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THE IMPACT OF RACE ON POLICING, ARREST PATTERNS, AND CRIME

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ABSTRACT

Race has long been recognized as playing a critical role in policing. In spite of this awareness, there has been virtually no previous research attempting to quantitatively analyze the issue. In this paper, we examine the relationship between the racial composition of a city's police force and the racial patterns of arrests and crime. Increases in the number of minority police are associated with significant increases in arrests of whites, but have little impact on arrests of non-whites. Similarly, more white police increase the number of arrests of non-whites, but do not systematically affect the number of white arrests. The race of police officers has a less clear-cut impact on crime rates. It appears that own-race policing may be more effective in reducing property crime, but no systematic differences are observed for violent crime. These results are consistent either with own-race policing leading to fewer false arrests or greater deterrence. In either case, own-race policing appears more "efficient" in fighting property crime.

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Race is a polarizing feature in American society. Nowhere is this more evident than in the criminal justice system. African Americans, who comprise twelve percent of the U.S. population, account for 47 percent of felony convictions and 54 percent of prison admissions. Studies suggest that one-third of African American males aged 20-29 are under the supervision of the criminal justice system on any given day (Mauer and Huling 1995). Minority communities are often suspicious of and hostile towards the criminal justice system and particular police (National Advisory Commission on Civil Disorders 1968, Mast 1970, NAACP 1995). Conflicts between police and citizens have been the flashpoint for virtually every recent urban riot.

As early as the Kerner Commission report (United States Kerner Commission 1968), the potential social benefits of minority police officers have been recognized. Minority officers may have an advantage when it comes to dealing with problems in predominantly minority neighborhoods, both because of a greater understanding of cultural norms and because of increased community acceptance. It is frequently argued that without the cooperation of community members in reporting crimes and identifying criminals, there may be little that police can do either to prevent crime or punish those who commit crimes (Wilson 1983, Skogan 1986, Skolnick and Bayley 1986, Moore 1992, Akerlof and Yellen 1994). Same-race police may lead to a greater willingness of victims of crime to report offenses to the police, an increased ability to solve cases due to community cooperation, and a reduction in the number of unjustified arrests or police harassment. The implication of such arguments is that matching police patrols to neighborhoods by race or ethnicity may provide social benefits.

On the other hand, if police are more reluctant to arrest suspects of their own race even when the arrest is justified (as might be predicted from research in social psychology, e.g. Crosby, Bromley, and Saxe 1980, Krieger 1995), same-race policing may be less effective in reducing crime than cross-race policing. Furthermore, the possibility of police corruption may increase with same-race policing.[1] Enforcement of illicit contracts between enterprises engaged in criminal activities (e.g. gangs, organized crime, chop-shops) and the police may be easier within a racial or ethnic group. There is ample anecdotal evidence of police-related corruption (Wilson 1968, Knapp Commission 1972, Morton 1993).[2] In fact, widespread corruption was one of the critical factors underlying the initial movement away from politically-based police appointments towards the professionalization of policing (Monkkonen 1992).

In spite of the importance of the issue of race in policing, we are aware of very little relevant academic research. Most of the analysis to date has taken the form of ethnographic research and case studies (Groves and Rossi 1970, Mast 1970, Bordua and Tifft 1971, Skolnick and Bayley 1986, Alpert and Dunham 1987). Although these studies are immensely useful, more systematic quantitative evidence is also needed. The quantitative research that has been done typically has focused on police and community perceptions (Decker and Smith 1980, Lasley 1994), but has stopped short of looking for impacts on tangible social outcomes such as reductions in crime or patterns of arrest.[3] The lone exception that we are aware of is Lott (1997)

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[1] Neither of these points necessarily argues for or against more minority police officers, but they may suggest that the optimal allocation of minority officers is on predominantly white neighborhoods, and vice versa.

Bowles and Garoupa (1997) provide a recent theoretical examination of police corruption.

[2] In addition, there is a burgeoning literature on corruption more generally. Important contributions to this literature include Shleifer and Vishny (1993), Mauro (1995), and Olson (1996).

[3] A separate economic literature documents the generally positive impact of affirmative action policies on the job opportunities of blacks (Ashenfelter and Heckman 1976, Brown 1984, Heckman and Wolpin 1976, Leonard 1984, Leonard 1990, Rodgers and Spriggs 1996), but without a focus on policing.

which focuses on the impact of affirmative action in policing on aggregate crime rates.

In this paper we first develop a simple model for analyzing the impact of race on police and the communities they serve, allowing for the possibility of false arrests, different standards of guilt for making arrests within and across racial lines, and police corruption. As the model demonstrates, different patterns of arrests and crime provide evidence about how race influences policing.

We then analyze empirically the relationship between minority representation in policing, arrest rates, and crime rates. The analysis combines publicly available city-level Federal Bureau of Investigation (FBI) data on crime and arrest rates and data on the racial composition of municipal police departments compiled over the last twenty years by the Equal Employment Opportunity Commission (EEOC). The EEOC data set is far superior in both the breadth of cities covered and the number of years for which data are available relative to data sets used in earlier studies of minority policing (Walker 1983, Hochstedler and Conley 1986, Stokes and Scott 1993). While aggregated statistics have been published by the EEOC, the city-level data have not previously been exploited for scholarly research.

We find evidence across a wide range of crime categories that own-race policing is associated with lower numbers of arrests than cross-race policing. The results with respect to crime rates are less clear-cut and far more sensitive to the choice of specification. Own race policing appears more effective in lowering property crime rates, but does not differ systematically from cross-race policing with respect to violent crime.[4] Taken together, these

[4] As will be discussed in the text, data on crimes committed by race are not available. The source of identification underlying this claim is interactions between the racial composition of the police force and the racial composition of the city population. When a white officer is added, property crime falls more in cities with a greater fraction of whites. When a non-white officer is added, property crime falls more when the city has a largel minority population.

results suggest that for property crime own-race policing is more "efficient" than same-race policing, i.e. similar or better crime outcomes are obtained with fewer arrests required. This implies either that deterrence is greater with own-race police (due possibly to greater community cooperation) or that fewer false arrests are made. The magnitude of our results is substantial. For the mean city in our sample, reallocating police from random assignment by race to an assignment that maximizes own-race policing (holding the number and racial composition of the police constant) is predicted to reduce arrests by more than 10 percent while decreasing property crime by as much as 20 percent. Violent crime is unaffected.

Our findings cannot be easily explained by changes in the crime reporting behavior of victims. In that case, one might expect both the number of arrests and reported crimes to rise with same-race policing (even if true victimization fell). It is possible, however, that omitted variables affecting both crime rates and the propensity to hire police of a given race (e.g. adoption of community policing strategies) help to account for our results. Consequently, we present two stage least squares estimates using the racial composition of a city's fire-fighters as an instrument for the racial composition of the police force.[5] We also investigate the robustness of our results to a range of other specifications.

The outline of the paper is as follows. Section I develops the theoretical model. Section II describes the data sets and empirical strategy in greater detail. Sections III and IV present the empirical results on arrests and crime rates respectively. Section V contains a summary of the findings and considers the broader implications of our results.

[5] If changing racial attitudes drive both changes in the composition of municipal government and crime rates, then the validity of the instruments would be called into question. If, however, any endogeneity in the racial composition of the police force is due to factors specific to the police department (such as adoption of innovative policing strategies), then the instruments are easier to defend.

Section I: A Stylized Model of Race in Policing

In this section, we model the interaction between police, criminals, and the community, allowing for police practices to affect both criminal behavior and the decision of victims to report crimes to the police. Also included implicitly in the modeling is the possibility either of false arrests or police corruption. After presenting the model, we explore the implications of the model with respect to the racial composition of police forces and the impact on arrest patterns, crime rates, and victim reporting rates.

The timing of the actions in the model are detailed in Figure 1. First, a criminal opportunity arises and the potential criminal decides whether or not to commit the crime.[6] If the offense is committed, the victim chooses whether or not to report the crime to the police. If the crime is not reported, the criminal goes unpunished. If the crime is reported to the police, the crime is investigated, leading either to the arrest of the criminal, to a false arrest, i.e. the arrest of someone other than the true offender, or no arrest.

Race enters the model through its impact on the likelihood that a true arrest is made (i.e. the actual criminal) or a false arrest is made (an arrest of anyone other than the actual criminal), conditional on a crime being reported. The frequency with which a reported crime results in either a true or false arrest is assumed to depend on whether the race of the victim and the investigating officer are the same or different.[7] When the race of the victim and the officer are the

[6] For simplicity, we restrict model development to the case of a single criminal faced with the choice of committing a single crime. Extending the model to crimes committed by multiple offenders does not alter the logic, nor does allowing for repeat offenses.

[7] There is nothing intrinsic to this modeling structure that limits consideration to racial differences. The model is equally applicable to consideration of systematic effects of ethnicity,

same, it is possible that there is greater community cooperation in helping to solve the crime, or more knowledge of the community and its cultural norms on the part of the officer, leading to a higher frequency of successful arrests and perhaps fewer false arrests. On the other hand, it may be the case that police have different standards for making arrests within and across racial lines due to tastes or prejudice. It is frequently alleged, for instance, that white officers disproportionately single out non-white youths for harassment. Also, collusive agreements between criminal organizations and police may be easier to enforce within a racial group, resulting in fewer successful arrests due to corruption.

Denoting the likelihood of a true arrest as T and a false arrest as F, the relationship between race and arrest probabilities is expressed formally as follows:

$$T_i = \begin{cases} T_i(P_w, P_n) & \text{if } R = 1 \\ 0 & \text{if } R = 0 \end{cases} \tag{1}$$

$$F_i = \begin{cases} F_i(P_w, P_n) & \text{if } R = 1 \\ 0 & \text{if } R = 0 \end{cases} \tag{2}$$

where the subscript i refers to the race of the criminal, R is an indicator variable equal to one if the crime is reported to the police and zero otherwise, and P_w and P_n are respectively the number of white and non-white police officers per capita. When R=0, both T and F are zero, i.e. no arrest will be made. When R=1, T and F are positive. Increasing the number of police (P_w and P_n) is likely to have a positive impact on true arrests, but may have an ambiguous effect on the number of false arrests. White and non-white police may have different effects on T and F, and the effects

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gender, or class differences.

may vary across the race of the victims.

The decision of a victim about whether or not to report a crime to the police is assumed to be a function of the likelihood that reporting the crime leads to either a true or false arrest which, from equations 1 and 2, depends upon the size and racial composition of the police force:[8]

$$R_i = R_i(T_i(P_w, P_n), F_i(P_w, P_n)) \qquad (3)$$

where i subscripts the race of the victim, and it is assumed that $\partial R_i/\partial T_i > 0$ and $\partial R_i/\partial F_i < 0$. A greater likelihood of a successful arrest increases the incentive to report the crime by increasing the likelihood a victim recovers lost property, and also may provide utility in the form of retribution. Although a greater chance of false arrest does not directly affect the victim financially, false arrests here serve as a proxy for the degree of harassment of the community by the police. If false arrests are common, police-community relations are likely to be strained, leading to less crime reporting.[9] Note that tastes for white or non-white police play no role in the crime victim's decision to report in this model, but that because arrest rates are a function of the racial composition of the police force, the race of police can have an indirect impact on victim reporting behavior.

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[8] While not critical to the model, for simplicity we assume that the race of the victim and the race of the criminal are the same. Data from the National Crime Victimization Survey roughly supports this assumption. White victims of violent crime done by a single offender are four times more likely to report that the offender is white than black. Black victims are eight times as likely to describe the offender as black rather than white.

[9] If more police increase the likelihood of true arrests and decrease the chance of false arrests, then reporting rates will unambiguously rise. Although a priori unlikely, it is at least theoretically possible that a decline in the reporting rate would be observed as police are added if a rise in false arrests overwhelmed the effects of an increase in true arrests. In practice, however, the responsiveness of reporting rates to true arrests is likely to be greater than that of false arrests since a rise in true arrests carries a direct benefit to the crime victim, whereas false arrests only have indirect effects.

Criminals in the model are risk-neutral expected utility maximizers whose decisions about whether to commit crimes depend upon the likelihood of a true arrest, which is a function of victim reporting rates and true arrests conditional on victim reporting:[10]

$$C_i = C_i(R_i(P_w, P_n) * T_i(P_w, P_n)) \qquad (4)$$

where C_i reflects crimes committed by criminals of race i. The product of $R * T$ is the likelihood with which a criminal will be arrested for committing a particular crime.

Based on equations 1-4 , the total number of arrests can be expressed as the following identity

$$A_i \equiv C_i * R_i * (T_i + F_i) \qquad (5)$$

where A_i reflects the total number of arrests of suspects of race i and the other variables are as described above, but with functional dependences omitted. The total number of arrests is equal to the number of reported crimes $(C*R)$ multiplied by the combined arrest rate for both true and false arrests per reported crime $(T + F)$.

Having laid out the elements of the model, it is now possible to examine the impact of changes in the number of officers on the measures of interest, and in particular, focus on the differential impacts of white and non-white police. Because the number of arrests is the crime measure for which the best data are available by race, we focus the analysis on equation 5. Taking the partial derivative of equation 5 with respect to both non-white and white police yields

$$\frac{\partial A_i}{\partial P_n} - \frac{\partial A_i}{\partial P_w} = R_i(T_i + F_i)\left(\frac{\partial C_i}{\partial P_n} - \frac{\partial C_i}{\partial P_w}\right) + C_i(T_i + F_i)\left(\frac{\partial R_i}{\partial P_n} - \frac{\partial R_i}{\partial P_w}\right) + C_iR_i\left(\frac{\partial T_i}{\partial P_n} - \frac{\partial T_i}{\partial P_w}\right) + C_iR_i\left(\frac{\partial F_i}{\partial P_n} - \frac{\partial F_i}{\partial P_w}\right) \qquad (6)$$

[10] Criminal decisions are not affected by the frequency of false arrests in this model, although in the real world it may be the case that a longer criminal record makes it more likely that a false arrest wrongly results in punishment for a crime that the criminal did not commit. This impact, however, is likely to be only of second-order importance.

where equation 6 is the marginal difference in the impact on arrests of suspects of race i from an increase in the number of non-white officers compared to white officers. Four factors help to determine this relationship, corresponding to the four terms in equation 6. The first term reflects the fact that reductions in crime, ceteris paribus, will lead to fewer arrests. Thus, if police of one race are more effective in deterring crime by criminals of race i, then adding police of that race may result in fewer arrests. The second term captures changes in reporting behavior of victims; if more crimes are reported when there are more minority police, then the presence of minority police will be associated with more arrests. The third and fourth terms represent the direct changes in arrests due to differential true arrest and false arrests per crime across officers of different races. In the empirical section that follows, variants on equation 6 will be estimated.

If race-specific estimates of criminal activity were available, one would also want to directly estimate the relationship between crime by race and the racial composition of the police force.

$$\frac{\partial C_i}{\partial P_n} - \frac{\partial C_i}{\partial P_w} = \frac{dC_i}{dT}\left(\frac{\partial T_i}{\partial P_n} - \frac{\partial T_i}{\partial P_n}\right) + \frac{dC_i}{dR}\left(\frac{\partial R_i}{\partial P_n} - \frac{\partial R_i}{\partial P_n}\right) \tag{7}$$

Unfortunately, data on the race of the offender is only available when a crime is solved by arrest. Often the victim does not observe the race of the criminal in crimes such as auto theft or burglary. For those crimes where a victim does identify the race of the suspect, this information is not systematically collected and reported in the available data sets.

There is, however, an indirect means of estimating a race-specific crime impact if one is willing to impose assumptions about the way white and non-white officers are assigned to patrol beats (which in turn dictates the distribution of arrest opportunities). Our baseline assumption is that white and non-white officers are randomly assigned to neighborhoods.[11] If this is the case, then white and non-white officers would each expect to face the same distribution of arrest opportunities. Denote the efficiency with which crimes committed by criminals of race i are reduced with each additional officer of race j as β_{ij}. Allowing crime rates to may systematically differ across race by a proportion γ, and assuming γ is constant over time for a given city, the total impact of police on crime can be modeled as follows:

$$\begin{aligned} Crime = {} & \beta_{ww}(White * P_w) + \beta_{nw}(\gamma(1 - White) * P_w) \\ & + \beta_{wn}(White * P_n) + \beta_{nn}(\gamma(1 - White) * P_n) \end{aligned} \tag{8}$$

where *Crime* is total crime in the city and year, *White* is the fraction of the population that is white, and the subscripts *w* and *n* correspond to white and non-white respectively. All other variables are as defined above. Adding in the appropriate control variables and an error term, equation 8 can be estimated with a city-level panel using only *aggregate* crime data, but will provide separate estimates of the relative impact of white and minority police on white crime (β_{ww} and β_{wn}) and on minority crime (β_{nw} and β_{nn}).[12]

Section II: Data Sources and Estimation Approach

The data set used in this paper is a panel of data containing the 134 U.S. cities with

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[11] Anecdotal evidence suggests that non-white officers may be disproportionately assigned to non-white neighborhoods. If police are not randomly assigned by race, the coefficients still provide information on the impact of the race of officers and citizens on crime, but a scaling factor accounting for the distribution of arrest opportunities must be incorporated in order to interpret the results on a per officer basis.

[12] Note that β_{nw} and β_{nn} cannot be separately identified from γ. Nonetheless, the presence of γ does not create a problem because it is only the relative magnitude of β_{nw} and β_{nn} that is required to assess the relative effectiveness of white and non-white officers.

population greater than 100,000 as of the year 1975. Panel data, with city-fixed effects and time dummies included as controls, are less likely to be adversely affected by unobserved heterogeneity than would cross-sectional data from cities in a given year. The limiting factor on our sample is data on the racial composition of municipal police forces, taken from the EEO-4 survey of governments conducted annually by EEOC since 1973. Working in concert with the technical staff of EEOC, we have obtained access to data for the years 1977, 1981, 1984, 1986, 1989, and 1993. For each department of the local government, the racial and gender composition of the work staff is reported by functional category (e.g. protective services, officials and administrators, administrative support, professionals).[13] Although greater detail on race is available in the data, we limit our analysis in this paper to the broad classifications of white and non-white. The primary motivation for doing so is concern over lack of comparability of the definition of Hispanic across data sources.[14] In some specifications, cities with Hispanic populations greater than ten percent using the Census definition are eliminated as a check on the sensitivity of the results.

We focus our analysis on those members of police departments whose job function is protective service. This definition captures patrol officers, excluding both officers assigned to

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[13] Given the remarkable scope and detail of this data set, it is surprising that it has not been used previously, to the best of our knowledge, in any published academic work. In addition to the data we will use on the racial and gender composition of the workforce, there is also data on the breakdown of new hires, as well as salary information by race, sex, and job function.

[14] Categorization in EEOC data is based on the supervisor's assessment of a worker's classification, with Hispanics treated as their own category, separate from white and black. In the U.S. Census, Hispanic status is treated as an ethnicity rather than a race, i.e. each individual is assigned both a racial classification and is denoted Hispanic/non-Hispanic. Further complicating comparisons is the fact that the definition of Hispanic has changed over time in the Census. Moreover, prior to 1980, Hispanics are not separately broken out in FBI arrest data. In the EEOC data, those classified as Hispanic are treated as non-white.

desk duties as well as supervisors. This categorization is closely related to, but somewhat more restrictive than, the Federal Bureau of Investigation's (FBI) measure of sworn police officers. For the cities in our sample, the raw correlation between sworn police officers, as reported in the FBI's Uniform Crime Reports, and the EEOC measure is .92. The EEOC-reported value is on average 18 percent smaller than the FBI measure. Summary statistics for the EEOC measure of sworn police officers by race, along with all of the other variables used in the analysis, are presented in Table 1.

The only crime variable for which a long panel of racial breakdowns by city is available is arrests. These data are collected by the FBI and are available disaggregated by crime category on an annual basis.[15] Including a relevant set of control variables, the impact of white and non-white officers on white and non-white arrests can consequently be directly estimated.

Unfortunately, equivalent data on crime commission by race are not available annually at the city level. As a consequence, we are forced to make due with aggregate city-level data, indirectly obtaining estimates of the coefficients of interest through the modeling assumptions imposed in the preceding section. The measure of crime used in estimating equations 10 and 11 is per capita crime as proxied by reported crime statistics compiled annually by the FBI in Uniform Crime Statistics.[16] The police variables are once-lagged to minimize the endogeneity

[15] Although the race of the arresting officer is not included in the arrest data, we can still estimate the impact on arrests of adding an additional officer of a particular race using city-level data. Given the endogenous assignment of officers to patrol beats based on race, trying to estimate the impact of the race of the arresting officer through the use of individual-level data would likely raise more difficulties than it would solve.

[16] One important shortcoming of using Uniform Crime Report (UCR) data is that it captures only those crimes that are reported to the police. This is especially unfortunate in light of the model presented earlier, which posits that victim reporting behavior may be a function of the racial composition of the police force.

between police and crime (Cameron 1988, Levitt 1997).

The set of covariates included in the regressions is constrained by the lack of data available on an annual basis at the city level. While some variables, such as city population and the presence of a black mayor, are available annually for cities, in other cases compromises must be made. We attempt to deal with these data limitations in three ways. First, where annual data for larger geographic areas exist, we use the most disaggregated data series available. Thus, SMSA-level unemployment rates, state per capita income, and state measures of the age distribution are included as regressors. Second, where city-level measures are critical, as with the percent black, we linearly interpolate between decennial censuses. Finally, as a substitute for effective covariates, we include year dummies, city-fixed effects, and, in some specifications, region-year interactions using the nine U. S. census regions. These variables absorb much of the variation in the data, particularly for demographic and socio-economic factors which tend to change slowly over time. For instance, year and city dummies alone eliminate over 95 percent of the variation in the demographic variables and over 90 percent of the variation in per capita income.[17] To the extent that other (unmeasured) demographic and socio-economic factors exhibit a similar pattern, the use of these indicator variables will reduce any omitted-variable bias from this source.

Section III: Results of Estimation

Table 2 presents the results from estimation of the relationship between arrest patterns by race of suspect and the racial composition of the police force. The specifications estimated are of the form

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[17] By comparison, roughly 25 percent of the overall variation in the police and crime variables remains after removing year and city-fixed effects.

$$White\_Arrests_{ct} = \beta_1 White\_Police_{ct-1} + \beta_2 Nonwhite\_Police_{ct-1} + X_{ct}\Gamma + \gamma_c + \lambda_t + \varepsilon_{ct} \quad (12)$$

$$Nonwhite\_Arrests_{ct} = \beta_3 White\_Police_{ct-1} + \beta_4 Nonwhite\_Police_{ct-1} + X_{ct}\Phi + \gamma_c + \lambda_t + \varepsilon_{ct} \quad (13)$$

where c indexes cities, t corresponds to years, and X is the vector of economic, socio-economic and demographic controls described above. The arrest variables are arrests in a given crime category per member of the racial classification (e.g. violent crime arrests per non-white resident). The police variables are per capita using the city population as the denominator. This specification assumes that any given individual's probability of arrest is a linear function of the number of white and non-white police per capita (all of the results are also robust to estimation in logs). One advantage of this choice of specification is that the interpretation of the β coefficients does not depend on racial composition of a city's population. The literal interpretation of the β coefficients is the change in the number of arrests when one officer of a given race is added to the police force. Our primary focus is on the difference in β by race of police, i.e. how much do arrests of suspects of a given race change as a function of the racial composition of the police force holding the total number of police constant. The γ and λ terms represent city-fixed effects and year dummies respectively.

The eight columns of Table 2 correspond to results by race of arrestee for four different arrest categories: total arrests (including the three other categories examined, as well as public order offenses, prostitution, drunk driving, and a wide range of other generally minor crimes), property crime (burglary, larceny, and auto theft), violent crime (murder, rape, robbery, and aggravated assault), and drug offenses (both possession and distribution). Almost two-thirds of all arrests are for offenses not covered by the last three categories. Year dummies and city-fixed effects are included in the regressions, but are not reported in the table. All specifications are estimated using weighted least squares with weights proportional to city population.

We turn our attention first to the top two rows of Table 2 which present the parameters of primary interest. The top row contains estimates of the change in the number of arrests of each type with respect to the number of white police officers. The second row presents that same coefficient for non-white officers. Comparing the coefficients in the top two rows of the first column, the addition of white police is associated with a statistically insignificant 0.13 increase in the number of white total arrests per capita, whereas additional non-white officers are associated with a statistically significant increase of 18.5 arrests of white suspects. Our primary interest is in the difference between these two coefficients, rather than the levels themselves. Therefore, the bottom row of the table reports the p-value from a t-test of the null hypothesis that the impact of white and non-white officers is the same. This hypothesis of equality is rejected.

The pattern of coefficients in the second column, corresponding to total arrests of non-whites, is the reverse of that in the first column. Non-white arrests are positively related to the addition of more white officers, but appear to decline with the addition of non-white officers. The null hypothesis of equality across these two coefficients, however, is only rejected at the .10 level.

Parallel results are presented for the other arrest categories in the top two rows of columns 3-8. The regularity of the results across arrest categories is striking. For all four arrests categories, the addition of a non-white officer has a greater impact on white arrests, and every case except drug arrests, each extra white officer is associated with more non-white arrests. The difference is statistically significant at the .05 level in four of the eight cases. In addition, comparing across *rows* rather than *columns* (i.e. looking at whether adding officers of a given race increases arrests more within race or across race), in all eight instances the across-race effect is greater than the within-race effect.

The differences between same-race and cross-race policing are substantively large. Consider the following alternative allocations of police: (1) assign police randomly by race, and (2) assign police to maximize same-race policing, holding the present number and racial composition of police officers constant. Evaluated at the sample means of our data, moving from the first allocation to the second allocation would involve shifting just over one-quarter of all police from cross-race to own-race policing.[18] Based on the coefficients in Table 2, this reassignment of officers would be predicted to yield a decrease in total arrests of 16.0 percent, and declines of 10.4, 17.1, and 11.5 percent in property, violent, and drug arrests respectively. These estimates are likely to be upper bounds on actual efficiency gains, both because minority officers may already be disproportionately assigned to minority neighborhoods in many cities, and because perfect racial matching could never actually be achieved.

Estimates of the other covariates in the regressions appear generally reasonable and consistent with past research. The fraction of non-white residents that are Black (as opposed to Asian or "other") is positively correlated with non-white arrests, implying higher arrest rates of

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[18] In our data set, whites are 78.7 percent of the police force and 64.3 percent of city residents. Thus, if police are randomly assigned by race, 64.3 percent of non-white police interactions (or 13.7 percent (.213*.643) of total police-citizen interactions) will involve non-white officers and white citizens If police are assigned to maximize same-race interactions, then non-white officers will only interact with non-white citizens, necessitating a shift of 13.7 percent of police interactions from non-white officer/white citizen to non-white officer/non-white citizen. An equal number of white officers will have to be shifted in the other direction, meaning that 27.4 percent of officers will be reassigned. If non-whites are more likely to be arrested than whites, then a smaller re-assignment of officers is necessary.

Blacks than other minorities.[19] This variable would not be expected to directly affect white arrests and with one exception is not statistically significant in the odd columns. City population is negatively related to arrest rates across all eight specifications. This result is consistent with both Glaeser and Sacerdote (1997) which documents lower probabilities of arrest in big cities, and Cullen and Levitt (1998) which finds that rising crime rates are associated with urban flight. All else equal, therefore, cities with rising populations tend to have falling crime rates.[20] Arrest rates are generally lower when a Black mayor holds office and, somewhat surprisingly, when unemployment rates are high. Income per capita appears to be positively related to drug arrests, but is not statistically significantly related to any of the other categories. State age shares do not carry a consistent sign (the omitted category is the percent of the population over age 45). This is not particularly surprising given the limited variation in these measures that remains once city and year effects have been removed.

Sensitivity of the Estimates of Differential Impact of Own-race and Cross-race Policing

Because the principal analysis of this paper is based not on the raw police coefficients themselves, but rather the differential between the coefficients on white and non-white officers, many of the standard critiques regarding bias in the estimation are not directly applicable. An omitted variable that leads to similar biases in both the police coefficients would not invalidate

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[19] The category "other" is primarily hispanics. Hispanics are expected to report their race as either White or Black, but many choose "other" instead.

[20] The results of Cullen and Levitt (1998) suggest that causality runs from crime rates to city population changes and not vice-versa. Given that interpretation, including the population variable as regressor in Table 2 may be inappropriate. The results we obtain are not sensitive to excluding the population variable.

comparisons of the relative effects of white and non-white officers. Similarly, the likely possibility that rising crime leads to increases in the size of the police force (Levitt 1997) does not invalidate the relative comparisons, as long as rising crime does not alter the racial composition of police forces.

More generally, however, omitted variables and possible endogeneity of the racial composition of the police force could be distorting the results. If it is the case, for instance, that falling crime rates (or expectations of declining crime) lead police departments to hire more minorities, then there may be a spurious negative relationship between minority police and the number and composition of arrests. Alternatively, changes in policing strategies (e.g. towards community policing) may lead to the hiring of more minority police. It may, however, be the policing strategy, rather than the minority officers that is responsible for a change in the pattern of arrests. Since it is difficult to control for changes in policing strategy, the effect will be mistakenly attributed to the minority police.

While the biases in the preceding paragraph suggest that the effectiveness of minority officers may be exaggerated in OLS regressions, there are other cases in which those results may understate the true impact of minority police. At the beginning of our sample, minorities were greatly under-represented on police forces. Over the period of our sample, the total number of police increased 28 percent, of which nearly all (25/28) was growth in the number of non-white police. To the extent that cities with rising violent crime tend to hire more police (Levitt 1997), there may be a spurious link between rising crime rates and minority police.

Table 3 presents a range of alternative specifications as a means of gauging the sensitivity of our estimates. The columns of Table 3 match those of Table 2. Each row represents a different specification. Only the differences in the own-race and cross-race arrest coefficients are reported (i.e. for odd columns the coefficient on white police minus the coefficient on black police, and for even columns the reverse). A negative value in Table 3 means that arrests are lower with own-race policing than with cross-race policing. The 72 entries in Table 3 (9 rows by 8 columns) represent coefficients from 72 different regressions. Coefficients that are statistically significant at the .05 level are highlighted in boldface.

For purposes of comparison, the top row of Table 3 presents the results of Table 2 as a baseline.[21] The results obtained are robust to a wide range of specifications. Eliminating city-fixed-effects does remarkably little to change the results. Similarly, little changes when all of the covariates are eliminated except year and city dummies, or when region-year interactions (using the nine census regions) are added. The results are more sensitive to the inclusion of city-level trends and the standard errors rise as well. Restricting the sample to cities with both a substantial Black population (>10 percent) and a small non-White, non-Black population has little systematic affect on the results. This suggests that lumping all non-whites into one category and including cities with few minorities in the sample is not greatly distorting the results. The coefficients shrink somewhat when robust regression techniques are used to reduce the influence of outliers. Nonetheless, three of the eight entries remain negative and statistically significant.

The two next-to-last rows of Table 3 present two stage least squares estimates using the racial composition of a city's firefighters as an instrument for the racial composition of the police force.[22] Even after controlling for the racial composition of city residents, the number of *non-*

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[21] For instance in column 1 of Table 3, the value -18.36 corresponds to .13 - 18.50 from the first two rows of Table 2 column 1.

[22] More precisely, our measure of firefighters are those employees of the fire department who are involved in protective service. Firefighters are classified as either white or non-white, as was the case with police.

white municipal firefighters is a good predictor of *non-white* police, but is only weakly correlated with *white* police. Similarly, the number of *white* employees in those other functions is correlated with *white* police, but not with *non-white* police.[23] Due to large standard errors, it is difficult to draw strong conclusions from the two-stage least squares estimates when city-fixed effects are included (the penultimate row). When city-fixed effects are dropped from the two-stage least squares regressions, however, statistically significant negative coefficients are obtained in four of the eight columns.

An important concern in interpreting the 2SLS coefficients is whether the exclusion of firefighters from the crime equation is valid. While one would certainly not expect a direct impact of the composition of the fire department on crime rates, it is possible that a large number of black firefighters is the consequence of other factors about a city that will influence crime, such as good race relations, or a thriving minority community.[24] Under the assumption that the composition of the fire department captures important omitted factors of a city's situation, these variables should be included as controls, not used as instruments. The final row of the table adds the firefighter variables as regressors. The coefficients are similar to those from the baseline specification, suggesting that firefighter composition is not capturing important omitted factors.

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[23] The first-stage regression coefficients are as follows. Each additional white firefighter is associated with a .45 (standard error=.07) increase in white police officers, whereas each non-white firefighter is associated with a -.14 (se=.16) change in white police. When non-white police is the dependent variable, the coefficient on non-white firefighters is .62 (se=.16) and the coefficient on white firefighters is .02 (se=.06). It is worth noting that similar first-stage regressions using jobs that less closely parallel policing, such as garbage collection, streets and highways maintenace, and administration, were not systematically correlated with the composition of the police force.

[24] At a minimum, these instruments should be free of any contamination in the police variables that arises from endogenous manipulation of the police force composition in response either to rising crime rates or changes in policing strategies.

Summarizing Table 3, over four-fifths of the coefficients presented are negative. All 37 of the coefficients that are statistically significant at the .05 level carry a negative sign. The only column for which a statistically significant negative sign is never obtained is for non-white drug arrests.

Section IV: Estimating the Relationship between Own-race and Cross-race Policing and Crime Rates

As noted in the theoretical section of this paper, there are a number of possible explanations for the lower level of arrests that is observed with own-race policing: (1) victim reporting of crime may be lower when the police officers are of the same race as the victim, (2) same-race police officers may be more effective in deterring or solving crime, leading to less crimes being committed and consequently fewer arrests, (3) same-race police may make fewer false/harrassment-based arrests, (4) same-race police officers may have a higher standard of proof for making an arrest when there is room for discretion, or (5) same-race police officers may be less effective in solving crimes, perhaps due to more police corruption Of these five explanations, only the first seems directly at odds with common sense -- precisely the opposite story, i.e. increased reporting of crime by victims to officers of the same race, would be expected.

By analyzing the impact of white and non-white police on white and non-white crime, it may be possible to distinguish between some of these alternative explanations. If same-race police are more effective in deterring crime, then fewer same-race arrests will be accompanied by lower crime rates than would be the case with cross-race policing. If same-race police make fewer false arrests, but are otherwise no different than cross-race police, than arrest rates will be lower, but crime will be unaffected. The other two scenarios (higher standard of proof and corruption) predict that lower arrest rates will be accompanied by *rising* crime since expected punishments will be lower.

Table 4 examines the relationship between racial composition of the police force and crime rates by race. Estimation is based on equation 10 of the theoretical model, employing the same panel data set, estimation methods, and control variables used earlier. The precise estimating equations are variations on

$$
\begin{aligned}
Crime_{ct} = {} & \theta_1(White\_Police_{ct-1} * \%White_{ct}) + \theta_2(Nonwhite\_Police_{ct-1} * \%White_{ct}) \\
& + \theta_3(White\_Police_{ct-1} * \%Nonwhite_{ct}) + \theta_4(Nonwhite\_Police_{ct-1} * \%Nonwhite_{ct}) \\
& + X_{ct}\Gamma + \gamma_t + \lambda_t + \varepsilon_{ct}
\end{aligned} \qquad (15)
$$

In interpreting Table 4, it is important to bear in mind the caveat that a direct measure of crime by race is not available. Identification of the model is based on the assumption that in cities with large minority populations, a greater fraction of a minority police officer's dealings with criminals will be with minorities, and similarly for white officers in cities with a larger white population share. By using interaction terms, it is possible to separately identify the differential impact that police of each race have on crime committed by race. If θ_2 is more negative than θ_1, then this suggests that an additional non-white officer is associated with a greater reduction in white crime than an additional white officer. As an informal check on the validity of this indirect approach, we also report estimates using arrest rates as the dependent variable. For arrests, we do have breakdowns by race. Thus, we can compare the results of the indirect approach to those from the direct estimation in the preceding section. To the extent that similar results are obtained, our confidence in the indirect approach increases.

Each column of Table 4 corresponds to a different dependent variable. The first four columns are the arrest variables, presented as a check on the indirect identification approach. The final two columns reflect property and violent crime. Because there are no measures of the frequency of "victimless" crimes such as drug dealing or prostitution, it is not possible to analyze either drug crimes or total crimes in this framework. The interaction terms between race of police and race of the populations are presented in the top four rows. For simplicity, the interpretation of the interaction term (e.g. white police on whites) is listed rather than the interaction term itself (e.g. white police * percent of the city population that is white). The first four columns with arrests as the dependent variables provide somewhat mixed support for the validity of the indirect identification strategy. In columns 1, 2 and 4, the same pattern of coefficients are obtained as is the case using the direct identification of Tables 2 and 3. For violent arrests, however, the pattern of coefficients is the reverse of Table 2.[25] Thus, there is mixed support for the validity of the indirect identification strategy and caution is required in interpreting results using this approach.

That caveat in mind, the results for property crime in column 5 are striking. For both whites (rows 1 and 2) and non-whites (rows 3 and 4), same-race policing is associated with greater reductions in crime rates. Adding a white officer to a city that is entirely white is associated with decline of almost nine property crimes; adding a white officer to a city with no

[25] Moreover, the magnitude of the cross-race differences in the coefficients varies substantially using the direct and indirect approaches. For total arrests and property crime, the indirect approach yields a greater reduction in arrests with own-race policing; the opposite holds for violent arrests and drug arrests. Taking a simple mean of the predictions across the four categories for the scenario shifting police from random assignment by race to maximizing own-race policing, the direct approach yields a reduction of 13.8 percent, the indirect approach predicts a reduction in arrests of 5.2 percent.

white residents is associated with an increase of almost four property crimes. For non-white police, an even more extreme differential exists. For violent crime, on the other hand, no clear pattern emerges.[26] White police are associated with slightly better impacts on crime than non-white officers regardless of the race of the citizens, but in neither case are the results statistically significant at the .05 level. Declining city populations are associated with rising crime, as are Black mayors, and (for property crime only) high unemployment rates and low incomes.

The coefficients obtained are again substantively large. Carrying out the same thought experiment as the preceding section (reallocating police from random assignment to maximizing own-race policing holding the number and composition of the police force constant), property crime is predicted to decline by 22.5 percent and violent crime is essentially unchanged (up by 0.2 percent). As noted earlier, this is likely to be an upper bound on the actual gains that could be realized from reallocating police by race.

Table 5 presents a sensitivity analysis of the results for crime rates using the same set of alternative specifications employed in Table 3. The coefficients reported in Table 5 are differences between own-race and cross-race policing (e.g. in column 1, the value reported is white police on white crime minus non-white police on white crime). A negative coefficient implies that own-race police are more effective in reducing crime. For property crime, 18 of the 20 coefficients are negative and 8 of these are statistically significant at the .05 level. For violent crime, the results continue to be mixed, with own-race policing generally appearing beneficial

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[26] The lack of a clear negative relationship between police and crime observed in Table 4 is typical of OLS regressions due to endogeneity of the police force. It is worth noting, however, that instrumenting with the number of firefighters leads to negative coefficients on the police variables for both property and violent crime (not shown in tabular form). The magnitude of the coefficients is consistent with Levitt(1997).

with whites but not with non-whites.

The combined findings on property crime of lower arrests and lower crime with own-race police are consistent with only one of the five hypotheses posed, namely greater deterrence.[27] The mixed results for violent crime make it difficult to clearly distinguish between alternative hypotheses.

<u>Section V: Conclusions</u>

This paper analyzes the role of race in policing, first developing a theoretical model, and then estimating that model using panel data for 134 large U.S. cities. The most striking finding of the paper is that the addition of officers of a given race is associated with an increase in the number of arrests of suspects of a different race, but has little impact on same-race arrests. The evidence for differential impacts on crime are less clear, although there is some evidence that same-race police lead to a greater reduction in property crime. Taken together, these findings point to the conclusion that same-race policing is potentially more "efficient" than cross-race policing, at least for property crime. In other words, a given number of officers will have a greater impact on crime while requiring fewer arrests, if deployed in an own-race setting.

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[27] An important assumption upon which this analysis is premised is that police officers are randomly assigned to neighborhoods by race If there is non-random assignment, then it is likely that non-white officers are disproportionately assigned to non-white neighborhoods. If that is the case, then an increase in minority representation on a police force may lead to a disproportionate increase in the police presence in minority neighborhoods. This would suggest that increases in non-white officers should be associated with especially large declines in minority crime, and more white officers should have a greater effect on white crime. While this scenario is consistent with the results we obtain with respect to crime, it cannot explain why arrests fall with own-race policing unless deterrence is the underlying force (McCormick and Tollison 1984).

Extrapolating from the coefficients obtained, moving from random assignment of officers by race to a scenario in which own-race policing is maximized, arrests are predicted to fall by 10-20 percent and property crime is predicted to fall by roughly 20 percent with violent crime unaffected. We urge great caution in interpretation of the crime effects, however, since the evidence presented on that issue is indirect and subject to many caveats.

To the extent that our results are true, they provide some indirect empirical support for an efficiency rationale for affirmative action programs in policing. In virtually every large U.S. city, minorities are under-represented on police forces relative to the population. Increasing minority representation on the police so as to mirror the makeup of the city (or more accurately, the makeup of the arrestee population) coupled with reallocation of police patrols to maximize own-race policing, would be expected to increase the efficiency of policing, at least for property crime.

Figure 1: Timing of the Model

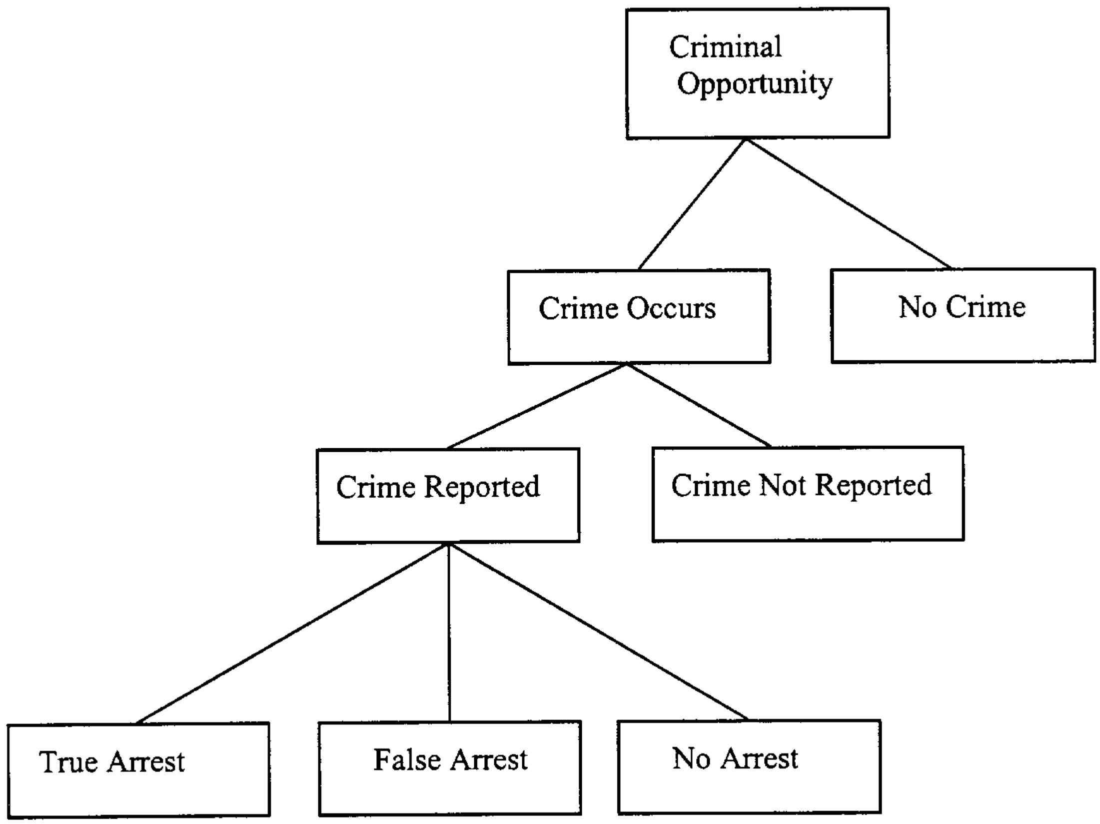

Table 1: Summary Statistics

Variable	Mean	Standard Deviation	Minimum	Maximum
Violent crime arrests per capita (whites)	.0026	.0018	.0001	.0120
Violent crime arrests per capita (non-whites)	.0085	.0038	.0016	.0272
Property crime arrests per capita (whites)	.0090	.0043	.0016	.0353
Property crime arrests per capita (non-whites)	.0174	.0089	.0018	.0581
Drug crime arrests per capita (whites)	.0048	.0036	.0001	.0333
Drug crime arrests per capita (non-whites)	.0074	.0054	0	.0391
Total arrests per capita (whites)	.0524	.0212	.0109	.2185
Total arrests per capita (non-whites)	.0872	.0466	.0074	.3727
White police per capita (x1000)	1.70	.69	.28	4.98
Non-white police per capita (x1000)	.46	.33	0	2.48
White firefighters per capita (x1000)	.94	.43	.13	2.81
Non-white firefighters per capita (x1000)	.19	.15	0	1.55
Violent crime per capita	.013	.007	.001	.044
Property crime per capita	.076	.019	.032	.155
Percent white (city population)	.643	.147	.163	.972
Percent black (city population)	.250	.163	.005	.806
Percent other (city population)	.106	.089	.003	.376
Black as a fraction of all non-white	.67	.26	.05	.99
Black mayor	.194	.395	0	1
SMSA unemployment rate (x100)	.068	.022	.017	.208
State income per capita (1993 dollars)	19,430	2,828	11,761	28,312
City population	1,711,032	2,242,702	100,024	7,322,564
Percent state population age 0-17	.274	.025	.222	.373
Percent state population age 18-24	.123	.011	.094	.154
Percent state population age 25-44	.296	.029	.233	.358

Notes: Data correspond to six years of observations (1977, 81, 84, 86, 89, 93) for 134 U.S. cities with population greater than 100,000 in 1975. Due to missing data on arrests and crime rates, actual number of observations varies between 586 and 600. City population breakdowns are linearly interpolated between decennial censuses. The reported means and standard deviations are weighted by city population.

Table 2: Racial Composition of the Police Force and the Pattern of White and Non-white Arrests

	Total Arrests		Property Arrests		Violent Arrests		Drug Arrests	
Variable	White	Non-white	White	Non-white	White	Non-white	White	Non-white
White Police	.13 (2.67)	9.16 (4.54)	.26 (.43)	.80 (.84)	-.32 (0.28)	1.57 (0.51)	-.64 (.50)	-.06 (.97)
Non-white Police	18.50 (3.96)	-7.53 (8.27)	1.05 (.63)	-2.72 (1.02)	1.21 (.50)	.38 (.62)	2.21 (.69)	.56 (1.57)
Black population/non-white population	-.071 (.034)	.130 (.050)	-.004 (.005)	.039 (.007)	-.005 (.004)	.010 (.004)	.006 (.007)	.029 (.012)
log(city population)	-.011 (.008)	-.031 (.014)	.0008 (.0002)	-.0036 (.0025)	-.0002 (.0006)	-.0050 (.0012)	-.0004 (.0015)	-.0007 (.0020)
Black mayor	-.0088 (.0049)	-.0280 (.0096)	.0003 (.0005)	.0004 (.0007)	-.0002 (.0002)	-.0008 (.0005)	.0002 (.0005)	-.0011 (.0008)
SMSA unemp. rate	-.180 (.074)	-.225 (.137)	-.009 (.009)	.015 (.014)	-.004 (.004)	-.012 (.009)	-.010 (.012)	-.022 (.021)
Percent state pop. age 0-17	.411 (.196)	-.273 (.335)	.050 (.033)	-.010 (.045)	.033 (.017)	-.049 (.026)	.187 (.034)	.061 (.057)
Percent state pop. age 18-24	-.100 (.306)	-1.282 (.684)	.006 (.047)	-.101 (.081)	.074 (.023)	.005 (.047)	.272 (.057)	.121 (.092)
Percent state pop. age 25-44	-.307 (.317)	-.336 (.518)	.034 (.049)	.073 (.078)	.033 (.024)	.099 (.056)	.016 (.058)	-.002 (.087)
State income per capita ($x10^6$)	.016 (.017)	-.047 (.421)	.009 (.023)	-.014 (.035)	-.006 (.012)	-.031 (.025)	.063 (.029)	.096 (.053)
Adjusted R-squared	.780	.831	.857	.894	.826	.800	.727	.636
P-value: white police = non-white police	<.001	.098	.32	.010	.012	.19	<.010	.75

Notes: Dependent variable (in per capita terms) is listed at the top of each column. Year dummies and city-fixed effects are also included in all regressions. The data set is a panel of six years of observations between the years 1977-1994 for all U.S. cities with a population greater than 100,000 in 1975. The police variables are once lagged. The number of observations ranges between 586 and 600 due to occasional missing data. The p-values reported are from a t-test of the equality of the white and non-white police coefficients. The estimation method is weighted least squares using city populations as weights. White heteroskedasticity robust standard errors are in parentheses.

Table 3: Sensitivity Analysis of Arrests Rates by Race and Racial Composition of the Police Force

Differential between own-race and cross-race arrest coefficients

	Total Arrests		Property Arrests		Violent Arrests		Drug Arrests	
Specification	White	Non-white	White	Non-white	White	Non-white	White	Non-white
Baseline	**-18.36** **(5.06)**	-16.70 (10.05)	-0.69 (0.79)	**-3.52** **(1.36)**	**-1.53** **(0.61)**	-1.19 (0.90)	**-2.86** **(0.78)**	0.63 (1.90)
No city-fixed effects	**-9.88** **(4.64)**	**-22.47** **(9.51)**	-0.65 (0.73)	**-7.42** **(1.82)**	**-1.25** **(0.34)**	-1.59 (0.94)	**-1.55** **(0.65)**	1.21 (1.65)
No economic, socio-economic, demographic covariates	**-18.67** **(5.82)**	-16.76 (10.93)	-0.63 (0.76)	**-3.80** **(1.62)**	**-1.91** **(0.50)**	0.02 (0.97)	**-4.05** **(1.34)**	3.05 (2.14)
Add region-year interactions	**-24.53** **(4.99)**	-6.54 (9.54)	-0.88 (0.80)	-2.38 (1.25)	**-1.62** **(0.48)**	-1.10 (0.95)	**-2.76** **(0.81)**	1.26 (1.99)
Add city-specific trends	**-15.88** **(7.04)**	19.21 (13.94)	0.49 (0.98)	-1.06 (2.26)	0.79 (0.48)	**-4.09** **(1.29)**	-0.41 (1.30)	-0.54 (2.20)
Restricted sample:black>10% and other non-white<10%	**-11.62** **(5.92)**	**-23.26** **(11.26)**	**-1.96** **(0.88)**	-2.20 (1.62)	**-1.08** **(0.45)**	-1.60 (1.21)	**-2.42** **(0.83)**	-0.07 (2.41)
Robust regression	-6.05 (3.80)	**-17.64** **(6.12)**	0.07 (0.70)	**-3.90** **(1.45)**	0.26 (0.19)	-0.18 (0.73)	**-2.05** **(0.52)**	1.77 (0.91)
Two stage least squares (baseline specification)	-30.19 (18.94)	50.40 (47.84)	-1.77 (2.58)	-5.39 (4.37)	**-2.25** **(1.10)**	5.42 (3.05)	-0.60 (3.00)	12.56 (6.62)
Two stage least squares (no city-fixed effects)	-3.04 (8.78)	**-40.43** **(16.74)**	**-3.51** **(1.16)**	**-9.25** **(2.83)**	0.42 (0.51)	**-2.96** **(1.33)**	-0.95 (0.96)	3.00 (1.82)
Add instruments as controls in baseline specification	**-17.85** **(5.69)**	**-27.86** **(.421)**	-0.85 (0.79)	**-3.24** **(1.44)**	**-1.58** **(0.66)**	**-2.23** **(0.97)**	**-3.52** **(0.88)**	-1.42 (2.14)

Notes: Values in the table reflect the difference between own-race and cross-race coefficients in regressions where arrest rates are the dependent variables. A negative sign implies that arrests are lower with own-race policing. Each entry in the table represents a different regression. The top row corresponds to the specifications reported in Table 2. Other rows differ from the baseline specification as noted in the table. In the two stage least squares estimates, the racial composition of city firefighters is used as an instrument for the racial composition of the police force. The estimation method is weighted least squares using city populations as weights. Standard errors are in parentheses. Coefficients that are significant at the .05 level are in boldface.

Table 4: Crime Rates, Arrest Rates and the Racial Composition of the Police Force: Estimates using City-Level Aggregates

	Total arrests per capita	Property arrests per capita	Violent arrests per capita	Drug arrests per capita	Property crime per capita	Violent crime per capita
White police on whites	-3.39 (6.78)	-2.02 (.96)	.45 (.47)	.50 (1.16)	-8.83 (4.97)	.26 (1.25)
Non-white police on whites	40.55 (12.27)	3.88 (1.73)	-2.56 (.85)	3.21 (2.10)	24.81 (9.00)	4.90 (2.26)
White police on non-whites	13.17 (11.42)	4.97 (1.61)	.42 (.79)	-2.95 (1.96)	3.70 (8.37)	-2.30 (2.10)
Non-white police on non-whites	-8.38 (8.42)	-4.08 (1.19)	3.27 (.58)	1.43 (1.44)	-20.53 (6.18)	2.45 (1.55)
Percent black city population	.020 (.060)	.002 (.008)	.000 (.004)	.017 (.010)	.042 (.044)	-.015 (.011)
Percent other non-white city population	-.014 (.046)	-.020 (.007)	-.004 (.003)	.003 (.008)	-.105 (.034)	.005 (.009)
log (city population)	-.022 (.009)	-.001 (.001)	-.002 (.001)	-.004 (.002)	-.020 (.007)	-.006 (.001)
Black mayor	-.019 (.003)	.0005 (.0004)	-.0005 (.0002)	.0001 (.0005)	.0069 (.0020)	.0026 (.0005)
SMSA unemp. rate	-.220 (.050)	-.006 (.007)	-.009 (.004)	-.017 (.009)	.098 (.037)	-.002 (.009)
Percent state pop. age 0-17	.109 (.179)	.027 (.025)	.012 (.012)	.122 (.031)	.213 (.131)	.031 (.033)
Percent state pop. age 18-24	-.462 (.278)	-.061 (.039)	.068 (.019)	.204 (.048)	.119 (.204)	.069 (.051)
Percent state pop. age 25-44	-.203 (.281)	.051 (.040)	.075 (.020)	-.003 (.048)	.725 (.206)	.110 (.052)
State income per capita (x10^6)	-.058 (.130)	-.018 (.018)	-.017 (.009)	.075 (.022)	-.265 (.095)	-.017 (.024)
Adjusted R-squared	.795	.813	.874	.710	.791	.887
P-value: white and non-white police have same impact on whites	.002	.003	.002	.26	.001	.07
P-value: white and non-white police have same impact on non-whites	.15	<.001	.006	.09	.03	.08

Notes to Table 4: Dependent variable (in per capita terms) is listed at the top of each column. Equations estimated are variations on equation 11. Sample used is the same as that described in earlier tables. The p-values reported are from t-tests of the equality of white and non-white police oficers on white and noon-white crime or arrests. The estimation method is weighted least squares using city populations as weights. White heteroskedasticity robust standard errors are in parentheses.

Table 5: Sensitivity Analysis of Crime Rates to Racial Composition of the Police Force

Values reported in table are own-race crime coefficient minus cross-race crime coefficient

	Property Crime		Violent Crime	
Specification	White	Non-white	White	Non-white
Baseline	**-33.64** **(10.22)**	**-24.23** **(10.95)**	-4.64 (2.55)	4.75 (2.75)
No city-fixed effects	-2.37 (8.96)	12.16 (10.79)	-1.31 (2.13)	3.63 (2.59)
No economic, socio-economic, demographic covariates	-10.41 (10.21)	-2.40 (10.73)	**-5.22** **(2.44)**	2.72 (2.65)
Add region-year interactions	-19.10 (9.80)	**-24.41** **(10.84)**	-4.91 (2.62)	1.63 (2.88)
Add city-specific trends	-31.34 (16.61)	**-58.61** **(17.16)**	5.76 (3.67)	-4.22 (3.79)
Restricted sample:black>10% and other non-white<10%	-21.33 (10.97)	**-26.93** **(13.60)**	-1.89 (3.11)	6.22 (3.88)
Robust regression	**-25.78** **(8.79)**	**-29.41** **(10.14)**	**-3.51** **(1.75)**	-1.62 (2.01)
Two stage least squares (baseline specification)	-22.29 (26.64)	6.84 (18.97)	**-15.36** **(7.07)**	**17.33** **(4.96)**
Two stage least squares (no city-fixed effects)	**-36.10** **(17.29)**	-2.51 (15.21)	-0.57 (2.33)	4.54 (2.98)
Add instruments as controls in baseline specification	-11.96 (10.61)	-15.18 (12.92)	0.30 (3.00)	0.27 (2.73)

Notes: Dependent variable in columns 1 and 2 is property crime, and in columns 3 and 4 it is violent crime. A negative sign implies that arrests are lower with own-race policing. Each entry in the table represents a different regression. Coefficients in each row of columns 1 and 2 are from the same regression, and similarly for columns 3 and 4. The sample matches that used in previous tables. The top row corresponds to the specifications reported in the first two columns of Table 4. Other specifications deviate from the baseline regression in the manner noted. In the two stage least squares estimates, the racial composition of city firefighters is used as an instrument for the racial composition of the police force. The estimation method is weighted least squares using city populations as weights. Standard errors are in parentheses. Coefficients that are significant at the .05 level are in boldface.

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