A STUDY OF RACIALLY DISPARATE OUTCOMES IN THE LOS ANGELES POLICE DEPARTMENT

Prepared for the **ACLU** of Southern California | **October 2008**

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Abstract: This report analyzes pedestrian and motor vehicle stops of the Los Angeles Police Department over a one-year period: July 2003 to June 2004. We find prima facie evidence that African Americans and Hispanics are over-stopped, over-frisked, over-searched, and over-arrested. After controlling for violent and property crime rates in specific LAPD reporting districts, as well as a range of other variables, we find that:

Per 10,000 residents, the black stop rate is 3,400 stops higher than the white stop rate, and the Hispanic stop rate is almost 360 stops higher.

Relative to stopped whites, stopped blacks are 127% more likely and stopped Hispanics are 43% more likely to be frisked.

Relative to stopped whites, stopped blacks are 76% more likely and stopped Hispanics are 16% more likely to be searched.

Relative to stopped whites, stopped blacks are 29% more likely and stopped Hispanics are 32% more likely to be arrested.

All of these disparities are statistically significant ($p < .01$). The findings of racial disparity are supported by ancillary analyses of investigative outcomes and officer race. We find that frisks and searches are systematically less productive when conducted on blacks and Hispanics than when conducted on whites:

Frisked African Americans are 42.3% less likely to be found with a weapon than frisked whites and that frisked Hispanics are 31.8% less likely to have a weapon than frisked non-Hispanic whites.

Consensual searches of blacks are 37.0% less likely to uncover weapons, 23.7% less likely to uncover drugs and 25.4% less likely to uncover anything else.

Consensual searches of Hispanics similarly are 32.8% less likely to uncover weapons, 34.3% less likely to uncover drugs and 12.3% less likely to uncover anything else.

It is implausible that higher frisk and search rates are justified by higher minority criminality, when these frisks and searches are substantially less likely to uncover weapons, drugs or other types of contraband. We also find that the black arrest disparity was 9 percentage points lower when the stopping officer was black than when the stopping officer was not black. Similarly, the Hispanic arrest disparity was 7 percentage points lower when the stopping officer was Hispanic than when the stopping officer was a non-Hispanic white. Taken as a whole, these results justify further investigation and corrective action.

TABLE OF CONTENTS

I.   Introduction

In July 2006, the Analysis Group released a report, "Pedestrian and Motor Vehicle Post-Stop Data Analysis Report" (hereafter "Analysis Report") that they had prepared for the City of Los Angeles.[1] This report analyzed more than 810,000 "field data reports" (FDRs) collected by the Los Angeles Police Department (LAPD) from July 1, 2003 through June 30, 2004.[2] FDRs are completed by LAPD officers at the time of a stop and must be completed whenever an officer stops a vehicle or pedestrian, with a small number of exemptions.[3] They record information on a number of outcomes—including: i) whether a pat-down, frisk or search was conducted; ii) whether contraband was uncovered; and iii) whether an arrest was made or a citation was issued.

The Analysis Report's authors tested whether minorities who were stopped were more likely than whites to be frisked, searched, cited or arrested. The Analysis Report concluded:

> Although some divisions/bureaus have statistically significant racial disparities for some outcomes and some races, when evaluated across all outcomes, *there is no consistent pattern of race effects across divisions or races*.[4]

The report was limited to what it called "Post-Stop" analysis in that it did not investigate whether minorities were disproportionately subjected to being stopped. Instead, the Analysis Report asked whether—contingent on a stop having occurred—minorities were more likely than whites to be subject to certain outcomes.

The ACLU of Southern California provided us with the data used in the Analysis Report and asked us to assess its validity.[5] In this report, we undertake to answer three central questions:

1. Are African Americans and Hispanics more likely than whites to be stopped?
2. Are African Americans and Hispanics who are stopped more likely than whites to be frisked, searched, cited or arrested?

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[1] A copy of the Analysis Report is available online. ANALYSIS GROUP, INC., PEDESTRIAN AND MOTOR VEHICLE POST-STOP DATA ANALYSIS REPORT (2006), http://www.analysisgroup.com/AnalysisGroup/uploadedFiles/Publishing/Articles/LAPD_Data_Analysis_Report_07-5-06.pdf (hereafter ANALYSIS REPORT).

[2] In 2001, the city of Los Angeles entered into a consent decree with the U.S. Department of Justice, under which the LAPD "embarked upon a project of systematically collecting data on pedestrian and motor vehicle stops in order to . . . [review] concerns and perceptions about potential racial profiling." *Id.* at 6.

[3] The exemptions include stops at checkpoints/roadblocks, commercial vehicle safety inspections, stops pursuant to an arrest or search warrant, stops of victims/witnesses, and stops involving calls for service relating to certain particularly dangerous crimes and situations.

[4] *Id.* at 4.

[5] The ACLU obtained the data through public records requests. The data was provided in SAS format but did not include any files containing the specific SAS commands or output files. The Analysis Group declined to provide these files. The data and statistical input and output files underlying this report can be found at http://www.yale.edu/ayres/.

1

3. Are African Americans and Hispanics who are searched more likely than whites to be found with contraband?

The three sections of this Report correspond to these questions.

In asking these questions, we seek to broaden the temporal range of the Analysis Report. We test for racial disparities at an earlier stage in the process. Instead of taking the stopping behavior of the LAPD as given, we ask whether, controlling for crime rates in particular areas, African Americans and Hispanics were more likely than whites to be stopped. We also test for racial disparities at a later stage in the process by analyzing whether frisks or searches conducted by the LAPD uncovered contraband, such as drugs or weapons. Evidence that searches of minorities were systematically less productive than searches of whites would suggest that police required less probable cause in searching minorities and subjected minorities to needless searches.[6]

A. Shortcomings of the Analysis Report

The data to conduct the broader analyses we undertake were available in the original dataset used to produce the Analysis Report. The Analysis Group's proposed methodology suggested that it was their intent to conduct some analogous tests.[7] But the final Analysis Report artificially restricted their attention to a limited set of racial disparities. In particular, they ignored tests of whether, controlling for crime rates, minorities were disproportionately subject to being stopped.

The approach of the Analysis Group in testing for racial disparities in "post-stop" outcomes is subject to four core criticisms:

1. **The Analysis Report failed to test whether, controlling for other factors, there was a statistically significant overall racial disparity across the LAPD as a whole.** Instead, the Report tested to see whether there were racial disparities within individual divisions. The Analysis Report found that there was not a consistent, statistically significant disparity disfavoring African Americans and Hispanics across different divisions. But the failure to find statistically significant outcomes in some divisions does not mean that there is not a statistically significant racial effect overall. Failing to test for an aggregate racial effect robs the test of statistical power, because it effectively reduces the sample size of the individual tests. There are eighteen divisions within the sample, and analyzing only the individual divisions rather than the whole thus reduces the sample approximately by a factor of eighteen. Using the same specification as the Analysis Report and controlling for the same variables, we show that, citywide,

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[6] *See* IAN AYRES, PERVASIVE PREJUDICE?: NON-TRADITIONAL EVIDENCE OF RACE AND GENDER DISCRIMINATION (2001).

[7] *See* ANALYSIS GROUP, INC., FINAL PEDESTRIAN AND MOTOR VEHICLE STOP DATA ANALYSES METHODOLOGY REPORT (2005),
http://www.lacity.org/cla/lapdstopreports/clalapdstopreports222134175_12082005.pdf.

stopped African Americans and Hispanics were more likely to be arrested than stopped whites and that this disparity was statistically significant.

2.   **The Analysis Group inappropriately limited their analysis to an unduly cramped definition of "racially biased policing."** The Report says that the Analysis Group was engaged "to determine whether [the data] provide evidence of racially biased policing" and a footnote explained that the "phrases *racial profiling by law enforcement* and *racially biased policing* are used interchangeably in this report."[8] On its face, it is not clear whether the Analysis Report was attempting to test for race-contingent decisions by police (what the law refers to as "disparate racial treatment") or whether the Report was attempting to test whether police policies produced unjustified disparate impacts. The term "racial profiling" usually denotes disparate racial treatment by police. But the term "racially biased policing" might denote policing policies that impose an unjustified burden on racial minorities—and hence resonate more with a "disparate impact" standard. The Analysis Report testing strategy inappropriately restricts its attention to the unduly narrow disparate treatment standard. For example, the Report's logistic regressions control for a host of officer characteristics—including the number of complaints that have been levied at the stopping officer. Controlling for officer complaints might make sense in a test of disparate racial treatment *by the officer*, because it would be appropriate to control for all non-race factors that might provide alternative (non-pretextual) explanations for a racial disparity in outcomes. But it would be inappropriate to control for officer complaints in a test of disparate racial impacts. Including controls for officer complaints might easily cause a regression to understate the true size of the unjustified racial impact. A policy of assigning officers with multiple complaints to predominantly-minority areas might produce an unjustified impact against minorities who are stopped. Including a control for officer complaints might inappropriately soak up some of the real racial disparity in the data.

3.   **The Analysis Report limits its attention to analyzing police behaviors that are conditional on stopping.** By focusing only on behaviors that occur after an officer makes a stop, it inappropriately ignores the causally prior question of whether there were unjustified racial disparities in who police decide to stop in the first place.

4.   **The Analysis Report failed to examine the outcome of searches and frisks.** It inappropriately ignores the whether the success rate of searches and frisks could provide evidence of whether in aggregate searching and frisk decisions were justified.

In our analysis, we address these shortcomings. Like the Analysis Report, our specification allows there to be division-specific racial disparities. But we then separately test whether across divisions there is a statistically significant department-wide disparity

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[8] ANALYSIS REPORT, *supra* note 1, at 3, 6 n.3.

disfavoring African Americans and Hispanics. Second, we avoid the problem of "included variable bias" by excluding variables such as officer complaints that do not offer a legitimate justification for explaining away disparate racial impacts. We show how excluding such inappropriate controls impacts the estimate of overall racial disparities. Finally, we avoid the third and fourth problems of only considering police behaviors that are conditional on stopping by widening the temporal frame to analyze stopping decisions and search outcome decisions.[9]

The Analysis Report points out that while the data contained in the FDRs provides a wealth of information about police practices, it does not contain direct evidence on "the rates at which different racial groups commit crimes."[10] These, and other, missing variables are said to limit the ability of regressions to produce iron-clad evidence of disparate treatment. A racial group might be exposed to more stops, searches, citations and arrests because members of the group disproportionately commit crimes. We note, however, that using an individual's race as a direct proxy for that individual's criminality is legally problematic under current prohibitions against racial profiling. The more direct missing data is not "the rates at which different racial groups commit crimes" but the rate at which individual suspects commit crimes (or display evidence of probable cause). Later in our analysis we will make use of indirect evidence in the data of the rate at which stopped individuals commit crimes, in that we have evidence of the rate at which contraband is actually found for individuals of different races frisked and searched by the LAPD. Our finding that the police have a lower likelihood of finding contraband when they search minorities than non-minorities is inconsistent with the thought that the searches and frisks are justified by higher minority criminality.

The Analysis Report catalogs a variety of other "omitted variables" that may be determinants of police behavior but which were not controlled for in the analysis:

> suspect attitude or demeanor;
> for motor vehicle suspects, vehicle condition . . .;
> differences in driving behavior;
> the presence of bystanders; and

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[9] In the post-stop and post-search regression portions of our analysis we exclude all stops by gang task-force officers. The Analysis Report separated out stops by gang officers because gang officers follow substantially different policing tactics. We chose to preserve this separation and furthermore not to reanalyze the stops by gang officers for three reasons. First, the Analysis treatment of gang officers was not as strongly subject to the aggregation criticism because gang officers were subdivided only into four areas rather than eighteen divisions in the Analysis Report. Second, issues of racial profiling are more complicated, both legally and logically, when dealing with gangs that may explicitly identify with particular racial groups. Third, the gang subsample was relatively very small, comprising less than 50,000 Field Data Reports. For these reasons, we decided to focus on the non-gang officer sample. We do not exclude stops by gang officers in the stop likelihood or stop-rate proportions of the analysis because of omitted problems created by excluding a particular subclass of stops when stops are the dependent variable of analysis.

[10] *Id.* at 18.

when there was a victim, whether the victim wanted the police to make an arrest.[11]

Some of these omitted variables also create the possibility of alternative, legitimate explanations for estimated racial disparities. For example, a racial group might be disproportionately searched if members of that group were "disproportionately antagonistic or disrespectful toward police." But some of these omitted variables do not raise as compelling an alternative, legitimate explanation for racial disparities. For example, imagine that victims disproportionately want the police to make an arrest when the perpetrator is of a particular race. A policy of the police to defer to victim arrest requests would provide an alternative explanation to the claim that the police are engaging in race-contingent behavior. But the police policy of delegating the arrest decision to self-reported victims might still have an illegitimate disparate impact against suspects of the disfavored racial group.

This report, in addition to correcting some of the failings of the original Analysis Report, will also use indirect benchmarks to assess whether African Americans and Hispanics are subjected to illegitimate police action. Holding the violent and property crime rates constant, we will test whether the likelihood of being stopped is higher in minority neighborhoods. Using officer race as a benchmark, we will test whether minority officers treat stopped suspects less harshly than non-minority officers that stopped suspects. And using the success rates on frisks and searches as a benchmark, we will test whether minorities have to endure systematically less productive incursions into their privacy.

B. Summary of Findings

Our main conclusions are the following:

Disparities in Stops

1. African Americans were much more likely to be stopped than non-minorities. In the single-year of data, there were more than 4,500 stops for every 10,000 African Americans residents but only 1,750 stops for every 10,000 non-minority residents. In two divisions (Central and Hollywood), there were more stops of African Americans in one year than there were African American residents, meaning that the average number of stops per resident was greater than one.[12] See Table 1.

2. This marked racial disparity in the likelihood of being stopped is not merely an artifact of different area crime rates. In regressions controlling for both violent and property crime rates in more than 900 Reporting Districts (RDs), the stop rate per 10,000 residents was more than 3,400

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[11] *Id.*

[12] In one division (Central), there were more stops of Hispanics in one year than there were Hispanic residents.

stops higher for African Americans and more than 350 stops higher for Hispanics than for non-minorities. See Table 4.

3.   This disparity in the likelihood of being stopped is not driven by a policy of assigning more police to minority neighborhoods. Indeed, the racial disparity in stop rates was higher in non-minority neighborhoods than in minority neighborhoods. For example, regressions, controlling for the crime rate, estimated that the stop rate disparity for African Americans was 3,400 stops higher in RDs where blacks and Hispanics were less than one-third of the residents relative to RDs where blacks and Hispanics were more than two-thirds of the residents. See Figure 1.

Disparities in Rates of Arrests, Frisks, Searches, and Requests to Exit Vehicle

4.   African Americans and Hispanics were not only more likely to be stopped by the police, but they were also more likely to be arrested when stopped. Conditional on being stopped, regressions controlling for a host of other variables showed that city-wide, stopped African Americans were 29% more likely than stopped whites to be arrested and that stopped Hispanics were 32% more likely than stopped whites to be arrested. These results were statistically significant and robust to a variety of different specifications. Indeed, reanalyzing the specification used in the Analysis Report itself shows city-wide that stopped African Americans were 21% more likely and stopped Hispanics were 29% more likely to be arrested than stopped whites. See Table 8.

5.   The racial disparities in arrests persist if we exclude less discretionary arrests (concerning arrests for outstanding warrants, violent crimes and DUIs). With regard to more discretionary arrests, stopped African Americans were 13% more likely to be arrested and stopped Hispanics were 21.4% more likely to be arrested than stopped whites. See Table 17.

6.   Even larger racial disparities were estimated with regard to police investigative techniques.
   a. Stopped African Americans were 166% more likely and Hispanics were 132% more likely to be asked to exit vehicles than stopped whites. See Table 20.
   b. Stopped African Americans were 127% more likely and Hispanics were 43% more likely to be frisked or patted down than stopped whites. See Table 13.
   c. Stopped African Americans were 76% more likely and stopped Hispanics were more than 16% more likely to be asked to consent to being searched than stopped whites. See Table 14.
   These statistically significant racial disparities persisted when the data was restricted to stops where police action was more discretionary—for

example, by excluding stops with violent felony arrests where police had less discretion in deciding whether to search.

7.   If we restrict attention to people who were stopped, Hispanics were cited more often than whites—but the size of the citation disparity was less than 7% and was not robustly significant across alternative specifications. Stopped African Americans were more than 30% *less* likely to be cited than stopped whites. This disparity favoring stopped African Americans was statistically significant. While the conditional probability of being cited favored stopped African Americans relative to stopped whites, African Americans were so much more likely to be stopped that the unconditional probability that African Americans would be cited was substantially higher. Indeed, we find that the citations per 10,000 residents were 1,300 citations higher for African American residents and 140 citations higher for Hispanic residents than for white residents. See Tables 6 and 9.

New Benchmarks: Search Productivity and "Same Race" Stops

8.   All in all, the stops of African Americans were systematically less productive than the stops of whites. Stopped African Americans were 21% more likely to be stopped without being either cited or arrested. This "no-action" finding, together with the substantially higher stop likelihood of African Americans, suggests that police required less justification to stop African Americans than to stop whites. Table 10.

9.   There is some evidence that stops by minority officers produced smaller racial disparities than stops by non-minority officers. The disparities for stopped African Americans were often smaller when the stops were conducted by African American officers. For example, the racial disparity in arrests disfavoring stopped African Americans fell from 27% with regard to non-minority officers to 18% when an African American was the stopping officer. And African American officers were nearly 20% less likely than non-minority officers to force African Americans to participate in a "no action" stop. These officer race disparities were statistically significant.   The relatively better treatment that stopped African Americans experienced when stopped by African American officers versus non-minority officers raises concerns of racially biased policing. See Tables 11 and 12.

10.   Finally, searches and frisks conducted upon stopped African Americans and Hispanics were systematically less productive in producing weapons, drugs or other contraband than those conducted upon whites:
        a. Searched African Americans were 37% less likely than searched whites to be found with weapons, 24% less likely to be found with

drugs, and 25% less likely to be found with other contraband. See Tables 26 through 28.

b. Searched Hispanics were 33% less likely than searched whites to be found with weapons, 34% less likely to be found with drugs, and 12% less likely to be found with other contraband. See Tables 26 through 28.

c. Frisked African Americans were 42% less likely than frisked whites to be found with weapons, 25% less likely to be found with drugs, and 33% less likely to be found with other contraband. See Tables 23 through 24.

d. Frisked Hispanics were 32% less likely than frisked whites to be found with weapons, 38% less likely to be found with drugs, and 15% less likely to be found with other contraband. See Tables 23 through 24.

These statistically significant racial disparities persisted when the data was restricted by excluding stops where police had less discretion in search. See Table 10.

These latter results suggest that African Americans and Hispanics are over-frisked and over-searched relative to whites. Once stopped, these minorities are: i) frisked and searched substantially more often than stopped whites; *and* ii) these actions have a substantially lower probability of uncovering weapons, guns or other contraband.

There is also some evidence that African Americans and Hispanics are over-stopped (although this result may be susceptible to alternative interpretations, given the absence of direct evidence on race-specific criminality). These minority groups are: i) disproportionately likely to be stopped; and ii) disproportionately likely to have the stop result in "no action."

A core question is whether the high propensity of stopped African Americans and Hispanics to be arrested is driven by higher probability of crime. In this regard, it is troubling that the racial disparities in arrest observed for stopped African Americans tend to be lower when the stopping officer is African American than when the stopping officer is not a minority.

II.   Stop Likelihood Regressions

A. Methodology

To assess the prevalence of stopping in different geographic areas, we first calculated the number of stops in 942 reporting districts (RDs) by three racial categories (African American, Hispanic, and white/other). We estimated the population in each of these reporting districts on the basis of U.S. Census population density and race composition statistics assigned to the reporting districts by the Analysis Report. This

8

assignment was itself based on a map supplied by the LAPD to the report's preparers. We then estimated stop rates, citation rates and arrest rates for each racial category.[13]

Our estimated stop, citation and arrest rates for each racial category are summarized in Tables 1 through 3. For each LAPD division, the race-specific rate is the average of the race-specific rate in each component Reporting District, weighted by the population of that race residing in the component Reporting District.

B. Results

Table 1 shows the results of our analysis with regard to stops. We can see that, for the city as a whole, in 2003-2004 there 4,569 stops per 10,000 African American residents, while there were only 1,750 stops per 10,000 residents for Whites/Other. Table 1 shows that in two divisions (Central and Hollywood) the rate of stops exceeded 10,000 per 10,000 residents. In the Central division there were more than 21,000 stops for every 10,000 residents. These results make clear that the descriptive statistics included in Table 1 are not stop likelihoods but rather the average number of stops per resident. Residents can be stopped more than once and non-residents who travel into a division can also be stopped.

Tables 2 and 3 report analogous descriptive statistics regarding arrests and citations per 10,000 residents. In contrast to the Analysis report, which analyzes only the likelihood of being arrested or cited conditional on being stopped, these tables describe the number of arrests and citations conditional on merely residing in Los Angeles. We must, however, again keep in mind the possibility that non-residents can be arrested or stopped, and the possibility that individuals can be arrested or cited multiple times in the course of a year.

Table 2 shows that overall the arrest and citations rates were much higher for African Americans than for Whites/Other. Per 10,000 residents, the citywide arrest rate for African Americans was 844, while it was only 159 for Whites/Other (and 277 for Hispanics). Table 3 shows that per 10,000 residents, the citation rate for African Americans was 2,251, while it was 1,358 for Whites/Other (and 1,103 for Hispanics).

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[13] We did this by counting the number of FDRs describing the relevant class of stop in each RD, and dividing this by the estimated race population in the RD. We defined the White/Other population category in the census data to be the total population, minus the population of blacks and Hispanics.

In the FDRs, "Hispanic" is a racial category exclusive with any other. Officers are instructed to assign a race to each individual based on their impression at the time they made the stop. In the census, on the other hand, race is a self-reported category, and "Hispanic" status is a separate category. It is thus possible to be white and Hispanic or black and Hispanic in the census statistics, but not on an FDR. This disjoint has the potential to bias our stop, citation, and arrest rate metrics. To the extent that there are individuals who report themselves white and Hispanic to the census but are just "white" to the LAPD, the stop and other action rates for Hispanics will be biased downwards (appear lower than they actually are), and the stop and other action rates for whites will be biased upwards (appear higher than they actually are). To the extent that there are individuals who report themselves as both black and Hispanic to the census, the population estimates for whites will be biased downwards, thus the stop and other action rates for whites will be biased upwards (appear higher than they actually are). Since the direction of the bias will tend to conceal evidence of racial disparities, we do not believe it throws doubt on our findings.

The descriptive statistics in these tables, however, do not control for the underlying characteristics of the areas where the police behavior occurs. One concern is that that the racial disparities might be driven by differences in the underlying crime rate in the RD where the stops occur. For example, if police stop more people in high crime areas and African Americans tend to live in high crime areas, then the racial disparities uncovered in Tables 1 to 3 might be justified.

To explore this possibility, Table 4 reports the results of regressing the stop rate for particular RDs and races on various RD characteristics. The regressions indicate that even after controlling for violent crime rate and property crime rate in the RD, that the stop rate per 10,000 residents for African Americans was 3,400 stops higher than the rate for Whites/Other. The rate for Hispanics is 350 stops higher than the rate for Whites/Other. Indeed, the right-hand column of Table 4 shows that these racial disparities persist even if one controls not only for the crime rates but for 10 other variables concerning the economic and demographic characteristics of the specific RD. What these regressions cannot control for, however, are race-specific rates of crime. Police might be justified in stopping a higher proportion of African Americans in a particular RD if a higher proportion of crimes in that RD were committed by African Americans. But the regressions in Table 4 are sufficient to show that the racial disparities of Table 1 are not merely a by-product of African Americans and Hispanics living in high-crime neighborhoods. Even after controlling for the crime rates, we find large and statistically significant disparities in the stop rate.[14]

Tables 5 and 6 report the results of parallel regressions concerning the arrest and citation rates per 10,000 residents. Again we see large and statistically significant elevations in the rates for African Americans relative to Whites/Other. Per 10,000 residents, African Americans had 866 more arrests and 1,306 more citations than Whites/Other. Per 10,000 residents, Hispanics had 160 more arrests and 140 more citations than Whites/Other (although the citation result was not statistically significant).

Table 7 provides descriptive statistics of race-specific stop rates for various ranges of the violent crime rate and for various percentage ranges of the minority population. Even after controlling for the violent crime rate, the stop rates of African Americans tend to be higher in non-minority neighborhoods than in minority neighborhoods. For example, looking at the rows relating to violent crime rates between 101 and 200 per 10,000 residents, we see that the black stop rate in RDs with less than 10% black and Hispanic residents was more than 12,000 per 10,000 residents, while the stop rate per 10,000 black residents in RDs that were more than 90% minority was only 3,314. In contrast, these same rows show that the stop rate per 10,000 white/other residents rises from 1,658 when minorities are less than 10% of the population to 1,946 when minorities are more than 90% of the population. Holding violent crime constant,

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[14] The statistical significant is indicated in the tables as a t-statistic. In a regression analysis, the t-statistic measures the number of standard deviations that an estimated coefficient is from zero. A t-statistic with an absolute value greater than approximately two indicates statistical significance (at the 5% level), and a higher t-statistic indicates greater significance.

there is a tendency for African Americans and Hispanics to have higher stop rates when they are a local minority compared to the stop rate when they are a local majority.

The results in Table 7 are consistent with the regression results reported in Figure 1. After controlling for a variety of RD-specific variables—including the violent and property crime rates, the unemployment and poverty rates, and the percent of the population under 24—the Figure shows that the black/white disparity is substantially higher in RDs where minorities are less than one-third of the resident population than in RDs where minorities are more than two-thirds of the resident population. The declining slopes related to both African American and Hispanic disparities are statistically significant.

Table 7 and Figure 1 make clear that the disproportionately high rate of stops for African Americans and Hispanics is not simply a product of their disproportionately living in areas with higher levels of crime. The results of this section raise serious concerns about racial disparities in police stopping. After controlling for local violent and property crime rates, police systematically stop African Americans and Hispanics more than non-minorities.

III.   Post-Stop Regressions

A. Methodology

This section analyzes what happens after a police stop. In particular, we explore the likelihood that police will frisk, search, cite or arrest people that they stop. The analysis of this section most closely parallels the approach of the Analysis Report. Like that report, we will present estimates of whether there are racial disparities in various outcomes after controlling for a host of variables—including the division where the stop occurred, the property and violent crime rates of the location in which the stop occurred, the reason for the stop, demographic characteristics of the RD, and demographic characteristics of the people being stopped.

Tables 8 through 20 report the core measures of racial disparities for all the post-stop outcomes based on 34 regressions. The underlying regressions can be found in Appendix Tables 2 through 35.

Because the Analysis Report and this report come to such different conclusions on the question of racial disparities using the same data set, it is appropriate to comment about two core differences in methodology.

1.   The Analysis Report inappropriately failed to assess whether there were statistically significant racial disparities for the city as a whole; and,
2.   The Analysis Report inappropriately included officer characteristics in their regressions.[15]

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[15] The Analysis Report and the analysis in this report also differed on two technical issues of regression specification. First, the Analysis Report employed a hierarchical linear model, which controlled for the fact

No Aggregate Disparity Estimate. The first problem is that the Analysis Report only includes tests of racial disparity at the division level, but never steps back and assesses whether there were statistically significant disparities for the city as a whole. As mentioned above, the Analysis Report concluded:

> Although some divisions/bureaus have statistically significant racial disparities for some outcomes and some races, when evaluated across all outcomes, *there is no consistent pattern of race effects across divisions or races*.[16]

The Report's approach implies that it would not be problematic if the police only engaged in racial profiling in certain divisions. The implicit idea seems to be that a department engaged in racially biased policing should produce a consistent pattern of racial disparities across all divisions (and possibly across all races). But policy makers could be concerned about unjustified racial disparities that only occur in a subset of divisions. Even the possibility that some divisions would show an estimated racial disparity disfavoring whites relative to blacks or Hispanics should not absolve the police force from a finding that there were unjustified disparities disfavoring minorities in other divisions.

An important problem with the division-specific estimates is their simple numerosity. The Analysis Report estimates so many different racial disparities with respect to each post-stop outcome that it reduces the power of any individual test.

In the extreme, a "divide and conquer" approach to estimation assures estimates that would vary substantially in the degree and significance of the racial disparity. For example, if the Analysis Report had instead decided to estimate the racial disparity in arrests for each individual city block (or for individual officers), we should not be surprised to find very few blocks (or very few individual officers) with statistically significant disparities for the simple reason that there would not be a sufficient number of stops for each test. This is the same reason that it is hard to tell if dice are loaded if you just roll them a few times. Indeed, as a matter of chance, we should expect to see some divisions estimated to have a statistically significant disparity favoring whites—even if there is in fact no racial disparity.

The problem here is not that the Analysis Report chose to estimate division-specific racial disparities. The problem is that it did not estimate the aggregate impact of these disparities for the city as a whole. It is possible to literally add up the individual

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that stops are ordered hierarchically among certain officers who stop suspects in certain RDs that are located in certain Divisions. In contrast, we employed a logistic regression with division-fixed effects, and in some specifications RD or even officer-fixed effects. Second, the Analysis Report employed a step-wise regression approach, which systematically dropped non-race control variables if those variables were initially estimated not to be statistically significant. In contrast, and to avoid issues of pre-testing bias, we retained the full set of right-hand side variables, even if the variables were not statistically significant. The two core differences discussed in the text substantially impact the estimates of racial disparities. But the two technical differences discussed in this footnote do not substantially impact either the estimates of racial disparities or their statistical significance.

[16] ANALYSIS REPORT, *supra* note 1, at 4.

division-specific disparities (with appropriate weights for the number of stops occurring in each division) to produce the overall racial disparities for the city as a whole. This reaggregation procedure is well-known in the literature and easy to do with a single command in virtually all statistical packages that produce evidence not only of the aggregate impact but also of whether that aggregate impact is statistically different from zero.[17] The Analysis Report seems to conclude that there is not on net a city-wide problem because some of the division-specific disparities were not statistically significant or suggested that the police slightly favored minorities. But a simple test of aggregation taking into account these individual division effects is the natural way to test whether summing the division effects produces a statistically significant racial disparity that cannot be explained away as a "no consistent pattern."

The Problem of "Included Variable" Bias. The second core problem with the Analysis Report was the inclusion of control variables that would not plausibly justify a racial disparity in outcomes. As mentioned above, the Analysis Group relied upon an unduly cramped definition of "racially biased policing" in deciding on their list of appropriate control variables. If one was concerned only as to whether individual officers were engaged in a pattern of race-contingent sanctioning, it might be appropriate to include variables controlling for the age of the officer, his assignment, his length of service and the number of complaints or commendations that the stopping officer had received. The possible tendency of officers with a large number of complaints to disproportionately stop Hispanics, and arrest whomever they stopped, would provide an alternative explanation to a theory that the officer or the department itself had a race-contingent policy of disproportionately arresting Hispanics. Including a control for officer complaints would only test to see whether a racial disparity remained *after* controlling for the potential heightened likelihood of high-complaint officers to stop minority suspects. But including a control for officer complaints would be inappropriate if the researcher instead is interested in testing for whether there are unjustified disparate impacts of departmental policing decisions.

This report tries to test whether there are unjustified disparate racial impacts. For such a test, it is only plausible to include control variables that would provide plausible justifications for particular police outcomes. For example, it is appropriate to add controls for the RD crime rate because it might be justified for the police to arrest more people in higher crime areas—even if the addition of crime rate controls lowers the estimated racial disparity. The reduction in disparities might well be appropriate because the goal is only to estimate unjustified racial disparities. But it is our opinion that none of the officer characteristics provide plausible justifications for explaining away racial disparities. For example, the possibility that older officers arrest more African Americans does not provide a plausible justification for explaining away what otherwise would be a larger racial disparity. Accordingly, in some of the post-stop regressions that follow, we exclude controls for the following officer attributes that were inappropriately included in the Analysis Report regressions:

Count of Complaints Against Officer

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[17] *See, e.g.*, Ian Ayres & John J. Donohue III, *Shooting Down the More Guns, Less Crime Hypothesis*, 55 STAN. L. REV. 1193 (2003).

     Count of Major Commendations Received by Officer
     Officer Age
     Officer Gender
     Officer Race
     Number of Months of Service of Officer
     Number of Months of Service of Officer Squared
     Officer Assignment (Traffic, Patrol, Other)
     Officer Race Interacted with Suspect Race.

Including controls for officer attributes might easily cause a regression to understate the true size of the unjustified racial impact.

The larger problem here is one of "included variable bias."[18] While statisticians testing for disparate racial treatment are normally worried about "omitted variable" bias, when testing for unjustified disparate impacts, it is often necessary to be more worried about "included variable bias."[19] Including controls for non-race factors that do not represent legitimate justifications for racial disparities can bias the estimate of whether a decision maker's policies produced an unjustified disparate impact. The statistician who includes such controls ends up controlling in part for the mechanism of the disparate impact, and thus obscuring the impact itself.

A statistical guide for judges and lawyers emphasizes how mistakenly including irrelevant variables can bias a regression's estimate of the racial effect:

> Lastly, and perhaps most important under the heading of legitimacy, is the problem of tainted independent variables. Suppose a regression analysis includes a variable for education that, in a race case, is a key determinant of salary differences between black and white employees in a clearly different job group. Regression analysis indicates a high t-statistic on education and an insignificant t-statistic on the race coefficient. Given that in almost all groups, white employees have received more formal education than black employees, it would appear that education goes a long way towards explaining salary differences between black and white employees. The burden is on the employer, however, to demonstrate separate from the regression, that education was required and affected performance, and hence directly determined salary. To the extent that education is

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[18] Ian Ayres, Three Tests for Measuring Unjustified Disparate Impacts in Organ Transplantation: The Problem of "Included Variable Bias," 48 PERSP. BIOLOGY AND MED. S68 (2005).

[19] The term "included variable bias" is also used in C.C. Clogg & A. Haritou, *The Regression Method of Causal Inference and the Dilemma Confronting this Method*, *in* CAUSALITY IN CRISIS 83, 100-01 (R. McKim & S.P. Turner eds., 1997). They point out that adding variables that are correlated with the error term of the regression can bias the estimate of other coefficients of interest:

> [Estimating a model with additional controls can cause] "included variable bias" in spite of the fact that this model may very well lead to reduction in the variance of the prediction. This term is conspicuous by its absence in the literature. But it is just as logical to talk of included variable bias as omitted variable bias once the term "bias" (not variance) is recognized for what it is . . . . In our judgment, social researchers using the RMCI [the Regression Method of Causal Inference] logic have deduced little more than they have assumed. They have mistakenly assumed that because a predictor correlated with X is important for the purpose of predicting Y, it follows that bias is removed or reduced if we "control" for this variable by adding it to the regression.

Id.

not related to job performance, it is an inappropriate variable to use in a regression. Excluding key variables and including irrelevant variables have the same impact.[20]

The purposeful exclusion of control variables from statistical analysis will accordingly be an essential part of any disparate impact inquiry. Indeed, as the foregoing authority suggests, a variable should be presumptively excluded from the statistical analysis unless the defendant can "demonstrate separate from the regression that [the variable] was required and affected performance."[21]

John Yinger succinctly describes: (i) the problem of "included variable bias" (what he calls "diverting variable bias"); (ii) the need to purposefully exclude certain non-legitimate controls from a regression; and (iii) what constitutes "legitimate" controls:

> Diverting variable bias arises when a variable that is not a legitimate control variable, but that is correlated with race or ethnicity, is included in the regression. The key issue, of course, is how to define what variables are "legitimate." Under most circumstances, economists are taught to err on the side of including too many variables. In this case, however, illegitimate controls may pick up some of the effect of race or ethnicity and lead one to conclude that there is no discrimination when in fact there is. According to the definition of discrimination used here, legitimate controls are those associated with a person's qualifications to rent or buy a house, buy a car or so on—or, to use a legal term business necessity.[22]

The problem of "included variable bias" can also be illustrated by a stylized version of *Griggs v. Duke Power Co.*,[23] the Supreme Court's first disparate impact case. One could imagine running a regression to test whether an employer was less likely to hire African American applicants than white applicants. It would be possible to control in this regression for whether the applicant had received a high school diploma. Under the facts of *Griggs*, such a control would likely have reduced the racial disparity in the hiring rates. But including in the regression a variable controlling for applicant's education would be inappropriate. The central point of *Griggs* was to determine whether the employer's diploma requirement had a disparate racial impact. The possibility that including a diploma variable would reduce the estimated race effect in the regression would in no way be inconsistent with a theory that the employer's diploma requirement disparately excluded African Americans from employment. Excluding non-race factors is inappropriate in disparate treatment tests, but such exclusion is *necessary* in disparate impact tests so as not to bias the coefficient of interest.

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[20] THOMAS R. IRELAND ET AL., EXPERT ECONOMIC TESTIMONY: REFERENCE GUIDE FOR JUDGES AND ATTORNEYS (1998).

[21] *Id.*

[22] John Yinger, *Evidence on Discrimination in Consumer Markets*, 12 J. ECON. PERSP. 23, 27 (1998).

[23] 401 U.S. 424, 431-32 (1971).

In disparate impact regressions, it is thus necessary to intentionally exclude even true "causal" variables from the analysis.[24] In a *Griggs* disparate impact regression estimating the probability that particular applicants will be promoted, the high school diploma variable is excluded even though it in fact is believed to truly influence whether particular applicants will be accepted. But only by excluding this causal variable can we estimate whether the employer's diploma requirement in fact has a disparate impact. In fact, by running the regression both with and without the diploma control, one can estimate how much the diploma requirement contributes to the overall disparate impact of the employer's hiring practices.

Just as it would be inappropriate to include a high school diploma variable in the *Griggs* context, it was inappropriate for the Analysis Group to include officer attribute variables in their attempts to test for "racially biased policing." Even if certain officer attributes cause higher arrest or search rates, that is not a reason for allowing these attributes to potentially soak up (and therefore reduce) some of the post-stop racial disparities.[25]

B. Results

Arrests. Tables 8 – 20 summarize our estimates of racial disparities from thirty-four regressions. The tables have a parallel structure with the estimated results from different regression specifications laid out in different columns. For example, in Table 8, which concerns the likelihood of arrest, the first column, labeled "Replication Model," shows the aggregate black and Hispanic disparities from a specification that includes the same controls as used in the Analysis Report.[26] The underlying regression results for this regression can be found in Appendix Table 2. Table 8 aggregates the 17 division race effects with the overall race effects to produce the overall racial disparity estimate. Controlling for the same variables as the Analysis Report, we find that the LAPD was 21.4% more likely to arrest stopped African Americans than stopped whites and 28.5% more likely to arrest stopped Hispanics than stopped whites. Both of these results were

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[24] James Heckman has shown that included variable bias can be a problem even in disparate treatment regressions. See Ayres, *supra* note 18, at n.9. Imagine, for example, that an employer's hiring decisions are known to be completely determined by four variables: the applicant's race, the applicant's education, the applicant's prior work experience and the applicant's age. It turns out that a regression that controls for three of these four attributes (race, age, education) may produce more biased estimates about the influence of race than a regression that controls for just two of the attributes (race and age). When a researcher cannot control for all the causal variables, then controlling for a larger subset of causal variables does not necessarily produce less biased results. But this problem of included variable bias is even larger in disparate impact analysis. In the foregoing example, a researcher who had access to all four of the causal variables might still need to intentionally exclude one in order to test whether its exclusion induced a disparate impact in the estimated race coefficient.

[25] There are other control variables that we have retained in our regression specifications that may not provide a plausible explanation for the police behavior in question to justify inclusion in a racial disparity test. For example, controlling for the percent of residents who are divorced in the RD does not seem like a plausible justification for police to arrest more or fewer people in a particular RD. But including these tenuous controls, if anything, makes our test of unjustified racial impacts more conservative.

[26] As described above, *supra* note 11, our replication used a slightly different regression specification with regard to the hierarchical structure of the observations, but this difference did not substantially impact the size or significance of the reported disparities.

highly statistically significant (p. < .01). Even using the Analysis Group's own controls, we find substantial and statistically significant racial disparities. The previous discussion of omitted variables (such as data on race-specific crime rates) might provide alternative justifications for these disparities. But the existence of such large and unexplained disparities is a reason for concern and a policy response.

The second column of Table 8, labeled "Unrestricted Model," reports the result of a specification that includes all the controls used by the Analysis Group in its various regressions. The only difference between this model and the previous "replication" model is that, unlike the Analysis Report, we did not drop control variables that were statistically insignificant. The Replication models in Tables 8-14 have different control variables for each outcome. Different controls were dropped because they happened not to be significant in the Analysis Group's iterations. But the Unrestricted Model has the same sets of controls across each of the post-stop tables. The same is true for each of the remaining columns. Adding the additional (insignificant) controls does not substantially alter the size or significance of the estimated racial disparities.

The third column of Table 8, labeled "No Officer Attributes," reports the results of the unrestricted model but omits the officer attributes because, as discussed previously, the officer attribute controls do not provide plausible justifications for racial disparities. We believe this specification is the most probative test of whether police behaviors produced unjustified racial disparities. One sees that excluding officer attributes tends to increase the size and the significance of racial disparity. For example, the black disparity rises from 21.4% in the replication model to 29.2% in the "No Officer Attributes" model, and the Hispanic disparity rises analogously from 28.5% to 32.4%. This pattern of heightened disparities and significance is repeated in several of the subsequent tables.

Finally, the last column of Table 8, labeled "RD Fixed Effects, No Officer Characteristics," reports the results of a regression that replaces the RD-invariant control variables with individual controls for each of 1008 RDs within the city of Los Angeles. Including these RD-specific controls may inappropriately absorb the race coefficient because after controlling for violent and property crime rates in the RD and a host of other RD characteristics, RD characteristics do not provide a plausible justification for racial disparities in arrest propensity. If two RDs have similar crime rates and are similar in other relevant characteristics, it seems inappropriate to allow one RD to be assigned a higher arrest propensity when this is likely to soak up part of what otherwise would be a larger estimate of racial disparities. We report this regression as a robust check—even after giving the regression literally hundreds of RD-specific possibilities to explain away elevated arrest rates for blacks and Hispanics, we still find large and largely unchanged estimates of racial disparity. That is, even if we look at differences within each of the 1,000 reporting districts and ignore the differences that play out between reporting districts, blacks are 27.5% more likely than whites to be arrested, while Hispanics are 32.6% more likely than whites to be arrested. The racial disparities in likelihood of arrest are robust to adding in RD-specific controls.

Citations. Table 9 reports the results from parallel regressions concerning the likelihood that stopped people will be cited by the police. The four different specifications show a consistent pattern that stopped African Americans are significantly less likely to be cited than stopped whites. For example, in our preferred "No Officer Attributes" specification, stopped African Americans were 31.6% less likely to be cited than stopped whites (p. < .01). But the exclusion of officer attributes again increases the size and significance of the estimated Hispanic disparity. In the "Replication" model, the stopped Hispanics were slightly (.2%) less likely to be cited than stopped whites. This result was not statistically significant. But when we excluded officer attributes, we found that stopped Hispanics were 4.8% more likely to be cited and that this disparity was statistically significant (p. < .01).

In interpreting the citation disparities, however, it is important to keep in mind the difference between the conditional and unconditional liability of being cited. While Table 9 shows that the citation likelihood *conditional on being stopped* is less for African Americans than whites, Table 6 shows the unconditional likelihood of African Americans being cited was significantly higher than that of whites. Even after controlling for the local crime rate, African Americans are so much more likely to be stopped than whites, that their probability of being cited is higher.

But there is a basic ambiguity in citation results. One troubling interpretation is that the police department is using the stop itself as a form of harassment. Table 10 indicates, for example, that African American stops are 21% less likely than white stops to generate an arrest or citation. This tendency for African American stops (again, after controlling for RD crime and host of other variables) to be systematically less productive raises the possibility that many of these stops are unnecessary. On the other hand, Table 8 indicates that stopped African Americans are systematically more likely to be arrested than stopped whites. A core ambiguity then is whether the heightened arrest rate of stopped blacks should be seen as a justification for the higher likelihood of "no action" stops or if the heightened arrest rate should be seen independently as a cause of potential police mistreatment of African Americans. Without better information about the underlying criminality of specific racial groups, it is difficult to adjudicate between these two different interpretations.[27]

Citations and Arrests: The "Same Race" Benchmark. In the absence of information about criminality of racial groups, an alternative benchmark that has been used in other racial disparity testing[28] is to investigate the behavior of minority officers. Minority officers themselves might be subject to racial bias against their own race. But if minority officers are likely to show *less* conscious or unconscious bias with regard to

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[27] It is also possible that both interpretations are true. The police may simultaneously stop an excessive number of African Americans (with these stops ending in "no action") and the police may excessively arrest stopped African Americans compared to similarly situated stopped whites.

[28] Stacia Gilliard-Matthews, Brian R. Kowalski & Richard J. Lundman, *Officer Race and Citizen-Reported Traffic Ticket Decisions by Police in 1999 and 2002*, 11 POLICE Q. 202-219 (2008); John J. Donohue III & Steven D. Levitt. *The Impact of Race on Policing and Arrests*, 44 J.L. & ECON. 367 (2001); Joe Price & Justin Wolfers, *Racial Discrimination Among NBA Referees* (Nat'l Bureau of Econ. Research, Working Paper No. 13,206, 2007).

suspects of the same race, then a discrepancy in the police behaviors of minority and non-minority officers could provide evidence of unjustified racial disparity. Tables 11 and 12 provide an analysis of racial disparities controlling for officer race using the controls from the "Unrestricted Model" discussed above.[29] Table 11 shows, for example, that with regard to arresting decisions, the racial disparities for minority officers are systematically lower than that of non-minority officers. Non-black officers were 26.9% more likely to arrest stopped blacks, but black officers were only 17.7% more likely to arrest stopped blacks. Similarly we find that non-Hispanic officers were 31.1% more likely to arrest stopped Hispanics, but Hispanic officers were only 24.5% more likely to arrest stopped Hispanics. This evidence of differential policing behavior by minority and non-minority officers is further evidence that at least some portion of the racial disparity is unjustified.

Black officers also seem to lean against the citation disparity discussed above. Table 12 shows that non-black officers were 25.0% less likely to cite stopped blacks, but black officers were only 4.5% less likely to cite them. This suggests that black officers were simultaneously less likely to arrest and less likely to "no action" stopped blacks. These disparities in arrest and citations rates were statistically significant. The different estimated racial disparities of minority and non-minority police officers (after controlling for a host of other characteristics) raise troubling concerns about whether policing behaviors are driven by substantive differences in the criminality of the suspects.

Frisks, Searches and Requests to Exit Vehicle. We conducted parallel tests of racial disparities regarding police decisions to frisk and search stopped individuals. With regard to blacks, we find very substantial racial disparities. Table 13 shows that in all specifications (controlling for crime and a host of other factors), stopped African Americans are more than twice as likely to be frisked as stopped whites, and that stopped Hispanics are more than 40% more likely to be frisked.

Table 14 shows that police are substantially more likely to request to search stopped African Americans than stopped whites—about 50% more likely in the replication and unrestricted models, rising to 76.4% more likely when officer attributes are excluded. This latter finding is particularly troubling, as it suggests that differences in the officers are driving a 25% point difference in the racial disparity. And excluding officer attributes, we find that police are 16.0% more likely to request to search stopped Hispanics than stopped non-Hispanic whites. Table 15 shows that the racial disparities for non-consensual stops are even higher—in our preferred "No Officer Attributes" specification, stopped blacks are 80.7% more likely than stopped whites to be subjected to a non-consensual search, and stopped Hispanics are 76.8% more likely.

Finally, we find that police are much, much more likely to ask minorities to exit vehicles. Table 16 shows in vehicle stops that police are 165.9% more likely to request that stopped African Americans exit their vehicles than stopped whites, and that stopped-

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[29] For simplicity in interpreting the results, we dropped the district/race interaction variables so that the arrest coefficient reflects a citywide number. We then included dummy variables denoting cases when a black officer stopped a black suspect and when a Hispanic officer stopped a white suspect.

Hispanics are 131.9% more likely to be asked. All these results are statistically significant (p. < .01) and robust to including a variety of different controls related to the crime rate and other attributes of the stop.

Excluding Low-Discretion Outcomes. The Analysis Report appropriately considers the impact of excluding stop observations where, because of policy, police officers had less discretion on whether to undertake a particular activity. For example, with regard to the decision of whether or not to search an individual, department policy requires that a search be conducted whenever an individual is placed under arrest. Excluding the low discretion observations of search incident to arrest could focus attention on stops where officers had more discretion. We have some suspicion of the methodology as applied to some of the sanctions, because the classification of a sanction as non-discretionary may depend on officer decisions just as discretionary as a sanction. For example, an arrest of a violent suspect is a non-discretionary arrest. However, we have no objective information about whether the suspect was actually violent—merely the officer's classification of him or her as such on the FDR. However, we have followed the Analysis Group's categories in excluding low-discretion searches. For example, for the regressions reported in this section relating to high-discretion searches, we only retained searches that fell into the Analysis Group's definition:

> For the purposes of this analysis, higher discretion searches were those for which the authority for the search was odor of contraband, parolee/probationer searches, and searches incident to patdown/frisks.[30]

To the extent that racial disparities are driven by low or no-discretion choices, we might conclude that the racial disparities are less problematic. In fact, one of the Analysis Report's conclusions might suggest just this interpretation, as the Report emphasized:

> Racial disparities are generally lower when stops with lower discretion are removed from the analysis, with the exception of non-gang officer requests to exit the vehicle.[31]

However, some of the low-discretion circumstances, such as search incident to arrest, might themselves be a by-product of higher discretion police choices. In other words, although a search is mandatory given the decision to arrest, the decision to arrest may itself be discretionary, or the result of a discretionary action such as a frisk. Moreover, a finding that racial disparities are not driven by officer discretion still leaves open a separate question of whether the police policies driving the disparities are themselves justified. But in this section, we address a narrower question. What is the impact on the aggregate city-wide disparities if we omit observations with low-discretion police actions?

We reran two of the four specifications outlined above—the unrestricted model and the no-officer attributes model without low-discretion action observations—and

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[30] ANALYSIS REPORT, *supra* note 1, at 11.
[31] *Id.* at 4.

recalculated the aggregate racial disparities. For example, as Table 17 shows, we excluded observations when there was an arrest for outstanding warrants, violent crimes and DUIs (Driving Under the Influence), and we found reduced racial disparities. But in our preferred specification, the arrest disparities for both blacks and Hispanics remained statistically significant. Stopped blacks were 13.7% more likely to be subjected to high-discretion arrests and stopped Hispanics 21.4% more likely to be subjected to high-discretion arrests. Table 18 reports the results of analogous regressions excluding low-discretion citations and finds that excluding these observations extinguishes the previously reported citation disparity disfavoring Hispanics (suggesting that this earlier disparity is driven by low discretion citations). The estimated black citation disparity favoring blacks grows slightly (suggesting that there is slightly greater racial disparity for high-discretion citations).

Finally, we find that the previously reported racial disparities in searches and in requests to exit vehicles are not driven by low-discretion police decisions to engage in these actions. If anything there is a tendency for the removal of low-discretion searches to increase the size of these disparities. For example, Table 14 reports that police were 76% more likely to request to search stopped blacks than stopped whites (and 16% more likely to request to search stopped Hispanics). But after excluding low-discretion searches, we find in Table 19 that police are 106% more likely to search stopped blacks than stopped whites (and 12% more likely to search stopped Hispanics). Similarly, we found in Table 16 that police were 166% more likely to request that stopped blacks exit their vehicles than to make that request to stopped whites (and 132% more likely to make the request to stopped Hispanics). But Table 20 reports that after excluding low-discretion requests (related to violent arrests), that these black and Hispanic disparities remain virtually unchanged.

In sum, we find with regard to some police actions that excluding low-discretion observations increases the previously estimated racial disparities—suggesting that the high-discretion actions are particularly prone to unjustified police bias. With regard to other police actions, excluding low-discretion observations reduces the estimated racial disparities—suggesting that the previous racial disparities are at least partly driven by police policies and not individual officer decision-making. However, the larger picture is that previous findings of statistical significance remain robust to exclusion of low-discretion observations. Even excluding low-discretion observations, we continue to find statistically significant racial disparities disfavoring stopped African Americans and Hispanics with regard to arrest, search and requests to exit the vehicle.

Individual Officer Effects. Although it is not appropriate to control for officer attributes when testing whether or not there are unjustified racial disparities, it is appropriate to look at individual officers when trying to discern the possible source of these racial disparities. This section explores this issue by: i) estimating black and

Hispanic disparities for individual officers; and ii) correlating these disparity estimates with underlying officer attributes.[32]

Figure 2 reports the results of the first part of this exercise. We reran the original "No Officer Attributes" regression reported in Table 8, but instead of estimating division-specific racial disparities, we estimated officer-specific racial disparities for 540 different officers. This subset of officers was selected on the basis that each of these officers had a high number of stops (greater than or equal to 200) and that each of these officers had at least one positive outcome for each race, that is, had arrested at least one white, one black, and one Hispanic.[33] Figure 2a reports the disparities in the propensity of individual officers to arrest stopped blacks relative to stopped whites. Figure 2b reports the analogous arrest disparities regarding stopped Hispanics relative to stopped non-Hispanic whites. As before, all of these officer disparity estimates are controlling for the crime rate, RD characteristics and a host of stop attributes. The point estimate for each officer is indicated by the darker circle and it is surrounded above and below by a gray bar indicating the 95% confidence interval for the disparity estimate. The figures sort the officer estimates from the estimates that most disfavor stopped whites (on the left) to those that most favor stopped whites (on the right).

Overall, the figures show that a majority of the officers are estimated to have a racial disparity in arrests that disfavor minorities. 57.2% (309/504) of the officers are estimated to have a heightened propensity to arrest stopped blacks relative to similarly situated stopped whites, and 59.4% (321/504) of the officers are estimated to have a heightened propensity to arrest stopped Hispanics relative to similarly situated stopped non-Hispanic whites. But as discussed above, it should not be surprising that few of these individual officer disparity estimates are statistically significant. Dividing the data so thinly robs the individual officer tests of their power to identify statistically significant differences. But as emphasized earlier with regard to division-specific disparity estimates, the failure of statistical significance with regard to subgroups does not mean that one cannot identify statistical significance in the aggregate. And in fact, Table 8 already shows that the aggregation of these individual officer disparities produces sizeable and statistically significant net differences disfavoring stopped African Americans and Hispanics.

Figure 3 is a simple scatter plot of the estimated black and Hispanic arrest disparities in a single graph. The figure shows that two estimates of disparity are positively correlated. Officers who are estimated to have a higher unjustified bias against stopped Hispanics are likely to have a higher estimated unjustified bias against stopped blacks. This provides some evidence that the estimated racial disparities are not simply

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[32] The Analysis Group initially intended to make "officer-to-officer comparisons." But "in response to public comments" the City apparently choose not to have the Analysis Group "identify[] individual officer[s] and/or groups of officers that may warrant further review." *See* ANALYSIS GROUP, *supra* note 7.

[33] Of the officers who met these criteria, one was dropped as an outlier. This officer had only stopped one white and more than 100 blacks and 100 Hispanics. While it is appropriate to exclude this anomalous officer from the statistical analysis of the department, from a management perspective, such an outlier should be the focus of additional scrutiny.

noise or randomness. Instead, it suggests that the same underlying factors drive two types of racial disparity.

Tables 21 and 22 investigate whether specific officer attributes are correlated with the estimated racial disparities displayed in Figure 2. Ordinary least square regressions of the black and Hispanic officer-specific regressions provide some evidence of officer attributes that are correlated with racial disparities. Across all six regressions in these tables, minority officers are estimated to have lower racial bias than white officers, but none of these effects is statistically significant. Officers with more commendations are estimated to have statistically significant higher black and Hispanic bias. In addition, officers assigned to traffic duty have statistically significant higher black and Hispanic racial bias than patrol officer assignments. At the moment, these correlations are at best suggestive of officer attributes that might predict in advance the degree of individual officer bias. But this is an issue that we will return to later in our "Conclusion and Recommendations" section.

IV.   Post-Search Regressions, or "Hit Rate" Analyses

A. Methodology

Our final major empirical effort concerns an assessment of the likelihood that frisks and searches will produce evidence of contraband. Although these assessments were performed using the same data on which Analysis Report is based, that report did not undertake these studies. These post-search regressions are a standard tool of testing for police racial disparities,[34] and are sometimes referred to as "outcome" tests or "hit rate" tests—because they investigate the probable outcome of searches (or the probability that a search will uncover contraband, i.e., a hit).[35] The *ex post* probability that a police search will uncover contraband or evidence of illegality is strong evidence of the average level of probable cause that police require before undertaking a search. A finding that minority searches are systematically less productive than white searches is accordingly evidence that police require less probable cause when searching minorities. To be sure, such a finding does not require that we infer that police engaged in disparate treatment— but, at a minimum, it is evidence that whatever criteria the police employed produced an unjustified disparate impact.[36] Such evidence would suggest that if police required the same level of probable cause when searching minorities as when searching whites, there would be fewer minorities searched (or proportionally more whites searched).

A major advantage of these outcome tests is that they are not susceptible to the omitted variable bias critique that has plagued traditional regression-based tests of disparate treatment. Researchers don't need to observe and control for all of the variables

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[34] John Knowles, Nicola Persico, & Petra Todd, *Racial Bias in Motor Vehicle Searches: Theory and Evidence*, 109 J. POL. ECON. 203, 203-29 (2001).

[35] Ian Ayres, *Outcome Tests of Racial Disparities in Police Practices*, 4 J. JUST. RES. & STAT. ASSOC. 131 (2002).

[36] Evidence of an unjustified disparate impact can be used as evidence of intentional discrimination (disparate treatment), and under current federal law, unjustified disparate racial impacts of police action can be challenged. See 28 C.F.R. § 42.203(3) (implementing 42 U.S.C. § 3798d(c) (2000)).

that police considered in deciding whether to search, as long as they can observe the outcome of their decision-making.

Indeed, in sharp contrast to disparate-treatment testing, an outcome-regression testing for unjustified disparate racial impacts in searching decisions needs only include controls for the race of the people who are stopped. The outcome tests are not embarrassed by omitted variable bias, because 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.[37] Indeed, perversely, the outcome test intentionally harnesses omitted variable bias to test whether any excluded (unjustified) determinant of decision-making is sufficiently correlated with the included racial characteristics to produce evidence of a statistically significant racial disparity.[38] Any finding that the police searches of individuals with a particular characteristic (such as minority status) induce a systematically lower probability of uncovering illegality suggests that police search criteria unjustifiably subject that class of individuals to the disability of being searched.

This omitted variable point can be restated in more legalistic terms. The outcome test is not susceptible to the "qualified pool" problem that plagues both traditional disparate impact and disparate treatment issues of proof. In an outcome test, the police by their own searching decisions define what they think the qualified pool is, and the outcome test then directly assesses whether the minorities and non-minorities so chosen are in fact equally qualified to be searched. A finding that the search hit rate (that is, the probability of finding evidence of illegality) is systematically lower for searched minorities than for searched whites suggests that minorities less deserved (that is, were less "qualified") to be searched.

Outcome tests are not always an infallible measure of whether policing decisions generate unjustified racial disparate impacts.[39] But a finding that minority searches are systematically less productive than searches of whites raises serious concerns and shifts the burden to explain a legitimate reason for the disparity. A defense that police searching decisions were driven by the underlying criminality of those searched—and that minorities make up a larger proportion of those deserving to be searched—would be contradicted by systematically lower success rates of minorities when such searches were in fact completed.

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[37] Knowles, Persico & Todd, *supra* note 34, suggests that the average success rate of searches for different races will also tend toward equality because of the strategic reaction of the individuals subject to searches.

[38] Stephen Ross & John Yinger, *The Default Approach to Studying Mortgage Discrimination: A Rebuttal*, *in* MORTGAGE LENDING DISCRIMINATION: A REVIEW OF EXISTING EVIDENCE 107, 112 (M. A. Turner & F. Skidmore eds., 1999), have noted that the default approach attempts to identify mortgage discrimination by purposely omitting variables from the regression.

[39] See, e.g., discussion of the infra-marginality problem and the sub-group validity problem in Ayres, *supra* note 35, and chapter 6 of Ayres, *supra* note 6.

B. Results

Tables 23 through 28 report the result of our post-search regressions on a variety of different investigative techniques, on a variety of different types of contraband, and on a variety of specifications. [The underlying regressions for these specifications can be found in Appendix Tables 36 through 53.] For example, Table 23 concerns the likelihood that a frisk would uncover a weapon. The first column reports the results of a regression that simply controls for the race of the person stopped. As just discussed, this specification produces our preferred estimate of racial disparity. In this column, we see that frisked African Americans are 42.3% less likely to be found with a weapon than frisked whites and that frisked Hispanics are 31.8% less likely to have a weapon than frisked non-Hispanic whites. These statistically significant findings are particularly important because pat-downs and frisks are constitutionally justified by the need to ensure officer safety by searching for accessible weapons, and are not intended to look for drugs or other contraband.[40]

But as a test of robustness, we include in each of our outcome tables two other regression specifications which add in successively more controls. The middle column of Table 23 adds in controls related to the attributes of the specific stop—such as whether the stop took place at night, whether the stop took place on the weekend, the division where the stop occurred, the role (driver, passenger or pedestrian) of the suspect and the assignment (traffic, patrol or other) of the officer. Adding these stop attribute controls slightly lowers the racial disparity estimates. The black frisk disparity is reduced to 39.9% and the Hispanic frisk disparity falls to 28.2%. But as emphasized above, it is not clear that allowing these additional controls to soak up part of the estimated racial disparity gives a more accurate view of the true unjustified disparate impact. Imagine, for example, that the police demand lower justification to frisk people at night and that Hispanics are disproportionately frisked at night. Including (as the middle specification does) a control for night-time stops might then reduce the estimated Hispanic disparity. But the policy of frisking people with a lower probable cause at night is producing a disparate impact on Hispanics and something would have to be produced to explain why this disparate impact was justified. As with the post-stop regression, including controls that don't provide a plausible justification can actually bias downward the disparate impact estimate.

This problem of included variable bias is even more severe with respect to the final column reported in Table 23. This specification includes controls for RD stop specifications as well. These include all the available information about the stop circumstances plus the number of gang stops in the RD, the violent and property crime rates in the RD and the number of business tax accounts and retail tax accounts in the RD (estimated by the Analysis Group). But it is even harder to understand why these extra

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[40] The Supreme Court has stated that "a protective search – permitted without a warrant and on the basis of reasonable suspicion less than probable cause – must be strictly limited to that which is necessary for the discovery of weapons which might be used to harm the officer or others nearby. If the protective search goes beyond what is necessary to determine if the suspect is armed, it is no longer valid . . . ." *Minnesota v. Dickerson,* 508 U.S. 366, 373 (1993).

controls would provide valid justifications for disparate racial impacts. The police might demand a lower probable cause for frisking people in RDs with higher divorce rates and this policy might disproportionately cause minorities to be searched. But if this causes searches of minorities to be less successful than searches of whites, then the policy is facially unjustified. Nonetheless, Table 23 shows that including these additional (inappropriate) controls does not eliminate the statistical significance of the sizable racial disparities. The black frisk disparity in this specification is still 38.4% and the Hispanic frisk disparity is still 24.1%.

Tables 24 and 25 calculate frisk disparities related to other types of contraband. In our preferred specification, frisked blacks are 25.4% less likely than frisked whites to be found with drugs and 32.8% less likely to be found with any other type of contraband. Frisked Hispanics are 38.2% less likely to be found with drugs and 15.1% less likely to be found with any other type of contraband.[41] These results are statistically significant. The size of the disparities diminishes and tends to become less statistically significant as controls are added. This indicates that police frisking policies correlated with these non-race attributes are driving part of the estimated racial disparities, but it does not speak directly as to whether those policies are justified.

Tables 26-28 report analogous outcome tests with regard to consensual searches. Again, in all of our preferred "Race Only" regressions we find that searches of blacks and Hispanics are substantially less productive than those conducted on stopped whites:

Consensual searches of blacks are 37.0% less likely to uncover weapons, 23.7% less likely to uncover drugs and 25.4% less likely to uncover anything else.

Consensual searches of Hispanics similarly are 32.8% less likely to uncover weapons, 34.3% less likely to uncover drugs and 12.3% less likely to uncover anything else.

In this specification, all of these results are statistically significant. Once again the sizes of these disparity estimates decrease and often become statistically insignificant when additional controls are added to the regression.

All in all, the results of this report raise grave concerns that stopped African Americans and Hispanics were over-frisked and over-searched. Tables 13 and 14 show that, after controlling for the crime and a host of other stop characteristics, stopped minorities were substantially more likely to be stopped. For example, Table 13 shows that a stopped African American was more than twice as likely to be frisked as a similarly situated stopped white. And the outcome results of this section provide powerful evidence that these elevated rates of frisking and searching were not justified. The frisks and searches of minorities were systematically less likely to produce weapons, drugs or any other form of contraband.

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[41] "Other contraband" ranged from stashes of money, to alcohol, to evidence of crime.

V.   Conclusion and Recommendations

The results of this study raise grave concerns that African Americans and Hispanics are over-stopped, over-frisked, over-searched, and over-arrested. After controlling for the violent and property crime rates in the specific reporting district and a host of other variables, we find that:

Per 10,000 residents, the black stop rate is 3,400 stops higher than the white stop rate and the Hispanic stop rate is almost 360 stops higher.

Relative to stopped whites, stopped blacks are 127% more likely to be frisked and stopped Hispanics are 43% more likely to be frisked.

Relative to stopped whites, stopped blacks are 76% more likely to be searched and stopped Hispanics are 16% more likely to be searched.

Relative to stopped whites, stopped blacks are 29% more likely to be arrested and stopped Hispanics are 32% more likely to be arrested.

All of these disparities are statistically significant (p. < .01). While the stop rate for whites/other was 1,750 stops per 10,000 residents, the stop rate for blacks was more than 4,500 stops. And in the Central division, there were more stops of blacks and Hispanics in a single year than there were black and Hispanic residents. These stark statistics from a single year of LAPD motor vehicle and pedestrian stops give a numeric lens for the lived experience of "driving while black" or "driving while Hispanic."

But, as emphasized by the Analysis Report, these substantial and statistically significant racial disparity estimates by themselves do not provide conclusive or incontrovertible evidence that the LAPD engaged in race-contingent profiling or even that policing decisions produced unjustified disparate impacts. Most importantly, the post-stop regressions in this report were not able to control for the race-specific criminality of the people who were stopped or the race-specific crime rates in the areas where the stops occurred.

To respond to this legitimate alternative explanation for the estimated disparities, we have exploited a variety of indirect benchmarks to try to control for race-specific criminality. Our Post-Search "outcome" tests strongly corroborate the finding that African Americans and Hispanics are over-frisked and over-searched. Because of the outcome tests, we know that the frisks and search rates are not only disproportionately high given the area crime rate and other factors, but we also know that these investigative techniques are systematically less productive when conducted on blacks and Hispanics than when conducted on whites. It is implausible that the higher frisk and search rates were justified by higher minority criminality, when these frisks and searches were less likely to uncover weapons, drugs or other types of contraband.

Our analysis of officer race provides another indirect control for race-specific criminality. Although we as researchers do not observe all the criminality of the specific people who are stopped, the stopping officers are exposed to several types of data that are not in the dataset. If we assume that officers are less likely to engage in racially biased policing against members of their own race, we can use the behaviors of police with regard to people of the same race as a benchmark to test the behaviors of police with regard to people of other races. Our "same race" regressions support the finding that African Americans and Hispanics are over-arrested. The black arrest disparity was nine percentage points lower when the stopping officer was black than when the stopping officer was non-black. Similarly, we found that the Hispanic arrest disparity was seven percentage points lower when the stopping officer was Hispanic than when the stopping officer was a non-Hispanic white. Furthermore, African American officers are more likely to cite African Americans, and significantly less likely to subject African Americans to a "no action" stop, suggesting that the lower citation rate of African Americans by non-black officers may be because African Americans are pulled over by non-black officers more often when committing no violation, rather than because African-American officers show leniency for African Americans who commit citable offenses.

These results taken as whole provide sufficient reason for the LAPD both to engage in further investigation and to take actions to mitigate these probable unjustified racial disparities.

This report is based on our statistical analysis of LAPD officers' self-reported actions, not a study of the Department's policies or history. We recognize that the Department has taken steps toward addressing these issues in recent weeks, but do not evaluate their response. Our recommendations grow from the statistical evidence and the concerns it raises and are not intended to be exhaustive. At a minimum, we recommend that the department undertake the following concrete actions: The department should: (i) undertake ongoing and enhanced data collection; (ii) establish an "early warning" system with feedback about disparities at the officer, RD and division levels, as well as independent statistical assessment of racial profiling complaints; and (iii) create anti-bias training and test whether the training is effective.

Ongoing and Enhanced Data Collection. The findings strongly support continued collection of stop data that allows for longitudinal assessment of whether racial disparities in stopping, searching and arresting are growing or diminishing over time. The current data-structure contains a wealth of information for analysis. Without continued collection, it will be impossible for the Department or others to determine whether, relative to the past, racial disparities are increasing or decreasing.

But the inclusion of two additional types of data would be particularly worthwhile. First, the department should incorporate information about race-specific crime rates taken from victimization reports. Victimization data is not error free (especially with regard to victimless crimes and crimes where victims do not know the race of the criminal). But victimization data is not produced directly by the police and

hence does not suffer from the contamination concern. Controlling for violent and property crime rates of specific races in specific RDs would powerfully respond to the concern that racial disparities were driven by race-specific differences in offending. Victimization evidence of this kind is collected by other police departments (see, for example, the NYPD) and has been used by other researchers to test for unjustified racial impacts.

Second, the department should require that all existing and new officers take the Implicit Association Test (IAT) and retain the results of these tests. The IAT is a free Internet test which takes about ten to fifteen minutes to complete.[42] It produces a measure of unconscious bias with regard to the attribute being tested. For example, the Black/White IAT produces a measure of whether an individual has unconscious negative associations with photographs of African Americans relative to photographs of whites. Collecting information about the IAT of individual officers would provide a powerful validation test of the estimated racial disparities reported in this report. If the individual officer disparities estimated in Figure 2 are positively correlated with the disparities estimated by the IAT, it would be very hard to accept that the policing disparities were justified by suspect or stop attributes. We would be willing to donate our time to set up an Internet-based system that would administer the test and retain confidential IAT results. We would also be willing to analyze the results to see if the IAT disparity measures correlate with the officer disparities estimated in this report.[43]

An Early Warning System With Periodic Assessment. In addition to enhanced, ongoing data collection, we recommend that the department establish a system to periodically assess the degree of black and Hispanic disparities on various outcome measures. This assessment should be undertaken at both the RD and division level. In addition, the department should develop an early warning system to proactively identify officers who start to display systematic racial discrepancies in their policing behavior. The purpose of this early warning system would not be to automatically reassign or restrict the freedom of these officers, but to trigger additional assessments during the officers' periodic evaluations and to allow the opportunity to intervene with additional training or other corrective measures.

In addition, the department should statistically assess the policing behavior of individual police who have been subjects of racial profiling complaints. When an officer has been accused of race discrimination, the department should do more than ask whether the stop or arrest in question was legally justified. An assessment of whether the accused behavior is consistent with a more general pattern or practice of discrimination should be undertaken.

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[42] Anyone can take the IAT through the Harvard website: https://implicit.harvard.edu/implicit/index.jsp.

[43] If the department preferred, we could arrange for a team of respected academics, including Mahzurin Banaji of Harvard University, Petra Todd of the University of Pennsylvania, and Justin Wolvers of the Wharton School at the University of Pennsylvania, to supervise the administration and analysis of IAT testing.

Anti-Bias Training and Testing. Finally, we recommend that the department develop additional anti-bias training to help reduce the estimated disparities uncovered in this report. We recommend, however, that the department test whether the training is in fact effective by initially conducting a randomized study to determine whether officers exposed to anti-bias training in fact exhibit lower racial disparities. IAT testing should be an important component of any anti-bias training. The IAT results can provide both a validation metric of racial disparity and a policy instrument for department action. If the IAT test is shown to correlate with estimates of individual officer policing disparities, then it might be appropriate to test whether anti-bias training has the effect of reducing the unconscious bias that officers display in the IAT.[44]

Reasonable people can differ about the exact interpretation of the results in this report and about the appropriate department response to these results. But the robust findings of racial disparity that persist after controlling for crime rates and a host of other factors are hard to dismiss. To our mind, they shift the burden to the department to provide a response.

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[44] The IAT results might also be used as a factor in officer assignment and even hiring decisions. *See* Ayres, *supra* note 6.

Table 1: Number of Stops per 10,000 Residents 942 Reporting Districts Containing 3,456,086 Los Angelenos			
	White or Other	**Black**	**Hispanic**
01 - Central	4,992	21,447	10,178
02 - Rampart	1,623	7,503	1,430
03 - Southwest	1,132	3,327	1,254
04 - Hollenbeck	975	1,509	1,409
05 - Harbor	1,402	3,724	1,862
06 - Hollywood	2,932	14,412	2,876
07 - Wilshire	2,104	3,792	2,003
08 - West L.A.	1,827	6,755	3,294
09 - Van Nuys	1,943	4,220	1,981
10 - West Valley	1,187	3,260	1,729
11 - Northeast	1,121	3,912	1,346
12 - 77th Street	1,954	3,715	1,344
13 - Newton Street	4,836	5,773	1,604
14 - Pacific	2,469	6,832	2,940
15 - North Hollywood	1,301	2,641	1,305
16 - Foothill	1,568	3,457	1,701
17 - Devonshire	1,207	3,056	2,169
18 - Southeast	1,254	3,748	1,110
Average	1,750	4,569	1,773

Table 2: Number of Arrests per 10,000 Residents 942 Reporting Districts Containing 3,456,086 Los Angelenos			
	White or Other	**Black**	**Hispanic**
01 - Central	514	7,165	1,514
02 - Rampart	261	2,840	316
03 - Southwest	50	368	120
04 - Hollenbeck	50	215	224
05 - Harbor	131	604	252
06 - Hollywood	347	4,020	497
07 - Wilshire	108	536	264
08 - West L.A.	96	912	380
09 - Van Nuys	151	674	282
10 - West Valley	114	542	289
11 - Northeast	78	802	218
12 - 77th Street	254	546	164
13 - Newton Street	447	1,007	169
14 - Pacific	267	1,167	518
15 - North Hollywood	142	428	279
16 - Foothill	166	701	281
17 - Devonshire	117	399	402
18 - Southeast	159	614	145
Average	159	844	277

	White or Other	**Black**	**Hispanic**
	Table 3: Number of Citations per 10,000 Residents		
	942 Reporting Districts Containing 3,456,086 Los Angelenos		
01 - Central	3,730	8,783	7,036
02 - Rampart	1,105	2,876	768
03 - Southwest	967	2,000	845
04 - Hollenbeck	788	843	791
05 - Harbor	950	1,565	1,054
06 - Hollywood	2,147	6,861	1,923
07 - Wilshire	1,826	2,085	1,343
08 - West L.A.	1,576	4,294	2,340
09 - Van Nuys	1,563	2,377	1,311
10 - West Valley	900	2,015	1,082
11 - Northeast	903	2,216	847
12 - 77th Street	1,223	1,493	799
13 - Newton Street	3,745	2,734	1,051
14 - Pacific	1,923	4,231	2,049
15 - North Hollywood	968	1,521	728
16 - Foothill	1,045	1,347	930
17 - Devonshire	896	1,970	1,426
18 - Southeast	763	1,615	653
Average	1,358	2,251	1,103

Table 4: Rate of Stops per 10,000 Residents			
N = 2825 (RD / Race Pairs)			
Black	3437.561 **	3437.580 **	3437.584 **
	12.66	12.7	12.73
Hispanic	359.156 **	359.165 **	359.169 **
	2.03	2.03	2.04
Violent Crime Rate	16.268 **	15.121 **	15.164 **
	19.98	16.86	16.73
Property Crime Rate	0.755 **	0.845 **	0.762 **
	3.93	4.36	3.9
Percent Black	-6383.787 **	-6009.155 **	-4808.607 **
	-12.38	-11.01	-7.34
Percent Hispanic	-1867.423 **	-941.424 **	83.801
	-6.18	-2.22	0.17
Percent Under 24		-4354.045 **	-2839.210 **
		-4.14	-2.24
Percent Unemployed		3598.202 **	3461.404 **
		2.12	2.04
Percent Poverty		502.709	855.359
		0.57	0.72
Percent Property Owner			-115.184
			-0.28
Percent Single Parent			-7983.661 **
			-4.35
Percent Divorced			2705.134
			0.91
Constant Term	1286.429	2047.579	1622.652
	8.57	7.81	2.83
R-squared	0.270	0.276	0.280

** Significance > 99%, * Significance > 95%
Disparities in stop rates of blacks and Hispanics controlling for crime rates, economic, and demographic characteristic. The numbers in the first two rows represent the estimated additional number of stops for blacks and Hispanics over whites, per 10,000 residents (and the correspondeing T-statistic). The three columns represent regressions controlling for an increasing number of variables. The minimal change in the stop rates indicates that the disparities persist even when controlling for these additional variables.

Table 5: Rate of Arrests per 10,000 Residents

N = 2825 (RD / Race Pairs)

Black	866.245 ** 11.76	866.252 ** 11.86	866.253 ** 11.91
Hispanic	160.843 ** 3.35	160.846 ** 3.38	160.848 ** 3.39
Violent Crime Rate	4.140 ** 18.75	3.541 ** 14.63	3.574 ** 14.63
Property Crime Rate	-0.158 ** -3.03	-0.110 ** -2.11	-0.140 ** -2.66
Percent Black	-1699.076 ** -12.15	-1658.409 ** -11.26	-1357.889 ** -7.7
Percent Hispanic	-373.059 ** -4.55	-139.584 -1.22	176.583 1.34
Percent Under 24		-1532.422 ** -5.41	-921.803 ** -2.7
Percent Unemployed		2166.463 ** 4.73	2112.995 ** 4.63
Percent Poverty		269.346 1.14	392.034 1.23
Percent Property Owner			-29.530 -0.27
Percent Single Parent			-2406.919 ** -4.87
Percent Divorced			1652.551 ** 2.07
Constant Term	151.053 3.71	373.030 5.27	117.094 0.76
R-squared	0.183	0.196	0.204

** Significance > 99%, * Significance > 95%

Disparities in arrest rates of blacks and Hispanics controlling for crime rates, economic, and demographic characteristic. The numbers in the first two rows represent the estimated additional number of stops for blacks and Hispanics over whites, per 10,000 residents (and the correspondeing T-statistic). The three columns represent regressions controlling for an increasing number of variables. The minimal change in the stop rates indicates that the disparities persist even when controlling for these additional variables.

Table 6: Rate of Citations per 10,000 Residents N = 2825 (RD / Race Pairs)			
Black	1306.785 **	1306.794 **	1306.797 **
	6.72	6.73	6.75
Hispanic	140.665	140.670	140.672 **
	1.11	1.11	1.11
Violent Crime Rate	8.443 **	8.157 **	8.170 **
	14.48	12.69	12.56
Property Crime Rate	0.985 **	1.006 **	0.964 **
	7.16	7.25	6.88
Percent Black	-3425.635 **	-3060.892 **	-2221.684 **
	-9.28	-7.82	-4.73
Percent Hispanic	-1536.321 **	-859.011 **	-282.816
	-7.1	-2.83	-0.81
Percent Under 24		-2387.874 **	-1726.125 *
		-3.17	-1.9
Percent Unemployed		480.987	427.007
		0.4	0.35
Percent Poverty		66.660	83.513
		0.11	0.1
Percent Property Owner			-159.489
			-0.54
Percent Single Parent			-4681.954 **
			-3.56
Percent Divorced			-175.408
			-0.08
Constant Term	1055.277	1551.727	1603.474
	9.82	8.26	3.9
R-squared	0.204	0.207	0.211

** Significance > 99%, * Significance > 95%

Disparities in citation rates of blacks and Hispanics controlling for crime rates, economic, and demographic characteristic. The numbers in the first two rows represent the estimated additional number of stops for blacks and Hispanics over whites, per 10,000 residents (and the correspondeing T-statistic). The three columns represent regressions controlling for an increasing number of variables. The minimal change in the stop rates indicates that the disparities persist even when controlling for these additional variables.

Table 7: Stop Rate Analysis of 942 Reporting Districts By Violent Crime Rate and By Minority (Black + Hispanic) Population (page 1 of 3)

Crime Rate		0-10% Population (Black/Hispanic/White)	0-10% Rate of Stops	10.1%-20% Population (Black/Hispanic/White)	10.1%-20% Rate of Stops	20.1%-30% Population (Black/Hispanic/White)	20.1%-30% Rate of Stops	30.1%-40% Population (Black/Hispanic/White)	30.1%-40% Rate of Stops
0 - 50	Black	4,097	7,835	9,759	5,047	6,875	3,914	5,863	2,720
	Hispanic	11,405	4,957	31,648	2,980	33,610	2,109	33,833	1,540
	White	216,674	1,310	247,905	1,186	118,391	1,388	76,458	1,146
51 - 100	Black	787	12,412	3,523	7,471	3,369	6,367	5,337	3,788
	Hispanic	2,205	5,619	9,008	4,477	11,347	4,117	30,355	2,009
	White	35,483	1,373	78,414	1,675	43,635	2,206	65,631	1,680
101 - 200	Black	192	12,834	1,227	22,120	771	6,831	2,972	4,387
	Hispanic	554	8,302	3,028	14,257	1,132	6,265	11,178	2,916
	White	10,811	1,658	25,676	3,705	6,906	2,359	24,163	1,784
200-400	Black	111	3,791	235	15,540	351	48,421	214	28,555
	Hispanic	175	4,461	888	11,158	1,110	18,026	896	8,134
	White	2,860	675	7,507	3,264	4,102	11,995	2,380	6,319
400-800	Black	-	-	-	-	20	302,934	-	-
	Hispanic	-	-	-	-	62	75,472	-	-
	White	-	-	-	-	279	34,278	-	-
800+	Black	-	-	-	-	-	-	-	-
	Hispanic	-	-	-	-	-	-	-	-
	White	-	-	-	-	-	-	-	-
% Hispanic * % Black		0-10%		10.1%-20%		20.1%-30%		30.1%-40%	

Table 7 (continued): Stop Rate Analysis of 942 Reporting Districts By Violent Crime Rate and By Minority (Black + Hispanic) Population (page 2 of 3)

Violent Crime Rate		40.1%-50%		50.1%-60%		60.1%-70%		70.1%-80%	
		Population	Rate of Stops	Population	Rate of Stops	Population	Rate of Stops	Population	Rate of Stops
0 - 50	Black	4,853	2,936	1,852	1,777	3,564	1,992	8,726	958
	Hispanic	28,081	1,529	25,907	869	32,386	1,135	57,086	1,102
	White	41,713	1,803	22,343	762	19,526	1,191	22,021	1,608
51 - 100	Black	7,199	3,316	6,104	4,610	13,240	3,016	9,275	2,957
	Hispanic	40,182	1,768	70,249	1,612	127,044	1,326	136,287	1,106
	White	59,034	1,637	60,935	1,510	74,796	1,413	47,888	1,604
101 - 200	Black	829	4,886	3,746	5,716	4,640	6,702	7,977	6,172
	Hispanic	5,748	3,041	26,735	2,301	13,689	2,924	41,737	2,685
	White	8,132	2,179	25,083	2,338	9,911	5,316	16,655	3,673
200-400	Black	639	9,621	279	15,141	353	32,203	961	23,552
	Hispanic	2,178	3,241	531	15,191	5,316	3,685	7,773	5,592
	White	3,420	5,666	594	10,403	3,471	9,647	3,179	4,973
400-800	Black	117	41,738	630	29,437	110	22,870	154	65,533
	Hispanic	334	26,667	2,251	8,850	1,158	3,005	101	81,960
	White	643	12,062	2,254	12,703	647	10,198	95	43,394
800+	Black	96	38,571	1,792	37,485	-	-	2,070	38,353
	Hispanic	275	48,842	1,462	43,967	-	-	2,310	15,465
	White	529	14,322	2,482	14,959	-	-	1,685	9,458
% Hispanic * % Black		40.1%-50%		50.1%-60%		60.1%-70%		70.1%-80%	

Table 7 (continued): Stop Rate Analysis of 942
Reporting Districts By Violent Crime Rate and By
Minority (Black + Hispanic) Population (page 3 of 3)

	Population	Rate of Stops	Population	Rate of Stops
	Black Hispanic White		Black Hispanic White	
0 - 50	7,995	1,400	5,252	1,299
	45,312	791	13,634	629
	7,031	1,072	1,247	1,949
51 - 100	40,977	2,998	27,871	2,957
	323,639	1,365	183,872	1,409
	37,468	2,256	10,475	3,052
101 - 200	107,528	3,333	90,424	3,314
	252,010	1,431	178,708	1,345
	24,864	2,198	10,899	1,946
200-400	104,939	4,576	99,126	4,423
	148,018	1,625	136,774	1,490
	9,666	3,742	7,479	3,093
400-800	6,056	6,690	5,620	5,463
	10,560	3,724	8,108	2,374
	922	11,413	401	4,192
800+	104	88,052	-	
	584	15,067	-	
	124	26,019	-	
% Hispanic * % Black	80.1%-90%		90.1%-100%	

Figure 1: Race Effect Interacted with Neighbourhood Minority Composition. Controlling for Violent and Property Crime Rates, Percent Residents Under 24 Years, Unemployment and Poverty Rates

Black

Hispanic

Additional Stops per 10,000 Residents (Compared to Whites)

6000
5000
4000
3000
2000
1000
0
-1000
-2000

Less than 33% Blacks/Hispanics

Between 33.33% and 66.67% Blacks/Hispanics

More than 66.67% Blacks/Hispanics
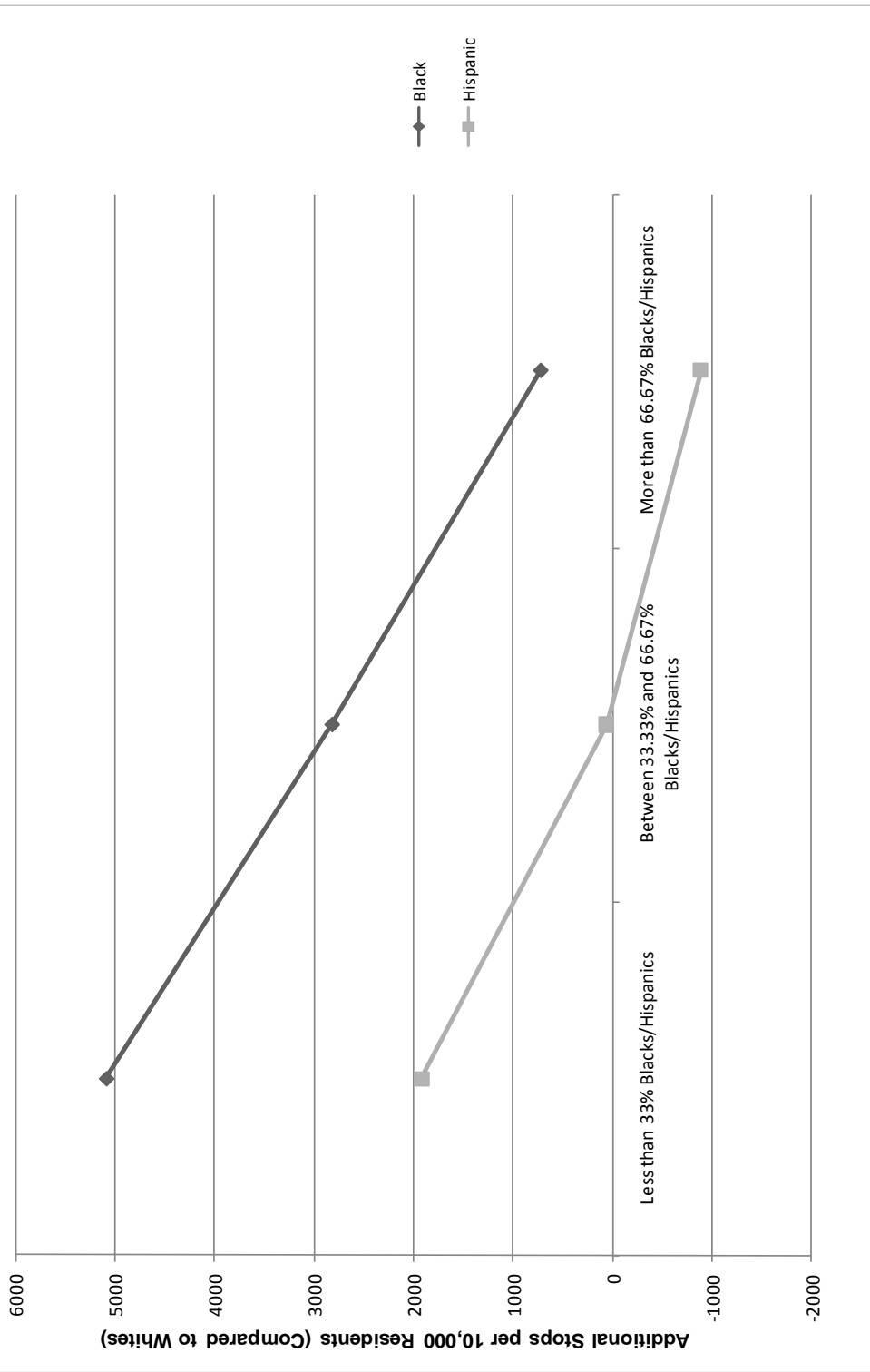
Table 8: Arrest Likelihood Given Stop				
Arrest Likelihood Given Stop	**Replication Model**	**Unrestricted Model**	**No Officer Attributes**	**RD Fixed Effects, No Officer Characteristics**
Black Effect	21.39% **	23.65% **	29.15% **	27.50% **
(T-statistic)	5.27	5.74	7.61	7.19
Hispanic Effect	28.53% **	29.73% **	32.35% **	32.60% **
(T-statistic)	11.68	12.09	16.49	16.25
Percentage Race Effects are 1 - Odds Ratio for the Recomposed Coefficient ** Significance > 99%, * Significance > 95%				

Table 9: Citation Likelihood Given Stop				
Citation Likelihood Given Stop	**Replication Model**	**Unrestricted Model**	**No Officer Attributes**	**RD Fixed Effects, No Officer Characteristics**
Black Effect	-33.24% **	-30.61% **	-31.62% **	-29.83% **
(T-statistic)	-13.67	-12.28	-14.69	-13.4
Hispanic Effect	-0.18%	2.53%	4.77% **	5.61% **
(T-statistic)	-0.1	1.41	3.44	3.94
Percentage Race Effects are 1 - Odds Ratio for the Recomposed Coefficient ** Significance > 99%, * Significance > 95%				

Table 10: Either Arrest or Citation Likelihood Given Stop				
Either Arrest or Citation Likelihood Given Stop	**Replication Model**	**Unrestricted Model**	**No Officer Attributes**	**RD Fixed Effects, No Officer Characteristics**
Black Effect	N/A	-20.94% **	-20.99% **	-19.87% **
(T-statistic)		-8.83	-10.09	-9.28
Hispanic Effect	N/A	11.35% **	14.18% **	13.79% **
(T-statistic)		6.5	10.57	10.07
Percentage Race Effects are 1 - Odds Ratio for the Recomposed Coefficient ** Significance > 99%, * Significance > 95%				

Table 11: Arrest Likelihood Given Stop	
	Results
Black Suspect/Non-Black Officer	26.88% **
(T-Statistic)	17.31
Black Suspect/Black Officer	17.73% **
(T-Statistic)	6.15
Hispanic Suspect/Non-Hispanic Officer	31.07% **
(T-Statistic)	20.68
Hispanic Suspect/Hispanic Officer	24.52% **
(T-Statistic)	14.91
Percentage Race Effects are 1 - Odds Ratio for the Recomposed Coefficient ** Significance > 99%, * Significance > 95%	

Table 12: Citation Likelihood Given Stop	
	Results
Black Suspect/Non-Black Officer	-24.98% **
(T-Statistic)	-23.29
Black Suspect/Black Officer	-4.53% *
(T-Statistic)	-2.07
Hispanic Suspect/Non-Hispanic Officer	10.15% **
(T-Statistic)	8.51
Hispanic Suspect/Hispanic Officer	12.41% **
(T-Statistic)	8.9
Percentage Race Effects are 1 - Odds Ratio for the Recomposed Coefficient ** Significance > 99%, * Significance > 95%	

Table 13: Frisk Likelihood Given Stop				
Frisk Likelihood Given Stop	**Replication Model**	**Unrestricted Model**	**No Officer Attributes**	**RD Fixed Effects, No Officer Characteristics**
Black Effect	105.85% **	104.24% **	126.64% **	119.87% **
(T-statistic)	22.1	21.75	28.56	27.17
Hispanic Effect	44.36% **	43.42% **	42.94% **	42.72% **
(T-statistic)	18.71	18.31	24.69	23.94
Percentage Race Effects are 1 - Odds Ratio for the Recomposed Coefficient				

Table 14: Likelihood of Police Request to Search Given Stop				
Likelihood of Police Request to Search Given Stop	**Replication Model**	**Unrestricted Model**	**No Officer Attributes**	**RD Fixed Effects, No Officer Characteristics**
Black Effect	51.18% **	50.68% **	76.36% **	70.04% **
(T-statistic)	9.57	9.45	14.52	13.45
Hispanic Effect	3.96%	4.38%	15.95% **	15.99% **
(T-statistic)	1.54	1.7	7.62	7.55
Percentage Race Effects are 1 - Odds Ratio for the Recomposed Coefficient				
** Significance > 99%, * Significance > 95%				

Table 15: Likelihood of Non-Consensual Search Given Stop				
Likelihood of Non-Consensual Search Given Stop	**Replication Model**	**Unrestricted Model**	**No Officer Attributes**	**RD Fixed Effects, No Officer Characteristics**
Black Effect	N/A	85.37% **	80.65% **	78.08% **
(T-statistic)		19.15	20.28	19.7
Hispanic Effect	N/A	95.07% **	76.77% **	75.13% **
(T-statistic)		36.62	39.85	38.66
Percentage Race Effects are 1 - Odds Ratio for the Recomposed Coefficient				
** Significance > 99%, * Significance > 95%				

Table 16: Likelihood of Request to Exit Vehicle, Given Stop				
Likelihood of Request to Exit Vehicle, Given Stop	**Replication Model**	**Unrestricted Model**	**No Officer Attributes**	**RD Fixed Effects, No Officer Characteristics**
Black Effect		163.375% **	165.911% **	
(T-statistic)		21.08	23.41	
Hispanic Effect		157.241% **	131.909% **	
(T-statistic)		40.49	45.33	
Percentage Race Effects are 1 - Odds Ratio for the Recomposed Coefficient ** Significance > 99%, * Significance > 95%				

Table 17: Likelihood of Arrest, Excluding Warrant, Violent and DUI, Given Stop				
Likelihood of Arrest, Excluding Warrant, Violent and DUI, given stop.	**Replication Model**	**Unrestricted Model**	**No Officer Attributes**	**RD Fixed Effects, No Officer Characteristics**
Black Effect		2.110%	13.710% *	
(T-statistic)		0.5	3.42	
Hispanic Effect		15.224% **	21.399% **	
(T-statistic)		5.59	9.85	
Percentage Race Effects are 1 - Odds Ratio for the Recomposed Coefficient ** Significance > 99%, * Significance > 95%				

Table 18: Likelihood of Citation, Excluding Lower Discretion Citation and Suspended Licence, Given Stop				
Likelihood of Citation, Excluding Lower Discretion Citation and Suspended Licence, Given Stop	**Replication Model**	**Unrestricted Model**	**No Officer Attributes**	**RD Fixed Effects, No Officer Characteristics**
Black Effect		-35.726% **	-36.664% **	
(T-statistic)		-14.98	-17.89	
Hispanic Effect		-9.875% **	-6.840% **	
(T-statistic)		-5.89	-5.3	
Percentage Race Effects are 1 - Odds Ratio for the Recomposed Coefficient ** Significance > 99%, * Significance > 95%				

Table 19: Likelihood of Higher Discretion Search, Given Stop				
Likelihood of Higher Discretion Search, Given Stop	**Replication Model**	**Unrestricted Model**	**No Officer Attributes**	**RD Fixed Effects, No Officer**
Black Effect		83.600% **	105.906% **	
(T-statistic)		11.72	15.33	
Hispanic Effect		10.027% **	12.343% **	
(T-statistic)		3.1	4.85	
Percentage Race Effects are 1 - Odds Ratio for the Recomposed Coefficient ** Significance > 99%, * Significance > 95%				

Table 20: Likelihood of Request to Exit Vehicle, Excluding Violent Arrests, Given Stop				
Likelihood of Request to Exit Vehicle, Excluding Violent Arrests, Given Stop	**Replication Model**	**Unrestricted Model**	**No Officer Attributes**	**RD Fixed Effects, No Officer Characteristics**
Black Effect		163.369% **	165.847% **	
(T-statistic)		21.07	23.39	
Hispanic Effect		157.396% **	132.072% **	
(T-statistic)		40.48	45.35	
Percentage Race Effects are 1 - Odds Ratio for the Recomposed Coefficient ** Significance > 99%, * Significance > 95%				

Figure 2a: Black/White Officer Arrest Disparity with 95% Confidence Interval

Officers
*One outlier was dropped
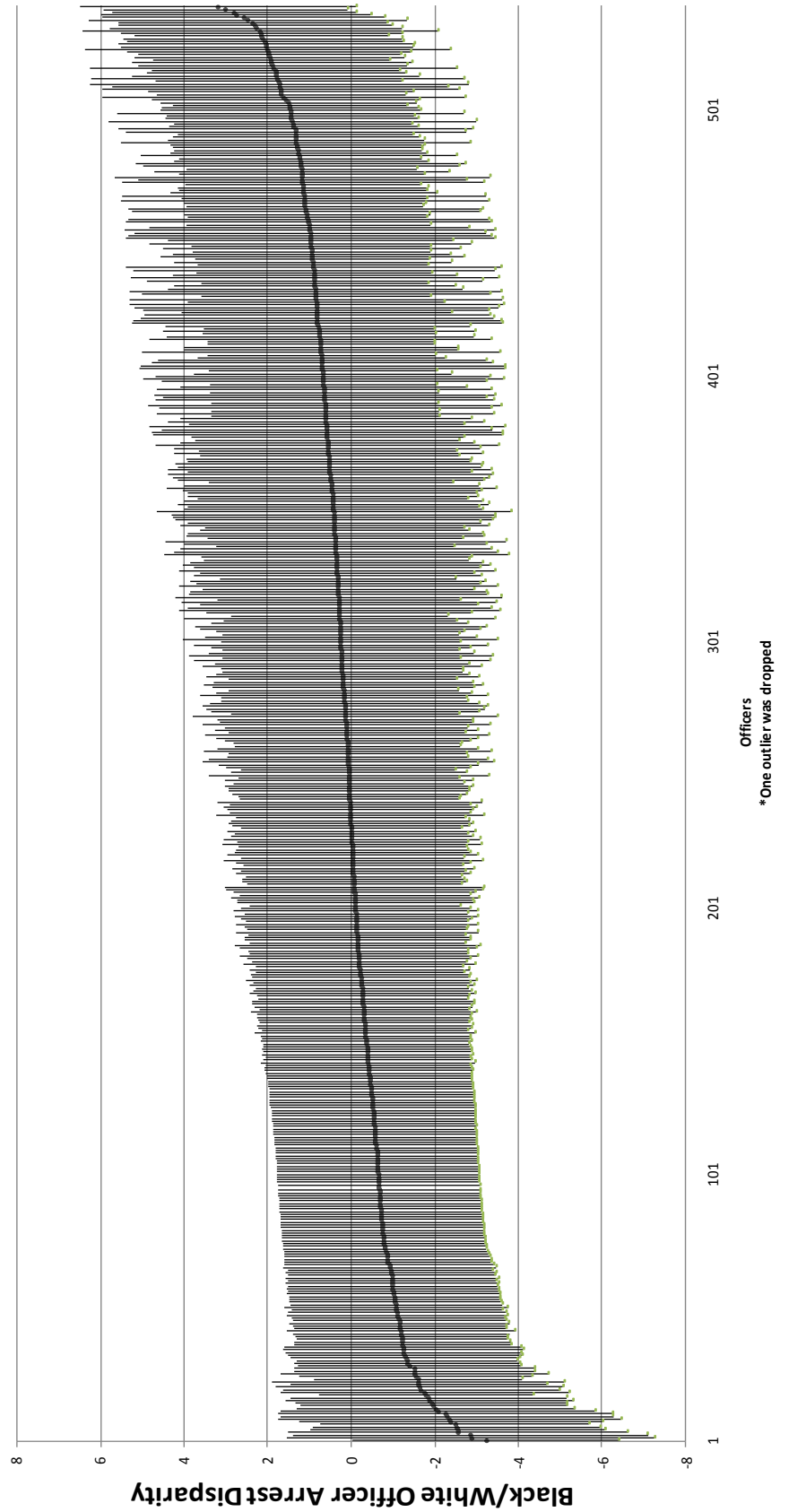
Figure 2b: Hispanic/White Officer Arrest Disparity with 95% Confidence Interval

Hispanic/White Officer Arrest Disparity

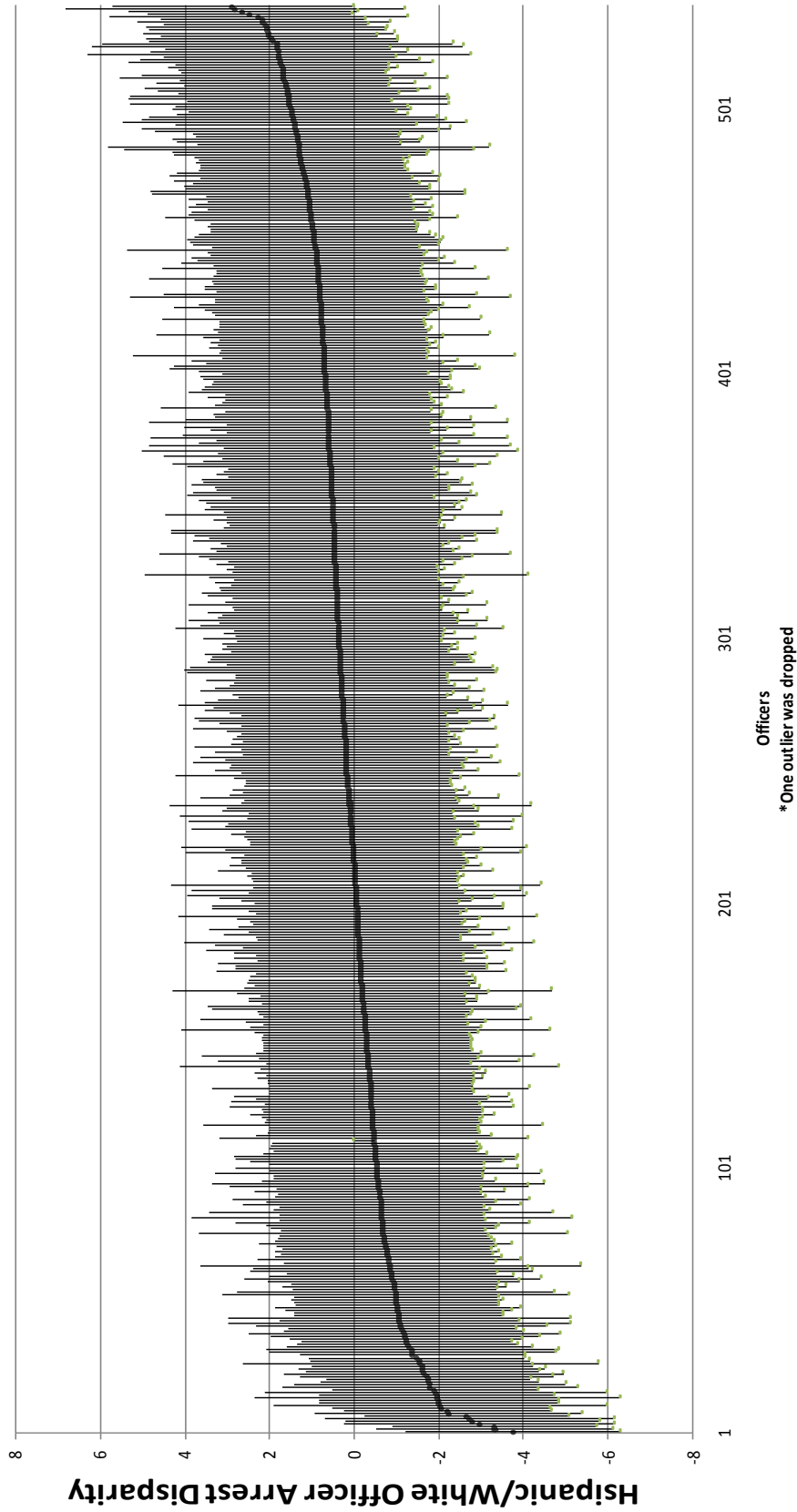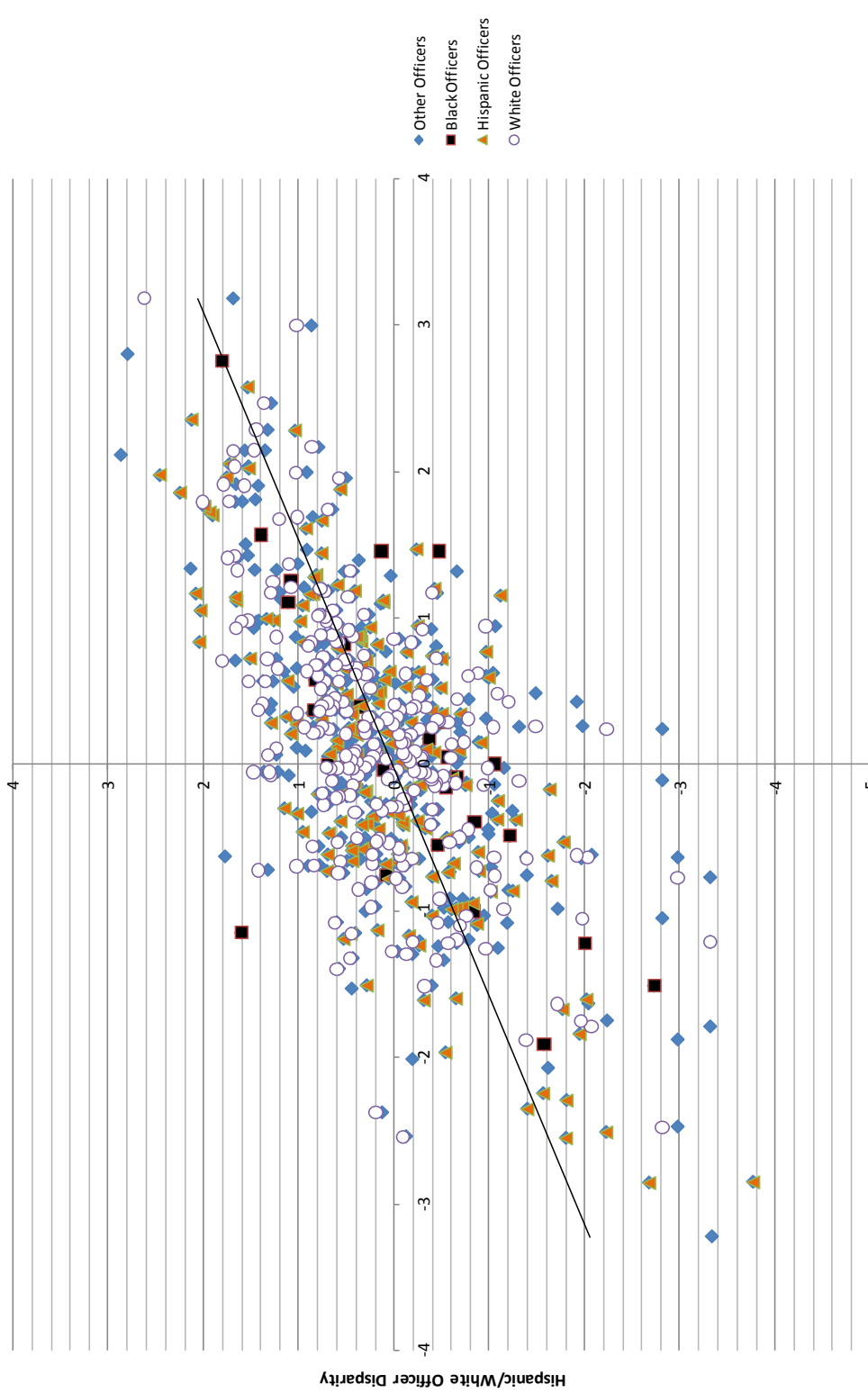Officers
*One outlier was dropped

Figure 3: Positive Correlation Between Black/White Officer Arrest Disparity and Hispanic/White Arrest Disparity

$y = 0.6429x + 0.0092$
$R^2 = 0.4013$

Other Officers
Black Officers
Hispanic Officers
White Officers

Hispanic/White Officer Disparity

Black/Whie Officer Disparity
*One outlier was dropped

	Black Restricted2	Black Restricted1	Black Full
Table 21: 2nd Stage Black Officer Effects			
N = 540			
Black	-0.093	-0.090	-0.091
	-0.48	-0.46	-0.47
Hispanic	-0.147	-0.147	-0.116
	-1.60	-1.60	-1.27
Asian	-0.130	-0.122	-0.073
	-0.93	-0.87	-0.52
American Indian	0.411	0.449	0.336
	-1.00	-1.09	-0.82
Count of Complaints Against Officer		0.015	0.014
		-1.00	-0.92
Count of Major Commendations Received by Officer		0.388 *	0.353 *
		2.45	2.22
Officer Male		0.012	-0.082
		-0.08	-0.58
Officer's Age			0.016
			-1.66
Number of Months of Service for Officer			-0.004
			-1.68
Number of Months of Service for Officer Squared			0.000
			-1.14
Officer Assignment- Other			0.165
			-0.35
Officer Assignment- Traffic			0.376 **
			3.41
Constant Term	0.337 **	0.263	-0.085
	5.79	-1.88	-0.28
R-squared	0.010	0.020	0.050
** Significance > 99%, * Significance > 95%			

Table 22: 2nd Stage Hispanic Officer Effects N = 540			
	Black Restricted2	Black Restricted1	Black Full
Black	-0.278	-0.261	-0.285
	-1.48	-1.39	-1.58
Hispanic	-0.123	-0.123	-0.104
	-1.39	-1.40	-1.22
Asian	-0.019	-0.020	0.039
	-0.14	-0.15	-0.30
American Indian	0.087	0.120	-0.008
	-0.22	-0.30	-0.02
Count of Complaints Against Officer		0.005	0.004
		-0.38	-0.31
Count of Major Commendations Received by Officer		0.394 **	0.305 *
		2.59	2.06
Officer Male		0.047	-0.120
		-0.35	-0.91
Officer's Age			0.009
			-1.03
Number of Months of Service for Officer			0.000
			-0.18
Number of Months of Service for Officer Squared			0.000
			-1.50
Officer Assignment- Other			-0.231
			-0.52
Officer Assignment- Traffic			0.620 **
			6.05
Constant Term	0.309 **	0.227	-0.032
	5.53	-1.68	-0.11
R-squared	0.010	0.020	0.110
** Significance > 99%, * Significance > 95%			

Table 23: Frisk: Likelihood that a Weapon Found			
	Race Only	**Controlling for Stop Characteristics**	**Controlling for Stop Characteristics and RD Characteristics**
Black Effect	-42.272% **	-39.858% **	-38.359% **
(T-statistic)	-6.33	-5.12	-4.80
Hispanic Effect	-31.812% **	-28.209% **	-24.092% **
(T-statistic)	-4.69	-3.83	-3.14
Percentage Race Effects are 1 - Odds Ratio for the Race Coefficient. Searches that are incident to either arrest or vehicle impound are excluded. ** Significance > 99%, * Significance > 95%			

Table 24: Frisk: Likelihood that Drugs Found			
	Race Only	**Controlling for Stop Characteristics**	**Controlling for Stop Characteristics and RD Characteristics**
Black Effect	-25.353% **	-2.353%	-3.828%
(T-statistic)	-7.09	-0.52	-0.84
Hispanic Effect	-38.156% **	-35.582% **	-33.635% **
(T-statistic)	-11.67	-10.07	-9.18
Percentage Race Effects are 1 - Odds Ratio for the Race Coefficient. Searches that are incident to either arrest or vehicle impound are excluded. ** Significance > 99%, * Significance > 95%			

Table 25: Frisk: Likelihood that Anything Else Found			
	Race Only	**Controlling for Stop Characteristics**	**Controlling for Stop Characteristics and RD Characteristics**
Black Effect	-32.754% **	-2.177%	-1.749%
(T-statistic)	-11.49	-0.57	-0.45
Hispanic Effect	-15.133% **	-0.853%	1.761%
(T-statistic)	-5.03	-0.25	0.50
Percentage Race Effects are 1 - Odds Ratio for the Race Coefficient. Searches that are incident to either arrest or vehicle impound are excluded. ** Significance > 99%, * Significance > 95%			

Table 26: Consent Search Request: Likelihood that a Weapon Found			
	Race Only	**Controlling for Stop Characteristics**	**Controlling for Stop Characteristics and RD Characteristics**
Black Effect	-36.975% **	-39.110% **	-36.318% **
(T-statistic)	-3.98	-3.75	-3.35
Hispanic Effect	-32.828% **	-32.650% **	-28.115% **
(T-statistic)	-3.60	-3.36	-2.76
Percentage Race Effects are 1 - Odds Ratio for the Race Coefficient. Searches that are incident to either arrest or vehicle impound are excluded. ** Significance > 99%, * Significance > 95%			

Table 27: Consent Search Request: Likelihood that Drugs Found			
	Race Only	**Controlling for Stop Characteristics**	**Controlling for Stop Characteristics and RD Characteristics**
Black Effect	-23.669% **	-3.439%	-4.520%
(T-statistic)	-5.64	-0.65	-0.84
Hispanic Effect	-34.294% **	-30.474% **	-27.800% **
(T-statistic)	-8.86	-7.28	-6.41
Percentage Race Effects are 1 - Odds Ratio for the Race Coefficient. Searches that are incident to either arrest or vehicle impound are excluded. ** Significance > 99%, * Significance > 95%			

| Table 28: Consent Search Request: Likelihood that Anything Else Found			
	Race Only	**Controlling for Stop Characteristics**	**Controlling for Stop Characteristics and RD Characteristics**
Black Effect	-25.400% **	-6.654%	-5.557%
(T-statistic)	-6.83	-1.41	-1.16
Hispanic Effect	-12.256% **	-4.318%	0.345%
(T-statistic)	-3.21	-1.03	0.08
Percentage Race Effects are 1 - Odds Ratio for the Race Coefficient. Searches that are incident to either arrest or vehicle impound are excluded. ** Significance > 99%, * Significance > 95%			
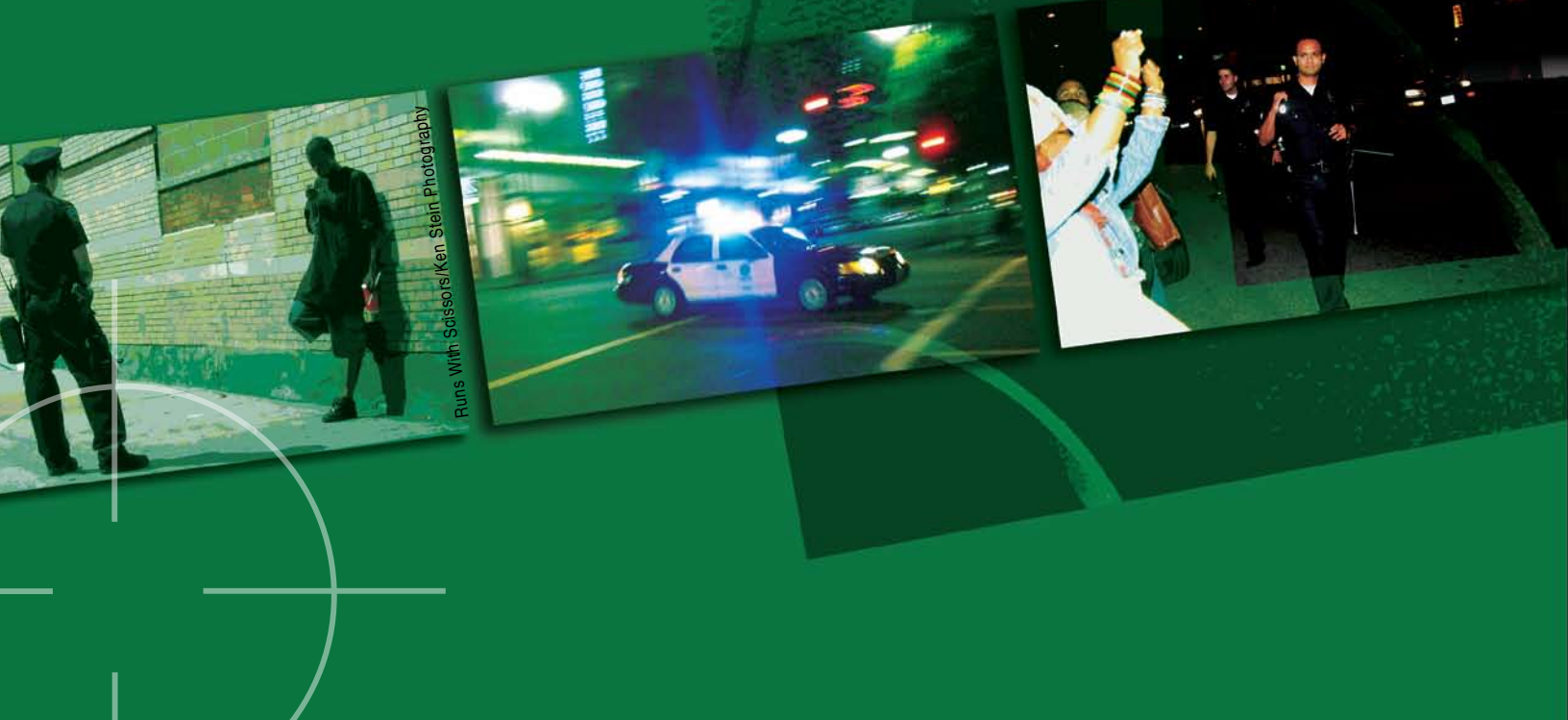
Runs With Scissors/Ken Stein Photography