

# Understanding the Effects of Changes in Racial Profiling

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January 2006

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## **Abstract**

Many police agencies have enacted measures designed to reduce racial profiling, yet little empirical evidence exists regarding the effects of such programs. In this paper I use the occurrence of a racial profiling scandal in New Jersey to quantify the effect of a move towards more race-neutral policing. Using a quadruple-differences approach, I estimate that the scandal and subsequent reforms led to a decrease in annual arrests of minorities of 10-40% for motor vehicle theft and DUI. I also present evidence that as enforcement against minorities fell, motor vehicle theft increased in areas populated by minorities. Detailed data on accidents in New Jersey generates little strong evidence of additional adverse responses by minorities to lessened police scrutiny. The findings are robust to a number of specification checks, and similar patterns are observable in Maryland, a state which experienced a profiling scandal several years prior to New Jersey.

Keywords: Crime; Racial profiling; New Jersey  
JEL classification: K42

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I wish to thank Steven Levitt, Charles Loeffler, Robert Sampson, Sam Schulhofer-Wohl, Mathis Wagner, and seminar participants at the University of Chicago for helpful discussions. Financial support for this research was provided by the National Science Foundation and the National Consortium on Violence Research (NCOVR). All opinions expressed are solely those of the author. My e-mail address is [psheaton@uchicago.edu](mailto:psheaton@uchicago.edu).

## I. Introduction

Recently the public and policymakers have given increased attention to racial profiling, which involves using race as a basis for increased suspicion--and therefore higher police scrutiny--of minority drivers. As police agencies have become more conscious of the issues surrounding racial profiling and racial bias in general, many have adopted reforms designed to reduce bias in policing. By 2005, 26 states had enacted legislation requiring police agencies to collect data on the race of motorists stopped, with selected police departments in 21 other states voluntarily agreeing to collect such data. A number of states, including California and Florida, have passed legislation mandating special training for all police officers to combat profiling. Yet there is little empirical evidence demonstrating the likely effects of such reforms on either arrest rates or other behaviors that may be responsive to arrest rates.

In this paper I use the historical experience of New Jersey, which enacted a number of reforms to combat racial bias after a profiling scandal, to quantify the effects of a move to more race-neutral policing. I find that the scandal led to substantial drop in arrests of Blacks relative to Whites for offenses susceptible to detection through traffic stops. I also present evidence that the resulting drop in arrests led to higher motor vehicle theft rates in minority areas. The estimated magnitudes of the drop in arrests and subsequent increase in offending generate plausible values for the elasticity of offending with respect to enforcement. I then confirm my findings using data from Maryland, a state that experienced a profiling scandal several years before New Jersey. Turning to other driving behavior that may be linked to the probability of police stops, I examine detailed crash records from New Jersey to gauge the extent to which intoxicated driving, speeding, and transporting young children without proper child restraints may have been affected by the scandal. I find little evidence that minorities adjusted to the decrease in enforcement along these margins.

There has been a recent upswing in profiling-related research in part attributable to a number of studies conducted by states that have found the Black motorists are significantly more likely to be stopped and searched than their White counterparts.<sup>1</sup> Broadly stated, past academic research has tended to focus on two questions regarding profiling: 1) Does racial profiling occur? and 2) Should race-based differences in enforcement be permitted?<sup>2</sup> A major thread in the legal literature, for example, examines whether efficiency is an acceptable criterion on which to base race-oriented enforcement policies. Economists have focused on developing models of profiling that allow researchers to statistically detect the presence of bias using traffic stop data, while criminological research has measured public perceptions of profiling. This paper, in contrast, answers a different but equally important question—what are the consequences of programs aimed at reducing racial bias by police?

One challenge in profiling-related research is that there exists no direct measure of the degree to which police engage in racial profiling. This study does not provide such a measure, but instead infers that the large decrease in arrests of minorities following a racial profiling scandal occurred

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<sup>1</sup> There are many such studies produced by both academics and government agencies. Spitzer (1999) and Barnes and Gross (2002) provide prominent examples.

<sup>2</sup> Harcourt (2004) provides a summary of the economics and legal literature regarding profiling. Batten and Kandleck (2004) summarize profiling research in sociology and criminology.

because police reduced profiling behavior. Because profiling could theoretically vary independently of arrests, a more ideal study would measure profiling directly. From a policy perspective, however, arrest data have useful empirical content. Most criticisms of profiling implicitly assume that it is "unfair" for a particular group to be subjected to search or arrest at a higher rate than others. Arrest data measure the extent to which these "undesirable" arrests might be mitigated as profiling decreases.

Section II of the paper presents a simple framework for understanding the likely effect of changes in profiling. Section III reviews the history of the racial profiling scandal in New Jersey. Section IV discusses the effect of the profiling scandal on arrest rates. Section V examines the extent to which decreased arrests led to increased crime and includes scandal-derived estimates of the elasticity of vehicle theft with respect to policing. Section VI demonstrates that the patterns in arrests and offending observable in the New Jersey are also evident in data from Maryland, which experienced a profiling controversy several years earlier. Section VII examines reckless driving, another type of behavior that may be linked to police stops, and finds little evidence of a substantial impact of the profiling reforms on this behavior. Section VIII concludes.

## II. A Simple Model of the Effects of Profiling Changes

Economics research into profiling has primarily been oriented towards modeling search behavior by police officers to develop econometric tests for the presence of profiling. One of the most influential papers by Knowles, Persico, and Todd (2001) presents a model in which officers select people to search based on observables with the objective of maximizing the number of successful searches leading to arrests or seizure of contraband. Rational criminals respond to officers' search efforts, resulting in an equilibrium in which "hit rates," or the proportion of searches leading to arrest, are equalized across races. If officers have a taste for discrimination, hit rates would be lower for groups against whom officers discriminate, reflecting the tendency of discriminatory officers to target such groups for searches.<sup>3</sup>

In this paper I am concerned with quantifying the effects of a shift in profiling behavior as opposed to testing for the presence of profiling. To understand the likely consequences of a reduction in profiling, consider a population of individuals belonging to one of two types,  $t_i$ ;  $t_i \in (A, W)$ . Let  $V_{ij}$  denote the value to individual  $i$  of committing crime  $j$ . One simple expression for  $V_{ij}$  is given by:

$$V_{ij} = b_{ij} - p(t_i, r) c_{ij} \tag{1}$$

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<sup>3</sup> A number of papers have extended this basic model in several directions. Hernandez-Murillo and Knowles (2001) modify the model to allow for non-discretionary search. Persico (2002) presents a more general model of police auditing that allows for stigma from being singled out by police. Anwar and Fang (2005) modify the model to relax the assumption that the probability of carrying contraband is equal for all citizens conditional on race and to allow differences in search behavior according to the race of the officer.

where  $b$  denotes the net psychic or monetary benefit of committing a crime and  $c$  the cost of being detected. I assume for simplicity that all traffic stops result in detection and that the probability of a stop,  $p$ , depends on the agent's type as well as the degree of racial profiling by police  $r$ , with  $\frac{\partial p(A,r)}{\partial r} > 0$  and  $\frac{\partial p(W,r)}{\partial r} \leq 0$  (2). These assumptions imply police can set separate search intensities for the different agent types and that as racial profiling increases the probability of detection increases for agents of type A but not those of type W. In this simple model I abstract from modeling policing efficiency, which is a major focus of the model in Knowles et. al. and related papers, and instead take the level of profiling and associated search probability as exogenous.<sup>4</sup>

In equilibrium all agents with positive valuations will commit the crime, and agents for which  $b_{ij}=p(t_i,r)c_{jt}$  will be indifferent between committing the crime or not. Let the cumulative distribution of  $\frac{b_{ij}}{c_{ij}} \Big|_{t_i=t}$ , or the relative benefit of crime  $j$  across individuals of type  $t$ , be given by  $F_{jt}$ . The equilibrium proportion of type  $t$  individuals who are criminals,  $\pi_t$ , can be expressed as:

$$\pi_t = 1 - F_{jt}(p(t,r)) \quad (3)$$

The effect of profiling changes on offenses can be found differentiating (3) with respect to  $r$ :

$$\frac{\partial \pi_t}{\partial r} = -f_{jt}(p(t,r)) \frac{\partial p(t,r)}{\partial r} \quad (4)$$

Given assumption (2), for type A agents, a decrease in profiling will lead to a higher proportion of offenders in the population. For type W agents, decreases in profiling decrease or leave unchanged the proportion of offenders. The effect of a decrease in profiling on the arrest rate,  $a_t$ , is given by:

$$\frac{\partial a_t}{\partial r} = \frac{\partial p(t,r)}{\partial r} [1 - F_{jt}(p(t,r)) - f_{jt}(p(t,r))p(t,r)] \quad (5)$$

In this simple model a decrease in profiling could actually increase the arrest rate of type A agents. Intuitively, such a perverse effect might occur if reducing profiling shifted enough additional individuals into criminality to more than offset the reduced probability of capture. Expression (5) is more likely to be negative when  $f_{jt}(p(t,r))$  or  $p(t,r)$  are large, implying that there are many individuals on the margin of committing crime or individuals who engage in a crime are likely to be captured. Given that the probability of capture is low for most crimes susceptible

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<sup>4</sup> Within the framework of Knowles et. al., one potential interpretation of the institution of data collection, police training, or other anti-profiling measures is the introduction of a constraint on officers that the difference between African-American and White search rates be bounded above by some level. As in my model, because potential criminals respond rationally to the expected search probability, if such a constraint binds the model predicts that the anti-profiling measures will lead to an increase in offense rates among African-Americans. In the more general model presented by Persico (2002), he offers exact conditions under which the implementation of this constraint would lead to more crime.

to detection through vehicle stops, it seems likely in practice that  $\frac{\partial a_i}{\partial r}$  will have the expected sign. Indeed, in the analysis that follows I empirically examine the magnitude of changes predicted by expressions (4) and (5) and demonstrate that offenses rise and arrests fall as profiling decreases.

### III. The Racial Profiling Scandal in New Jersey

The identifying variation used in this paper comes from changes in policing behavior that arose due to a scandal involving the New Jersey State Police which developed during 1998 and 1999. The scandal was precipitated by an incident in April 1998 in which white police officers shot four African-American and Hispanic men driving on the New Jersey Turnpike. Although police claimed that the drivers were speeding and engaging in threatening behavior, it was later revealed that none of the victims was armed and the police did not have radar equipment that would have permitted them to ascertain the speed of the targeted vehicle. The shootings led to a significant public outcry, including public protests led by Al Sharpton and a civil suit filed against the state of New Jersey by a legal team led by Johnnie Cochran. In response to the events, New Jersey Governor Christine Todd Whitman ordered the state's attorney general to launch a comprehensive investigation into the state police's policies and procedures regarding race. The investigation included compilation of data on the race of individuals stopped by state troopers on selected highways collected in 1997 and 1998.

In February 1999 the attorney general's office released a preliminary report stating that racial profiling was a significant problem, citing data suggesting that minority motorists were disproportionately stopped and indicating that over 75% of police searches were directed at Black and Hispanic motorists.<sup>5</sup> The report recommended a number of reforms to combat racial profiling, including better collection and publication of data on police stops, new training, changes to the standard operating procedures associated with stops, and establishment of clear criteria governing acceptable stops and searches. In a follow-up report issued in July the Attorney General's office claimed that significant progress had been made in implementing the recommendations of the earlier report.

A number of other events in 1999 drew public attention to the problem. The profiling scandal was exacerbated by comments made by New Jersey state police superintendent Carl Williams, who stated in an interview in late February with the *Newark Star-Ledger* that minorities were more likely to be involved in cocaine or marijuana-related drug crimes. Williams was fired by Whitman after his comments were published. In April, the nomination of Attorney General Peter Verniero to fill a vacancy on the State Supreme Court stalled after allegations surfaced that he had seen evidence of profiling several years before the turnpike shooting. In September, two of the troopers involved in the original shooting incident were indicted for attempted murder.

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<sup>5</sup> Interestingly, the report also states that the hit rates for the searches considered were 10.5% for Whites, 13.5% for Blacks, and 38.1% for Hispanics. The efficiency model presented by Knowles et. al. (2001) would suggest that these hit rates imply *under*-searching of minority motorists.

These events, particularly Williams' firing, may have helped to increase the perception among state troopers that the new reforms would lead to zero tolerance for profiling.

One advantage of examining profiling in New Jersey as opposed to other localities is that the turnpike shooting led to fairly substantial changes in police policy and practice that were implemented over a relatively short time period. Figure 1, for example, demonstrates that while there was virtually no mention of racial profiling in New Jersey newspapers prior to 1998, the number of articles discussing profiling increased substantially in 1998 and again in 1999, suggesting that the turnpike shooting and subsequent events had a dramatic effect on public awareness of profiling. The abruptness of the change in police procedure makes it possible to isolate effects due to changes in profiling behavior from other secular trends in criminality and enforcement that may have affected arrest and offending rates.

#### **IV. The Effect of the Scandal on Arrest Rates**

An important empirical question is the degree to which the profiling scandal affected police behavior. To examine this question, I compare arrest rates of Blacks, who presumably would be subjected to less police scrutiny after the post-scandal reforms, to those of Whites.<sup>6</sup> Figure 2 plots the number of arrests by race for several types of crimes. Data on arrests was obtained from the FBI's Uniform Crime Reports (UCR). To facilitate comparison across races, the total number of crimes in each year is divided by the number of crimes committed in 1997, so that the crime incidence for both groups is equal to one in 1997. Several patterns are apparent from the top panel. Consistent with the general national trend, crime in New Jersey fell considerably between 1997 and 2003. For aggregate crime, relative arrest rates for Blacks and Whites closely track each other throughout the entire period, typically differing by fewer than two percentage points.

The bottom panels of Figure 2 plot arrest trends for motor vehicle theft and DUI, two types of arrests closely linked to the tendency of police to stop vehicles.<sup>7</sup> Although Black and White relative motor vehicle arrests track each other closely prior to 1998, the arrest rates diverge sharply in 1998 and 1999, with Black relative arrest rates dropping 20-40% below White rates. These differences persist through 2003. DUI arrests exhibit a similar pattern, with an enormous, persistent drop in Black arrests beginning in 1999, leaving Black relative arrest rates about 15% below White rates by 2003.<sup>8</sup> Qualitatively it appears that there were substantial decreases in arrests due to the profiling scandal.

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<sup>6</sup> Although Hispanic motorists would likely also be affected by profiling changes, the UCR data for many jurisdictions in New Jersey does not include ethnicity of arrestees.

<sup>7</sup> I am unaware of data sources that directly measure the proportion of arrests for various crimes that are made via vehicle stops as opposed to other means. Aggregate data provide some evidence indicating that vehicle stops are important for these two types of crimes. In California in 2000, for example, vehicle theft and DUI respectively accounted for 1.5% and 12.9% of all arrests statewide. For the California Highway Patrol, however, vehicle theft and DUI accounted for 3.1% and 76.8% of arrests.

<sup>8</sup> Drug arrests exhibit a similar pattern to motor vehicle theft and DUI arrests, although the arrest rates for Blacks and Whites are less comparable in the pre-period. Other types of property crime and violent crime do not exhibit this pattern.

Figure 3 presents 3-month moving averages of motor vehicle theft arrests by race. In the period prior to April 1998 the average for Blacks was always above that of Whites. Beginning in April 1998 arrests of Blacks declined substantially. Although the arrest counts converged somewhat in the later half of 1998, this was mainly due to a decrease in White arrests that did not persist into 1999. The timing of the shift in arrest patterns suggests the shift was precipitated by the onset of the scandal as opposed to other factors. Interestingly, in the case of vehicle thefts the turnpike shooting itself appears to have prompted a rapid decline in arrests even before the completion of investigations and the institution of new police procedures.

To formally estimate the effect of the profiling scandal, I employ a quadruple-differences (DDDD) approach. The basic insight underlying this choice of methodology is that the effects of the scandal are likely concentrated in terms of the time periods, types of crimes, types of people, and locations affected. There were likely few changes in profiling in the years prior to 1998, so fluctuations in the number of Whites and Blacks arrested in this period provide a measure of the degree of variability in arrests arising from factors other than profiling. Arrests for Whites are presumably less sensitive to profiling than those of Blacks, allowing Whites to act as a comparison group for Blacks<sup>9</sup>. Additionally, the only types of arrests that are likely to be affected by changes in profiling are those associated with vehicle stops, such as vehicle theft, DUI, and drug and weapons arrests, so arrests for other types of crimes provide a natural control group. Finally, the profiling scandal was precipitated by action by the New Jersey State Police and the scandal was focused in New Jersey, allowing changes in arrest patterns in other states to potentially act as controls.

Multiple-difference estimates of the causal effect of the scandal can be implemented within a regression framework. Let  $y_{ijkt}$  denote the log offense rate of group  $i$  for crime  $j$  in location  $k$ ,  $I_n$ ,  $n \in N = \{i, j, k, t\}$  an indicator equal to 1 for an observation affected along dimension  $n$ <sup>10</sup>,  $d_t$  a vector of year fixed effects, and  $a_{ijkt}$  an indicator for an affected group, crime, location, and time period. In the regression:

$$y_{ijkt} = \beta_0 a_{ijkt} + \sum_{n \in N} \beta_n I_n + \sum_{\substack{n_1, n_2 \in N \\ n_1 \neq n_2}} \beta_{n_1 n_2} I_{n_1} I_{n_2} + \sum_{\substack{n_1, n_2, n_3 \in N \\ n_1 \neq n_2 \neq n_3}} \beta_{n_1 n_2 n_3} I_{n_1} I_{n_2} I_{n_3} + d_t + \varepsilon_{ijkt} \quad (6)$$

$\beta_0$  measures the percentage change in offending due to the scandal-driven adjustments in profiling. The DD and DDD regressions can be obtained by omitting the relevant summation terms and dimensions of  $N$  from (6).

Table 1 presents DDDD estimates of the effect of the scandal on arrests. The unit of observation is the logarithm of the arrest rate for a particular state, crime, racial group, and year. Years from 1994 to 2003 are included. The first column reports traditional differences-in-differences (DD)

<sup>9</sup> A reduction in profiling might lead to lower scrutiny of Blacks, which would manifest itself in a decrease in Black arrests. Alternatively, profiling could be reduced by elevating the degree of scrutiny given Whites (i.e. substituting police attention from Blacks to Whites) to equal the degree of scrutiny given Blacks, which would increase the number of arrests of Whites. Either mechanism will be manifested by a change in the *difference* in arrests between Whites and Blacks.

<sup>10</sup> Thus,  $I_i$  is 1 for all observations involving Black offense rates,  $I_j$  is 1 for DUI or motor vehicle theft observations,  $I_k$  is 1 for New Jersey observations, and  $I_t$  is 1 for all observations in the post-scandal period.

estimates of the scandal's effect by subtracting the pre-post change for Whites from that of Blacks. This and subsequent specifications include a full set of year fixed effects to control for macroeconomic and demographic changes over time. Under the assumption that omitted factors had an equal influence on both White and Black arrest rates, the DD approach estimates the causal impact of the scandal. Motor vehicle theft and DUI arrest rates for Blacks both dropped substantially post-scandal, with estimated decreases of 37% and 15% respectively. These estimates are highly statistically significant.

Although the DD estimates provide evidence consistent with a large effect of the scandal on arrest rates, the effects could conceivably have arisen as a result of a general shift away from minority-targeted enforcement (for example, due to an increase in hiring of minority police officers) as opposed to the specific mechanism of the profiling scandal. Columns II and III attempt to control for such a possibility by including an additional difference between affected and unaffected crimes. In Column III all crimes are used as the control group, while in column II the broader set of property crimes is used as a control for motor vehicle theft and other public order arrests<sup>11</sup> are used as a control for DUI. The estimated effect of the scandal remains negative and significant, with the estimates for vehicle theft ranging between -20% and -32% and those for DUI ranging from -9% to -15%.

The final four columns of Table 1 exploit the fact because regulations and policies regarding police behavior are typically instituted at the local or state level, the scandal effects were likely concentrated within the state of New Jersey. Columns IV and VI use Connecticut as a reference group. Although smaller than New Jersey, Connecticut is geographically proximate and has a roughly comparable racial composition.<sup>12</sup> Columns V and VII use the entire United States as a comparison. In both cases, the estimated effects of the scandal arrests remain in the 17-40% range for vehicle theft. For DUI, using all crimes as a comparison in the fourth difference increases the estimated effects of the scandal from about 15% to 40%. The fourth differences are generally statistically significant.

One explanation for a smaller decrease in DUI arrests relative to theft arrests may be the "probable cause" rationale for police stops. Suspicion regarding motor vehicle theft may be based upon more subjective factors such as the type of car being driven and the age and race of the driver. Intoxicated drivers, in contrast, may be more likely to display clearly erratic driving behavior. Officers concerned about stopping minority drivers for fear of accusations of race-based policing might be more willing to pull over minorities exhibiting unsafe driving, since such behavior gives officers clear probable cause for a stop.

Table 2 examines the robustness of these results to alternative assumptions about the timing of the scandal. Broadening the scope of years or including 1998 and 1999 in either the pre or post-scandal period do not substantively alter the results. Estimating the pre- and post-scandal means using the Prais-Winsten method to allow for serially correlated error terms also has little effect on the estimates, although the estimated standard errors increase somewhat. Finally, vehicle thefts in the city of Newark may have been affected by the availability of the Lojack vehicle

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<sup>11</sup> DUI is considered to belong to a class of crimes known as public order crimes which includes among others prostitution, gambling, and liquor law violations.

<sup>12</sup> Delaware or Southeastern Pennsylvania might seem to be more natural control geographies; unfortunately the UCR data for these areas is incomplete.

recovery system.<sup>13</sup> Omitting Newark from the sample actually slightly increases the estimated effects of the scandal on arrests.

The final rows of Table 2 report placebo tests of the differences approach by re-estimating the original specification on assaults and burglaries using violent crime and property crimes as controls. Given that arrests for assault and burglary have no connection with traffic stops, these crimes should not be affected by changes in profiling. In contrast to the findings for vehicle theft and DUI, the point estimates for assault and burglary change sign depending on the control groups employed and are typically not statistically different from zero. These placebo findings strengthen the evidence that the baseline estimates indeed measure the causal effect of the profiling changes on arrest rates.

## V. Did Changing Arrest Rates Lead to More Crime?

In the previous section I demonstrated that there was a substantial decrease in arrests of Blacks relative to Whites associated with the occurrence of the scandal. The simple model presented implies that such a decrease of enforcement, which lessens the cost of committing crime, should lead to an increase in the amount of crime. Most policy discussions of racial profiling, however, have ignored the potential equilibrium effects on crime that might be expected from policies designed to combat profiling. In this section I focus attention on motor vehicle theft, a crime category that is closely associated with police stops and for which there is also available data on offenses.<sup>14</sup>

Figure 4 shows aggregate annual time series of motor vehicle thefts for New Jersey from 1994-2003. The bottom panels include similar time-series for burglaries and larcenies, the other two index property crimes, as a basis for comparison. All three index crimes demonstrated a steady downward trend from 1994-1998. While burglary and larceny continued their downward trend from 1998-2003, dropping by an average of about 4% per year, the steady downward decline in motor vehicle theft came to an abrupt halt in 1998. Between 1998 and 2003, motor vehicle theft remained almost constant in New Jersey. The timing of the change in vehicle theft offending corresponds to the large decrease in arrests brought about by the profiling scandal.

Examining higher frequency offending data supports the conclusion that the offending shift was tied to the drop in arrests. Figure 5 plots monthly moving averages of the number of vehicle thefts in New Jersey. The downward trend in vehicle thefts is apparent through 1996 and 1997 but then appears to halt in April 1998, the same month that the Turnpike shooting occurred and arrests of minorities fell substantially. Whereas thefts had fallen almost 25% between January 1996 and March 1998, they did not decline at all from April 1998 through December 2000.

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<sup>13</sup> See Ayres and Levitt (1998) for a discussion of the deterrent effect of Lojack.

<sup>14</sup> Just as changes in profiling have an asymmetric effect on arrest patterns, they are also likely to result in increases in only certain types of crime. Although drug and weapons offending may be responsive to the probability of detection due to police stops, because drug and weapons offenders are often captured through means other than vehicle stops, profiling changes likely exert a relatively small effect on the overall costs of committing these crimes. DUI may be also responsive to profiling, but there are no available crime data permitting an estimate of DUI offense rates independent of arrests. An additional advantage of focusing on vehicle theft is that because of insurance reporting requirements, it is likely that official offending data closely approximates the true incidence of theft.

To capture the intuition arising from Figure 4, consider the regression:

$$y_{it} = \gamma t + \sum_{k=1998}^{2003} \beta_k D_k + \delta_i + \varepsilon_{it} \quad (7)$$

The log offense rates for a crime in location  $i$  and time  $t$  ( $y_{it}$ ) are regressed on a time trend ( $t$ ) and separate dummy variables for the years 1998-2003 ( $D_k$ ). The coefficients on the dummies ( $\beta_k$ ) provide estimates of the degree to which the change in a particular crime rate was above or below the trend in a particular year. The left three columns of Table 3 report such regressions for the three index property crimes aggregated to the state level while the right three columns report estimates based upon county-level variation. Whereas changes in the two index crimes unrelated to profiling were below trend for all years from 1998-2003, motor vehicle thefts rose substantially relative to trend by 2003. The state estimates indicate that the reversal began in 1999 and that by 2003 motor vehicle theft was 30% higher than its predicted level based upon the pre-1998 trend. Although the county estimates suggest a later and smaller reversal in motor vehicle thefts, they provide evidence generally consistent with the state estimates.

The aggregate time series evidence, while suggestive, cannot conclusively demonstrate that the offending changes arose as a result of the policing adjustments following the scandal as opposed to other factors. With data on the racial distribution of offenders it would be possible to directly examine the extent to which minority offending increased after the scandal. Unfortunately, the UCR data do not provide information about the race of the offenders.<sup>15</sup> If the increase in motor vehicle thefts arose because of a decrease in scrutiny of Black motorists and offenders are drawn from the community in which the offense occurs, however, offending should shift towards areas populated primarily by Blacks. I can empirically examine whether this is the case using UCR data from individual police agencies within New Jersey.

This approach is limited in a number of important ways. If the shift in offending takes place within communities, so that White offenders in a particular community are replaced by Black offenders from the same community, my approach will not detect a change in the racial composition of offenders. If the assumption of within-community offending fails, agency-level demographic information does not provide useful information about the characteristics of offenders. Finally, the approach does not clearly distinguish between offenders and victims. An exogenous change that made it easier to victimize Black vehicle owners might appear to be a shift towards increased Black offending using this empirical approach.

I have attempted to match 494 police agencies in New Jersey to the demographic characteristics of the communities that they represent.<sup>16</sup> If the departure of motor vehicle thefts from trend is truly driven by changes in racial profiling, one would expect a larger increase relative to trend in areas with a higher proportion of Black residents. To test this formally, consider regressions of the form:

$$\beta_{ik} = \alpha_k \pi_i + \Lambda X_i + e_{ik} \quad (8)$$

<sup>15</sup> Because few property crimes are directly observed by victims and many go unsolved, such data will likely be unavailable even as crime data reporting is improved.

<sup>16</sup> Details of the construction of the matched data set can be found in the Appendix.

where  $\beta_{ik}$  represent the departure from trend in year  $k$  and location  $i$  as in equation (7),  $\pi_i$  is the proportion of the population that is Black, and  $X_i$  represents a vector of location-specific controls. The coefficients on percent Black,  $\alpha_k$ , provide reduced-form estimates of the extent to which crime increased or decreased more in areas with a higher proportion of Blacks. In my implementation, I first estimate (7) separately by agency to obtain  $\hat{\beta}_{ik}$  and then perform yearly cross-sectional regressions following (8) to estimate  $\alpha_k$ .<sup>17</sup>

Table 4 reports the relationship between percent Black and departure from trend for motor vehicle thefts, burglary, and larceny. After 1998 the coefficients on percent Black for vehicle theft are positive and significant. The magnitude of the estimated coefficients suggest that a community at the 75% percentile of the percent Black distribution (20% Black) in New Jersey would have a vehicle theft rate 17% higher in 2003 than a community at the 25% percentile (1.8% Black). For burglary and larceny, however, the estimated coefficients are negative, suggesting that for these types of crime offending was moving away from communities with large proportions of Blacks.

With an additional assumption regarding the propensity to commit crime across races, it is possible to structurally estimate the number of Black and White offenders in each year and directly examine the extent to which offending patterns changed following the profiling reforms. For example, assuming that Blacks and Whites have an equal propensity for committing vehicle theft, in an agency with 20% of the population who are Black in which 100 vehicle thefts occurred in a particular year, we could impute 20 of the thefts to Black offenders and 80 of the thefts to White offenders. If Blacks are twice as likely as Whites to commit vehicle theft, then we would impute 33 offenses to Blacks and 67 offenses to Whites. Summing these imputed offender characteristics across all agencies in New Jersey provides estimates of the statewide offending patterns. In the analysis that follows I use a propensity factor of 2.43, which is the propensity implied by the fact that approximately 50% of those arrested for vehicle theft in New Jersey are Black while Blacks on average account for 30% of the population in jurisdictions experiencing vehicle thefts.<sup>18</sup>

The results of the estimation exercise are presented in Figure 6. The top panel demonstrates that after dropping fairly steadily in the pre-scandal period, the imputed percentage of Black vehicle theft offenders abruptly reverses direction in 1998, increasing by three percentage points over the next four years. No such pattern emerges using the same imputation method for the two other index property crimes.<sup>19</sup> For burglary and larceny, the estimated percent Black offenders declines steadily until 2002. The timing of the shift in offending patterns and the fact that this shift was confined to vehicle theft but not other crimes unaffected by profiling strongly suggests that the changes in motor vehicle offense rates were linked to post-scandal adjustments in arrest patterns.

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<sup>17</sup> Saxonhouse (1976) points out that efficient estimation with estimated regressands requires weighting the second-stage regressions using the variances calculated in the first stage, an approach I adopt.

<sup>18</sup> Using a propensity of 1 will scale down the estimates reported in Figure 5 but will not qualitatively alter the results. One could also estimate area-specific propensities using the agency-level arrest data; doing so does not substantively alter the results. More details regarding the calculation of the propensities can be found in the data appendix.

<sup>19</sup> In the control state of Connecticut, the trends in minority offending computed using the same methods also suggest no divergence between minority offending in vehicle theft and other property crimes.

The imputation procedure provides an estimate of the total number of vehicle thefts committed by Blacks and Whites each year. Employing the same quadruple-differences approach as in Section III, it is possible to estimate the effect of the scandal on offending rates. Table 5 reports these estimates. There is a small estimated increase in Black motor vehicle thefts relative to Whites in the post-scandal period consistent with the previous evidence of a post-scandal increase in the percent Black committing this crime. The DDD estimates are statistically different from zero. The imprecision of some of the difference estimates may reflect the limitations of the imputation approach employed as opposed to the absence of a real effect. Because the approach is sensitive to only across-jurisdiction changes in the racial composition of offenders, the reported coefficients likely represent a low estimate of the true effect of the scandal.

Combining the estimated effects of the scandal on arrest rates and offense rates generates estimates of the elasticity of offending with respect to arrests. Using the time-series estimates in Table 3, and assuming as an extreme that the entire time series change is attributable to increased offending by Blacks and that Blacks account for 50% of all vehicle thefts, the implied elasticity ranges between -1.04 and -2.45.<sup>20</sup> Using the difference estimates, which likely provide a low estimate of the elasticity, yields estimated elasticities between -.20 and -.57.

If changes in profiling-related police scrutiny have similar behavioral effects to changes in police presence<sup>21</sup>, these estimates can be compared with past estimates of the elasticity of offending with respect to police. Levitt (2002) uses firefighters as an instrument for police and estimates an auto theft elasticity of -1.70, a magnitude comparable to my upper estimates. Di Tella and Schargrodsky (2004) and Klick and Tabbarok (2005) estimate elasticities using changes in police patrols due to heightened fears of terrorism and report elasticities of -.33 to -.17 and -.86 respectively. Corman and Mocan (2000) use the fact the police-responses to crime waves occur with a lags to calculate an elasticity of -.37 to -.40. My lower estimates of -.20 to -.57 are roughly similar to these other estimated elasticities.

## **VI. Profiling Evidence from Maryland**

If the reduction in arrests and increase in offenses documented in the previous section were caused by anti-profiling measures, such patterns should be observable in other states that experienced changes in profiling. Unfortunately, few jurisdictions experienced scandals that appeared with the immediacy and magnitude of the New Jersey scandal, making it potentially difficult to isolate the effects of profiling changes from other intervening factors. In this section I examine evidence from Maryland, which experienced a significant profiling controversy in the mid-1990's. Although the Maryland controversy generated less press attention than the New Jersey scandal, it was important in that it represented one of the earliest examples of legal scrutiny of the profiling practices of a large police organization. The motor vehicle arrest and

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<sup>20</sup> Averaging the state level coefficients from Table 3 for 1999-2003 gives an estimated effect on offenses of .1664. These estimates should be multiplied by 1/4 and then divided by the estimated change in arrests, which range from .17 to .40 depending on the reference crime group.

<sup>21</sup> This would be true if there are constant returns to police at observed levels of crime and policing.

offense patterns in Maryland are strongly consistent with the conclusions regarding the effects of profiling presented previously.<sup>22</sup>

The impetus for the profiling changes in Maryland was a court case filed against the state police in 1993 by Robert Wilkins, an African-American attorney who was stopped and forced to submit to a drug search while driving with his family on US-68. During the case internal police documents were uncovered revealing that the police had developed a drug courier profile that encouraged officers to stop Black males driving expensive vehicles. Faced with this damaging evidence, the state chose in 1995 to settle the case and as part of the settlement agreed to end profiling and develop a system for collecting data on the race stopped motorists, with results reported to the Maryland ACLU.

The top panel of Table 6 reports difference estimates of the effect of Maryland's profiling changes on motor vehicle theft arrests. These estimates are the analogs of those reported for New Jersey in Table 1; the procedure is the same except the scandal date has been adjusted to 1995 and North Carolina is used as a comparison group instead of Connecticut. The estimates of an approximately 30% decrease in arrests due to profiling changes are comparable to the 17-40% estimated decreases in New Jersey and are statistically significant. The second row reports difference estimates of the increase in offending in Maryland equivalent to those reported for New Jersey in Table 5. As with New Jersey, the increases in offenses are imprecisely measured but suggest an increase in elasticity of offending with respect to arrests of approximately -.5. Figure 7 reports the percentage Black of all offenders for the three index property crimes estimated using the imputation approach described previously; the percentages are reported relative to 1990 to allow comparison across crime types. For all types of property crime the percentage of Black offenders has increased over time in Maryland. Whereas the increase in Black offending for vehicle thefts closely follows the other property crimes prior to 1995, beginning in the year of the scandal and enforcement changes Black vehicle thefts began rising at a faster rate. By 2003, the increase in Black offending for vehicle thefts was more than twice that of the other two index property crimes. Despite its occurrence several years prior to the New Jersey scandal, Maryland's scandal brought about changes in arrests and offending of comparable sign and magnitude. This evidence suggests that the estimates from New Jersey may be more broadly applicable to other jurisdictions enacting measures to reduce profiling.

## **VII. Did Changes in Profiling Affect Other Driving Behaviors That Might Be Sensitive to Police Stops?**

Although the most obvious way in which citizens might respond to a less police scrutiny is by increasing offending, there are other dimensions of public behavior that may be affected by police stops. Most police stops occur for minor traffic violations; citizens who believe that they are less likely to be stopped by police might respond by driving recklessly or engaging in other unsafe driving practices.

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<sup>22</sup> I focus on motor vehicle theft as opposed to DUI because Maryland introduced a number of new community crime initiatives from 1995-1997 that dramatically increased enforcement of quality-of-life crimes, including DUI. In Baltimore, for example, public order arrests increased by over 50% between 1993 and 1998.

To examine changes in driving behavior before and after the scandal, I utilize data from the New Jersey Department of Transportation (NJDOT) accident records database. The data set includes records on the universe of car crashes occurring on roads in New Jersey during the years 1997-2004, a total of almost 3 million accidents. Each record contains information about the severity of the crash, type of vehicles involved, crash location, and driver characteristics. Although driver race is not directly reported, the zip code of the driver's residence is included in each record, permitting a test of the extent to which accidents shifted towards drivers from areas with predominantly minority residents. Such a shift would be consistent with an environment in which minorities responded to decreased police scrutiny by driving more recklessly.

Table 7 reports triple-differences estimates of the extent to which three different types of reckless driving behavior increased by Blacks following the scandal. The behaviors examined are speeding, driving while intoxicated, and carrying children without using the legally required safety equipment.<sup>23</sup> I estimate the total number of incidents of reckless behavior by race using matched demographic information for each zip code. For example, in 2000 there were 83 drivers involved in accidents in which speeding was the main factor who resided in the zip code 07083 (Union, NJ). This zip code has 15.5% percent Black residents, so I would impute 13 accidents to Black drivers and 70 to White drivers. I limit that analysis to only drivers whose residence is within New Jersey, which covers 582 zip codes. As with the offense data, this approach will only capture across-community as opposed to within-community changes, and the other caveats mentioned in Section IV apply here as well.

The differences estimates employ contrasts by race (Black vs. White) and time period (post-scandal versus pre-scandal). As an additional difference I compare the incidents of accidents associated with the reckless behavior to accidents caused by driver inattention. Accidents due to driver inattention account for about 22% of the accidents in the data and presumably would not be affected by changes in profiling. Including this additional difference helps to more fully control for race-specific trends in miles driven or vehicle safety features that may have occurred between 1997 and 2004.

The first row of the table analyzes all accidents. The other rows of the table limit the analysis to circumstances in which citizens might have reasonably expected larger effects of profiling. The second row focuses on accidents reported by the New Jersey State Police. Because news reports of the scandal focused primarily on actions by the state police, minority drivers may have believed it particularly unlikely that they would be stopped on roads patrolled by state troopers following the scandal. Grogger and Ridgeway (2005) suggest that racial profiling is less likely a factor in nighttime driving because it is difficult for officers to identify the race of drivers under

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<sup>23</sup> I measure speeding using accidents for which excessive speed was reported as the primary contributing factor. Intoxicated drivers are drivers who failed or refused an alcohol test. There is some evidence that this may be an imperfect proxy for alcohol use while driving due to selective reporting of testing by police. In later years the crash data provides a separate indicator of alcohol involvement; this indicator suggests more than twice as many crashes involving alcohol as the number indicated by the testing data. In addition, the testing data suggest 2.3% of total accidents involve alcohol in New Jersey, which is substantially lower than the rates of 3.5-7% found in recent DOT reports from New York, West Virginia, Ohio, and Minnesota. Vehicles with children aged 1-4 with no safety restraint and children aged 1 without any restraint other than a child safety seat were classified as carrying children unsafely.

low-light conditions. The final row of the table thus limits the analysis to accidents occurring between 8 a.m. and 4 p.m., when profiling may be more likely.

Columns 1 and 2 of the table indicate that for speeding and intoxicated driving, there appears to be little effect of the scandal on behavior, with most point estimates actually suggesting a shift away from Black offending for these violations, although the estimates are close to zero. For carrying children unsafely, however, the point estimates are positive and larger, suggesting that these behaviors increased by 6-14% following the scandal. The estimates for child carrying are quite imprecise, reflecting the fact that it is fairly uncommon for young children who are improperly restrained to be involved in car accidents. Limiting the analysis to state police and daytime cases does not substantially alter the basic results.

Although the imprecision of the estimates make drawing concrete inferences from these data problematic, the general pattern appears consistent with the previous findings regarding vehicle theft. Speeding and intoxicated driving are likely to be outwardly observable to police officers while driving. Because these offenses by their nature can generate probable cause for a stop, their detection may change little with changes in profiling. Motor vehicle theft and carrying children with improper restraints, in contrast, are typically only observable by officers following a stop and examination of a vehicle. Profiling training which discourages officers from making stops without concrete suspicion of a particular violation could have a larger effect on detection of these types of less easily observed violations.

## **VIII. Conclusion**

This paper provides some of the first empirical estimates of the effects of anti-profiling measures on arrest rates, offending, and other behaviors. From a policymaker's perspective, an ideal evaluation might randomly assign different types of anti-profiling measures (video surveillance of police stops, sensitivity training, etc.) to agencies to evaluate the effects of each particular program on police and citizen behavior. This study cannot provide such clean estimates of the effects of profiling changes. Because a number of different anti-profiling measures were implemented simultaneously in New Jersey, it is difficult to disentangle the effects of any particular measure on arrests and offending. Related to this limitation, because other jurisdictions typically have not implemented anti-profiling programs with the extensiveness and immediacy of New Jersey, the findings of this study should only be applied to other agencies with extreme circumspection, although evidence presented from Maryland supports the primary conclusions. At the same time, the abruptness of New Jersey's changes makes it difficult to reconcile the observed shifts in arrests and offending with causal mechanisms other than changes in profiling. It appears that official and public scrutiny of profiling behavior by police can lead to substantial reductions in arrests of minorities, although this enforcement reduction may carry the unintended consequence of encouraging crime in minority areas.

The New Jersey experience points to several possible lessons for policymakers. As noted previously, the occurrence of the profiling scandal itself led to an almost immediate reduction in arrests (and presumably profiling behavior) even prior to the institution of new training programs or official procedures. Given proper incentives and oversight officers appear to be able to

effectively self-regulate. Encouragingly, despite the fact that public discussion of profiling in New Jersey has lessened over time, the apparent reduction in profiling following the scandal appears to have persisted through the most recent available data. One fruitful area for progress for policymakers might be the design of programs that can reduce profiling while avoiding the offending increases that could result from lessened police scrutiny of some groups.

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**Table 1: Differences Estimates of the Effect of the Profiling Scandal on Arrest Rates**

Crime Type	Estimation Approach						
	DD (I)	DDD		(IV)	DDDD		
	(II)	(III)	(V)	(VI)	(VII)		
Motor Vehicle Theft	-0.371*** (0.0537)	-0.195** (0.0934)	-0.317*** (0.119)	-0.171 (0.154)	-0.352* (0.187)	-0.264** (0.115)	-0.401*** (0.144)
DUI	-0.145*** (0.0153)	-0.153*** (0.037)	-0.092*** (0.0211)	-0.149* (0.0856)	-0.099 (0.0685)	-0.455*** (0.129)	-0.376*** (0.119)
No. Obs.	20	40	40	80	80	80	80
First Difference	Post-Pre	Post-Pre		Post-Pre			
Second Difference:	Black-White	Black-White		Black-White			
Third Difference:		Affected Crime- Property/Public Order	Affected Crime- All Crimes	Affected Crime- Property/Public Order		Affected Crime-All Crimes	
Fourth Difference:				New Jersey- Connecticut	New Jersey- United States	New Jersey- Connecticut	New Jersey- United States

Note: The first row of the table reports differences estimates of the effect of a racial profiling scandal on arrest rates. The dependent variable is the log arrest rate for a particular year, racial group, crime type, and state. The included years are 1994-2003. The affected groups are the years 1999-2003, Blacks, DUI or motor vehicle theft, and New Jersey, and North Carolina is used as a geographic control. All regressions include year fixed effects. Standard errors adjusted for heteroskedasticity are reported in parenthesis. \* indicates significance at the 10% level, \*\* the 5% level, and \*\*\* the 1% level.

**Table 2: Robustness Checks of Estimates of the Effect of the Profiling Scandal on Arrest Rates**

Crime Type	Specification	Estimation Approach						
		DD (I)	DDD (II) (III)		DDDD (IV) (V) (VI) (VII)			
Motor Vehicle Theft	Use years 1990-2003	-0.441*** (0.0467)	-0.256*** (0.0866)	-0.402*** (0.116)	-0.248* (0.135)	-0.437*** (0.167)	-0.307*** (0.111)	-0.461*** (0.14)
	Include 1999 in pre-period	-0.3*** (0.0829)	-0.121 (0.109)	-0.249* (0.138)	-0.042 (0.152)	-0.217 (0.19)	-0.148 (0.125)	-0.285* (0.158)
	Include 1998 in post period	-0.38*** (0.049)	-0.208*** (0.0762)	-0.328*** (0.0889)	-0.119 (0.15)	-0.307* (0.174)	-0.26*** (0.1)	-0.389*** (0.118)
	Allow for AR(1) errors	-0.381*** (0.0463)	-0.183* (0.107)	-0.282** (0.135)	-0.195 (0.194)	-0.36 (0.246)	-0.274* (0.148)	-0.379** (0.179)
	Omit Newark	-0.411*** (0.0534)	-0.216** (0.0943)	-0.363*** (0.117)	-0.193 (0.153)	-0.392** (0.184)	-0.285** (0.116)	-0.438*** (0.144)
DUI	Use years 1995-2002	-0.176*** (0.0223)	-0.213*** (0.075)	-0.138*** (0.0511)	-0.513*** (0.147)	-0.424*** (0.121)	-0.241** (0.115)	-0.168* (0.089)
	Include 1999 in pre-period	-0.138*** (0.0223)	-0.149*** (0.0396)	-0.087*** (0.0256)	-0.401*** (0.127)	-0.317** (0.126)	-0.153* (0.0836)	-0.101 (0.0666)
	Include 1998 in post period	-0.121*** (0.0281)	-0.13*** (0.0387)	-0.07** (0.0288)	-0.427*** (0.131)	-0.35*** (0.122)	-0.117 (0.0879)	-0.074 (0.0746)
	Allow for AR(1) errors	-0.119*** (0.021)	-0.14*** (0.045)	-0.092*** (0.0203)	-0.451*** (0.15)	-0.375*** (0.142)	-0.143 (0.106)	-0.098 (0.0777)
Assault/Violent	Original	-0.205*** (0.0382)	0.238*** (0.0738)	-0.151*** (0.0513)	-0.357* (0.19)	-0.191* (0.101)	-0.08 (0.118)	-0.115 (0.076)
Burglary/Property	Original	-0.123*** (0.0283)	0.082 (0.0546)	-0.069 (0.0552)	0.11 (0.105)	-0.072 (0.127)	0.049 (0.0751)	-0.101 (0.0909)
	<b>First Difference</b>	Post-Pre	Post-Pre		Post-Pre			
	<b>Second Difference:</b>	Black-White	Black-White		Black-White			
	<b>Third Difference:</b>		Affected Crime- Property/ Public Order	Affected Crime-All Crimes	Affected Crime- Property/Public Order		Affected Crime-All Crimes	
	<b>Fourth Difference:</b>				NJ-CT	NJ-US	NJ-CT	NJ-US

Note: See notes for Table 1. The Assault/Violent and Burglary/Property rows respectively use aggravated assault as the affected crime with other violent crimes as a control and burglary as the affected crime with other property crimes as a control.

**Table 3: Time-Series Estimates of Changes in Index Property Crimes**

Expalnatory Variable	State-Level			County-Level		
	Vehicle Theft	Larceny	Burglary	Vehicle Theft	Larceny	Burglary
Trend (1990-1997)	-0.091*** (0.00629)	-0.031*** (0.00478)	-0.046*** (0.00721)	-0.077*** (0.00656)	-0.031*** (0.00494)	-0.048*** (0.00575)
1998 Relative to Trend	-0.089*** (0.0256)	-0.069** (0.0279)	-0.102*** (0.0359)	-0.069** (0.0303)	-0.074*** (0.0142)	-0.086*** (0.0213)
1999 Relative to Trend	-0.002 (0.0312)	-0.108*** (0.0319)	-0.211*** (0.0424)	-0.049 (0.0375)	-0.111*** (0.0199)	-0.186*** (0.0393)
2000 Relative to Trend	0.046 (0.037)	-0.121*** (0.0362)	-0.242*** (0.0491)	-0.031 (0.0527)	-0.118*** (0.0274)	-0.205*** (0.048)
2001 Relative to Trend	0.226*** (0.0429)	-0.097** (0.0405)	-0.142** (0.056)	0.136** (0.0601)	-0.09** (0.0351)	-0.109* (0.057)
2002 Relative to Trend	0.256*** (0.0489)	-0.128*** (0.045)	-0.167*** (0.0629)	0.151** (0.0672)	-0.121*** (0.0361)	-0.124** (0.0586)
2003 Relative to Trend	0.306*** (0.055)	-0.144*** (0.0495)	-0.14** (0.0699)	0.16** (0.0733)	-0.137*** (0.0404)	-0.117** (0.0553)
N	14			294		

Note: This table reports coefficient estimates from regressions of log yearly offense rates for the listed crimes on a time trend and indicator variables for each of the years 1998-2003. The county level regressions are population-weighted. Heteroskedasticity-robust standard errors for the state-level regressions and standard errors clustered on county for the county-level regressions are reported in parenthesis. Positive point estimates are highlighted in grey. \* indicates significance at the 10% level, \*\* the 5% level, and \*\*\* the 1% level.

**Table 4: Cross-Sectional Estimates of the Relationship Between Deviation from Trend and Community Racial Composition**

Dependent Variable	Coefficient on Percent Black for:		
	Vehicle Theft	Larceny	Burglary
1998 Relative to Trend	-0.157** (0.075)	-0.261*** (0.0555)	-0.197*** (0.0414)
1999 Relative to Trend	0.267*** (0.0895)	-0.093 (0.0681)	-0.107* (0.0545)
2000 Relative to Trend	0.322*** (0.124)	-0.277*** (0.0777)	-0.249*** (0.07)
2001 Relative to Trend	0.371*** (0.132)	-0.407*** (0.1)	-0.458*** (0.0687)
2002 Relative to Trend	0.583*** (0.168)	-0.571*** (0.0938)	-0.46*** (0.0863)
2003 Relative to Trend	0.94*** (0.148)	-0.398*** (0.109)	-0.15 (0.103)

Note: This table reports estimates of the degree to which offending shifted towards areas in New Jersey with a higher proportion of the population who were Black in 1998-2003. The unit of observation is a police agency. In the first stage, separate regressions of the log offense rate on a yearly trend from 1990-1997 and indicator variables for 1998 to 2003 were performed for each agency in the sample. The estimated coefficients on the indicators were then regressed on the proportion of the agency's population that is Black in a second stage. Each entry in the table reports a coefficient on percent Black from a separate second stage regression. Log population was included as an additional control in the second stage regressions, and these regressions were population weighted and weighted using the estimated standard errors from the first stage following Saxonhouse (1976). A total of 494 police agencies were included in the sample. Population data are from the 2000 Census. Standard errors are reported in parenthesis. Positive point estimates are highlighted in grey. \* indicates significance at the 10% level, \*\* the 5% level, and \*\*\* the 1% level.

**Table 5: Differences Estimates of the Effect of the Profiling Scandal on Offense Rates**

Crime Type	Estimation Approach				
	DD (I)	DDD (II) (III)		DDDD (IV) (V)	
Motor Vehicle Theft	0.036 (0.0231)	0.123** (0.0566)	0.11** (0.0508)	0.069 (0.096)	0.04 (0.0816)
No. Obs.	20	40	40	80	80
First Difference	Post-Pre	Post-Pre		Post-Pre	
Second Difference:	Black-White	Black-White		Black-White	
Third Difference:		Affected Crime- Property	Affected Crime-All Crimes	Affected Crime- Property	Affected Crime-All Crimes
Fourth Difference:				NJ-CT	NJ-CT

Note: This table reports differences estimates of the effect of a racial profiling scandal on offense rates. The dependent variable is the log offense rate for a particular year, racial group, crime type, and state. Race-specific offense counts were imputed using agency-level population and offense data. The included years are 1994-2003. The affected groups are the years 1999-2003, Blacks, motor vehicle theft, and New Jersey. All regressions include year fixed effects. Standard errors adjusted for heteroskedasticity are reported in parenthesis. \* indicates significance at the 10% level, \*\* the 5% level, and \*\*\* the 1% level.

**Table 6: Estimated Effects of Maryland Scandal on Arrest and Offense Rates**

Dep. Variable	Estimation Approach						
	DD (I)	DDD		DDDD			
	(II)	(III)	(IV)	(V)	(VI)	(VII)	
Vehicle Theft Arrests	-0.189*** (0.0567)	-0.134* (0.0789)	-0.326*** (0.092)	-0.353*** (0.111)	-0.449*** (0.116)	-0.142 (0.102)	-0.326*** (0.121)
Vehicle Theft Offenses	0.152** (0.0698)	0.152 (0.105)	0.171* (0.0947)	0.123 (0.119)	0.127 (0.108)		
First Difference	Post-Pre	Post-Pre		Post-Pre			
Second Difference:	Black-White	Black-White		Black-White			
Third Difference:		Affected Crime- Property/Public Order	Affected Crime-All Crimes	Affected Crime- Property/Public Order		Affected Crime-All Crimes	
Fourth Difference:				MD-NC	MD-US	MD-NC	MD-US

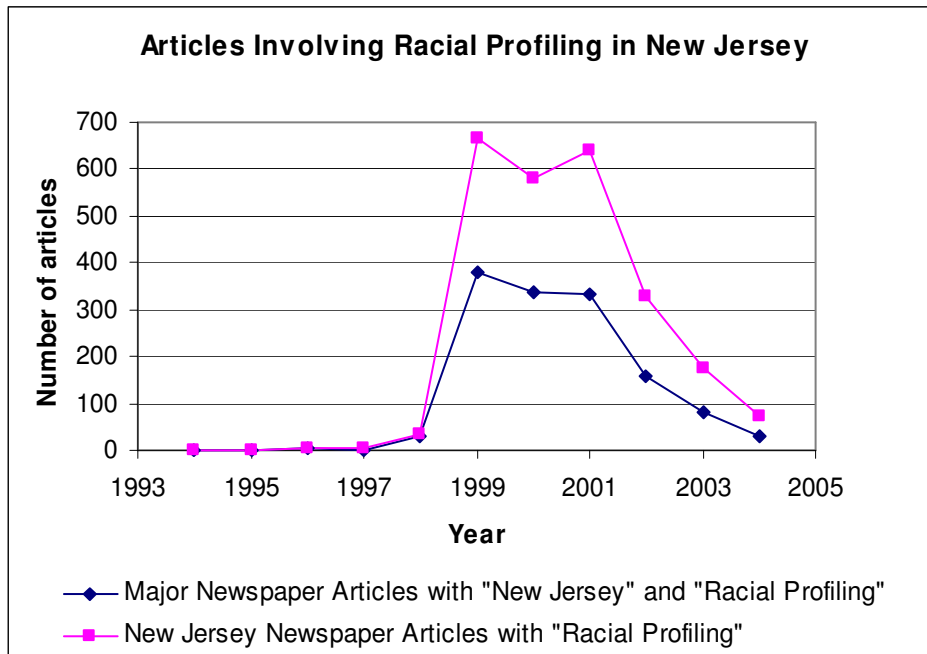
Note: The first row of the table reports differences estimates of the effect of a racial profiling scandal on arrest rates in Maryland, and corresponds to the New Jersey estimates in Table 1. The dependent variable is the log arrest rate for a particular year, racial group, crime type, and state. The included years are 1990-2003. The affected groups are the years 1995-2003, Blacks, motor vehicle theft, and Maryland. The second row reports differences estimates of the effect of a racial profiling scandal on offense rates and corresponds to the New Jersey estimates in Table 5. The dependent variable is the log offense rate for a particular year, racial group, crime type, and state. Race-specific offense counts were imputed using agency-level population and offense data, with data being drawn from 131 police agencies in Maryland and 472 agencies in North Carolina. All regressions include year fixed effects. Standard errors adjusted for heteroskedasticity are reported in parenthesis. \* indicates significance at the 10% level, \*\* the 5% level, and \*\*\* the 1% level.

**Table 7: Estimated Effects of the Scandal on Unsafe Driving Behaviors**

<b>Accident Specification</b>	<b>Violation Type</b>		
	<b>Speeding</b>	<b>Intoxicated Driving</b>	<b>Unsafe Child</b>
All accidents	-0.044 (0.0601)	-0.033 (0.054)	0.147 (0.327)
Accidents reported by state police	0.011 (0.161)	-0.061 (0.127)	0.064 (0.2)
Daytime accidents	-0.034 (0.0604)	-0.005 (0.0965)	0.088 (0.313)

Note: This table reports triple-difference estimates of the effect of the New Jersey racial profiling scandal on unsafe driving behaviors by Black drivers. Data is drawn from the NJDOT crash records database covering 1997-2004. The dependent variable is the log annual number of New Jersey drivers involved in an accident of the specified type. The race of drivers is imputed using demographics matched on driver zip code of residence. Each entry in the table represents a separate triple-difference estimate. The first difference is post-scandal (1999-2004) minus pre-scandal, the second difference is Black minus White, and the third difference is accidents associated with a particular violation minus accidents due to driver inattention. The treated group is Blacks involved in violation-related accidents post-scandal. Each regression contains 32 observations and the regressions control for year fixed effects. The second row of the table uses only accidents reported by the state police and the third row uses only accidents occurring between 8 a.m. and 4 p.m. Heteroskedasticity-robust standard errors are reported in parenthesis; none of the estimates is statistically different from zero at the 10% level.

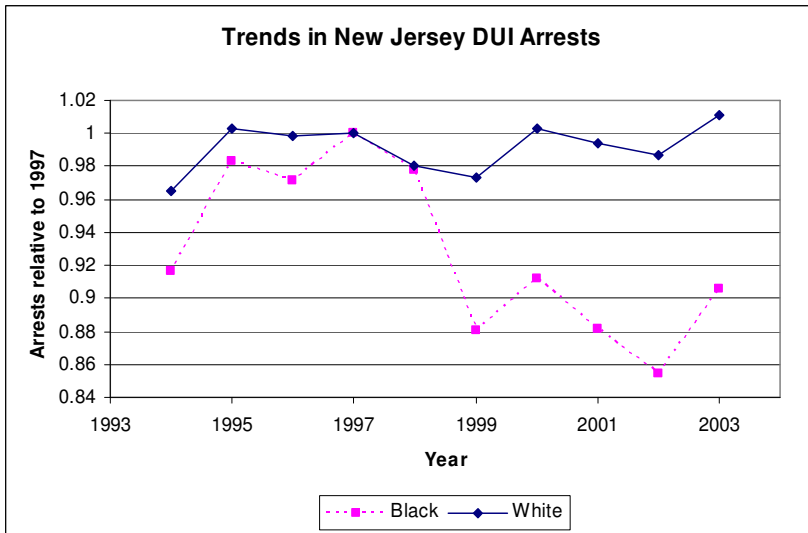
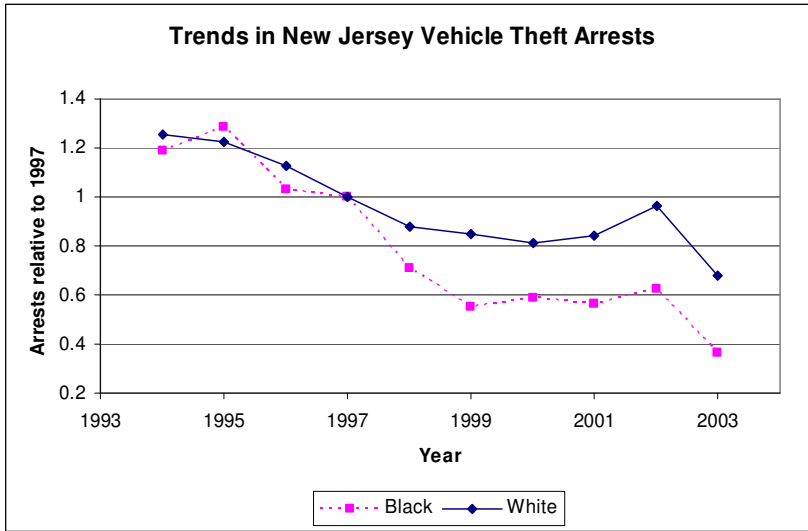
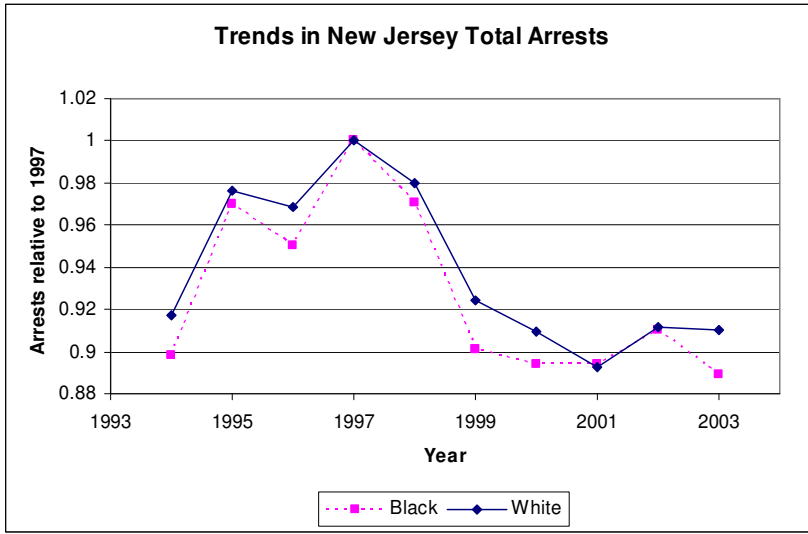
**Figure 1: Articles on Racial Profiling in New Jersey**



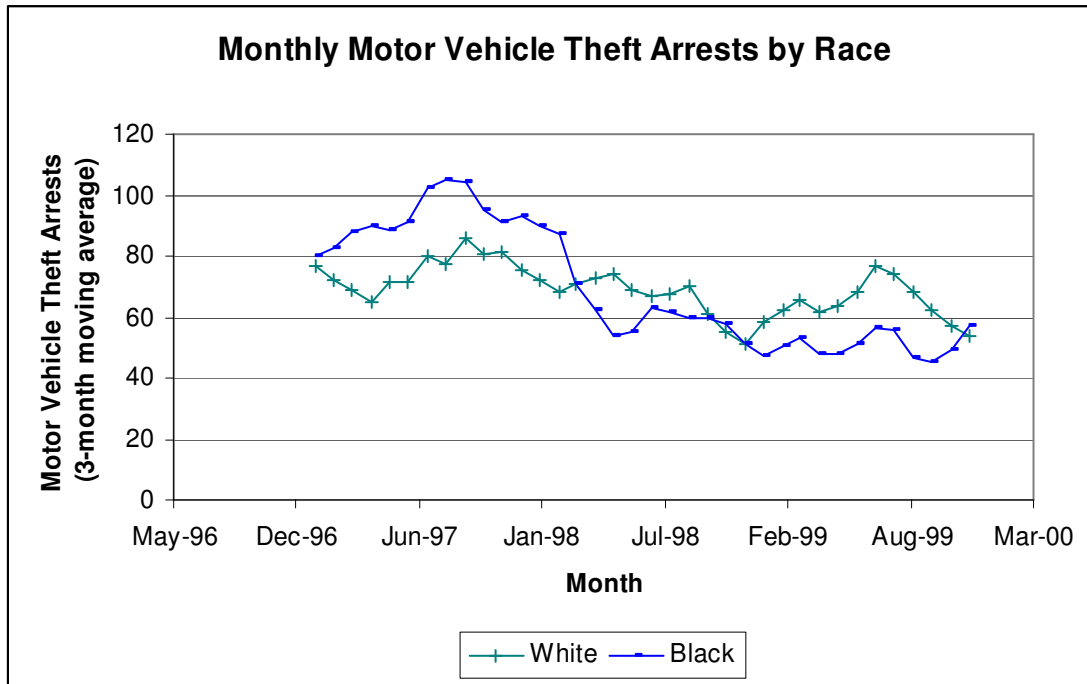
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Source: Lexis-Nexis

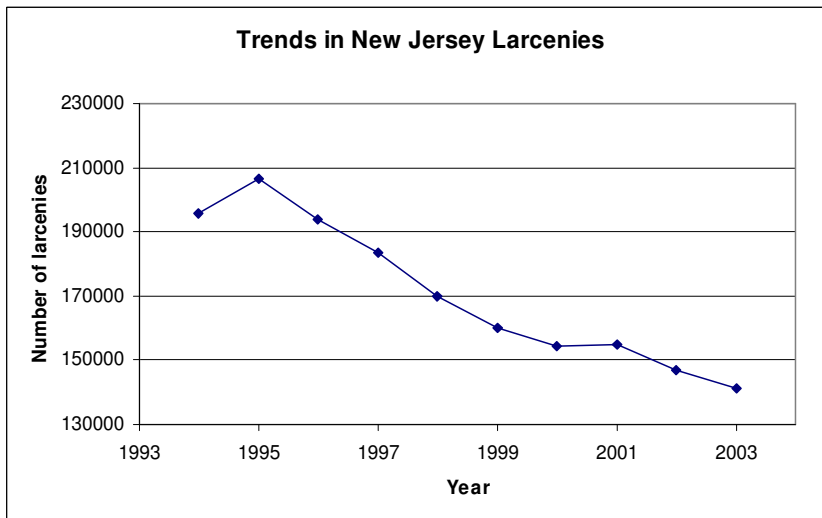
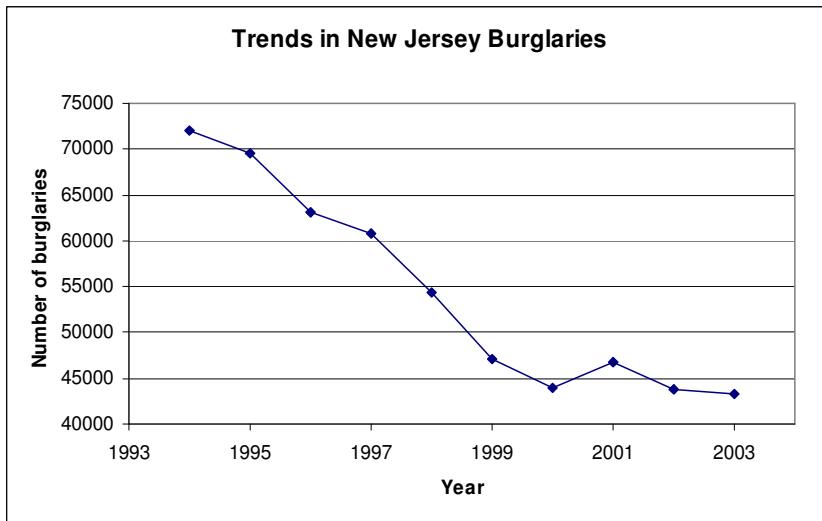
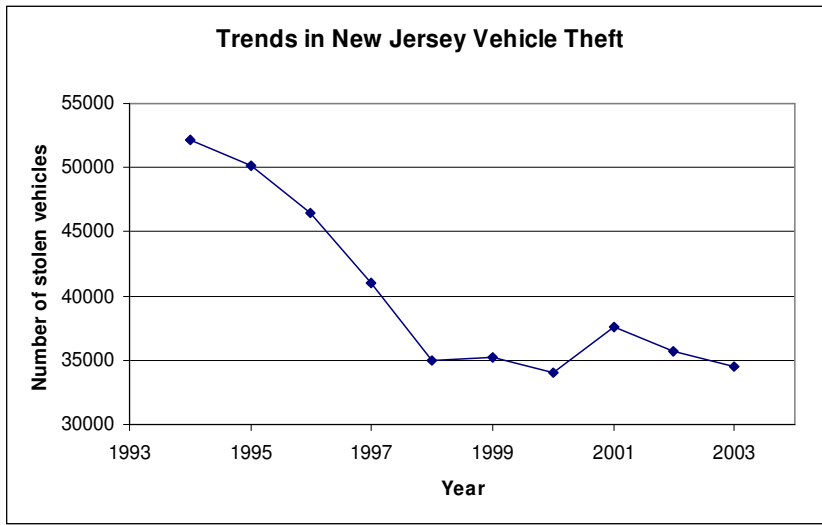
**Figure 2: New Jersey Arrests by Race, Relative to 1997**



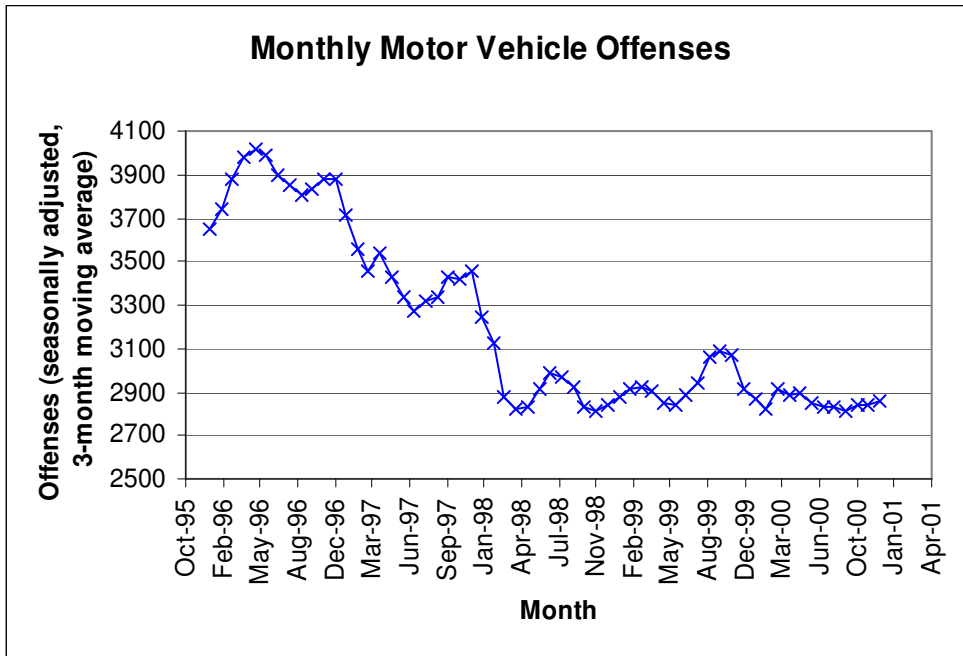
**Figure 3: Monthly Motor Vehicle Theft Arrests by Race**



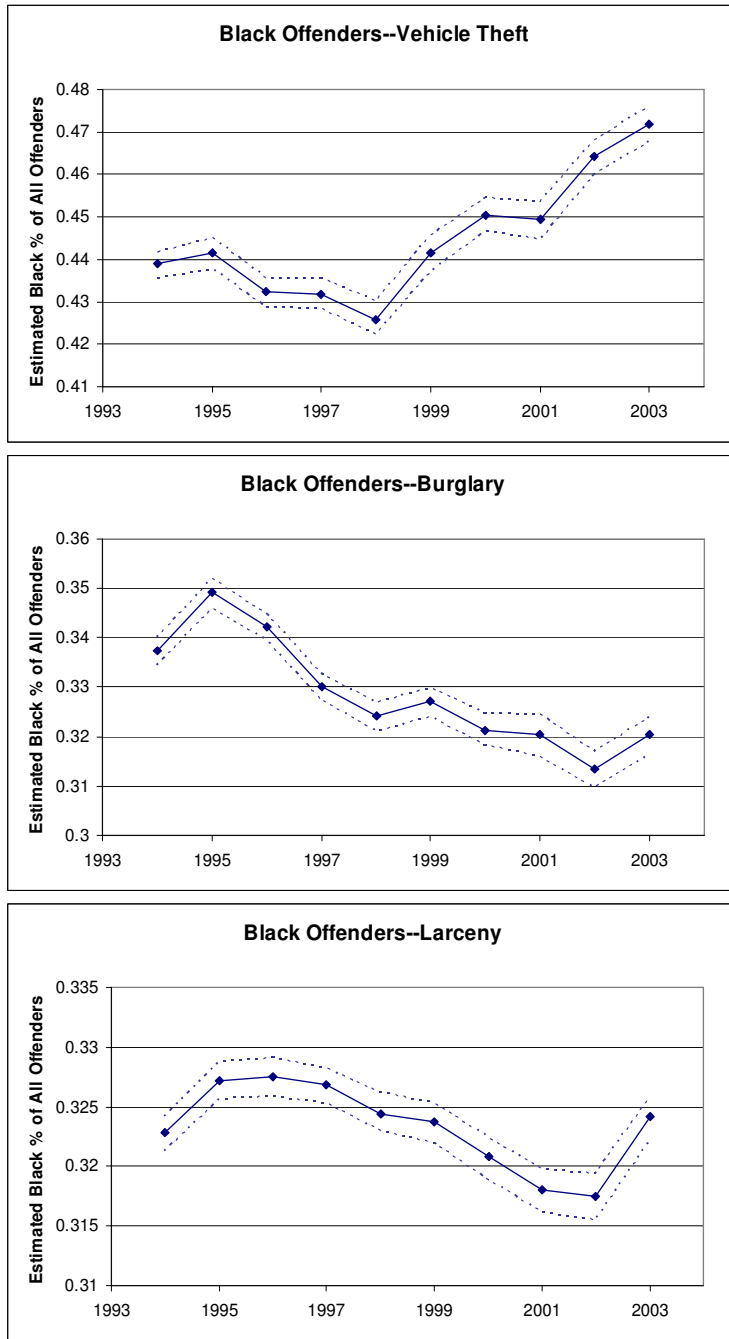
**Figure 4: Changes in New Jersey Index Property Crimes, 1991-2003**



**Figure 5: Motor Vehicle Theft Offenses in New Jersey by Month**

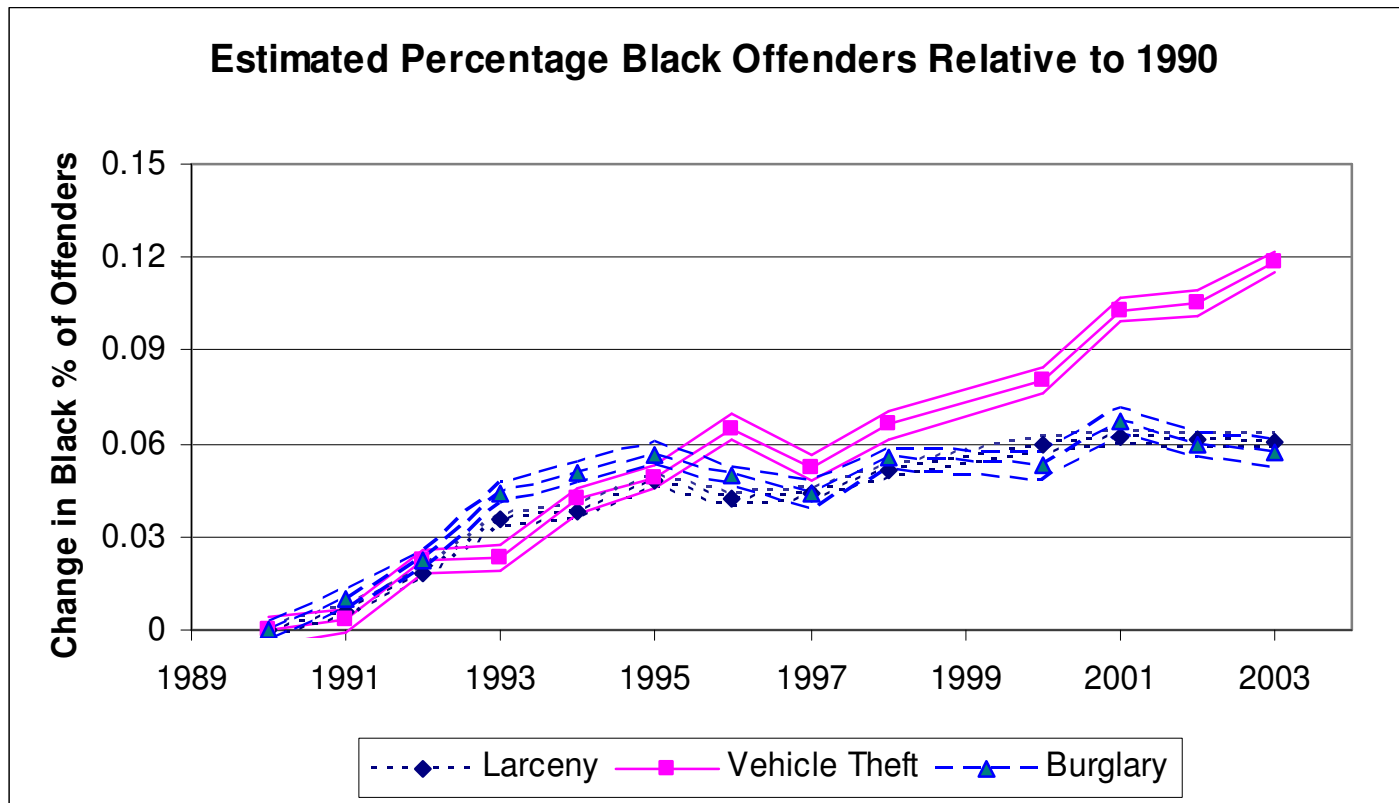


**Figure 6: Estimated Race of Offenders for Index Property Crimes—New Jersey**



Note: These figures plot the estimated percentage of index property crimes committed by Blacks in New Jersey. The estimates assume that the demographic characteristics of the population covered by each police agency in New Jersey represent the characteristics of the offender population and that arrest rates reflect the relative propensity of individuals to commit crime by race. Data from 494 separate agencies is used to construct the estimates. The dotted lines represent 95% confidence bands generated via the bootstrap.

**Figure 7: Changes in Black Offending for Property Crimes in Maryland**



Note: These figures plot the estimated percentage of index property crimes committed by Blacks in Maryland relative to 1990. The estimates assume that the demographic characteristics of the population covered by each police agency in Maryland represent the characteristics of the offender population and that Blacks and Whites have an equal propensity for committing crime. Data from 131 separate agencies is used to construct the estimates. The upper and lower bounds represent 95% confidence bands generated via the bootstrap

## Data Appendix

### Data Matching

The data on arrests and offenses used in the paper were obtained from the National Consortium for Violence Research via its Data Cubes application. The Data Cubes provide agency-level data derived from the FBI's Uniform Crime Reports (UCR) and are accessible online at <http://www.ncovr.org>. Because NCOVR has performed some data checking and cleaning, the NCOVR data differs slightly from other sources of UCR data (such as those available through the ICPSR); the NCOVR data tend to be more complete and more consistent across years. In addition to raw arrest counts, the Data Cubes provide agency-level breakdowns of arrests by age and race (although, unfortunately, not ethnicity). For 1997 onward the New Jersey State Police provide summary reports of crimes committed in New Jersey available online at <http://www.njsp.org/info/stats.html>. I compared the arrest and offense totals in the NCOVR data to those in the published reports and found that the NCOVR totals were very close but not identical to those in the reports.

I used Census files from 1990 and 2000 to match each law enforcement agency in New Jersey, Maryland, Connecticut, to the demographics of their communities. I used linear interpolation to obtain the annual populations by race in intercensal years and extrapolated the 1990-2000 trend for years 2001-2003. In a few cases I combined multiple agencies within a single jurisdiction. Jurisdictions with population data available in 2000 but not 1990, which were typically emergent communities with populations below 2000 residents were omitted from the analysis. Special agencies such as park police, court police, or other agencies without a clear population base were omitted from the analysis, as were university police departments. The table below provides summary statistics describing the quality of matches for each of the states. The matched jurisdictions represent the majority of the population in all of the states and the racial composition of the matched agencies is comparable to that of the states as a whole.

<b>State</b>	<b>Year</b>	<b>Police Agencies</b>	<b>Percentage of Agencies Matched</b>	<b>Population % Black</b>	<b>Matched % Black</b>
New Jersey	1990	494	98.0%	13.4%	14.5%
	2000	494	100.0%	13.6%	14.3%
Connecticut	1990	96	99.0%	8.3%	9.0%
	2000	96	100.0%	9.1%	9.7%
Maryland	1990	134	97.8%	24.9%	21.3%
	2000	134	97.8%	27.9%	25.5%
North Carolina	1990	473	98.7%	21.9%	24.3%
	2000	473	98.3%	21.6%	24.0%

### Calculating the Propensities

The structural estimates reported in Tables 5 and 6 and Figures 6 and 7 require assumptions regarding the relative propensity of individuals to commit crimes by race. For simplicity assume

that all agents are either Black or White. Let  $n_b^k$  denote the number of Black residents of location  $k$ ,  $p_{bj}^k$  the propensity of Blacks to commit crime  $j$  in location  $k$ , and  $c_{bj}^k$  the total number of crimes of type  $j$  committed by Blacks in location  $k$ . For Blacks and Whites we have that:

$$n_b^k p_{bj}^k = c_{bj}^k$$

$$n_w^k p_{wj}^k = c_{wj}^k$$

Combining the two expressions:

$$\frac{p_{bj}^k}{p_{wj}^k} = \frac{c_{bj}^k n_w^k}{c_{wj}^k n_b^k} = \frac{\mu_j^k (1 - \pi^k)}{(1 - \mu_j^k) \pi^k}$$

where  $\mu_j^k = \frac{c_{bj}^k}{c_{bj}^k + c_{wj}^k}$  and  $\pi^k = \frac{n_b^k}{n_b^k + n_w^k}$

In my baseline structural estimates I use the arrest proportions as a measure of  $\mu_j^k$  and the crime-weighted average percent of the population that is Black for  $\pi^k$ . Alternative assumptions regarding these propensities change the estimated absolute number of crimes committed by each group but have little qualitative effect on the results.

The following table reports the propensity values used in the structural estimation.

State	Crime	Avg. Weighted Population % Black	Black Proportion of Arrestees	Implied Relative Propensity
New Jersey	Vehicle Theft	31.1%	52.3%	2.43
New Jersey	Burglary	24.5%	37.9%	1.88
New Jersey	Larceny	18.5%	40.7%	3.02
New Jersey	All Crimes	23.5%	40.8%	2.24
Connecticut	Vehicle Theft	19.9%	42.3%	2.95
Connecticut	Burglary	16.1%	24.8%	1.72
Connecticut	Larceny	14.3%	29.5%	2.51
Connecticut	All Crimes	17.2%	30.1%	2.07
Maryland	Vehicle Theft	42.4%	75.0%	4.08
Maryland	Burglary	35.6%	51.5%	1.92
Maryland	Larceny	32.6%	54.3%	2.46
Maryland	All Crimes	35.3%	51.9%	1.98
North Carolina	Vehicle Theft	29.4%	56.8%	3.16
North Carolina	Burglary	28.4%	46.1%	2.16
North Carolina	Larceny	28.8%	52.4%	2.72
North Carolina	All Crimes	29.0%	48.3%	2.29