

Understanding the Effects of Anti-Profiling Policies

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December 2006

JOB MARKET PAPER

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 race-neutral policing. 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. My implied elasticities do not suggest minorities respond differently to policing intensity than the general population. Additional 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

I wish to thank Gary Becker, Steven Levitt, Charles Loeffler, Kevin M. Murphy, Alex Piquero, 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.

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.

In the considerable racial profiling research by economists, criminologists, and legal scholars, there has been little attention devoted to evaluating these anti-profiling policies. Instead, past academic research has focused on determining if profiling occurs and whether such profiling is efficient. Spurred by a number of studies conducted that have found that Black motorists are significantly more likely to be stopped and searched than their White counterparts¹, economists and criminologists have developed models of profiling that allow researchers to statistically detect the presence of bias using traffic stop data. This paper, in contrast, answers a different but equally important question—what are the behavioral consequences of programs aimed at reducing racial bias by police?

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 towards race-neutral policing. I find that the scandal led to a substantial drop in arrests (10-40%) of Blacks relative to Whites for offenses susceptible to detection through traffic stops. I also present evidence that the

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

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 minority-specific values for the elasticity of offending with respect to enforcement. My estimates of this elasticity, which are important parameters in evaluating the efficiency of profiling regimes, appear comparable to past estimates from the general population. 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.

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. 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, such as Harris (1999), 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 potentially "undesirable" arrests might be mitigated as anti-profiling policies are implemented.

Section II of the paper compares and contrasts this study with prior work on profiling and police incentives. Section III presents a simple behavioral model designed to capture the likely effects of changes in profiling policy. Section IV reviews the history of the racial profiling scandal in New Jersey. Section V discusses the effect of the profiling scandal on arrest rates.

Section VI 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 VII 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 VIII 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 IX concludes.

II. Related Work on Profiling and Police Incentives

Although a complete catalogue of scholarly work on the issue of racial profiling is beyond the scope of this paper², in this section I will attempt to frame the present investigation of profiling policies in the context of past profiling research. Some of the most prominent research concerning profiling has attempted to address issues relating to the measurement and detection of profiling. Among economists, a primary focus has been developing behavioral models that allow for statistical tests for profiling using traffic stop data (Knowles et. al 2001, Hernandez-Murillo and Knowles 2001, Persico 2002, Anwar and Fang 2006). Given the myriad of different types of data used in the profiling debate, including audit data (Lamberth 1994, Smith et. al. 2003), traffic stop data (Smith and Petrocelli 2001, Petrocelli et. al. 2003), survey data (Engel and Calnon 2004, Lange et. al. 2005), and video and other electronic data (Lange et. al. 2001, Waddington et. al 2004), there has been substantial interest in understanding the extent to which different sources of data can inform questions regarding the extent of profiling. Although there have been some attempts to characterize measurement best practices (Ramirez et. al. 2000,

² More thorough reviews of this literature include Batten and Kandleck (2004) and Harcourt (2004).

Farrell et. al. 2005), there remain a number of unresolved questions on this front (Engel et. al. 2002, Parker et. al. 2004).

Another area of measurement research emphasis has been the “benchmarking” or “denominator problem”, a term used to refer to the difficulty of adequately characterizing the at-risk population for traffic stops--a necessary ingredient to profiling detection--without detailed data regarding the travel patterns of minorities versus the population at-large. A number of papers have criticized the approach of consultancies hired by jurisdictions to perform profiling assessments, particularly with reference to the use of Census data as benchmarks (Walker 2001, Smith and Alpert 2002, Fridell 2004, Rojek et. al. 2004). Numerous solutions to the benchmarking problem have been proposed, including use of accident data (Alpert et. al. 2004), comparisons between daytime and night driving (Grogger and Ridgeway 2006), and the employment of statistical techniques designed to control for local variation in stop frequencies (Gelman et. al. 2006).

A different strain of the profiling literature examines public perceptions of profiling. A particular area of interest has been cataloguing the extent to which profiling perceptions differ across racial and ethnic groups (Henderson et. al. 1997, Lundman and Kaufman 2003, Engel 2005) as well as among those with differing levels of prior police interaction (Weitzer and Tuch 2005). One fairly consistent message across a number of studies is belief among adult Americans that profiling is a widespread phenomenon (Gallup Organization 2001, Weitzer and Tuch 2002).

Broadly stated, past research has thus focused on two questions: 1) How prevalent is racial profiling? and 2) What are public perceptions of racial profiling? This body of research on whether profiling occurs and whether citizens perceive such profiling as a problem is critically

important for policymakers, who must decide whether intervene with programs for controlling profiling. The present paper is complementary to previous research in that it answers a different but equally important question—if such policy interventions occur, what are their likely effects on police and citizen behavior?

This study also relates to a number of papers in the organizational economics literature that demonstrate that police officers respond in ways similar to employees of private businesses or other large organizations when faced with incentive changes. Prendergast (2001), for example, provides evidence that Los Angeles police officers weakened enforcement efforts following an administrative change that increased punishments for citizen complaints. Shi (2005) presents similar evidence for Cincinnati. Mas (2006) demonstrates that New Jersey police officers who lost salary arbitration cases in the early 1990's responded with laxer policing. One way to characterize the effect of the New Jersey scandal is as an increase in the cost to officers of scrutinizing minority drivers; organizational theory suggests that such an incentive change could lead to reduced policing and higher crime.

III. A Simple Model of the Effects of Profiling Changes

In this paper I am concerned with quantifying the effects of a shift in profiling policy as opposed to testing for the presence of profiling. To understand the likely consequences of anti-profiling policies, 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:

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

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 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, π_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, policies which decrease 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 to detection through vehicle stops, it seems likely in practice that $\frac{\partial a_t}{\partial r}$ 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 policies are enacted which raise the cost of profiling.

IV. 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.³ 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. 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

³ 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 typically employed by economists (Knowles et. al. 2001) would suggest that these hit rates imply *under*-searching of minority motorists.

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 anti-profiling policies from other secular trends in criminality and enforcement that may have affected arrest and offending rates.

V. 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.⁴ Figure 2 plots annual arrest rates by race for several types of crimes. State-level data on arrests was obtained from the FBI's Uniform Crime Reports (UCR).⁵ To facilitate comparison across races, I present the Black and White arrests rates using logarithmic scales centered on different values, although the magnitude of the scale in log points is the same for both races. 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.

⁴ 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.

⁵ Given that the scandal was focused on the state police, it may seem more natural to analyze arrests by the state police as opposed to arrests by all agencies within the state. New Jersey's UCR reports do not separately report arrests by the state police, however, instead matching arrests to the locality in which an offense occurred regardless of the arresting agency. In addition, media and other evidence suggest the scandal likely generated spillovers to other police departments within the state.

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.⁶ 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 2003.⁷ Qualitatively it appears that there were substantial decreases in arrests due to the profiling scandal.

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 full 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 policy in the years prior to

⁶ 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.

⁷ 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.

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 policies than those of Blacks, allowing Whites to act as a comparison group for Blacks⁸. Additionally, the only types of arrests that are likely to be affected by changes in profiling policy are those associated with vehicle stops, such as vehicle theft and DUI, 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 arrest 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 ⁹, 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:

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

β_0 measures the percentage change in arrests due to the scandal and associated reforms. The DD and DDD regressions can be obtained by omitting the relevant summation terms and dimensions of N from (6).

⁸ 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.

⁹ 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.

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) 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 anti-profiling reforms. 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 are used as a control for DUI.¹⁰ 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 that because regulations and policies regarding police behavior are typically instituted at the local or state level, the scandal effects

¹⁰ If criminals are not highly specialized (Piquero et. al. 2003), this third difference also helps to solve the denominator problem by differencing out any changes due to shifts in the at-risk population.

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.¹¹ 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 and focuses on the more stringent DDD and DDDD specifications. 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 GLS to allow for serially correlated error terms also has little effect on the estimates.¹² Finally, vehicle thefts in the city of Newark may have been affected by the availability of the Lojack

¹¹ Delaware or Southeastern Pennsylvania might seem to be more natural control geographies; unfortunately the UCR data for these areas is incomplete.

¹² Although use of clustered standard errors may seem warranted in this situation (Bertrand et. al. 2004), because there are fewer panels than time periods estimation with clustered standard errors is problematic. Other methods of accounting for serial correlation in the error structure such as the block bootstrap or Prais-Winsten generate similar results.

vehicle recovery system.¹³ Omitting Newark from the sample actually slightly increases the estimated effects of the scandal on arrests.

Table 3 reports placebo tests of the differences approach by re-estimating the original specification on crimes, groups, and geographic areas presumably unaffected by the policy changes. Arrests for violent crimes and burglary have no connection with traffic stops so these crimes should not be affected by changes in profiling.¹⁴ Similarly, arrests in Pennsylvania or arrests of Asians should have been unaltered by the scandal. In contrast to the findings for vehicle theft and DUI, the point estimates in these placebo regressions 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.

The final two specifications of Table 3 estimate the effects of the profiling reforms exclusively on Whites. One implication of the model is that profiling policy changes could shift enforcement resources towards the majority group, increasing arrests of this group. The reported DD and DDD estimates do not indicate that White arrests systematically changed as a result of the profiling reforms, however, suggesting that the new policies may have resulted in a net reduction in traffic stop enforcement as opposed to a shift of scrutiny from Blacks to Whites.

VI. 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. A rational choice model of criminal behavior implies that such a decrease of enforcement, which lessens the cost of

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

¹⁴ Sampson and Cohen (1988) and Kubrin et. al. (2006) suggest that robbery offense rates may be linked to the aggressiveness of policing; I thus do not include robbery in the placebo regressions.

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.¹⁵

The upper left panel of Figure 4 shows monthly counts of New Jersey motor vehicle thefts from 1994-2003. The upper right panel includes as a basis for comparison a time-series plot for the combined total of burglaries and larcenies, the other two index property crimes. All three index crimes demonstrated a steady downward trend from 1995 through the beginning of 1998. While burglary and larceny continued their downward trend after 1998, 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.

To formally test for a structural break in the offending time series I employ the approach of Andrews (1993). For each potential point at which a structural break may have occurred, Andrews proposes fitting a model allowing for separate time trends before and after the break and calculating the Wald statistic under the null of common coefficients pre- and post-break. Andrews (1993, 2003) compiles critical values for the supremum of the Wald statistics generated in this fashion, allowing for a formal statistical test for the presence of a structural break.

Whether and at what point a break occurs is thus determined by the data. In my analysis I

¹⁵ 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 also be 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.

examine each of the months between 1997 and 2000 for evidence of a break, fit the time trend using a quartic polynomial, and include month indicators in my analysis to control for seasonal variations in crime.¹⁶

The bottom panels of Figure 4 depict the fitted polynomials generated through the Andrews procedure. For vehicle theft the structural break analysis indicates a break in December 1998 that is statistically significant ($p=.03$)¹⁷; moreover, the fitted polynomials imply that the structural shift moved offending from a downward trajectory to a higher, essentially flat one. For burglary and larceny, the model of best fit suggests a possible break in mid-1997 that is not statistically significant ($p=.20$) and indicates a decreasing crime profile from 1997 through the end of the sample. This timing and nature of the shift in vehicle theft offending in contrast to other crimes are consistent with the notion that the decreased arrests following the scandal led to these offending changes.

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.¹⁸ 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

¹⁶ Including larger windows for break dates and polynomials of differing degree similarly yields evidence of a break in the time series for vehicle theft during 1998 but much weaker evidence that a break occurred for burglary and larceny.

¹⁷ Given that my times series spans only 120 periods the revised asymptotic critical values in Andrews (2003) may not be applicable. I performed a 10000 iteration Monte-Carlo simulation using the error distribution implied by my data to calculate the appropriate critical values for my sup Wald statistic.

¹⁸ 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.

shift towards areas populated primarily by Blacks.¹⁹ 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 such 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.²⁰ If the departure of motor vehicle thefts from trend is truly driven by changes in scrutiny of minority motorists, one would expect a larger increase relative to trend in areas with a higher proportion of Black residents. To test this formally, consider two-step regressions of the form:

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

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

The log offense rates for a crime in location i and time t (y_{it}) are first regressed separately by agency on a time trend (t) and dummy variables for the years 1998-2003 (D_k). The coefficients on the dummies (β_{ik}) provide location-specific estimates of the degree to which the change in a

¹⁹ Numerous past studies have documented the local nature of vehicle theft offending. Wiles and Costello (2000) find that 92% of vehicle theft offenders reside in the local jurisdiction where the offense occurs, while Levine (2004) documents that approximately 60% of a sample of 2548 vehicle thefts in Baltimore County occurred within 5 miles of the offender's residence. Lu (2003) finds that vehicles are recovered an average of 2.2 miles from the location in which they are stolen.

²⁰ Details of the construction of the matched data set can be found in the Appendix.

particular crime rate was above or below the trend in a particular year. The second stage, given by equation (8), models the cross-section variation across locations in the departure from trend in year k as a function of the proportion of the population that is Black, π_i , along with a vector of other location specific controls, X_i . The coefficients on percent Black, α_k , provide reduced-form estimates of the extent to which crime increased or decreased more in areas with a higher proportion of Blacks.²¹

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 75th 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 25th percentile (1.8% Black). For burglary and larceny, however, the estimated coefficients are negative, suggesting that for these types of crimes 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

²¹ 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.

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.²²

The results of the estimation exercise are presented in Figure 5. 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.²³ 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 the profiling reforms strongly suggests that the changes in motor vehicle offense rates were linked to post-scandal adjustments in arrest patterns.

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

²² 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.

²³ 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.

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 only the aggregate time-series 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. Using the difference estimates, which likely provide a low estimate of the elasticity, yields estimated elasticities between -.10 and -.63. These scandal-derived elasticity estimates differ from past estimates in that they are minority specific, which in fact makes them particularly informative in evaluating the efficiency of profiling regimes.²⁴

If changes in profiling-related police scrutiny have similar behavioral effects to changes in police presence²⁵, 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 Schargrotsky (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 lag to calculate an elasticity of -.37 to -.40. My lower estimates of -.20 to -

²⁴ The consequences of profiling differ if minorities are systematically more or less responsive to changes in policing intensity than Whites, yet there is little empirical evidence regarding this issue. Persico (2002), Harcourt (2004), and Durlauf (2006) provide a more detailed discussion.

²⁵ This would be true if there are constant returns to police at observed levels of crime and policing.

.57 are roughly similar to these other estimated elasticities. It does not appear that minorities differ systematically from the general population in responsiveness to enforcement.

The elasticity and arrest estimates permit simple back-of-the-envelope calculations of the effect of the anti-profiling policies on crime. Using mid-range estimates of -.5 for the elasticity and 30% for the drop in arrests implies a 15% increase in minority offending following the scandal. If minorities account for 50% of vehicle theft in New Jersey, this translates to approximately 3000 additional vehicle thefts annually.

VII. 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 policy. 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 policy 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 offense patterns in Maryland are strongly consistent with the conclusions regarding the effects of anti-profiling policies presented previously.²⁶

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

²⁶ 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.

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 policy 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.²⁷ The estimates of an approximately 30% decrease in arrests due to profiling policy 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 elasticity of offending with respect to arrests of approximately -.5. Figure 6 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

²⁷ To maintain consistency with the New Jersey results I include crimes reported by all Maryland police agencies in these regression. For Maryland, however, separate counts of arrests by the state police are also available. As one might expect given that state police were the focus of the profiling scandal, the drops in arrests following the scandal are even more pronounced (30% larger on average across specifications) after limiting the analysis to only the state police.

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.

VIII. 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 decreased police scrutiny is through increased offending, there are other dimensions of citizen 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.

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.²⁸ 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 only to 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 factors beyond the control of drivers.^{29,30} Such accidents account for about 4% of the accidents in the data and presumably would not be affected by changes in profiling policy. Including this additional difference helps

²⁸ 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 with any restraint other than a child safety seat were classified as carrying children unsafely.

²⁹ These accidents include, among others, accidents due to defective roads or signals, actions by pedestrians or animals, standing water, vehicle malfunction, and improper lane markings.

³⁰ Alpert et. al. (2004) argue that not-at-fault accident statistics provide information on driver racial composition comparable to that obtainable through audit studies.

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 policy changes. 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 (2006) 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 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 suggesting a small shift toward Black offending for these violations, although the estimates are statistically indistinguishable from zero. For carrying children unsafely, however, the point estimates are positive and larger, suggesting that these behaviors increased by around 20% 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 police scrutiny. 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.

IX. Conclusions

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, a more 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 policy. 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 effects of the reforms appear 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. For example, policies which couple profiling reforms with increased deployment of bait cars in minority areas or incentives for citizen use of anti-theft technologies such as Lojack may achieve fairer policing without corresponding increases in auto theft.

Future research into the effects of profiling policy could proceed on several fronts. One obvious extension would be to investigate the degree to which the various types of anti-profiling measures contribute to the changes in police behavior. In addition, it seems reasonable to think that the effects of profiling policies, particularly any tendency for these policies to lead to increased crime, would be dependent on the characteristics of the communities in which the profiling policies are implemented. Further research into the contextual effects of profiling policies is warranted.

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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		DDDD			
	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)
Motor Vehicle Theft	-0.371*** (0.0537)	-0.195** (0.0934)	-0.317*** (0.119)	-0.171 (0.154)	-0.264** (0.115)	-0.352* (0.187)	-0.401*** (0.144)
DUI	-0.145*** (0.0153)	-0.153*** (0.037)	-0.092*** (0.0211)	-0.455*** (0.129)	-0.149* (0.0856)	-0.376*** (0.119)	-0.099 (0.0685)
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:				NJ-CT	NJ-US	NJ-CT	NJ-US

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. For the third difference public order offenses other than DUI are used as control crimes for DUI and property crimes other than motor vehicle theft are used as control crimes for vehicle theft (column II); the set of all crimes is also used as a comparison group for each type of crime (column III). For the fourth difference, Connecticut is used as a comparison geography (columns IV and VI) as is the entire United States (columns V and VII). 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)			
Vehicle Theft	Use years 1990-2003	-0.441*** (0.0467)	-0.256*** (0.0866)	-0.402*** (0.116)	-0.248* (0.135)	-0.307*** (0.111)	-0.437*** (0.167)	-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)	-0.148 (0.125)	-0.217 (0.19)	-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)	-0.26*** (0.1)	-0.307* (0.174)	-0.389*** (0.118)
	Allow for AR(1) Errors	-0.381*** (0.0288)	-0.207** (0.0836)	-0.284*** (0.106)	-0.181 (0.146)	-0.35*** (0.11)	-0.311* (0.18)	-0.385*** (0.139)
	Omit Newark	-0.411*** (0.0534)	-0.216** (0.0943)	-0.363*** (0.117)	-0.193 (0.153)	-0.285** (0.116)	-0.392** (0.184)	-0.438*** (0.144)
DUI	Use years 1990-2003	-0.176*** (0.0223)	-0.213*** (0.075)	-0.138*** (0.0511)	-0.513*** (0.147)	-0.241** (0.115)	-0.424*** (0.121)	-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)	-0.153* (0.0836)	-0.317** (0.126)	-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)	-0.117 (0.0879)	-0.35*** (0.122)	-0.074 (0.0746)
	Allow for AR(1) Errors	-0.136*** (0.0114)	-0.152*** (0.0326)	-0.089*** (0.0154)	-0.464*** (0.115)	-0.113 (0.0815)	-0.389*** (0.108)	-0.08 (0.0624)
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:					NJ-CT	NJ-US	NJ - CT	NJ-US

Note: See notes for Table 1. The specifications allowing for AR(1) standard errors adjust the standard errors to account for panel-specific autocorrelation and heteroskedasticity using GLS.

Table 3: Placebo Regression Tests of Multiple Differences Estimation Approach

Group Comparison	Crime Comparison	Geography Comparison	(I)	(II)	(III)	(IV)	(V)	(VI)
			DDD		DDDD			
Black/White	Murder/Violent,All	NJ/CT,US	0.104 (0.13)	-0.027 (0.126)	0.278 (0.209)	0.103 (0.158)	0.185 (0.225)	0.015 (0.169)
Black/White	Rape/Violent,All	NJ/CT,US	-0.131 (0.0905)	-0.262** (0.116)	-0.137 (0.151)	-0.054 (0.106)	-0.229 (0.151)	-0.143 (0.127)
Black/White	Assault/Violent,All	NJ/CT,US	0.027 (0.0359)	-0.103** (0.0479)	0.035 (0.105)	0.023 (0.0569)	-0.058 (0.111)	-0.066 (0.0629)
Black/White	Burglary/Property,All	NJ/CT,US	0.082 (0.0546)	-0.069 (0.0552)	0.11 (0.105)	0.049 (0.0751)	-0.072 (0.127)	-0.101 (0.0909)
Black/White	Vehicle Theft/Property,All	PA/CT,US	0.015 (0.098)	-0.066 (0.1)	0.038 (0.178)	-0.054 (0.141)	-0.101 (0.188)	-0.15 (0.151)
Black/White	DUI/Public Order,All	PA/CT,US	0.034 (0.0534)	0.084* (0.0457)	-0.267* (0.139)	0.038 (0.105)	-0.2 (0.13)	0.077 (0.0934)
Asian/White	DUI/Public Order,All	NJ/CT,US	0.077 (0.0964)	0.105 (0.0649)	-0.295* (0.163)	0.309** (0.122)	-0.28* (0.149)	0.274*** (0.0938)
			DD		DDD			
None (White only)	DUI/Public Order	NJ/CT,US	0.007 (0.0288)	0.066*** (0.0142)	-0.143* (0.0841)	-0.002 (0.0512)	-0.077 (0.0757)	0.025 (0.04)
None (White only)	Vehicle Theft/Property	NJ/CT,US	-0.024 (0.0563)	-0.184** (0.0841)	0.102 (0.07)	0.019 (0.0697)	0.127 (0.0992)	-0.01 (0.0931)

Note: See notes for Table 1. The placebo tests rerun the original statistical analysis on crimes, groups, and geographic areas that should not be affected by changes in profiling policy to examine whether the multiple differences approach is likely to generate spurious findings of a policy effect. In the original specification the group comparison was Black/White, the crime comparison was Vehicle Theft/Property, All or DUI/Public Order, All and the geographic comparison was NJ/CT, All. For the first seven specifications the column ordering is as in Tables 1 and 2. The final two specifications omit Blacks completely from the analysis; for these specifications the first two columns represent DD estimates with the first difference being post scandal minus pre scandal and the second difference affected crime minus unaffected crime. The final four columns are DDD estimates with an additional difference being taken between affected location and unaffected location. Asians accounted for an insufficient number of motor vehicle arrestees to permit a valid placebo test for motor vehicle theft.

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			
	(I)	(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)	-0.142 (0.102)	-0.449*** (0.116)	-0.311 (0.268)
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

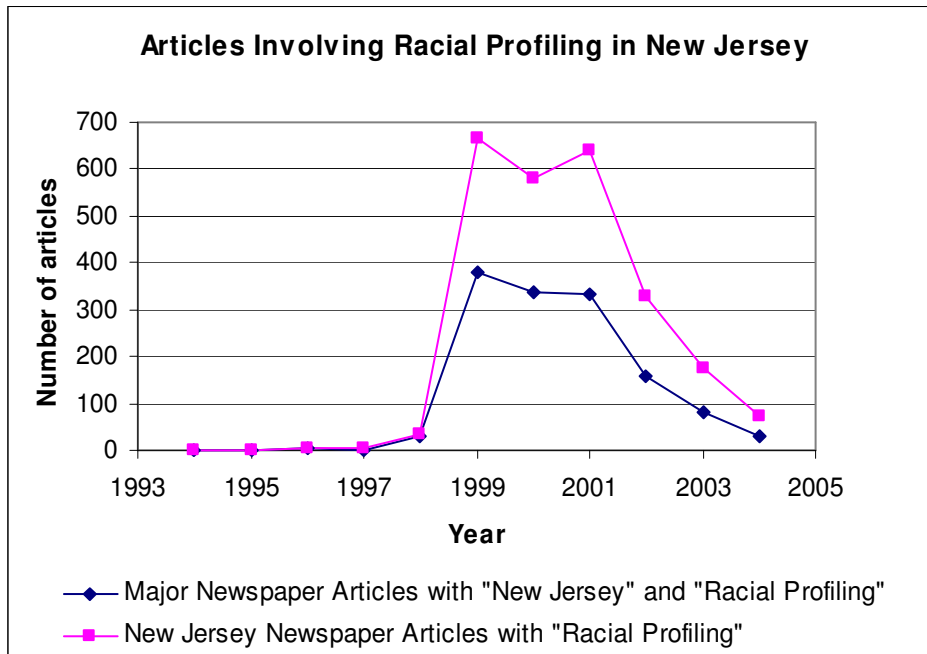
Note: The first row of the table reports differences estimates of the effect of a racial profiling scandal on arrest rates in Maryland, and corresponds to the New Jersey estimates in Table 1. The dependent variable is the log arrest rate for a particular year, racial group, crime type, and state. The 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

Accident Specifications	Violation Type		
	Speeding	Intoxicated Driving	Unsafe Child
All accidents	0.054 (0.0946)	0.065 (0.092)	0.244 (0.367)
Accidents reported by state police	0.109 (0.178)	0.037 (0.124)	0.161 (0.217)
Daytime accidents	0.064 (0.0993)	0.093 (0.156)	0.186 (0.354)

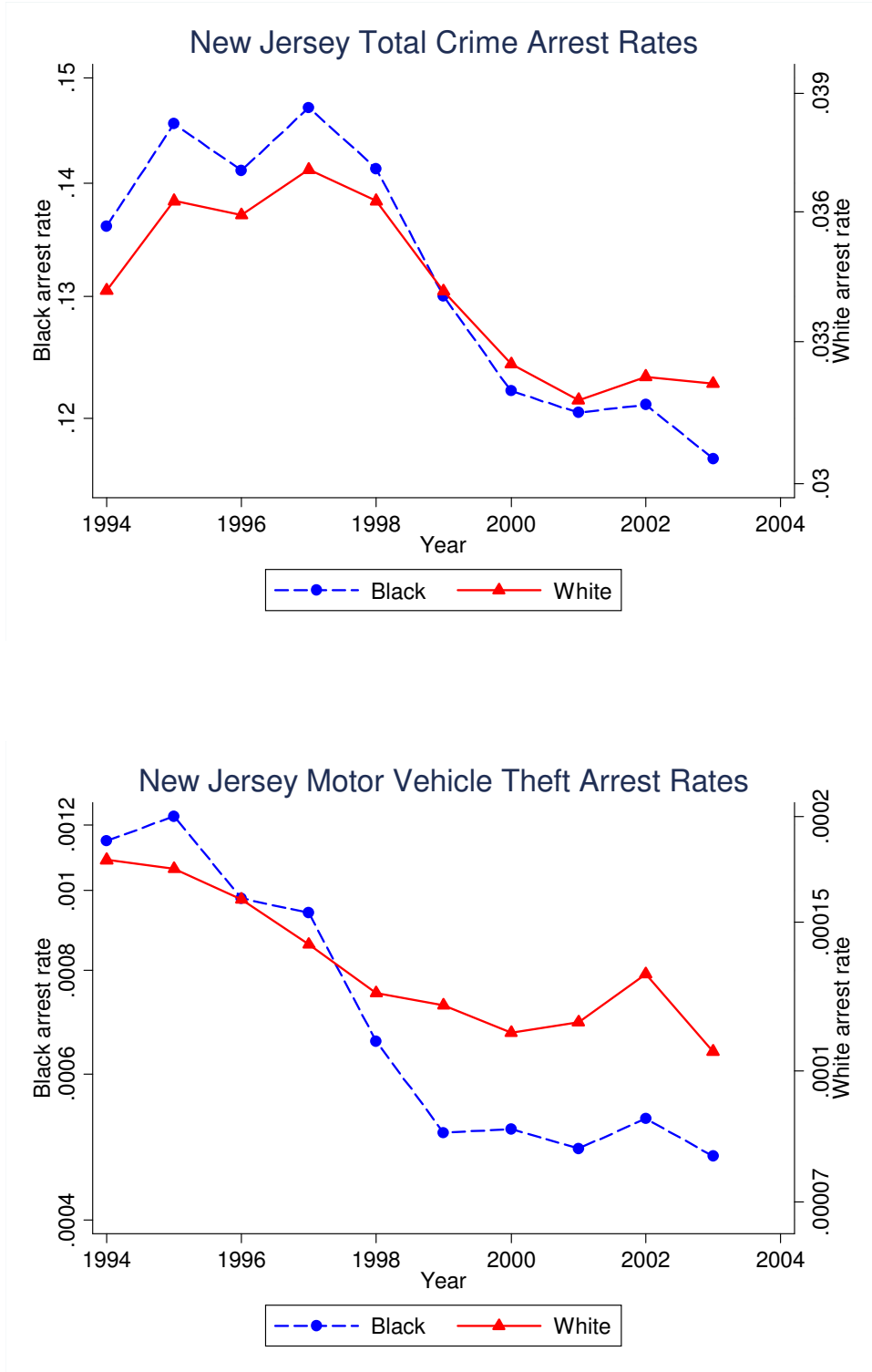
Note: This table reports triple-difference estimates of the effect of the New Jersey racial profiling scandal on unsafe driving behaviors by Black drivers. Data is drawn from the NJDOT crash records database covering 1997-2004. The dependent variable is the log annual number of New Jersey drivers involved 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 factors beyond the driver's control. 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.

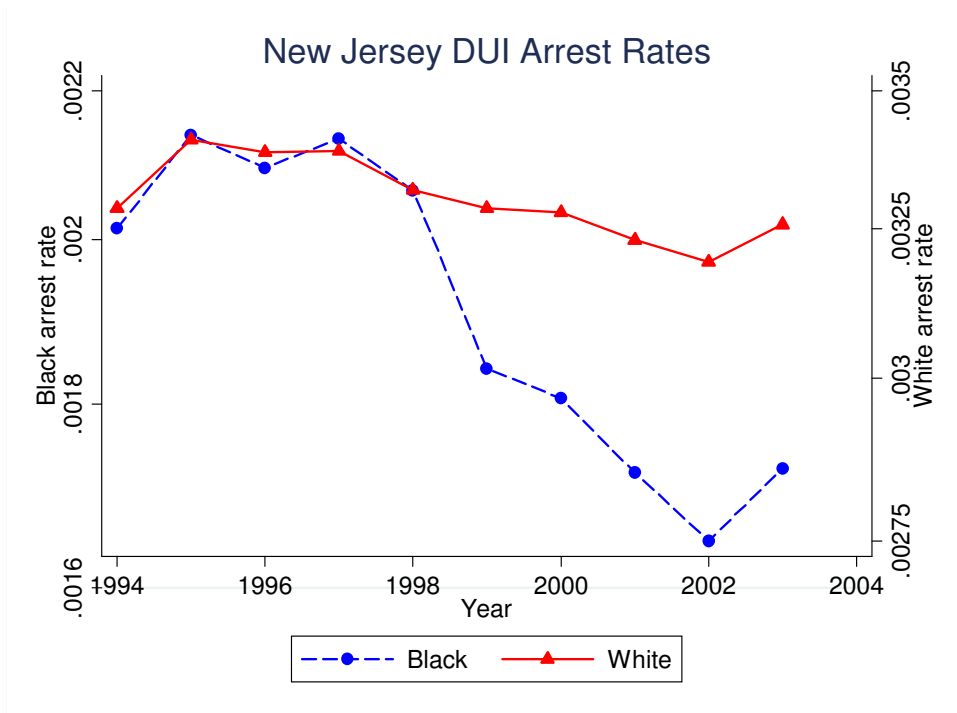
Figure 1: Articles on Racial Profiling in New Jersey



Source: Lexis-Nexis

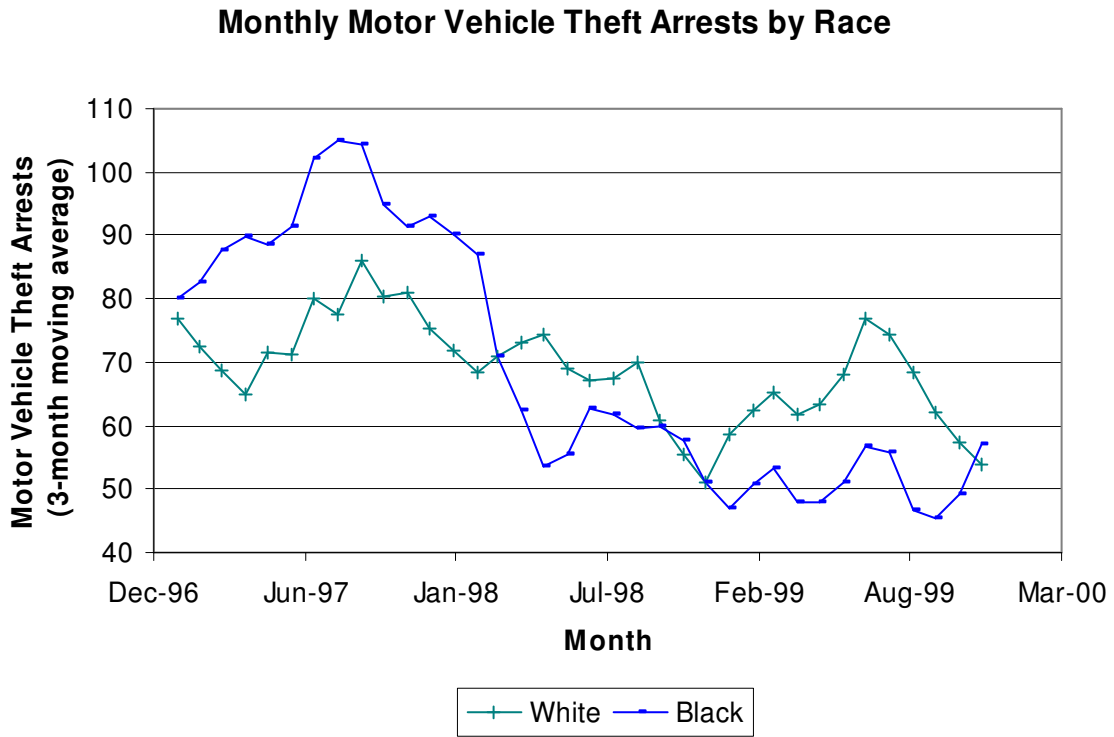
Figure 2: New Jersey Arrests by Race





Note: These figures plot raw annual arrest rates in New Jersey for several categories of crime. Arrest rates are arrests per population. Black and White rates are plotted on separate logarithmic scales; the magnitude of the scales in log points is equivalent across groups in each chart to facilitate comparison.

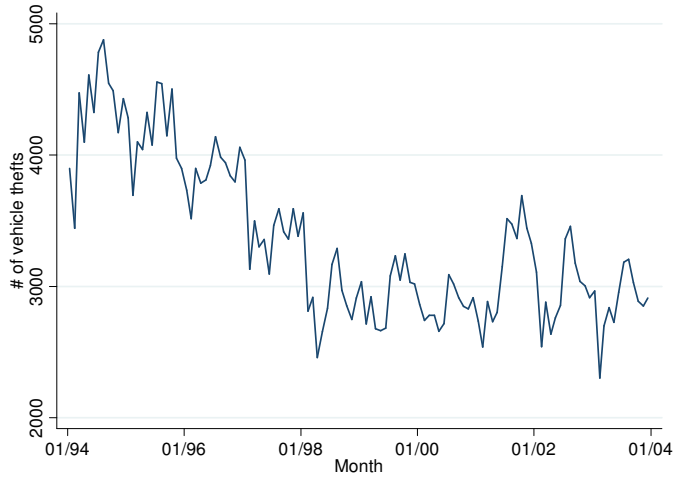
Figure 3: Monthly Motor Vehicle Theft Arrests by Race



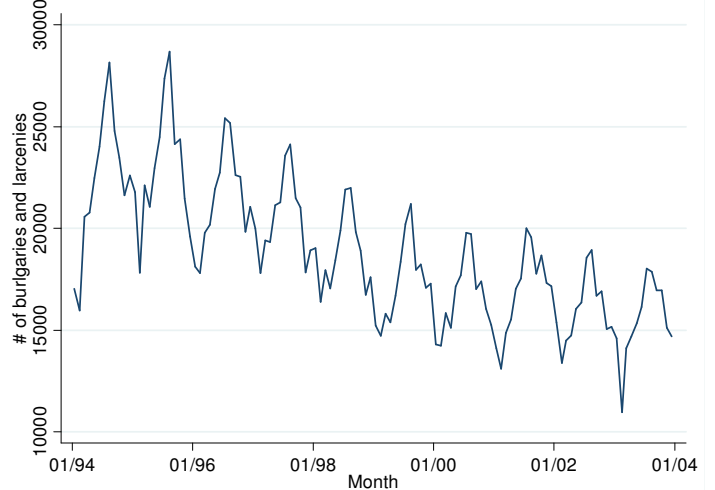
Note: This figure plots monthly arrest counts by race for motor vehicle theft in New Jersey. Three-month moving averages are presented to smooth seasonal variation.

Figure 4: Monthly Property Crimes in New Jersey, 1994-2003

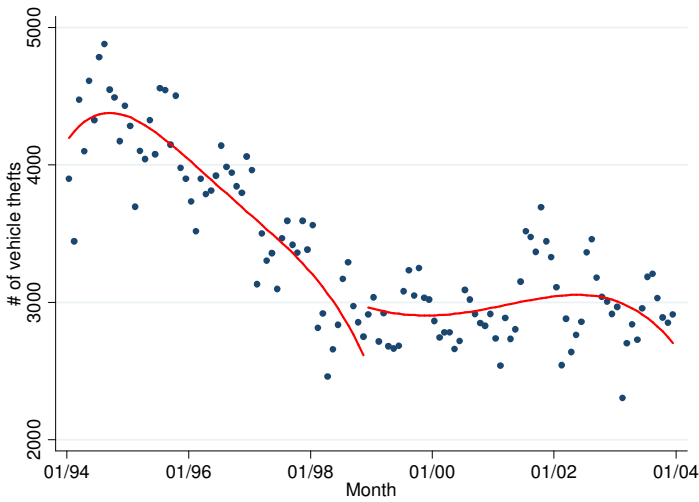
Vehicle Theft



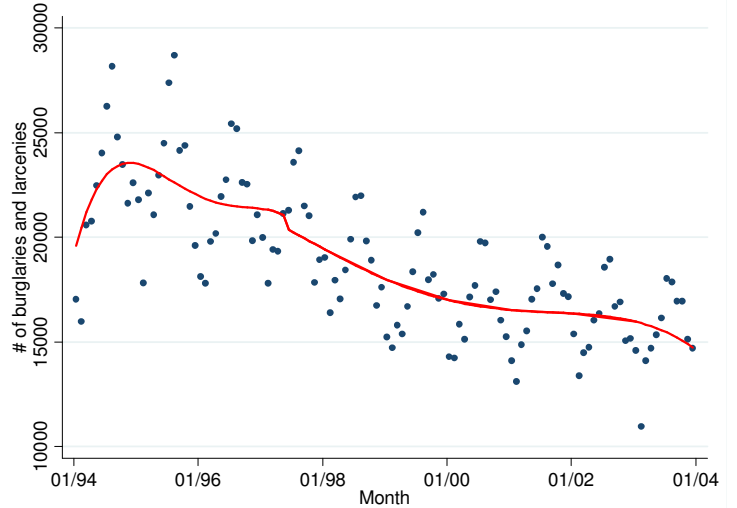
Other Property Crimes



Fitted Polynomial for Vehicle Theft

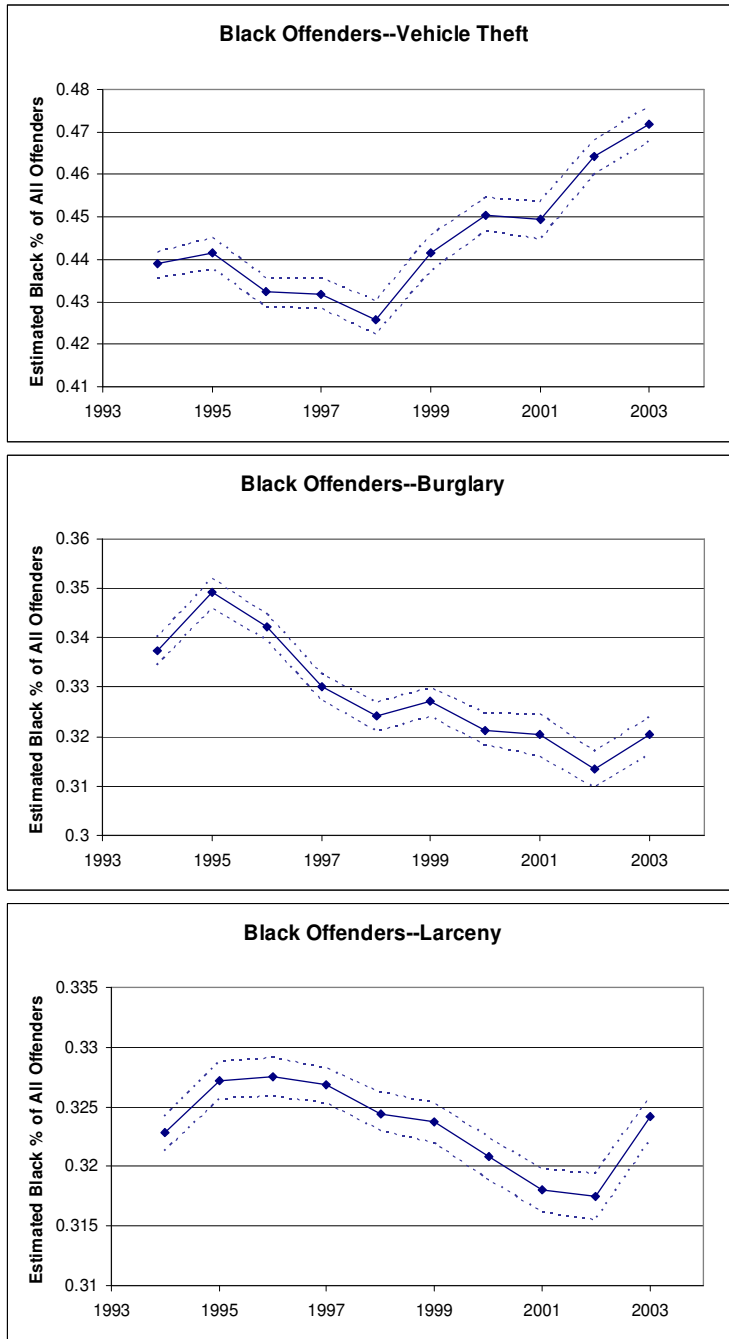


Fitted Polynomial for Other Property Crimes



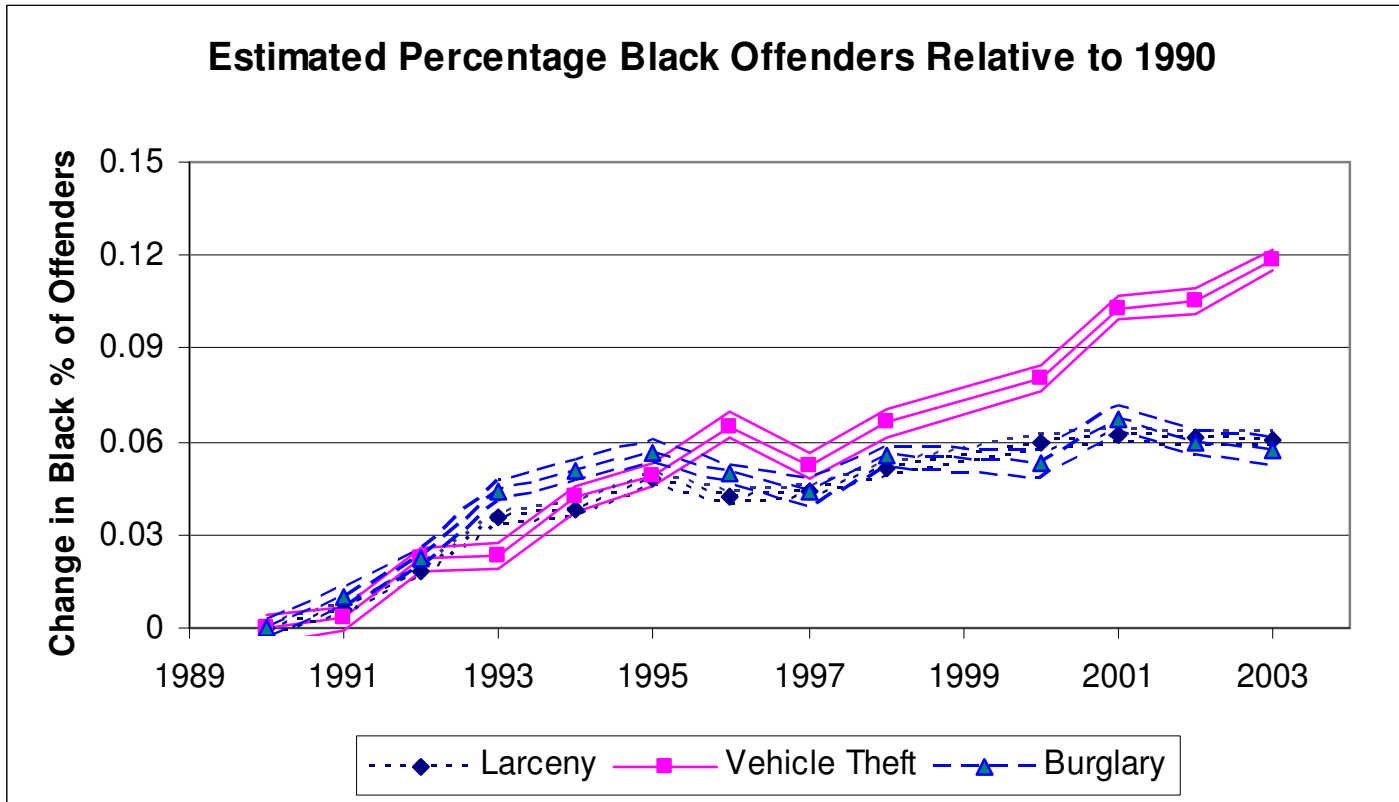
Note: These figures plot raw monthly offense counts for vehicle theft and the combined total of burglary and larceny in New Jersey. In the bottom panels, the red lines depict the best-fit polynomial trend generated through the Andrews (1993) procedure. Statistical tests for trend continuity in December 1998 can be rejected at the 5% level for vehicle theft but there is no statistically significant structural break for other property crimes.

Figure 5: 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 6: 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 on 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. Because Black residents tend to be more concentrated in urban areas which are more likely to be covered by multiple police agencies, the matched percent Black is typically slightly higher than that of the state.

State	Year	Police Agencies	Percentage of Agencies Matched	Population % Black	Matched % Black
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 μ_j^k and the crime-weighted average percent of the population that is Black for π^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