Stops and Stares: Street Stops, Surveillance and Race in the New Policing

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Abstract

The use of proactive tactics to disrupt criminal activities, such as Terry street stops and concentrated misdemeanor arrests, are essential to the “new policing.” This model applies complex metrics, strong management, and aggressive enforcement and surveillance to focus policing on high crime risk persons and places. The tactics endemic to the “new policing” gave rise in the 1990s to popular, legal, political and social science concerns about disparate treatment of minority groups in their everyday encounters with law enforcement. Empirical evidence showed that minorities were indeed stopped and arrested more frequently than similarly situated whites, even when controlling for local social and crime conditions. In this article, we examine racial disparities under a unique configuration of the street stop prong of the “new policing” – the inclusion of non-contact observations (or surveillances) in the field interrogation (or investigative stop) activity of Boston Police Department officers. We show that Boston Police officers focus significant portions of their field investigation activity in two areas: suspected and actual gang members, and the city’s high crime areas. Minority neighborhoods experience higher levels of field interrogation and surveillance activity net of crime and other social factors. Relative to white suspects, Black suspects are more likely to be observed, interrogated, and frisked or searched controlling for gang membership and prior arrest history. Moreover, relative to their black counterparts, white police officers conduct high numbers of field investigations and are more likely to frisk / search subjects of all races. We distinguish between preference-based and statistical discrimination by comparing stops by officer-suspect racial pairs. If officer activity is independent of officer race, we would infer that disproportionate stops of minorities reflect statistical discrimination. We show instead that officers seem more likely to investigate and frisk or search a minority suspect if officer and suspect race differ. We locate these results in the broader tensions of racial profiling that pose recurring social and constitutional concerns in the “new policing.”
I. INTRODUCTION

A. The New Policing

In an essay published in 2000 in the *Fordham Urban Law Journal*, Professor Philip Heymann credited the “new policing” for the sharp crime declines of the preceding decade.1 Heymann was not alone in noting the sea change in policing. In 1990, at the peak of a homicide epidemic in the U.S., Professor Herman Goldstein argued that police should actively engage with communities to solve the recurring problems in small places that were animating stubborn crime problems.2 Several scholars, including one of us, had argued for policing “hot spots,” an effort to bring to scale policing models that featured allocation principles that matched police resources to the small areas that seemed to have recurring crime problems.3 Some police departments had revised their strategies to target crime markets and other locations of recurring crime.4 Others relied on research showing that aggressive enforcement of minor crimes – usually through arrest – deterred crime by signaling the risks of detection and punishment to criminal offenders.5

Professor Debra Livingston had summarized this vector of tactics that comprised the “new policing” in a 1997 essay, but stopped short of crediting the police for producing the crime decline.6 She discussed the new police focus on order maintenance and the aggressive enforcement of quality-of-life crimes7, on the enforcement of minor

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7 See Id. The policy also focused intensively on misdemeanor drug crimes, especially marijuana possession, as part of the Broken Windows strategy. Part of the logic of such enforcement was also to find more serious criminal offenders among those committing such minor crimes. Jack Maple and Chris Mitchell, *The Crime Fighter: How You Can Make Your City Safe* (2000). Maple and Mitchell claimed that marijuana arrests would prevent more serious crimes since
crimes as a way to suppress more serious crimes, and on stop and frisk tactics to disrupt crimes and especially to seize weapons, and on new developments in community policing that brought police into closer collaborative relationships with citizens. She also noted the importance of the strategic concentration of police resources in the city’s highest crime areas based on new methods of crime mapping and analysis.\(^8\) Heymann’s essay took a broader view, endorsing both community engagement to solve the problems that generated persistent crime, and prospective solutions that anticipated crime problems and took prophylactic steps to prevent them.\(^9\) Aggressive street stops were one of the tactics highlighted by both Livingston and Heymann.\(^10\)

Heymann credited the “new policing” for the dramatic reductions in crime in the 1990s. He was not alone. One study showed a sharp decline in gun violence starting in the early 1990s, both nationally and in New York City, and gave partial credit to new police tactics in New York City and elsewhere.\(^11\) Others were more full-throated in their endorsement of police as the essential element in crime declines. Professors George Kelling and Catherine Coles cited the place-based policing tactics built on Broken Windows theory of disorder and crime\(^12\) as the engine driving local crime declines in three case studies.\(^13\) Professors Hope Corman and Naci Mocan credited aggressive policing in the form of misdemeanor arrests for drug crimes for the reduction in murder and other violence in New York City the 1990s.\(^14\) Reflecting on sharp reductions in citizen calls for serious crimes generated by a policing disorder strategy targeted on crime hot spots in Lowell, Massachusetts, Professors Anthony Braga and Brenda Bond

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suggested that their results provided some support to claims that new policing strategies deserve some credit for reducing serious crime in New York City and elsewhere.\(^\text{15}\)

Other scholars were less certain about the role of the police in bringing about crime declines during that era, citing crime declines in several cities under a variety of policies that varied in the centrality and tactics of the police.\(^\text{16}\) These dissenters claimed that comparisons with cities that used methods other than these “new policing” tactics enjoyed the same crime declines after controlling for the number of police and other correlates. Several years later, and despite the contentious and unresolved debate over the effectiveness of the “new policing,” a National Academy of Sciences panel endorsed the “hot spots” model as the most effective and efficient approach to reducing crime.\(^\text{17}\) A recent systematic review of the evaluation evidence on hot spots policing also found that these programs generated significant reductions in crime.\(^\text{18}\) Still, research on “hot spots” emphasized the value of targeting places, but was largely silent on what police should do (tactically) in those places.

Three essential features characterized the “new policing.” First, police innovators developed real-time policing metrics both for internal personnel management and to inform how and where police are deployed across their respective cities.\(^\text{19}\) Second, these metrics were used to hold local police commanders accountable for crime trends in their precincts.\(^\text{20}\) Failure to lower crime rates resulted in a form of public shaming in meetings

\(^{15}\) Anthony A. Braga and Brenda Bond, Policing Crime and Disorder Hot Spots: A Randomized Controlled Trial, 46 Criminology 599 (2008).


\(^{19}\) Heymann, supra note 6.

of high-level police executives, and possibly demotion and re-assignment. Accountability and heightened management control were essential tools to incentivize commanders to closely watch and react to local crime conditions.

The third tool of the policing was the use of proactive tactics to disrupt criminal activities. The U.S. Supreme Court had expanded the legal boundaries under which police could conduct field interrogations, or street stops, in a 1968 decision, *Terry v Ohio.*\(^{21}\) *Terry* permitted temporary stops and detentions based on reasonable suspicion that crime was “afoot,” supplanting the more demanding probable cause standard\(^{22}\) and memorializing police discretion as the gateway to street stops.\(^{23}\) Subsequent decisions further expanded the tolerances around the concept of “reasonable suspicion.”\(^{24}\) The “new policing” embraced the use of street stops as a critical tool to disrupt criminal activity, despite the absence of any evidence of the comparative advantage of street stops over other policing tactics.\(^{25}\)

Proactivity was the animating theory of the “new policing,” whether in the context of data-driven management metrics such as CompStat\(^{26}\), a computerized crime accounting system, or in the aggressive use of arrests for minor crimes, or the conduct of street stops at the first signs of suspicious behavior. A Google NGram on “proactive policing” shows the first mentions in the late 1960s and the peak number of mentions in 2000 before dropping slowly through 2008.\(^{27}\) In the current era, the term was first used without fanfare by Professors Jerome Skolnick and David Bayley in their description of

\(^{21}\) 368 U.S. 1, 21-2 (“The reasonableness of any particular search and seizure must be assessed in light of the particular circumstances against the standard of whether a man of reasonable caution is warranted in believing that the action taken was appropriate.”) See, infra Section II and accompanying notes.

\(^{22}\) *Mapp v Ohio,* 367 U.S. 632 (1960) (reaffirming the probable cause standard under the Fourth Amendment to justify a “search and seizure.”)

\(^{23}\) *Terry* at 301

\(^{24}\) See Section II and accompanying notes.

\(^{25}\) A decade later, the NRC policing panel conspicuously avoided the question of what police should do once they got to the targeted places. However, a recent systematic review and meta-analysis of the effects of hot spots policing on crime suggests that community problem-solving approaches generated stronger crime reduction impacts relative to increased enforcement efforts. See Braga, et al., *The Effects of Hot Spots Policing,* supra note 23 at 633.

\(^{26}\) David Weisburd, et al., *Reforming To Preserve: CompStat and Strategic Problem Solving In American Policing,* 2 Criminology & Public Policy 421-456 (2003) (showing that many police departments adopted elements of the “new policing” without incorporating the metrics-driven management algorithms for targeting and assessment of police actions. In fact, the authors critique management metrics as a retarding organization reform and reinforcing the paramilitary model of police innovation).

\(^{27}\) Google Ngram on Proactive Policing. Ngrams show the frequency of mentions of a word or phrase by year for a specified period. See, for example, “Google Ngram Database Tracks Popularity Of 500 Billion Words” *Huffington Post,* 17 December 2010, at [http://www.huffingtonpost.com/2010/12/17/google-ngram-database-tra_n_798150.html](http://www.huffingtonpost.com/2010/12/17/google-ngram-database-tra_n_798150.html). The most common application currently is through Google’s website, a tool that was initially developed at the Cultural Observatory at Harvard.
policing innovations in the 1980s.\textsuperscript{28} New York City police first used proactive policies to disrupt open-air drug markets starting in the early 1990s.\textsuperscript{29}

Over time, proactivity became a broad umbrella for a wide range of police tactics. One study defines “proactive policing” as the vigorous enforcement of law against minor (misdemeanor) offenses.\textsuperscript{30} Other studies mention the use of stop and frisk, or investigative stops, as central to a proactive policing policy.”\textsuperscript{31} Still others portray a curious admixture of drug enforcement and community policing as “proactive.”\textsuperscript{32} Accordingly, there is no consensus on what constitutes “proactive policing” other than its emphasis on anticipation of criminal activity and directing action to those places or persons, and its commitment to systematic criminal enforcement of minor crimes.\textsuperscript{33} Tactics such as investigative stops (stop and frisk, or \textit{Terry} stops)\textsuperscript{34}, order maintenance and aggressive responses to quality of life enforcement, the same tactics cited by Livingston, are basic to proactive policing.

B. Policing Crime, Policing Race

The metrics of the “new policing” pointed to the neighborhoods with the highest crime rates as the targets of police activity. These usually were the places with concentrated poverty and often were minority neighborhoods.\textsuperscript{35} At first glance, this

\begin{itemize}
  \item \textsuperscript{28} Jerome Skolnick and David Bayley, \textit{The New Blue Line: Police Innovation in Six American Cities} (1986) (discussing a shift in police tactics from being reactive to crime complaints toward acting to response to chronic criminal problems in specific places).
  \item \textsuperscript{29} Zimmer, \textit{Proactive Policing}, supra note 9 at 43.
  \item \textsuperscript{30} Charis Kubrin, et al., \textit{Proactive Policing and Robbery Rates across U.S. Cities}, 48 Criminology 57 (2010).
  \item \textsuperscript{31} Jeffrey Fagan, et al., \textit{Street Stops and Broken Windows Revisited: The Logic and Demography of Proactive Policing in a Safe and Changing City}, 309 Race, Ethnicity, and Policing (Stephen Rice and Michael White, eds. 2010).
  \item \textsuperscript{33} The original “broken windows” essay, whose ideas informed much of the next decade of proactive policing, suggested that arrest was a last resort if other efforts failed to ameliorate the disorderly conditions that invited crime. Wilson and Kelling, \textit{Broken Windows}, supra note 17 at 29. By 2000, Kelling had embraced the notion of using arrest authority systematically and aggressively to stop minor crime from growing into more serious crime patterns and problems. See, Kelling and Coles, \textit{Fixing Broken Windows}, supra note 18 at 108-156.
  \item \textsuperscript{34} See, infra Section II and accompanying notes.
\end{itemize}
seems a rational and proportional response, consistent with most benchmarking strategies to assess fairness or bias. Yet, regardless of the distribution and allocation function to assign police to neighborhoods – a linear allocation of police to neighborhoods based on differences in their crime rates, for example – disproportionate allocations raise both fairness and efficiency questions. In such instances, minority citizens are exposed to “more” policing than their crime conditions would dictate, and persons in other neighborhoods placed at risk due to under- or de-policing of their neighborhoods. Assuming limits on the effectiveness of police in an area – after all, there is only so much crime to go around – then over-policing risks adverse consequences from unnecessary and unproductive police contacts. And since these stops are neither pleasant nor without consequences37, allocations framed this way raise constitutional questions of disparate treatment.38

The fact remains that prophylactic police street stops and misdemeanor arrests typically are concentrated in those areas by design, creating ambivalence and anger among local residents. The anger stemmed from two sources. First, while welcoming police presence, local residents also became angered at the harsh tone and style of investigative stops.39 In the “new policing,” involuntary police-citizen contacts rarely were gentle or neutral, nor were they intended to be that way.40 Interviews across cities


with Black and Latino young persons reveal similar stories of violence, property
destruction, racial and sexual degradation, and unlawful detentions as routine experiences
in their encounters with police. Similar reports came from studies of highway stops,
where drivers were humiliated in the course of unproductive pretextual stops and car
searches. A 2012 study by the Civilian Complaint Review Board in New York City
shows that the police precincts with the highest number of civilian complaints against
officers had the highest stop and frisk rates. The same study showed that 25-30 percent
of all complaints alleged abuse of authority by police when conducting these street
encounters. Commissioner William Bratton recently summed up these noxious
interactions as “lawful but awful.”

A second source of resentment and anger resulted from the poor returns from
these unpleasant interactions. In Philadelphia, New York, and Los Angeles, hundreds
of thousands of stops resulted in few seizures of weapons or other contraband.

the police officer and his sergeant, Cruz asked for the reason why the officers were arresting him.
One replied: “For being a fucking mutt!”). See, also, Peter Moskos, Cop in the Hood (2008) at
114-115; Bernard E. Harcourt, Unconstitutional Police Searches and Collective Responsibility, 3
invoked a drug-enforcement rationale to stop a suspect without any indicia of drug use or
possession and proceeded to conduct a fruitless cavity search).

See, Rod K. Brunson and Ronald Weitzer, Police Relations with Black and White Youths in
routine use of violence by officers in conducting investigative stops). See also generally Victor
M. Rios, Punished: Policing the Lives of Black and Latino Boys (2011) (describing the gratuitous
humiliation by police during investigative stops by police officers such as waiting until youths
purchased food before seizing it for search and throwing it on the ground); Patrick Carr, et al., We
Never Call the Cops and Here is Why: A Qualitative Examination of Legal Cynicism in Three

degradation and racially selective enforcement in stops and searches in vehicle highway stops)

Jennifer Cunningham, Complaints against NYPD Cops Mirror Stop and Frisk Numbers, New
cops-mirror-stop-and-frisk-numbers-article-1.1388735.

NYC Civilian Complaint Review Board, Stop, Question and Frisk, July 2013, at

Bratton characterized the July 2014 killing of Eric Garner by Officer Daniel Pantaleo by a
chokehold as “lawful but awful,” summarizing the decision of the Grand Jury to not indict the
officer. See, http://www.cbsnews.com/videos/nypd-commissioner-department-reviewing-eric-
garner-case/ (December 7, 2015). He repeated the phrase again on February 13, 2015, to
characterize stop and frisk tactics and other order maintenance policing actions that are the core
features of the New York City policing model, and to acknowledge the racial animosity that such
comments-on-police-minority-relations-resonate/

See Section XX and accompanying notes, infra, on dignitarian concerns

Jeffrey Fagan and John MacDonald, Policing, Crime and Legitimacy in New York and Los
Angeles: The Uncertain Future (D. Halle and A. Beveridge, eds. 2013). Ian Ayres and Jonathan
federal civil rights trial in New York revealed that the burden for the extremely low gun or other contraband seizure rates – about one gun for every 1,000 stops, one drug seizure for every 100 stops – fell primarily on African American and Latino persons.\textsuperscript{48} Highway stops generated similarly poor returns in drug seizures, again in a regime where stops and searches were conducted with African American and Latino motorists.\textsuperscript{49} One study showed lower “hit rates” of drug seizures from minority motorists on New Jersey highways by the State Police despite the higher number of stops of those drivers.\textsuperscript{50} Drug searches on the streets and highways were particularly irritating for minority citizens given the racial balance in the use of controlled substances.\textsuperscript{51}

Overall, the poor returns from these stops, both on the streets and highways coupled with their racial concentration led to bitterness and a racial divide in distrust of legal authorities, especially the police.\textsuperscript{52} The persistence of these errors in the context of the extensive use of these tactics suggests that these practices and disparities took on the characteristics of a government program rather than the exercise of individual officers’

\begin{footnotesize}
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\textsuperscript{48} Floyd opinion. See, infra sections II.B and accompanying notes.


\textsuperscript{51} Data from the National Survey of Drug Use and Health (NSDUH) showed very small differences in marijuana use rates between black and white teenagers, and lower rates among Hispanics. 2004 and 2005 NSDUH data from the Substance Abuse and Mental Health Services Administration (SAMHSA) reports that 6.7% of whites in large metropolitan counties report past-month marijuana use, while 7.9% of blacks and 4.9% of Hispanics do. Reports in the same surveys of cocaine and heroin use show similar balance across racial and ethnic groups.

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judgment and discretion. These stories became common knowledge, fueled in part by the quick media dissemination of images and details of tragic events. Word spread fast. The effects of well-publicized troubling events in police misconduct were felt and shared vicariously beginning with the Rodney King video as these depictions traveled through information networks in digital sources. Whether directly or vicariously affected, the shared experiences from interactions with the police contributed to a broad perception that racial profiling was deeply rooted in law enforcement treatments of minorities.

The subjective perception of racially selective enforcement and harsh treatment among African Americans and Latinos gave rise to policy, constitutional and political questions of racial profiling.

C. Is it Bias?

The debate on whether or how bias infects the street stop prong of the “new policing” has occupied both litigation and social science. The large number of court-sanctioned consent decrees and other policing litigation outcomes over the past two decades suggests that in many places, the practice of street stops produces racial disparities that meet the constitutional standard for disparate treatment or racially selective enforcement. But given differences in race-specific rates of violent and other serious crime, the question of bias either institutionally or among individual officers is more difficult to discern. Disparities need not reflect either bias or even

56 Epp et al., supra note 42; Lawrence Bobo and Devon Johnson, A Taste for Punishment: Black and White Americans’ Views on the Death Penalty and the War on Drugs, 1 DuBois Review 170 (2004).
57 Jerome Skolnick and Abigail Caplovits, Guns, Drugs and Profiling, 43 Ariz. L. Rev. 413, 416-7 (2001) (discussing a pattern of citizen complaints, litigation, low poll ratings and widespread protests, and other signs of discontent with policing that crystalized in the term “racial profiling.”)
disproportionality. Elevated rates of stops or discretionary arrests or other police actions could reflect a rational strategy of officers to maximize public safety. But those rates could also reflect either the individual or aggregate actions of police officers with a taste for punishment.

Disparate treatment is an observable fact, but the whether it is motivated by implicit bias, policy, or rational preferences raises additional questions and distinct research strategies focusing more closely on the connections between officers’ (and police executives’) perceptions and their actions. The biases could be explicit, or they may be implicit biases among officers which animate them to see either culpability or suspicion more often among minorities and in minority neighborhoods, and more important, to translate that suspicion into action. Some studies have used officer race as a control to estimate officer bias. These studies assume that officers’ within-race encounters will differ from cross-race encounters in decisions both on how to conduct the encounter and the outcome of the encounter.

Court rulings often skirt the question of whether bias is the dynamic that produces disparities, preferring instead to examine discriminatory intent. The Fourteenth Amendment’s Equal Protection Clause forbids state actors from denying the equal protection of the law. Intentional discrimination by race is the standard, not simply whether a policy or practice has a disproportionate racial impact. Whether that intent is a matter of bias or preferences is not central to a legal determination, intent instead is the predicate to determine discrimination. Courts have developed standards to establish discriminatory intent that would satisfy an equal protection claim, such as intentionally classifying persons by race for differential treatment.

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62 U.S. Const. Amend. XIV § 1


64 See, for example, Brown v. City of Oneonta, New York, 221 F.3d 329 (2d Cir. 2000) (permitting a permissible racial classification for police only when police are pursuing a person meeting a specific suspect description). Normally, all such classifications are evaluated at the highest standard of strict scrutiny. See, e.g., Loving v. Virginia, 388 U.S. 1, 11 (1967); Johnson v. California, 543 U.S. 499, 505 (2005) (holding that “all racial classification” imposed by government “must be analyzed by a reviewing court under strict scrutiny”)).
contemporary policing is an “as applied” determination: that a seemingly neutral policy is applied in an intentionally discriminatory manner. Courts have argued that an equal protection claim is satisfied by evidence of a discriminatory “purpose” as a “motivating factor” for the practice under scrutiny. The question of bias is secondary to the question of the complex task of discerning discriminatory intent. In the case of a widespread program of Terry stops or street stops, an intentionally disproportionate application of the stop authority to persons of one race raises the specter of intent.

Courts that find constitutional violations on race generally apply a standard of “preponderance of evidence” – often based on statistical analyses – to decide equal protection – or disparate treatment – violations in policing cases. The evidence in these cases is usually based on specific studies that reflect the institutional, criminological and sociological conditions in each place. Still, there remain several empirical challenges to detecting bias in the institutional practices of law enforcement agencies and in the actions of individual police officers. Control groups, benchmarks to assess proportionality, endogeneity of crime and policing, varying spatial boundaries, individual biases versus institutional practices, police-citizen interpersonal interactions, characteristics of officers and the composition of both the civilian and police populations, multiple outcomes of stops and processes within stops, and several important contextual factors all contribute to the challenges facing researchers. All these parameters are basic to design decisions in assessing the extent of police discrimination. Designs focusing on specific cities are essential to assess the unique policy contexts in these places.

D. Stops and Stares: The Surveillance Prong of the “New Policing”

In this article, we examine the extent of racial disparities under a unique configuration of the street stop prong of the “new policing” – the use of Field Interrogation, Observation, Frisk and/or Search (BPD form 2487; hereafter, FIO) reports by the Boston Police Department (BPD). As in many other big cities, the Boston FIO strategy included the basic element of investigative stops or field interrogations as a staple of its proactive tactics.

Beyond this core tactic, however, two additional components distinguish the Boston design from most forms of the “new policing.” First, Boston officers conduct non-contact observations of known criminal offenders or others gathering in known

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65 See, for example, Brown v Oneonta, id at 337. See, also, Yick Wo v Hopkins, 118 U.S. 356 (1886).


“Because discriminatory intent is rarely susceptible to direct proof, litigants may make “a sensitive inquiry into such circumstantial and direct evidence of intent as may be available. The impact of the official action — whether it bears more heavily on one race than another — may provide an important starting point.”

67 Meares, Programming Errors (2015), supra 58.
crime locations. Officers are required to complete FIO reports for both in-person encounters and non-contact observations. Officers are required to enter the information from non-contact observations or surveillance in the same databases that house data from field interrogations (or investigative stops). Department policy requires that observations be more than a stakeout or a hunch. That is, the observation must be specific to a suspected crime, rather than general surveillance of individuals. Whether that stipulation is honored in practice is doubtful, as we show later on.

Second, the policy explicitly recognizes the role of surveillance and intelligence-gathering in the local strategy. Surveillance of known offenders and their associates and their gathering places “plays an important part in the department’s intelligence efforts to collect and disseminate data on the activities and whereabouts of known and suspected criminals and their associates in the city.” In effect, the strategy allows the department to build a network database of the movements and associations of individuals through time and space in the city. The observations also are considered documentary evidence and therefore admissible if relevant in any future proceedings.

Few police departments have acknowledged the potential for intelligence-gathering that is created from Terry stops, much less how the data could be aggregated and analyzed for that purpose. Even fewer acknowledge the Fourth Amendment implications on privacy and anonymity. While intelligence-gathering is not unusual in policing in response to known crime groups such as street gangs or drug selling organizations, the transformation of Terry stops into an intelligence regime seems

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68 See, Boston Police Department, Special Order SO 05-023, June 3, 2005, Sec. 1: “An officer should….complete an FIOFS report whenever (a) he/she observes an individual who (sic) the officer knows to have a criminal record.”

69 Id. at Sec. 4: “An officer shall complete an FIOFS report whenever: (a) he/she observes, detains, or interrogates a person suspected of unlawful design….The officer must enter the type of crime suspected….[I]t is not sufficient to simply enter ‘suspicious person’ or ‘SP’.” See, also, Boston Police Department, Rules and Procedures, Rule 323 (March 9, 2011).

70 Special Order SO -5-023, at Sec. 1: “The FIOFS report has been prepared so that the department may accumulate up-to-date information concerning known criminals and their associates, the vehicles they use, the places they frequent, and persons suspected of unlawful design.”

71 Id. at General Considerations.

72 Boston Police Department, Rules and Procedures, Rule 323, Section 1.

73 Christopher Slobogin, Privacy at Risk: The New Government Surveillance and the Fourth Amendment, 79 (2007). See, specifically, Chapter 4, Public Privacy: Surveillance of Public Places and the Right to Anonymity (discussing the elasticity of the reasonable suspicion prong of Fourth Amendment doctrine to accommodate contemporary surveillance practices by police of gang members and others suspected of potential criminal participation).

74 William Bloss, Escalating U.S. Surveillance after 9/11: An Examination of Causes and Effects, 4 Surveillance & Society 208 (2002) (documenting the expansion of police surveillance in response to perceived threats from crime, drug selling and national security concerns). See, also, Debra Livingston, Gang Loitering, the Court, and Some Realism about Police Patrol, Supreme Court Review 141 (1999) (expressing concern that granting police authority to focus on public
somewhat new. While surveillance of individuals or gangs is permissible, surveillance without a warrant for places and persons wandering through them raises constitutional concerns.  

Pretextual stops made on thin grounds for reasonable suspicion can create opportunities to record time-place-network activities of suspected offenders or other valued targets such as gang members or drug traffickers. Surveillance without contact is another matter. Recorded observations by officers who have knowledge of the identity and affiliations of that target can in effect double down on the information gathered through in-person contacts. These non-contact observations can in fact lead to further contacts, assuming usable intelligence that can be linked to specific persons or groups. For example, the U.S. Customs service, under former NYPD Commissioner Raymond Kelly, used “intelligence developed by another officer” to target persons for searches.

The observational or intelligence components of the “new policing” have received little attention in either legal or empirical scholarship on policing, or on the constitutional implications of these types of “passive” stops. Certainly, there are Fourth Amendment implications when police use prior suspicionless observations as a partial basis for a later interdiction that risks arrest and its aftermath. Chicago’s constitutional troubles with its gang loitering ordinance show the difficult line that police face when determining when to escalate observation of non-criminal conduct such as loitering into reasonable

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75 See, generally, Orin Kerr, The Fourth Amendment and New Technologies: Constitutional Myths and the Case for Caution, 102 Mich. L. Rev. 801 (2004); Katz v. U.S. 389 U.S. 347 (finding that there is no Fourth Amendment violation when evidence is obtained without “physical entrance into the area”); But see Kyllo v. United States, 533 U.S. 27 (2001) (holding that the use of a thermal imaging device from a public position to monitor the radiation of heat from a person’s home was a ”search” under the Fourth Amendment, and thus required a warrant).


77 Since there is no seizure of the subject, Terry’s ground rules for street detentions may not be applicable to non-contact observations. Yet observations can be bootstrapped by police officers into information that can serve as the basis for the reasonable suspicion that Terry requires. See, Terry v Ohio, 392 U.S. 1 (1968)

78 United States General Accounting Office, Better Targeting of Airline Passengers for Personal Searches Could Produce Better Results, GAO/GGD-00-38, 87 (2000), at 5-6, 16 (cited in Jerome Skolnick and Abigail Caplovits, Guns, Drugs and Profiling, 43 Ariz. L. Rev. 413, 433 (2001))

79 See, Commonwealth v. Cruz, 945 N.E.2d 899, 908 (Mass. 2011). Cruz was ordered to exit a vehicle when police officers spotted it parked next to a fire hydrant and then smelled marijuana smoke. Possession of small amounts of marijuana had been decriminalized in Massachusetts since 2008. Cruz had fallen under the police gaze in this instance because of his prior encounters with police, and those encounters heightened their suspicion leading to the search of the vehicle for what amounts to a civil infraction. See, also, David Keenan and Tina M. Thomas, Redefining Fourth Amendment Reasonableness: A Crime-Severity Model for Terry Stops, 123 Yale L.J. 1448 (2013).
suspicion of a crime that would justify a coercive stop.\textsuperscript{80} If there is no notice to citizens about what conduct might authorize police attention, it may be equally unclear to the police. There also are distinct Equal Protection implications when individuals are disproportionately targeted by race for observations that can lead to more intrusive or coercive police actions in the short term. Racial disparities in this passive component of the new policing would be compounded in subsequent police contacts, given the leveraging of intelligence into further stop activity. The deeper (though not wider)\textsuperscript{81} pool of information may well increase the odds of an in-person contact, raising additional questions about disparities and the potential for a constitutional claim.

Surveillance is fundamental to the street stop component of the “new policing.” The sequence of interactions leading to an investigative stop, or a contact stop, often include a period of observation of a suspect before an officer decides if there is sufficient suspicion under the Fourth Amendment to proceed to contact and interaction with the suspect.\textsuperscript{82} In most places, these predicate observations are not recorded if the surveillance doesn’t convert to a contact stop; only those observations that trigger actions are included in databases.\textsuperscript{83} This is what sets the Boston FIO regime apart from other forms of the new policing: its expansion under Fourth Amendment ground rules to include surveillance of the type that normally is reserved for national security concerns or complex criminal organizations. Here, the everyday movements of persons fall under the police gaze and are memorialized in databases. Whether these observations are contributory to disparate treatment under the Equal Protection clause on their own or in conjunction with direct contacts is the focus of this paper.

E. This Article

Empirical tests for Equal Protection violations in policing have become more

\textsuperscript{80} \textit{Morales v City of Chicago}, 521 U.S 41 (holding that a gang loitering ordinance is vague as to what behaviors would be actionable for police and therefore encourages arbitrary and discriminatory enforcement). See, Livingston, \textit{Gang Loitering}, supra note 78 at 163 and accompanying notes.

\textsuperscript{81} The pool is deeper in the sense that more information is gathered about the same person or persons over time. But if an area or group is targeted, the information is narrowly focused on person or one social network or one or more groups of persons moving through a targeted space. While \textit{Wardlow} requires presence in a high crime area as an component of suspicion, passive stops based on presence in a high crime area de-temporize that presence and disconnect it from other indicia of suspicion that are present at the time of the street stop. See, \textit{Illinois v Wardlow}, 528 U.S. 119 (2000).

\textsuperscript{82} See, Jeffrey Fagan and Amanda Geller, \textit{Following the Script}, supra note 80.

\textsuperscript{83} See, for example, NYPD SQF database, various years, at \url{http://www.nyc.gov/html/nypd/html/analysis_and_planning/stop_question_and_frisk_report.shtml}. The databases include only in-person stops or field interrogations. For each stop, the period of observation preceding the stop is also recorded. Nearly all of the observation periods are less than two minutes. Fagan and Geller, \textit{Following the Script}, supra note 80.
common and urgent as political and legal challenges to the “new policing” have grown.\textsuperscript{84} Research to date on vehicle and highway stops raises difficult questions as to the construction and vetting of claims of racial discrimination. These difficulties have been compounded and perhaps have become more complex with the increase in attention to policing inequalities by the U.S. Department of Justice,\textsuperscript{85} and civil litigation on a variety of statutory and constitutional grounds.\textsuperscript{86} These cases have generated new databases and competing analytic strategies to identify the causal role of race in the observed disparities in policing. In this article, we examine the role of race in explaining how pre-arrest coercive policing and passive surveillance-based policing are carried out in an urban setting of complex and varied crime problems.

The addition of passive or surveillance-based policing models expands the underlying normative tensions in policing by placing citizens under the police gaze in a setting more closely approximating a panopticistic vision of policing.\textsuperscript{87} Police surveillance of organized crime groups and political dissidents has a long history, with court interventions that established procedural and substantive boundaries on these activities.\textsuperscript{88} Incorporating surveillance into a prophylactic Terry regime, whether for street gangs or other loosely organized offender networks or for everyday criminal or delinquent activity, merges acute public safety and national security concerns into everyday policing. This spillover from national security to the “new policing,” raises important questions about how equal protection doctrine applies. The further dimension of racial disparity that seems inherent in street stop regimes, deepens the stakes in this analysis of the “new policing” by linking race and national security rationales.\textsuperscript{89}

The article unfolds in the following five sections. Section II provides background on the transformation of police and the emergence of proactive policing since Terry.


\textsuperscript{85} Rachel Harmon, Promoting Civil Rights through Proactive Policing Reform, 62 Stan. L.Rev. 1-68 (2009) (discussing the limitations of “retail” litigation under §1983 and other individual cases to produce institutional reform in policing and arguing for the stronger, institutionally-focused mechanisms of 42 U.S.C. § 14141 to produce meaningful institutional changes.)

\textsuperscript{86} Joanna Schwartz, Police Indemnification, 89 NYU L. Rev. 885 (2014) (showing results of litigation under Monell liability claims for police misconduct).


\textsuperscript{88} Benjamin S. Mishkin, Filling the Oversight Gap: The Case for Local Intelligence Oversight, 88 NYU L. Rev. 1414 (2013).

\textsuperscript{89} Paul Chevigny and Paul Chevigny, The Edge of the Knife: Police Violence in the Americas (New York Press, 1995).
Despite the efforts of the Terry court to scrub race from its analysis of the original Terry stop, the role of race in the jurisprudence of Terry stops has become clearer over time, leading to the important cases today that closely connect Terry, race and proactive policing. We trace the political and normative conflicts over racial profiling into the courts, and also into the sociological analysis of race and policing. We draw a distinction between discrimination and bias, and show the divide in empirical strategies to test the two explanations of observed racial disparities.

Section III describes in detail the research site, and the integration of surveillance into the regime of proactive policing in Boston. Section IV discusses the research strategy to identify the role of race in producing the patterns of policing that we observe in Boston. We weigh alternatives facing empirical researchers in measurement and analysis of these data and explain the methodological choices that we made. We distinguish the role of race in surveillance versus in-person encounters. We discuss the use of officer race as a metric to identify the extent of racial preferences among officers. We also discuss modeling choices for data where policing is spatially disaggregated across parts of the city that vary extensively in their social and crime condition.

Section V presents the empirical results. We show that contact stops and non-contact observations of Black suspects are more common than for white suspects after controlling for local crime and social conditions in Boston neighborhoods. The patterns are robust to several alternate empirical specifications. The local emphasis on surveillance and interdiction of gang members explains some but not all of the racial disparities in the conduct of FIO’s; these disparities are present across most tactical units and locales in the BPD. Diversity in policing matters in these estimates: Black officers appear to be less active in FIO reporting than their white colleagues, both in general patrol activities as well as in the specialized gang enforcement units. They make fewer stops of Black suspects and search Black suspects less often. The results suggest that Black and Hispanic officers seem to act with statistical discrimination whereas White officers seem to act based on preferences for discrimination that are present beyond what statistical discrimination would suggest. We show continuity in racial disparities in police contacts from the general Terry regime of street stops to the expanded surveillance activities, indicative of the broader expansion of Terry doctrine over the past half century.

We conclude in Section VI with a discussion two intersecting implications of this case study: efficiency and fairness. As a preliminary question, we assess the adequacy and probative of this empirical strategy to detect equal protection violations in the conduct of stops coupled with a more arms-length program of surveillance. The new policing inevitably will produce racially disparate impacts, given crime patterns and the actuarialism that is baked into its strategy and logic. Its expansion to surveillance, which sidesteps Terry’s rules, raises new questions about the constitutional regulation of the “new policing.” The devolution of Terry to a program of both surveillance and administrative stops raises important questions for the regulation of this activity and more
broadly for the governance of police with respect both to privacy considerations and for its racial dimensions.\textsuperscript{90}

\section*{II. Investigative Stops in American Policing}

\subsection*{A. Terry v. Ohio and the Re-invention of Police Street Stops}

Investigative stops have been a staple of American policing since the first municipal police departments were formed in the 19\textsuperscript{th} century, when slave patrols first roamed the streets of Northern U.S. cities checking the credentials of African Americans to verify their status as free men.\textsuperscript{91} For the first century of urban policing, there was little constitutional or political regulation of stops, nor of other police procedures including searches, arrests and interrogations.\textsuperscript{92} By the late 1950s, at the outset of the civil rights era in American political and legal culture and with the beginnings of an uptick in urban crime, investigative stops and searches had become a contentious fact of policing.\textsuperscript{93} As public discourse about crime became increasingly racialized in that era, civil rights advocates framed these issues not only in terms of the limits of state intrusions on


\textsuperscript{91} H.M. Henry, The Police Control of the Slave in South Carolina (Vanderbilt University, 1914). As Tracey Maclin points out, the racial profiling debate involves practices that date back to colonial times. Tracey Maclin, \textit{Race and the Fourth Amendment}, 51 Vand. L. Rev. 333, 336 (1998) ("Today, police departments across the nation... continue to target blacks in a manner reminiscent of the slave patrols of colonial America."). See John S. Dempsey and Linda S. Forst, \textit{An Introduction to Policing} 10 (2011) ("Policing experts conclude . . . that the patrol function and concept were first accepted as a police practice by slave patrols in the South."). Victor E. Kappeler and Larry K. Gaines, \textit{Community Policing: A Contemporary Perspective} 62 (6\textsuperscript{th} ed., 2011) (describing slave patrols as precursors to modern-day police departments); Michael J. Palmiotto & N. Prabha Unnithan, \textit{Policing and Society: A Global Approach} 172 (Cengage Learning, 2010) (discussing slave patrols and noting that “America’s experience with police-race relations can be traced back to 1619 when the first slaves from Africa arrived in Jamestown.”). Sally E. Hadden, \textit{Slave Patrols: Law and Violence in Virginia and the Carolinas} 19 (Cambridge, MA: Harvard University Press, 2001) (“The disparity between what masters would allow slaves (typically, a Sunday free from work and a pass to visit town) and what the town might tolerate . . . generated security concerns.”).

\textsuperscript{92} There was some constitutional regulation of police interaction with minors in the years first half of the 20\textsuperscript{th} century. In \textit{Haley v Ohio}, 332 U.S. 596 (1948), the Supreme Court found that a confession obtained from a 15 year old African American boy who was interrogated from midnight to 5AM by a relay of police detective was invalid. The court ruled that Haley’s 14\textsuperscript{th} Amendment due process rights were violated when police failed to advise the boy of his rights. The Court noted that “[a]ge 15 is a tender and difficult age for a boy of any race” and that a boy in his position was “no match for the police” in the “overpowering presence of the law”(Haley at 600). In \textit{Gallegos v Colorado} (370 U.S. 49 (1962), citing Haley, the five-day interrogation and subsequent confession of a 14 year old boy also deprived the suspect of his due process rights.

\textsuperscript{93} U.S. Senate, Committee on the Judiciary, 1959
privacy, but as a matter of racial discrimination and racial justice. Accordingly, when the Supreme Court decided Mapp v Ohio, requiring probable cause to justify police searches and excluding evidence obtained under lesser proof, it did more than simply extend Fourth Amendment rights to criminal suspects. The decision placed limits on police that re-balanced security and liberties in contexts that disproportionately affected minority suspects.

While Mapp limited the circumstances under which police could search suspects, police retained the authority to conduct temporary street stops and investigations whose intrusions stopped short of search. As crime rates increased in the ensuing years, primarily in the nation’s urban centers, stops and other encounters between minority citizens and police became more common and more contentious. Many of the riots that scarred the inner cities of dozens of American cities from 1964-8 were sparked by anger over police abuse of Black citizens. The importance of investigative stops in policing in that era clashed deepened the tensions surrounding the practice.

Police and prosecutors pushed back on the Mapp decision, arguing that public safety was being harmed by the limitations on police for stops and searches. The tension that brewed since Mapp nearly boiled over politically in the ensuing years. In a 1968 case, the core issues on the limits on police investigative stops were revisited in Terry v Ohio, where the U.S. Supreme Court relaxed the conditions under which police could conduct investigative stops and temporary street detentions. The Court, by an 8-
margin, decided that the police stop of John Terry was reasonable by comparing it to activity that would typically require a warrant:

“In justifying the particular intrusion the police officer must be able to point to specific and articulable facts which, taken together with rational inferences from those facts, reasonably warrant the intrusion.”  

_Terry_ granted police broad authority to conduct investigative stops when they have reasonable and particularized suspicion to believe that crime is imminent, in progress, or has just occurred. Once a stop began, _Terry_ authorized police to frisk the suspect’s person, usually with a pat-down, if the officer feared the suspect is armed or if the officer believes that her safety is otherwise at risk. _Terry_ relaxed the standards for conducting warrantless searches or seizures to allow these brief street interrogations and detentions. _Terry_ also permitted searches of the suspect’s belongings or person if the officer formed probable cause that the suspect is carrying contraband or weapons. The _Terry_ Court “agonized” over its decision, worrying about an excess of “petty indignities” for suspects when police power was used too broadly or aggressively.

_Terry_ lowered the probable cause requirement of _Mapp_, replacing it with a standard of reasonable suspicion that “crime is afoot” to justify police intrusions on citizens’ freedoms. _Terry_ was both a practical and political decision, where the Court struggled to balance fairness and efficiency in law enforcement, mindful of the volatile social and historical context of the late 1960s. The resulting compromise retained the

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104 _Terry v. Ohio_, supra 106 at 16, 17. The warrant requirement was the predicate for the comparison of McFadden’s stop of Terry to the probable cause standard. The Court reasoned that in a situation where a police obtained a warrant, the facts and inferences that were required of a judge or magistrate to issue a warrant were equivalent to the analysis of the officer who decides to exercise the stop power. The Court required that the facts be not only reasonable but also articulable and individualized to the specific situation. These facts and inferences, in turn, would facilitate judicial regulation of Terry stops in appellate review.

105 _Terry v. Ohio_, supra 106. Officers could temporarily detain and question a suspect when they had reasonable and particularized suspicion to believe that crime was “afoot.”


107 The _Terry_ opinion anticipated that stops would be confined to situations where officers had reasonable suspicion that there was (a) an ongoing or prospective offense (a crime) that threatened violence to persons or property. See, David Keenan and Tina Thomas, _Redefining Fourth Amendment Reasonableness: A Crime-Severity Model for Terry Stops_, 123 Yale L. J. 1 1118 (2014).


109 See John Q. Barrett, _Deciding the Stop and Frisk Cases: A Look Inside the Supreme Court’s Conference_, 72 St. John’s L. Rev. 749, 839 (1998) (concluding that “[m]any thus think of Terry and the law of ’stop and frisk’ as . . . a sensible balancing of public interests in law enforcement
exclusionary rule but granted broader discretion under the Fourth Amendment to police officers to decide the circumstances when citizens could be detained and interrogated.\textsuperscript{110} Perhaps most important to this article, the racial dimensions of \textit{Terry} were lost in its aftermath, despite its implications for everyday policing in the social and demographic parameters of crime in the following decades.\textsuperscript{111}

B. \textit{Terry}, Race and Law

The issues of race that were minor features of the original \textit{Terry} case became explicit concerns over the next several decades in the law, politics and policy of policing.\textsuperscript{112} As part of the Violent Crime Control Act of 1994, the Congress included a provision that allowed the U.S. Department of Justice to sue local law enforcement agencies when it observes a pattern or practice of systemic violations of people’s rights.\textsuperscript{113} Litigation under “14141” beginning in the 1990s identified both Fourth and Fourteenth Amendment violations in racial profiling, leading to court supervision of both state and local police agencies.\textsuperscript{114} Consent decrees were approved by federal district

against relatively lesser intrusions on personal freedom”). See, also, Michael J. Graetz and Linda Greenhouse, \textit{Unequal Protections: The Lasting Legacy of Warren Burger’s Supreme Court} (2016, forthcoming)

\textsuperscript{110} \textit{Terry}, supra note 106. See, William J. Stuntz, \textit{Terry’s Impossibility}, 72 St. John’s L. Rev. 1213, 1213–15, 1217 (1998) (arguing that any attempt to legally regulate street policing is prone to error since courts are incapable of systematically accounting for the realities of why police engage in certain types of behaviors).


\textsuperscript{113} The \textit{Violent Crime Control and Law Enforcement Act of 1994}, 42 U.S.C. § 14141. Section 14141 provides, in part, that:

[It shall be unlawful for any governmental authority, or any agent thereof, or any person acting on behalf of a governmental authority, to engage in a pattern or practice of conduct by law enforcement officers . . . that deprives persons of rights, privileges, or immunities secured or protected by the Constitution or laws of the United States.

courts in three jurisdictions in the 1990s, imposing obligations on local police departments to reform policy and practice to remedy constitutional violations including race discrimination in both stops and the use of force.

The first consent decrees focused on police use of force in Pittsburgh, PA., and Stubenville, OH. The first consent decree alleging racial profiling was formalized with a state police agency in late 1999 in New Jersey, citing constitutional violations in the selection of motorists for stops and searches on the New Jersey Turnpike. A 1999 investigation by the New York State Attorney General cited both Fourth and Fourteenth Amendment violations by the New York City Police Department in its conduct of Terry stops under its “stop and frisk” regime. In 2001, the Los Angeles Police Department finalized a consent decree to remedy a pattern of “false arrests, using excessive force, conducting stops without reasonable suspicion, and …… improper searches and seizures.” Between 2002 and 2013, consent decrees were implemented in 15 cities across the country, revealing a set of systemic concerns about both the Fourth Amendment core of Terry and the more hidden Fourteenth Amendment prong.

However, after several decades that witnessed the transformation of Terry stops from a legal practice to a program and policy, its tensions remain an unfortunate feature of urban policing. A less regulated police stop power was exercised increasingly in minority communities and with persons of color, or on highways with disproportionate

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122 Tracey Meares, Programming Errors supra note 53; Charles Epp, et al., Pulled Over, supra note 43; Bernard Harcourt, Against Profiling, supra note 111.
stops of non-White motorists. Controversies over the racial prong of Terry’s “reasonable suspicion” standard arose shortly after Terry in a case where the Supreme Court justified the use of Mexican or Latino ethnicity to sanction police stop authority near the U.S.-Mexico border. That case, in conjunction with U.S. v. Brignoni-Ponce, a case decided a year earlier, are the only U.S. Supreme Court cases to specifically sanction the use of race or ethnicity in the decision to stop an individual under the Fourth Amendment rules stated in Terry.

A similar logic of profiling based on race and ethnicity was internalized in the early 1980s in drug enforcement training that led to racial disproportionality in highway stops. In 1996, the U.S. Supreme Court sanctioned pretextual rationales motivating automobile stops in Whren v. U.S. While not explicitly a rationale for the use of race in selective enforcement, the Court refused to consider any factors other than objective considerations that animated the officer’s actions. In other words, having satisfied probable cause requirements under the Fourth Amendment, a search motivated by race would not pose a constitutional violation. Whren effectively separated Fourteenth Amendment equal protection considerations from Fourth Amendment protections against unreasonable searches, a separation that was repaired in Floyd v. City of New York in 2013. We discuss this case in more detail infra.

While maintaining its ban on the explicit use of race as an objective factor to justify stops, the Supreme Court expanded its analysis of race and policing in Illinois v. Wardlow (2000) to permit police stops based in part on robust correlates of race. The Court noted that although an individual’s presence in a “high crime area” does not meet the standard for a particularized suspicion of criminal activity, a location’s characteristics are relevant to determining whether a behavior is sufficiently suspicious to warrant further investigation. But the Wardlow court offered little guidance to police and courts to define a “high crime area,” in terms of its boundaries, crime levels, duration of crime, type of crime, or whether the standard varies across cities or parts of the

126 David A. Harris, The Stories, supra note 122. See, Samuel Gross and Debra Livingston, Racial Profiling Under Attack, supra note 111.
127 517 U.S. 806, (1996) (citing Robinson v U.S. 436 U.S. 138, stating that “the fact that the officer does not have the state of mind which is hypothesized by the reasons which provide the legal justification for the officer's action does not invalidate the action.”).
country. The Wardlow court also left it up to the judgment not just of police officials but of the individual officer to determine whether the location where she encountered a person whose behavior attracts her gaze meets the definition of “high crime area.” In other words, Wardlow created a vague and subjective standard that would be difficult to regulate either institutionally or by courts. Since “high crime areas” and social disadvantage often are conflated both perceptually and statistically with concentrations of minority citizens, this logic places minority neighborhoods at risk for elevating the suspiciousness of their residents in the eyes of the police.

C. Profiling and Discontent

By the mid-1990s, almost three decades after Terry, controversies about the inflation of what courts validate as suspicion after Terry arose on two fronts. One source of controversy was the use of race as a marker of suspicion leading to police stops on highways and streets. Consent decrees in Maryland, New Jersey and Los Angeles, and a Stipulated Settlement in New York City, all were based foundations of empirical evidence of racially selective police enforcement. Racial profiling became a fundamental component of both legal and popular culture. Figure 1 shows that mentions in books alone increasing 350% from 1990 to 2008. Its mention in news stories and other journalism outlets began with one story in 1987 and exponentially through 2000:

“Only one other use of racial profile showed up before 1990; between 1990 and the start of 1994, 8 uses; from 1994 until 1996, there were 31 hits; 1996 until 1998, there were 63; from 1998 until 1999, there were 187; and then an enormous spike occurred from 1999 to 2000, over 1000 hits; and from January 2000 through October 1, 2000, over 1000 again.”


\[133\] See, for reviews, Bernard Harcourt, Against Prediction; Charles Epp et al, Pulled Over; Harris, Statistics; Banks et al, Race and the War on Drugs; Ian Ayres and Jonathan Borowsky; LAPD; Jeffrey Fagan and Garth Davies, 2000; Joanna Schwartz, Myths and Mechanics of Deterrence.

\[134\] Jerome H. Skolnick and Abigail Caplovitz, Guns, Drugs and Profiling: Ways to Target Guns and Minimize Racial Profiling, 43 Ariz. L. Rev. 413, 419 and n.6 (2001).
Most of these mentions were critical. The term was used to label police practices that either animated litigation or were politically contested. Other mentions simply showed the downside risks of profiling for false positives but also greater dignitarian harms. Rarely were the benefits of profiling mentioned as an aide to law enforcement or as contributing to public safety. More likely, uncritical discussions of racial profiling – usually embedded in a larger social and legal context of the war on drugs – saw race-based selection of individuals for police attention as an unfortunate byproduct or necessity of the empirical “realities” of drug-related crime and violence. Through all the contentious debates, however, no empirical study could identify a statistically sound effect of the crime control benefits of profiling.

Profiling controversies also led to political action. Racial disparities in street and highway stops prompted the U.S. Department of Justice to promote best practices for police agencies to collect and analyze data on police stops. Adoption of these measures was less than widespread, and data were collected in some places only under the pressures of pending or threatened litigation. Across the U.S., 17 states developed and implemented systems for collecting and reporting vehicle and street stop activity. In some instances, the rapid expansion of data collection mechanisms resulted in research scholarship that allowed for testing of criminological theories of policing, race and social contexts. The resulting research was extensive, and spanned the range of social science disciplines while raising contentious normative and philosophical debates.

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135 See, e.g., Nedra Pickler, Judge Robert Wilkins, Obama Nominee, Successfully Sued Md. State Police For Racial Profiling, Huffington Post, August 2013, http://www.huffingtonpost.com/2013/06/04/judge-robert-wilkins_n_3386013.html (describing the 1992 highway stop of Federal District Court Judge Robert Wilkins where Maryland State Troopers forced him to stand outside his vehicle in the rain while waiting for a canine unit to show up to conduct a search of his automobile. Wilkins, a public defender at the time, was returning from an all-night journey home following his grandfather’s funeral. Wilkins missed a court date the following morning due to his detention on the highway.)

136 See, e.g., William Stuntz, Race, Class and Drugs, 98 Columbia L. Rev. 1795 (1999). See, also, Skolnick and Caplovitz, id., for a more detailed accounting of the racial prong of street and highway stops.


139 See, Illinois v Chavez litigation. Data collection in New York City on its controversial stops and frisk program began under political pressure from the 1999 investigation by the New York State Attorney General following the police killing of Amadou Diallo is a botched stop. See, Eliot Spitzer, New York City Police Department’s “Stop & Frisk” Practices: A Report to the People of the State of New York From the Office of the Attorney General (1999)

140 See, e.g., Fagan and Davies; Rojek, Rosenfeld and Decker, Policing Race: The Racial Stratification of Searches in Police Traffic Stops, 50 Criminology 993 (2012); Katherine Y. Barnes, Assessing the Counterfactual: The Efficacy of Drug Interdiction Absent Racial Profiling,
The second controversy was generated by an instrumental analysis of profiling as a tradeoff of harms that leads to a moral imperative to create wide space for police to act on race-based signals of suspicion. Proponents of race-based selection of suspects, or racially disproportionate selection, based their preferences on three prongs: (a) higher crime rates among African American and Latino people (or in places where they are demographically concentrated, (b) the greater efficiency and effectiveness of police methods that apply race-based strategies, and (c) the moral imperative to pursue tactics that maximize social welfare and security. To ignore race in the design of police tactics would be to risk greater exposure of individuals, including those in the affected groups, to unjustified harms.  The arguments for profiling assume that the social good produced by welfare outweighs the harms of the inequalities inherent in race-based selection of persons.

The arguments advancing profiling ran headlong into its constitutional weaknesses, even under a newly capacious Fourth Amendment suspicion standard that invited the substitution of race-based correlates of suspicion for explicit racial categories. The inherent constitutional violations deflated the moral arguments, as did the dubious claim of its effectiveness. But other critiques emerged that also pushed back on profiling. Beyond the failure of proponents to find empirical support for the claim of its benefits, the utilitarian view tended to discount the serious harms to the innocent who are stopped, particularly innocent African Americans who bore the brunt of police actions.

Recognition of those harms was inherent in the *Terry* decision itself, which acknowledged that police stops, especially for the innocent, amounted to more than a "petty indignity." The harms of *Terry* stops are several: the stigma harm of being singled out when innocent, the shaming of being singled out by the police and physically

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142 A similar argument was made by Sunstein and Vermeule on capital punishment. They argued that if the death penalty was an effective deterrent to murder, then the life-life tradeoff of capital punishment created a more imperative to execute those convicted of capital murder, and a moral offense when executions are not carried out in the face of evidence of deterrence and the possibility of lives lost. Cass Sunstein and Adrien Vermeule, *Is Capital Punishment Morally Required?* 58 Stanford L. Rev. 703 (2005).

143 See, *Whren* and *Wardlow* discussions, supra.


145 “Moreover, it is simply fantastic to urge that such a procedure performed in public by a policeman while the citizen stands helpless, perhaps facing a wall with his hands raised, is a "petty indignity." It is a serious intrusion upon the sanctity of the person, which may inflict great indignity and arouse strong resentment, and it is not to be undertaken lightly.” *Terry*, at 17-18
interrogated in front of one’s family and neighbors, the racial stigma that attaches when minorities are disproportionately targeted for stops, and the potential for physical violence when stops arouse anger or when police use aggressiveness when confronted by their own safety fears.\footnote{Stuntz, Terry’s Impossibility, supra note 110. See, also, Bernard E. Harcourt, Rethinking Racial Profiling: A Critique of the Economics, Civil Liberties, and Constitutional Literature, and of Criminal Profiling More Generally, 71 U. Chi. L. Rev. 1275, 1292 (2004); Sherry F. Colb, Innocence, Privacy, and Targeting in Fourth Amendment Jurisprudence, 96 Colum. L. Rev. 1456 (1996); I. Bennett Capers, Rethinking the Fourth Amendment: Race, Citizenship, and the Equality Principle, 46 Harvard Civil Rights-Civil Liberties L. Rev. (CR-CL) 10 (2010).} \footnote{Amanda Geller, et al., Aggressive Policing and the Mental Health of Young Urban Men, 104 American Journal of Public Health 2321 (2014).} Stops can be verbally harsh, physically aggressive, or laced with racial or sexual invectives trigger a variety of emotional reactions. Accordingly, a robust and consistent stream of research reveals numerous harms that people of color experience as a result of accumulated, proactive police contacts. Survey research in New York City under its stop and frisk program showed elevated rates of symptoms of post-traumatic stress disorder among the young adults most often stopped and most intrusively policed.\footnote{Randall Kennedy, Race, Crime, and the Law (Vintage Books 1998)}

D. Two Visions of Neighborhood and Police

At the same time that the clashes between minorities and police have occupied center stage, a different form of equal protection complaints remained generally unheard and have only recently been taken seriously. Minority citizen concerns about the everyday under-policing of distressed neighborhoods also have received less scholarly attention.\footnote{Id. at 20.} Nonetheless, police executives have increasingly come to understand that disadvantaged, high-crime minority communities may indeed simultaneously experience under- and over-policing. Professor Randall Kennedy notes that “in terms of misery inflicted by direct criminal violence, blacks (and other people of color) suffer more from the criminal acts of their racial ‘brothers’ and ‘sisters’ than they do from the racist misconduct of white police officers.”

Given that both under- and over-policing have been shown to seriously undermine public confidence in and trust of the police, policy makers face a weighty challenge concerning how best to deliver effective crime control strategies without engaging in potentially racially discriminatory policing practices.

Further, police leaders’ allocation and prioritization of resources may unwittingly result in strategies that have the potential to exacerbate longstanding tensions between police and members of minority communities. Specifically, police departments seldom have strong relationships with residents of distressed, black neighborhoods, places where violent crime is also disproportionately concentrated. In fact, blacks consistently report being more distrustful of and having less confidence in the police than their counterparts from other racial groups. Black citizens’ persistent negative attitudes toward the police have important implications for contemporary crime-control efforts. Simply put,
individuals’ are more inclined to support and assist police in their overall crime-fighting mission if they view the police (and their routine tactics) favorably.

Nearly a century of legal and social trends set the stage for the current debate on race and policing. Historically, close surveillance by police has been a part of everyday life for African Americans and other minority groups. In recent decades, the U.S. Supreme Court has sanctioned border interdictions of persons of Mexican or Hispanic ethnicity to halt illegal immigration, as well as the racial components of drug courier profiling by airlines. The U.S. Supreme Court has also allowed the use of race as a basis for a police stop as long as there were other factors that motivated the stop, and a federal district court permitted the use of race as a search criterion if there was an explicit racial description of the suspect.

The legal standard to regulate the constitutionality of police conduct in citizen stops derives from Terry v. Ohio (1968), which involved a pedestrian stop that established the parameters of the “reasonable suspicion” standard for police conduct in detaining citizens for purposes of search or arrest. Recently, the courts have expanded the concept of “reasonable suspicion” to include location as well as the individual's behavior. In fact, the Court has articulated and refined this “high crime area” doctrine in subsequent cases. This line of cases allows police to consider the character of a neighborhood as a factor justifying a standard lower than the constitutionally-defined threshold in individualized “reasonable” suspicion articulated in Terry v Ohio (1968).

But in connecting race and policing, the Court was only formalizing what criminologists had known for decades. Early studies on police selection of citizens for stops suggested that both the racial characteristics of the suspect and the racial composition of the suspect's neighborhood influence police decisions to stop, search, or arrest a suspect. Particularly in urban areas, suspect race interacts with neighborhood

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152 U.S. v. Harvey, 961 F.2d 1361 (1992)
154 Brown v. Oneonta (2000), 221 F.3d 329
155 Terry v Ohio, 392 U.S. 1 (1968)
156 See, for example, Adams v Williams, 407 U.S. 143 (1972); Illinois v Wardlow, 528 U.S. 119 (2000)
characteristics to animate the formation of suspicion among police officers. For example, Alpert and his colleagues showed that police are more likely to view a minority citizen as suspicious—leading to a police stop—based on non-behavioral cues while relying on behavioral cues to develop suspicion for white citizens.

Individuals—including police and political leaders—also may substitute racial characteristics of communities for racial characteristics of individuals in their cognitive schema of suspicion, and, more important, act on them. Urban residents’ perceptions of crime in their neighborhoods are significantly predicted by the prevalence of young black men, even after crime levels and other neighborhood characteristics are controlled for. Police perceptions may be similarly skewed, resulting in elevated stop rates in neighborhoods with high concentrations of minority populations, and the pathway is through the translation of perceptions into neighborhood stigma. For example, in a study of police practices in three cities, suspects in poor neighborhoods were more likely to be arrested, after controlling for suspect behavior and the type of crime. Suspects’ race and racial composition of the suspect’s neighborhood were also significant predictors of police response. It seems that social psychological mechanisms interact with cultural processes (patterns of behavior) and structural features of neighborhoods (poverty, concentrations of minority citizens) to produce perceptions of disorder that perpetuate urban inequality through several forms of discrimination, including policing intensity and tactics.

Fagan and Davies showed that street stops in New York were predicted not by disorder but by race and poverty, despite policing theories that emphasized disorder as a pathway to elevated crime. Poor neighborhoods are stigmatized in this way, and people both within these areas as well as those who reside elsewhere—including those with administrative authority to withhold or allocate various services—are likely to act on their perceptions.

Alternatively, these coercive police responses may relate to the perception that poor neighborhoods may have limited capacity for social control and self-regulation. This strategy was formalized in the influential “broken windows” essay of James Q. Wilson

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160 Alpert, et al., supra note 121.
165 See id.
and George L. Kelling. They argued that police responses to disorder were critical to communicate intolerance for crime and to halt its contagious spread. Broken windows called for the targeting of police resources to neighborhoods where public order was deteriorating, with the expectation that stopping disorderly behavior would stem the “developmental sequence” to more serious crime. In the original essay, Wilson and Kelling worried about “criminal invasion” of disorderly neighborhoods. Neighborhood disorder has explicitly been used as a criterion for allocating police resources in New York City since 1994, when Commissioner William Bratton set policies to focus on minor offenses such as subway fare evasion and aggressive panhandling, in addition to felonies and other serious crime. The policy also called for aggressive responses to social disorder that was endogenous to neighborhoods, in contrast to the “criminal invasion” concern in the theory’s pristine form.

This order-maintenance approach also has been disputed, however, as critics question the causal link between disorder and more serious crime. Despite the potential endogeneity of race and both social and physical disorder, several of these studies suggest that a focus on disorder might have a disparate impact on citizens of different races. For example, residents’ perceptions of disorder in Chicago neighborhoods conflate systematically observable conditions with their neighborhoods’ racial and socioeconomic makeup. The association between race, poverty, and

167 See id.
perceived disorder is significant in residents of all racial and ethnic backgrounds; race and concentrated poverty predict both local residents’ and outsiders’ perceptions of disorder even more strongly than does systematically observed disorder. And the effect grows stronger as the concentration of poverty and minority groups increase.

E. Is It Bias Revisited: Approaches to Studying Police Stops and Searches

Recent empirical evidence on police stops supports perceptions among minority citizens that police disproportionately stop African American and Hispanic motorists, and that once stopped, these citizens are more likely to be searched or arrested.172 For example, surveys with nationwide probability samples, completed in 1999, 2002, and 2008 showed that African-Americans were far more likely than other Americans to report being stopped on the highways by police.173 Each survey showed that minority drivers also were more likely to report being arrested, handcuffed, or searched by police, and that they more often were threatened with force or had force used against them. These disparities in stop rates exact high social costs that animate culturally meaningful forms of stigma that reinforce racial inequalities, especially in the practice of law enforcement.174 These stigmas often translate into withdrawal of minority populations from cooperation with the police and other legal authorities in the co-production of security.175

Traffic violations often serve as the rationale or pretext for stops of motorists,176 just as “suspicious behavior” is the spark for both pedestrian and traffic stops.177 As with

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traffic violations, the range of suspicious behaviors is broad enough to challenge efforts to identify an appropriate baseline to which to compare race-specific stop rates. Pedestrian stops are at the very core of policing, used to enforce narcotics and weapons laws, to identify fugitives or other persons for whom warrants may be outstanding, to investigate reported crimes and “suspicious” behavior, and to improve community quality of life. Indeed, because low-level “quality of life” and misdemeanor offenses are more likely to be committed in the open, the “reasonable suspicion” standard is more easily satisfied in these sorts of crimes.

Two distinct approaches characterize recent efforts to model and understand racial disparities in police stops. Each focuses less on identifying racial bias than on understanding the role of race in explaining patterns of police behavior. Attributing bias is difficult: causal claims about discrimination would require far more information than the typical administrative (observational) datasets can supply. For example, when Officer McFadden stopped suspect Terry in the events leading to the landmark 1968 U.S. Supreme Court decision in *Terry v Ohio*, he used his law enforcement “experience” to interpret Terry’s behavior in front of the jewelry store. The multiplicity of interacting factors complicated the identification of the role of race in the decision to detain Terry, but several analyses of the facts and jurisprudence of *Terry* suggest that the Supreme Court opinion discounted the influence of race in the opinion.

In *Terry*, it would difficult to identify race alone, apart from the context in which race was observed, as the factor that animated McFadden’s decision to stop and frisk suspect Terry. Instead, reliable evidence of ethnic or racial bias in these instances would require experimental designs that control for these competing and interacting factors – situational context, demeanor of suspect – so as to isolate differences in outcomes that

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could only be attributed to race or ethnicity. Such experiments are routinely used in tests of discrimination in housing and employment.¹⁸² But observational studies that lack such controls are often embarrassed by omitted variable biases: few studies can control for all the variables that police consider in deciding whether to stop or search someone, much less their several combinations or permutations. Research in situ that relies on direct observation of police behavior¹⁸³ requires officers to articulate the reasons for their actions, a task that is vulnerable to numerous validity threats. Sampling considerations, as well as the presence of researchers in the context of the decision, also challenge the validity of observational studies.

The first approach to studying racial disparities bypasses the question of whether police intend to discriminate on the basis of ethnicity or race, and instead focuses on disparate impacts of police stop strategies. This strategy is prevalent in studies of decisions in the context of highways stops. In this approach, comparisons of “hit rates”, or efficiencies in the proportion of stops that yield positive results, serve as evidence of disparate impacts of police stops. This type of analysis has been used in several studies,¹⁸⁴ and many other studies of police behaviors on highways.¹⁸⁵ This approach bypasses the supply-side question of who is stopped (and for what reason), and instead looks only at disparate impacts or outcomes for different groups.

Outcome tests are agnostic with respect to race-based motivations for stops or frisks versus a search for efficiency and deterrence.¹⁸⁶ They can show when a particular policy or decision-making outcome has a disparate impact whose racial disproportionality is not justified by heightened institutional productivity, negating an efficiency rationale. In the context of profiling, outcome tests assume that the ex post probability that a police search will uncover drugs or other contraband is a function of the degree of probable cause that police use in deciding to stop and search a suspect.¹⁸⁷ If searches of minorities are less productive than searches of whites, this could be evidence that police

¹⁸³See, e.g., Gould and Mastrofski (2004); Alpert et al. (2005).
have a lower threshold of probable cause when searching minorities. At the very least, it is a sign of differential treatment of minorities that in turn produces a disparate impact.

Knowles, Persico, and Todd consider this “hit rate” approach theoretically as well as empirically in a study finding that, of the drivers on Interstate 95 in Maryland stopped by police on suspicion of drug trafficking, African Americans were as likely as whites to have drugs in their cars.\textsuperscript{188} Their theoretical analysis posits a dynamic process that considers the behaviors of police and citizens of different races, and integrates their decisions in equilibrium where police calibrate their behavior to the probabilities of detecting illegal behavior, and citizens in different racial groups adjust their propensities to accommodate the likelihood of detection. They concluded that the search for drugs was an efficient allocation of police resources, despite the disparate impacts of these stops on minority citizens.\textsuperscript{189}

Outcome tests can be constructed as quasi-experiments, with race as a treatment, to identify the role of race in the selection of citizens for searches. Ridgeway matched suspects within officers to compare the post-stop outcomes of white suspects to those of minority suspects in similar locations, stopped at similar times and for the same reasons.\textsuperscript{190} He reports no differences in post-stop arrests (“hit rates”) despite the greater number of stops of non-whites. But this approach seeks to explain away contextual variables, especially neighborhood context, rather than explicitly incorporate these factors in an identification strategy. Close and Mason construct a disparate outcome quasi-experiment to identify the role of race in police searches by comparing the preferences of officers of different races to search motorists, controlling for the motorist’s race.\textsuperscript{191} They use both an outcomes-based non-parametric (quasi-experimental) analysis and a standard benchmarking parametric (regression) approach, and report both personal biases and police cultural bias in their propensity to search African American and Latino drivers.

These are useful but limited strategies. The robustness of these designs is compromised, by the omission of several factors – some unobservable and others usually absent from administrative data – that might bias their claims, such as racial differences in the attributes that police consider when deciding which motorists or pedestrians to

stop, search or arrest,\(^{192}\) or differences in police behavior in neighborhoods or other social contexts with different racial makeup.\(^{193}\) The omission of neighborhood context also biases estimates of the proportionality of police stops of citizens. The randomizing equilibrium assumptions in the Knowles et al. approach – that both police and potential offenders adjust their behavior in response to the joint probabilities of carrying contraband and being stopped – tend to average across broad heterogeneous conditions both in police decision making and offenders’ propensities to crime,\(^{194}\) and discount the effects of race-specific sensitivities toward crime decisions under varying conditions of detection risk via police stop.\(^{195}\) When these two concerns are addressed, Dharmapala and Ross identify different types of equilibria that lead to different conclusions about racial prejudice in police stops and searches.\(^{196}\)

Accordingly, the nature and extent of racial bias in the policing of motorists and pedestrians remains unsettled empirically.\(^{197}\) Supply-side issues, both in the number and characteristics of the persons available for stops by virtue of law violation or even suspicious behavior, complicate the search game paradigm by skewing the population of stopped drivers according to the \textit{ex ante} probabilities of criminality that police officers assign to different racial groups. Institutional or individual differences in the goals of law enforcement may also create heterogeneity both in the selection of individuals to be stopped and the decisions to engage them in searches for drugs, weapons, or other contraband. Officers may pursue one set of law enforcement goals for one group – maximizing arrests – while pursuing a different set of goals – minimizing crime – for another. Racial nepotism or antagonism may lead to differences in police stop and search

\(^{193}\) Smith, 1986; Fagan and Davies 2000; Alpert et al., 2005
behaviors when officers of one race face choices as to stop or search a driver of the same or a different racial or ethnic group.\footnote{198}

These complexities illustrate the difficulty of identifying the role of race in producing racial disparities in stops and searches, and suggest a second approach that incorporates the contexts in which individual officers consider race in their everyday interactions with citizens. Gelman et al. and Alpert et al. show how neighborhood context influences both the attribution of suspicion that animates an encounter and the outcomes of police-citizen encounters.\footnote{199} The institutional context of policing also may influence individual officers’ decisions through stigmatizing neighborhoods as “high crime” or disorderly, skewing how officers perceive and interpret the actions of citizens. Institutional cultures also may implicitly tolerate such perceptual or cognitive schema and internalize them into policy preferences and strategic decisions, as well as internal preferences for reward, promotion or discipline.

These contextual concerns, informed by crime plus social and demographic dimensions of neighborhoods, suggest the second approach, one that explicitly incorporates either a multilevel approach that examines officer-place interactions, or shifts the focus from the actions of individual officers and individual suspects to the behaviors of cohorts of officers who collectively patrol neighborhoods with measurable attributes that incorporate race and ethnicity, and where aggregation biases from racial concentration may shape officers’ Bayesian priors about crime and thresholds of suspicion.

\section*{III. Field Interrogation and Observations by the Police in Boston}

\subsection*{A. Observing the Observed and the Observers}

Boston, Massachusetts, is the site for this research. The FIO strategy in Boston encompasses street encounters of the sort envisioned in Terry, where officers temporarily detain and question persons for whom they have reasonable and articulable suspicion that “crime is afoot.”\footnote{200} This regime also includes non-contact observations, which we discussed earlier.\footnote{201}

Worries about the panopticonic approach range from anxiety about privacy and anonymity of individuals in their everyday movements, to anxiety about easy detection of and over-enforcement of low-stakes crimes, to the racial disparities that may be inherent

\footnote{198 Billy R. Close and Patrick Leon Mason, Searching for Efficient Enforcement: Officer Characteristics and Racially Biased Policing (2007).}

\footnote{199 Gelman, Andrew, Jeffrey Fagan, and Alex Kiss An Analysis of the New York City Police Department’s ‘Stop-and-Frisk’ Policy in the Context of Claims of Racial Bias. 102 Journal of the American Statistical Association 813-823 (2007); Alpert et al. (2005).}

\footnote{200 Terry at 30-31.}

\footnote{201 See, discussion in Section I.D and accompany notes.}
in such regimes. But one advantage of this approach is that it can accommodate transparency, as these analyses show. When police scan for suspicious behavior, we have only vague ideas about how their discretion is managed, and even more vague ideas about what exactly it is that they are looking for.

While there may be nothing like an algorithm to explain how observations are formed, there at least are observable patterns. The worry in this regime is about race: unconscious patterns that shape the formation of suspicion based on archetypes such as the “symbolic assailant” and other processes that shape cognition and interpretation of behavioral cues. Transparency at least provides a window to observe what those processes produce. In other words, it lets observers, assuming access to these records, observe the observers as they conduct surveillance.

What remains unknown in this process is the harms that may accrue from these routine invasions of privacy. The dignitarian concerns pose one type of harm: the fact that one is a target of surveillance signals to other observers and perhaps to the watching public the person is a potential threat. That alone can have stigmatizing consequences throughout the community of the observed. Apart from the dignity worries, conducting these observations and stops can have negative effects on the way that police regard citizens and respect their autonomy and privacy.

B. The Boston FIO Regime

In this study, we analyzed data provided by the BPD on its FIO activity. The BPD Boston Regional Intelligence Center (BRIC) maintains an electronic database of FIO reports. These forms are used to document BPD officer interactions with individuals suspected of criminal activity, or associates of those individuals, including direct encounters and non-contact observations. FIO reports are a central activity in the BPD’s intelligence efforts to collect and analyze data on the activities and whereabouts of known and suspected criminals and their associates in Boston. The reports document the

202 Daniel Solove, Nothing to Hide: The False Tradeoff between Privacy and Security (Yale University Press 2011); I. Bennett Capers, Rethinking the Fourth Amendment: Race, Citizenship, and the Equality Principle, 46 Harv. C.R.-C.L. L. Rev. 10-15 (2010);
204 Stuntz, Fourth Amendment and Privacy, supra note 94.
205 See, e.g., Jane Bambauer, Hassle 113 Mich. L. Review 461-585 (2015) (noting that when police conduct stops, the community watching those stops internalizes the stigma attached to the person who has been stopped. When no wrongdoing is found, the stigma may remain. See, also, Amanda B. Geller et al., Aggressive Policing and the Mental Health of Young Urban Men, 104 (12) Amer. J. Pub. Health 2321-2327 (2014). There is no reason to believe that when police conduct such observations, a stigma signal may be produced even in the absence of any contact.
206 David Sklansky, Too Much Information: How Not to Think About Privacy and the Fourth Amendment, Cal. L. Rev. (Forthcoming, 2014).
name, date-of-birth, sex, and race of FIO subjects as well as the date, time, and location of interaction.

Our analysis focuses on the period from 2007 through 2010. During that time, BPD officers made N=204,739 FIO reports. Compared to the residential population, the targets of FIO reports were disproportionately male, young, and Black. For these 204,739 FIO reports, the subjects were 89.0 percent male, 54.7 percent ages 24 or younger, and 63.3 percent Black. According to the U.S. Census Bureau, in 2010, Boston had some 617,594 residents that were 47.9 percent male, 36.2 percent ages 24 or younger, and 25.1 percent black.208

At first glance, these differences are suggestive of racially disparate treatment in BPD FIO activity. However, these differences could also reflect crime risk differences in Boston’s neighborhoods and population groups. Criminological research has long documented that criminal offenders are more likely to be young and male.209 Violent crime problems also tend to concentrate in highly disadvantaged urban neighborhoods that are disproportionately populated by black residents.210

BPD officers are required to document the reason for the completion of each FIO report and required to note whether they conducted Terry frisks for officer safety purposes and/or searches for the purposes of seizing evidence. Some 40.5 percent of the FIO reports involve a frisk and/or search of the subject (82,919).211 Officers have very

211 38.6% of the FIO reports indicated that the subjects were frisked and 11.6% of the FIO reports indicated that the subjects were searched. All but 1.8% of the searches were reported in conjunction with a frisk of the subject. Moreover, descriptive statistical analyses revealed that the biggest differences between FIO type and subject race arose when the FIO involved a frisk and/or search relative to a more simple observation and/or interrogation. Some 29.5% percent of White subjects were frisked / searched during an FIO relative to the 45.4% percent of Black subjects, 40.5% of Hispanic subjects, and 35.6% of Asian /other race subjects. As such, FIO type was
limited space on the form to record their reasons for the FIO and, unfortunately, 75.0 percent (153,554) of the FIO reports simply state “investigation person” as the justification. This lack of documentation of stop rationales prevents a Fourth Amendment analysis of the legal justifications for discretionary stops and searches of FIO subjects. Also, there is no information on the outcomes of the FIO events about whether the frisks and searches led to arrests, summons, or seizure of weapons or contraband. In fact, FIO events that did lead to either of those outcomes are not recorded. Officers default to the completion of an arrest report in those circumstances. In turn, the type of outcome analysis that was essential to resolving the Fourth and Fourteenth Amendment claims in the Floyd stop-and-frisk litigation in New York could not be completed here.

C. Empirical Strategy

Our empirical strategy combines two distinct approaches to estimate racial disparities. The first strategy is a disparate treatment strategy that examines stops in alternate empirical specifications looking at first aggregates – neighborhoods or police districts – and then individuals nested within those districts. We drew upon regression models developed by Fagan and colleagues\textsuperscript{212} to investigate alleged violations of the Fourteenth Amendment of the U.S. Constitution by the New York City Police Department (NYPD) in their stop, question, and frisk (SQF) practices as part of the David Floyd et al. v. City of New York et al. U.S. District Court complaint.\textsuperscript{213} Their analyses examined whether the racial composition of NYPD precinct residents predicted stop patterns after controlling for the influences of crime, social conditions, and the allocation of police resources. Here, we adapted their analytical framework to examine whether the racial composition of Boston neighborhoods predicts BPD FIO patterns, adjusting for crime, social and economic predictors, and police resources.

The second strategy exploits the availability of data on officer race to determine whether the observed differences in stop rates for White and non-white youths are a function of preference-based discrimination, or statistical discrimination. Statistical discrimination would reflect a tendency to stop one group at a higher rate than another based on observable characteristics such as known crime rates. But preference-based discrimination would reflect a tendency to prefer one group for stops over others based on factors unrelated to their observable differences in the targeted behavior.

collapsed into two categories: 0 = No Search (Observed and/or Interrogated only) and 1 = Frisk and/or Search Conducted.


\textsuperscript{213} Second Amended Complaint, David Floyd et al. v. City of New York et al., U.S. District Court for the Southern District of New York, 08 Civ. 01034 (SAS), October 28.
A study by Professors Antonovics and Knight (hereafter, AK) conducted this type of analysis in Boston based on traffic stops by the BPD. AK extended the Knowles, Persico and Todd (KPT) hit rate (or equilibrium) test to test for bias in traffic stops by Boston police officers from 2001-3. Following the KPT equilibrium model, AK assume that officers will rationally stop motorists according to their beliefs about the comparative propensities to violate the law by carrying contraband – drugs or weapons. If population groups understand the risks of violating the law, they will adjust their behaviors accordingly and their propensities should fall into equilibrium with other groups and with officers’ preferences. In a statistical model, differences in “hit rates” between groups should be negligible, a sign that equilibrium has been reached. This form of statistical discrimination stands in contrast to preference based discrimination, where police will stop motorists based on preferences to detain or arrest citizens in a particular racial group independent from their assumptions about that group’s propensity to carry contraband.

In this case, AK were able to observe the race of both the officer and the suspect and estimate the magnitude of preference-based discrimination. AK used officer race-suspect race dyads as a benchmark for discrimination, assuming that officers would be more likely to stop and search a suspect from a different racial group. Perhaps officers believe they are better able to detect signals of wrongdoing among persons within their own race, or same-group membership may lead to preferential treatment. Regardless of motive, evidence of higher rates of cross-race differences in search rates would suggest preference-based discrimination rather than simply statistical discrimination based on general beliefs about crime-propensity within each racial or ethnic group. Other studies have used the same strategy in different settings to reach the same conclusions.


216 Knowles, Persico and Todd, supra note 218. But see, Bernard Harcourt, Against Profiling, supra note 116, arguing that the assumptions of consistent responses – or elasticities – across different racial groups is unrealistic given their overall exposure to both legal earning and the potential punishment costs from detection.

However, the previous studies using officer-race benchmarks have examined bias in vehicle searches pursuant to traffic stops. In our strategy, we exploit the availability of these same data to estimate race-based preferences among officers in street stops, preferences that are not easily explained by assumptions about each group’s propensity for crime.

1. Disparate Treatment by Race

The general test for evidence of disparate treatment is a regression equation that takes the form:

\[ \text{Outcome} = \alpha + \beta_1 \times \text{Minority} + \Sigma \beta_i \times (\text{Plausible Non-Race Influences}) + \epsilon_i \]

where \( \text{Outcome} \) is the event or status of interest, \( \text{Minority} \) is an indicator for the racial composition or status of the unit observed (i.e., neighborhood or person, depending on the outcome), \( \text{Plausible Non-Race Influences} \) are a set of variables representing non-race factors that also might influence the outcome, and an error term \( \epsilon \) that captures the variation in the outcome that cannot be explained by either Minority status or the Non-Race Influences. These models may include non-race influences that are correlated with race, so as to better identify the unique effects of race that are present once the influence of proxies for race are removed.\(^{218}\) The goal in specifying these models is to identify the effects of race on outcomes after simultaneously considering factors that may be relevant to race.\(^{219}\) Under a disparate treatment theory, the critical question is whether an applicant’s race was the cause of being denied employment. Failure to do so raises the risk of “omitted variable bias,” which could lead to erroneous conclusions about the effects of variables that do appear in a regression test.\(^{220}\)

2. Measures and Model Specification


\(^{219}\) See, e.g., *Griggs v. Duke Power Co.*, 401 U.S. 424 (1971). In a disparate treatment claim, we would ask if the use of a high school diploma requirement biases the hiring process since African American job applicants may be less likely to have obtained a high school diploma. Once this race-correlated control is introduced, it would likely reduce the racial disparity in the hiring rates and provide a different test than would a simple disparate impact test.

We analyze differences in stop rates by neighborhood to determine whether FIO activity is explained by local crime rates, or if there is additional variance that is explained by race. A race-neutral practice would predict a positive effect for local crime rates and non-significant effects for race once we control for crime.

The neighborhood analyses were conducted using 2010 U.S. Census tracts as the principal unit of analysis. Census tracts were used instead of BPD geographic units (e.g. districts, reporting areas) or smaller areal units (e.g. Census block groups, street segments). Tracts are areas roughly equivalent to neighborhoods developed by the U.S. Census Bureau for the purposes of analyzing populations. According to the 2010 U.S. Census, Boston was comprised of N=181 tracts. Data on the social and economic conditions in these tracts were obtained from the 2007-2010 American Community Survey (ACS). Eight tracts were excluded from the analysis because there were no residents in these areas for a total N=173 tracts. The FIO data included date and geographical location (x-y coordinates) information that permitted aggregation of FIO counts to Census tracts and by differing time periods. The main outcome variable was the monthly count of FIOs made in each Census tract between 2007 and 2010 (N=8,304; 173 Census tracts with 48 observations each).

The specific estimation technique for this analysis, or the functional form of the regression equation, was responsive to the specific measure of FIO activity (monthly counts in Census tract units). Accordingly, models were estimated using negative binomial regressions. This class of regression models is appropriate for counts of events, such as FIO reports in a specific area, where assumptions about the independence of events cannot be reliably made. These models also are appropriate for counts where the distribution are over-dispersed; that is, where the variance exceeds the sample mean. The model takes the form of:

\[ p(y) = P(Y_i = y_i) = \frac{\Gamma(y_i + \phi)}{y_i! \Gamma(\phi)} \frac{\phi^y_i \lambda_i^{y_i}}{(\phi + \lambda_i)^{y_i}} \]


222 http://www.census.gov/acs/

223 These eight Census tracts included the Stony Brook reservation, Belle Isle Marsh reservation, the Harbor Islands, the Esplanade recreational area, the Franklin Park recreational area, and three commercial property waterfront areas.

224 95.2% (194,858 of 204,739) of the FIO reports were geocoded to 2010 Census Tracts in Boston.

where \( Y_i \) is the expected count of events in each unit \( i \) given parameters that we observe. We estimate the incidence of events for overdispersed models as:

\[
E[y_i | x_i, \epsilon_i] = \exp(\alpha + x_i^t \beta + \epsilon_i) = h_i \lambda_i
\]

where \( h_i = \exp(\epsilon_i) \) is assumed to have a one parameter gamma distribution, \( G(\theta, \theta) \) with mean 1 and variance \( 1 / \theta = \kappa \).

We used a specific form of negative binomial regression known as General Estimating Equations (GEEs). GEEs are beneficial for nested or hierarchically organized data, such as years within Census tracts, as they allow for the specification of within-subject correlations of observations. These nesting variables are treated as random effects in the estimating models. Random effects here include census tract correlations. To adjust for difference in population densities in the census tracts, we estimated population-averaged models. Since the analyses include a sequence of time periods (calendar months), the models include an AR(1) variance estimation function that adjusts for the serial autocorrelation (or autoregression) of the counts of events within sampling units over long periods of time. We controlled for yearly and seasonal variations in the monthly counts of FIO reports by including fixed-effects for calendar quarter and year.

Police activity in Boston is closely linked to crime. As such, we test whether crime rates in a neighborhood are linked to the intensity of BPD FIO activity in that area. We use crime incident data generated by the BPD on 113,419 “index” crime incidents in

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\text{226} \quad \text{James W. Hardin and Joseph M. Hilbe, Generalized Estimating Equations (2003); Gary A. Ballinger, Using Generalized Estimating Equations for Longitudinal Data Analysis, 7 Organizational Research Methods 127 (2004).}
\]

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\text{227} \quad \text{AR(1) adjustments reflect the reality that the best predictor of what the crime rate will be in the next month is what it was in last month. This is an empirical constraint in identifying the relationship between crime and policing. Failure to correct for this temporal dependence will bias the standard errors in estimates of crime effects on policing, and this distortion remains even when fixed effects are used to control for temporal trends. See, Badi Baltagi, Econometric Analysis of Panel Data (2001); Badi Baltagi and Qi Li, Testing AR(1) Against MA(1) Disturbances in an Error Component Model, 68 Journal of Econometrics 133 (1995).}
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\text{228} \quad \text{There is a long tradition of studies of the seasonality of crime and the theoretical explanations for why crime varies by season. See, e.g., John R. Hipp, et al., Crime of Opportunity or Crimes of Emotion? Testing Two Explanations of Seasonal Change in Crime, 82 Social Forces 1333 (2004).}
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\text{229} \quad \text{We created indicator variables to account for seasonal variations by calendar quarter. Quarter 1 represented January, February, and March monthly FIO counts (1 = Yes, 0 = No). Quarter 2 represented April, May, and June monthly FIO counts (1 = Yes, 0 = No). Quarter 3 represented July, August, and September monthly FIO counts (1 = Yes, 0 = No). Quarter 4 represented October, November, and December monthly FIO counts (1 = Yes, 0 = No). Quarter 1 served as the reference category for the seasonal polychotomous dummy variable. We also created indicator variables for year to account for annual variations in the data.}
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\text{230} \quad \text{Anthony A. Braga, et al., An Ex-Post-Facto Evaluation Framework for Place-Based Police Interventions, 35 Evaluation Review 592 (2011).}
\]
Boston between 2007 and 2010. These crime incident data were geocoded, and then aggregated by Census tract and month of occurrence to create a covariate measuring lagged and logged monthly counts of serious crime in Boston census tracts. As Figure 2 reveals, FIO reports made by BPD officers in 2010 tended to concentrate in census tracts with higher rates of total crime incidents and higher percentages of black resident populations. Figure 2 also shows a high degree of spatial autocorrelation in the concentration of FIO reports across Census tracts. We controlled for this spatial dependence in our regression models by including a Moran’s I spatial effects covariate.

We also control for police deployment patterns. The allocation of police and targeting of police activity frequently involved “saturation” deployment of police patrols in higher crime areas. Since these areas in Boston and elsewhere often had higher concentrations of non-White residents, asymmetrical deployments of police increased exposure of citizens to police and thus the increased probability of encounters with minority citizens as compared to Whites, in turn producing racial or ethnic differences in contact patterns. Accordingly, an analysis of FIO patterns by neighborhood required an understanding of the allocation of police patrol resources in each unit of analysis. Patrol strength data were provided by the BPD for each of their eleven policing districts.

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231 Index crimes, as defined by the FBI, included murder, rape, robbery, aggravated assault, burglary, auto theft, and larceny. See http://www.fbi.gov/about-us/cjis/ucr (accessed August 1, 2014). Using ArcGIS 10.2 mapping software, the BRIC was able to geocode 113,152 of these incidents to their respective Census tracts (99.8 % of 113419 total crime incidents).

232 All models control for the one-month-lag of logged total crime incidents. The natural log transformation of the actual number of crimes was used. Log transformation is necessary to adjust when the distributions are highly skewed and non-linear. The lag reflects the police planning process whereby FIO reports and other enforcement activity are adjusted to reflect actual crime conditions.

233 Spatial dependence, or autocorrelation, violates the assumption of independence among observations used in most statistical models. Spatial regression analyses of the variation of crime, etc. across neighborhood units account for spatial autocorrelation through the addition of a spatial effects covariate such as Moran’s I. The argument is that analyses that do not compensate for spatial dependency can have unstable parameter estimates and yield unreliable significance tests. See, Michael D. Ward and Kristian Skrede Gleditsch, Spatial Regression Models, Quantitative Applications in the Social Sciences series, No. 155, 8 – 10 (2008).

234 ArcGIS 10.2 was used to export a shapefile containing the total number of FIOs made per U.S. Census Tract during the study time period to GeoDa 1.4.6 spatial analysis software. Using queen’s contiguity, a Moran’s I = 0.674689 was estimated (199 permutations, \( z = 14.73, p<.005 \); 99 permutations, \( z = 15.18, p<.01 \)). The Moran’s I spatial autocorrelation lag for each Census Tract was exported to Stata 13.1 and included in the neighborhood analysis.


between 2007 and 2010. These patrol data were then allocated to the each Boston census tract.

It is also important to note that the regulation and oversight of FIO policy and activities takes place at the police district level. There are 12 police districts in Boston, each commanded by a police captain who reports directly to the Superintendent of the Bureau of Field Services. BPD Captains are accountable for district-level crime trends and have discretion to allocate officers tactically within districts. Since tracts are nested within Boston’s policing districts, we included fixed effects to account for any unobserved effects of conditions in the districts that might influence police activity, such as district-level variations in the use of FIOs to gather intelligence and maintain contact with potential offenders.

Several studies show that neighborhood crime rates, including violent crime, are strongly associated with concentrated social disadvantage. The concentrated disadvantage index is a standardized index composed of the percentage of residents who are black, the percentage of residents receiving public assistance, the percentage of families living below the poverty line, the percentage of female-headed households with children under the age of 18, and the percentage of unemployed residents (as measured by the percentage of men over the age 16 who did not work in the previous year).

Since we are explicitly interested the independent impact of race on the number of FIO reports in a neighborhood controlling for other factors, we excluded the percentage of black residents from the construction of the Boston concentrated disadvantage used in this analysis. Because of the high correlation among these variables, we conducted

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237 Because BPD districts do not, as a rule, share boundaries with Census tracts, we allocated patrol strength to tracts based on the percent of each district’s area that falls into each tract. For example, if Census tract A shares area with three police districts (A1, A2, and A3), the Census tract patrol strength was estimated as \[\{(\% \text{ of A1 falling into tract A} \times \text{patrol strength of A1}) + (\% \text{ of A2 falling into tract A} \times \text{patrol strength of A2}) + (\% \text{ of A3 falling into tract A} \times \text{patrol strength of A3})\}\].

238 The BPD has 12 districts that provide policing services across Boston’s neighborhoods: A-1 serving Downtown, Beacon Hill, and Chinatown neighborhoods; A-15 serving Charlestown; A-7 serving East Boston; B-2 serving Roxbury and Mission Hill neighborhoods; B-3 serving Mattapan and parts of North Dorchester; C-6 serving South Boston; C-11 serving most of Dorchester; D-4 serving Back Bay, Fenway, and South End neighborhoods; D-14 serving Allston and Brighton neighborhoods; E-5 serving West Roxbury and Roslindale neighborhoods; E-13 serving Jamaica Plain; and E-18 serving Hyde Park. The reference category for the BPD district dummy variable was E-13. For a basic review of the use of dummy variables in regression models, see: Melissa A. Hardy, Regression with Dummy Variables, 93 Quantitative Applications in the Social Sciences series 7-16 (1993).


principal components factor analysis to identify the underlying dimensions among the variables.\textsuperscript{241} This procedure revealed that variables load on a single factor (which was retained as a standardized disadvantage index variable).\textsuperscript{242} The presence of concentrations of recent immigrants is a protective factor that reduces the risk of crime in a neighborhood.\textsuperscript{243} As such, we created a variable that measured the percentage of foreign-born residents in each Census tract.

3. Benchmarks

The selection of a benchmark against which to assess police enforcement activity is a basic question in reliably measuring the extent of racial disparities in police-citizen interactions.\textsuperscript{244} A benchmark allows us to determine if Boston Police are selectively, on the basis of race or another prohibited factor, singling out persons for FIO reports. As such, we compare the police decision to complete an FIO report on someone to their availability and eligibility for such reports, and compare that calculation across racial and ethnic groups. It is not hard to see that the reliability of an estimate of the extent of racial disproportionality or fairness is likely to depend on – and be particularly sensitive to – the benchmark used to measure criminal behavior.

Population is one measure of the supply of people available to the police for surveillance and possibly stops. However, there are constraints on local population

\textsuperscript{241} Factor analysis is a statistical technique that captures consistency among observed variables to generate a composite measure using a lower number of unobserved variables. The method produces factors that represent the correlations among the observed measures. See Jae-On Kim et al., \textit{Factor Analysis: Statistical Methods and Practical Issues} (1978). The principal components factor analysis was completed using STATA 13.1.

\textsuperscript{242} For example, a Boston Census tract featuring a disadvantage index score of 1.5 would be 1.5 standard deviations more disadvantaged than the mean Boston Census tract. As such, the disadvantage index is adjusted specifically for the city of Boston using 2010 ACS variables, even while the components used to construct the index remain constant across much neighborhood research and remain robust predictors of crime across a variety of city types and spatial aggregations. See Sampson et al., Collective Efficacy, supra note 243; Morenoff et al., Neighborhood Inequality and Collective Efficacy, supra note 243.


estimates that limit its utility as a benchmark for the behavior of the police. Residential population estimates in commercial parts of Boston are often unreliable estimates of the actual composition of persons who are visible and available to the police during certain hours of the day. And, similarly, if people leave residential areas to work in commercial areas, the estimates in the residential areas will also be biased and inaccurate.

Another reason that population may not be an incomplete benchmark is that BPD officers do not complete FIO reports randomly based on the population parameters of an area. In fact, police complete FIO reports of persons based on, at least in theory, their perceptions of suspected crime, or their evaluation of citizen behaviors that may provide reasonable indicia of the potential that a crime has occurred or is about to take place. To the extent that rates of crime suspicion are correlated with rates of crime commission, observed crime rates are useful candidates to serve as a component of a benchmark.

For this analysis of BPD FIO activity, a valid benchmark requires estimates of the supply of individuals of each racial or ethnic group who are engaged in the targeted behaviors and who are available to the police as targets of their stop authority and intelligence gathering activities. Since police often target resources to the places where crime rates and risks are highest, and where populations are highest, some measure of population that is conditioned on crime rates is an optimal candidate for inclusion as a benchmark.

The challenge in following this strategy is to identify a valid measure of crime. Ideally, we would include measures of the race-specific crime rates in each tract (or other social area) to help construct precise benchmarks based on the participation in the behavior of interest by persons of each race and ethnicity. However, there are practical problems in this approach. For example, many crimes are unreported to the police, and there are no valid victim surveys from which we can measure crime rates. There are similarly no surveys of self-reported crimes. Race-specific arrest rates have been used as a proxy for race-specific crime rates, with a lag function that reduces (but hardly eliminates) the problem of correlated error terms between current enforcement and past enforcement. However, there is strong disagreement about the validity of prior arrest rates, with some analysts offering positive rationales, while others have been critical.

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249 Arrest data incorporate information about crime patterns, but also contain uncertainty about unobservable components because of police decisions about allocating officers to specific places.
An alternative measure is crimes reported to the police. However, crime reports don’t provide a complete picture of the racial makeup of the offenders in those crimes. While crime reports may provide a snapshot of the racial composition of those involved in crime commission, it is just that: a snapshot with only partial coverage of criminal activity. The data are further limited by the fact that many reported crimes lack a suspect identification or description. Moreover, some types of suspected crimes that motivate FIO activity, such as weapons possession or drug possession, often do not follow from crime reports that identify the race of a suspect, so these base rates of offending are unknown. Calls for service to the police are yet a third index, but 911 calls of this sort are difficult to apply to proactive patrol or the “new policing” given varying incidents of mistaken reports and the heterogeneity of the purpose of the calls that include serious crimes, cats in trees, multiple reports of the same gunshot, domestic disturbances, or car break-ins.²⁵⁰

To the extent that observed or reported crimes are leading indicators of those behaviors that are correlated with crime, crimes known to the police are important part of a valid benchmark. So too is population, as an index of the overall exposure of citizen as available targets for surveillance and interdiction. Accordingly, these analyses use both population and reported crime as benchmarks for understanding the racial distribution of FIO reports. Sensitivity tests applied alternate benchmarks including lagged race-specific arrest rates²⁵¹ and lagged race-specific suspect rates²⁵². Natural log of the Census tract population, total number of arrested individuals in Census tract, and total number of suspects reported in Census tract were used as the offsets in the regression models.

These analyses were designed to test whether monthly counts of FIO reports in Census tracts were disproportionate to the racial composition of tract residents, racial

²⁵⁰ Greg Ridgeway and John MacDonald, Race, Ethnicity and Policing: New and Essential Readings, 180 (S.K. Rice and M.D. White, eds. 2010).

²⁵¹ See, Robin S. Engel, Michael R. Smith and Francis T. Cullen, Race, Place, and Drug Enforcement, 11 Criminology & Public Policy 603 (2012) (claiming that 911 calls are a more robust and accurate measure of the relative crime problems in an area than are either arrests or reported crimes). But see, Stephen D. Mastrofski, Race, Policing, and Equity, 11 Criminology & Public Policy 593 (2012) (critiquing the use of 911 calls as a basis of suspicion to either allocate officers or justify the formation of suspicions necessary for a valid Terry stop or probable cause for a search or an arrest).

²⁵² Between 2007 and 2010, the BPD arrested 28,427 suspects. The racial distribution of arrested suspects was as follows: 50.4% Black, 26.8% White, 20.6% Hispanic, and 2.2% Asian or other race category. Using ArcGIS 10.2 mapping software, the BRIC was able to geocode 24,590 of these arrests to their respective Census tracts (86.5% of 28,427 total arrests). While a 100% geocoding rate is always desired, the geocode rate in the current study exceeds the minimum acceptable threshold of 85%. See Jerry H. Ratcliffe, Geocoding Crime and a First Estimate of a Minimum Acceptable Hit Rate, 18 International Journal of Geographical Information Science, 61-72 (2004).

²⁵³ As described earlier, between 2007 and 2010, there were 113,419 Part I UCR crime incidents in Boston. Victims in these incidents reported information on 340,585 suspects. The racial distribution of these suspects was as follows: 41.2% Black, 21.8% White, 17.3% Hispanic, 2.0% Asian or other race category, and 17.7% unknown race.
composition of arrested suspects in the tract, and the racial composition of crime suspects as reported by victims in crime incident reports, after controlling for the known crime rate in the previous month and other characteristics that are correlated with crime. For each racial composition benchmark, three race categories (percent Black, percent Hispanic, and Percent Asian / other) are included and the category of percent White is omitted. This was done to avoid collinearity in the model estimation. As such, the coefficients for each racial group are based on comparison with the percent White of the benchmark in the tract. When a racial composition variable is significant, this means that its relationship to FIO activity is significantly different from that of the White racial composition of that benchmark in the Census tract. The parameter estimates were expressed as incidence rate ratios (i.e., exponentiated coefficients) and robust standard errors clustered by tracts were used.

V. RESULTS

A. Suspects and Officers

Table 1 shows the characteristics of both suspects and officers. Suspect identifiers were available for 199,331 (97.4% of 204,739) FIO encounters between 2007 and 2010. From these, we were able to identify N = 72,619 unique subjects. Using gang intelligence databases maintained by BPD, we estimated that 5.5 percent (3,967 of 72,619) of the suspects in FIO encounters were classified as gang members. The number of FIO’s

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253 Incidence rate ratios are interpreted as the rate at which things occur; for example, an incidence rate ratio of 1.10 would suggest that, controlling for other independent variables, a one unit increase in the selected independent variable was associated with a 10% increase in the rate at which the dependent variable occurs. See, Sophie Rabe-Hesketh and Anders Skrondal, Multilevel and Longitudinal Modeling Using Stata, Volume II: Categorical Responses, Counts and Survival, 3rd ed. (2012). See, also, Kenneth Rothman and S. Greenland, Modern Epidemiology, 3rd ed. (2008).


255 See, Anthony A. Braga, David M. Hureau, and Leigh Grossman, Managing the Group Violence Intervention: Using Shooting Scorecards to Track Group Violence, 15 (2014). The Boston Regional Intelligence Center (BRIC) created a classification system using several parameters to identify individuals as gang members. To be classified as a gang member by BRIC, a person has to accumulate 10 points based upon the following criteria: prior validation by a BRIC-affiliated criminal justice agency that uses the same selection criteria (9 points), prior validation by a non-BRIC-affiliated criminal justice agency that uses similar selection criteria (8 points), self-admitted gang membership (8 points), use and/or possession of gang paraphernalia or identifiers (4 points), gang-related photograph (2 points), known gang tattoo or marking (8 points), information from reliable confidential informant (5 points), information from anonymous source or tipster (1 point), crime victim associated with rival gang (3 or 8 points depending on incarceration status), possession of gang documents such as by-laws (3 or 8 points depending on incarceration status), possession of gang publications (2 points), participation in gang publication
per suspect ranged from 1 to 249, with an average of 2.74 FIO events per suspect, during the study period. About half (48.5 percent) had been arrested, with the number of arrests ranged from 1 to 63, with a mean of 5 arrests.

Table 1 here

Most suspects were young: nearly half were younger than 25 years of age. One in three (33.7%) were between 18 and 24 years of age. Most were male (81.8%), consistent with known gender differences in crime rates by gender. Most suspects were Black (42.5%) or Hispanic (13.3%), each above their respective share of population in Boston in the 2010 census. Whites were under-represented in the FIO subject pool relative to population share. As we discussed earlier, population is a weak benchmark, and we control for local crime rates in subsequent analyses.

About half of the FIO suspects (48.5%) had one or more prior arrests, and half did not. To the extent that stops in general carry risks of social and psychological harms, the reach of FIOs to persons with no prior record extends an umbrella of suspicion to a group of primarily young people with no known criminal involvement.

Gangs are a focus of Boston police tactics. Yet few of the FIO suspects (5.5%) were known to the police as gang members. The department’s gang unit was proportionately small, with 3.7% of the population of officers whose shields were in the FIO database.

BPD Officers were older, not surprisingly, but their age distribution suggests that they were experienced. More than half were over 40 years of age (50.9%), with a median age of 41.3 years. Nearly two officers in three were White (65.1%), and about one in four were Black (23.9%). Most were assigned to patrol commands, with about one in eight (12.1%) holding a detective’s shield.

The number of repeat FIO reports per subject is concentrated among a small number of individuals who experience large numbers of FIO encounters. Table 2 shows that about two FIO subjects in three (67.5 percent) experienced one FIO. As a group, they accounted for 24.6 percent of the total number of FIO reports from 2007 – 2010. About one in 20 (5.2 percent) experienced 10 or more FIOs and, as a group, accounted

(8 points), possession of court and/or investigative documents involving an identified gang member (9 points), possession of printed or electronic media indicating membership (1 point), contact with known gang members via Field Interrogation Observation reports (2 points per report), named in police incident report involving known gang member (4 points per report), possession of gang membership material (9 points), information developed during surveillance and/or surveillance (5 points), and other information (1 point).


for 40.2 percent of the total number of FIO reports made by BPD officers during this time.

Table 2 here

FIO forms also report the badge numbers of the BPD officers who filled out the reports. Officer badge numbers were available for N=200,103 FIO reports (97.7% of 204,739). BPD personnel records identified 2,359 unique officers in its workforce between 2007 and 2010, including new hires and retirements during that time period. Personnel records were used to determine officer demographic information, years on the job, rank, assignment, and detective status for all sworn BPD officers. Badge numbers on FIO reports were used to identify the N=1,750 unique BPD officers.

Table 3 here

About three officers in four (74.2% of 2,359) made one or more FIO reports between 2007 and 2010. The counts ranged from 1 to 2,315 FIOs. Officers averaged 84.3 FIOs over the four years, or 21 per year. Table 3 shows that, similar to the distribution of repeat FIOs among subjects, the number of repeat FIO reports per officer is also highly concentrated among a small number of individuals (Table 3). Nearly half (45.0 percent) generated fewer than 50 FIO reports and, as a group, accounted for 6.9 percent of the total number of FIO reports during the study time period. A small group (4.0 percent, or approximately 70 officers) generated 500 or more FIOs; they accounted for 43.3 percent of the total number of FIO reports made by BPD officers from 2007 - 2010.

C. Race, Crime and FIOs

1. FIOs by Neighborhood Crime and Social Conditions

Table 4 shows the results of the estimates of FIO activity using alternate benchmarks for racial composition. The monthly number of total Index crimes (logged, lagged) in a tract was a consistently significant positive predictor of the monthly count of FIO reports in a tract across models with varying benchmarks. This suggests that the intensity of BPD FIO activity in a tract is associated with the amount of serious crime experienced in a tract controlling for other conditions. An increase of 1 percent more total index crime incidents in the previous month leads to an increase of 10.6 percent (IRR=1.106) FIO reports in the following month. This is a large effect, considering that the average Boston census tract experiences 12.2 index crimes per month. Each of the models in Table 4 show that the Boston police prioritized crime problems in the allocation of FIO activity by tract and police district during this period.

Table 4 here

After controlling for crime, Table 4 also shows that the racial composition variables for Percent Black and Percent Hispanic are positive and significant for all three models. The pattern of race effects suggests evidence of disparate treatment in FIO activity based on neighborhood racial composition. After controlling for local crime rates, we observe higher rates of FIO activity for census tracts based on their Black or Hispanic racial composition, whether in residents, arrestees, or the race of known crime
suspects. In each of these specifications, the percentage of Foreign Born Residents in a tract was also a statistically-significant predictor of increased FIO activity. Since foreign born residents of Boston are primarily persons of color, the focus of FIO activity in those neighborhoods reinforces the notion of disparate treatment by race and ethnicity.

The consistent size and direction of the race and ethnicity coefficients suggests a robust race effect controlling for crime, police activity, and other relevant factors, even if they were modest in size. Still, even modest effects can have practical significance. The disparity in the monthly count of FIO reports can be meaningful in census tracts with larger shares of minority residents, arrestees, and reported suspects. Using the residential racial composition variable as an example, the incidence rate ratio on Percent Black suggests that a one-unit increase in the black percentage of residents relative to the white percentage of residents in a Census tract is associated with a 2.2 percent increase (IRR=1.022) in the monthly count of FIO reports made by the BPD controlling for crime and other factors. The effects of race (and foreign born residents) in Table 4 were observed after controlling for the number of officers deployed in each police district, a measure of the exposure of local residents to police and their availability for FIO contacts.

Figure 3 here

Figure 3 shows the marginal increase in the predicted count of monthly FIO reports in a census tract as the percentages of Black and Hispanic residents in a tract increase. The figure shows the nearly linear and monotonic increase in the adjusted (for predictors) monthly count of FIO reports increases as the percentages of minority residents increases in a tract. Simply to illustrate, Figure 3 shows that a tract with 85 percent black residents would experience an additional 53 FIO reports per month compared to a tract with 15 percent black residents. Over the course of one year, residents in that tract would be subjected to an additional 636 FIO reports and, over the four-year study time period, this difference would represent an additional 2,544 FIO reports in that tract.

Because crime and racial composition are unevenly distributed across tracts and neighborhoods in Boston, similar to other cities, we tested for the possible leverage of outliers in the estimates in Table 4. That is, both of the central findings in Table 4 on crime and race could reflect the undue leverage and influence of neighborhood outliers in each of these distributions. For example, Figure 2 shows the concentration of crimes and race in particular corners of the city. To test for the effects of outliers, we conducted a sensitivity test by trimming 20 percent of tracts at the extremes of the FIO activity distributions. The results were largely unchanged. Using a population benchmark

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259 For an example of an estimation of leverage effects of outliers, see Richard A. Berk, New claims about executions and general deterrence: Déja Vu all over again? 2 Journal of Empirical Legal Studies 303 (2005) (showing the undue influence of Texas in state-year fixed effects estimates of the deterrent effects of executions on homicides).
(Model 1 in Table 4), the IRR for percent Black population decline from 1.022 to 1.018 in the narrower model. For crime, the IRR of crime on FIO counts dropped from 1.106 to 1.088. In other words, the FIO / race / crime relationship is robust to the removal of the extremes.

2. FIO Activity by Suspect Characteristics

FIOs are a first-stage intrusion by police on individual liberty and privacy. But in Boston, the use of non-contact FIOs carries a lower level of intrusion. While privacy may be violated in the sense that one’s movements in these contacts are recorded by a police officer acting on behalf of the state, a non-contact incident does not have the same physical intrusion nor temporary detention and liberty implications of a full contact stop. To compare race effects on contact versus non-contact encounters, we estimated negative binomial regressions of subject race and other individual characteristics on FIO counts. The models were estimated with and without gang membership status and arrest history to examine how individual criminality might mediate any observed race effects. Model 1 in Table 5 shows the results for all FIO encounters. Model 2 controls for arrest history and gang membership, an adjustment that acknowledges the more intense surveillance and contact rates with suspected gang members or persons suspected by the police to be involved in criminal activity. Model 3 re-estimates Model 2 for only non-contact FIO encounters.

Table 5 here

In Model 1, Black and Hispanic suspects have significantly higher FIO activity compared to Whites. The effect size for Blacks is especially large and more modest for Hispanic suspects. For Asian and Other Race suspects, they are less likely to be the subject of an FIO encounter compared to Whites, and the results also are significant. Older suspects and females are less likely to be subjects of FIO encounters.

Comparing Models 1 and 2, prior arrest history and gang membership each mediate the influence of race on the number of FIO encounters experienced by subjects, reducing the size of the race estimates but they remain statistically significant. Model 1 shows that compared to White subjects, Black subjects experienced 72.5 percent more FIO encounters per month across the city and Hispanic subjects experienced 13.6 percent more FIO encounters. When the prior arrest and gang status covariates are included, in Model 2, Black subjects experienced only 8.8 percent more FIO encounters per month and Hispanic subjects experienced 3.1 percent fewer FIO encounters compared to their White counterparts. The results for Asians and Other or Unknown race suspects remains unchanged. Gangs evidently are a priority in using FIO authority, and account for at least some of the racial disparity in FIO encounters.

The pattern for non-contact FIO activity in Model 3 is similar to the pattern shown in Model 2. The effects of gang membership increase from Model 2 to Model 3, suggesting even greater attention to gang members, albeit without contact or interpersonal interaction. This makes sense, since gang members or reputed gang members are well known to the specialized Youth Violence Strike Force (YVSF, informally known as the gang unit), and their observations can be recorded for
surveillance and intelligence purposes. Perhaps observing gang member movements and associations has intelligence payoffs, which might explain and rationalize the use of police powers in this way. Since there is a privacy but not liberty interest at stake in these non-contact encounters, there is little regulatory leverage in this practice.

The importance of Table 5 is the finding suggesting intense police attention to gang members by Boston police. Gangs are thought to be an important source of the city’s gun violence problem, which leads to this attention, and gang membership also is skewed by both individual and neighborhood racial composition.

3. Frisks and Searches by Suspect Race

Table 6 shows that Black and Hispanic suspects were more likely to be frisked or searched during an FIO encounter, after controlling for non-racial suspect characteristics. Compared to White suspects, Black suspects were 12.4 percent more likely to be frisked / searched, and Hispanic subjects were 4.5 percent more likely to be frisked / searched during FIO encounters with arrest and gang status covariates included in the model. Gang members were 11.7 percent more likely to be frisked / searched during FIO encounters relative to their non-gang counterparts, controlling for other factors. For every additional arrest in their history, suspects were 1.8 percent more likely to be frisked or searched during FIO encounters. Asian and other race subjects were significantly less likely to be frisked / searched during FIO encounters when compared to White subjects. Here, the gang effect that explained FIO activity in Table 5 seems to have comparable and independent influence on the decision to frisk as does the suspect’s race.

Table 6 here

Taken together, Tables 5 and 6 show troubling racial disparities in the number of repeated FIO contacts and the probability of being frisked / searches experienced by Black and Hispanic suspects. The effects in these tables are adjusted for the influences of age, gang membership, neighborhood and other relevant non-race influences. In fact, we see the frisk estimates in Table 6 as conservative and expected to see even greater effects by suspect race considering the attention to gangs in this setting and BPD’s use of FIOs for intelligence gathering purposes, especially among gang members. Other Terry stop “programs” do not document non-contact observations, in line with the Supreme Court dicta limiting constitutional regulation to the physical aspect of investigative stops.

The large FIO differences in counts of encounters – both observational and face-to-face – compared to the incidence of frisks or searches suggests more extensive use of FIO reports to monitor gang members at a distance rather than repeatedly initiating physical


261 Slobigin, Privacy at Risk, supra __.
contact to search them for weapons, drugs, or other contraband. Perhaps this is a safety consideration, or it may be that there are information yields from non-contact encounters, such as understanding gang membership and associations, that can address tactical and policy goals. Whatever the purpose and rational, more research is needed on the reasons and circumstances for this component of the FIO strategy, as well as its informational payoff.

4. FIO Activity by Unit and Officer Race

Table 7 shows the effects of officer characteristics on FIO patterns. There were large differences in FIO activity by officer race or ethnicity. Black officers made 42.5 percent fewer FIO reports per month compared to White officers, controlling for age, sex, rank, detective status, and assignment. Asian officers also made significantly fewer FIO reports. Relative to White officers, Asian officers made 44.8 percent fewer FIO reports controlling for officer demographic, rank, and assignment covariates. Hispanic officers made slightly smaller numbers of FIO reports than their White officers but the observed differences were not statistically significant. Controlling for assignment, rank, and other factors, older officers and female officers made significantly fewer FIO reports relative to their younger and male counterparts, respectively.

Table 7 here

Unit assignment also was a significant predictor of officers’ FIO activity. BPD officers assigned to the YVSF make almost 12 times as many FIO reports per month compared to officers assigned to other specialized units or policing districts, controlling for other factors. Their mission explains in part this emphasis: YVSF officers are charged with preventing outbreaks of gang violence. Completing FIO reports on gang member whereabouts, their associations and routine activities represent a central activity in pursuing that mission by massing information on the routine activities of gang members.

Compared to line level patrol officers, Captains, Deputy Superintendents, and Superintendents make significantly fewer FIO reports holding other officer characteristics constant. These high-ranking officers have extensive managerial responsibilities and, while they maintain a presence in the community, they are much less likely to be engaging in street-level law enforcement work.\(^\text{262}\)

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\(^{262}\) The model used for the estimates in Table 7 is a zero-inflated negative binomial regression, which is employed in situations where there are large numbers of observations of zero events in the data and there are separate functions to determine any participation and then frequency of participation. See, for example, Kelvin KW Yau, Kui Wang, and Andy H. Lee, \textit{Zero-Inflated Negative Binomial Mixed Regression Modeling of Over-Dispersed Count Data with Extra Zeros}, 45 Biometrical Journal 437 (2003). This regression first estimates factors that explain when there are one or more events, and then explains the count of those events given one or more. The first stage analyzes the inflation factors associated with \textit{any} participation. The medical leave and administrative position variables were statistically significant predictors of zero FIO activity during the study time period, controlling for other factors. BPD officers who were not able to perform their duties or were assigned to administrative positions generally do not complete FIO reports.
The heavy influence of the YVSF officers on FIO activity, coupled with the race-specific patterns shown in Table 7, leads to a further question: whether FIO activity within the YVSF command also varies by officer race. Table 8 shows the results of regressions with only officers having one or more FIO encounters, and disaggregating officers by race and YVSF assignment. The six groups shown in Model 2 in Table 8 are compared to Asian and Other Race officers, a move that exploits the fact that there are so few Asian officers in the YVSF. This permits direct comparisons of the regression estimates in Model 2.

Table 8 here

Model 1 in Table 8 shows, for this narrower sample of officers, that White and Hispanic officers had substantially more FIO encounters that White officers. Without controlling for assignment, the effect size for White officers is more than three times the size for Black officers; the effect size for Hispanic officers is more than three times the size for White officers. Model 2 shows that this effect is an artifact of YVSF assignment. Within officer race, YVSF officers have far more frequent FIO activity than their non-YVSF counterparts. The differences again are very large. White YVSF officers have about 6.5 times more FIO encounters per month than White officers in other units. The differences for Black and Hispanic officers in the YVSF units are even greater.

Here again, we see the importance of the YVSF unit in explaining racial disparities in FIO encounters between citizens and police. This is not to say that there is no evidence of racially disparate treatment by officers in other commands; the data show that in fact, regardless of command, White officers and Hispanic officers are more active in FIO work. Rather, Table 8 shows that within this focus of police efforts, the race disparities within officer racial categories are quite large, and officers from all racial and ethnic groups are more active once assigned to this command. The results suggest an institutional dimension to explain officer FIO activity that is separate from an individual officer’s taste or preference for discrimination.

5. Frisks and Searches by Officer Race and Assignment

Table 9 shows differences in frisk/search probability by officer race and assignment. Black officers were 15.0 percent less likely to frisk/search subjects during FIO encounters when compared to White officers, controlling for age, sex, rank, detective status, and assignment. Asian officers were also less likely to frisk/search FIO subjects. Relative to White officers, Asian officers were 32.6 percent less likely to frisk/search subjects during FIO encounters controlling for officer demographic, rank, and assignment covariates. Hispanic officers were only 4.4 percent less likely to frisk/search subjects during FIO encounters holding the other variables constant; that result was not statistically significant. More experienced officers and female officers were significantly
less likely to frisk / search subjects during FIO encounters relative to their younger and male counterparts, respectively, controlling for assignment, rank, and other factors.

Table 9 here

Two assignments show extremely elevated rates of frisk / search activity. Detectives were 49.5 percent more likely to frisk / search subjects during FIO encounters relative to non-detectives, controlling for assignment, rank, and other factors. Given their responsibility for investigating unsolved crimes, detectives were presumably more likely to frisk / search FIO subjects for evidence of criminal activity during the course of an investigation. YVSF officers were 24.3 percent more likely to frisk / search subjects during FIO encounters relative to non-YVSF officers, controlling for assignment, rank, detective status, and other factors. YVSF officers focus FIO encounters on gang members who pose a higher risk of carrying weapons relative to other FIO subjects, which explains in part their preferences for search relative to other BPD officers. Compared to line level patrol officers, Sergeants, Lieutenants Captains, Deputy Superintendents, and Superintendents were significantly less likely to frisk / search subjects during FIO encounters holding other officer characteristics constant.

Despite the frequent FIO activity by YVSF officers, these results suggest that they exercise caution in proceeding from an encounter to a frisk or search. YVSF officers were far more active in FIO activity, by orders of magnitude, than their non-YVSF counterparts, yet only a fraction of their encounters proceeded to a frisk or search.

The disparity between FIO encounters by this group and frisks or searches could suggest problems in their formation of the requisite suspicion necessary to conduct a frisk or search contingent on a stop. The high rate of non-frisk encounters suggests a reduced level of reasonable suspicion in many encounters that falls below constitutional thresholds permitting a frisk – primarily officer safety or suspicion of weapon possession – or a search. Searches require probable cause, a stricter standard.263 Another interpretation of this gap could simply be that the purpose of YVSF encounters is simply to establish contact, to signal to young males under suspicion that the police are present and watching, and to gather intelligence. This may be a reasoned activity in terms of policy, but it falls short of being reasonable, under constitutional requirements for even a momentary deprivation of liberty and detention. ‘Getting it wrong’ at a high rate suggests problems in the bases of suspicion animating a stop, a finding with implications for constitutional regulation of FIO activity.264

5. Officer-Suspect Racial Asymmetries

The higher incidence of FIO encounters for non-White suspects and also encounters initiated by White officers suggest the possibility of discrimination. But these results leave open the question of statistical versus preference-based discrimination.


264 See, for example, Floyd v City of New York, Opinion and Order, supra note 43 (finding that the high rate of unproductive stops was a sign of inaccuracy in the formation of reasonable suspicion that is a prerequisite to an investigative stop).
Most studies testing for discrimination have relied on “hit rates” or the probability of guilt to distinguish between these forms of discrimination. Generally, we assume an absence of preference-based discrimination if racial differences in police encounters are independent of the race of the police officer. But if there are differences in, for example, the stops of Black suspects by White officers compared to Black officers, we might conclude preferences for discrimination. But that evidence alone is only a partial explanation. Those preferences might be explained by the greater ease with which officers may be able to approach and conduct searches of persons of their own race or ethnicity. Alternately, if officers are not randomly assigned to neighborhoods, then Black officers in White neighborhoods where crime rates may be lower will conduct fewer stops of Whites. Since crime rates are higher in predominantly Black neighborhoods in Boston and other cities, the opposite condition would be observed: White officers would have more encounters with Black suspects. Testing for discrimination using these metrics therefore requires not only knowledge of officer and suspect race, but also controls for the crime rates of the different areas where they patrol and encounter suspects.

Table 10a shows the results of analyses that disaggregate patterns of FIO encounters by both officer race and suspect race four racial groups. We estimated models of the count of FIO encounters using negative binomial regressions, following the functional form used in the previous models of FIO activity. Controls included age and gender of the suspect and age, gender, rank and assignment for officers. Separate models were conducted for each officer race group. Fixed effects for police districts controlled for differential exposure of officers to crime and to different local racial concentrations. The first three columns compare FIO reports of each suspect racial group by officers of each race to FIO reports done by White officers. The fourth column compares FIO’s by White officers to FIO reports of Black Officers. The cells in Table 10a show the incidence rate ratio for each comparison. To test for different discrimination patterns in frisks and searches, we use multilevel logistic regression models as the functional form to estimate the probability of a frisk or search across racial groups. The results in Table 10b show the odds ratio for each comparison.

Table 10a and 10b here

Table 10a shows higher FIO activity for White officers for suspects of all races, including White suspects, compared to Black officers. White officers have significantly more encounters with White suspects than they have with suspects of other races. On the surface, this suggests greater FIO activity compared to Black officers across all suspect

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266 This approach also discounts the problem of the “suspicious outsider,” or the person who crosses neighborhoods of different racial composition. This problem may be more salient in studies of vehicle stops where crossing of neighborhood boundaries is more common and feasible. Here, our analysis examines pedestrian stops almost exclusively.
race groups, but not preferences for stops of one racial group over others. Column 1 shows that Black officers, compared to White officers, are significantly less active across all suspect race groups, again suggesting discrimination other than preference-based.

However, comparing within-suspect race results across rows, suggests preferences for discrimination by White officers. FIO activity Column 4 in Table 10a shows that White officers have about 55 percent more FIO encounters per month with Black suspects compared to Black officers. Black officers have 35 percent fewer stops per month of Black suspects compared to White officers. This between-officer within-suspect comparison suggest preferences by White officers compared to Black officers in FIO activity for Black suspects. Similar differences are evident between Black and White officers in stops of Hispanic suspects, Asian suspects, and White suspects.

The pattern for frisks and searches in Table 10b is similar. White officers are more likely to frisk or search both Black and White suspects compared to cross-racial frisks or searches by Black officers. Black officers again show lower rates of frisks and searches compared to White officers, and are equally likely to frisk or search both White and Black suspects. White officers are 23 percent more likely to frisk or search a Black suspect, but Black officers are 19 percent less likely to search Black suspects compared to White officers. Hispanic officers are less likely compared to White officers to frisk Black and White suspects, while White officers are more likely than Hispanic officers to frisk or search both Black and White suspects.

One way to understand Table 10a is that while White officers may not discriminate between suspects of different races, they do have stronger preferences for stops between races than Black officers. This is evident for suspects of all races. This presents a more complex picture of the preference-statistical discrimination distinction than previous studies have reported. White officers are more active than are Black or Hispanic officers in FIO activity overall, but they also prefer within each race to conduct FIOs relative to Black officers. There may not be preferences by race, but there does appear to be stronger preferences for FIO activity overall. Put another way, White officers are biased toward everyone compared to Black, Hispanic or Asian officers.

Given the higher rates of FIO encounters by YVSF officers, we tested to see if the results in Tables 10a and 10b would be robust to the exclusion of those officers. The results led to the same conclusions, with only minor changes in the regression coefficients and standard errors once the YVSF officer were excluded. We observed the same mixed pattern of statistical and preference-based discrimination that analyses with the full sample produced.

VI. CONCLUSION

Two features of Boston’s practices of investigative stops distinguish it from the “new policing” regimes in other large cities. First, Boston focuses a significant portion of its field investigation activity on suspected and actual gang members. Boston police have pursued this targeted strategy within its FIO activity for quite some time. This is

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consistent with the elevated rates of crime, especially youth crime, in the neighborhoods with the highest concentration of gang members. In contrast, New York City’s investigative stop program only recently reoriented from widespread investigative stops to a more spatially concentrated effort focused on gang activity in public housing sites. Second, the Boston Terry stop design includes both contact encounters and non-contact observations of suspects. Intelligence and surveillance may not be unusual in other cities, but rarely is there formal recording of observations that do not convert into contact encounters. Boston is distinctive in recording those observations in the same database as its contact encounters.

The records of these encounters provided a basis to assess the claims of discrimination that have infected the contemporary practice of Terry stops or investigative stops as practiced in the “new policing.” We conducted analyses to assess the allocation of officers and FIO activity by neighborhood and suspect race, using metrics and methods that were cited in recent litigation on other investigative stop programs. Both crime and race contribute to variations over time and place in FIO activity. The regressions are estimated so that effects of each are mutually adjusted. The results are robust to the exclusion of the specialized and very active YVSF gang unit, suggesting a generalized pattern of preferences for encounters with Black and Hispanic suspects. We expect a rational allocation of police activity to match variation in times and places with local crime rates, and we observe that to be the case. But we also observe a marginal effect of racial composition in census tracts, suggesting statistical discrimination in those areas.

The evidence suggests a complex answer to the question of whether that discrimination is evidence of bias. Using a racial mismatch model, we find that White officers were consistently more active than Black or Hispanic officers in conducting FIO reports, regardless of suspect race. But within suspect race, the preference of White officers to FIO Black suspects is far greater than Black officers’ preference to FIO Black or Hispanic suspects. And Black officers are less likely to FIO a White suspect than is a


269 Richard Aborn, Crime Commission Statement on NYPD’s Operation Crew Cut, October 2, 2012 at www.nycrimecommission.org/pdfs/ccc-10-02-12.pdf (“Operation Crew Cut is a smart and proactive approach to curtail youth violence which accounts for 30% of shootings in New York City. By utilizing the latest technology, doubling the size of the gang violence unit and coordinating closely with District Attorneys, the New York City Police Department is making effective use of targeted resources to not only combat crime, but create an overall deterrent effect. This is the kind of smart solution which keeps our crime rate low, and continues to set New York apart as one of the safest large cities in the world.”)

White officer. These patterns are robust to the exclusion of YVSF officers, which suggests that the results also are robust to the exclusion of gang members.

Is this evidence of bias, or preference-based discrimination? The data are not well suited to answer this question. We defined statistical discrimination as a rational decision to focus efforts on one group or to exclude another group from engagements of any sort. Following Gary Becker’s notion of discrimination, officers who stopped Black or Hispanic suspects more often perceived a net benefit in the form of increased attention to crime detection. As a matter of efficiency, attention to populations with a lower probability of return would sacrifice the returns in crime control from allocating attention to the presumed higher rate group. Becker also identifies discrimination based on tastes or values, where the decision maker discounts known facts. In the case of FIO activity, decision makers may inflate crime propensity beyond its true value, leading to a subjectively rational but still preference-based form of discrimination. In our study, the marginal rate of FIOs by census tract based on racial composition after controlling for local crime rates may reflect that type of process. It suggests that subjective evaluations of the returns from a FIO encounter may be inflated based on race-based distortions of information.

Kenneth Arrow described this process as “positive valuation” of one group with higher expected return, even if that valuation is inflated. Arrow describes sources of “cheap information” that might help a decision maker to identify a discrimination target at low cost: skin color, poor neighborhoods, or other substitutes for crime. These sources of cheap information may also prime decision makers – police officers, in this case – to increase their valuation of the suspect’s behavior. Since there is no cost for a wrong decision, there are only weak incentives to correct or update that information.

Using the officer-suspect racial mismatch metric, the results suggest preference-based discrimination. But without additional evidence of the outcomes of FIO encounters, or the reasonable suspicion bases animating these events, we cannot fully explain the motivations for FIO encounters. These encounters may be efficient, or they may simply be a form of routine administrative searches based on actuarial suspicion in the absence of individualized or particularized suspicion or a clear expectation of arrests or seizures of weapons. These encounters may simply reflect an institutional bias or norm based on a closed system of information that reinforces command staff and individual officers’ prior beliefs about whom to observe or engage, setting aside

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271 See, Gary S. Becker, The Economics of Discrimination (2nd ed.) 10 (1971)


273 Eve Bresinke Primus, Disentangling Administrative Searches, 111 Colum. L. Rev. 254 (2011) (defining administrative search exceptions to Fourth Amendment regulation to justify airport searches, subway backpack searches, employer drug testing, and vehicle checkpoints).

274 See, e.g., Brooks Holland, The Road ’Round Edmond: Steering Through Primary Purposes and Crime Control Agendas, 111 Penn State L. Rev. 293 (2006) (citing the U.S. Supreme Court opinion in Edmond v City of Indianapolis, 531 U.S. 37, 44 (2000), stating that “[w]e cannot sanction stops justified only by the generalized and ever-present possibility that interrogation and inspection may reveal that any giver motorist has committed some crime”).
questions of fairness or efficiency. FIOs here may be based on location, peer network, or other actuarial markers that substitute for individual markers of suspicion, raising Fourth Amendment concerns. Explaining how those factors translate into perceptions, decisions, attributions or behaviors requires different research designs.²⁷⁵

Still, evidence of officer race disparities suggests that there is more than just statistical discrimination or institutional preference at work here. That these stops are disproportionately target minority suspects in non-white neighborhoods beyond what local crime rates predict, raises Equal Protection concerns that seem to be collateral consequences of the “new policing.”

²⁷⁵ Several studies have used research designs that vary race and use a variety of cues to assess how race consciously or subconsciously affects decision making by legal actors. These studies differ from the officer-suspect mismatch paradigm in that they examine specific cues that influence officers’ perceptions and permit bias to infect decisions from shooting at suspects to the construction of pre-sentence probation reports for trial courts. Many are laboratory experiments, which strengthens their internal validity but to some extent at the cost of external validity considerations of context and multiple causation. Others exploit natural variation in legal settings to discern the influence of race on decision making, increasing their external validity but at some unknown cost to internal validity and measurement equivalence on race. In most but not all cases, these studies show evidence of bias toward African-American suspects or defendants. See, for example, Joshua Correll, Sean M. Hudson, Steffanie Guillermo and Debbie S. Ma, The Police Officer’s Dilemma: A Decade of Research on Racial Bias in the Decision to Shoot, 8 Journal of Social and Personality Compass 201 (2014); Joshua Correll, Bernd Wittenbrink, Matthew T. Crawford, and Melody S. Sadler, Stereotypic Vision: How Stereotypes Disambiguate Visual Stimuli, 1089 Journal of Personality and Social Psychology 219 (2015); Modupe Akinola, and Wendy Berry Mendes, Stress-Induced Cortisol Facilitates Threat-Related Decision Making among Police Officers, 126 Behavioral Neuroscience 167 (2012); Sandra Graham and Brian S. Lowery, Priming Unconscious Racial Stereotypes about Adolescent Offenders, 28 Law and Human Behavior 483 (2004); Jennifer L. Eberhardt, Paul G. Davies, Valerie J. Purdie-Vaughns, and Sheri Lynn Johnson, Looking Deathworthy: Perceived Stereotypicality of Black Defendants Predicts Capital-Sentencing Outcomes, 17 Psychological Science 383 (2006); Geoffrey P. Alpert, John M. MacDonald, and Roger G. Dunham, Police Suspicion and Discretionary Decision Making During Citizen Stops, 43 Criminology 407 (2005); George S. Bridges and Sara Steen, Racial Disparities in Official Assessments of Juvenile Offenders: Attributional Stereotypes as Mediating Mechanisms, 63 American Sociological Review 554 (1998).
Table 1. Age, Gender, and Race of Unique BPD FIO Subjects and Officers

<table>
<thead>
<tr>
<th></th>
<th>FIO Subjects, N=72,619</th>
<th>FIO Officers, N=1750*</th>
</tr>
</thead>
<tbody>
<tr>
<td>Gender</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Male</td>
<td>59,438</td>
<td>1,558</td>
</tr>
<tr>
<td>Female</td>
<td>13,181</td>
<td>192</td>
</tr>
<tr>
<td></td>
<td>81.8%</td>
<td>89%</td>
</tr>
<tr>
<td></td>
<td>18.2%</td>
<td>11%</td>
</tr>
<tr>
<td>Age</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Below 18</td>
<td>9,201</td>
<td>0</td>
</tr>
<tr>
<td>18 – 24</td>
<td>24,471</td>
<td>10</td>
</tr>
<tr>
<td>25 – 30</td>
<td>12,375</td>
<td>208</td>
</tr>
<tr>
<td>31 – 35</td>
<td>6,417</td>
<td>286</td>
</tr>
<tr>
<td>36 – 40</td>
<td>5,636</td>
<td>356</td>
</tr>
<tr>
<td>41 – 50</td>
<td>9,650</td>
<td>609</td>
</tr>
<tr>
<td>51 and older</td>
<td>4,869</td>
<td>281</td>
</tr>
<tr>
<td></td>
<td>12.7%</td>
<td>0.6%</td>
</tr>
<tr>
<td></td>
<td>33.7%</td>
<td>11.9%</td>
</tr>
<tr>
<td></td>
<td>8.8%</td>
<td>16.3%</td>
</tr>
<tr>
<td></td>
<td>7.8%</td>
<td>20.3%</td>
</tr>
<tr>
<td></td>
<td>13.3%</td>
<td>34.8%</td>
</tr>
<tr>
<td></td>
<td>6.7%</td>
<td>16.1%</td>
</tr>
<tr>
<td>Mean</td>
<td>29.2</td>
<td>41.3</td>
</tr>
<tr>
<td>Median</td>
<td>26</td>
<td>41</td>
</tr>
<tr>
<td>Range</td>
<td>12 to 71 years</td>
<td>23 to 64 years</td>
</tr>
<tr>
<td>Race</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Black</td>
<td>30,849</td>
<td>418</td>
</tr>
<tr>
<td>White</td>
<td>25,758</td>
<td>1,139</td>
</tr>
<tr>
<td>Hispanic</td>
<td>9,693</td>
<td>150</td>
</tr>
<tr>
<td>Asian / Other</td>
<td>1,321</td>
<td>43</td>
</tr>
<tr>
<td>Unknown</td>
<td>4,998</td>
<td>0</td>
</tr>
<tr>
<td>Selected Characteristics</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Subjects</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Gang member</td>
<td>3,967</td>
<td>5.5</td>
</tr>
<tr>
<td>Prior arrest (1+)</td>
<td>35,256</td>
<td>48.5</td>
</tr>
<tr>
<td>Officers</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Gang Unit (YVSF)</td>
<td>65</td>
<td>3.7</td>
</tr>
<tr>
<td>Detective (any rank)</td>
<td>212</td>
<td>12.1</td>
</tr>
<tr>
<td>Patrol Officer</td>
<td>1,379</td>
<td>78.8</td>
</tr>
<tr>
<td>Patrol Sergeant</td>
<td>130</td>
<td>7.4</td>
</tr>
<tr>
<td>Patrol Lieutenant / Captain</td>
<td>23</td>
<td>1.3</td>
</tr>
<tr>
<td>Dep. Supt. / Superintendent</td>
<td>6</td>
<td>0.3</td>
</tr>
</tbody>
</table>

a. These are the officers who have had one or more FIO encounter over the study interval.
Table 2. FIO Report Distribution by Unique Subjects

<table>
<thead>
<tr>
<th>N of FIOs</th>
<th>N Subjects</th>
<th>% Subjects</th>
<th>Cum. % Subjects</th>
<th>Sum FIOs</th>
<th>% FIOs</th>
<th>Cum. % FIOs</th>
</tr>
</thead>
<tbody>
<tr>
<td>51+</td>
<td>211</td>
<td>0.3</td>
<td>0.3</td>
<td>14,886</td>
<td>7.5</td>
<td>7.5</td>
</tr>
<tr>
<td>25 – 50</td>
<td>671</td>
<td>0.9</td>
<td>1.2</td>
<td>22,314</td>
<td>11.2</td>
<td>18.7</td>
</tr>
<tr>
<td>10 – 24</td>
<td>2,933</td>
<td>4</td>
<td>5.2</td>
<td>42,787</td>
<td>21.5</td>
<td>40.2</td>
</tr>
<tr>
<td>5 – 9</td>
<td>4,926</td>
<td>6.8</td>
<td>12</td>
<td>31,798</td>
<td>15.9</td>
<td>56.1</td>
</tr>
<tr>
<td>2 – 4</td>
<td>14,860</td>
<td>20.5</td>
<td>32.5</td>
<td>38,528</td>
<td>19.3</td>
<td>75.4</td>
</tr>
<tr>
<td>1 only</td>
<td>49,018</td>
<td>67.5</td>
<td>100</td>
<td>49,018</td>
<td>24.6</td>
<td>100</td>
</tr>
<tr>
<td>Total</td>
<td>72,619</td>
<td>100</td>
<td>100</td>
<td>199,331</td>
<td>100</td>
<td>100</td>
</tr>
</tbody>
</table>

Table 3. FIO Report Distribution by Unique BPD Officers

<table>
<thead>
<tr>
<th>N of FIOs</th>
<th>N Officers</th>
<th>% Officers</th>
<th>Cum % Officers</th>
<th>Sum FIOs</th>
<th>% FIO</th>
<th>Cum % FIO</th>
</tr>
</thead>
<tbody>
<tr>
<td>1,000+</td>
<td>28</td>
<td>1.2</td>
<td>1.2</td>
<td>42,399</td>
<td>21.2</td>
<td>21.2</td>
</tr>
<tr>
<td>500 - 999</td>
<td>65</td>
<td>2.8</td>
<td>4</td>
<td>44,153</td>
<td>22.1</td>
<td>43.3</td>
</tr>
<tr>
<td>250 - 499</td>
<td>128</td>
<td>5.4</td>
<td>9.4</td>
<td>44,809</td>
<td>22.4</td>
<td>65.7</td>
</tr>
<tr>
<td>100 - 249</td>
<td>253</td>
<td>10.7</td>
<td>20.1</td>
<td>39,693</td>
<td>19.8</td>
<td>85.5</td>
</tr>
<tr>
<td>50 - 99</td>
<td>214</td>
<td>9.1</td>
<td>29.2</td>
<td>15,179</td>
<td>7.6</td>
<td>93.1</td>
</tr>
<tr>
<td>1 - 49</td>
<td>1,062</td>
<td>45</td>
<td>74.2</td>
<td>13,870</td>
<td>6.9</td>
<td>100</td>
</tr>
<tr>
<td>Zero</td>
<td>609</td>
<td>25.8</td>
<td>100</td>
<td>0</td>
<td>0</td>
<td>100</td>
</tr>
<tr>
<td>Total</td>
<td>2,359</td>
<td>100</td>
<td>100</td>
<td>200,103</td>
<td>100</td>
<td>100</td>
</tr>
</tbody>
</table>
Table 4. Negative Binomial Regressions of Monthly FIO Report Counts Controlling for Census Tract Characteristics, Crime, Police Activity, and Other Conditions for Three Racial Benchmarks (IRR’s, SE, p)

<table>
<thead>
<tr>
<th></th>
<th>Residents</th>
<th>Arrestees</th>
<th>Crime Victims</th>
</tr>
</thead>
<tbody>
<tr>
<td>Percent Black</td>
<td>1.022 (.006) **</td>
<td>1.025 (.005) **</td>
<td>1.029 (.009) **</td>
</tr>
<tr>
<td>Percent Hispanic</td>
<td>1.041 (.008) **</td>
<td>1.016 (.008) *</td>
<td>1.040 (.011) **</td>
</tr>
<tr>
<td>Percent Asian / other</td>
<td>1.020 (.012)</td>
<td>0.917 (.052)</td>
<td>0.967 (.063)</td>
</tr>
<tr>
<td>Percent Unknown Race</td>
<td>----</td>
<td>----</td>
<td>0.922 (.015) **</td>
</tr>
<tr>
<td>Total Crime (logged, lagged)</td>
<td>1.106 (.026) **</td>
<td>1.125 (.036) **</td>
<td>1.091 (.027) **</td>
</tr>
<tr>
<td>Disadvantage Index</td>
<td>0.894 (.157)</td>
<td>0.911 (.178)</td>
<td>0.924 (.143)</td>
</tr>
<tr>
<td>Percent Foreign Born</td>
<td>1.016 (.009) +</td>
<td>1.017 (.007) *</td>
<td>1.019 (.009) *</td>
</tr>
<tr>
<td>Patrol Strength</td>
<td>1.006 (.006)</td>
<td>1.002 (.005)</td>
<td>1.006 (.006)</td>
</tr>
<tr>
<td>Moran’s I (lagged)</td>
<td>1.285 (.369)</td>
<td>1.124 (.280)</td>
<td>1.054 (.282)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.063 (.052) **</td>
<td>0.168 (.131) *</td>
<td>0.916 (.035) **</td>
</tr>
</tbody>
</table>

District Fixed Effects? Yes Yes Yes
Year Fixed Effects? Yes Yes Yes
Season Fixed Effects? Yes Yes Yes
Standard Errors Clustered by Tract? Yes Yes Yes

Observations 8,303 8,303 8,303
Groups 173 173 173
Wald Chi-Square 460.36 492.63 582.82
Wald degrees of freedom 25 25 26
Wald Chi-Square p .000 .000 .000

Notes: Estimates reported as Incident Rate Ratios. Robust standard errors were clustered by census tract. Percent White is the reference category for the resident, arrestee, and suspect race dummy variables. The natural log of the total number of residents, total number of arrestees, and total number of suspects for each tract-month were used as exposure offsets in the respective regression models. Significance: * p<=.10, * p<=.05, ** p<=.01.
Table 5. Negative Binomial Regression of the Number of FIO Reports by Individual Suspect Characteristics Controlling for Gang Membership (IRR, SE, p)

<table>
<thead>
<tr>
<th>Characteristic</th>
<th>All FIO Reports</th>
<th>Non-Contact FIO Reports</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Model 1</td>
<td>Model 2</td>
</tr>
<tr>
<td>Black Suspect</td>
<td>1.725 (.026) **</td>
<td>1.088 (.011) **</td>
</tr>
<tr>
<td>Hispanic Suspect</td>
<td>1.136 (.026) **</td>
<td>0.969 (.013) *</td>
</tr>
<tr>
<td>Asian / Other Suspect</td>
<td>0.725 (.024) **</td>
<td>0.791 (.021) **</td>
</tr>
<tr>
<td>Unknown Race</td>
<td>0.501 (.007) **</td>
<td>0.681 (.007) **</td>
</tr>
<tr>
<td>Age</td>
<td>0.990 (.001) **</td>
<td>0.988 (.001) **</td>
</tr>
<tr>
<td>Female Suspect</td>
<td>0.670 (.011) **</td>
<td>0.830 (.009) **</td>
</tr>
<tr>
<td>Gang Member</td>
<td>----</td>
<td>3.339 (.076) **</td>
</tr>
<tr>
<td>Arrest History</td>
<td>----</td>
<td>1.108 (.001) **</td>
</tr>
<tr>
<td>Constant</td>
<td>2.788 (.058) **</td>
<td>2.103 (.029) **</td>
</tr>
</tbody>
</table>

|                      | Yes             | Yes                     | Yes                     |
| District Fixed Effects? | Yes             | Yes                     | Yes                     |
| Year Fixed Effects?   | Yes             | Yes                     | Yes                     |
| Season Fixed Effects? | Yes             | Yes                     | Yes                     |
| SE’s Clustered by Tract? | Yes             | Yes                     | Yes                     |
| Observations         | 72,619          | 72,619                  | 72,619                  |
| Log Pseudo-likelihood| -153,503.52     | -133,092.42             | -117,323.91             |
| Wald Chi-Square       | 9,269.43        | 22,813.61               | 19,112.43               |
| Wald Chi-Square p     | 0.000           | 0.000                   | 0.000                   |

Notes: Models estimated with robust standard errors clustered by tract. Race variables contrasted with White. Significance: + p<=.10, * p<=.05, ** p<=.01
Table 6. Hierarchical Logistic Regression Estimating Impact of Suspect Race on Probability of a Frisk and/or Search

<table>
<thead>
<tr>
<th>Characteristic</th>
<th>OR</th>
<th>SE</th>
<th>p</th>
</tr>
</thead>
<tbody>
<tr>
<td>Age</td>
<td>0.977</td>
<td>-0.001</td>
<td>**</td>
</tr>
<tr>
<td>Female</td>
<td>0.347</td>
<td>-0.007</td>
<td>**</td>
</tr>
<tr>
<td>Suspect Race – Black</td>
<td>1.124</td>
<td>-0.018</td>
<td>**</td>
</tr>
<tr>
<td>Suspect Race – Hispanic</td>
<td>1.045</td>
<td>-0.018</td>
<td>**</td>
</tr>
<tr>
<td>Suspect Race – Asian/Other</td>
<td>0.837</td>
<td>-0.021</td>
<td>**</td>
</tr>
<tr>
<td>Suspect Race – Unknown</td>
<td>0.588</td>
<td>-0.018</td>
<td>**</td>
</tr>
<tr>
<td>Gang Member</td>
<td>1.117</td>
<td>-0.017</td>
<td>**</td>
</tr>
<tr>
<td>Arrest History</td>
<td>1.018</td>
<td>-0.001</td>
<td>**</td>
</tr>
<tr>
<td>Constant</td>
<td>0.459</td>
<td>-0.082</td>
<td>***</td>
</tr>
</tbody>
</table>

Observations: 199,331
Log Likelihood: -121413.72
Wald Chi-square: 2603.82
p(Wald Chi-square): 0.000

Notes: Robust standard errors clustered by tract. Fixed effects for police districts, year and season. Random effects for tract characteristics (not shown) include tract population (logged), total violent crime in tract (logged, lagged), disadvantage index, and Moran’s I. Race variables contrasted with White suspects. Significance: + p<=.10, * p<=.05, ** p<=.01
Table 7. Zero Inflated Negative Binomial Regressions of FIO Counts on Officer Characteristics (IRR, SE, p)

<table>
<thead>
<tr>
<th>Characteristic</th>
<th>OR</th>
<th>SE</th>
<th>p</th>
</tr>
</thead>
<tbody>
<tr>
<td>Years on Job</td>
<td>0.902</td>
<td>-0.007</td>
<td>**</td>
</tr>
<tr>
<td>Female</td>
<td>0.377</td>
<td>-0.069</td>
<td>**</td>
</tr>
<tr>
<td><strong>Officer Race</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Black</td>
<td>0.575</td>
<td>-0.066</td>
<td>**</td>
</tr>
<tr>
<td>Hispanic</td>
<td>0.901</td>
<td>-0.156</td>
<td></td>
</tr>
<tr>
<td>Asian</td>
<td>0.552</td>
<td>-0.121</td>
<td>**</td>
</tr>
<tr>
<td><strong>Officer Rank</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Detective</td>
<td>0.885</td>
<td>-0.187</td>
<td></td>
</tr>
<tr>
<td>Sergeant or Lt.</td>
<td>0.893</td>
<td>-0.151</td>
<td></td>
</tr>
<tr>
<td>Captain or Command</td>
<td>0.778</td>
<td>-0.133</td>
<td></td>
</tr>
<tr>
<td><strong>Officer Unit</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mobile Operations</td>
<td>1.021</td>
<td>-0.583</td>
<td></td>
</tr>
<tr>
<td>Drug Control</td>
<td>1.131</td>
<td>-0.263</td>
<td></td>
</tr>
<tr>
<td>YVSF</td>
<td>11.953</td>
<td>-2.655</td>
<td>**</td>
</tr>
<tr>
<td>Other Patrol</td>
<td>0.358</td>
<td>-0.112</td>
<td>**</td>
</tr>
<tr>
<td>Other Investigation</td>
<td>0.215</td>
<td>-0.069</td>
<td>**</td>
</tr>
<tr>
<td>Constant</td>
<td>206.322</td>
<td>-49.72</td>
<td>**</td>
</tr>
<tr>
<td><strong>Zero Inflation Parameters</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Administrative Assignment</td>
<td>4.946</td>
<td>-0.404</td>
<td>**</td>
</tr>
<tr>
<td>On Leave</td>
<td>4.592</td>
<td>-0.389</td>
<td>**</td>
</tr>
<tr>
<td>Constant</td>
<td>-4.734</td>
<td>-0.301</td>
<td>**</td>
</tr>
</tbody>
</table>

Observations: 2,359
Log Likelihood: -9,833.14
Wald Chi-square: 1059.06
p (Chi-square): 0

Notes: Models estimated with robust standard errors, not clustered due to mobility of officers. Fixed effects for police district, year, season, and police district. Significance: *p<=.10, **p<=.05, ***p<=.01
Table 8. Negative Binomial Regression of the Number of FIO Reports by Officer Race and YVSF Status (IRR, SE, p)

<table>
<thead>
<tr>
<th></th>
<th>Model 1</th>
<th>Model 2</th>
</tr>
</thead>
<tbody>
<tr>
<td>Age</td>
<td>.916 (.006) **</td>
<td>.922 (.006) **</td>
</tr>
<tr>
<td>Female</td>
<td>.307 (.059) **</td>
<td>.383 (.074) **</td>
</tr>
<tr>
<td>White Officer</td>
<td>1.752 (.335) **</td>
<td>----</td>
</tr>
<tr>
<td>Black Officer</td>
<td>1.171 (.243)</td>
<td>----</td>
</tr>
<tr>
<td>Hispanic Officer</td>
<td>1.613 (.338) *</td>
<td>----</td>
</tr>
<tr>
<td>White YVSF</td>
<td>----</td>
<td>9.022 (2.136) **</td>
</tr>
<tr>
<td>White Other</td>
<td>----</td>
<td>1.488 (.287) *</td>
</tr>
<tr>
<td>Black YVSF</td>
<td>----</td>
<td>8.358 (2.081) **</td>
</tr>
<tr>
<td>Black Other</td>
<td>----</td>
<td>.826 (.170)</td>
</tr>
<tr>
<td>Hispanic YVSF</td>
<td>----</td>
<td>10.788 (3.706) **</td>
</tr>
<tr>
<td>Hispanic Other</td>
<td>----</td>
<td>1.112 (.265)</td>
</tr>
<tr>
<td>Constant</td>
<td>191.969 (37.743) **</td>
<td>175.144 (34.663) **</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th></th>
<th>Model 1</th>
<th>Model 2</th>
</tr>
</thead>
<tbody>
<tr>
<td>Observations</td>
<td>1,750</td>
<td>1,750</td>
</tr>
<tr>
<td>Log Pseudo-likelihood</td>
<td>-9,245.30</td>
<td>-9,116.84</td>
</tr>
<tr>
<td>Wald Chi-Square</td>
<td>312.99</td>
<td>652.49</td>
</tr>
<tr>
<td>Wald Chi-Square p</td>
<td>0</td>
<td>0</td>
</tr>
</tbody>
</table>

Notes: Models estimated with robust standard errors, not clustered due to mobility of officers. Officers included in this analysis made at least one FIO report between 2007 and 2010. Asian is the contrast category for the FIO officer race tests.

Significance:  + p<=.10, * p<=.05, ** p<=.01
Table 9. Hierarchical Logistic Regression Estimating Impact of Officer Race on Probability of a Frisk or Search (OR, SE, p)

<table>
<thead>
<tr>
<th>Characteristic</th>
<th>OR</th>
<th>SE</th>
<th>p</th>
</tr>
</thead>
<tbody>
<tr>
<td>Years on Job</td>
<td>0.973</td>
<td>(.007)</td>
<td>**</td>
</tr>
<tr>
<td>Female</td>
<td>0.618</td>
<td>(.069)</td>
<td>**</td>
</tr>
<tr>
<td><strong>Officer Race</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Black</td>
<td>0.850</td>
<td>(.066)</td>
<td>**</td>
</tr>
<tr>
<td>Hispanic</td>
<td>0.956</td>
<td>(.156)</td>
<td></td>
</tr>
<tr>
<td>Asian</td>
<td>0.674</td>
<td>(.121)</td>
<td>**</td>
</tr>
<tr>
<td><strong>Officer Rank</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Detective</td>
<td>1.495</td>
<td>(.187)</td>
<td></td>
</tr>
<tr>
<td>Sergeant or Lt.</td>
<td>0.847</td>
<td>(.151)</td>
<td></td>
</tr>
<tr>
<td>Captain or Command</td>
<td>0.5</td>
<td>(.133)</td>
<td>*</td>
</tr>
<tr>
<td><strong>Officer Unit</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>YVSF</td>
<td>1.243</td>
<td>(2.655)</td>
<td>**</td>
</tr>
<tr>
<td>Constant</td>
<td>315.322</td>
<td>(49.720)</td>
<td>**</td>
</tr>
</tbody>
</table>

Observations: 200,103
Log Likelihood: -123,410.23
Wald Chi-square: 1,618.47
p (Chi-square): 0.000

Notes: Robust standard errors clustered by police district. Random effects (not shown) included census tract population (logged), total crime in tract (logged, lagged), disadvantage index, and Moran’s I. Fixed effects for year, season, and police district. Significance: + p<=.10, * p<=.05, ** p<=.01
Table 10a. Negative Binomial Regression Analyses of the Joint Distribution of Officer Race and Subject Race on FIO Counts (IRR, SE)

<table>
<thead>
<tr>
<th>Subject Race</th>
<th>Officer Race</th>
<th>Black</th>
<th>Hispanic</th>
<th>Asian</th>
<th>White</th>
</tr>
</thead>
<tbody>
<tr>
<td>Black</td>
<td></td>
<td>.645**</td>
<td>.865</td>
<td>.504**</td>
<td>1.548*</td>
</tr>
<tr>
<td>Hispanic</td>
<td></td>
<td>.581**</td>
<td>.128</td>
<td>.664</td>
<td>1.722**</td>
</tr>
<tr>
<td>Asian / Other</td>
<td></td>
<td>.616**</td>
<td>1.219</td>
<td>1.113</td>
<td>1.623**</td>
</tr>
<tr>
<td>White</td>
<td></td>
<td>.426**</td>
<td>.731*</td>
<td>.702*</td>
<td>2.345**</td>
</tr>
</tbody>
</table>

Table 10b. Hierarchical Logistic Regression Analyses of the Joint Distribution of Officer Race and Subject Race on the Likelihood of a Frisk / Search (OR, SE)

<table>
<thead>
<tr>
<th>Subject Race</th>
<th>Officer Race</th>
<th>Black</th>
<th>Hispanic</th>
<th>Asian</th>
<th>White</th>
</tr>
</thead>
<tbody>
<tr>
<td>Black</td>
<td></td>
<td>.813**</td>
<td>.922**</td>
<td>.649**</td>
<td>1.229**</td>
</tr>
<tr>
<td>Hispanic</td>
<td></td>
<td>.991</td>
<td>.968</td>
<td>.605**</td>
<td>1.008</td>
</tr>
<tr>
<td>Asian / Other</td>
<td></td>
<td>.949</td>
<td>1.031</td>
<td>.724*</td>
<td>1.052</td>
</tr>
<tr>
<td>White</td>
<td></td>
<td>.874**</td>
<td>.926*</td>
<td>.811**</td>
<td>1.143**</td>
</tr>
</tbody>
</table>

Note: Models estimated with robust standard errors clustered by police district. Estimates control for suspect and officer age and gender. Fixed effects include year, season, police district, and officer rank and assignment. White is the contrast category for officer race variables in the regressions in the first three columns of coefficients. Black is the contrast category for the White officer race dummy variable in the regressions in the fourth column. Significance: + p<=.10, * p<=.05, ** p<=.01
Figure 1. Google N-Gram of “Racial Profiling”
Figure 2. Crime Rate and Population Demography, 2010
Figure 3. Predicted Counts of FIO’s per Month by Percent Black and Hispanic Residents in Tract, 2007-10, Controlling for Crime and Social Conditions