Chapter 13

Street Stops and Broken Windows Revisited

The Demography and Logic of Proactive Policing in a Safe and Changing City

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I. Introduction

The role of policing in New York City's crime decline has been the subject of contentious debate for well over a decade. Violent crime reached its modern peak in New York City in 1991, followed by a 10 percent decline in 1992–93 (Fagan, Zimring, and Kim, 1998). This initial crime decline was spurred by the hiring and quick deployment in 1991 of five thousand additional officers under the Safe Streets Program (McCall, 1997; Greene, 1999; Waldeck, 2000; Karmen, 2000). During this initial decline, police tactics remained largely unchanged from the preceding years. Following the mayoral election in 1993, newly appointed police commissioner William Bratton implemented a regime of "order-maintenance policing" (OMP), which—together with other management reforms and innovations—dramatically and suddenly changed both the strategy and tactics of policing across the City. The new strategy was grounded in *Broken Windows* theory (Wilson and Kelling, 1982; Kelling and Cole, 1996) and focused on the connection between physical and social disorder and violence (Greene, 1999; Livingston, 1997; Spitzer, 1999; Sampson and Raudenbush, 1999; Duneier, 1999; Waldeck, 2000; Fagan and Davies, 2000; Taylor, 2001; Harcourt, 2001).

In the new policing model, police tactics, resources, and attention were redirected toward removal of visible signs of social disorder—"broken windows"—by using police resources both for vigorous enforcement of laws on minor "quality of life" offenses, while aggressively interdicting citizens in an intensive and widespread search for weapons (Kelling and Cole, 1996; Bratton and Knobler, 1998; Silverman, 1999). Tactically, policing in this era had several faces, from frequent arrests for low-level crimes such as public drinking, graffiti, and marijuana possession (Golub, Johnson, and Dunlap, 2007; Harcourt and Ludwig, 2007; Levine and Small, 2008), to aggressive street-level interdictions and searches of citizens whose behaviors signaled their potential for any of several types of crime, but most notably carrying weapons (Harcourt, 1998; Fagan and Davies, 2000; Gelman, Fagan, and Kiss, 2007). Using aggressive "stop and frisk" tactics, this brand of OMP was designed to reduce violence and weapons possession (Spitzer, 1999; Waldeck, 2000; Fagan and Davies, 2000; Harcourt, 2001).

The origins of the tactical shift are revealed in strategy documents issued by the New York City Police Department (NYPD) in 1994. First, Police Strategy No. 5, *Reclaiming the Public Spaces of New York*, articulated a reconstructed version of *Broken Windows* theory (Wilson and Kelling, 1982) as the driving force in the development of policing policy. It stated that the NYPD would apply its enforcement efforts to "reclaim the streets" by systematically and aggressively enforcing laws against low-level *social* disorder: graffiti, aggressive panhandling, fare beating, public drunkenness, unlicensed vending, public drinking, public urination, and other misdemeanor offenses. Second, Police Strategy No. 1, *Getting Guns Off the Streets of New York*, formalized the strategic focus on the eradication of gun violence through the tactical measure of intensifying efforts to seize illegal firearms. Homicide trends in New York City since 1985 provided strong empirical support for emphasizing gun violence in enforcement policy (Davis and Matea-Gelabert, 1999). Nearly all the increases in homicides, robberies, and assaults from 1985 to 1991 were attributable to gun violence (Fagan et al., 1998). The homicide crisis was a critical theme in the mayoral election campaign of 1993, and focused the attention of the incoming Giuliani administration's crime-control policy on gun violence (Silverman, 1999).

By the end of the decade, stops and frisks of persons suspected of crimes had become a flashpoint for grievances by the City's minority communities, who came under the closest surveillance of the police and were most often stopped and frisked (Spitzer, 1999; Kocieniewski, 1999; Roane, 1999; Jackson, 2000). In a fifteen-month period from January 1998 through March 1999, non-Hispanic Black, Hispanic Black, and Hispanic White New Yorkers were three times more likely than their White counterparts to be stopped and frisked on suspicion of weapons or violent crimes relative to each group's participation in each of those two types of crimes (Gelman et al., 2007). These excess stops—stops beyond the rate that one would predict from the race-specific crime rates—could be explained neither by the crime rates in those areas in the City's poorest areas, nor by signs and manifestations of social disorder, nor by the presence of physical disorder in the form of actual "broken windows" or building or neighborhood decay. Instead, Fagan and Davies (2000) reported that policing was disproportionately concentrated in the City's poorest neighborhoods with the highest concentrations of minority citizens, even after controlling for rates of crime and physical disorder in those places (see also Gelman et al., 2007).

Despite its racial disproportionality, the harsh spotlight of a federal court order enjoining the NYPD from racially selective enforcement (*Daniels et al. v. City of New York*, 2003), and arrest rates of less than 15 percent resulting from stops (Spitzer, 1999; Gelman et al., 2007), the OMP policy continued far into the next decade (Baker, 2009). Yet New York City had changed dramatically during this period, even after rates of crime and disorder had fallen. Housing prices had soared for more than a decade in all neighborhoods, including those that had the highest violence rates in the preceding decade (Fagan and Davies, 2007), and new housing replaced abandoned lots and decaying buildings across the City (Schwartz, 1999). Welfare rolls thinned, the number of immigrants landing in the City's poorest neighborhoods rose sharply, and populations of African Americans declined by more than 10 percent (U.S. Census Bureau, 2006). With minor and random ticks up and down, crime remained nearly flat and low since 2000 (Levine and Small, 2008).

Yet, in a safe and thriving city, the number of citizen stops grew by 500 percent between 2003 and 2008 (Baker, 2008, 2009; Ridgeway, 2007), long after crime had precipitously declined to and remained at historic lows. The efficiency of these stops —that is, the rate at which crime was detected leading to an arrest—declined from about 15 percent in 1998–99 (Gelman et al., 2007) to 7.8 percent in 2003 to less than 4.1 percent in 2006 (table 13.1 infra; Ridgeway, 2007).

As we show in this chapter, street stops continue to be disproportionately concentrated in the City's poorest areas, not unlike a decade earlier. The logic of a sharp rise in street stops and a corresponding sharp decline in their efficiency, in an era of flat crime rates, demands analysis and explanation. In this chapter, we examine the exponential rise in street stops in an era of stable crime rates and look to the community contexts of these stops to identify the predictors of stops and their outcomes.

The everyday routines of New Yorkers of different ethnic and racial groups take place in vastly different local contexts, and it is in these contexts that the heterogeneity and disparate impact of policing practices are most observable. Accordingly, we identify local area characteristics of crime, disorder, and social structure that predict race-specific police stop activity. We extend the work of Fagan and Davies (2000) from 1999 to two time periods in the current decade, across an extended era of declining and then stably low crime rates. We find that the dramatic increase in stop activity in recent years is concentrated predominantly in minority neighborhoods, and that minority residents are likely to be disproportionately subjected to law enforcement contact based on the neighborhoods in which they live rather than the crime problems in those areas. Moreover, this disproportionate contact is based on more than the level of neighborhood crime and disorder; demographic makeup predicts stop activity above and beyond what local crime conditions suggest is necessary and justifiable.

We also test the efficiency of street stops to detect wrongdoing and sanction offenders, and find it to be low and declining over time: as stops have become more prevalent in recent years, they are substantially less likely to lead to arrests. These limitations are particularly pronounced in neighborhoods with high Black populations, suggesting that Black citizens are not only at an elevated risk of police contact compared to non-Hispanic Whites and Hispanics, but that the standards used to justify stops in their neighborhoods may be lower than those in neighborhoods with higher White populations. Finally, we examine and compare specific age-race-cohort impacts of policing to illustrate the extraordinary concentration of policing along racial and ethnic lines.

Our analysis begins with a brief history of the constitutional and theoretical frameworks for New York's OMP strategy, with attention to the racial dimensions of modern policing. We then discuss the data, models, and results, followed by discussion and conclusions.

II. Background

A. Race, Neighborhoods, and Police Stops

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 (see, for example, Musto, 1973; Kennedy, 1997; Cole, 1999; Loury, 2002; Weitzer and Tuch, 2006). In recent decades, the U.S. Supreme Court has sanctioned border interdictions of persons of Mexican or Hispanic ethnicity to halt illegal immigration (*U.S. v. Martinez-Fuerte*, 1976), as well as the racial components of drug courier profiling by airlines (*U.S. v. Harvey*, 1992). In *U.S. v. Whren* (1996), the Supreme Court allowed the use of race as a basis for a police stop as long as there were other factors that motivated the stop, and in *Brown v. Oneonta* (2000), 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 cases from *Adams v. Williams* (1972) to *Illinois v. Wardlow* (2000). 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) (Ferguson and Bernache, 2008). For example, in *Wardlow*, the Supreme 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. Since "high-crime areas" and social disadvantage often are conflated both perceptually and statistically with concentrations of minority citizens (Massey and Denton, 1993; Sampson and Lauritsen, 1994; Loury, 2002; Fagan, 2008; Sampson and Raudenbush, 1999, 2004; Alpert et al., 2005; Ferguson and Bernache, 2008; Massey, 2007), this logic places minority neighborhoods at risk for elevating the suspiciousness of their residents in the eyes of the police.

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 (Reiss, 1971, Bittner, 1970). Particularly in urban areas, suspect race interacts with neighborhood characteristics to animate the formation of suspicion among police officers (Smith, 1986; Thompson, 1999; Smith et al., 2006). For example, Alpert and colleagues (2005) showed that police are more likely to view a minority citizen as suspicious—leading to a police stop—based on nonbehavioral 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. Quillan and Pager (2001) find that 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, Smith (1986) showed that suspects in poor neighborhoods were more likely to be arrested, after controlling for suspect behavior and the type of crime. Suspects' race and the 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 (Sampson and Raudenbush, 2004) through several forms of discrimination, including policing intensity and tactics (Fagan and Davies, 2000). Recall that 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 Wilson and Kelling (1982). 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 (Kelling and Cole, 1996). 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 (compare Harcourt, 1998, 2001; Sampson and Raudenbush, 1999, 2004; and Taylor, 2001; with Skogan, 1990; Corman and Mocan, 2000; Rosenfeld, Fornango, and Rengifo, 2007). Moreover, these studies suggest that a focus on disorder might have a disparate impact on citizens of different races. A study of Chicago neighborhoods finds that city residents' perceptions of disorder conflate systematically observable conditions with their neighborhoods' racial and socioeconomic makeup (Sampson and Raudenbush, 2004). The association between race, poverty, and perceived disorder is significant in residents of all racial and ethnic backgrounds; race and concentrated poverty predict both 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.

So the concentration of "order maintenance" policing in poor places with high concentrations of poor residents should come as no surprise: order-maintenance policing strategies ostensibly targeted at "disorderly" neighborhoods were in fact focused on minority neighborhoods, characterized by social and economic disadvantage (Fagan and Davies, 2000). This racial bait and switch with disorder is fundamental to understanding the broad spatial and social patterns of policing in New York in the past decade. Most interesting and important is the persistence of these policies even as the objective indicia of poverty and disorder fade in what we show below is a steadily improving and safe City.

B. Approaches to Studying Police Stops

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 (Cole, 1999; Veneiro and Zoubeck, 1999; Harris, 1999; Zingraff et al., 2000; Gross and Barnes, 2002; Weitzer and Tuch, 2006; Ayres, 2008). For example, two surveys with nationwide probability samples, completed in 1999 and in 2002, showed that African Americans were far more likely than other Americans to report being stopped on the highways by police (Langan et al., 2001; Durose et al., 2005). Both surveys showed that minority drivers also were more likely to report being ticketed, 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, according to Loury (2002), animate culturally meaningful forms of stigma that reinforce racial inequalities, especially in the practice of law enforcement. These stigma translate into withdrawal of minority populations from cooperation with the police and other legal authorities in the coproduction of security (Tyler and Huo, 2002; Tyler and Fagan, 2008).

Traffic violations often serve as the rationale or pretext for stops of motorists (Walker, 2001; Harris, 2002), just as "suspicious behavior" is the spark for both pedestrian and traffic stops (Alpert et al., 2005; Ayres, 2008). As with traffic violations, the range of suspicious behaviors is broad enough to challenge efforts to identify an appropriate baseline against which to compare race-specific stop rates (see Miller, 2000; Smith and Alpert, 2002; Gould and Mastrofski, 2004). 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. For the NYPD, a "stop" provides an occasion for the police to have contact with persons presumably involved in low-level criminality without having to effect a formal arrest, and under the lower constitutional standard of "reasonable suspicion" (Spitzer, 1999). Indeed, because low-level "quality of life" and misdemeanor offenses were more likely to be committed in the open, the "reasonable suspicion" standard is more easily satisfied in these sorts of crimes (Rudovsky, 2001, 2007).

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) data sets 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.[1] Were McFadden's notions of "suspicious" behavior skewed by his longtime work in poor and minority neighborhoods? Was the timing of the event (shortly after the closing of the store) or the location (a deserted part of the downtown area) influential? What role did Terry's and McFadden's race play? Would Terry's actions have been interpreted differently if he were White? If McFadden were Black? If the store was in a residential neighborhood instead of downtown? In a minority neighborhood or a predominantly White one? The multiplicity of interacting factors complicated the identification of the role of race in the decision to detain Terry (Kennedy, 1997), but several analyses of the facts and jurisprudence of *Terry* suggest that the Supreme Court opinion discounted the influence of race in the opinion (Thompson, 1999; Carbado, 2002; Carbado and Gulati, 2000; Roberts, 1999; Rudovsky, 2007).

In *Terry*, it would be 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 could only be attributed to race or ethnicity. Such experiments are routinely used in tests of discrimination in housing and employment (see, for example, Pager, 2003, 2007; Thacher, 2008). 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 behavior (e.g., Gould and Mastrofski, 2004; Alpert et al., 2005) 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 the 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, including Knowles, Persico, and Todd (2001); Ayres (2002a,b); Gross and Barnes (2002); and many other studies of police behaviors on highways (see, e.g., Durlauf, 2006b). 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 (Ayres, 2002b; Dominitz and Knowles, 2006). 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. 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 (Ayres, 2002a). If searches of minorities are less productive than searches of Whites, this could be evidence that police 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 (2001) 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. 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 (Lamberth, 1997; Ayres, 2002a; Gross and Barnes, 2002; but see Sanga, 2009, for different conclusions).

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 (2007) 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. 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 (2007) 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. They use both an outcomes-based nonparametric (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 (see, for example, Alpert et al., 2005; Smith et al., 2006), or differences in police behavior in neighborhoods or other social contexts with different racial makeup (Smith, 1986; Fagan and Davies, 2000; Alpert et al., 2005). For example, Ridgeway (2007) estimated the racial proportionality of police stops of citizens based on victim reports of suspect race. This is a sound strategy, but only for the approximately 20 percent of stops based on a rationale of "fits suspect description" (see, for example, Spitzer, 1999), and only if we are confident in the accuracy of victim identification of the suspect(s) and the accompanying classification of race.[2]

The omission of neighborhood context also biases estimates of the proportionality of police stops of citizens. The randomizing equilibrium assumptions in the Persico and colleagues 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 (Dharmapala and Ross, 2004; Durlauf, 2006a, 2006b), and discount the effects of race-specific sensitivities toward crime decisions under varying conditions of detection risk via police stop (Alpert et al., 2005; Dominitz and Knowles, 2006). When these two concerns are addressed, Dharmapala and Ross (2004) identify different types of equilibria that lead to different conclusions about racial prejudice in police stops and searches.

Accordingly, the nature and extent of racial bias in the policing of motorists and pedestrians remains unsettled empirically (Persico and Todd, 2005; Antonovics and Knight, 2004; Bjerk, 2007; Donohue and Levitt, 2001; Close and Mason, 2007). 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 perceptually skewing the population of stopped drivers according to the *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 behaviors when officers of one race face choices of whether to stop or search a driver of the same or a different racial or ethnic group (Close and Mason, 2007).

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 and colleagues (2007) and Alpert and colleagues (2005) show how neighborhood context influences both the attribution of suspicion that animates an encounter and the outcomes of police-citizen encounters. The institutional context of policing also may influence individual officers' decisions by 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' preferences about crime and thresholds of suspicion.

These issues inform several features of the analyses reported in this chapter. First, to explain the distribution and predictors of street stops and then of arrests ("hit rates"), we focus on neighborhoods, not individual officers. Neighborhoods are the focal point of the underlying theories of order-maintenance policing. Place also is the unit of analysis for the allocation and deployment of police resources, and neighborhood crime rates are the metrics by which the resources of the police are managed and evaluated. Place also imparts meaning to the interpretation of routine actions and movements of citizens, whether local residents or outsiders whose appearance may evoke special attention. And the benchmark of the social composition of place, in conjunction with actual crime, is sensitive to the actual allocation of police resources as well as tactical decisions by the NYPD, and is widely used in research on selective enforcement in policing (Alpert et al., 2005; Fagan, 2002; Fridell, 2004; Skogan and Frydl, 2004).

Next we address supply-side and omitted-variable problems by controlling for the prevalence of the targeted behaviors in patrolled areas, assessing whether stop-and-search rates exceed what we would predict from knowledge of local criminal activity. This responds to the benchmark problem in research on selective enforcement. This approach requires estimates of the supply of individuals engaged in the targeted behaviors, and the extent of racial disproportionality is likely to depend on the benchmark used to measure criminal behavior (see Miller, 2000; Fagan and Davies, 2000; Walker, 2001; Smith and Alpert, 2002; Ayres, 2008; Durlauf, 2006a, 2006b; Ridgeway and MacDonald, this volume). Ideally, we would know race-specific crime rates in each social area to disaggregate benchmarks by race and ethnicity. But we observed practical problems in this approach. For example, clearance rates vary by crime type, and so the race of suspects is often unknown. Fewer than one in four stops in 2007 were based on a match between the person detained and a suspect description known to the police (Ridgeway, 2007). And suspected crimes that animate a large share of stops, such as weapons 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.

Accordingly, we use homicide arrests as a measure of reported crime. Homicide victimization and arrests are stably measured over time, limiting measurement error. In New York, its racial distribution—both offending and victimization—is highly correlated with the demography of the neighborhood where the crime takes place (Fagan and Davies, 2004; Fagan et al., 2007). In New York City, the site of this research, homicide records are both a strong lag and lead indicator of crime, correlated at .75 or more with reported crimes for other Part I felonies for the seventeen years from 1984 to 2000. Homicides also are the most stably and reliably measured indicator of crime over time and through police administrations, whereas other violent crimes (e.g., aggravated assault) are subject to classifications biases that vary over time and place (Zimring and Hawkins, 1997).

Following Gelman and colleagues (2007), we estimate whether the stop rate and "hit rate" within neighborhoods is predicted by local crime conditions, the physical and social composition of the neighborhood, or its racial composition. Since race is correlated with neighborhood composition and crime, we expect that race will not be a significant predictor either of stop patterns or of efficiency (the rate at which stops produce arrests), once we account for crime and other neighborhood conditions. But as we show below, race does predict stop rates and hit rates, after controlling for crime and local conditions. Is this evidence of racial animus, targeted collectively by officers in a neighborhood or through institutional and administrative levers that mark neighborhoods characterized by their racial or ethnic composition as worthy of heightened suspicion? The fact that police are stopping minorities, and others in minority neighborhoods, at a higher rate than is justified by local crime conditions does not require that we infer that police engaged in disparate treatment—but, at a minimum, it is evidence that whatever criteria the police employed produced an unjustified racially disparate impact.

III. Data and Methods

A. Data

We examine changes in OMP enforcement patterns beginning with the period examined by Spitzer (1999), Fagan and Davies (2000), and Gelman and colleagues (2007). Including that period (1998–99), we examine three distinct periods, termed the "early" (1998–1999), "middle" (2002–2004), and "recent" (2005–2006) periods. In each period, data on stop activity are based on records from the New York Police Department. The department has a policy of keeping records on stops (on "UF-250 forms") (see Spitzer, 1999; *Daniels et al. v. City of New York*, 2003); this information was collated for all stops from January 1998 through March 1999, and the 2003 and 2006 calendar years. Stops are recorded and aggregated for each precinct. Appendix A discusses the legal requirements for a stop, frisk, and arrest pursuant to a stop. Data on stops, frisks, and arrests from 2003 to 2007 were made publicly available by the NYPD following a Freedom of Information Law (FOIL) request and subsequent court order (NYCLU, 2008). Data from the "early" period were published in Spitzer (1999) and Fagan and Davies (2000).

Stop rates are analyzed in the context of citywide crime, demographic, and socioeconomic conditions. We use total stop rates (undifferentiated by suspected crime) and stop rates disaggregated by the race of person stopped. We use two measures of crime in the preceding year. First, in the figures, we use reported homicides in the police precinct in the preceding year as the measure of crime. This lagged function allows us to avoid simultaneity concerns from using contemporaneous measures of crime and police actions. Second, in the multivariate models, we use homicide arrests as the marker of crime.

We measure homicides for the "early" period using the NYPD's arrest-and-complaint file, and the city's COMPSTAT records for the "middle" and "recent" periods. In the multivariate estimates in tables 13.2 and 13.3, we use lagged homicide arrests in each neighborhood as the benchmark for estimating the proportionality of police stops and frisks. There are obvious strengths and weaknesses in this measure. Arrests are subject to police preferences for resource allocation, and also to police skills in identifying and capturing offenders. Homicide arrests also may vary by neighborhood based on externalities such as the extent of citizen cooperation with police investigations. Arrests also are vulnerable to measurement error: they often are reduced to other charges when evidence is too inconclusive to sustain a greater charge. But arrests also have strengths as a measure of crime. Reported homicides and homicide arrests are highly correlated over time across police precincts in New York: the partial correlation by month and precinct from 1989 through 2001 was .952.[3] This endogeneity of crime and policing within neighborhoods captures the preferences of police to allocate resources to particular areas in the search for offenders. Also, homicide arrests are a strong indicator of both arrests and complaints for other serious crimes.[4] To the extent that crime in the prior year is influenced *both* by crime and the policing that it attracts, the use of arrests as a measure of both the presence of police and of local crime conditions avoids omitted-variable problems when using only measures of reported crimes. Finally, arrest trends in preceding periods incorporate the priors of both individual officers and their supervisors as well as neighborhood characteristics, and in fact may capture officers' propensities to stop citizens based on the joint influence of individual and neighborhood racial markers.

We also incorporate demographic and socioeconomic variables in each area that might compete with or moderate crime as influences on stop activity: concentrated neighborhood disadvantage, residential turnover, and ethnic heterogeneity have each been associated with low levels of neighborhood collective efficacy and informal social control. These are both indicia of perceived disorder (Sampson and Raudenbush, 1999) and risk factors for crime (Fagan and Davies, 2004). More important, Fagan and Davies (2000) showed that these were salient predictors of stop activities in the "early" period, and we examine their influences over time as time-varying predictors. Areas in which these phenomena are concentrated might therefore be unable to informally regulate local residents, requiring law enforcement agencies to impose formal social control instead and leading to greater search activity.

Demographic and socioeconomic data for each period is based on the New York City Housing and Vacancy Survey (HVS), a survey completed every three years by the City's Department of Housing Preservation and Development, in cooperation with the U.S. Bureau of the Census (http://www.census.gov/hhes/www/housing/nychvs/nychvs.html). We analyze the 1999, 2002, and 2005 waves of the survey to generate baseline estimates of neighborhood social and economic status. Each wave covers approximately eighteen thousand housing units, classified into fifty-five "subboros," based on the Public Use Microdata Areas (PUMAs) for New York City (Community Studies of New York, 2007). We used shape files provided by the New York City Department of City Planning to reconcile the subboro boundaries with the police precincts (see Fagan and Davies, 2000). In the small number of precincts where there was overlap in the boundaries, precincts were assigned to the subboro that contained the majority of its population.

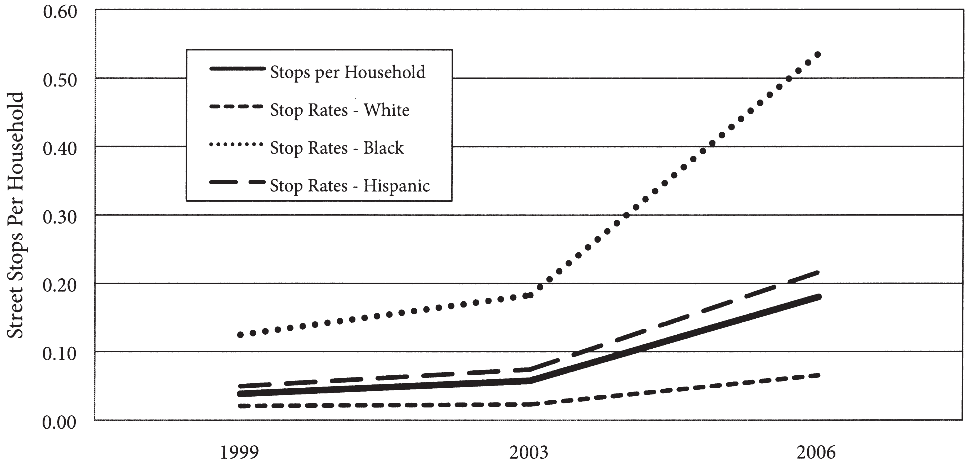
B. Base Rates and Citywide Trends

A quick look at the data on New York City neighborhoods suggests that the social and demographic makeup of the City has changed significantly since 1999. Table 13.1 shows that the city's racial and ethnic makeup has become more diverse. The bulk of the city's population growth has come from racial and ethnic minorities, plus

TABLE 13.1
Stop Activity and Neighborhood Socioeconomic Conditions

	1999 Stops per 1,000 persons		2002–2003 Stops per 1,000 persons		2005–2006 Stops per 1,000 persons		% change (99–05)
Citywide Stop Rates							
Stops per 1,000 Population							
Total Stops	12.5		19.4		60.2		381.6%
Blacks	26.6		37.7		130.8		391.7%
Whites	3.5		6.0		17.9		411.4%
Hispanics	15.1		19.5		63.9		323.2%
	Mean	SD	Mean	SD	Mean	SD	
Neighborhood Stop Activity							
Number of Stops	1813.4	1098.9	2922.5	1670.5	9208.9	6480.4	407.8%
Stops of Blacks	988.1	864.3	1411.9	1368.6	4863.0	5479.1	392.2%
Stops of Whites	187.0	145.3	320.1	273.8	972.7	860.8	420.2%
Stops of Hispanics	583.9	559.9	810.2	599.5	2688.4	2173.9	360.5%
Physical Disorder							
Exterior Walls	3.09%	0.03	2.63%	0.02	2.83%	0.02	−8.5%
Exterior Windows	3.36%	0.03	3.45%	0.03	2.36%	0.02	−29.8%
Stairways	5.25%	0.04	5.29%	0.04	4.24%	0.03	−19.3%
Floors	5.08%	0.04	4.75%	0.04	4.06%	0.03	−20.1%
Structural Characteristics							
Public Assistance	18.24%	0.13	15.17%	0.10	16.41%	0.11	−10.0%
Foreign-Born	46.19%	0.16	43.56%	0.14	49.61%	0.16	7.4%
Immigrant (different in HVS)	36.34%	0.16	43.56%	0.14	41.18%	0.16	13.3%
Entropy	89.02%	0.24	93.64%	0.25	95.48%	0.22	7.3%
Mobility (% Living < 5 years)	40.26%	0.05	35.88%	0.05	36.08%	0.05	−10.4%
Vacancy Rate	5.62%	0.03	6.87%	0.04	6.68%	0.03	18.8%
Households							
Total	52153	19305	54642	16552	55236	16803	5.9%
Black	12150	11930	13115	13382	12570	12603	3.5%
White	24112	23404	24359	22015	24191	21426	0.3%
Hispanic	11682	9155	12200	9063	12881	9206	10.3%

Sources: Socioeconomic and Household Data from New York City Housing and Vacancy Surveys, 1999, 2002, 2005, Stop data from NYPD, Population data from U.S. Census Bureau.



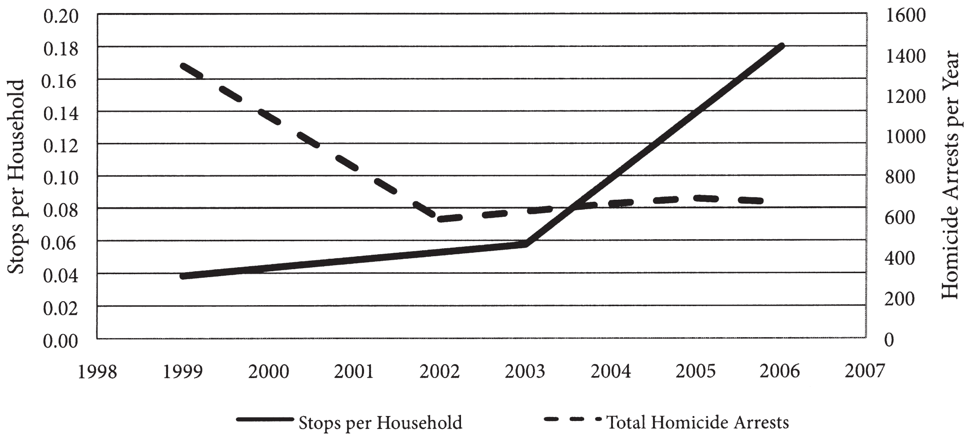


Figure 13.1 (*top*). Stops per household, New York City, 1999–2006. Sources: (Stops) NYS, Office of the Attorney General, 1999; NYC Police Department, Stop Frisks and Search Data, 2003–2007, (Households) NYC Housing and Vacancy Survey.

Figure 13.2 (*bottom*). Stops per household and total homicide arrests, New York City, 1999–2006. Sources: (Stops) NYS, Office of the Attorney General, 1999; NYC Police Department, Stop Frisks and Search Data, 2003–2007, (Households) NYC Housing and Vacancy Survey, (Arrests) NYS Division of Criminal Justice Services.

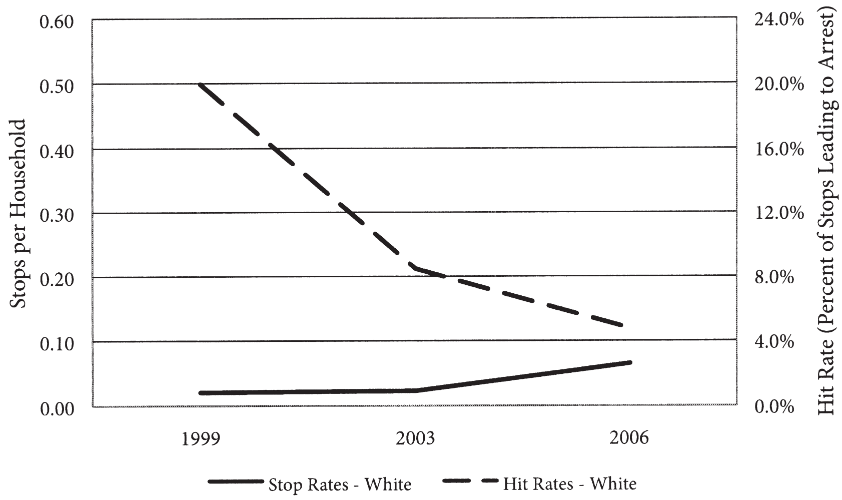
a notable increase among immigrants. Individual neighborhoods have also become more integrated, as shown by the increase in neighborhood entropy. At the same time, socioeconomic conditions have improved, with a decline in both public assistance receipt and neighborhood levels of physical disorder.

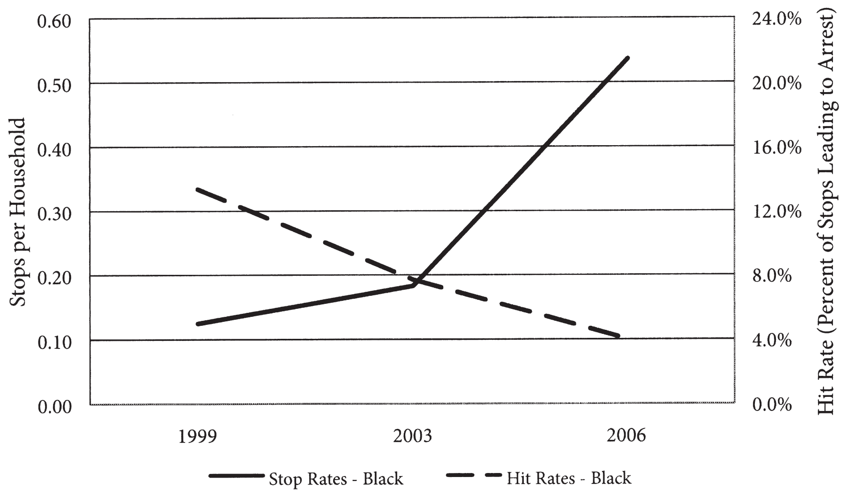
Even as the city has changed demographically and improved socioeconomically, stops and searches have become far more prevalent. Figure 13.1 shows the average neighborhood—subboro—stop rate, computed as stops per household. We use household because this is the population parameter in the HVS in each analysis period. While city residents of all races have become increasingly likely to be stopped by the police, stop rates vary dramatically by race; by 2006, Blacks were more than twice as likely to be stopped as either Whites or Hispanics. The increase in stop activity is particularly striking when considering that New York City crime rates fell dramatically between 1999 and 2006. As shown in figure 13.2, homicide arrests in the City fell by more than 50 percent between 1999 and 2002, and, albeit with a slight increase, remained low through 2006.

Following the examples of Knowles and colleagues (2001), Ayres (2002a,b), Gross and Barnes (2002), Gelman and colleagues (2007), and Ridgeway (2007), we measure the effectiveness of street stops by their "hit rates," the rate at which stops result in arrests. Figures 13.3a–c, like figure 13.1, present average neighborhood stop rates per household in each of the three time periods of interest, disaggregated by race, with average hit rates overlaid onto the graph. And since crime rates remained relatively stable across the period, there is no evidence that the increase in stops contributes to crime minimization. While not as pronounced as the differences in stop rates, hit rates also suggest substantial racial disparities. Figure 13.3b shows that even as stop rates have increased dramatically for Blacks from 2003 to 2006, hit rates have fallen steadily, suggesting that the increase in stop activity has added little value in maximizing efficiency via generating arrests. Stops of Whites appear more likely than stops of Blacks to lead to arrest, suggesting that Blacks are disproportionately subjected to stops, with little public safety payoff.

C. Stop Activity by Neighborhood

Stop rates have not only increased dramatically, but between-neighborhood differences in stop rates have become far more pronounced. Figure 13.4 displays one data point for each of the fifty-five HVS subboros in each period, each representing the average neighborhood stop rate per household in each year. We also show the count of homicides citywide over the same period. While earlier studies have identified neighborhoods that have the greatest racial disparity in stop-and-frisk practices, figure 13.4 shows that the dramatic growth in average stop rates from 2003 to 2006 is explained by extreme increases in a subset of neighborhoods with high rates of African American and Latino residents: Brownsville, East New York, Central Harlem, East Harlem, Bedford-Stuyvesant, and Mott Haven. Although some of this increase may be due to improved reporting, it is curious that all the improved reporting has been in neighborhoods with the highest non-White populations in the City. These neighborhoods are predominantly African American, according to the Department of City Planning.[5]


0.60
0.50
0.40
0.30
0.20
0.10
0.00
Stops per Household
24.0%
20.0%
16.0%
12.0%
8.0%
4.0%
0.0%
Hit Rate (Percent of Stops Leading to Arrest)
1999
2003
2006
Stop Rates - White
Hit Rates - White


0.60
0.50
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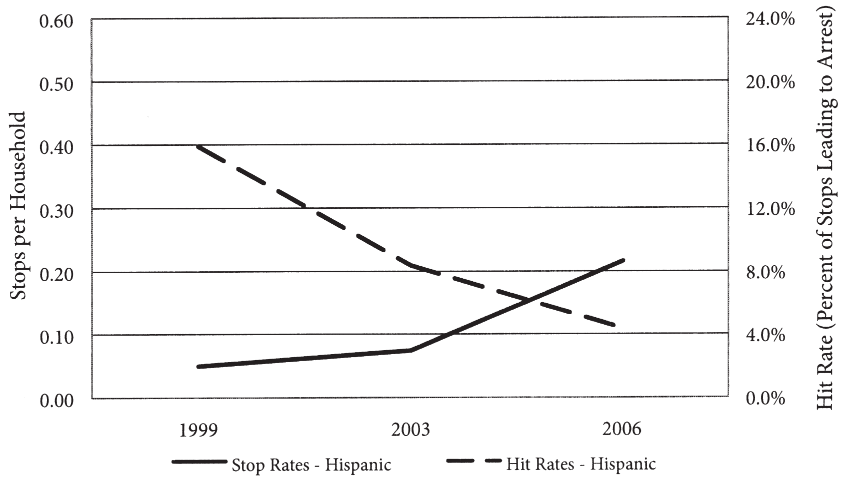
0.20
0.10
0.00
Stops per Household
24.0%
20.0%
16.0%
12.0%
8.0%
4.0%
0.0%
Hit Rate (Percent of Stops Leading to Arrest)
1999
2003
2006
Stop Rates - Black
Hit Rates - Black


0.60
0.50
0.40
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0.20
0.10
0.00
Stops per Household
24.0%
20.0%
16.0%
12.0%
8.0%
4.0%
0.0%
Hit Rate (Percent of Stops Leading to Arrest)
1999
2003
2006
Stop Rates - Hispanic
Hit Rates - Hispanic

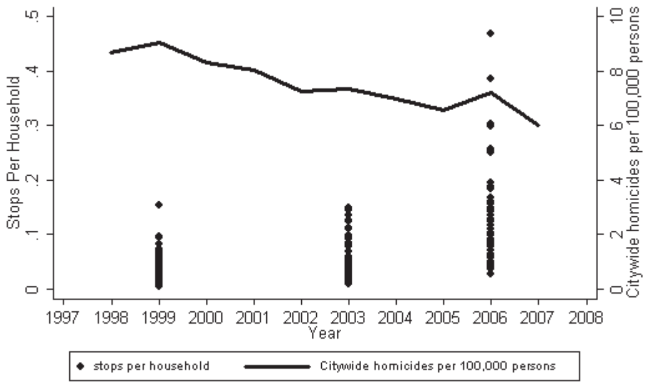


Figure 13.4. Street stops per neighborhood, selected years, 1999–2006.

Given the degree of racial segregation across New York City neighborhoods, we address this disparity below by examining neighborhood-level drivers of stop activity.

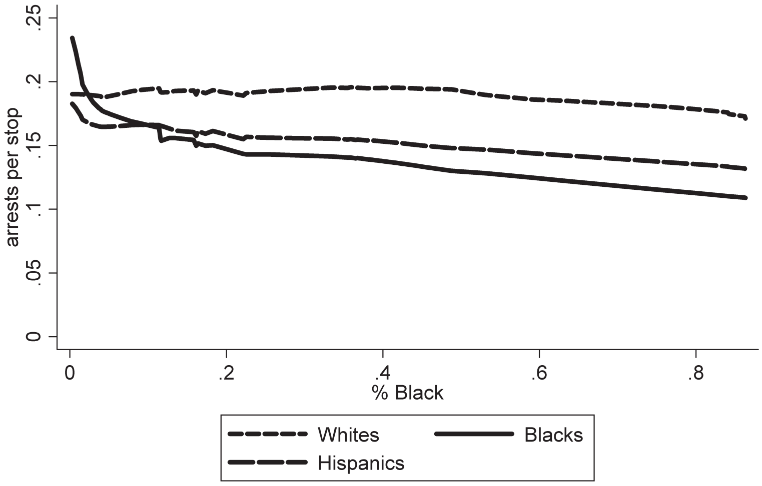
Figures 13.5a–c suggest that neighborhood racial composition explains not only stop activity but also hit rates and stop efficacy. Each figure shows, for 1999, 2003, and 2006, respectively, a LOWESS-smoothed estimate of the relationship between hit rates and the percentage of Blacks in each of the fifty-five neighborhoods for each period of time. As in figure 13.3 (a,b,c), these graphs suggest that hit rates are falling over time in stops of all racial groups. Particularly in 2006, however, the year when between-neighborhood differences are most pronounced (see figure 13.4), there is a visible difference in neighborhoods with the highest concentrations of Black households. In neighborhoods where 60 percent of households (or more) are Black, stops are not only less effective than in more mixed or White neighborhoods, but hit rates are particularly low in stops of Black and Hispanic individuals.

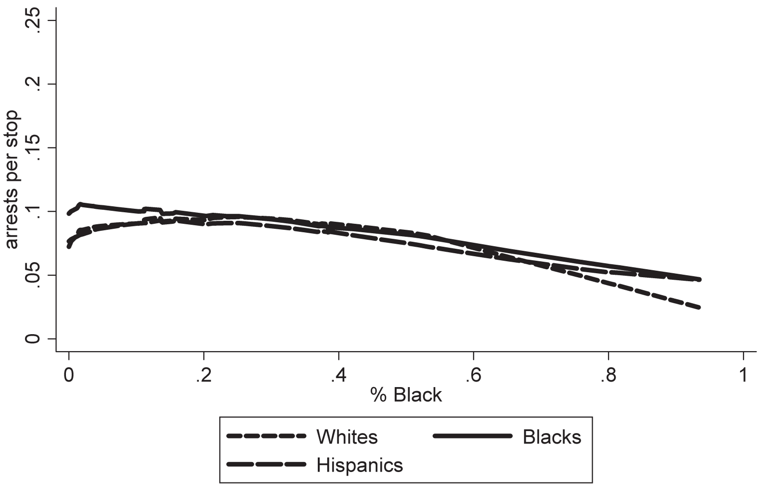
Opposite page:

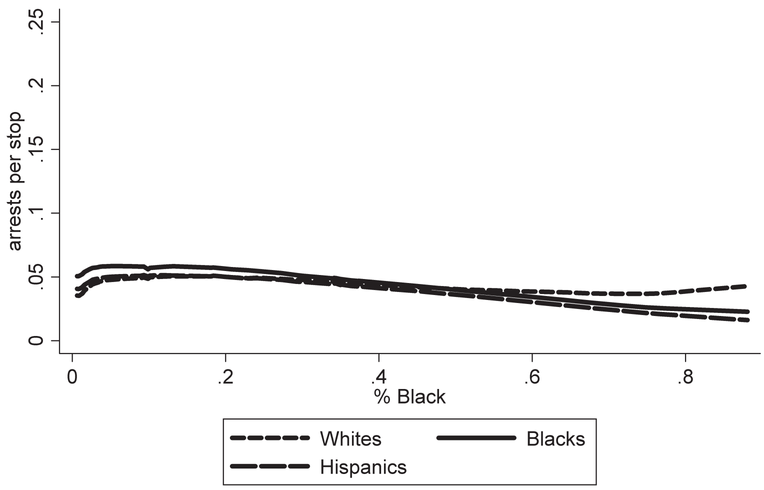
Figure 13.3a (*top*). Stops per household and arrests per stop, White suspects, New York City, 1999–2006. Source: (Stops and Arrests) New York State, Office of the Attorney General, 1999; New York City Police Department, Stop Frisks and Search Data, 2003–2007, (Households) NYC Housing and Vacancy Survey.

Figure 13.3b (*middle*). Stops per household and arrests per stop, Black suspects, New York City, 1999–2006. Source: (Stops and Arrests) New York State, Office of the Attorney General, 1999; New York City Police Department, Stop Frisks and Search Data, 2003–2007, (Households) NYC Housing and Vacancy Survey.

Figure 13.3c (*bottom*). Stops per household and arrests per stop, Hispanic suspects, New York City, 1999–2006. Source: (Stops and Arrests) New York State, Office of the Attorney General, 1999; New York City Police Department, Stop Frisks and Search Data, 2003–2007, (Households) NYC Housing and Vacancy Survey.


arrests per stop
0
.05
.1
.15
.2
.25
0
.2
.4
.6
.8
% Black
Whites
Blacks
Hispanics


arrests per stop
0
.05
.1
.15
.2
.25
0
.2
.4
.6
.8
1
% Black
Whites
Blacks
Hispanics


arrests per stop
0
.05
.1
.15
.2
.25
0
.2
.4
.6
.8
% Black
Whites
Blacks
Hispanics

D. Modeling Strategy

1. Predicting Stop Activity

Given the between-neighborhood disparities shown in figure 13.4, we examine stop activity at the neighborhood level to identify factors that explain between-neighborhood differences both within periods and over time. Following Gelman and colleagues (2007), we estimate a series of Poisson regressions to predict the number of stops conducted in each neighborhood in each time period. The racial disparities shown in figures 13.1 and 13.3 may be driven not by race, but rather by differences in neighborhood social conditions where Blacks, Whites, and Hispanics are concentrated, or by differences in their *ex ante* crime conditions. If, for example, the police make more stops in high-crime areas, but treat individuals similarly within similarly situated localities, racial disparities in stop rates could be explained entirely by neighborhood crime conditions. Or the NYPD's focus on "broken windows" and order-maintenance policing might lead stop activity to be most prevalent in neighborhoods with disorderly conditions (Wilson and Kelling, 1982; Kelling and Cole, 1996). We therefore estimate a model where the stop count y_i in neighborhood i is distributed based on predictors X, with an expected value of:

$$E[y_i|X_i] = e^{X\beta}$$

The vector X includes a measure of neighborhood crime (homicide arrests, lagged), and several socioeconomic characteristics we expect to be correlated with crime rates and policing practices. First, we explicitly control for crime conditions in the previous year, using the number of homicide arrests in each neighborhood. To reflect the NYPD focus on disorder in the 1990s and early 2000s, we estimate and control for a single principal components factor (computed for each year) that summarizes the physical condition ("broken windows," literally) of local residences (based on the percentage of buildings whose windows, walls, floors, and stairways have problems visible to outside observers). The disorder theories animating OMP strategies considered both physical and social disorder as cues of weak informal social control and low guardianship of neighborhoods. We consider only physical disorder since some elements of social disorder—such as fighting, visible drug use—are in fact crimes and would be correlated with stop activity.[6] Also, physical disorder tends to be highly correlated with social disorder, and its component behaviors, including public intoxication, loitering, and fighting (Sampson and Raudenbush, 1999). These are targeted in OMP as a wedge to reduce crime opportunities and to identify persistent criminals. To reflect the likelihood that police activity is higher in more populated areas, we control for the logged number of households in each neighborhood.

Opposite page:

Figure 13.5a (*top*). Lowess-smoothed arrest rates by neighborhood racial composition, 1999.

Figure 13.5b (*middle*). Lowess-smoothed arrest rates by neighborhood racial composition, 2003.

Figure 13.5c (*bottom*). Lowess-smoothed arrest rates by neighborhood racial composition, 2006.

We also control for traditional and temporally stable predictors of neighborhood crime (Shaw and McKay, 1942; Sampson and Lauritsen, 1994; Land et al., 1990; Fagan and Davies, 2004, Fagan, 2008; Kirk and Laub, in press): concentrated disadvantage (measured by the percentage of households receiving public assistance), residential instability (measured by the percentage of families who have moved to the their current residence within five years, and by the residential vacancy rates), ethnic diversity (measured by the percent of residents who are Black or Hispanic, the percentage who are foreign-born, and a measure of entropy, which captures the degree of ethnic heterogeneity in the neighborhood). We expect, however, that these factors will be correlated with police activity only to the extent that they predict crime; once crime conditions are controlled for, there should be no marginal relationship between social structure and stop activity. Variables (with the exception of logged population) are standardized to a mean of zero and variance of one, and neighborhood observations are weighted based on the number of households in each.

To assess the extent to which neighborhood conditions, and their influence on policing, change over time, we first estimate three separate cross-sectional models, one for each time period of interest. We then combine the observations into a pooled cross section (model 4), and add controls for year fixed effects in Model 5. Model 6 contains year fixed effects and random intercepts with standard errors clustered by neighborhood to account for neighborhood differences.

Although the City has changed for the better over the period of analysis, and stop activity has increased dramatically over time, the crime, disorder, and socioeconomic predictors vary far more between neighborhoods than they do within neighborhoods over time, and these differences—at least in ordinal position—are stable over time (see Sampson and Morenoff, 2006). Accordingly, we rejected the option to control for neighborhood fixed effects in Model 6, preferring instead to focus on differences between neighborhoods. Controlling for neighborhood fixed effects identifies the relationship between crime and stop activity, and social structure and stop activity, solely from within-neighborhood variation. Because we acknowledge that the allocation of police resources is determined by differences between neighborhoods, model 6 is specified to reflect between-neighborhood differences, with random intercepts and standard errors clustered by neighborhood.

2. PREDICTING STOP EFFECTIVENESS

We next examine the crime and socioeconomic conditions predicting stop effectiveness, the "hit rate" at which stops lead to arrests. We expect that this rate might be tied to the same conditions of crime and disorder that predict stop activity, since "excess stops" above the crime rate are likely to be concentrated in poor neighborhoods with concentrations of minority population. Accordingly, we estimate a series of linear probability models using the predictors detailed above. As we hypothesize with stop activity, however, in the case of race-neutral policing hit rates should not be significantly related to neighborhood social structure. For these analyses, we estimate the effects of neighborhood racial composition on stop rates using both neighborhood fixed effects and, also, as above, using random intercepts.

IV. Results

A. Explaining Neighborhood Differences in Stop Rates

Table 13.2 shows the relationship between neighborhood conditions and the incidence of street stops. Models 1–3 show results for each year. As expected, stops are more frequent in neighborhoods in which crime is more prevalent for all years, but in larger neighborhoods only in 1999. Controlling for homicides, stops are more frequent in neighborhoods with higher Black populations. The effect size is fairly stable across years, even as the overall number of stops rose over time. Model 4 is a pooled cross-sectional model for all years, with no controls for time. Standard errors are clustered by neighborhoods. The effect for Black population remains significant, and population is again significant when the three time periods are pooled.

TABLE 13.2
Poisson Regressions of Stops per Neighborhood, Controlling for Social Structure and Crime, 1998–2006

	Model					
	(1)	(2)	(3)	(4)	(5)	(6)
Sample Year	1999	2003	2006	All Years	All Years	All Years
Homicide Arrests (lagged)	.202**	.163*	.182**	.172**	.183**	.027*
	[.074]	[.069]	[.067]	[.084]	[.055]	[.052]
% Receiving Public Assistance	.106	.056	.169	.257*	.159	.198*
	[.127]	[.089]	[.099]	[.131]	[.086]	[.082]
% Foreign-Born	−.011	.006	−.045	−.056	−.032	−.076
	[.079]	[.062]	[.083]	[.072]	[.060]	[.065]
Racial Entropy	.186*	.007	.091	.090	.082	.085
	[.086]	[.060]	[.064]	[.066]	[.050]	[.059]
% Black	.216*	.198**	.262**	.260**	.237**	.279***
	[.109]	[.072]	[.068]	[.068]	[.060]	[.064]
% Hispanic	.053	.002	.054	−.023	.021	.031
	[.113]	[.078]	[.083]	[.072]	[.063]	[.074]
% Moved Within 5 years	.005	−.056	−.012	−.007	−.006	.008
	[.098]	[.065]	[.098]	[.082]	[.069]	[.064]
Vacancy Rate	.038	−.074	.090	−.007	.050	.026
	[.090]	[.076]	[.076]	[.074]	[.044]	[.042]
Physical Disorder	.028	.152	−.109	−.011	−.053	−.048
	[.081]	[.074]*	[.105]	[.114]	[.071]	[.064]
Log Population	.505*	.438	.451**	.769**	.445**	.407**
	[.231]	[.230]	[.173]	[.212]	[.157]	[.065]
2003 FE					.460**	.451**
					[.060]	[.065]
2006 FE					1.590**	1.585**
					[.078]	[.083]
Constant	1.953	3.140	4.115	−.003	2.600	1.002
	[2.523]	[2.521]	[1.911]	[2.323]	[1.727]	[1.729]
Observations	55	55	55	165	165	165
Wald Chi-squared	114.76	64.32	119.12	156.3	1081.5	832.1
Neighborhood FE?	No	No	No	No	No	No[a]
Year FE?	No	No	No	No	Yes	Yes

Socioeconomic predictors are standardized to a mean of 0 and variance of 1.
Observations weighted by the number of households per neighborhood.
Robust standard errors in brackets; models 4–6 cluster standard errors by neighborhood.
[a] Model 6 includes random intercepts for neighborhoods and AR(1) covariance.
* $p < .05$; ** $p < .01$.

Model 5 includes year fixed effects, but not neighborhood fixed effects, and the standard errors are clustered by neighborhood. The results are unchanged from model 4. The year fixed effects for 2003 and 2006 are significant, reflecting the increase in the stops in the subboros in those periods relative to the 1999 rate. Physical disorder is not significant, nor are the majority of other covariates that characterize neighborhoods. But stops are more frequent in areas with higher concentrations of public assistance receipt, and with higher Black populations, after controlling for homicides and physical disorder. Since homicide rates in New York and physical disorder are correlated with Black population concentration (Fagan and Davies, 2000, 2004), we estimated models including interaction terms for percentage of Black residents and local disorder conditions (physical disorder). The relationship of Black population and the stop rate is robust to the inclusion of either interaction term (data available from authors).

Thus far, model 5 shows a strong and significant relationship between neighborhood racial composition and stop activity; police stop significantly more people in neighborhoods with more Black households. Given that all predictors are standardized, with the exception of the logged number of households, the coefficient magnitudes suggest a particularly strong relationship; racial composition is as important as local crime conditions in predicting police stop activity.

For Model 6, we also included two types of sensitivity analyses. First, we estimated the models including interactions of percent black by lagged crime and percent Hispanic by lagged crime. The results were unchanged. Next, recognizing the potential endogeneity of crime, disorder, neighborhood social and racial composition and stop rates, we estimated propensity scores for the racial composition measures and included them as predictors (results available from the authors). We estimated propensity scores to predict separately the Hispanic and Black concentrations in each neighborhood, and fixed effects for year. We then re-estimated Model 6 to include these propensity scores together with the main racial composition predictor. Following Bang and Robins (2005), we included a predictor that expressed the propensity scores for each racial composition variable in two ways:

$$(1)\ X_{i,j} = 1 / PS_{i,j}$$
$$(2)\ X_{i,j} = 1 / (1\text{-}PS_{i,j})$$

In equations 1 and 2, X is the expression of the transformed propensity score PS_i, the estimated (predicted) racial composition for each race *i* and in neighborhood *j*. We repeated this procedure using the standardized residuals from the propensity score estimation, creating two additional propensity scores expressions. Again, the results using these estimators were unchanged (results available from the authors). Accordingly, the results in Table 13.2 are robust with respect to a variety of controls and specifications of the local crime and social conditions that might influence stop rates.

We also estimated Model 6 using both neighborhood and year fixed effects, but the model fits were unacceptably poor and the results uninterpretable. Which modeling strategy produces the most accurate and reliable accounting for the relationship among neighborhood, crime, and stop activity? Which is a more accurate identification strategy for estimating the effects of policing on neighborhoods? We are confident in the results in models 5 and 6, and reject the unstable results for the neighborhood fixed effects model, for four reasons. First, as mentioned earlier, while there were strong within-neighborhood changes over time, the relative position of neighborhoods in terms of both crime and concentrated socioeconomic disadvantage over time was largely unchanged. In other words, the worst places still are the worst places—the places with the highest homicide rates still are the places with the highest homicide rates, the places with the highest concentration of physical disorder are still the places with the most bad housing, even as the extent of disorder in those places dissipates over time. Neighborhood fixed effects are somewhat helpful in identifying differences between places, but such differences are likely to be unimportant in this analysis. Inclusion of fixed effects for neighborhoods in this context would overdetermine the model, explaining everything and nothing at the same time.

Second, the neighborhoods are changing over time, but the rates of change are dissimilar. The social, economic, and crime conditions in poorer neighborhoods changed more than in wealthier neighborhoods (Fagan, 2008). The assumptions of stable between-unit rates of change in fixed effects models are challenged under these conditions. Third, fixed effects estimators are quite limited when the possibility exists of dynamic selection, or changes in the circumstances or preferences that would affect the assignment of the intervention—police stops, in this case—over time (Bjerk, 2008). Dynamic selection is intrinsic to the policy preferences in the allocation of police resources and tactics in the OMP model (Bratton and Knobler, 1998; Silverman, 1999). This in turn leads to our fourth concern: we think that fixed effects models in this context ask the wrong question. Our interest here is estimating the probabilities of being stopped in neighborhoods of different racial makeup and crime conditions, not with differentials by race of persons within neighborhoods. In other words, ours is a within-neighborhood design, and we seek to explain differences in stop probabilities that are quite dramatic across places and over time.

B. The Efficiency of Street Stops in Detecting Crime

Table 13.3 presents the relationship between neighborhood conditions and "hit rates," or the percent of stops that lead to arrests. As suggested earlier, by figures 13.3a–3c and 13.5a–5c, stop efficacy has declined over the period of analysis, a trend underscored by the year fixed effects in models 5 and 6. We would expect that neighborhood hit rates, driven by the likelihood of stopped residents to be engaged in illegal activity, would not be tied to neighborhood social structure; models 1–5, however, show that arrests per stop are lower in neighborhoods whose populations are predominantly Black: over time, stops in predominantly Black neighborhoods are significantly less productive in yielding arrests than in other parts of the City. Table 13.2 shows that stops are far more prevalent in these areas, to a degree beyond

TABLE 13.3
OLS Regression of Arrrests per Stop, Controlling for Social Structure and Crime, 1998–2006

	Model					
Sample Year	(1) 1999	(2) 2003	(3) 2006	(4) All Years	(5) All Years	(6) All Years
Homicide Arrests (lagged)	.010	.002	.008*	.010*	.007*	.003
	[.010]	[.005]	[.003]	[.004]	[.003]	[.007]
% Receiving Public Assistance	−.010	.000	.006	−.002	.003	−.018
	[.012]	[.007]	[.004]	[.006]	[.004]	[.012]
% Foreign-Born	.000	.002	−.003	.001	−.001	.013
	[.011]	[.005]	[.004]	[.005]	[.004]	[.016]
Racial Entropy	−.007	.000	.007	.003	.004	.011
	[.010]	[.004]	[.004]	[.004]	[.003]	[.016]
% Black	−.029*	−.009	−.012**	−.018**	−.014**	−.013
	[.011]	[.006]	[.004]	[.006]	[.004]	[.039]
% Hispanic	.007	.009	−.004	.001	.001	−.010
	[.014]	[.008]	[.005]	[.006]	[.005]	[.029]
% Moved Within 5 years	−.008	.000	.001	−.004	−.003	−.007
	[.010]	[.004]	[.003]	[.004]	[.002]	[.006]
Vacancy Rate	−.003	.011**	.001	.006	.005	.009
	[.012]	[.003]	[.004]	[.005]	[.003]	[.005]
Physical Disorder	.001	−.009	−.007	−.005	−.006	−.007
	[.011]	[.005]	[.005]	[.006]	[.004]	[.006]
Log Population	−.024	.047*	.016	−.010	.017	.080
	[.031]	[.019]	[.013]	[.014]	[.011]	[.102]
2003 FE					−.070**	−.074**
					[.009]	[.012]
2006 FE					−.108**	−.109**
					[.007]	[.011]
Constant	.412	−.433	−.131	.170	−.035	−.602
	[.339]	[.205]*	[.146]	[.156]	[.125]	[1.085]
Observations	55	55	55	165	165	165
R-squared	.280	.380	.410	.130	.690	.830
Log Likelihood (model)	92.91	119.83	143.11	278.03	363.02	412.01
BIC	−141.7	−195.6	−242.1	−499.9	−659.7	−762.8
Year FE?	No	No	No	No	Yes	Yes
Neighborhood FE?	No	No	No	No	No	Yes

Socioeconomic predictors are standardized to a mean of 0 and variance of 1.
Observations weighted by the number of stops made.
Robust standard errors in brackets; models 4–6 cluster standard errors by neighborhood.
* $p < .05$; ** $p < .01$.

what differential criminal activity would suggest; the models in Table 13.3 suggest that there is little public safety payoff. The results in model 6, however, suggest that race is no longer a significant predictor of hit rates when we treat neighborhoods as fixed effects. But when we estimate Model 6 using random intercepts and population-averaged models, we obtain the same results as in Model 5: arrest rates are significantly lower in neighborhoods with greater black population (for percent black, $b = .13$, $s.e. = .005$, $p = .017$). Again, we face the same issues in interpretation with respect to the neighborhood fixed effects models, and for the same reasons as discussed earlier, we reject the neighborhood fixed effects model in favor of other identification strategies that rely on clustering of standard errors by neighborhood.

Finally, to put the hit rate analysis in perspective of gains and losses, we computed the number of firearms obtained from stops. In 2003, a total of 633 firearms were seized pursuant to stops, a rate of 3.9 firearms per 1,000 stops. More than 90 percent of the firearms seized were pistols. By 2006, following a 300 percent increase in the number of stops, the seizure rate fell to 1.4 firearms seized per 1,000 stops. The firearm seizure rates for Blacks, who were stopped more than ten times the rate per person compared to whites, were slightly higher: 4.6 firearms seized per 1,000 stops in 2003, and 1.6 seizures per stop in 2006. The seven hundred firearms seized in 2006 through stops accounted for about 10 percent of the total number of firearm seizures in New York City that were traced in the nationwide firearm trace system. On the surface, the expenditure of police resources to seize only a fraction of seizures made by other means seems inefficient, to say the least. Since removal of guns from the street was the animating goal of OMP, the low seizure rate is further evidence of the inefficiency if not futility of the strategy.

C. How Much Is Too Much? How Much Is Enough?

The burden of OMP policing in the decade since the Spitzer (1999) report has fallen disproportionately on African Americans, and, to a lesser extent, on Latinos. The strategic goal of OMP has principally been one of law enforcement—maximization of arrests and punishment. This was evident in the policy memoranda that were issued at that the outset of the OMP experiment in 1994. Crime minimization goals were path-dependent on the law enforcement goals, rooted in the putative benefits of increased stops and arrests of citizens for both minor crimes plus the detection of weapons and other contraband. Through careful allocation of police resources, the focus was on "high-crime" areas, which—in the logic of OMP—were those places with the highest concentrations of poor, non-White citizens. The high-crime area concept has proven to be elastic, though, and has expanded now to include public housing developments, despite equivocal evidence that crime in public housing is higher than in the adjacent areas (Fagan and Davies, 2000; Fagan et al., 2006). The result has been a dramatic increase in street stops since 2003, with nearly five hundred thousand New Yorkers stopped in both 2006 and 2007. In addition, tens of thousands of misdemeanor marijuana arrests (Golub et al., 2007; Levine and Small, 2008) are part of the totality of enforcement that nearly blankets some parts of the City.

Crime rates, though, have remained relatively stable in the years since 2003 as stops have increased. Figure 13.4 shows that homicide rates have remained stable after 1999, rising and falling randomly over an eight-year period. One might have expected crime rates to plunge further with the mobilization of OMP tactics, especially with the increase beginning in 2003, but that hasn't been the case. After all, a secondary benefit of maximizing punishment through street stops would be to raise the risk of detection and arrest for carrying weapons, increasing the deterrent threats of OMP tactics. But we are hard-pressed to detect such trends, given the stability of crime rates. Nor have marijuana arrests declined, despite the sharp rise in the likelihood of detection and arrest, so New Yorkers continue to use marijuana, often openly, flouting the law and discounting or ignoring the risks and consequences of arrests.

The inelasticity of crime relative to street stops raises two related questions. First, if crime minimization is the goal of OMP, rather than maximizing punishment without tangible linkages to crime reduction, how many stops are enough to maintain or lower the crime rate? Economists and criminologists have long sought algorithms that would create an optimal level of law enforcement (see Garoupa, 1997; Polinsky and Shavell, 2000, 2007; Curtin et al., 2007) or incarceration (Blumstein and Nagin, 1978) to control crime. For example, Persico and colleagues (2001) suggest that an optimal level of police searches of motorists can achieve an equilibrium across racial groups in the propensities of motorists to transport drugs or other contraband. So are five hundred thousand stops too many? Not enough to control crime? These are important questions, but we do not address them in this chapter.

The second question, though, is a first step in the process of answering the first question. Under current OMP tactics, what is the likelihood of police contact for citizens of specific racial and ethnic groups? Knowing the exposure of different population groups to detection and enforcement is a necessary antecedent to discerning whether there is leverage in these contact rates that can influence crime rates for any population group, or even for the areas where specific groups are concentrated. And if race, neighborhood, and crime are conflated to shape perceptions of "high-crime areas" that merit intensive patrol and enforcement, we would expect the exposure to be highest for non-Whites, and, as we see in figure 13.4, for African Americans in particular.

Accordingly, we estimated the probability of contact during 2006 for non-Hispanic African American males ages eighteen and nineteen, a group that has been the focus of criminal justice policy debate and research attention for nearly two decades (Fagan and Wilkinson, 1998; Cook and Laub, 1998; Loury, 2002; Feld, 1999). There were 28,945 stops of this group during 2006. The total population in 2006, according to the U.S. Bureau of the Census (U.S. Census Bureau, 2006), was 30,999. Accordingly, the point estimate for contact is .93, a figure that on its face is shocking. We reestimated this probability excluding stops made in police precincts in the City's central business districts and park areas: lower Manhattan, Midtown (including Times Square), and Central Park. With these restrictions, we reestimated the probability of contact at .92 (28,539 stops).[7] This compares to estimates of less than .20 for eighteen- and nineteen-year-old White males and .50 for Hispanic males (both Black and White Hispanics).

The stop totals are likely to include persons stopped more than once, so we reestimated these probabilities under varying assumptions about the number of persons stopped more than once and the total number of stops that were repeat stops. Table 13.4a shows that if 10 percent of the African American males ages eighteen and nineteen were stopped more than once, and these repeaters accounted for 25 percent of all stops, the probability of being stopped by the police of anyone in this age cohort is now .79. For example, if 10 percent of the population of Black men aged eighteen and nineteen (approximately 3,100 individuals) are considered "high-stop individuals," and this group makes up 25 percent of all stops within this demographic bracket, then these 3,100 people were stopped a combined 7,135 times. These men were stopped an average of 2.3 times over the course of the year, rather than the 0.92 suggested by the raw numbers. Assuming that the remaining stops (21,404) are distributed one-per-person, the total number of people stopped over the course of the year would be 24,504. Although the raw ratio of stops to people in this demographic bracket is 0.92, the actual percentage of the population stopped by the police is lower, 0.79, shown in the upper-left cell of table 13.4a. If 25 percent of the persons were stopped more than once and they accounted for 50 percent of all stops, the probability declines to .71. Note that in table 13.4a, some cells could not be computed because the total number of stops would exceed the population in that group.[8]

We next expand the age boundaries for these estimates to include males ages eighteen to twenty-four. This age group was disproportionately involved in lethal violence throughout the 1990s in New York (Fagan and Wilkinson, 1998; Fagan et al., 1998) and elsewhere in the United States (Cook and Laub, 1998; Zimring and Hawkins, 1997). Also, desistance from crime increases substantially as persons reach their mid-twenties (Farrington, 1998). The unadjusted probability of being stopped in 2006, before accounting for repeaters, is .14 for non-Hispanic Whites, .78 for African Americans, and .39 for Hispanics.

Tables 13.4b–d show the rates accounting for different assumptions about the number of repeaters and the number of repeat stops. Given the lower stop rates of Whites and Hispanics, we rescaled the probabilities in tables 13.4c and 13.4d, hence the comparisons reflect distributions that are unique for each racial or ethnic group. Under the most likely scenarios, tables 13.4b–d show that when 10 percent of the persons account for 25 percent of the stops, the probability that an African American male is stopped (.69) is still far greater than the probability that a White or Hispanic male is stopped. Under more restrictive and conservative assumptions—that 50 percent of the persons account for 75 percent of the stops, we still estimate rates for African Americans that are twice the rate of Hispanics.

The important context in which to view these numbers is whether they are productive; by any reasonable standard, however, they are not. Figure 13.3 (a,b,c) shows two important features of hit rates: there are only negligible differences between hit rates for Whites, African Americans, and Hispanics, and the rates themselves are approximately 5 percent. Beyond the evidence of racial disparity, we are also concerned that these extraordinary stop rates of African Americans include a high volume of excess stops, stops that express unwarranted blanket suspicion that may have little marginal deterrent or law enforcement returns. But with stop rates this high and inefficiencies running at 96 percent, claims of a general deterrent effect from these stops are empirically strained by the scarcity of sanctions. So deterrence or crime control may be a secondary goal to maximization of punishment. And efficiency concerns are only one side of the social and public good of policing: equity, fairness, and distributive considerations co-occupy another dimension of policing (Moore, 2002). Even if we thought that there were crime control returns, it seems unlikely that most citizens would condone trading in the private harm of excess stops of African Americans, not to mention the stigma and internalized psychological costs, against putatively lower susceptibility to crime for the majority group. The costs of this regime lie in the harm to the 95 percent who are innocent in these excess stops.

TABLE 13.4A

Probability of Stops for African American Males, Ages 18–19, 2006

	% Repeat Stops		
	25%	50%	75%
% Stopped More Than Once			
10%	0.79	0.56	0.33
25%		0.71	0.48
50%			0.73

Note: Excludes stops that were made in 1st, 14th, 22d, and 18th precincts.
Population: 30,999. Stops: 28,539.

TABLE 13.4B

Probability of Stops for African American Males, Ages 18–24, 2006

	% Repeat Stops		
	25%	50%	75%
% Stopped More Than Once			
10%	0.69	0.49	0.30
25%		0.64	0.45
50%			0.70

Note: Excludes stops that were made in 1st, 14th, 22d, and 18th precincts.
Population: 104,880. Stops: 82,125.

TABLE 13.4C

Probability of Stops for Hispanic Males, Ages 18–24, 2006

	% Repeat Stops		
	25%	50%	75%
% Stopped More Than Once			
10%		0.29	0.20
20%			0.30
25%			0.35

Note: Excludes stops that were made in 1st, 14th, 22d, and 18th precincts.
Population: 127,128. Stops: 48,968.

TABLE 13.4D

Probability of Stops for Non-Hispanic White Males, Ages 18–24, 2006

	% Repeat Stops		
	25%	50%	75%
% Stopped More Than Once			
2%	0.12	0.09	0.05
5%		0.12	0.08
10%			0.13

Note: Excludes stops that were made in 1st, 14th, 22d, and 18th precincts.
Population: 107,936. Stops: 15,065.

V. Discussion

For nearly a decade, through a prolonged era of stably low crime rates and improving social and economic health across the City's neighborhoods, the number and rate of stops of citizens has increased by more than 500 percent while the efficiency of those stops has declined by nearly 50 percent. The burdens and benefits of these stops are disproportionately concentrated in the City's poorest neighborhoods, the places with both the highest crime rates and the highest proportions of non-White households. Our focus in this chapter is not on the race or ethnicity of individual stops of citizens, but on the rates of stops in neighborhoods with the highest concentrations of Black residents. We focus on neighborhoods because place, not individuals, has been most closely linked to the logic of policing under OMP since its inception fifteen years ago. It is place that is the focal point of the underlying theories of order-maintenance policing, place is the unit of analysis for the allocation and deployment of police resources, and the indicia of crime in places are the metrics by which the resources of the police are managed and evaluated. And the benchmark of place, in conjunction with crime, is sensitive to the actual allocation of police resources as well as tactical decisions by the NYPD, and is widely used in research on selective enforcement in policing (Alpert et al., 2005; Fagan, 2002; Fridell, 2004; Skogan and Frydl, 2004).

The effects we observe in these analyses are notable in three ways. First, stops within neighborhoods take place at rates in excess of what would be predicted from the separate and combined effects of population demography, physical and social conditions, and the crime rate. This excess seems to be concentrated in predominantly Black neighborhoods. Second, the excess stops in these neighborhoods persist over time, even as the Black population declines, crime rates remain low and effectively unchanged, the City's overall social and economic health improves, and housing and other investments increase across the City's neighborhoods, including its poorest and most segregated neighborhoods. Third, there appears to be a declining return in crime detection from marginal increases in enforcement, and this efficiency gap seems to grow over time. Like the stops that supply the arrests, the declining number of arrests that take place pursuant to stops are disproportionately concentrated in neighborhoods with higher Black populations, after controlling for crime, poverty, and disorder in those places.

The preferences for neighborhood selection for intensified stops seems to be inelastic to changes in crime rates or to the limited payoffs in arrest efficiencies from marginal increases in stops. This inelasticity is difficult to understand as either individual preferences of police officers, or as a rational tactical or management decision. As the rank and file of police in New York become more diverse and reflective of the City's demography, it is unlikely that individual preferences or subjective assessments of suspiciousness by individual officers would continue to be racially skewed over time and through changes in the social contexts of the areas they patrol.

Institutionally, the declining returns to crime control from marginal increases in stop activity is the opposite of economics. We assume, from the policy statements of police in New York, that the goal of stops is to minimize and deter crime rather than to maximize the hit rate of stops. An elastic policy sensitive to crime rates might seek to locate an optimal level of stop activity within each neighborhood or patrol area and adjust in real time. Dominitz and Knowles (2006) suggest that such a crime minimization approach works only if the priors of illegal behavior are known to vary across groups in specific ways. Perhaps the absence of assumptions or knowledge of specific variation in between-group (and by extension, between-neighborhood) crime preferences explains the persistence of these stop patterns. But we doubt that the NYPD is flying blind, since the allocation of police to neighborhoods and smaller areas is driven by real-time data about group- or area-specific crime rates.

So there is no simple explanation for the exponential growth over time in stops in the face of broad, long-term secular declines in crimes across all population groups in all places, and in the face of declining yields of legally sustainable arrests (Weiser, 2008). What then can explain the durability of a policy whose utility is weakening over time? Two possibilities come to mind. The first is that these patterns over time reflect a durable institutionalized preference to maintain these tactics even as their necessity and value is less apparent, and even as the practice's political costs mount. The practice has persisted through sharp political and legal criticism (Spitzer, 1999) and civil rights litigation against the NYPD that resulted in injunctive relief and oversight by private legal groups (*Daniels et al. v. City of New York*, 2003).

Beyond political costs, the persistence of policing tactics with disparate neighborhood impacts has salient social costs. Normative considerations—the absence of tangible returns from the policy and practice in the face of high social costs to citizens that are unevenly distributed by race and by place—suggest that the policy diminishes the social good of policing and weakens its welfarist ideology (Durlauf, 2006b), while making the job of the police harder (Skogan and Frydl, 2004; Harris, 2002). The dissipation of the social good itself has one-off costs—the withdrawal of citizens' cooperation with the police in the civic project of the coproduction of security (Tyler and Fagan, 2008; Fagan and Meares, 2007), or, in the worst case, defiance of legal and social norms (Fagan and Meares, 2007; Paternoster et al., 1997; Sherman, 1993). But such external criteria are beside the point if the preference is internalized; it need only be justified within the internal logic of the organization. Whether habit or something more, the maintenance of this policy responds to internalized incentives that remain invisible to outside observers. Its persistence requires a form of "racial blindsight" (Taslitz, 2007) to deracialize institutional recognition and acknowledgement of its consequences.

The second possibility is more mundane, and has two faces. Stops and searches of citizens are simple productivity measures for the police. Generating accurate and detailed information about stops conducted by police provides a numerical measure of police activity and outputs that is easily conveyed to citizens and oversight entities. This is especially important as crime rates decline and the traditional metrics of police productivity—arrests, crimes—no longer are sufficiently sensitive to gauge the efforts of a large and complex organization (Moore, 2002). If policing is a public good, the stop numbers provide a valuable measure of the services that produce that good.

Stops also generate a cheap form of intelligence. Intelligence was the traditional utility of the data generated in the course of stops and searches of citizens (Spitzer, 1999).[9] For years, the reports generated by stops of citizens sat in file drawers in precincts and were examined as police searched for suspects when crime patterns emerged. The information was entered into databases starting in the late 1990s, in part as a response to external investigations in reaction to political conflict following a sequence of violent, tragic, and well-publicized deaths of two citizens during encounters with the police (Spitzer, 1999). This rudimentary neural network of information was automated in the late 1990s, and has evolved into a systematic database that is one of the primary sources of information on police activity.

These institutionalized preferences, which endure in the face of persistent utility, serve the bureaucratic interests of the police hierarchy. Normative concerns over racial impacts take a backseat to the institutional interests that are indifferent to the potential for externalized costs and racial inequalities that ensue from a sustained policy with declining returns. Yet everyone has a stake in a safe society, and so security—which is primarily the province of the police—is a public good (Loader and Walker, 2007). Policing is not a discretionary service, nor is it nontrivial in the sense that it is cost-free. In New York, the cost burden of this safety—which largely accrues to White New Yorkers—is shifted to the 95 percent of African American citizens who are stopped but innocent of whatever suspected crime triggered the action. The benefits of policing—safety, calling offenders to account, conflict resolution, order, information—are social goods that are available to everyone at a low social cost, or at least at a cost that is equitably distributed. The production of this social good is not well served by the patterns we observe over the past decade of order-maintenance policing in New York.

Appendix A: Specific Police Conduct Permitted under DeBour

A. What Is a Stop?

Police stop-and-frisk procedures have been ruled constitutional under specific conditions articulated in *Terry v. Ohio* (1968). Under *Terry*, Fourth Amendment restrictions on unreasonable searches and seizures allow a police officer to stop a suspect on the street and search him or her without probable cause if the police officer has a reasonable suspicion that the person has committed, is committing, or is about to commit a crime. For their own protection, police may perform a quick surface search of the person's outer clothing for weapons if they have reasonable suspicion that the person stopped is armed. This reasonable suspicion must be based on "specific and articulable facts" and not merely on an officer's hunch.

B. Permissible Behaviors

New York law regulates police conduct more thoroughly than does *Terry*. The state law articulates a four-step analysis articulated in *People v. DeBour* (1976) and *People v. Holmes* (1996). Stops are governed by N.Y. Crim. Proc. Law § 140.50(1) (2007):

"In addition to the authority provided by this article for making an arrest without a warrant, a police officer may stop a person in a public place located within the geographical area of such officer's employment when he reasonably suspects that such person is committing, has committed or is about to commit either (a) a felony or (b) a misdemeanor defined in the penal law, and may demand of him his name, address and an explanation of his conduct."

"Stops" and "frisks" are considered separately under New York statutes. A police officer may stop a suspect but not to frisk him given the circumstances. Frisks and

TABLE 13.A1
DeBour's Four Levels of Street Encounters[a]

Predicate	Permissible Response
Level 1	Objective Credible Reason Approach to Request Information
Level 2	Founded Suspicion—Common Law Right of Inquiry
Level 3	Reasonable Suspicion Stop and (If Fear of Weapon) Frisk
Level 4	Probable Cause Arrest and Full Search Incident

[a] *People v. DeBour*, 40 N.Y. 2d 210 (1976).

TABLE 13.A2
Permissible Actions by Police Officers during Stops

Predicate	Permissible Response
Level 1	PO can ask nonthreatening questions regarding name, address, destination, and, if person carrying something unusual, police officer can ask about that. Encounter should be brief and nonthreatening. There should be an absence of harassment and intimidation. PO can: • say "STOP" (if not "forceful") • approach a stopped car • touch holster. PO cannot: • request permission to search • cause people to reasonably believe they're suspected of crime, no matter how calm and polite the tone of the questions.
Level 2	PO can ask pointed questions that would reasonably lead one to believe that he/she is suspected of a crime. Questions can be more extended and accusatory, and focus on possible criminality. PO can: • request permission to search. PO cannot: • pursue • forcibly detain.
Level 3	PO can: • forcibly detain • frisk for weapons if in fear • pull car out of traffic flow • order defendant to lie on the ground • handcuff (for good reason) • pursue.
Level 4	PO can: • arrest and search suspect.

searches are governed by N.Y. Crim. Proc. Law § 140.50(3), which requires a legitimate "stop" as a predicate to any frisk.[10] In many cases, reasonable suspicion that a person is engaging in violent or dangerous crime (such as murder, burglary, assault, etc.) will justify both a stop *and* a frisk. Table 13.A1 shows the circumstances that are necessary for a stop to escalate to a frisk and ultimately to an arrest. Table 13.A2 shows the specific police actions that are permitted at each level of a *Terry/DeBour* stop in New York.

NOTES

1. The facts of the case and its doctrinal implications have been the subject of intense interest in both constitutional criminal procedure, case law, and legal scholarship. On October 31, 1963, Cleveland police detective Martin McFadden saw two men (John W. Terry and Richard Chilton) standing on a street corner and acting suspiciously. One man would walk past a certain store window, stare in, walk on a short distance, turn back, stare in the store window again, and walk back to the other man and converse for a short period of time. The two men repeated this ritual alternately between five and six times apiece—in all, roughly a dozen trips. Each completion of the route was followed by a conference between the two on a corner, at one of which they were joined by a third man, who subsequently left swiftly. Suspecting the two men of casing the store for a robbery, McFadden followed them and saw them rejoin the third man a couple of blocks away. The officer approached the three men, identified himself as a police officer, and asked their names. When they "mumbled something" in response, McFadden patted them down for weapons and discovered that Terry and Chilton were armed. He removed their guns and arrested them for carrying concealed weapons. When the trial court denied his motion to suppress, Terry pleaded not guilty, but the Court found him guilty and sentenced him to one to three years in prison.

2. The procedure to generate a stop rationale takes place pursuant to the stop, not before, and therefore may be endogenous to the stop. Except in "radio runs," where officers are dispatched to a crime scene or location based on a citizen report or a report by another officer, and where a suspect description is provided by the dispatcher, the classification of a stop as being motivated by the match between a citizen and a "suspect description" is determined after the stop is concluded and the UF-250 form is completed. There is no method to verify the basis for the formation of suspicion for the stop. And since many stops are generated simply because the suspect "looked like a perp" (Bacon, 2009), there is considerable potential for error and theoretical misspecification. To put it less politely or scientifically, the stated rationale for the stop may in fact be either racialized, highly conditional on the conditions where the stop takes place, or simply a fiction.

3. We preferred to use both homicide arrests and homicides to test the robustness of our estimates, as well as a wider range of localized crime rates. Unfortunately, we were not privileged by the NYPD with access to its data of reported crimes that could be disaggregated to precincts, neighborhoods, and subboros. Those data were not published by the NYPD in summary form after 2001.

4. The partial correlations by year and precinct from 1984 to 2000 between homicide arrests and arrests for other Part I felony crimes was .633, and .711 for all felony crimes. For crime complaints, the partial correlation by year and precinct from 1984 to 2000 between homicide arrests and crime-specific complaints were .810 for murder, .704 for rape, .629 for robbery, and .791 for assault.

5. The stop rate and racial and ethnic distribution in these areas are:

TABLE 13.N1

Neighborhood	Stops per Household 2006	Percent African American	Percent Latino
Brownsville/Ocean Hill	.68	78	15
East New York	.65	45	38
Central Harlem	.52	71	14
East Harlem	.51	36	45
Bedford Stuyvesant	.49	72	16
Mott Haven/Hunts Point	.44	21	76

Source: New York City, Department of City Planning.

6. When arrests are made by the police upon observation of a crime, such as smoking marijuana, a stop report is completed to back-fill the case record. Accordingly, some portion of both crime complaints and stops reflect arrest-generated activity rather than independent police events.

7. In these estimates, we include Black Hispanics among Hispanics, not among African Americans.

8. Table cells are left blank in cases where the hypothesized population/stop allocations do not correspond to a "high-stop" population stopped multiple times per year. For example, in table 13.4a, the lower-left cell posits a distribution where 50 percent of the population accounts for 25 percent of the stops. If 25 percent of stops (7,135) were evenly distributed over 50 percent of the population (14,270 people), this would roughly correspond to only one-half of a stop per person. Since police stops are discrete events, an average stop rate of less than one stop per person suggests that either the "high-stop" population is overestimated, or that the portion of stops allocated to this group is underestimated. In either case, the cell is left blank, since the combination does not represent a scenario where a portion of the population is stopped repeatedly.

9. For juveniles, the parallel intelligence-gathering mechanism is the issuance of so-called YD cards to minors who are stopped by the police but not arrested. YD (for Youth Division) cards are not entered into electronic databases.

10. "When upon stopping a person under circumstances prescribed in subdivisions one and two a police officer or court officer, as the case may be, reasonably suspects that he is in danger of physical injury, he may search such person for a deadly weapon or any instrument, article or substance readily capable of causing serious physical injury and of a sort not ordinarily carried in public places by law-abiding persons. If he finds such a weapon or instrument, or any other property possession of which he reasonably believes may constitute the commission of a crime, he may take it and keep it until the completion of the questioning, at which time he shall either return it, if lawfully possessed, or arrest such person." N.Y. Crim. Proc. Law § 140.50(3).

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