0	0
4040	109	29	26.6	58	53.2	8	7.3	9	8.3	5	4.6
4041.01	125	43	34.4	63	50.4	11	8.8	5	4	3	2.4
4041.02	57	23	40.4	20	35.1	6	10.5	6	10.5	2	3.5
4042	39	16	41.0	12	30.8	3	7.7	7	17.9	1	2.6
4043	18	7	38.9	9	50	1	5.6	0	0	1	5.6
4044	25	14	56	6	24	2	8	3	12	0	0
4045.01	2	1	50	0	0	0	0	1	50	0	0
4045.02	41	26	63.4	10	24.4	0	0	3	7.3	2	4.9
4046	11	4	36.4	4	36.4	2	18.2	0	0	1	9.1
4047	10	4	40	3	30	1	10	2	20	0	0
4048	30	6	20	21	70	1	3.3	1	3.3	1	3.3
4049	58	20	34.5	21	36.2	6	10.3	10	17.2	1	1.7

Appendix E: Descriptive Statistics by Census Tract: Stops by Race

Census Tract	All Stops	White		African American		Hispanic		Asian		Other	
	Raw Number	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent
4050	67	24	35.8	26	38.8	7	10.4	10	14.9	0	0
4051	41	22	53.7	9	22.0	1	2.4	4	9.8	5	12.2
4052	79	20	25.3	39	49.4	9	11.4	6	7.6	5	6.3
4053.01	92	22	23.9	46	50	6	6.5	16	17.4	2	2.2
4053.02	85	13	15.3	41	48.2	11	12.9	17	20	3	3.5
4054.01	308	25	8.1	122	39.6	40	13.0	103	33.4	18	5.8
4054.02	189	16	8.5	99	52.4	30	15.9	31	16.4	13	6.9
4055	76	9	11.8	33	43.4	6	7.9	20	26.3	8	10.5
4056	29	6	20.7	12	41.4	4	13.8	7	24.1	0	0
4057	61	6	9.8	36	59.0	11	18.0	7	11.5	1	1.6
4058	73	3	4.1	42	57.5	13	17.8	14	19.2	1	1.4
4059.01	694	59	8.5	351	50.6	145	20.9	114	16.4	25	3.6
4059.02	616	44	7.1	317	51.5	127	20.6	111	18.0	17	2.8
4060	369	48	13.0	163	44.2	63	17.1	65	17.6	30	8.1
4061	495	61	12.3	192	38.8	189	38.2	34	6.9	19	3.8
4062.01	437	34	7.8	267	61.1	96	22.0	30	6.9	10	2.3
4062.02	374	24	6.4	200	53.5	111	29.7	29	7.8	10	2.7
4063	51	1	2.0	31	60.8	11	21.6	6	11.8	2	3.9
4064	33	2	6.1	22	66.7	9	27.3	0	0	0	0
4065	189	12	6.3	108	57.1	47	24.9	16	8.5	6	3.2
4066.01	76	12	15.8	39	51.3	12	15.8	8	10.5	5	6.6
4066.02	84	12	14.3	49	58.3	19	22.6	4	4.8	0	0
4067	56	12	21.4	23	41.1	7	12.5	8	14.3	6	10.7
4068	65	5	7.7	44	67.7	6	9.2	7	10.8	3	4.6

Appendix E: Descriptive Statistics by Census Tract: Stops by Race

Census Tract	All Stops	White		African American		Hispanic		Asian		Other	
	Raw Number	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent
4069	67	4	6.0	46	68.7	9	13.4	5	7.5	3	4.5
4070	158	21	13.3	98	62.0	28	17.7	7	4.4	4	2.5
4071.01	101	8	7.9	40	39.6	46	45.5	3	3.0	4	4.0
4071.02	141	6	4.3	80	56.7	42	29.8	7	5.0	6	4.3
4072	977	93	9.5	392	40.1	405	41.5	61	6.2	26	2.7
4073	373	46	12.3	152	40.8	154	41.3	12	3.2	9	2.4
4074	625	32	5.1	311	49.8	230	36.8	27	4.3	25	4
4075	395	16	4.1	256	64.8	98	24.8	12	3.0	13	3.3
4076	288	26	9.0	200	69.4	52	18.1	4	1.4	6	2.1
4077	157	9	5.7	120	76.4	21	13.4	1	0.6	6	3.8
4078	82	9	11.0	56	68.3	16	19.5	1	1.2	0	0
4079	55	17	30.9	29	52.7	5	9.1	3	5.5	1	1.8
4080	8	3	37.5	3	37.5	1	12.5	1	12.5	0	0
4081	34	4	11.8	18	52.9	4	11.8	5	14.7	3	8.8
4082	126	4	3.2	108	85.7	8	6.3	2	1.6	4	3.2
4083	283	10	3.5	228	80.6	34	12.0	3	1.1	8	2.8
4084	307	9	2.9	238	77.5	51	16.6	3	1.0	6	2.0
4085	662	9	1.4	499	75.4	126	19.0	10	1.5	18	2.7
4086	550	17	3.1	390	70.9	123	22.4	10	1.8	10	1.8
4087	628	23	3.7	443	70.5	131	20.9	18	2.9	13	2.1
4088	368	11	3.0	238	64.7	103	28.0	8	2.2	8	2.2
4089	226	6	2.7	157	69.5	57	25.2	5	2.2	1	0.4
4090	422	52	12.3	247	58.5	94	22.3	12	2.8	17	4.0
4091	87	3	3.4	74	85.1	10	11.5	0	0	0	0

Appendix E: Descriptive Statistics by Census Tract: Stops by Race

Census Tract	All Stops	White		African American		Hispanic		Asian		Other	
	Raw Number	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent
4092	57	1	1.8	42	73.7	13	22.8	0	0	1	1.8
4093	263	12	4.6	165	62.7	79	30.0	6	2.3	1	0.4
4094	286	8	2.8	164	57.3	104	36.4	2	0.7	8	2.8
4095	336	7	2.1	208	61.9	107	31.8	7	2.1	7	2.1
4096	1,523	38	2.5	1,173	77.0	284	18.6	15	1.0	13	0.9
4097	531	6	1.1	430	81.0	81	15.3	6	1.1	8	1.5
4098	176	8	4.5	140	79.5	20	11.4	2	1.1	6	3.4
4099	28	4	14.3	13	46.4	6	21.4	2	7.1	3	10.7
4100	8	1	12.5	4	50	1	12.5	0	0	2	25
4101	212	12	5.7	164	77.4	27	12.7	2	0.9	7	3.3
4102	167	10	6.0	124	74.3	29	17.4	2	1.2	2	1.2
4103	554	20	3.6	380	68.6	134	24.2	12	2.2	8	1.4
4104	225	20	8.9	124	55.1	66	29.3	5	2.2	10	4.4
4105	387	56	14.5	257	66.4	31	8.0	32	8.3	11	2.8
9819	433	83	19.2	76	17.6	200	46.2	53	12.2	21	4.8
9820	124	20	16.1	51	41.1	33	26.6	15	12.1	5	4.0
9832	192	65	33.9	83	43.2	19	9.9	15	7.8	10	5.2

Appendix E: Descriptive Statistics by Census Tract: Time of Stop and Day of Week

Census Tract	All Stops	Day (7am to 7pm)		Night (7pm to 7am)		Week (Monday to Thursday)		Weekend (Friday to Sunday)	
	Raw Number	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent
4001	9	6	66.7	3	33.3	6	66.7	3	33.3
4002	79	49	62.0	30	38.0	37	46.8	42	53.2
4003	321	163	50.8	158	49.2	203	63.2	118	36.8
4004	129	35	27.1	94	72.9	90	69.8	39	30.2
4005	72	41	56.9	31	43.1	36	50	36	50
4006	57	39	68.4	18	31.6	30	52.6	27	47.4
4007	180	132	73.3	48	26.7	112	62.2	68	37.8
4008	67	46	68.7	21	31.3	36	53.7	31	46.3
4009	52	35	67.3	17	32.7	32	61.5	20	38.5
4010	449	282	62.8	167	37.2	267	59.5	182	40.5
4011	832	288	34.6	544	65.4	536	64.4	296	35.6
4012	225	104	46.2	121	53.8	155	68.9	70	31.1
4013	812	438	53.9	374	46.1	532	65.5	280	34.5
4014	733	451	61.5	282	38.5	449	61.3	284	38.7
4015	355	260	73.2	95	26.8	233	65.6	122	34.4
4016	391	281	71.9	110	28.1	261	66.8	130	33.2
4017	265	154	58.1	111	41.9	166	62.6	99	37.4
4018	170	81	47.6	89	52.4	89	52.4	81	47.6
4022	351	169	48.1	182	51.9	207	59.0	144	41.0
4024	100	70	70	30	30	74	74	26	26
4025	213	111	52.1	102	47.9	135	63.4	78	36.6
4026	161	106	65.8	55	34.2	94	58.4	67	41.6
4027	282	193	68.4	89	31.6	159	56.4	123	43.6
4028	625	272	43.5	353	56.5	397	63.5	228	36.5

Appendix E: Descriptive Statistics by Census Tract: Time of Stop and Day of Week

Census Tract	All Stops	Day (7am to 7pm)		Night (7pm to 7am)		Week (Monday to Thursday)		Weekend (Friday to Sunday)	
	Raw Number	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent
4029	988	286	28.9	702	71.1	483	48.9	505	51.1
4030	735	320	43.5	415	56.5	390	53.1	345	46.9
4031	327	148	45.3	179	54.7	181	55.4	146	44.6
4033	96	65	67.7	31	32.3	66	68.8	30	31.2
4034	212	120	56.6	92	43.4	138	65.1	74	34.9
4035.01	222	86	38.7	136	61.3	117	52.7	105	47.3
4035.02	22	11	50	11	50	13	59.1	9	40.9
4036	30	16	53.3	14	46.7	19	63.3	11	36.7
4037.01	124	42	33.9	82	66.1	84	67.7	40	32.3
4037.02	200	90	45	110	55	120	60	80	40
4038	70	33	47.1	37	52.9	41	58.6	29	41.4
4039	25	17	68	8	32	20	80	5	20
4040	109	54	49.5	55	50.5	74	67.9	35	32.1
4041.01	125	60	48	65	52	75	60	50	40
4041.02	57	30	52.6	27	47.4	37	64.9	20	35.1
4042	39	22	56.4	17	43.6	26	66.7	13	33.3
4043	18	13	72.2	5	27.8	11	61.1	7	38.9
4044	25	20	80	5	20	19	76	6	24
4045.01	2	2	100	0	0	2	100	0	0
4045.02	41	34	82.9	7	17.1	29	70.7	12	29.3
4046	11	5	45.5	6	54.5	7	63.6	4	36.4
4047	10	6	60	4	40	5	50	5	50
4048	30	17	56.7	13	43.3	17	56.7	13	43.3
4049	58	33	56.9	25	43.1	31	53.4	27	46.6

Appendix E: Descriptive Statistics by Census Tract: Time of Stop and Day of Week

Census Tract	All Stops	Day (7am to 7pm)		Night (7pm to 7am)		Week (Monday to Thursday)		Weekend (Friday to Sunday)	
	Raw Number	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent
4050	67	44	65.7	23	34.3	47	70.1	20	29.9
4051	41	32	78.0	9	22.0	29	70.7	12	29.3
4052	79	40	50.6	39	49.4	47	59.5	32	40.5
4053.01	92	34	37.0	58	63.0	54	58.7	38	41.3
4053.02	85	34	40	51	60	45	52.9	40	47.1
4054.01	308	179	58.1	129	41.9	185	60.1	123	39.9
4054.02	189	64	33.9	125	66.1	109	57.7	80	42.3
4055	76	42	55.3	34	44.7	44	57.9	32	42.1
4056	29	19	65.5	10	34.5	18	62.1	11	37.9
4057	61	40	65.6	21	34.4	35	57.4	26	42.6
4058	73	56	76.7	17	23.3	41	56.2	32	43.8
4059.01	694	346	49.9	348	50.1	473	68.2	221	31.8
4059.02	616	292	47.4	324	52.6	400	64.9	216	35.1
4060	369	178	48.2	191	51.8	224	60.7	145	39.3
4061	495	302	61.0	193	39.0	329	66.5	166	33.5
4062.01	437	259	59.3	178	40.7	278	63.6	159	36.4
4062.02	374	182	48.7	192	51.3	244	65.2	130	34.8
4063	51	38	74.5	13	25.5	25	49.0	26	51.0
4064	33	23	69.7	10	30.3	22	66.7	11	33.3
4065	189	87	46.0	102	54.0	123	65.1	66	34.9
4066.01	76	40	52.6	36	47.4	37	48.7	39	51.3
4066.02	84	49	58.3	35	41.7	48	57.1	36	42.9
4067	56	12	21.4	44	78.6	31	55.4	25	44.6
4068	65	37	56.9	28	43.1	37	56.9	28	43.1

Appendix E: Descriptive Statistics by Census Tract: Time of Stop and Day of Week

Census Tract	All Stops	Day (7am to 7pm)		Night (7pm to 7am)		Week (Monday to Thursday)		Weekend (Friday to Sunday)	
	Raw Number	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent
4069	67	42	62.7	25	37.3	33	49.3	34	50.7
4070	158	110	69.6	48	30.4	110	69.6	48	30.4
4071.01	101	71	70.3	30	29.7	69	68.3	32	31.7
4071.02	141	94	66.7	47	33.3	88	62.4	53	37.6
4072	977	465	47.6	512	52.4	664	68.0	313	32.0
4073	373	208	55.8	165	44.2	182	48.8	191	51.2
4074	625	336	53.8	289	46.2	374	59.8	251	40.2
4075	395	248	62.8	147	37.2	227	57.5	168	42.5
4076	288	188	65.3	100	34.7	184	63.9	104	36.1
4077	157	123	78.3	34	21.7	112	71.3	45	28.7
4078	82	63	76.8	19	23.2	66	80.5	16	19.5
4079	55	32	58.2	23	41.8	39	70.9	16	29.1
4080	8	7	87.5	1	12.5	6	75	2	25
4081	34	13	38.2	21	61.8	25	73.5	9	26.5
4082	126	82	65.1	44	34.9	88	69.8	38	30.2
4083	283	210	74.2	73	25.8	204	72.1	79	27.9
4084	307	193	62.9	114	37.1	193	62.9	114	37.1
4085	662	397	60.0	265	40.0	411	62.1	251	37.9
4086	550	336	61.1	214	38.9	344	62.5	206	37.5
4087	628	440	70.1	188	29.9	376	59.9	252	40.1
4088	368	278	75.5	90	24.5	208	56.5	160	43.5
4089	226	156	69.0	70	31.0	128	56.6	98	43.4
4090	422	278	65.9	144	34.1	260	61.6	162	38.4
4091	87	43	49.4	44	50.6	58	66.7	29	33.3

Appendix E: Descriptive Statistics by Census Tract: Time of Stop and Day of Week

Census Tract	All Stops	Day (7am to 7pm)		Night (7pm to 7am)		Week (Monday to Thursday)		Weekend (Friday to Sunday)	
	Raw Number	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent
4092	57	39	68.4	18	31.6	38	66.7	19	33.3
4093	263	135	51.3	128	48.7	167	63.5	96	36.5
4094	286	140	49.0	146	51.0	205	71.7	81	28.3
4095	336	184	54.8	152	45.2	199	59.2	137	40.8
4096	1,523	768	50.4	755	49.6	964	63.3	559	36.7
4097	531	338	63.7	193	36.3	356	67.0	175	33.0
4098	176	118	67.0	58	33.0	112	63.6	64	36.4
4099	28	19	67.9	9	32.1	20	71.4	8	28.6
4100	8	4	50	4	50	5	62.5	3	37.5
4101	212	76	35.8	136	64.2	100	47.2	112	52.8
4102	167	93	55.7	74	44.3	106	63.5	61	36.5
4103	554	249	44.9	305	55.1	347	62.6	207	37.4
4104	225	132	58.7	93	41.3	163	72.4	62	27.6
4105	387	205	53.0	182	47.0	223	57.6	164	42.4
9819	433	80	18.5	353	81.5	183	42.3	250	57.7
9820	124	47	37.9	77	62.1	69	55.6	55	44.4
9832	192	110	57.3	82	42.7	89	46.4	103	53.6

Appendix E: Descriptive Statistics by Census Tract: Arrests, Handcuffing

Census Tract	All Stops	Any Arrest		Felony Arrest		Misdemeanor Arrest		Any Handcuffing		Handcuffing excluding Arrests	
	Raw Number	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent
4001	9	0	0	0	0	0	0	0	0	0	0
4002	79	1	1.3	0	0	1	1.3	2	2.5	0	0
4003	321	9	2.8	5	1.6	4	1.2	19	5.9	7	2.2
4004	129	3	2.3	2	1.6	1	0.8	5	3.9	2	1.6
4005	72	2	2.8	1	1.4	1	1.4	4	5.6	3	4.2
4006	57	6	10.5	4	7.0	2	3.5	8	14.0	4	7.0
4007	180	22	12.2	20	11.1	2	1.1	45	25	27	15
4008	67	17	25.4	6	9.0	11	16.4	27	40.3	18	26.9
4009	52	13	25	6	11.5	7	13.5	19	36.5	4	7.7
4010	449	70	15.6	65	14.5	5	1.1	124	27.6	62	13.8
4011	832	53	6.4	34	4.1	19	2.3	87	10.5	41	4.9
4012	225	20	8.9	18	8	2	0.9	37	16.4	18	8
4013	812	133	16.4	108	13.3	25	3.1	227	28.0	119	14.7
4014	733	172	23.5	134	18.3	38	5.2	318	43.4	191	26.1
4015	355	90	25.4	64	18.0	26	7.3	180	50.7	96	27.0
4016	391	89	22.8	70	17.9	19	4.9	157	40.2	87	22.3
4017	265	27	10.2	21	7.9	6	2.3	48	18.1	26	9.8
4018	170	26	15.3	20	11.8	6	3.5	59	34.7	30	17.6
4022	351	36	10.3	26	7.4	10	2.8	72	20.5	43	12.3
4024	100	23	23	14	14	9	9	39	39	21	21
4025	213	26	12.2	21	9.9	5	2.3	47	22.1	30	14.1
4026	161	12	7.5	10	6.2	2	1.2	19	11.8	5	3.1
4027	282	38	13.5	27	9.6	11	3.9	61	21.6	28	9.9
4028	625	67	10.7	49	7.8	18	2.9	120	19.2	53	8.5

Appendix E: Descriptive Statistics by Census Tract: Arrests, Handcuffing

Census Tract	All Stops	Any Arrest		Felony Arrest		Misdemeanor Arrest		Any Handcuffing		Handcuffing excluding Arrests	
	Raw Number	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent
4029	988	42	4.3	26	2.6	16	1.6	94	9.5	45	4.6
4030	735	36	4.9	18	2.4	18	2.4	70	9.5	31	4.2
4031	327	52	15.9	32	9.8	20	6.1	73	22.3	25	7.6
4033	96	6	6.2	5	5.2	1	1.0	18	18.8	11	11.5
4034	212	14	6.6	9	4.2	5	2.4	33	15.6	15	7.1
4035.01	222	19	8.6	9	4.1	10	4.5	35	15.8	11	5.0
4035.02	22	3	13.6	2	9.1	1	4.5	7	31.8	4	18.2
4036	30	1	3.3	1	3.3	0	0	5	16.7	4	13.3
4037.01	124	6	4.8	2	1.6	4	3.2	12	9.7	5	4.0
4037.02	200	17	8.5	12	6	5	2.5	30	15	17	8.5
4038	70	5	7.1	2	2.9	3	4.3	8	11.4	2	2.9
4039	25	2	8	0	0	2	8	2	8	1	4
4040	109	7	6.4	4	3.7	3	2.8	14	12.8	7	6.4
4041.01	125	17	13.6	12	9.6	5	4	29	23.2	14	11.2
4041.02	57	5	8.8	1	1.8	4	7.0	9	15.8	6	10.5
4042	39	0	0	0	0	0	0	0	0	0	0
4043	18	1	5.6	0	0	1	5.6	3	16.7	2	11.1
4044	25	0	0	0	0	0	0	0	0	0	0
4045.01	2	0	0	0	0	0	0	0	0	0	0
4045.02	41	2	4.9	1	2.4	1	2.4	5	12.2	3	7.3
4046	11	3	27.3	2	18.2	1	9.1	4	36.4	3	27.3
4047	10	0	0	0	0	0	0	2	20	2	20
4048	30	4	13.3	1	3.3	3	10	6	20	2	6.7
4049	58	3	5.2	2	3.4	1	1.7	9	15.5	6	10.3

Appendix E: Descriptive Statistics by Census Tract: Arrests, Handcuffing

Census Tract	All Stops	Any Arrest		Felony Arrest		Misdemeanor Arrest		Any Handcuffing		Handcuffing excluding Arrests	
	Raw Number	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent
4050	67	5	7.5	5	7.5	0	0	9	13.4	3	4.5
4051	41	1	2.4	1	2.4	0	0	3	7.3	3	7.3
4052	79	2	2.5	0	0	2	2.5	7	8.9	3	3.8
4053.01	92	6	6.5	3	3.3	3	3.3	11	12.0	5	5.4
4053.02	85	5	5.9	4	4.7	1	1.2	12	14.1	7	8.2
4054.01	308	57	18.5	37	12.0	20	6.5	106	34.4	60	19.5
4054.02	189	10	5.3	5	2.6	5	2.6	21	11.1	13	6.9
4055	76	6	7.9	5	6.6	1	1.3	9	11.8	3	3.9
4056	29	5	17.2	2	6.9	3	10.3	10	34.5	5	17.2
4057	61	9	14.8	6	9.8	3	4.9	17	27.9	10	16.4
4058	73	12	16.4	9	12.3	3	4.1	26	35.6	16	21.9
4059.01	694	113	16.3	51	7.3	62	8.9	203	29.3	106	15.3
4059.02	616	99	16.1	26	4.2	73	11.9	176	28.6	82	13.3
4060	369	63	17.1	24	6.5	39	10.6	108	29.3	51	13.8
4061	495	70	14.1	23	4.6	47	9.5	114	23.0	49	9.9
4062.01	437	115	26.3	51	11.7	64	14.6	163	37.3	79	18.1
4062.02	374	79	21.1	15	4.0	64	17.1	108	28.9	35	9.4
4063	51	18	35.3	7	13.7	11	21.6	29	56.9	14	27.5
4064	33	9	27.3	7	21.2	2	6.1	14	42.4	9	27.3
4065	189	37	19.6	26	13.8	11	5.8	60	31.7	27	14.3
4066.01	76	14	18.4	10	13.2	4	5.3	23	30.3	14	18.4
4066.02	84	10	11.9	9	10.7	1	1.2	19	22.6	15	17.9
4067	56	2	3.6	2	3.6	0	0	8	14.3	4	7.1
4068	65	5	7.7	4	6.2	1	1.5	15	23.1	9	13.8

Appendix E: Descriptive Statistics by Census Tract: Arrests, Handcuffing

Census Tract	All Stops	Any Arrest		Felony Arrest		Misdemeanor Arrest		Any Handcuffing		Handcuffing excluding Arrests	
	Raw Number	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent
4069	67	4	6.0	3	4.5	1	1.5	17	25.4	12	17.9
4070	158	25	15.8	16	10.1	9	5.7	39	24.7	20	12.7
4071.01	101	10	9.9	4	4.0	6	5.9	30	29.7	21	20.8
4071.02	141	21	14.9	14	9.9	7	5.0	35	24.8	20	14.2
4072	977	116	11.9	44	4.5	72	7.4	183	18.7	79	8.1
4073	373	72	19.3	41	11.0	31	8.3	119	31.9	51	13.7
4074	625	114	18.2	59	9.4	55	8.8	175	28	73	11.7
4075	395	71	18.0	54	13.7	17	4.3	136	34.4	85	21.5
4076	288	50	17.4	36	12.5	14	4.9	104	36.1	60	20.8
4077	157	17	10.8	13	8.3	4	2.5	53	33.8	38	24.2
4078	82	12	14.6	7	8.5	5	6.1	22	26.8	15	18.3
4079	55	9	16.4	5	9.1	4	7.3	15	27.3	5	9.1
4080	8	1	12.5	1	12.5	0	0	4	50	3	37.5
4081	34	3	8.8	2	5.9	1	2.9	3	8.8	1	2.9
4082	126	22	17.5	19	15.1	3	2.4	63	50	39	31.0
4083	283	36	12.7	25	8.8	11	3.9	78	27.6	52	18.4
4084	307	52	16.9	44	14.3	8	2.6	126	41.0	78	25.4
4085	662	122	18.4	96	14.5	26	3.9	262	39.6	174	26.3
4086	550	94	17.1	65	11.8	29	5.3	189	34.4	112	20.4
4087	628	85	13.5	68	10.8	17	2.7	203	32.3	134	21.3
4088	368	79	21.5	64	17.4	15	4.1	140	38.0	84	22.8
4089	226	33	14.6	26	11.5	7	3.1	89	39.4	65	28.8
4090	422	75	17.8	43	10.2	32	7.6	146	34.6	86	20.4
4091	87	32	36.8	29	33.3	3	3.4	51	58.6	26	29.9

Appendix E: Descriptive Statistics by Census Tract: Arrests, Handcuffing

Census Tract	All Stops	Any Arrest		Felony Arrest		Misdemeanor Arrest		Any Handcuffing		Handcuffing excluding Arrests	
	Raw Number	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent
4092	57	20	35.1	16	28.1	4	7.0	30	52.6	15	26.3
4093	263	61	23.2	46	17.5	15	5.7	120	45.6	76	28.9
4094	286	48	16.8	35	12.2	13	4.5	121	42.3	84	29.4
4095	336	74	22.0	59	17.6	15	4.5	146	43.5	89	26.5
4096	1,523	339	22.3	280	18.4	59	3.9	639	42.0	395	25.9
4097	531	89	16.8	65	12.2	24	4.5	213	40.1	145	27.3
4098	176	32	18.2	28	15.9	4	2.3	54	30.7	27	15.3
4099	28	6	21.4	5	17.9	1	3.6	11	39.3	8	28.6
4100	8	0	0	0	0	0	0	1	12.5	1	12.5
4101	212	43	20.3	30	14.2	13	6.1	69	32.5	39	18.4
4102	167	40	24.0	31	18.6	9	5.4	67	40.1	41	24.6
4103	554	82	14.8	64	11.6	18	3.2	188	33.9	119	21.5
4104	225	41	18.2	35	15.6	6	2.7	84	37.3	56	24.9
4105	387	30	7.8	23	5.9	7	1.8	75	19.4	47	12.1
9819	433	8	1.8	1	0.2	7	1.6	12	2.8	4	0.9
9820	124	6	4.8	3	2.4	3	2.4	14	11.3	6	4.8
9832	192	15	7.8	6	3.1	9	4.7	32	16.7	17	8.9

Appendix E: Descriptive Statistics by Census Tract: Searches, Recoveries

Census Tract	All Stops	All Searches		High-Discretion Searches		Recoveries made during all searches		Recoveries made during high-discretion searches	
	Raw Number	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent
4001	9	1	11.1	1	11.1	0	0	0	0
4002	79	2	2.5	0	0	1	50	0	
4003	321	25	7.8	13	4.0	8	32	4	30.8
4004	129	8	6.2	3	2.3	2	25	1	33.3
4005	72	9	12.5	4	5.6	0	0	0	0
4006	57	10	17.5	2	3.5	3	30	2	100
4007	180	51	28.3	19	10.6	22	43.1	9	47.4
4008	67	28	41.8	12	17.9	8	28.6	6	50
4009	52	20	38.5	4	7.7	10	50	3	75
4010	449	135	30.1	31	6.9	58	43.0	11	35.5
4011	832	103	12.4	27	3.2	30	29.1	6	22.2
4012	225	40	17.8	9	4	16	40	2	22.2
4013	812	267	32.9	62	7.6	106	39.7	21	33.9
4014	733	342	46.7	67	9.1	95	27.8	21	31.3
4015	355	203	57.2	31	8.7	61	30.0	5	16.1
4016	391	183	46.8	40	10.2	53	29.0	16	40
4017	265	68	25.7	17	6.4	28	41.2	6	35.3
4018	170	56	32.9	14	8.2	14	25	2	14.3
4022	351	105	29.9	36	10.3	32	30.5	14	38.9
4024	100	45	45	17	17	12	26.7	4	23.5
4025	213	62	29.1	24	11.3	13	21.0	7	29.2
4026	161	17	10.6	3	1.9	4	23.5	0	0
4027	282	73	25.9	18	6.4	20	27.4	6	33.3

Appendix E: Descriptive Statistics by Census Tract: Searches, Recoveries

Census Tract	All Stops	All Searches		High-Discretion Searches		Recoveries made during all searches		Recoveries made during high-discretion searches	
	Raw Number	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent
4028	625	140	22.4	35	5.6	32	22.9	7	20
4029	988	95	9.6	28	2.8	21	22.1	8	28.6
4030	735	89	12.1	25	3.4	13	14.6	1	4
4031	327	83	25.4	25	7.6	18	21.7	4	16
4033	96	23	24.0	11	11.5	4	17.4	2	18.2
4034	212	39	18.4	10	4.7	8	20.5	4	40
4035.01	222	31	14.0	10	4.5	12	38.7	4	40
4035.02	22	7	31.8	1	4.5	2	28.6	0	0
4036	30	6	20	3	10	3	50	2	66.7
4037.01	124	15	12.1	3	2.4	4	26.7	1	33.3
4037.02	200	40	20	13	6.5	12	30	1	7.7
4038	70	7	10	2	2.9	1	14.3	0	0
4039	25	3	12	2	8	0	0	0	0
4040	109	22	20.2	12	11.0	7	31.8	3	25
4041.01	125	30	24	7	5.6	12	40	2	28.6
4041.02	57	8	14.0	3	5.3	2	25	0	0
4042	39	1	2.6	0	0	0	0	0	
4043	18	4	22.2	2	11.1	0	0	0	0
4044	25	0	0	0	0	0		0	
4045.01	2	0	0	0	0	0		0	
4045.02	41	7	17.1	3	7.3	0	0	0	0
4046	11	4	36.4	3	27.3	1	25	1	33.3
4047	10	2	20	0	0	0	0	0	

Appendix E: Descriptive Statistics by Census Tract: Searches, Recoveries

Census Tract	All Stops	All Searches		High-Discretion Searches		Recoveries made during all searches		Recoveries made during high-discretion searches	
	Raw Number	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent
4048	30	7	23.3	3	10	0	0	0	0
4049	58	12	20.7	9	15.5	1	8.3	1	11.1
4050	67	13	19.4	6	9.0	2	15.4	1	16.7
4051	41	3	7.3	2	4.9	0	0	0	0
4052	79	10	12.7	7	8.9	0	0	0	0
4053.01	92	14	15.2	6	6.5	4	28.6	0	0
4053.02	85	15	17.6	6	7.1	5	33.3	0	0
4054.01	308	121	39.3	51	16.6	36	29.8	12	23.5
4054.02	189	28	14.8	13	6.9	9	32.1	0	0
4055	76	8	10.5	0	0	0	0	0	
4056	29	9	31.0	3	10.3	1	11.1	0	0
4057	61	21	34.4	8	13.1	3	14.3	1	12.5
4058	73	32	43.8	17	23.3	7	21.9	5	29.4
4059.01	694	234	33.7	84	12.1	65	27.8	14	16.7
4059.02	616	186	30.2	55	8.9	32	17.2	12	21.8
4060	369	119	32.2	35	9.5	21	17.6	4	11.4
4061	495	132	26.7	26	5.3	32	24.2	7	26.9
4062.01	437	208	47.6	83	19.0	54	26.0	17	20.5
4062.02	374	115	30.7	25	6.7	15	13.0	3	12
4063	51	30	58.8	11	21.6	19	63.3	7	63.6
4064	33	14	42.4	8	24.2	8	57.1	3	37.5
4065	189	71	37.6	27	14.3	15	21.1	3	11.1
4066.01	76	26	34.2	12	15.8	6	23.1	4	33.3

Appendix E: Descriptive Statistics by Census Tract: Searches, Recoveries

Census Tract	All Stops	All Searches		High-Discretion Searches		Recoveries made during all searches		Recoveries made during high-discretion searches	
	Raw Number	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent
4066.02	84	28	33.3	16	19.0	3	10.7	2	12.5
4067	56	8	14.3	4	7.1	0	0	0	0
4068	65	15	23.1	2	3.1	3	20	0	0
4069	67	18	26.9	7	10.4	2	11.1	0	0
4070	158	42	26.6	11	7.0	10	23.8	0	0
4071.01	101	32	31.7	14	13.9	7	21.9	5	35.7
4071.02	141	37	26.2	12	8.5	10	27.0	4	33.3
4072	977	235	24.1	82	8.4	72	30.6	28	34.1
4073	373	137	36.7	30	8.0	31	22.6	6	20
4074	625	195	31.2	54	8.6	57	29.2	9	16.7
4075	395	148	37.5	45	11.4	46	31.1	13	28.9
4076	288	117	40.6	53	18.4	26	22.2	12	22.6
4077	157	58	36.9	25	15.9	12	20.7	4	16
4078	82	26	31.7	9	11.0	9	34.6	5	55.6
4079	55	16	29.1	4	7.3	2	12.5	0	0
4080	8	4	50	3	37.5	1	25	0	0
4081	34	5	14.7	3	8.8	1	20	1	33.3
4082	126	66	52.4	32	25.4	16	24.2	8	25
4083	283	90	31.8	37	13.1	21	23.3	9	24.3
4084	307	156	50.8	64	20.8	34	21.8	18	28.1
4085	662	300	45.3	134	20.2	93	31	38	28.4
4086	550	232	42.2	95	17.3	63	27.2	25	26.3
4087	628	238	37.9	89	14.2	61	25.6	23	25.8

Appendix E: Descriptive Statistics by Census Tract: Searches, Recoveries

Census Tract	All Stops	All Searches		High-Discretion Searches		Recoveries made during all searches		Recoveries made during high-discretion searches	
	Raw Number	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent
4088	368	166	45.1	44	12.0	50	30.1	12	27.3
4089	226	102	45.1	36	15.9	40	39.2	19	52.8
4090	422	175	41.5	62	14.7	43	24.6	10	16.1
4091	87	56	64.4	13	14.9	22	39.3	3	23.1
4092	57	41	71.9	20	35.1	17	41.5	6	30
4093	263	149	56.7	70	26.6	48	32.2	18	25.7
4094	286	138	48.3	60	21.0	34	24.6	8	13.3
4095	336	163	48.5	54	16.1	55	33.7	16	29.6
4096	1,523	744	48.9	229	15.0	259	34.8	63	27.5
4097	531	255	48.0	108	20.3	57	22.4	33	30.6
4098	176	67	38.1	27	15.3	19	28.4	3	11.1
4099	28	11	39.3	4	14.3	3	27.3	1	25
4100	8	1	12.5	1	12.5	0	0	0	0
4101	212	81	38.2	26	12.3	23	28.4	7	26.9
4102	167	89	53.3	32	19.2	32	36.0	12	37.5
4103	554	217	39.2	80	14.4	55	25.3	20	25
4104	225	93	41.3	34	15.1	33	35.5	13	38.2
4105	387	88	22.7	31	8.0	25	28.4	6	19.4
9819	433	19	4.4	6	1.4	3	15.8	1	16.7
9820	124	11	8.9	3	2.4	2	18.2	1	33.3
9832	192	35	18.2	8	4.2	6	17.1	0	0

Appendix F: Descriptive Statistics at the Level of Area

Descriptive Statistics by Area: Stops by Type of Stop

Area	Number of Stops	Vehicle		Bicycle		Other		Pedestrian	
		Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent
1	7,456	4,992	67.0%	357	4.8%	110	1.5%	1,997	26.8%
2	4,119	2,955	71.7%	218	5.3%	47	1.1%	899	21.8%
3	4,563	3,222	70.6%	106	2.3%	132	2.9%	1,103	24.2%
4	4,834	3,636	75.2%	133	2.8%	74	1.5%	991	20.5%
5	7,147	4,663	65.2%	267	3.7%	212	3.0%	2,005	28.1%

Descriptive Statistics by Area: Stops by Reason for Encounter

Area	Number of Stops	Traffic Violation		Consensual Encounter		Probable Cause		Probation/Parole		Reasonable Suspicion	
		Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent
1	7,456	5,053	67.8%	295	4.0%	1,470	19.7%	119	1.6%	519	7.0%
2	4,119	2,890	70.2%	113	2.7%	819	19.9%	45	1.1%	252	6.1%
3	4,563	2,736	60.0%	228	5.0%	1,065	23.3%	64	1.4%	470	10.3%
4	4,834	3,326	68.8%	182	3.8%	852	17.6%	69	1.4%	405	8.4%
5	7,147	4,095	57.3%	396	5.5%	1,648	23.1%	201	2.8%	807	11.3%

Descriptive Statistics by Area: Stops by Time and Day of the Week

		Day (7am to 7pm)		Night (7pm to 7am)		Week (Monday to Thursday)		Weekend (Friday to Sunday)	
Area	Number of Stops	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent
1	7,456	3,732	50.1%	3,724	49.9%	4,285	57.5%	3,171	42.5%
2	4,119	1,996	48.5%	2,123	51.5%	2,598	63.1%	1,521	36.9%
3	4,563	2,349	51.5%	2,214	48.5%	2,850	62.5%	1,713	37.5%
4	4,834	2,919	60.4%	1,915	39.6%	2,984	61.7%	1,850	38.3%
5	7,147	4,107	57.5%	3,040	42.5%	4,561	63.8%	2,586	36.2%

Descriptive Statistics by Area: Stops by Race

		White		African American		Hispanic		Asian		Other	
Area	Number of Stops	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent
1	7,456	1,304	17.5%	4,589	61.5%	802	10.8%	506	6.8%	255	3.4%
2	4,119	1,221	29.6%	2,129	51.7%	286	6.9%	322	7.8%	161	3.9%
3	4,563	504	11.0%	2,297	50.3%	891	19.5%	694	15.2%	177	3.9%
4	4,834	363	7.5%	2,609	54.0%	1,517	31.4%	201	4.2%	144	3.0%
5	7,147	269	3.8%	5,194	72.7%	1,437	20.1%	104	1.5%	143	2.0%

Descriptive Statistics by Area: Stops by Age Group

		18 to 29 years		Under 17 years		30 to 39 years		Over 40 years	
Area	Number of Stops	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent	Raw Number	Percent
1	7,456	2,793	37.5%	131	1.8%	1,669	22.4	2,863	38.4
2	4,119	1,293	31.4%	73	1.8%	1,024	24.9	1,729	42.0
3	4,563	2,135	46.8%	130	2.8%	978	21.4	1,320	28.9
4	4,834	2,346	48.5%	178	3.7	1,031	21.3	1,279	26.5
5	7,147	3,337	46.7%	289	4.0	1,527	21.4	1,994	27.9

Descriptive Statistics by Area: Stops by Gender

		Male		Female	
Area	Number of Stops	Raw Number	Percent	Raw Number	Percent
1	7,456	5,659	75.9%	1,796	24.1%
2	4,119	2,855	69.3%	1,263	30.7%
3	4,563	3,244	71.1%	1,315	28.8%
4	4,834	3,662	75.8%	1,172	24.2%
5	7,147	5,622	78.7%	1,523	21.3%

Descriptive Statistics by Area: Handcuffed by Race

	Handcuffed		African American		White		Hispanic		Asian		Other	
Area	Raw Number	Percent[159]	Raw Number	Percent[160]	Raw Number	Percent[161]	Raw Number	Percent[162]	Raw Number	Percent[163]	Raw Number	Percent[164]
1	1,683	22.6%	1,439	31.4%	132	10.1%	71	8.9%	25	4.9%	16	6.3%
2	631	15.3%	496	23.3%	81	6.6%	31	10.8%	12	3.7%	11	6.8%
3	1,245	27.3%	743	32.3%	102	20.2%	198	22.2%	170	24.5%	32	18.1%
4	1,390	28.8%	908	34.8%	79	21.8%	339	22.3%	29	14.4%	35	24.3%
5	2,786	39.0%	2,238	43.1%	65	24.2%	423	29.4%	22	21.2%	38	26.6%

Note: This table includes all handcuffing, including handcuffing during stops that resulted in an arrest. See the following table, *Descriptive Statistics by Area: Handcuffed Excluding Stops That Resulted in Arrest by Race* for handcuffing that excludes stops that resulted in an arrest.

[159] Percent of stops in area that resulted in a person being handcuffed.
[160] Percent of African American stops in area that resulted in an African American person being handcuffed.
[161] Percent of White stops in area that resulted in a White person being handcuffed.
[162] Percent of Hispanic stops in area that resulted in a Hispanic person being handcuffed.
[163] Percent of Asian stops in area that resulted in an Asian person being handcuffed.
[164] Percent of Other stops in area that resulted in an Other person being handcuffed.

Descriptive Statistics by Area: Handcuffed Excluding Stops that Resulted in Arrest[165] by Race

	Handcuffed		African American		White		Hispanic		Asian		Other	
Area	Number	Percent[166]	Number	Percent[167]	Number	Percent[168]	Number	Percent[169]	Number	Percent[170]	Number	Percent[171]
1	816	25.0%	696	17.5%	63	5.1%	36	4.7%	15	3.0	6	2.4
2	302	16.4%	236	12.3%	37	3.1%	15	5.5%	7	2.2	7	4.5
3	533	31.2%	319	16.4%	40	8.8%	89	11.1%	72	11.6	13	8.1
4	647	32.9%	445	19.9%	24	7.6%	155	11.4%	11	5.9	12	9.8
5	1,488	45.2%	1,194	27.1%	29	12.1%	236	18.2%	7	7.7	22	17.1

[165] Handcuffing excludes handcuffing during stops that resulted in an arrest.
[166] Percent of stops in area that resulted in the person being handcuffed, excluding stops that resulted in an arrest.
[167] Percent of African American stops in area that resulted in an African American person being handcuffed, excluding stops that resulted in an arrest.
[168] Percent of White stops in area that resulted in a White person being handcuffed, excluding stops that resulted in an arrest.
[169] Percent of Hispanic stops in area that resulted in a Hispanic person being handcuffed, excluding stops that resulted in an arrest.
[170] Percent of Asian stops in area that resulted in an Asian person being handcuffed, excluding stops that resulted in an arrest.
[171] Percent of Other stops in area that resulted in an Other person being handcuffed, excluding stops that resulted in an arrest.

Descriptive Statistics by Area: All Searches by Race

	All Searches		African American		White		Hispanic		Asian		Other	
Area	Number	Percent[172]	Number	Percent[173]	Number	Percent[174]	Number	Percent[175]	Number	Percent[176]	Number	Percent[177]
1	1,942	26.0%	1,664	36.3%	132	10.1%	93	11.6%	33	6.5%	20	7.8%
2	727	17.6%	580	27.2%	86	7.0%	38	13.3%	11	3.4%	12	7.5%
3	1,441	31.6%	860	37.4%	116	23.0%	241	27.0%	191	27.5%	33	18.6%
4	1,598	33.1%	1,030	39.5%	93	25.6%	413	27.2%	26	12.9%	36	25%
5	3,267	45.7%	2,625	50.5%	76	28.3%	499	34.7%	24	23.1%	43	30.1%

Note: This table includes all searches. For high-discretion searches, see the following table, *Descriptive Statistics by Area: High-Discretion Searches by Race.*

[172] Percent of all stops in area that result in a search.
[173] Percent of African American stops in area that result in a search.
[174] Percent of White stops in area that result in a search.
[175] Percent of Hispanic stops in area that result in a search.
[176] Percent of Asian stops in area that result in a search.
[177] Percent of Other stops in area that result in a search.

Descriptive Statistics by Area: High-Discretion Searches[178] by Race

	All Searches		White		African American		Hispanic		Asian		Other	
Area	Number	Percent[179]	Number	Percent[180]	Number	Percent[181]	Number	Percent[182]	Number	Percent[183]	Number	Percent[184]
1	480	8.0%	32	2.7%	398	12.0%	33	4.4%	13	2.7%	4	1.7%
2	222	6.2%	28	2.4%	168	9.8%	15	5.7%	6	1.9%	5	3.2%
3	523	14.4%	38	8.9%	293	17.0%	97	13.1%	86	14.6%	9	5.9%
4	503	13.5%	27	9.1%	312	16.5%	151	12.1%	4	2.2%	9	7.7%
5	1,251	24.5%	29	13.2%	961	27.4%	232	19.9%	7	8.0%	22	18.2%

[178] High-discretion searches exclude any stops based on Probation/Parole (498) and any stops that result in an Incident to Arrest Search (3,090), a Probation/Parole search (2,363), or an Inventory search (120).

[179] Percent of all stops fitting above exclusion criteria that resulted in a high-discretion search.

[180] Percent of all stops of a White person fitting high-discretion exclusion criteria that resulted in a high-discretion search.

[181] Percent of all stops of an African American person fitting high-discretion exclusion criteria that resulted in a high-discretion search.

[182] Percent of all stops of a Hispanic person fitting high-discretion exclusion criteria that resulted in a high-discretion search.

[183] Percent of all stops of an Asian person fitting high-discretion exclusion criteria that resulted in a high-discretion search.

[184] Percent of all stops of an Other person fitting high-discretion exclusion criteria that resulted in a high-discretion search.

Descriptive Statistics by Area: Recoveries during All Searches by Race

	All Recoveries		African American		White		Hispanic		Asian		Other	
Area	Number	Percent[185]	Number	Percent[186]	Number	Percent[187]	Number	Percent[188]	Number	Percent[189]	Number	Percent[190]
1	533	27.4%	452	27.2%	44	33.3%	27	29.0%	4	12.1%	6	30%
2	251	34.5%	203	35%	30	34.9%	13	34.2%	2	18.2%	3	25%
3	327	22.7%	182	21.2%	22	19.0%	58	24.1%	55	28.8%	10	30.3%
4	435	27.2%	305	29.6%	19	20.4%	91	22.0%	9	34.6%	11	30.6%
5	977	29.9%	784	29.9%	20	26.3%	151	30.3%	7	29.2%	15	34.9%

[185] Percent of any searches in area that led to a recovery.
[186] Percent of any searches of an African American person stopped in area that led to a recovery.
[187] Percent of any searches of a White person stopped in area that led to a recovery.
[188] Percent of any searches of a Hispanic person stopped in area that led to a recovery.
[189] Percent of any searches of an Asian person stopped in area that led to a recovery.
[190] Percent of any searches of an Other person stopped in area that led to a recovery.

Descriptive Statistics by Area: Recoveries during High-Discretion Searches[191] by Race

	All Recoveries		African American		White		Hispanic		Asian		Other	
Area	Number	Percent[192]	Number	Percent[193]	Number	Percent[194]	Number	Percent[195]	Number	Percent[196]	Number	Percent[197]
1	119	24.8%	93	23.4%	9	28.1%	12	36.4%	3	23.1%	2	50%
2	71	32.0%	56	33.3%	8	28.6%	4	26.7%	2	33.3%	1	20%
3	99	18.9%	49	16.7%	4	10.5%	23	23.7%	21	24.4%	2	22.2%
4	131	26.0%	87	27.9%	3	11.1%	38	25.2%	0	0%	3	33.3%
5	334	26.7%	253	26.3%	11	37.9%	61	26.3%	1	14.3%	8	36.4%

[191] Recoveries made during high-discretion searches exclude stops for probation/parole, probation/parole searches, inventory searches, and incident to arrest searches.
[192] Percent of recoveries made during high-discretion searches.
[193] Percent of recoveries made during high-discretion searches of an African American person.
[194] Percent of recoveries made during high-discretion searches of a White person.
[195] Percent of recoveries made during high-discretion searches of a Hispanic person.
[196] Percent of recoveries made during high-discretion searches of an Asian person.
[197] Percent of recoveries made during high-discretion searches of an Other person.

Descriptive Statistics by Area: Arrests by Race

	Any Arrest		White		African American		Hispanic		Asian		Other	
Area	Number	Percent[198]	Number	Percent[199]	Number	Percent[200]	Number	Percent[201]	Number	Percent[202]	Number	Percent[203]
1	908	12.2%	71	5.4%	776	16.9%	40	5.0%	10	2.0%	11	4.3%
2	343	8.3%	47	3.8%	270	12.7%	16	5.6%	6	1.9%	4	2.5%
3	730	16.0%	65	12.9%	431	18.8%	114	12.8%	100	14.4%	20	11.3%
4	770	15.9%	56	15.4%	482	18.5%	190	12.5%	19	9.5%	23	16.0%
5	1,348	18.9%	39	14.5%	1,081	20.8%	196	13.6%	15	14.4%	17	11.9%

[198] Percent of all stops in area that resulted in any arrest
[199] Percent of White stops in area that resulted in any arrest
[200] Percent of African American stops in area that resulted in any arrest
[201] Percent of Hispanic stops in area that resulted in any arrest
[202] Percent of Asian stops in area that resulted in any arrest
[203] Percent of Other stops in area that resulted in any arrest

Descriptive Statistics by Area: Misdemeanor Arrests by Race

	All Misdemeanor Arrests		White		African American		Hispanic		Asian		Other	
Area	Number	Percent[204]	Number	Percent[205]	Number	Percent[206]	Number	Percent[207]	Number	Percent[208]	Number	Percent[209]
1	248	3.3%	26	2.0%	192	4.2%	23	2.9%	4	0.8%	3	1.2%
2	108	2.6%	23	1.9%	73	3.4%	6	2.1%	5	1.6%	1	0.6%
3	400	8.8%	47	9.3%	253	11.0%	59	6.6%	29	4.2%	12	6.8%
4	304	6.3%	34	9.4%	157	6.0%	88	5.8%	11	5.5%	14	9.7%
5	291	4.1%	12	4.5%	216	4.2%	51	3.5%	5	4.8%	7	4.9%

[204] Percent of stops in area that resulted in a misdemeanor arrest
[205] Percent of White stops in area that resulted in a misdemeanor arrest
[206] Percent of African American stops in area that resulted in a misdemeanor arrest
[207] Percent of Hispanic stops in area that resulted in a misdemeanor arrest
[208] Percent of Asian stops in area that resulted in a misdemeanor arrest
[209] Percent of Other stops in area that resulted in a misdemeanor arrest

Descriptive Statistics by Area: Felony Arrests by Race

	All Felony Arrests		White		African American		Hispanic		Asian		Other	
Area	Number	Percent[210]	Number	Percent[211]	Number	Percent[212]	Number	Percent[213]	Number	Percent[214]	Number	Percent[215]
1	660	8.9%	45	3.5%	584	12.7%	17	2.1%	6	1.2%	8	3.1%
2	235	5.7%	24	2.0%	197	9.3%	10	3.5%	1	0.3%	3	1.9%
3	330	7.2%	18	3.6%	178	7.7%	55	6.2%	71	10.2%	8	4.5%
4	466	9.6%	22	6.1%	325	12.5%	102	6.7%	8	4.0%	9	6.2%
5	1,057	14.8%	27	10.0%	865	16.7%	145	10.1%	10	9.6%	10	7.0%

[210] Percent of stops in Area that resulted in a felony arrest
[211] Percent of White stops within area that resulted in a felony arrest
[212] Percent of African American stops within area that resulted in a felony arrest
[213] Percent of Hispanic stops within area that resulted in a felony arrest
[214] Percent of Asian stops within area that resulted in a felony arrest
[215] Percent of Other stops within area that resulted in a felony arrest

Appendix G: Descriptive Statistics at the Level of Officer

Stops & Experience

	Mean	Median	Mode
Number of stops	55	35	1
Years of experience	9.4	7.2	

Demographic: Gender & Race

		Number	Percent
Gender	**Male**	456	89.4%
	Female	54	10.6%
Race	**White**	220	43.1%
	African American	89	17.5%
	Hispanic	111	21.8%
	Asian	69	13.5%
	Other	21	4.1%

Notes

As in the likelihood tables in the main text:

1. The “no moderator” analyses include all ethnic groups. The moderator analyses include only Whites, Hispanics, and Blacks.

2. The Arrest tables comprise all observations. The Handcuffing tables do not include arrests. The search analyses do not include incident to arrest, inventory, probation/parole searches and probation/parole stops.

3. The analyses with covariates include all categories of special assignment, but the moderator analyses of special assignment lump the categories into fewer, hence the new variable.

Appendix H: Handcuffing (No Moderator)

	Handcuffing (No Moderator)		
	No Covariates	All Covariates	Fixed Effects
Constant	-2.922*** (0.129)	-3.216*** (0.313)	-16.511 (1,161.825)
SDRace2Afr American	1.422*** (0.136)	0.860*** (0.146)	0.685*** (0.158)
SDRace2Asian	-0.546^{+} (0.292)	-0.194 (0.301)	-0.343 (0.323)
SDRace2Hispanic	-0.082 (0.214)	-0.320 (0.228)	-0.199 (0.240)
SDRace2Other	-0.759^{+} (0.433)	-0.627 (0.446)	-0.648 (0.458)
Area2	-0.504* (0.211)	-0.260 (0.231)	-0.585* (0.298)
Area3	0.622** (0.210)	0.629** (0.238)	0.084 (0.335)
Area4	0.454^{+} (0.249)	0.295 (0.282)	-0.305 (0.399)
Area5	0.986*** (0.237)	0.670* (0.279)	0.091 (0.407)
Sex_RecodedFemale		-1.045*** (0.059)	-1.020*** (0.063)
AgeGroupUnder 17		0.368*** (0.106)	0.388*** (0.115)
AgeGroup30-39		-0.288*** (0.053)	-0.214*** (0.057)
AgeGroupOver 40		-0.743*** (0.051)	-0.632*** (0.056)
ReasonForEncounterConsensual Encounter		0.452*** (0.101)	0.375*** (0.113)
ReasonForEncounterProbable Cause		1.558*** (0.061)	1.696*** (0.074)
ReasonForEncounterProbation/Parole		2.192*** (0.133)	2.053*** (0.148)
ReasonForEncounterReasonable Suspicion		1.492*** (0.072)	1.442*** (0.082)
EncounterTypeBicycle		0.479*** (0.096)	0.423*** (0.104)
EncounterTypeOther		0.539*** (0.130)	0.449** (0.149)
EncounterTypePedestrian		0.265*** (0.061)	0.222** (0.068)
Contact_WeekOrWeekendFri-Sun		0.136** (0.042)	0.056 (0.047)
ContactTimeOfDay7pm-7am		0.156*** (0.043)	0.208*** (0.053)
Hispanic_percent		0.279 (0.243)	
BlackAloneNotHisp_percent		0.545* (0.254)	
TotalPop24YrsorYounger_percent		0.146 (0.470)	
Percent15yrsOrOlder_Divorced		0.469 (0.858)	
TotalUnemployed_percent		0.018 (0.500)	
Owneroccupiedhousingunits_percent		0.702** (0.225)	
Incomein2013belowpovertylevel_percent		1.363*** (0.308)	
RateViolentCrime_tract		-0.001* (0.0003)	

RatePropertyCrime_tract		-0.0001* (0.00005)	
RateNarcoticsCrime_tract		0.002*** (0.0004)	
PopDensity		-0.00000 (0.00001)	
BusinessCount_persqmile		0.00004 (0.00003)	
QOL_per10000		0.0001* (0.0001)	
OfficerAge_Dynamic		-0.006 (0.004)	
OfficerYearsExperience_Dynamic		-0.014* (0.005)	
OfficerGenderFemale		-0.291*** (0.076)	
OfficerRace_RecodedBlack		0.059 (0.062)	
OfficerRace_RecodedHispanic		0.136* (0.054)	
OfficerRace_RecodedAsianFili		-0.104+ (0.060)	
OfficerRace_RecodedUnknowUndecl		0.397*** (0.112)	
SpecialAssignmentType_RecodedViolence Suppression		-0.174** (0.053)	-0.207** (0.065)
SpecialAssignmentType_RecodedProstitution		1.722*** (0.318)	1.959*** (0.346)
SpecialAssignmentType_RecodedNarcotics		1.393*** (0.181)	1.108*** (0.197)
SpecialAssignmentType_RecodedCruising		-0.242 (0.241)	-0.408 (0.277)
SpecialAssignmentType_RecodedSpecial Event		0.059 (0.340)	-0.153 (0.386)
SpecialAssignmentType_RecodedOther		-0.632*** (0.067)	-0.478*** (0.080)
SDRace2Afr American:Area2	0.075 (0.226)	0.122 (0.239)	0.107 (0.259)
SDRace2Asian:Area2	0.184 (0.509)	0.009 (0.518)	0.302 (0.547)
SDRace2Hispanic:Area2	0.674+ (0.380)	0.588 (0.397)	0.234 (0.434)
SDRace2Other:Area2	1.119+ (0.604)	0.842 (0.622)	1.210+ (0.653)
SDRace2Afr American:Area3	-0.701** (0.223)	-0.574* (0.241)	-0.426 (0.263)
SDRace2Asian:Area3	0.865* (0.359)	0.198 (0.376)	0.215 (0.409)
SDRace2Hispanic:Area3	0.337 (0.293)	0.105 (0.315)	-0.032 (0.338)
SDRace2Other:Area3	0.654 (0.547)	0.067 (0.576)	0.003 (0.604)
SDRace2Afr American:Area4	-0.285 (0.258)	0.012 (0.275)	0.084 (0.296)
SDRace2Asian:Area4	0.270 (0.477)	-0.016 (0.505)	0.242 (0.543)
SDRace2Hispanic:Area4	0.527+ (0.314)	0.610+ (0.334)	0.472 (0.355)
SDRace2Other:Area4	1.020+ (0.570)	0.576 (0.603)	0.484 (0.633)
SDRace2Afr American:Area5	-0.380 (0.243)	-0.224 (0.263)	-0.245 (0.281)
SDRace2Asian:Area5	0.021 (0.529)	-0.543 (0.556)	-0.418 (0.587)
SDRace2Hispanic:Area5	0.570+ (0.301)	0.523 (0.324)	0.176 (0.343)

SDRace2Other:Area5	1.142* (0.531)	0.660 (0.559)	0.518 (0.584)
Officers dummy	No	No	Yes
Census dummy	No	No	Yes
Observations	24,020	23,709	23,709
Log Likelihood	-9,806.938	-8,074.546	-7,191.523
Akaike Inf. Crit.	19,663.880	16,277.090	15,693.050
Note:	+ p < .10; * p < .05; ** p < .01; *** p<0.001		

Appendix I: Handcuffing by Type of Encounter

	Handcuffing by Type of Encounter		
	No Covariates	All Covariates	Fixed Effects
Constant	-3.688*** (0.207)	-3.507*** (0.363)	-16.273 (1,190.255)
SDRace2Afr American	1.561*** (0.217)	1.136*** (0.223)	0.895*** (0.232)
SDRace2Hispanic	0.176 (0.311)	-0.294 (0.324)	-0.161 (0.336)
Area2	-1.120** (0.411)	-0.929* (0.421)	-1.293** (0.467)
Area3	0.776* (0.318)	0.655+ (0.337)	0.092 (0.425)
Area4	0.465 (0.398)	0.160 (0.419)	-0.585 (0.522)
Area5	1.159** (0.355)	0.697+ (0.384)	-0.111 (0.497)
Sex_RecodedFemale		-1.096*** (0.061)	-1.063*** (0.066)
AgeGroupUnder 17		0.371*** (0.107)	0.377** (0.117)
AgeGroup30-39		-0.295*** (0.055)	-0.221*** (0.059)
AgeGroupOver 40		-0.763*** (0.052)	-0.653*** (0.058)
ReasonForEncounterConsensual Encounter		0.459*** (0.104)	0.359** (0.117)
ReasonForEncounterProbable Cause		1.547*** (0.064)	1.653*** (0.076)
ReasonForEncounterProbation/Parole		2.186*** (0.136)	2.072*** (0.153)
ReasonForEncounterReasonable Suspicion		1.499*** (0.074)	1.455*** (0.085)
EncounterTypeBicycle	1.426** (0.513)	0.481 (0.562)	0.247 (0.564)
EncounterTypeOther	2.877*** (0.635)	2.059** (0.761)	1.957* (0.832)
EncounterTypePedestrian	2.052*** (0.287)	1.039*** (0.305)	0.926** (0.333)
Contact_WeekOrWeekendFri-Sun		0.119** (0.043)	0.026 (0.048)
ContactTimeOfDay7pm-7am		0.184*** (0.044)	0.230*** (0.054)
Hispanic_percent		0.428+ (0.254)	
BlackAloneNotHisp_percent		0.600* (0.267)	
TotalPop24YrsorYounger_percent		0.383 (0.484)	
Percent15yrsOrOlder_Divorced		0.661 (0.889)	
TotalUnemployed_percent		-0.141 (0.514)	
Owneroccupiedhousingunits_percent		0.577* (0.240)	
Incomein2013belowpovertylevel_percent		1.273*** (0.319)	
RateViolentCrime_tract		-0.001** (0.0004)	
RatePropertyCrime_tract		-0.0001+ (0.00005)	
RateNarcoticsCrime_tract		0.002*** (0.0004)	

	Handcuffing by Type of Encounter		
	No Covariates	All Covariates	Fixed Effects
PopDensity		-0.00000 (0.00001)	
BusinessCount_persqmile		0.00004 (0.00003)	
QOL_per10000		0.0002* (0.0001)	
OfficerAge_Dynamic		-0.007 (0.004)	
OfficerYearsExperience_Dynamic		-0.014* (0.006)	
OfficerGenderFemale		-0.275*** (0.079)	
OfficerRace_RecodedBlack		0.072 (0.063)	
OfficerRace_RecodedHispanic		0.152** (0.055)	
OfficerRace_RecodedAsianFili		-0.108^{+} (0.062)	
OfficerRace_RecodedUnknowUndecl		0.352** (0.117)	
SpecialAssignmentType_RecodedViolence Suppression		-0.143** (0.054)	-0.173** (0.066)
SpecialAssignmentType_RecodedProstitution		1.880*** (0.333)	2.157*** (0.369)
SpecialAssignmentType_RecodedNarcotics		1.274*** (0.187)	1.067*** (0.201)
SpecialAssignmentType_RecodedCruising		-0.194 (0.247)	-0.343 (0.282)
SpecialAssignmentType_RecodedSpecial Event		-0.110 (0.366)	-0.302 (0.413)
SpecialAssignmentType_RecodedOther		-0.625*** (0.071)	-0.460*** (0.084)
SDRace2Afr American:Area2	0.872* (0.429)	0.917* (0.436)	0.915* (0.452)
SDRace2Hispanic:Area2	0.848 (0.654)	0.922 (0.664)	0.723 (0.693)
SDRace2Afr American:Area3	-0.457 (0.334)	-0.444 (0.346)	-0.292 (0.364)
SDRace2Hispanic:Area3	0.037 (0.428)	0.039 (0.445)	-0.132 (0.467)
SDRace2Afr American:Area4	0.031 (0.409)	0.222 (0.419)	0.361 (0.440)
SDRace2Hispanic:Area4	0.627 (0.473)	0.882^{+} (0.489)	0.774 (0.511)
SDRace2Afr American:Area5	-0.223 (0.363)	-0.066 (0.377)	0.013 (0.393)
SDRace2Hispanic:Area5	0.628 (0.433)	0.731 (0.451)	0.449 (0.470)
SDRace2Afr American:EncounterTypeBicycle	-0.478 (0.540)	0.255 (0.590)	0.461 (0.595)
SDRace2Hispanic:EncounterTypeBicycle	-0.054 (0.936)	0.167 (1.012)	0.136 (1.007)
SDRace2Afr American:EncounterTypeOther	-1.495* (0.697)	-1.603^{+} (0.828)	-1.472 (0.897)
SDRace2Hispanic:EncounterTypeOther	0.635 (1.207)	1.838 (1.549)	1.742 (1.721)
SDRace2Afr American:EncounterTypePedestrian	-0.599* (0.302)	-0.654* (0.318)	-0.558 (0.346)
SDRace2Hispanic:EncounterTypePedestrian	-0.223 (0.477)	-0.046 (0.503)	-0.163 (0.535)
Area2:EncounterTypeBicycle	1.334 (0.820)	1.905* (0.875)	1.952* (0.894)

	Handcuffing by Type of Encounter		
	No Covariates	All Covariates	Fixed Effects
Area3:EncounterTypeBicycle	-0.712 (0.937)	0.070 (1.047)	0.954 (1.091)
Area4:EncounterTypeBicycle	0.881 (1.039)	1.917+ (1.078)	2.109+ (1.163)
Area5:EncounterTypeBicycle	-0.284 (1.264)	0.611 (1.327)	0.834 (1.371)
Area2:EncounterTypeOther	-10.635 (162.373)	-10.543 (155.759)	-16.071 (1,625.645)
Area3:EncounterTypeOther	-0.253 (1.023)	0.063 (1.155)	-0.713 (1.263)
Area4:EncounterTypeOther	-12.220 (162.373)	-11.895 (149.888)	-17.064 (1,806.774)
Area5:EncounterTypeOther	-2.294+ (1.277)	-1.982 (1.400)	-2.147 (1.445)
Area2:EncounterTypePedestrian	1.171* (0.507)	0.967+ (0.521)	0.976+ (0.562)
Area3:EncounterTypePedestrian	-0.136 (0.471)	-0.043 (0.501)	-0.171 (0.553)
Area4:EncounterTypePedestrian	-0.135 (0.544)	0.064 (0.573)	0.266 (0.618)
Area5:EncounterTypePedestrian	-0.180 (0.524)	-0.036 (0.560)	0.385 (0.614)
SDRace2Afr American:Area2:EncounterTypeBicycle	-1.755* (0.876)	-2.273* (0.936)	-2.398* (0.958)
SDRace2Hispanic:Area2:EncounterTypeBicycle	-1.320 (1.606)	-1.468 (1.690)	-1.531 (1.785)
SDRace2Afr American:Area3:EncounterTypeBicycle	-0.013 (1.031)	-0.712 (1.141)	-1.159 (1.185)
SDRace2Hispanic:Area3:EncounterTypeBicycle	-0.264 (1.438)	-0.769 (1.566)	-1.038 (1.603)
SDRace2Afr American:Area4:EncounterTypeBicycle	-1.644 (1.102)	-2.465* (1.145)	-2.448* (1.233)
SDRace2Hispanic:Area4:EncounterTypeBicycle	-1.992 (1.441)	-2.481+ (1.508)	-2.280 (1.585)
SDRace2Afr American:Area5:EncounterTypeBicycle	-0.141 (1.285)	-0.963 (1.352)	-1.357 (1.397)
SDRace2Hispanic:Area5:EncounterTypeBicycle	-0.939 (1.555)	-1.471 (1.645)	-1.627 (1.688)
SDRace2Afr American:Area2:EncounterTypeOther	10.866 (162.374)	10.647 (155.760)	15.830 (1,625.645)
SDRace2Hispanic:Area2:EncounterTypeOther	-1.659 (363.077)	-3.209 (360.168)	-4.569 (4,277.159)
SDRace2Afr American:Area3:EncounterTypeOther	0.273 (1.113)	-0.140 (1.260)	0.669 (1.393)
SDRace2Hispanic:Area3:EncounterTypeOther	-0.560 (1.533)	-1.762 (1.862)	-0.621 (2.060)
SDRace2Afr American:Area4:EncounterTypeOther	11.370 (162.374)	10.618 (149.889)	15.933 (1,806.774)
SDRace2Hispanic:Area4:EncounterTypeOther	9.624 (162.379)	7.581 (149.896)	12.824 (1,806.775)
SDRace2Afr American:Area5:EncounterTypeOther	1.888 (1.324)	1.790 (1.455)	1.949 (1.505)
SDRace2Hispanic:Area5:EncounterTypeOther	-0.432 (1.687)	-2.106 (1.992)	-1.900 (2.135)
SDRace2Afr American:Area2:EncounterTypePedestrian	-1.527** (0.537)	-1.234* (0.555)	-1.243* (0.597)
SDRace2Hispanic:Area2:EncounterTypePedestrian	-0.154 (0.873)	-0.205 (0.900)	-0.442 (0.962)
SDRace2Afr American:Area3:EncounterTypePedestrian	-0.611 (0.499)	-0.385 (0.532)	-0.364 (0.583)
SDRace2Hispanic:Area3:EncounterTypePedestrian	0.301 (0.658)	0.138 (0.698)	0.388 (0.757)

	Handcuffing by Type of Encounter		
	No Covariates	All Covariates	Fixed Effects
SDRace2Afr American:Area4:EncounterTypePedestrian	-0.111 (0.565)	-0.328 (0.596)	-0.523 (0.643)
SDRace2Hispanic:Area4:EncounterTypePedestrian	0.271 (0.694)	-0.258 (0.732)	-0.289 (0.783)
SDRace2Afr American:Area5:EncounterTypePedestrian	-0.433 (0.537)	-0.600 (0.575)	-0.952 (0.629)
SDRace2Hispanic:Area5:EncounterTypePedestrian	-0.042 (0.676)	-0.395 (0.721)	-0.635 (0.776)
Officers dummy	No	No	Yes
Census dummy	No	No	Yes
Observations	21,538	21,263	21,263
Log Likelihood	-8,687.255	-7,536.892	-6,738.263
Akaike Inf. Crit.	17,494.510	15,265.780	14,842.530
Note:	+ p < .10; * p < .05; ** p < .01; *** p<0.001		

Appendix J: Handcuffing by Reason for Encounter

	Handcuffing by Reason for Encounter		
	No Covariates	All Covariates	Fixed Effects
Constant	-3.561*** (0.188)	-3.306*** (0.351)	-16.478 (1,248.896)
SDRace2Afr American	1.281*** (0.200)	0.981*** (0.205)	0.789*** (0.214)
SDRace2Hispanic	-0.125 (0.315)	-0.484 (0.328)	-0.373 (0.339)
Area2	-1.094** (0.384)	-0.777* (0.394)	-1.130** (0.439)
Area3	0.185 (0.360)	0.397 (0.373)	-0.233 (0.456)
Area4	-0.018 (0.455)	-0.113 (0.470)	-0.701 (0.563)
Area5	0.709+ (0.391)	0.471 (0.413)	-0.247 (0.520)
Sex_RecodedFemale		-1.087*** (0.061)	-1.062*** (0.066)
AgeGroupUnder 17		0.343** (0.108)	0.362** (0.117)
AgeGroup30-39		-0.298*** (0.055)	-0.217*** (0.059)
AgeGroupOver 40		-0.760*** (0.052)	-0.650*** (0.058)
ReasonForEncounterConsensual Encounter	1.163 (0.762)	1.124 (0.795)	0.814 (0.859)
ReasonForEncounterProbable Cause	2.095*** (0.295)	1.929*** (0.312)	2.031*** (0.348)
ReasonForEncounterProbation/Parole	3.967*** (0.932)	3.728*** (0.957)	3.295** (1.010)
ReasonForEncounterReasonable Suspicion	2.126*** (0.532)	1.640** (0.556)	1.794** (0.595)
EncounterTypeBicycle		0.458*** (0.097)	0.411*** (0.105)
EncounterTypeOther		0.430** (0.139)	0.376* (0.158)
EncounterTypePedestrian		0.247*** (0.063)	0.214** (0.070)
Contact_WeekOrWeekendFri-Sun		0.117** (0.043)	0.024 (0.048)
ContactTimeOfDay7pm-7am		0.175*** (0.044)	0.235*** (0.055)
Hispanic_percent		0.420+ (0.253)	
BlackAloneNotHisp_percent		0.651* (0.266)	
TotalPop24YrsorYounger_percent		0.234 (0.485)	
Percent15yrsOrOlder_Divorced		0.531 (0.888)	
TotalUnemployed_percent		-0.157 (0.514)	
Owneroccupiedhousingunits_percent		0.571* (0.239)	
Incomein2013belowpovertylevel_percent		1.291*** (0.320)	
RateViolentCrime_tract		-0.001* (0.0004)	
RatePropertyCrime_tract		-0.0001+ (0.00005)	

	Handcuffing by Reason for Encounter		
	No Covariates	All Covariates	Fixed Effects
RateNarcoticsCrime_tract		0.002*** (0.0004)	
PopDensity		-0.00000 (0.00001)	
BusinessCount_persqmile		0.00004 (0.00003)	
QOL_per10000		0.0002** (0.0001)	
OfficerAge_Dynamic		-0.008+ (0.004)	
OfficerYearsExperience_Dynamic		-0.012* (0.006)	
OfficerGenderFemale		-0.288*** (0.079)	
OfficerRace_RecodedBlack		0.060 (0.064)	
OfficerRace_RecodedHispanic		0.145** (0.055)	
OfficerRace_RecodedAsianFili		-0.097 (0.062)	
OfficerRace_RecodedUnknowUndecl		0.388*** (0.116)	
SpecialAssignmentType_RecodedViolence Suppression		-0.120* (0.054)	-0.166* (0.066)
SpecialAssignmentType_RecodedProstitution		1.827*** (0.333)	2.112*** (0.368)
SpecialAssignmentType_RecodedNarcotics		1.292*** (0.186)	1.101*** (0.200)
SpecialAssignmentType_RecodedCruising		-0.203 (0.245)	-0.354 (0.282)
SpecialAssignmentType_RecodedSpecial Event		-0.098 (0.366)	-0.289 (0.409)
SpecialAssignmentType_RecodedOther		-0.626*** (0.071)	-0.446*** (0.084)
SDRace2Afr American:Area2	0.806* (0.405)	0.773+ (0.411)	0.727+ (0.424)
SDRace2Hispanic:Area2	0.155 (0.847)	0.211 (0.853)	-0.065 (0.879)
SDRace2Afr American:Area3	0.020 (0.378)	-0.087 (0.383)	0.136 (0.405)
SDRace2Hispanic:Area3	0.555 (0.481)	0.494 (0.492)	0.486 (0.514)
SDRace2Afr American:Area4	0.477 (0.466)	0.525 (0.470)	0.529 (0.485)
SDRace2Hispanic:Area4	0.942+ (0.536)	1.116* (0.546)	0.940+ (0.562)
SDRace2Afr American:Area5	0.130 (0.400)	0.202 (0.407)	0.230 (0.422)
SDRace2Hispanic:Area5	0.878+ (0.477)	0.858+ (0.490)	0.582 (0.507)
SDRace2Afr American:ReasonForEncounterConsensual Encounter	-0.165 (0.787)	-0.285 (0.820)	-0.196 (0.887)
SDRace2Hispanic:ReasonForEncounterConsensual Encounter	-0.186 (1.308)	-0.532 (1.340)	-0.580 (1.407)
SDRace2Afr American:ReasonForEncounterProbable Cause	-0.372 (0.312)	-0.277 (0.329)	-0.076 (0.363)
SDRace2Hispanic:ReasonForEncounterProbable Cause	0.015 (0.501)	0.260 (0.524)	0.375 (0.560)
SDRace2Afr American:ReasonForEncounterProbation/Parole	-1.014 (0.968)	-1.104 (0.995)	-0.748 (1.056)
SDRace2Hispanic:ReasonForEncounterProbation/Parole	-0.281 (1.713)	-0.914 (1.732)	-0.452 (1.799)

	Handcuffing by Reason for Encounter		
	No Covariates	All Covariates	Fixed Effects
SDRace2Afr American:ReasonForEncounterReasonable Suspicion	-0.366 (0.548)	-0.108 (0.571)	-0.438 (0.609)
SDRace2Hispanic:ReasonForEncounterReasonable Suspicion	0.712 (0.765)	0.967 (0.803)	0.811 (0.856)
Area2:ReasonForEncounterConsensual Encounter	1.294 (1.117)	1.061 (1.148)	1.629 (1.221)
Area3:ReasonForEncounterConsensual Encounter	-0.731 (1.314)	-0.792 (1.339)	-0.371 (1.432)
Area4:ReasonForEncounterConsensual Encounter	0.624 (1.068)	0.748 (1.102)	1.242 (1.182)
Area5:ReasonForEncounterConsensual Encounter	0.591 (0.982)	0.552 (1.020)	1.039 (1.111)
Area2:ReasonForEncounterProbable Cause	0.430 (0.515)	0.578 (0.528)	0.661 (0.576)
Area3:ReasonForEncounterProbable Cause	0.504 (0.516)	0.369 (0.537)	0.596 (0.595)
Area4:ReasonForEncounterProbable Cause	0.651 (0.634)	0.851 (0.652)	0.797 (0.707)
Area5:ReasonForEncounterProbable Cause	-0.069 (0.640)	0.150 (0.684)	0.122 (0.747)
Area2:ReasonForEncounterProbation/Parole	0.688 (1.727)	1.050 (1.816)	1.206 (1.833)
Area3:ReasonForEncounterProbation/Parole	-0.590 (1.721)	-0.589 (1.851)	-0.660 (1.839)
Area4:ReasonForEncounterProbation/Parole	-12.954 (187.494)	-14.027 (287.284)	-17.522 (2,232.650)
Area5:ReasonForEncounterProbation/Parole	-13.680 (229.631)	-14.889 (357.294)	-18.997 (2,682.295)
Area2:ReasonForEncounterReasonable Suspicion	1.241^{+} (0.745)	1.264 (0.769)	0.783 (0.830)
Area3:ReasonForEncounterReasonable Suspicion	0.557 (0.726)	0.791 (0.752)	0.800 (0.820)
Area4:ReasonForEncounterReasonable Suspicion	-0.116 (0.834)	0.420 (0.874)	-0.076 (0.932)
Area5:ReasonForEncounterReasonable Suspicion	0.321 (0.780)	0.494 (0.804)	0.500 (0.854)
SDRace2Afr American:Area2:ReasonForEncounterConsensual Encounter	-1.383 (1.182)	-1.296 (1.219)	-1.803 (1.302)
SDRace2Hispanic:Area2:ReasonForEncounterConsensual Encounter	-10.213 (132.587)	-10.861 (214.837)	-14.856 (1,503.921)
SDRace2Afr American:Area3:ReasonForEncounterConsensual Encounter	-0.136 (1.359)	-0.062 (1.385)	-0.411 (1.484)
SDRace2Hispanic:Area3:ReasonForEncounterConsensual Encounter	1.365 (1.774)	1.805 (1.806)	1.896 (1.912)
SDRace2Afr American:Area4:ReasonForEncounterConsensual Encounter	-1.130 (1.120)	-1.306 (1.158)	-1.513 (1.242)
SDRace2Hispanic:Area4:ReasonForEncounterConsensual Encounter	-0.416 (1.578)	-0.736 (1.615)	-0.848 (1.704)
SDRace2Afr American:Area5:ReasonForEncounterConsensual Encounter	-1.684^{+} (1.017)	-1.609 (1.056)	-1.894^{+} (1.146)
SDRace2Hispanic:Area5:ReasonForEncounterConsensual Encounter	-0.188 (1.478)	0.021 (1.517)	-0.115 (1.609)
SDRace2Afr American:Area2:ReasonForEncounterProbable Cause	-1.049^{+} (0.552)	-1.106^{+} (0.568)	-1.396^{*} (0.617)
SDRace2Hispanic:Area2:ReasonForEncounterProbable Cause	0.637 (1.043)	0.470 (1.063)	0.303 (1.136)
SDRace2Afr American:Area3:ReasonForEncounterProbable Cause	-0.891 (0.549)	-0.745 (0.571)	-1.139^{+} (0.628)
SDRace2Hispanic:Area3:ReasonForEncounterProbable Cause	-0.567 (0.713)	-0.679 (0.741)	-1.036 (0.804)
SDRace2Afr American:Area4:ReasonForEncounterProbable Cause	-0.815 (0.659)	-1.060 (0.679)	-1.213^{+} (0.735)

	Handcuffing by Reason for Encounter		
	No Covariates	All Covariates	Fixed Effects
SDRace2Hispanic:Area4:ReasonForEncounterProbable Cause	-0.436 (0.787)	-0.961 (0.814)	-1.156 (0.875)
SDRace2Afr American:Area5:ReasonForEncounterProbable Cause	-0.176 (0.655)	-0.471 (0.699)	-0.680 (0.761)
SDRace2Hispanic:Area5:ReasonForEncounterProbable Cause	-0.213 (0.790)	-0.786 (0.837)	-1.087 (0.902)
SDRace2Afr American:Area2:ReasonForEncounterProbation/Parole	-1.543 (1.840)	-1.777 (1.930)	-2.050 (1.967)
SDRace2Hispanic:Area2:ReasonForEncounterProbation/Parole	12.817 (324.752)	14.079 (535.417)	16.818 (3,956.181)
SDRace2Afr American:Area3:ReasonForEncounterProbation/Parole	-0.191 (1.797)	-0.061 (1.926)	-0.328 (1.929)
SDRace2Hispanic:Area3:ReasonForEncounterProbation/Parole	-0.332 (2.330)	0.297 (2.441)	-0.188 (2.464)
SDRace2Afr American:Area4:ReasonForEncounterProbation/Parole	12.670 (187.494)	13.630 (287.285)	17.282 (2,232.650)
SDRace2Hispanic:Area4:ReasonForEncounterProbation/Parole	12.029 (187.500)	13.708 (287.288)	16.979 (2,232.651)
SDRace2Afr American:Area5:ReasonForEncounterProbation/Parole	12.568 (229.631)	13.968 (357.294)	18.091 (2,682.295)
SDRace2Hispanic:Area5:ReasonForEncounterProbation/Parole	12.014 (229.635)	14.172 (357.297)	18.404 (2,682.295)
SDRace2Afr American:Area2:ReasonForEncounterReasonable Suspicion	-0.754 (0.784)	-0.907 (0.811)	-0.124 (0.873)
SDRace2Hispanic:Area2:ReasonForEncounterReasonable Suspicion	-0.466 (1.305)	-0.355 (1.341)	-0.012 (1.452)
SDRace2Afr American:Area3:ReasonForEncounterReasonable Suspicion	-1.148 (0.758)	-1.206 (0.786)	-1.043 (0.855)
SDRace2Hispanic:Area3:ReasonForEncounterReasonable Suspicion	-1.461 (0.976)	-1.700^{+} (1.018)	-1.718 (1.103)
SDRace2Afr American:Area4:ReasonForEncounterReasonable Suspicion	-0.161 (0.861)	-0.714 (0.902)	-0.107 (0.961)
SDRace2Hispanic:Area4:ReasonForEncounterReasonable Suspicion	-0.154 (1.034)	-0.845 (1.086)	-0.330 (1.159)
SDRace2Afr American:Area5:ReasonForEncounterReasonable Suspicion	-0.795 (0.798)	-1.049 (0.823)	-0.873 (0.873)
SDRace2Hispanic:Area5:ReasonForEncounterReasonable Suspicion	-0.438 (0.987)	-0.571 (1.026)	-0.565 (1.092)
Officers dummy	No	No	Yes
Census dummy	No	No	Yes
Observations	21,538	21,263	21,263
Log Likelihood	-8,218.954	-7,536.716	-6,724.582
Akaike Inf. Crit.	16,587.910	15,293.430	14,843.160
Note:	+ p < .10; * p < .05; ** p < .01; *** p<0.001		

Appendix H: Handcuffing by Special Assignment

	Handcuffing by Special Assignment		
	No Covariates	All Covariates	Fixed Effects
Constant	-2.703*** (0.195)	-3.276*** (0.350)	-17.434 (1,256.105)
SDRace2Afr American	1.443*** (0.202)	0.971*** (0.213)	0.696** (0.228)
SDRace2Hispanic	-0.267 (0.311)	-0.388 (0.324)	-0.375 (0.343)
Area2	0.001 (0.287)	0.359 (0.310)	-0.219 (0.386)
Area3	0.753** (0.276)	1.139*** (0.304)	0.442 (0.402)
Area4	0.550+ (0.316)	0.673+ (0.347)	-0.068 (0.459)
Area5	0.928** (0.311)	0.919** (0.351)	0.246 (0.472)
Sex_RecodedFemale		-1.076*** (0.060)	-1.044*** (0.065)
AgeGroupUnder 17		0.332** (0.107)	0.357** (0.116)
AgeGroup30-39		-0.286*** (0.054)	-0.206*** (0.059)
AgeGroupOver 40		-0.753*** (0.052)	-0.645*** (0.057)
ReasonForEncounterConsensual Encounter		0.450*** (0.103)	0.359** (0.116)
ReasonForEncounterProbable Cause		1.640*** (0.063)	1.725*** (0.075)
ReasonForEncounterProbation/Parole		2.164*** (0.135)	2.030*** (0.151)
ReasonForEncounterReasonable Suspicion		1.535*** (0.073)	1.484*** (0.084)
EncounterTypeBicycle		0.490*** (0.096)	0.429*** (0.104)
EncounterTypeOther		0.370** (0.137)	0.321* (0.157)
EncounterTypePedestrian		0.218*** (0.062)	0.197** (0.069)
Contact_WeekOrWeekendFri-Sun		0.097* (0.043)	0.013 (0.048)
ContactTimeOfDay7pm-7am		0.159*** (0.044)	0.200*** (0.054)
Hispanic_percent		0.455+ (0.251)	
BlackAloneNotHisp_percent		0.753** (0.262)	
TotalPop24YrsorYounger_percent		-0.042 (0.478)	
Percent15yrsOrOlder_Divorced		0.253 (0.879)	
TotalUnemployed_percent		0.020 (0.512)	
Owneroccupiedhousingunits_percent		0.546* (0.234)	
Incomein2013belowpovertylevel_percent		1.315*** (0.316)	
RateViolentCrime_tract		-0.001** (0.0003)	
RatePropertyCrime_tract		-0.0001* (0.00005)	
RateNarcoticsCrime_tract		0.002*** (0.0004)	

	Handcuffing by Special Assignment		
	No Covariates	All Covariates	Fixed Effects
PopDensity		-0.00000 (0.00001)	
BusinessCount_persqmile		0.0001 (0.00003)	
QOL_per10000		0.0002** (0.0001)	
OfficerAge_Dynamic		-0.008+ (0.004)	
OfficerYearsExperience_Dynamic		-0.012* (0.006)	
OfficerGenderFemale		-0.265*** (0.078)	
OfficerRace_RecodedBlack		0.061 (0.063)	
OfficerRace_RecodedHispanic		0.159** (0.055)	
OfficerRace_RecodedAsianFili		-0.114+ (0.061)	
OfficerRace_RecodedUnknowUndecl		0.343** (0.116)	
SpecialAssignment2Violence Suppression	-0.762* (0.327)	-0.172 (0.341)	-0.174 (0.360)
SpecialAssignment2Other	0.093 (0.303)	0.359 (0.326)	-0.048 (0.363)
SDRace2Afr American:Area2	-0.184 (0.306)	-0.240 (0.325)	-0.187 (0.359)
SDRace2Hispanic:Area2	0.955+ (0.504)	0.870 (0.532)	0.591 (0.599)
SDRace2Afr American:Area3	-0.905** (0.291)	-0.896** (0.311)	-0.658+ (0.342)
SDRace2Hispanic:Area3	0.290 (0.397)	-0.166 (0.420)	-0.196 (0.453)
SDRace2Afr American:Area4	-0.456 (0.327)	-0.260 (0.345)	-0.104 (0.368)
SDRace2Hispanic:Area4	0.676 (0.412)	0.510 (0.433)	0.431 (0.458)
SDRace2Afr American:Area5	-0.497 (0.318)	-0.420 (0.340)	-0.362 (0.361)
SDRace2Hispanic:Area5	0.690+ (0.404)	0.517 (0.427)	0.269 (0.452)
SDRace2Afr American:SpecialAssignment2Violence Suppression	-0.011 (0.345)	0.091 (0.359)	0.120 (0.376)
SDRace2Hispanic:SpecialAssignment2Violence Suppression	0.529 (0.543)	0.404 (0.562)	0.345 (0.581)
SDRace2Afr American:SpecialAssignment2Other	-0.471 (0.326)	-0.371 (0.352)	-0.069 (0.387)
SDRace2Hispanic:SpecialAssignment2Other	-0.002 (0.506)	-0.203 (0.548)	0.398 (0.579)
Area2:SpecialAssignment2Violence Suppression	-0.392 (0.503)	-0.664 (0.521)	-0.565 (0.562)
Area3:SpecialAssignment2Violence Suppression	-0.479 (0.701)	-1.341+ (0.721)	-1.180 (0.768)
Area4:SpecialAssignment2Violence Suppression	-11.651 (114.924)	-11.939 (109.893)	-14.360 (465.577)
Area5:SpecialAssignment2Violence Suppression	0.378 (0.733)	-0.451 (0.769)	-0.882 (0.836)
Area2:SpecialAssignment2Other	-2.016** (0.688)	-2.370*** (0.706)	-1.372+ (0.746)
Area3:SpecialAssignment2Other	-0.942+ (0.530)	-0.974+ (0.560)	-0.345 (0.611)
Area4:SpecialAssignment2Other	-0.383 (0.579)	-0.498 (0.610)	-0.106 (0.682)

	Handcuffing by Special Assignment		
	No Covariates	All Covariates	Fixed Effects
Area5:SpecialAssignment2Other	-0.570 (0.578)	-0.627 (0.616)	-0.201 (0.668)
SDRace2Afr American:Area2:SpecialAssignment2Violence Suppression	0.126 (0.541)	0.037 (0.563)	0.207 (0.597)
SDRace2Hispanic:Area2:SpecialAssignment2Violence Suppression	-1.358 (1.032)	-1.499 (1.060)	-1.162 (1.112)
SDRace2Afr American:Area3:SpecialAssignment2Violence Suppression	0.901 (0.728)	1.021 (0.751)	0.963 (0.795)
SDRace2Hispanic:Area3:SpecialAssignment2Violence Suppression	0.507 (0.875)	0.993 (0.903)	1.101 (0.950)
SDRace2Afr American:Area4:SpecialAssignment2Violence Suppression	12.224 (114.924)	12.029 (109.893)	14.327 (465.577)
SDRace2Hispanic:Area4:SpecialAssignment2Violence Suppression	11.344 (114.925)	11.574 (109.894)	14.140 (465.577)
SDRace2Afr American:Area5:SpecialAssignment2Violence Suppression	0.402 (0.746)	0.617 (0.783)	0.749 (0.849)
SDRace2Hispanic:Area5:SpecialAssignment2Violence Suppression	-0.012 (0.871)	0.350 (0.911)	0.686 (0.975)
SDRace2Afr American:Area2:SpecialAssignment2Other	1.497* (0.729)	1.766* (0.751)	1.490^{+} (0.793)
SDRace2Hispanic:Area2:SpecialAssignment2Other	0.775 (1.048)	0.838 (1.088)	0.178 (1.174)
SDRace2Afr American:Area3:SpecialAssignment2Other	0.831 (0.569)	0.628 (0.603)	0.327 (0.655)
SDRace2Hispanic:Area3:SpecialAssignment2Other	0.576 (0.721)	0.749 (0.771)	0.034 (0.823)
SDRace2Afr American:Area4:SpecialAssignment2Other	-0.213 (0.620)	-0.146 (0.655)	-0.106 (0.724)
SDRace2Hispanic:Area4:SpecialAssignment2Other	-0.529 (0.745)	-0.154 (0.794)	-0.417 (0.859)
SDRace2Afr American:Area5:SpecialAssignment2Other	0.509 (0.600)	0.283 (0.641)	0.178 (0.693)
SDRace2Hispanic:Area5:SpecialAssignment2Other	-0.135 (0.734)	-0.111 (0.788)	-0.739 (0.845)
Officers dummy	No	No	Yes
Census dummy	No	No	Yes
Observations	21,538	21,263	21,263
Log Likelihood	-9,093.304	-7,652.223	-6,830.332
Akaike Inf. Crit.	18,276.610	15,460.450	14,990.660
Note:	+ p < .10; * p < .05; ** p < .01; *** p<0.001		

Appendix I: Search (No Moderator)

	Search (No Moderator)		
	No Covariates	All Covariates	Fixed Effects
Constant	-3.600*** (0.179)	-3.681*** (0.391)	-4.566** (1.480)
SDRace2Afr American	1.610*** (0.187)	1.040*** (0.204)	0.993*** (0.222)
SDRace2Asian	0.006 (0.333)	0.488 (0.350)	0.507 (0.373)
SDRace2Hispanic	0.533* (0.253)	0.399 (0.275)	0.618* (0.296)
SDRace2Other	-0.473 (0.535)	-0.267 (0.554)	-0.140 (0.579)
Area2	-0.101 (0.262)	-0.013 (0.289)	-0.194 (0.393)
Area3	1.279*** (0.247)	1.211*** (0.286)	0.543 (0.426)
Area4	1.301*** (0.270)	0.938** (0.316)	0.656 (0.471)
Area5	1.715*** (0.268)	1.020** (0.327)	0.587 (0.493)
Sex_RecodedFemale		-0.976*** (0.064)	-0.937*** (0.072)
AgeGroupUnder 17		0.943*** (0.108)	1.024*** (0.121)
AgeGroup30-39		-0.551*** (0.064)	-0.478*** (0.071)
AgeGroupOver 40		-1.049*** (0.061)	-0.938*** (0.069)
ReasonForEncounterConsensual Encounter		1.269*** (0.105)	1.245*** (0.122)
ReasonForEncounterProbable Cause		2.076*** (0.068)	2.348*** (0.087)
ReasonForEncounterReasonable Suspicion		1.960*** (0.080)	2.019*** (0.095)
EncounterTypeBicycle		-0.317* (0.151)	-0.354* (0.167)
EncounterTypeOther		1.042*** (0.137)	0.830*** (0.168)
EncounterTypePedestrian		0.194** (0.068)	0.170* (0.080)
Contact_WeekOrWeekendFri-Sun		0.061 (0.049)	-0.029 (0.057)
ContactTimeOfDay7pm-7am		-0.006 (0.050)	0.120+ (0.063)
Hispanic_percent		1.012*** (0.285)	
BlackAloneNotHisp_percent		1.319*** (0.293)	
TotalPop24YrsorYounger_percent		0.124 (0.561)	
Percent15yrsOrOlder_Divorced		-0.037 (1.014)	
TotalUnemployed_percent		0.563 (0.571)	
Owneroccupiedhousingunits_percent		0.598* (0.272)	
Incomein2013belowpovertylevel_percent		0.595 (0.368)	
RateViolentCrime_tract		-0.002*** (0.0004)	
RatePropertyCrime_tract		-0.0001 (0.0001)	

	Search (No Moderator)		
	No Covariates	All Covariates	Fixed Effects
RateNarcoticsCrime_tract		0.001^{+} (0.001)	
PopDensity		-0.00000 (0.00001)	
BusinessCount_persqmile		0.0001^{**} (0.00004)	
QOL_per10000		0.0001^{+} (0.0001)	
OfficerAge_Dynamic		-0.008^{+} (0.005)	
OfficerYearsExperience_Dynamic		-0.007 (0.006)	
OfficerGenderFemale		-0.222^{*} (0.086)	
OfficerRace_RecodedBlack		-0.058 (0.073)	
OfficerRace_RecodedHispanic		0.251^{***} (0.062)	
OfficerRace_RecodedAsianFili		-0.215^{**} (0.070)	
OfficerRace_RecodedUnknowUndecl		0.494^{***} (0.126)	
SpecialAssignmentType_RecodedViolence Suppression		-0.271^{***} (0.064)	-0.380^{***} (0.080)
SpecialAssignmentType_RecodedProstitution		0.928^{**} (0.305)	0.392 (0.381)
SpecialAssignmentType_RecodedNarcotics		1.920^{***} (0.217)	1.526^{***} (0.239)
SpecialAssignmentType_RecodedCruising		0.160 (0.246)	-0.207 (0.296)
SpecialAssignmentType_RecodedSpecial Event		-1.082^{*} (0.518)	-1.313^{*} (0.576)
SpecialAssignmentType_RecodedOther		-0.707^{***} (0.078)	-0.565^{***} (0.095)
SDRace2Afr American:Area2	-0.127 (0.280)	-0.075 (0.300)	-0.015 (0.336)
SDRace2Asian:Area2	-0.252 (0.564)	-0.589 (0.585)	-0.554 (0.640)
SDRace2Hispanic:Area2	0.363 (0.414)	0.144 (0.443)	0.183 (0.491)
SDRace2Other:Area2	0.780 (0.728)	0.340 (0.762)	0.044 (0.867)
SDRace2Afr American:Area3	-0.872^{***} (0.261)	-0.841^{**} (0.288)	-0.861^{**} (0.328)
SDRace2Asian:Area3	0.549 (0.392)	-0.301 (0.421)	-0.362 (0.466)
SDRace2Hispanic:Area3	-0.108 (0.323)	-0.452 (0.356)	-0.728^{+} (0.399)
SDRace2Other:Area3	0.029 (0.658)	-0.712 (0.702)	-0.790 (0.752)
SDRace2Afr American:Area4	-0.930^{***} (0.282)	-0.765^{*} (0.307)	-0.917^{**} (0.338)
SDRace2Asian:Area4	-1.485^{*} (0.638)	-2.217^{**} (0.725)	-2.287^{**} (0.775)
SDRace2Hispanic:Area4	-0.219 (0.335)	-0.333 (0.365)	-0.706^{+} (0.397)
SDRace2Other:Area4	0.287 (0.669)	-0.361 (0.713)	-0.917 (0.773)
SDRace2Afr American:Area5	-0.700^{*} (0.276)	-0.523^{+} (0.309)	-0.695^{*} (0.337)
SDRace2Asian:Area5	-0.557 (0.553)	-1.351^{*} (0.606)	-1.479^{*} (0.653)

	Search (No Moderator)		
	No Covariates	All Covariates	Fixed Effects
SDRace2Hispanic:Area5	-0.040 (0.330)	-0.139 (0.367)	-0.641 (0.397)
SDRace2Other:Area5	0.854 (0.618)	0.191 (0.660)	-0.014 (0.701)
Officers dummy	No	No	Yes
Census dummy	No	No	Yes
Observations	22,048	21,759	21,759
Log Likelihood	-8,106.776	-6,098.352	-5,093.658
Akaike Inf. Crit.	16,263.550	12,322.700	11,491.320
Note:	+ p < .10; * p < .05; ** p < .01; *** p<0.001		

Appendix J: Search by Type of Encounter

	Search by Type of Encounter		
	No Covariates	All Covariates	Fixed Effects
Constant	-4.924*** (0.379)	-4.278*** (0.534)	-5.393** (1.980)
SDRace2Afr American	2.235*** (0.389)	1.814*** (0.407)	1.584*** (0.418)
SDRace2Hispanic	1.186** (0.461)	0.812+ (0.481)	0.919+ (0.498)
Area2	-0.580 (0.628)	-0.557 (0.646)	-0.974 (0.730)
Area3	1.523** (0.488)	1.253* (0.517)	0.300 (0.633)
Area4	2.170*** (0.469)	1.566** (0.507)	0.966 (0.635)
Area5	2.164*** (0.500)	1.418** (0.544)	0.714 (0.672)
Sex_RecodedFemale		-1.040*** (0.067)	-0.986*** (0.076)
AgeGroupUnder 17		0.959*** (0.110)	1.031*** (0.124)
AgeGroup30-39		-0.587*** (0.066)	-0.511*** (0.074)
AgeGroupOver 40		-1.074*** (0.063)	-0.979*** (0.072)
ReasonForEncounterConsensual Encounter		1.274*** (0.110)	1.236*** (0.128)
ReasonForEncounterProbable Cause		2.099*** (0.073)	2.337*** (0.091)
ReasonForEncounterReasonable Suspicion		2.004*** (0.084)	2.063*** (0.099)
EncounterTypeBicycle	1.096 (1.080)	-0.430 (1.167)	-0.691 (1.282)
EncounterTypeOther	3.943*** (0.776)	3.520*** (0.917)	2.939** (0.952)
EncounterTypePedestrian	2.932*** (0.445)	1.552** (0.473)	1.367** (0.499)
Contact_WeekOrWeekendFri-Sun		0.062 (0.051)	-0.026 (0.059)
ContactTimeOfDay7pm-7am		0.014 (0.052)	0.122+ (0.066)
Hispanic_percent		1.137*** (0.300)	
BlackAloneNotHisp_percent		1.378*** (0.310)	
TotalPop24YrsorYounger_percent		0.284 (0.580)	
Percent15yrsOrOlder_Divorced		-0.568 (1.054)	
TotalUnemployed_percent		0.314 (0.591)	
Owneroccupiedhousingunits_percent		0.353 (0.296)	
Incomein2013belowpovertylevel_percent		0.452 (0.384)	
RateViolentCrime_tract		-0.002*** (0.0004)	
RatePropertyCrime_tract		-0.0001 (0.0001)	
RateNarcoticsCrime_tract		0.001 (0.001)	
PopDensity		-0.00000 (0.00001)	

	Search by Type of Encounter		
	No Covariates	All Covariates	Fixed Effects
BusinessCount_persqmile		0.0001* (0.00004)	
QOL_per10000		0.0001+ (0.0001)	
OfficerAge_Dynamic		-0.009+ (0.005)	
OfficerYearsExperience_Dynamic		-0.007 (0.007)	
OfficerGenderFemale		-0.202* (0.090)	
OfficerRace_RecodedBlack		-0.083 (0.076)	
OfficerRace_RecodedHispanic		0.238*** (0.064)	
OfficerRace_RecodedAsianFili		-0.225** (0.073)	
OfficerRace_RecodedUnknowUndecl		0.441** (0.134)	
SpecialAssignmentType_RecodedViolence Suppression		-0.244*** (0.065)	-0.345*** (0.083)
SpecialAssignmentType_RecodedProstitution		0.881** (0.320)	0.483 (0.402)
SpecialAssignmentType_RecodedNarcotics		1.870*** (0.228)	1.497*** (0.249)
SpecialAssignmentType_RecodedCruising		0.064 (0.261)	-0.255 (0.313)
SpecialAssignmentType_RecodedSpecial Event		-0.904+ (0.516)	-1.121+ (0.581)
SpecialAssignmentType_RecodedOther		-0.749*** (0.083)	-0.644*** (0.102)
SDRace2Afr American:Area2	0.671 (0.645)	0.701 (0.661)	0.975 (0.708)
SDRace2Hispanic:Area2	0.023 (0.895)	-0.102 (0.913)	0.065 (0.960)
SDRace2Afr American:Area3	-0.823 (0.503)	-0.792 (0.527)	-0.621 (0.568)
SDRace2Hispanic:Area3	-0.450 (0.578)	-0.506 (0.605)	-0.461 (0.648)
SDRace2Afr American:Area4	-1.448** (0.483)	-1.257* (0.510)	-1.168* (0.535)
SDRace2Hispanic:Area4	-0.727 (0.547)	-0.592 (0.576)	-0.734 (0.605)
SDRace2Afr American:Area5	-0.811 (0.510)	-0.700 (0.539)	-0.741 (0.560)
SDRace2Hispanic:Area5	-0.106 (0.571)	-0.169 (0.602)	-0.568 (0.628)
SDRace2Afr American:EncounterTypeBicycle	-0.921 (1.118)	0.211 (1.210)	0.572 (1.329)
SDRace2Hispanic:EncounterTypeBicycle	1.102 (1.280)	1.794 (1.439)	1.896 (1.579)
SDRace2Afr American:EncounterTypeOther	-1.626+ (0.833)	-2.286* (0.985)	-1.791+ (1.020)
SDRace2Hispanic:EncounterTypeOther	0.488 (1.473)	13.711 (622.208)	18.188 (4,597.380)
SDRace2Afr American:EncounterTypePedestrian	-1.248** (0.460)	-1.251* (0.490)	-0.981+ (0.517)
SDRace2Hispanic:EncounterTypePedestrian	-0.788 (0.599)	-0.586 (0.637)	-0.452 (0.674)
Area2:EncounterTypeBicycle	-10.157 (156.053)	-9.199 (139.780)	-12.878 (960.195)
Area3:EncounterTypeBicycle	0.226 (1.350)	1.460 (1.532)	2.282 (1.841)

	Search by Type of Encounter		
	No Covariates	All Covariates	Fixed Effects
Area4:EncounterTypeBicycle	0.273 (1.578)	2.215 (1.650)	3.316^{+} (1.814)
Area5:EncounterTypeBicycle	0.278 (1.588)	1.137 (1.745)	1.124 (1.844)
Area2:EncounterTypeOther	-13.005 (509.653)	-13.618 (496.236)	-17.731 (3,626.338)
Area3:EncounterTypeOther	-1.928 (1.395)	-2.717^{+} (1.578)	-4.717* (1.883)
Area4:EncounterTypeOther	-15.755 (624.194)	-16.120 (569.173)	-18.758 (4,444.228)
Area5:EncounterTypeOther	-2.793* (1.382)	-16.713 (361.290)	-19.878 (2,728.949)
Area2:EncounterTypePedestrian	0.997 (0.707)	0.817 (0.730)	1.331^{+} (0.798)
Area3:EncounterTypePedestrian	0.037 (0.601)	0.360 (0.638)	0.727 (0.712)
Area4:EncounterTypePedestrian	-1.453* (0.617)	-1.024 (0.654)	-0.717 (0.709)
Area5:EncounterTypePedestrian	-0.517 (0.636)	-0.334 (0.681)	-0.177 (0.734)
SDRace2Afr American:Area2:EncounterTypeBicycle	8.978 (156.054)	8.031 (139.782)	11.562 (960.195)
SDRace2Hispanic:Area2:EncounterTypeBicycle	9.952 (156.059)	9.691 (139.788)	13.736 (960.196)
SDRace2Afr American:Area3:EncounterTypeBicycle	-0.492 (1.482)	-1.524 (1.667)	-2.075 (1.987)
SDRace2Hispanic:Area3:EncounterTypeBicycle	-1.899 (1.697)	-2.528 (1.929)	-2.999 (2.252)
SDRace2Afr American:Area4:EncounterTypeBicycle	-1.699 (1.761)	-3.373^{+} (1.841)	-4.172* (1.999)
SDRace2Hispanic:Area4:EncounterTypeBicycle	-3.352^{+} (2.004)	-4.700* (2.125)	-5.978* (2.339)
SDRace2Afr American:Area5:EncounterTypeBicycle	-0.552 (1.631)	-1.178 (1.793)	-1.365 (1.899)
SDRace2Hispanic:Area5:EncounterTypeBicycle	-2.482 (1.800)	-2.878 (2.013)	-2.929 (2.148)
SDRace2Afr American:Area2:EncounterTypeOther	11.676 (509.653)	11.988 (496.236)	15.438 (3,626.338)
SDRace2Hispanic:Area2:EncounterTypeOther	-1.697 (1,019.306)	-14.920 (1,188.540)	-19.471 (8,765.332)
SDRace2Afr American:Area3:EncounterTypeOther	1.754 (1.466)	2.383 (1.662)	4.607* (2.007)
SDRace2Hispanic:Area3:EncounterTypeOther	0.413 (1.948)	-12.524 (622.209)	-14.500 (4,597.380)
SDRace2Afr American:Area4:EncounterTypeOther	14.999 (624.195)	14.778 (569.174)	17.217 (4,444.228)
SDRace2Hispanic:Area4:EncounterTypeOther	13.060 (624.196)	-1.102 (843.268)	-2.892 (6,394.300)
SDRace2Afr American:Area5:EncounterTypeOther	2.521^{+} (1.433)	16.607 (361.290)	19.817 (2,728.949)
SDRace2Hispanic:Area5:EncounterTypeOther	-0.785 (1.921)	-1.042 (719.495)	-2.025 (5,346.313)
SDRace2Afr American:Area2:EncounterTypePedestrian	-1.517* (0.736)	-1.255 (0.764)	-1.661* (0.840)
SDRace2Hispanic:Area2:EncounterTypePedestrian	0.719 (1.071)	0.703 (1.115)	1.074 (1.248)
SDRace2Afr American:Area3:EncounterTypePedestrian	-0.561 (0.628)	-0.654 (0.670)	-1.066 (0.745)
SDRace2Hispanic:Area3:EncounterTypePedestrian	0.207 (0.766)	-0.179 (0.817)	-0.866 (0.905)
SDRace2Afr American:Area4:EncounterTypePedestrian	1.146^{+} (0.643)	0.627 (0.685)	0.148 (0.744)

	Search by Type of Encounter		
	No Covariates	All Covariates	Fixed Effects
SDRace2Hispanic:Area4:EncounterTypePedestrian	1.103 (0.766)	0.308 (0.818)	-0.132 (0.885)
SDRace2Afr American:Area5:EncounterTypePedestrian	-0.139 (0.652)	-0.406 (0.701)	-0.611 (0.755)
SDRace2Hispanic:Area5:EncounterTypePedestrian	0.223 (0.779)	-0.263 (0.838)	-0.510 (0.900)
Officers dummy	No	No	Yes
Census dummy	No	No	Yes
Observations	19,607	19,353	19,353
Log Likelihood	-6,918.160	-5,629.458	-4,702.262
Akaike Inf. Crit.	13,956.320	11,448.920	10,764.520
Note:	+ p < .10; * p < .05; ** p < .01; *** p<0.001		

Appendix K: Search by Reason for Encounter

	Search by Reason for Encounter		
	No Covariates	All Covariates	Fixed Effects
Constant	-4.853*** (0.355)	-4.011*** (0.505)	-5.305* (2.311)
SDRace2Afr American	1.676*** (0.370)	1.353*** (0.375)	1.157** (0.390)
SDRace2Hispanic	1.117* (0.441)	0.728 (0.453)	0.794+ (0.472)
Area2	-2.003+ (1.062)	-1.949+ (1.066)	-2.182* (1.103)
Area3	0.707 (0.574)	0.834 (0.585)	-0.181 (0.701)
Area4	1.457** (0.523)	1.023+ (0.541)	0.546 (0.665)
Area5	1.775*** (0.525)	1.110* (0.548)	0.439 (0.678)
Sex_RecodedFemale		-1.011*** (0.067)	-0.970*** (0.075)
AgeGroupUnder 17		0.927*** (0.110)	1.016*** (0.124)
AgeGroup30-39		-0.581*** (0.066)	-0.490*** (0.074)
AgeGroupOver 40		-1.067*** (0.063)	-0.967*** (0.072)
ReasonForEncounterConsensual Encounter	3.518*** (0.615)	3.474*** (0.667)	3.078*** (0.739)
ReasonForEncounterProbable Cause	2.962*** (0.445)	2.654*** (0.464)	2.762*** (0.500)
ReasonForEncounterReasonable Suspicion	2.907*** (0.712)	2.385** (0.738)	2.714*** (0.764)
EncounterTypeBicycle		-0.357* (0.152)	-0.355* (0.168)
EncounterTypeOther		0.882*** (0.150)	0.708*** (0.183)
EncounterTypePedestrian		0.158* (0.071)	0.130 (0.083)
Contact_WeekOrWeekendFri-Sun		0.062 (0.051)	-0.025 (0.059)
ContactTimeOfDay7pm-7am		0.004 (0.052)	0.131* (0.066)
Hispanic_percent		1.160*** (0.298)	
BlackAloneNotHisp_percent		1.562*** (0.309)	
TotalPop24YrsorYounger_percent		0.088 (0.582)	
Percent15yrsOrOlder_Divorced		-0.864 (1.054)	
TotalUnemployed_percent		0.282 (0.589)	
Owneroccupiedhousingunits_percent		0.455 (0.294)	
Incomein2013belowpovertylevel_percent		0.541 (0.385)	
RateViolentCrime_tract		-0.002*** (0.0004)	
RatePropertyCrime_tract		-0.0001 (0.0001)	
RateNarcoticsCrime_tract		0.001 (0.001)	
PopDensity		-0.00000 (0.00001)	

BusinessCount_persqmile		0.0001* (0.00004)	
QOL_per10000		0.0001 (0.0001)	
OfficerAge_Dynamic		-0.009^{+} (0.005)	
OfficerYearsExperience_Dynamic		-0.004 (0.007)	
OfficerGenderFemale		-0.212* (0.090)	
OfficerRace_RecodedBlack		-0.082 (0.076)	
OfficerRace_RecodedHispanic		0.220*** (0.064)	
OfficerRace_RecodedAsianFili		-0.179* (0.073)	
OfficerRace_RecodedUnknowUndecl		0.488*** (0.133)	
SpecialAssignmentType_RecodedViolence Suppression		-0.199** (0.065)	-0.330*** (0.083)
SpecialAssignmentType_RecodedProstitution		0.862** (0.318)	0.362 (0.400)
SpecialAssignmentType_RecodedNarcotics		1.831*** (0.227)	1.494*** (0.249)
SpecialAssignmentType_RecodedCruising		0.069 (0.258)	-0.285 (0.311)
SpecialAssignmentType_RecodedSpecial Event		-0.967^{+} (0.519)	-1.150* (0.576)
SpecialAssignmentType_RecodedOther		-0.737*** (0.083)	-0.625*** (0.102)
SDRace2Afr American:Area2	2.195* (1.075)	2.240* (1.079)	2.378* (1.091)
SDRace2Hispanic:Area2	0.426 (1.483)	0.503 (1.488)	0.341 (1.501)
SDRace2Afr American:Area3	0.033 (0.594)	-0.117 (0.599)	0.165 (0.648)
SDRace2Hispanic:Area3	0.117 (0.660)	-0.005 (0.671)	0.038 (0.720)
SDRace2Afr American:Area4	-0.628 (0.542)	-0.620 (0.548)	-0.563 (0.571)
SDRace2Hispanic:Area4	-0.484 (0.599)	-0.389 (0.611)	-0.489 (0.638)
SDRace2Afr American:Area5	-0.406 (0.539)	-0.325 (0.546)	-0.308 (0.571)
SDRace2Hispanic:Area5	-0.100 (0.595)	-0.179 (0.609)	-0.434 (0.637)
SDRace2Afr American:ReasonForEncounterConsensual Encounter	-1.487* (0.655)	-1.602* (0.708)	-1.155 (0.780)
SDRace2Hispanic:ReasonForEncounterConsensual Encounter	-1.574 (1.015)	-1.960^{+} (1.084)	-1.754 (1.148)
SDRace2Afr American:ReasonForEncounterProbable Cause	-0.605 (0.466)	-0.415 (0.486)	0.016 (0.521)
SDRace2Hispanic:ReasonForEncounterProbable Cause	-0.767 (0.606)	-0.357 (0.635)	0.151 (0.679)
SDRace2Afr American:ReasonForEncounterReasonable Suspicion	-0.333 (0.731)	-0.093 (0.756)	-0.421 (0.783)
SDRace2Hispanic:ReasonForEncounterReasonable Suspicion	-0.270 (0.953)	-0.003 (0.999)	-0.095 (1.045)
Area2:ReasonForEncounterConsensual Encounter	2.422^{+} (1.270)	2.310^{+} (1.306)	2.915* (1.382)
Area3:ReasonForEncounterConsensual Encounter	-0.624 (0.950)	-0.580 (0.994)	0.285 (1.116)
Area4:ReasonForEncounterConsensual Encounter	-0.969 (0.874)	-0.676 (0.925)	0.032 (1.050)
Area5:ReasonForEncounterConsensual Encounter	-0.758 (0.863)	-0.712 (0.917)	-0.150 (1.001)

Area2:ReasonForEncounterProbable Cause	1.416 (1.135)	1.825 (1.145)	2.344* (1.189)
Area3:ReasonForEncounterProbable Cause	0.545 (0.694)	0.521 (0.715)	0.936 (0.811)
Area4:ReasonForEncounterProbable Cause	-0.622 (0.717)	-0.261 (0.742)	0.016 (0.814)
Area5:ReasonForEncounterProbable Cause	-0.290 (0.717)	-0.109 (0.765)	-0.400 (0.816)
Area2:ReasonForEncounterReasonable Suspicion	2.888* (1.287)	2.999* (1.308)	2.266+ (1.354)
Area3:ReasonForEncounterReasonable Suspicion	0.497 (0.926)	0.834 (0.950)	1.003 (1.032)
Area4:ReasonForEncounterReasonable Suspicion	-0.854 (0.930)	-0.118 (0.961)	-0.438 (1.028)
Area5:ReasonForEncounterReasonable Suspicion	-1.008 (0.992)	-0.781 (1.024)	-0.421 (1.101)
SDRace2Afr American:Area2:ReasonForEncounterConsensual Encounter	-3.172* (1.341)	-3.123* (1.382)	-3.745* (1.470)
SDRace2Hispanic:Area2:ReasonForEncounterConsensual Encounter	0.947 (1.984)	1.183 (2.042)	2.140 (2.315)
SDRace2Afr American:Area3:ReasonForEncounterConsensual Encounter	-0.356 (1.009)	-0.542 (1.058)	-1.463 (1.187)
SDRace2Hispanic:Area3:ReasonForEncounterConsensual Encounter	-0.254 (1.407)	0.007 (1.474)	-0.711 (1.610)
SDRace2Afr American:Area4:ReasonForEncounterConsensual Encounter	-0.035 (0.950)	-0.376 (1.007)	-1.252 (1.133)
SDRace2Hispanic:Area4:ReasonForEncounterConsensual Encounter	0.598 (1.273)	0.018 (1.348)	-0.765 (1.468)
SDRace2Afr American:Area5:ReasonForEncounterConsensual Encounter	-0.543 (0.907)	-0.445 (0.965)	-0.958 (1.047)
SDRace2Hispanic:Area5:ReasonForEncounterConsensual Encounter	0.028 (1.225)	0.071 (1.301)	-0.376 (1.386)
SDRace2Afr American:Area2:ReasonForEncounterProbable Cause	-2.656* (1.163)	-2.883* (1.176)	-3.636** (1.225)
SDRace2Hispanic:Area2:ReasonForEncounterProbable Cause	0.027 (1.631)	-0.401 (1.653)	-0.149 (1.742)
SDRace2Afr American:Area3:ReasonForEncounterProbable Cause	-0.907 (0.727)	-0.849 (0.750)	-1.628+ (0.850)
SDRace2Hispanic:Area3:ReasonForEncounterProbable Cause	-0.193 (0.850)	-0.423 (0.882)	-0.940 (0.990)
SDRace2Afr American:Area4:ReasonForEncounterProbable Cause	0.482 (0.747)	0.135 (0.776)	-0.484 (0.850)
SDRace2Hispanic:Area4:ReasonForEncounterProbable Cause	1.021 (0.859)	0.294 (0.896)	-0.640 (0.978)
SDRace2Afr American:Area5:ReasonForEncounterProbable Cause	0.023 (0.737)	-0.185 (0.786)	-0.187 (0.839)
SDRace2Hispanic:Area5:ReasonForEncounterProbable Cause	0.379 (0.855)	-0.097 (0.911)	-0.513 (0.972)
SDRace2Afr American:Area2:ReasonForEncounterReasonable Suspicion	-2.596* (1.318)	-2.835* (1.343)	-2.050 (1.392)
SDRace2Hispanic:Area2:ReasonForEncounterReasonable Suspicion	-0.906 (1.834)	-0.832 (1.875)	0.323 (1.996)
SDRace2Afr American:Area3:ReasonForEncounterReasonable Suspicion	-1.342 (0.959)	-1.494 (0.986)	-1.302 (1.073)
SDRace2Hispanic:Area3:ReasonForEncounterReasonable Suspicion	-1.139 (1.177)	-1.437 (1.225)	-1.704 (1.330)
SDRace2Afr American:Area4:ReasonForEncounterReasonable Suspicion	0.132 (0.964)	-0.639 (0.999)	-0.331 (1.067)
SDRace2Hispanic:Area4:ReasonForEncounterReasonable Suspicion	0.893 (1.158)	0.043 (1.211)	0.068 (1.294)
SDRace2Afr American:Area5:ReasonForEncounterReasonable Suspicion	0.265 (1.012)	0.004 (1.046)	-0.324 (1.125)
SDRace2Hispanic:Area5:ReasonForEncounterReasonable Suspicion	1.125 (1.206)	0.908 (1.258)	0.401 (1.347)
Officers dummy	No	No	Yes

Census dummy	No	No	Yes
Observations	19,607	19,353	19,353
Log Likelihood	-6,335.292	-5,643.802	-4,711.627
Akaike Inf. Crit.	12,790.580	11,477.600	10,783.250
Note:	+ p < .10; * p < .05; ** p < .01; *** p<0.001		

Appendix L: Search by Special Assignment

	Search by Special Assignment		
	No Covariates	All Covariates	Fixed Effects
Constant	-2.972*** (0.224)	-3.272*** (0.417)	-4.926** (1.576)
SDRace2Afr American	1.324*** (0.233)	0.796** (0.250)	0.689* (0.276)
SDRace2Hispanic	0.252 (0.314)	0.267 (0.335)	0.423 (0.368)
Area2	0.375 (0.302)	0.532 (0.335)	0.041 (0.450)
Area3	0.952** (0.306)	1.246*** (0.345)	0.393 (0.497)
Area4	1.046** (0.323)	0.902* (0.368)	0.293 (0.525)
Area5	1.316*** (0.327)	0.907* (0.383)	0.148 (0.552)
Sex_RecodedFemale		-1.004*** (0.065)	-0.975*** (0.074)
AgeGroupUnder 17		0.918*** (0.108)	1.020*** (0.123)
AgeGroup30-39		-0.569*** (0.065)	-0.476*** (0.073)
AgeGroupOver 40		-1.054*** (0.062)	-0.955*** (0.071)
ReasonForEncounterConsensual Encounter		1.247*** (0.108)	1.253*** (0.126)
ReasonForEncounterProbable Cause		2.180*** (0.071)	2.412*** (0.090)
ReasonForEncounterReasonable Suspicion		2.021*** (0.082)	2.076*** (0.098)
EncounterTypeBicycle		-0.305* (0.151)	-0.318+ (0.166)
EncounterTypeOther		0.756*** (0.146)	0.585** (0.179)
EncounterTypePedestrian		0.113 (0.070)	0.109 (0.082)
Contact_WeekOrWeekendFri-Sun		0.040 (0.050)	-0.039 (0.059)
ContactTimeOfDay7pm-7am		0.014 (0.052)	0.126+ (0.065)
Hispanic_percent		1.186*** (0.294)	
BlackAloneNotHisp_percent		1.645*** (0.303)	
TotalPop24YrsorYounger_percent		-0.257 (0.573)	
Percent15yrsOrOlder_Divorced		-0.859 (1.039)	
TotalUnemployed_percent		0.533 (0.586)	
Owneroccupiedhousingunits_percent		0.402 (0.285)	
Incomein2013belowpovertylevel_percent		0.635+ (0.380)	
RateViolentCrime_tract		-0.002*** (0.0004)	
RatePropertyCrime_tract		-0.0001 (0.0001)	
RateNarcoticsCrime_tract		0.001+ (0.001)	
PopDensity		-0.00000 (0.00001)	

	Search by Special Assignment		
	No Covariates	All Covariates	Fixed Effects
BusinessCount_persqmile		0.0001** (0.00004)	
QOL_per10000		0.0001* (0.0001)	
OfficerAge_Dynamic		-0.011* (0.005)	
OfficerYearsExperience_Dynamic		-0.002 (0.006)	
OfficerGenderFemale		-0.218* (0.089)	
OfficerRace_RecodedBlack		-0.086 (0.075)	
OfficerRace_RecodedHispanic		0.241*** (0.063)	
OfficerRace_RecodedAsianFili		-0.214** (0.072)	
OfficerRace_RecodedUnknowUndecl		0.429** (0.133)	
SpecialAssignment2Violence Suppression	-1.412** (0.468)	-0.705 (0.485)	-0.828 (0.514)
SpecialAssignment2Other	-1.054* (0.504)	-0.900+ (0.530)	-1.510* (0.588)
SDRace2Afr American:Area2	-0.301 (0.325)	-0.306 (0.354)	-0.247 (0.406)
SDRace2Hispanic:Area2	0.121 (0.528)	-0.284 (0.576)	-0.353 (0.647)
SDRace2Afr American:Area3	-0.663* (0.323)	-0.706* (0.353)	-0.719+ (0.407)
SDRace2Hispanic:Area3	0.153 (0.402)	-0.452 (0.438)	-0.825+ (0.498)
SDRace2Afr American:Area4	-0.878** (0.338)	-0.752* (0.366)	-0.799* (0.405)
SDRace2Hispanic:Area4	0.040 (0.405)	-0.290 (0.438)	-0.495 (0.480)
SDRace2Afr American:Area5	-0.595+ (0.336)	-0.463 (0.370)	-0.479 (0.409)
SDRace2Hispanic:Area5	0.082 (0.405)	-0.218 (0.442)	-0.563 (0.486)
SDRace2Afr American:SpecialAssignment2Violence Suppression	0.026 (0.497)	0.177 (0.516)	0.090 (0.544)
SDRace2Hispanic:SpecialAssignment2Violence Suppression	0.231 (0.723)	-0.451 (0.804)	-0.719 (0.833)
SDRace2Afr American:SpecialAssignment2Other	0.631 (0.525)	0.886 (0.556)	1.173+ (0.613)
SDRace2Hispanic:SpecialAssignment2Other	0.544 (0.671)	0.377 (0.720)	1.290+ (0.766)
Area2:SpecialAssignment2Violence Suppression	-12.557 (109.869)	-13.721 (168.899)	-15.065 (445.468)
Area3:SpecialAssignment2Violence Suppression	0.542 (0.725)	-0.435 (0.757)	-0.171 (0.869)
Area4:SpecialAssignment2Violence Suppression	-0.012 (0.889)	-0.381 (0.911)	0.425 (0.969)
Area5:SpecialAssignment2Violence Suppression	1.682* (0.696)	0.954 (0.758)	1.035 (0.844)
Area2:SpecialAssignment2Other	-1.386 (0.893)	-1.695+ (0.917)	-0.360 (0.975)
Area3:SpecialAssignment2Other	0.374 (0.656)	0.322 (0.700)	0.578 (0.806)
Area4:SpecialAssignment2Other	0.146 (0.757)	0.136 (0.801)	0.702 (0.898)
Area5:SpecialAssignment2Other	-0.640 (0.910)	-1.238 (1.186)	-0.431 (1.229)

	Search by Special Assignment		
	No Covariates	All Covariates	Fixed Effects
SDRace2Afr American:Area2:SpecialAssignment2Violence Suppression	12.329 (109.869)	13.030 (168.899)	15.220 (445.468)
SDRace2Hispanic:Area2:SpecialAssignment2Violence Suppression	12.398 (109.872)	13.922 (168.902)	16.346 (445.469)
SDRace2Afr American:Area3:SpecialAssignment2Violence Suppression	0.250 (0.766)	0.372 (0.802)	0.339 (0.914)
SDRace2Hispanic:Area3:SpecialAssignment2Violence Suppression	0.065 (0.959)	1.012 (1.045)	1.668 (1.147)
SDRace2Afr American:Area4:SpecialAssignment2Violence Suppression	1.258 (0.919)	1.167 (0.945)	0.483 (1.006)
SDRace2Hispanic:Area4:SpecialAssignment2Violence Suppression	0.385 (1.075)	1.141 (1.146)	0.543 (1.211)
SDRace2Afr American:Area5:SpecialAssignment2Violence Suppression	-0.335 (0.722)	-0.264 (0.787)	-0.524 (0.870)
SDRace2Hispanic:Area5:SpecialAssignment2Violence Suppression	-0.192 (0.905)	0.602 (1.014)	0.650 (1.092)
SDRace2Afr American:Area2:SpecialAssignment2Other	0.717 (0.937)	0.809 (0.968)	0.318 (1.033)
SDRace2Hispanic:Area2:SpecialAssignment2Other	1.288 (1.183)	1.529 (1.241)	0.611 (1.327)
SDRace2Afr American:Area3:SpecialAssignment2Other	-0.519 (0.694)	-0.776 (0.746)	-0.662 (0.859)
SDRace2Hispanic:Area3:SpecialAssignment2Other	-0.530 (0.838)	-0.311 (0.907)	-0.641 (1.006)
SDRace2Afr American:Area4:SpecialAssignment2Other	-0.554 (0.799)	-0.481 (0.849)	-0.529 (0.945)
SDRace2Hispanic:Area4:SpecialAssignment2Other	-0.584 (0.910)	-0.312 (0.976)	-1.165 (1.067)
SDRace2Afr American:Area5:SpecialAssignment2Other	0.825 (0.928)	1.186 (1.205)	0.882 (1.249)
SDRace2Hispanic:Area5:SpecialAssignment2Other	0.454 (1.034)	1.237 (1.305)	0.016 (1.357)
Officers dummy	No	No	Yes
Census dummy	No	No	Yes
Observations	19,607	19,353	19,353
Log Likelihood	-7,388.143	-5,750.469	-4,794.013
Akaike Inf. Crit.	14,866.290	11,654.940	10,912.030
Note:	+ p < .10; * p < .05; ** p < .01; *** p<0.001		

Appendix M: Arrest (No Moderator)

	Arrest (No Moderator)		
	No Covariates	All Covariates	Fixed Effects
Constant	-2.855*** (0.122)	-3.848*** (0.320)	-18.999 (1,906.010)
SDRace2Afr American	1.263*** (0.128)	0.615*** (0.148)	0.526*** (0.160)
SDRace2Asian	-1.049** (0.342)	-0.608+ (0.366)	-0.684+ (0.388)
SDRace2Hispanic	-0.093 (0.203)	0.002 (0.223)	-0.008 (0.241)
SDRace2Other	-0.245 (0.332)	0.197 (0.356)	0.181 (0.383)
Area2	-0.363+ (0.192)	-0.211 (0.219)	-0.541+ (0.285)
Area3	0.944*** (0.180)	0.341 (0.239)	0.209 (0.344)
Area4	1.153*** (0.190)	0.673** (0.245)	0.668+ (0.376)
Area5	1.080*** (0.212)	0.574* (0.266)	0.545 (0.395)
Sex_RecodedFemale		-0.400*** (0.053)	-0.362*** (0.057)
AgeGroupUnder 17		0.179+ (0.097)	0.138 (0.105)
AgeGroup30-39		-0.012 (0.053)	0.005 (0.057)
AgeGroupOver 40		-0.241*** (0.049)	-0.178*** (0.054)
ReasonForEncounterConsensual Encounter		0.951*** (0.098)	0.934*** (0.108)
ReasonForEncounterProbable Cause		2.206*** (0.058)	2.271*** (0.068)
ReasonForEncounterProbation/Parole		1.847*** (0.112)	1.594*** (0.121)
ReasonForEncounterReasonable Suspicion		1.309*** (0.074)	1.179*** (0.082)
EncounterTypeBicycle		0.514*** (0.104)	0.497*** (0.109)
EncounterTypeOther		0.475*** (0.111)	0.308* (0.127)
EncounterTypePedestrian		0.490*** (0.054)	0.463*** (0.059)
Contact_WeekOrWeekendFri-Sun		0.095* (0.042)	0.044 (0.046)
ContactTimeOfDay7pm-7am		-0.034 (0.043)	0.086+ (0.051)
Hispanic_percent		0.782** (0.241)	
BlackAloneNotHisp_percent		1.133*** (0.254)	
TotalPop24YrsorYounger_percent		-0.091 (0.464)	
Percent15yrsOrOlder_Divorced		-0.579 (0.859)	
TotalUnemployed_percent		-0.679 (0.503)	
Owneroccupiedhousingunits_percent		0.408+ (0.239)	
Incomein2013belowpovertylevel_percent		0.234 (0.307)	
RateViolentCrime_tract		-0.001** (0.0003)	

	Arrest (No Moderator)		
	No Covariates	All Covariates	Fixed Effects
RatePropertyCrime_tract		-0.0001 (0.00005)	
RateNarcoticsCrime_tract		0.002*** (0.0004)	
PopDensity		0.00000 (0.00000)	
BusinessCount_persqmile		0.00002 (0.00003)	
QOL_per10000		0.0001 (0.0001)	
OfficerAge_Dynamic		-0.001 (0.004)	
OfficerYearsExperience_Dynamic		-0.012* (0.005)	
OfficerGenderFemale		-0.405*** (0.081)	
OfficerRace_RecodedBlack		-0.176** (0.063)	
OfficerRace_RecodedHispanic		0.084 (0.051)	
OfficerRace_RecodedAsianFili		-0.084 (0.059)	
OfficerRace_RecodedUnknowUndecl		0.051 (0.114)	
SpecialAssignmentType_RecodedViolence Suppression		-0.077 (0.054)	-0.129* (0.066)
SpecialAssignmentType_RecodedProstitution		3.210*** (0.167)	3.115*** (0.189)
SpecialAssignmentType_RecodedNarcotics		0.737*** (0.115)	0.518*** (0.126)
SpecialAssignmentType_RecodedCruising		-0.184 (0.273)	-0.483 (0.301)
SpecialAssignmentType_RecodedSpecial Event		0.0001 (0.330)	-0.156 (0.391)
SpecialAssignmentType_RecodedOther		-0.287*** (0.066)	-0.246** (0.076)
SDRace2Afr American:Area2	0.026 (0.207)	0.110 (0.227)	-0.053 (0.251)
SDRace2Asian:Area2	0.304 (0.556)	-0.069 (0.618)	0.119 (0.658)
SDRace2Hispanic:Area2	0.485 (0.360)	0.183 (0.385)	0.125 (0.428)
SDRace2Other:Area2	-0.207 (0.623)	-0.674 (0.650)	-0.381 (0.685)
SDRace2Afr American:Area3	-0.818*** (0.192)	-0.323 (0.241)	-0.325 (0.265)
SDRace2Asian:Area3	1.178** (0.382)	1.019* (0.426)	1.108* (0.458)
SDRace2Hispanic:Area3	0.083 (0.263)	0.085 (0.311)	0.048 (0.339)
SDRace2Other:Area3	0.094 (0.429)	-0.092 (0.484)	-0.103 (0.526)
SDRace2Afr American:Area4	-1.046*** (0.200)	-0.445+ (0.237)	-0.401 (0.259)
SDRace2Asian:Area4	0.491 (0.443)	0.371 (0.503)	0.522 (0.541)
SDRace2Hispanic:Area4	-0.150 (0.261)	-0.083 (0.299)	-0.071 (0.324)
SDRace2Other:Area4	0.286 (0.428)	-0.102 (0.484)	-0.135 (0.523)
SDRace2Afr American:Area5	-0.824*** (0.218)	-0.546* (0.248)	-0.579* (0.266)

	Arrest (No Moderator)		
	No Covariates	All Covariates	Fixed Effects
SDRace2Asian:Area5	1.043* (0.474)	0.723 (0.523)	0.671 (0.552)
SDRace2Hispanic:Area5	0.022 (0.278)	-0.026 (0.309)	-0.156 (0.331)
SDRace2Other:Area5	0.016 (0.455)	-0.448 (0.493)	-0.258 (0.521)
Officers dummy	No	No	Yes
Census dummy	No	No	Yes
Observations	28,119	27,749	27,749
Log Likelihood	-11,264.050	-8,436.059	-7,640.227
Akaike Inf. Crit.	22,578.090	17,000.120	16,606.450
Note:	+ p < .10; * p < .05; ** p < .01; *** p<0.001		

Table N: Arrest by Type of Encounter

	Arrest by Type of Encounter		
	No Covariates	All Covariates	Fixed Effects
Constant	-4.118*** (0.252)	-4.198*** (0.400)	-19.446 (2,022.512)
SDRace2Afr American	1.597*** (0.263)	1.074*** (0.276)	0.909** (0.285)
SDRace2Hispanic	-0.066 (0.406)	-0.203 (0.419)	-0.295 (0.436)
Area2	-0.070 (0.362)	-0.190 (0.379)	-0.342 (0.429)
Area3	1.441*** (0.329)	0.475 (0.391)	-0.037 (0.490)
Area4	1.749*** (0.336)	1.038** (0.384)	0.573 (0.497)
Area5	1.587*** (0.375)	0.982* (0.411)	0.628 (0.521)
Sex_RecodedFemale		-0.397*** (0.055)	-0.367*** (0.060)
AgeGroupUnder 17		0.209* (0.098)	0.170 (0.106)
AgeGroup30-39		-0.022 (0.055)	-0.003 (0.059)
AgeGroupOver 40		-0.229*** (0.050)	-0.165** (0.055)
ReasonForEncounterConsensual Encounter		0.955*** (0.101)	0.933*** (0.111)
ReasonForEncounterProbable Cause		2.209*** (0.061)	2.268*** (0.070)
ReasonForEncounterProbation/Parole		1.838*** (0.115)	1.590*** (0.125)
ReasonForEncounterReasonable Suspicion		1.330*** (0.076)	1.191*** (0.084)
EncounterTypeBicycle	1.534** (0.577)	0.683 (0.630)	0.671 (0.639)
EncounterTypeOther	2.939*** (0.625)	1.210 (0.810)	0.776 (0.942)
EncounterTypePedestrian	2.753*** (0.300)	1.218*** (0.323)	0.970** (0.340)
Contact_WeekOrWeekendFri-Sun		0.109* (0.043)	0.066 (0.048)
ContactTimeOfDay7pm-7am		0.005 (0.044)	0.104* (0.052)
Hispanic_percent		0.741** (0.252)	
BlackAloneNotHisp_percent		1.091*** (0.266)	
TotalPop24YrsorYounger_percent		-0.085 (0.475)	
Percent15yrsOrOlder_Divorced		-0.551 (0.884)	
TotalUnemployed_percent		-0.858+ (0.519)	
Owneroccupiedhousingunits_percent		0.366 (0.255)	
Incomein2013belowpovertylevel_percent		0.208 (0.317)	
RateViolentCrime_tract		-0.001* (0.0003)	
RatePropertyCrime_tract		-0.0001 (0.00005)	
RateNarcoticsCrime_tract		0.002*** (0.0004)	

	Arrest by Type of Encounter		
	No Covariates	All Covariates	Fixed Effects
PopDensity		-0.00000 (0.00001)	
BusinessCount_persqmile		0.00001 (0.00003)	
QOL_per10000		0.0001 (0.0001)	
OfficerAge_Dynamic		-0.0003 (0.004)	
OfficerYearsExperience_Dynamic		-0.013^{*} (0.006)	
OfficerGenderFemale		-0.426^{***} (0.085)	
OfficerRace_RecodedBlack		-0.152^{*} (0.065)	
OfficerRace_RecodedHispanic		0.083 (0.053)	
OfficerRace_RecodedAsianFili		-0.081 (0.061)	
OfficerRace_RecodedUnknowUndecl		0.054 (0.120)	
SpecialAssignmentType_RecodedViolence Suppression		-0.023 (0.055)	-0.094 (0.067)
SpecialAssignmentType_RecodedProstitution		3.199^{***} (0.173)	3.115^{***} (0.196)
SpecialAssignmentType_RecodedNarcotics		0.777^{***} (0.118)	0.543^{***} (0.129)
SpecialAssignmentType_RecodedCruising		-0.228 (0.285)	-0.563^{+} (0.314)
SpecialAssignmentType_RecodedSpecial Event		0.069 (0.337)	-0.014 (0.404)
SpecialAssignmentType_RecodedOther		-0.252^{***} (0.068)	-0.226^{**} (0.079)
SDRace2Afr American:Area2	-0.130 (0.388)	0.131 (0.402)	-0.254 (0.427)
SDRace2Hispanic:Area2	0.447 (0.661)	0.412 (0.675)	0.642 (0.701)
SDRace2Afr American:Area3	-0.912^{**} (0.346)	-0.414 (0.403)	-0.368 (0.431)
SDRace2Hispanic:Area3	0.206 (0.481)	0.480 (0.533)	0.594 (0.565)
SDRace2Afr American:Area4	-1.283^{***} (0.352)	-0.747^{+} (0.388)	-0.565 (0.410)
SDRace2Hispanic:Area4	-0.030 (0.475)	0.145 (0.508)	0.331 (0.535)
SDRace2Afr American:Area5	-1.018^{**} (0.386)	-0.835^{*} (0.408)	-0.862^{*} (0.425)
SDRace2Hispanic:Area5	0.257 (0.503)	0.226 (0.524)	0.144 (0.548)
SDRace2Afr American:EncounterTypeBicycle	-0.875 (0.608)	-0.257 (0.665)	-0.225 (0.675)
SDRace2Hispanic:EncounterTypeBicycle	-0.295 (1.219)	-0.267 (1.276)	-0.346 (1.298)
SDRace2Afr American:EncounterTypeOther	-1.606^{*} (0.684)	-0.859 (0.864)	-0.592 (0.993)
SDRace2Hispanic:EncounterTypeOther	-13.322 (441.372)	-12.934 (489.549)	-16.794 (3,359.867)
SDRace2Afr American:EncounterTypePedestrian	-0.943^{**} (0.314)	-0.732^{*} (0.336)	-0.580 (0.353)
SDRace2Hispanic:EncounterTypePedestrian	0.379 (0.488)	0.474 (0.511)	0.640 (0.542)
Area2:EncounterTypeBicycle	-11.912 (149.212)	-11.295 (139.677)	-15.251 (999.013)

	Arrest by Type of Encounter		
	No Covariates	All Covariates	Fixed Effects
Area3:EncounterTypeBicycle	-1.853 (1.195)	-0.777 (1.289)	-0.804 (1.362)
Area4:EncounterTypeBicycle	-1.111 (1.235)	0.346 (1.273)	0.212 (1.316)
Area5:EncounterTypeBicycle	-0.612 (1.269)	-0.472 (1.389)	-0.346 (1.372)
Area2:EncounterTypeOther	-13.318 (441.372)	-12.375 (422.062)	-16.824 (2,636.713)
Area3:EncounterTypeOther	-0.822 (0.910)	0.212 (1.172)	-0.190 (1.337)
Area4:EncounterTypeOther	-0.571 (0.970)	0.589 (1.132)	1.088 (1.300)
Area5:EncounterTypeOther	-1.101 (0.918)	-0.564 (1.084)	-0.094 (1.206)
Area2:EncounterTypePedestrian	-0.090 (0.443)	0.057 (0.461)	-0.246 (0.500)
Area3:EncounterTypePedestrian	-0.636 (0.422)	-0.108 (0.496)	0.265 (0.543)
Area4:EncounterTypePedestrian	-1.128^{**} (0.437)	-0.737 (0.490)	-0.320 (0.529)
Area5:EncounterTypePedestrian	-0.940^{+} (0.492)	-0.680 (0.525)	-0.583 (0.559)
SDRace2Afr American:Area2:EncounterTypeBicycle	11.816 (149.213)	11.227 (139.677)	14.989 (999.013)
SDRace2Hispanic:Area2:EncounterTypeBicycle	12.688 (149.219)	12.597 (139.684)	15.783 (999.014)
SDRace2Afr American:Area3:EncounterTypeBicycle	0.721 (1.319)	0.309 (1.420)	0.449 (1.505)
SDRace2Hispanic:Area3:EncounterTypeBicycle	-11.416 (188.208)	-11.793 (174.908)	-15.276 (1,259.162)
SDRace2Afr American:Area4:EncounterTypeBicycle	1.621 (1.273)	0.733 (1.317)	1.051 (1.362)
SDRace2Hispanic:Area4:EncounterTypeBicycle	0.068 (1.749)	-0.657 (1.811)	-0.317 (1.865)
SDRace2Afr American:Area5:EncounterTypeBicycle	0.459 (1.296)	0.372 (1.419)	0.153 (1.404)
SDRace2Hispanic:Area5:EncounterTypeBicycle	0.355 (1.707)	0.576 (1.831)	0.418 (1.834)
SDRace2Afr American:Area2:EncounterTypeOther	13.810 (441.372)	12.469 (422.062)	17.258 (2,636.714)
SDRace2Hispanic:Area2:EncounterTypeOther	28.200 (624.196)	27.019 (646.371)	35.005 (4,270.944)
SDRace2Afr American:Area3:EncounterTypeOther	0.564 (1.000)	-0.390 (1.268)	-0.203 (1.437)
SDRace2Hispanic:Area3:EncounterTypeOther	12.796 (441.373)	12.314 (489.550)	16.927 (3,359.867)
SDRace2Afr American:Area4:EncounterTypeOther	0.947 (1.060)	-0.481 (1.230)	-0.925 (1.399)
SDRace2Hispanic:Area4:EncounterTypeOther	13.217 (441.373)	12.263 (489.550)	15.851 (3,359.867)
SDRace2Afr American:Area5:EncounterTypeOther	0.956 (0.977)	0.391 (1.143)	0.008 (1.263)
SDRace2Hispanic:Area5:EncounterTypeOther	12.437 (441.373)	12.085 (489.550)	15.827 (3,359.867)
SDRace2Afr American:Area2:EncounterTypePedestrian	-0.091 (0.475)	-0.172 (0.498)	0.216 (0.536)
SDRace2Hispanic:Area2:EncounterTypePedestrian	-0.755 (0.850)	-0.951 (0.876)	-1.439 (0.935)
SDRace2Afr American:Area3:EncounterTypePedestrian	0.215 (0.447)	0.048 (0.525)	-0.128 (0.570)
SDRace2Hispanic:Area3:EncounterTypePedestrian	-0.670 (0.612)	-0.760 (0.684)	-1.121 (0.740)

	Arrest by Type of Encounter		
	No Covariates	All Covariates	Fixed Effects
SDRace2Afr American:Area4:EncounterTypePedestrian	0.809^{+} (0.460)	0.476 (0.515)	0.195 (0.554)
SDRace2Hispanic:Area4:EncounterTypePedestrian	-0.136 (0.608)	-0.322 (0.660)	-0.638 (0.709)
SDRace2Afr American:Area5:EncounterTypePedestrian	0.493 (0.506)	0.468 (0.541)	0.531 (0.575)
SDRace2Hispanic:Area5:EncounterTypePedestrian	-0.396 (0.649)	-0.444 (0.686)	-0.469 (0.729)
Officers dummy	No	No	Yes
Census dummy	No	No	Yes
Observations	25,412	25,079	25,079
Log Likelihood	-9,550.626	-7,895.874	-7,156.933
Akaike Inf. Crit.	19,221.250	15,983.750	15,695.870

Note: + p < .10; * p < .05; ** p < .01; *** p<0.001

Appendix O: Arrest by Reason for Encounter

	Arrest by Reason for Encounter		
	No Covariates	All Covariates	Fixed Effects
Constant	-4.248*** (0.260)	-4.163*** (0.399)	-19.384 (2,110.366)
SDRace2Afr American	1.194*** (0.276)	0.899** (0.279)	0.799** (0.286)
SDRace2Hispanic	-0.155 (0.441)	-0.224 (0.444)	-0.293 (0.456)
Area2	-0.821+ (0.485)	-0.792 (0.492)	-1.003+ (0.524)
Area3	-0.173 (0.566)	-0.194 (0.577)	-0.531 (0.645)
Area4	0.642 (0.489)	0.405 (0.501)	0.130 (0.594)
Area5	0.934+ (0.490)	0.553 (0.505)	0.301 (0.596)
Sex_RecodedFemale		-0.404*** (0.055)	-0.367*** (0.060)
AgeGroupUnder 17		0.203* (0.099)	0.163 (0.106)
AgeGroup30-39		-0.027 (0.055)	-0.009 (0.059)
AgeGroupOver 40		-0.229*** (0.050)	-0.164** (0.055)
ReasonForEncounterConsensual Encounter	3.150*** (0.484)	2.827*** (0.504)	3.089*** (0.568)
ReasonForEncounterProbable Cause	3.007*** (0.320)	2.384*** (0.335)	2.316*** (0.357)
ReasonForEncounterProbation/Parole	3.332*** (0.876)	2.282* (1.041)	1.428 (1.075)
ReasonForEncounterReasonable Suspicion	3.188*** (0.466)	2.401*** (0.486)	2.241*** (0.513)
EncounterTypeBicycle		0.488*** (0.106)	0.487*** (0.111)
EncounterTypeOther		0.388** (0.119)	0.242+ (0.136)
EncounterTypePedestrian		0.470*** (0.055)	0.471*** (0.061)
Contact_WeekOrWeekendFri-Sun		0.109* (0.043)	0.063 (0.048)
ContactTimeOfDay7pm-7am		-0.003 (0.044)	0.101+ (0.053)
Hispanic_percent		0.753** (0.252)	
BlackAloneNotHisp_percent		1.102*** (0.265)	
TotalPop24YrsorYounger_percent		-0.096 (0.477)	
Percent15yrsOrOlder_Divorced		-0.559 (0.889)	
TotalUnemployed_percent		-0.918+ (0.520)	
Owneroccupiedhousingunits_percent		0.455+ (0.256)	
Incomein2013belowpovertylevel_percent		0.342 (0.318)	
RateViolentCrime_tract		-0.001* (0.0003)	
RatePropertyCrime_tract		-0.0001 (0.00005)	
RateNarcoticsCrime_tract		0.002*** (0.0004)	

	Arrest by Reason for Encounter		
	No Covariates	All Covariates	Fixed Effects
PopDensity		-0.00000 (0.00001)	
BusinessCount_persqmile		0.00002 (0.00003)	
QOL_per10000		0.00005 (0.0001)	
OfficerAge_Dynamic		0.001 (0.004)	
OfficerYearsExperience_Dynamic		-0.013* (0.006)	
OfficerGenderFemale		-0.431*** (0.085)	
OfficerRace_RecodedBlack		-0.149* (0.065)	
OfficerRace_RecodedHispanic		0.079 (0.053)	
OfficerRace_RecodedAsianFili		-0.059 (0.061)	
OfficerRace_RecodedUnknowUndecl		0.086 (0.120)	
SpecialAssignmentType_RecodedViolence Suppression		-0.015 (0.055)	-0.084 (0.068)
SpecialAssignmentType_RecodedProstitution		3.177*** (0.175)	3.079*** (0.198)
SpecialAssignmentType_RecodedNarcotics		0.748*** (0.119)	0.513*** (0.130)
SpecialAssignmentType_RecodedCruising		-0.227 (0.285)	-0.540^{+} (0.314)
SpecialAssignmentType_RecodedSpecial Event		0.049 (0.341)	0.003 (0.409)
SpecialAssignmentType_RecodedOther		-0.250*** (0.069)	-0.218** (0.079)
SDRace2Afr American:Area2	0.734 (0.513)	0.868^{+} (0.516)	0.498 (0.525)
SDRace2Hispanic:Area2	1.507* (0.753)	1.420^{+} (0.758)	1.407^{+} (0.770)
SDRace2Afr American:Area3	0.178 (0.590)	0.369 (0.596)	0.293 (0.611)
SDRace2Hispanic:Area3	1.425* (0.700)	1.359^{+} (0.709)	1.258^{+} (0.727)
SDRace2Afr American:Area4	-0.153 (0.506)	-0.016 (0.509)	0.063 (0.523)
SDRace2Hispanic:Area4	0.572 (0.623)	0.588 (0.627)	0.718 (0.643)
SDRace2Afr American:Area5	-0.423 (0.504)	-0.310 (0.507)	-0.404 (0.517)
SDRace2Hispanic:Area5	0.598 (0.621)	0.508 (0.625)	0.456 (0.639)
SDRace2Afr American:ReasonForEncounterConsensual Encounter	-1.476** (0.521)	-1.553** (0.540)	-1.795** (0.601)
SDRace2Hispanic:ReasonForEncounterConsensual Encounter	-0.420 (0.870)	-0.637 (0.892)	-1.042 (0.960)
SDRace2Afr American:ReasonForEncounterProbable Cause	-0.366 (0.339)	-0.157 (0.353)	-0.076 (0.372)
SDRace2Hispanic:ReasonForEncounterProbable Cause	0.283 (0.535)	0.541 (0.549)	0.683 (0.578)
SDRace2Afr American:ReasonForEncounterProbation/Parole	-0.930 (0.904)	-0.463 (1.065)	0.046 (1.100)
SDRace2Hispanic:ReasonForEncounterProbation/Parole	-12.495 (378.594)	-12.559 (378.594)	-16.813 (4,612.202)
SDRace2Afr American:ReasonForEncounterReasonable Suspicion	-1.170* (0.487)	-0.844^{+} (0.505)	-0.950^{+} (0.529)

	Arrest by Reason for Encounter		
	No Covariates	All Covariates	Fixed Effects
SDRace2Hispanic:ReasonForEncounterReasonable Suspicion	0.012 (0.749)	0.173 (0.766)	0.294 (0.813)
Area2:ReasonForEncounterConsensual Encounter	-0.383 (0.976)	-0.460 (0.990)	-0.931 (1.058)
Area3:ReasonForEncounterConsensual Encounter	-12.294 (119.724)	-12.182 (118.932)	-17.050 (1,339.600)
Area4:ReasonForEncounterConsensual Encounter	-1.202 (0.839)	-1.164 (0.853)	-1.524+ (0.918)
Area5:ReasonForEncounterConsensual Encounter	-2.831* (1.207)	-2.909* (1.217)	-3.315** (1.265)
Area2:ReasonForEncounterProbable Cause	0.283 (0.560)	0.734 (0.570)	0.715 (0.602)
Area3:ReasonForEncounterProbable Cause	1.414* (0.626)	0.975 (0.651)	1.008 (0.684)
Area4:ReasonForEncounterProbable Cause	0.686 (0.576)	0.684 (0.597)	0.821 (0.632)
Area5:ReasonForEncounterProbable Cause	0.467 (0.596)	0.741 (0.613)	0.833 (0.643)
Area2:ReasonForEncounterProbation/Parole	2.654* (1.279)	3.201* (1.403)	3.701* (1.490)
Area3:ReasonForEncounterProbation/Parole	1.090 (1.421)	1.651 (1.549)	1.891 (1.591)
Area4:ReasonForEncounterProbation/Parole	-0.131 (1.331)	0.486 (1.453)	1.301 (1.537)
Area5:ReasonForEncounterProbation/Parole	-0.711 (1.562)	0.086 (1.691)	0.516 (1.713)
Area2:ReasonForEncounterReasonable Suspicion	0.217 (0.745)	0.527 (0.759)	0.155 (0.800)
Area3:ReasonForEncounterReasonable Suspicion	-0.558 (0.839)	0.004 (0.854)	0.284 (0.903)
Area4:ReasonForEncounterReasonable Suspicion	-0.869 (0.740)	-0.396 (0.774)	-0.257 (0.832)
Area5:ReasonForEncounterReasonable Suspicion	-1.483+ (0.831)	-1.201 (0.844)	-1.100 (0.887)
SDRace2Afr American:Area2:ReasonForEncounterConsensual Encounter	-0.521 (1.079)	-0.365 (1.094)	0.131 (1.166)
SDRace2Hispanic:Area2:ReasonForEncounterConsensual Encounter	-12.195 (218.585)	-11.745 (217.438)	-16.540 (2,523.204)
SDRace2Afr American:Area3:ReasonForEncounterConsensual Encounter	11.651 (119.724)	11.640 (118.933)	16.338 (1,339.600)
SDRace2Hispanic:Area3:ReasonForEncounterConsensual Encounter	11.330 (119.727)	11.455 (118.936)	16.241 (1,339.600)
SDRace2Afr American:Area4:ReasonForEncounterConsensual Encounter	0.529 (0.904)	0.520 (0.921)	0.869 (0.985)
SDRace2Hispanic:Area4:ReasonForEncounterConsensual Encounter	0.186 (1.184)	0.033 (1.211)	0.401 (1.286)
SDRace2Afr American:Area5:ReasonForEncounterConsensual Encounter	2.140+ (1.234)	2.343+ (1.245)	2.778* (1.291)
SDRace2Hispanic:Area5:ReasonForEncounterConsensual Encounter	1.110 (1.458)	1.367 (1.475)	1.937 (1.537)
SDRace2Afr American:Area2:ReasonForEncounterProbable Cause	-0.768 (0.596)	-1.118+ (0.607)	-0.968 (0.639)
SDRace2Hispanic:Area2:ReasonForEncounterProbable Cause	-1.396 (0.915)	-1.640+ (0.929)	-1.801+ (0.993)
SDRace2Afr American:Area3:ReasonForEncounterProbable Cause	-0.962 (0.656)	-1.057 (0.682)	-0.955 (0.715)
SDRace2Hispanic:Area3:ReasonForEncounterProbable Cause	-2.222** (0.803)	-2.176** (0.832)	-2.128* (0.873)
SDRace2Afr American:Area4:ReasonForEncounterProbable Cause	-0.747 (0.601)	-0.819 (0.622)	-0.969 (0.655)
SDRace2Hispanic:Area4:ReasonForEncounterProbable Cause	-0.796 (0.746)	-1.026 (0.768)	-1.280 (0.810)

	Arrest by Reason for Encounter		
	No Covariates	All Covariates	Fixed Effects
SDRace2Afr American:Area5:ReasonForEncounterProbable Cause	-0.707 (0.614)	-0.900 (0.630)	-0.917 (0.659)
SDRace2Hispanic:Area5:ReasonForEncounterProbable Cause	-0.887 (0.761)	-1.183 (0.778)	-1.425+ (0.815)
SDRace2Afr American:Area2:ReasonForEncounterProbation/Parole	-1.389 (1.352)	-1.770 (1.471)	-2.173 (1.563)
SDRace2Hispanic:Area2:ReasonForEncounterProbation/Parole	10.227 (378.598)	10.402 (378.599)	14.697 (4,612.202)
SDRace2Afr American:Area3:ReasonForEncounterProbation/Parole	-1.408 (1.504)	-1.844 (1.626)	-1.940 (1.670)
SDRace2Hispanic:Area3:ReasonForEncounterProbation/Parole	9.925 (378.596)	10.027 (378.597)	14.696 (4,612.202)
SDRace2Afr American:Area4:ReasonForEncounterProbation/Parole	-0.400 (1.390)	-0.927 (1.509)	-1.788 (1.598)
SDRace2Hispanic:Area4:ReasonForEncounterProbation/Parole	11.231 (378.596)	11.249 (378.596)	15.447 (4,612.202)
SDRace2Afr American:Area5:ReasonForEncounterProbation/Parole	0.122 (1.588)	-0.402 (1.717)	-0.599 (1.740)
SDRace2Hispanic:Area5:ReasonForEncounterProbation/Parole	11.135 (378.597)	11.168 (378.597)	15.622 (4,612.202)
SDRace2Afr American:Area2:ReasonForEncounterReasonable Suspicion	-0.236 (0.790)	-0.637 (0.807)	-0.162 (0.848)
SDRace2Hispanic:Area2:ReasonForEncounterReasonable Suspicion	-2.407 (1.473)	-2.204 (1.487)	-2.382 (1.554)
SDRace2Afr American:Area3:ReasonForEncounterReasonable Suspicion	0.040 (0.876)	-0.483 (0.893)	-0.559 (0.942)
SDRace2Hispanic:Area3:ReasonForEncounterReasonable Suspicion	-0.640 (1.077)	-1.018 (1.097)	-0.962 (1.165)
SDRace2Afr American:Area4:ReasonForEncounterReasonable Suspicion	0.459 (0.772)	0.008 (0.807)	-0.005 (0.862)
SDRace2Hispanic:Area4:ReasonForEncounterReasonable Suspicion	-0.340 (0.987)	-0.696 (1.019)	-1.038 (1.092)
SDRace2Afr American:Area5:ReasonForEncounterReasonable Suspicion	0.789 (0.851)	0.586 (0.865)	0.691 (0.907)
SDRace2Hispanic:Area5:ReasonForEncounterReasonable Suspicion	-0.129 (1.053)	-0.077 (1.069)	-0.296 (1.126)
Officers dummy	No	No	Yes
Census dummy	No	No	Yes
Observations	25,412	25,079	25,079
Log Likelihood	-8,455.308	-7,869.313	-7,135.009
Akaike Inf. Crit.	17,060.620	15,958.630	15,680.020

Note: + p < .10; * p < .05; ** p < .01; *** p<0.001

Table P: Arrest by Special Assignment

	Arrest by Special Assignment:		
	No Covariates	All Covariates	Fixed Effects
Constant	-2.294*** (0.156)	-3.489*** (0.331)	-19.131 (2,152.969)
SDRace2Afr American	0.891*** (0.164)	0.266 (0.181)	0.182 (0.200)
SDRace2Hispanic	-0.285 (0.251)	-0.230 (0.273)	-0.174 (0.294)
Area2	-0.068 (0.234)	0.119 (0.263)	-0.642+ (0.336)
Area3	-0.195 (0.280)	-0.036 (0.312)	-0.464 (0.416)
Area4	0.637** (0.246)	0.516+ (0.286)	0.195 (0.417)
Area5	0.200 (0.301)	-0.020 (0.349)	-0.336 (0.469)
Sex_RecodedFemale		-0.187*** (0.051)	-0.189*** (0.056)
AgeGroupUnder 17		0.201* (0.096)	0.149 (0.104)
AgeGroup30-39		-0.032 (0.054)	-0.011 (0.058)
AgeGroupOver 40		-0.282*** (0.049)	-0.215*** (0.055)
ReasonForEncounterConsensual Encounter		1.052*** (0.101)	1.028*** (0.110)
ReasonForEncounterProbable Cause		2.456*** (0.059)	2.482*** (0.068)
ReasonForEncounterProbation/Parole		1.920*** (0.115)	1.658*** (0.124)
ReasonForEncounterReasonable Suspicion		1.443*** (0.075)	1.302*** (0.083)
EncounterTypeBicycle		0.493*** (0.106)	0.466*** (0.111)
EncounterTypeOther		0.282* (0.116)	0.121 (0.132)
EncounterTypePedestrian		0.405*** (0.053)	0.399*** (0.059)
Contact_WeekOrWeekendFri-Sun		0.023 (0.043)	-0.003 (0.047)
ContactTimeOfDay7pm-7am		-0.048 (0.043)	0.077 (0.052)
Hispanic_percent		0.676** (0.241)	
BlackAloneNotHisp_percent		0.998*** (0.256)	
TotalPop24YrsorYounger_percent		-0.117 (0.470)	
Percent15yrsOrOlder_Divorced		-0.811 (0.866)	
TotalUnemployed_percent		-0.826 (0.508)	
Owneroccupiedhousingunits_percent		0.336 (0.245)	
Incomein2013belowpovertylevel_percent		0.388 (0.312)	
RateViolentCrime_tract		-0.001+ (0.0003)	
RatePropertyCrime_tract		-0.0001+ (0.00005)	
RateNarcoticsCrime_tract		0.002*** (0.0004)	

	Arrest by Special Assignment:		
	No Covariates	All Covariates	Fixed Effects
PopDensity		-0.00000 (0.00001)	
BusinessCount_persqmile		0.00001 (0.00003)	
QOL_per10000		0.0001^{+} (0.0001)	
OfficerAge_Dynamic		-0.002 (0.004)	
OfficerYearsExperience_Dynamic		-0.013^{*} (0.005)	
OfficerGenderFemale		-0.353^{***} (0.081)	
OfficerRace_RecodedBlack		-0.167^{**} (0.063)	
OfficerRace_RecodedHispanic		0.082 (0.052)	
OfficerRace_RecodedAsianFili		-0.135^{*} (0.061)	
OfficerRace_RecodedUnknowUndecl		-0.011 (0.118)	
SpecialAssignment2Violence Suppression	-2.525^{***} (0.526)	-1.681^{**} (0.540)	-1.622^{**} (0.558)
SpecialAssignment2Other	-0.292 (0.271)	-0.094 (0.305)	-0.342 (0.331)
SDRace2Afr American:Area2	-0.071 (0.254)	0.112 (0.279)	0.032 (0.313)
SDRace2Hispanic:Area2	0.769^{+} (0.427)	0.637 (0.472)	0.389 (0.528)
SDRace2Afr American:Area3	-0.205 (0.296)	0.146 (0.321)	0.162 (0.352)
SDRace2Hispanic:Area3	1.052^{**} (0.366)	0.774^{+} (0.398)	0.638 (0.432)
SDRace2Afr American:Area4	-0.750^{**} (0.259)	-0.222 (0.286)	-0.188 (0.311)
SDRace2Hispanic:Area4	0.163 (0.330)	0.162 (0.362)	0.050 (0.389)
SDRace2Afr American:Area5	-0.250 (0.308)	-0.002 (0.338)	0.040 (0.362)
SDRace2Hispanic:Area5	0.473 (0.374)	0.458 (0.408)	0.309 (0.435)
SDRace2Afr American:SpecialAssignment2Violence Suppression	1.656^{**} (0.538)	1.726^{**} (0.553)	1.543^{**} (0.571)
SDRace2Hispanic:SpecialAssignment2Violence Suppression	1.572^{*} (0.697)	1.337^{+} (0.727)	1.049 (0.766)
SDRace2Afr American:SpecialAssignment2Other	0.205 (0.291)	0.409 (0.328)	0.476 (0.354)
SDRace2Hispanic:SpecialAssignment2Other	-0.574 (0.533)	-0.209 (0.576)	-0.075 (0.625)
Area2:SpecialAssignment2Violence Suppression	0.498 (0.690)	0.167 (0.710)	0.547 (0.751)
Area3:SpecialAssignment2Violence Suppression	1.782^{*} (0.823)	0.778 (0.852)	0.683 (0.893)
Area4:SpecialAssignment2Violence Suppression	1.490^{+} (0.761)	1.174 (0.802)	1.630^{+} (0.861)
Area5:SpecialAssignment2Violence Suppression	3.554^{***} (0.691)	2.867^{***} (0.743)	2.594^{***} (0.784)
Area2:SpecialAssignment2Other	-1.470^{**} (0.554)	-1.707^{**} (0.582)	-0.468 (0.629)
Area3:SpecialAssignment2Other	1.715^{***} (0.399)	1.618^{***} (0.449)	1.839^{***} (0.511)
Area4:SpecialAssignment2Other	0.576 (0.419)	0.494 (0.472)	0.629 (0.520)

	Arrest by Special Assignment:		
	No Covariates	All Covariates	Fixed Effects
Area5:SpecialAssignment2Other	0.728 (0.488)	0.586 (0.545)	0.961 (0.592)
SDRace2Afr American:Area2:SpecialAssignment2Violence Suppression	-0.633 (0.717)	-0.908 (0.742)	-0.488 (0.781)
SDRace2Hispanic:Area2:SpecialAssignment2Violence Suppression	-1.279 (1.057)	-1.287 (1.108)	-0.636 (1.178)
SDRace2Afr American:Area3:SpecialAssignment2Violence Suppression	-1.270 (0.849)	-1.058 (0.882)	-0.925 (0.920)
SDRace2Hispanic:Area3:SpecialAssignment2Violence Suppression	-2.297* (1.026)	-1.846+ (1.070)	-1.599 (1.127)
SDRace2Afr American:Area4:SpecialAssignment2Violence Suppression	-0.845 (0.782)	-1.100 (0.826)	-1.432 (0.881)
SDRace2Hispanic:Area4:SpecialAssignment2Violence Suppression	-1.602+ (0.928)	-1.439 (0.981)	-1.409 (1.052)
SDRace2Afr American:Area5:SpecialAssignment2Violence Suppression	-2.389*** (0.705)	-2.425** (0.759)	-2.371** (0.799)
SDRace2Hispanic:Area5:SpecialAssignment2Violence Suppression	-2.411** (0.852)	-2.207* (0.915)	-1.922* (0.971)
SDRace2Afr American:Area2:SpecialAssignment2Other	1.061+ (0.592)	1.009 (0.625)	0.306 (0.676)
SDRace2Hispanic:Area2:SpecialAssignment2Other	-10.352 (102.620)	-10.566 (92.638)	-14.469 (650.840)
SDRace2Afr American:Area3:SpecialAssignment2Other	-0.488 (0.429)	-0.885+ (0.485)	-0.929+ (0.547)
SDRace2Hispanic:Area3:SpecialAssignment2Other	-0.888 (0.646)	-1.242+ (0.708)	-1.225 (0.784)
SDRace2Afr American:Area4:SpecialAssignment2Other	-0.117 (0.450)	-0.251 (0.509)	-0.316 (0.557)
SDRace2Hispanic:Area4:SpecialAssignment2Other	0.259 (0.646)	0.061 (0.707)	-0.091 (0.771)
SDRace2Afr American:Area5:SpecialAssignment2Other	-0.363 (0.508)	-0.564 (0.568)	-0.783 (0.615)
SDRace2Hispanic:Area5:SpecialAssignment2Other	0.236 (0.696)	-0.163 (0.762)	-0.438 (0.827)
Officers dummy	No	No	Yes
Census dummy	No	No	Yes
Observations	25,412	25,079	25,079
Log Likelihood	-10,320.730	-8,120.578	-7,332.482
Akaike Inf. Crit.	20,731.460	16,397.150	16,010.970
Note:	+ p < .10; * p < .05; ** p < .01; *** p<0.001		