

DOES A HELPING HAND PUT OTHERS AT RISK?: AFFIRMATIVE ACTION, POLICE DEPARTMENTS, AND CRIME

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Will increasing the number of minority and women police officers make law enforcement more effective by drawing on abilities that have gone untapped and creating better contact with communities and victims? Or will standards have to be lowered too far before large numbers of minorities and women can be hired? Using cross-sectional time-series data for U.S. cities for 1987, 1990, and 1993, I find that hiring more black and minority police officers increases crime rates, but this apparently arises because lower hiring standards involved in recruiting more minority officers reduces the quality of both new minority and new nonminority officers. The most adverse effects of these hiring policies have occurred in the areas most heavily populated by blacks. There is no consistent evidence that crime rates rise when more women are hired, and this raises questions about whether norming tests or altering their content to create equal pass rates is preferable. The article examines how the changing composition of police departments affects such measures as the murder of and assaults against police officers. (JEL J72, K14, H42)

I. INTRODUCTION

Using preferential standards to aid minority groups is frequently justified as rectifying past wrongs. Yet, since *Richmond v. Croson Co.* [1989],¹ the U.S. Supreme Court has held that these preferences must pass the

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1. See also Epstein [1992, 429–33]. Coate and Loury [1993] provide an important discussion on the costs and benefits of affirmative action policies. They rigorously list out conditions under which these policies will break down negative stereotypes and those cases where they will make them even worse.

2. Adarand overturned the decision in *Metro Broadcasting, Inc. v. Federal Communications Commission*.

difficult “strict scrutiny test” and will be invalidated unless they promote a “compelling” governmental interest. Correcting “societal discrimination” was not viewed as a compelling interest. Remedial efforts to rectify past discrimination will only be approved if narrowly tailored to correct specific instances of discrimination. The question of what goals constitute a sufficiently “compelling” interest has never been clearly specified by the Supreme Court, though in a very closely decided case it reversed its own past decision that Federal Communication Commission (FCC) allocation of licenses by race is acceptable to promote diversity in entertainment and news programming and applied these high standards of strict scrutiny and “compelling” interest to federal building projects.²

The standards set by the Supreme Court in *Richmond* and *Adarand* were motivated by the desire that “The [strict scrutiny] test also ensures that the means chosen ‘fit’ this

ABBREVIATIONS

FBI: Federal Bureau of Investigation
FCC: Federal Communication Commission
LEMAS: Law Enforcement Management and Administration Statistics

compelling goal so closely that there is little or no possibility that the motive for the classification was illegitimate racial prejudice or stereotype."³ One can hypothesize what compelling goals would meet these standards where there is "little or no possibility" that an ulterior race-based motive might be the true motivation behind an affirmative action rule, but the most obvious case would be when the racial preferences actually help to further the central purpose of the governmental agency.⁴ In the case of police, this means that minority police officers are being employed not because diversity is intrinsically valued but because it is believed to help lower the crime rate.

The potential law enforcement advantages from multiracial or female officers seem obvious. Minority police officers may be more effective in minority areas simply because residents could be more forthcoming about information that will lead to arrests and convictions or because of the officers' ability to serve as undercover agents. Trust is also important for other reasons, as reports of riots erupting after white police officers have shot a black man may attest.⁵ Officers from a

3. *Adarand*.

4. A distinction must be drawn between two different types of affirmative action programs: quotas and preferential treatment. While preferential treatment already must meet a very high threshold to be approved, the requirements are if anything even more difficult for quotas. "It is doubtful that even a federal law establishing an affirmative action racial classification would be upheld if the law used a racial quota system" (Nowak and Rotunda [1995, 695]).

5. For example, in 1996 riots erupted in St. Petersburg Florida, after a white police officer shot and killed an 18-year-old black man driving a stolen car and in Leland, Mississippi, after a white police officer fatally shot a black businessman named Aaron White ("Kissimmee chief wants riot gear for police: The city should learn from St. Petersburg's riots, John Sutphin said," *Orlando Sentinel*, Saturday, April 26, 1997, p. 1, and *Bartholomew Sullivan*, "Shooting death prior to Leland riot ruled accidental," *Commercial Appeal* (Memphis, Tenn), Friday, April 18, 1997, p. A15). Further back "In 1980, one of the worst recent U.S. race riots erupted in Liberty City and spread through Miami after an all-white jury acquitted white police officers accused of killing a black man" (Angus MacSwan, "Drug gangs rule, children suffer in Miami's Liberty City," *Reuters World Service*, Friday, February 14, 1997). Of course, probably the worst recent riots occurred in 1992 after white police officers were found not guilty in the Rodney King beating. On the other hand, having a racially diverse police department does not guarantee that these riots will be prevented. The Los Angeles Police Department's share of blacks very closely matched the city's.

community may also be better at understanding the behavior of criminals in those areas or even something as basic as understanding the language of immigrants.⁶ In any event, police efforts to reduce crime are surely dependent on the help that they receive from the community (Wilson [1983]).

Rape victims or women abused by their spouses plausibly find it easier to discuss the traumatic events with women officers. Without female officers, many attacks against women may go undetected—thus lowering the expected penalty from attacking women and resulting in even more attacks. Policing is a rare case where the government output is likely to be advanced by race- or sex-based preferences. Indeed, reducing reliance on cognitive tests for police entrance examinations has been justified with the motivation that "police departments cannot function effectively in minority neighborhoods when virtually all police officers are white males" (Dunnette et. al. [1996]).

Another case might be education, where a frequently made claim is that a diverse student body better prepares students for a "diverse world."⁷ These goals have also been used to justify weighting applicants by race or sex along with their test scores. By contrast, how people use roads or machines seems likely to be unrelated to the race of those who built them. Even the case of fire departments, obtaining racial diversity seems tangential to the ultimate goal of extinguishing a fire.

Although the foregoing benefits are clear, there are countervailing factors that must be taken into account. Most important is whether explicit race or sex preferences result in less-capable individuals being hired. For women, this might result because of

6. Community leaders frequently claim that "We want police who know the community. We want them to spend time and become part of the community." (Quote from Dennis L. Chinn, founder of the Asian Plaza Youth Foundation, as reported by Phat X. Chiem [1995, B1]. The same article reports on the importance of having bilingual officers.

7. For example, see Katyal [1995] and Keohane [1995].

less-stringent physical requirements.⁸ Slower running speed might make it more difficult for women to catch criminals.⁹ Weaker physical strength might cause police departments to substitute away from single officer patrol units (either foot or car) and into units with two officers. If criminals believe that they have a greater chance of resisting arrest when officers are weaker, more assaults may be committed by criminals against women officers. In compensating for their weaker strength, women may substitute into other ways of controlling criminals—the most obvious method being guns. Although guns are a “great equalizer,” they may not completely

8. Testing of the physical strengths of men and women public safety employees consistently finds large differences. These studies indicate that “women’s strength rang[es] from 44 to 68% of men’s in the upper body and 55 to 82% in the lower body” (Landy [1992, 4–56]). The norming adopted by most police departments for physical fitness tests creates equal probabilities for passing by men and women (Flannery, [1995, 2]). The same types of rules are adopted by the military where “women recruits must run two miles in 18 minutes, 54 seconds, which is three minutes slower than the required time for men. [Women] must do 18 push-ups in two minutes and 50 sit-ups in two minutes, while men must do 42 push-ups and 52 sit-ups in the same time.” Tom Collingwood, a consultant on physical testing standards in Dallas, estimates that between 70% and 80% of police departments explicitly use norming of physical standards in their hiring practices. However, he believes that most of the departments that use objective standards do not enforce these rules. Women who fail to meet the absolute standards during academy training are unlikely to be failed out of the program. This belief was confirmed by conversations with other experts in this area (e.g., Mike Bahrke at Fitforce in Champaign, Illinois). This creates a difficult problem for testing the impact of norming physical standards because it implies that all cities really have the same standards whether they explicitly claim so or not (See also Bahrke and Hoffman [1997]). Courts have also disallowed other types of tests that produce differential pass rates between men and women. For example, in a 1980 case involving the Philadelphia Police Department, the district court ruled that it was unlawful to discharge women who “failed to achieve a passing score on the firearms qualifying test” (499 F. Supp. 1196).

9. The New York City Police Department is said to illustrate this point. “The department abandoned all physical screening of applicants in the ‘80’s out fear of lawsuits by minority applicants and women. Some officers hired under relaxed testing lack the strength to pull the trigger on a gun,” said Michael Julian, former NYPD chief of personnel. “There are hundreds, if not thousands, of police officers on the streets today who, when a suspect runs from them, have no other option than to call another cop, because they do not have the physical ability to pursue them,” Julian said” (Marzulli and Lewis, [1997, 7]).

offset differences in strength.¹⁰ Being less able to fall back on their physical strength to protect themselves when faced with a possible attack, women may have to determine whether they will fire their gun before the possible attacker gets into physical contact with them. If true, shorter reaction times risk resulting in more accidental shootings.

Although the U.S. Department of Justice states that the appropriate testing procedures nearly eliminate disparate impact while improving merit hiring (Gottfredson [1997]),¹¹ critics of affirmative action in policing argue that these tests lower reliance on important cognitive skills. According to a 1993 survey of 23 large police and sheriff departments (conducted for the Department of Justice and Nassau County, New York), the cognitive portion of police tests have been completely removed in three cases, in an attempt to increase minority recruitment. Even the remaining 20 had reduced their emphasis on cognitive skills, with all the respondents indicating “that adverse impact was considered when determining the selection process” (Dunnette et. al. [1993, 18]). Using this survey to help justify its decision, Nassau County removed all cognitive tests except for a reading comprehension test, which is graded pass-fail and requires that “applicants had to score only as well as the bottom 1% of current police officers.” The Louisiana State Police replaced a cognitive exam with a test that initially contained six parts: three personality, one biographical, and two cognitive,

10. A gun might not be as much of an equalizer for female officers as it is for women who use a gun defensively. Officers are frequently called on to have physical contact with the criminals that they are pursuing, whereas women who use a gun defensively merely use the gun to keep a threatening person at bay.

11. Some testing consultants back up the Department of Justice’s position, and note the different ways that questions can be worded which will hurt minority applicants. In particular, the use of double-negatives, homonyms, questions reflecting middle-class experiences, or “complex sentence structures toward the end of an exam” all work to lower minority scores (Wilson, [1996, A]). President Clinton’s recent nominee as assistant attorney general for civil rights (Bill Lann Lee) argues that “admission standards for schooling “may not disproportionately exclude members of any race, ethnicity, or gender” unless “justified by an educational necessity and no less discriminatory but equally effective alternatives to the practice exist.” Lee argued that “[University of California] cannot demonstrate any educational necessity” for standardized tests. (Clint Bolick, “A Vote for Lee Is a Vote for Preferences,” *Wall Street Journal* October 27, 1997, p. A23).

but later threw out one of the cognitive sections to further reduce the impact on minorities (Price [1997]).¹² After spending "\$5.1 million to have consultants develop unbiased exams, only to have minorities fare poorly again," Chicago moved to a heavily weighted seniority system for promoting police officers and a lottery system for hiring firefighters (Spielman [1996, 16]).¹³ The Department of Justice has used legal action (or so-called consent decrees) to force police departments to adopt these rules.

Some academics have charged that the new tests are consciously designed "to work little better than simply picking applicants at random" so that the pass rate is the same across different racial groups (Gottfredson [1997, 1996]). If minority applicants with low cognitive skills are hired and if these skills predict how good a police officer a candidate would be, preferential treatment adversely affects the effectiveness of police departments. Indeed, some shocking reports have been made about the importance of cognitive skills. Expressing concerns about the poor English skills of new police recruits, a *Washington Post* editorial [1993] claimed that "between 1986 and 1990, 311 of the 938 murder cases the D.C. police brought to the U.S. attorney's office—roughly a third—were dismissed. . . . One local prosecutor says many D.C. cases were thrown out because prosecutors couldn't read or understand the

12. The Louisiana case provides a good example of how these cases work. As part of an agreement with the Department of Justice, the Louisiana State Police agreed "to set aside \$1 million to pay African Americans who failed the test and hire new troopers from among qualified African Americans who failed the test" (Shinkle, 1996, B1-B2). The test that was developed by the Cooperative Personnel Services, Inc., had been used in other jurisdictions where it had been upheld as not discriminating against minority applicants by a federal judge in a Torrance, California case. The Louisiana State Police "denied the allegations of discrimination, but agreed to settle the case with the federal government 'to avoid the burdens of contested litigation.'" The Department of Justice pointed to the "disparate impact" that the test was having on blacks and that the test was not job-related. From August 1991 to May 1996, "Of the 2,721 white applicants who took the test, 66 percent passed; of the 1,293 African Americans who took the test, just 25 percent passed."

13. The number of people participating in the lottery is to be adjusted so as to ensure that enough minorities are found in the pool from which the new hires will be chosen (Kass and O'Connor, [1995, A1]). Other stories on the affirmative action process and its consequences in Chicago are provided by Martin (1997, A4) and Oclander (1995, 22).

arrest reports [written by the police]."¹⁴ Still, some designers of the new tests defend the changes: "the validity of the cognitive ability test was not high" (Dunnette et. al. [1996]).

The basic economics of these affirmative action regulations is fairly straightforward. Voters value many objectives but face limited resources. The question is whether voters were previously discriminating against certain politically unfavored groups of potential police officers at the cost of higher crime rates or whether affirmative action laws are forcing departments to accept higher crime rates as the cost of changing hiring policies.

This article examines the relationship between the changing racial and gender composition of police departments and the crime rate. As mentioned above, there are possibly opposing forces, and the net effect is not obvious and it may not be the same for all crime categories. For example, women police officers may deter rapists better than they deter armed robbers.

Affirmative action can also affect crime rates in many different ways, for example, through changing the marginal quality of new officers or affecting which officers are promoted and thus altering the incentives of the existing police force. If the critics of the new rules are correct that the replacements for cognitive tests simply introduce randomness into the hiring process, all new officers, and not simply the officers the new tests were designed to encourage, could be of lower quality. After first examining how court orders altering the hiring and promotion process affect the crime rate, this article seeks to provide a comprehensive picture for how the changing demographic characteristics of police departments affect crime rates. The evidence will try to sort out the differential impact of affirmative action on new hires and the existing police force as well as try to test whether the changes in effectiveness are due to the minority officers that are hired or the changing quality of all officers.

14. The *Washington Post* editorial went on to claim that: "Of the murder suspects who are indicted, many end up being acquitted because of weak cases prepared by police. Washington's Pretrial Services Agency reports that only 44 percent of the murder cases filed in 1990 and closed by the first part of 1992 resulted in convictions."

Alternative explanations for the results are examined, such as whether any observed higher crime rates merely reflect higher reporting rates and whether police experience levels are affected by the altered hiring policies. I also examine how changing gender and racial compositions alter how police departments operate and other measures of effectiveness such as arrest rates.

II. THE CHANGING COMPOSITION OF POLICE DEPARTMENTS

During 1987, 1990, and 1993, the U.S. Department of Justice conducted a comprehensive national survey of state and local law enforcement agencies with 100 or more officers, known as the "Law Enforcement Management and Administrative Statistics" (LEMAS). My study focuses on city police department data because they allow a more precise study of the relationship between how police departments were organized and the crime rate. By contrast, state and county departments are more difficult to investigate, because they have jurisdiction over larger but overlapping areas.

I separated the data into two sets: (1) the entire Justice Department Survey and (2) a

subset in which demographic data are also available. The results that I report are considerably more significant statistically and important empirically when using the entire Department of Justice survey, yet I will focus on the subset with the demographic data, because changing demographics are related to both the changing hiring patterns by police departments and crime.

Two characteristics stand out from the survey: city police departments vary greatly in their racial and gender makeup, and there have been large increases in the proportion of black and women officers. Tables I and II illustrate these two points, with Table I illustrating the distribution of the racial and gender composition of police departments and Table II examining the distribution of the change in the composition. The first table shows that although most departments have no blacks, Hispanics, or Asians, the range is large with the tenth and ninetieth percentile departments, respectively, employing 0% and 18% blacks. The diversity for women officers is not quite as large, ranging from 0% at the tenth percentile to 14% at the ninetieth.

It is possible to subdivide these categories even further, but some of the racial and sex categories have very small changes in the

TABLE 1
The Race and Gender Composition of Police Departments

	Distribution of Race and Gender Characteristics for Police Departments					
	10th Percentile		Median		90th Percentile	
	Entire Sample	Sample for Which Yearly Demographic Estimates are available	Entire Sample	Sample for which Yearly Demographic Estimates are available	Entire Sample	Sample for which Yearly Demographic Estimates are available
% of the police force that is Asian Pacific	0%	0%	0%	0%	.63%	1.5%
% of the police force that is black	0%	1.3%	0%	7.8%	18.3%	26%
% of the police force that is Hispanic	0%	0%	0%	2.1%	8%	14.5%
% of the police force that is white	72%	65%	98.5%	85.5%	100%	96.5%
% of the police force that is male	86%	86%	97%	91.7%	100%	96.6%

Notes: The entire sample has 4,158 city/ year observations for 1987, 1990 and 1993. The sample for which yearly demographic estimates are available from the Current Population Survey Contains 664 city/year observations: 204 Police Departments in 1987, 240 in 1990, 220 in 1993.

TABLE II
The Changing Racial Composition of Police Departments

	Change at the 10th Percentile		Change at the Median		Change at the 90th Percentile	
	No Consent Decree	Consent Decree	No Consent Decree	Consent Decree	No Consent Decree	Consent Decree
A. Changes in the Racial Composition of Police Departments With and Without Consent Decrees That Occurred from 1987 to 1993.						
Percentage Point Change in the % of the Police Force that is Asian Pacific	-.6	-.23	0	.2	1.0	1.8
Percentage Point Change in the % of the Police Force that is Black	-6.8	-.2	.73	3.2	6.0	18.2
Percentage Point Change in the % of the Police Force that is Hispanic	-.14	-.12	.7	1.1	5.4	7.0
Percentage Point Change in the % of the Police Force that is White	-11.5	-21	-2.3	-5.9	.98	-.7
B. Changes in the Sex Composition of Police Departments With and Without Consent Decrees From 1987 to 1993						
Percentage Point Change in the % of the Police Force that is Male	-5.3	-6.3	-1.1	-2.8	6.4	0

Notes: Panel A again breaks down the sample on the basis of the complete LEMAS Survey and those cities for which information on changing city demographics are available. The table shows the change in the racial and gender compositions of police departments. The entire sample contains 333 cities without consent decrees for which information is available for the same city for all three years. Twenty one cities with consent decrees meet this criteria. By contrast, the restricted sample that is used for the regressions contains 163 and 19 cities in these two categories, though it provides very similar results.

Panel B again breaks down the sample on the basis of the complete LEMAS Survey and those cities for which information on changing city demographics are available. The entire sample contains 343 cities without consent decrees for which information is available for the same city for all three years. Fourteen cities with consent decrees meet this criteria. By contrast, the restricted sample that is used for the regressions contains 163 and 19 cities in these two categories, though it produces very similar results.

total number of officers. In my restricted sample, 189 cities had detailed employment data within each race category by sex for both 1987 and 1990. These cities employed 155,071 (or 40%) of the 387,534 sworn full-time officers employed by local governments in 1990. As examples of the small number of officers in some of these subgroups, the number of male American Indian officers between 1987 and 1990 grew from 280 to 378 officers; for female American Indians, the change was from 47 to 91; and for female Asian Americans, 83 to 203. Even Hispanic females, the next-largest category, saw an increase of only 378 officers. The number of male white officers, the only category to decline, fell by 6,912.

The second table illustrates the different rates of changes over time as well as the impact of the consent decrees which the Department of Justice entered into with city police departments regarding a city's hiring and promotion practices.¹⁵ Past work has studied the effect of these decrees on hiring of black men and found that indeed they do have an impact (Lewis [1989]). The Department of Justice's Civil Rights Division provided information on both racial and/or gender-based consent decrees over the pe-

15. These decrees are contracts that the Department of Justice and cities have signed that have been approved by a court, which obligate the city to act in certain ways in the future.

riod from 1972 to 1994: 19 of these 189 cities were covered by consent decrees during the 1987–93 period, though only three of these cities had consent decrees that were imposed as late as the end of 1987. The 19 cities were Birmingham, Ala.; Montgomery, Ala.; Los Angeles, Calif.; San Francisco, Calif.; Ft. Lauderdale, Fla.; Pompano Beach, Fla.; Miami, Fla.; Tallahassee, Fla.; Macon, Ga.; Chicago, Ill.; Indianapolis, Ind.; Jackson, Miss.; Omaha, Neb.; Las Vegas, N.V.; Syracuse, N. Y. Cincinnati, Ohio.; Philadelphia, Penn.; Memphis, Tenn.; and Milwaukee, Wisc.

Many cities are adopting affirmative action rules on their own either because of their own support for such rules or because of the threat of Justice Department actions. Any examination of consent decrees is thus likely to underestimate the impact of such policies. Yet, consent decrees appear to have clear impacts for both blacks and women. The median change in the percent of black police officers was 2.5 percentage points more in cities with consent decrees than those without them, and for women the median increase was 1.7 percentage points. These may seem like small changes in the share of police employment going to these groups, but compared to the median percent of black and women officers over this seven-year period, these changes represent at least a 57% increase over past employment practices.

Finally, despite the large difference in sample sizes between the entire sample and the restricted one, both sets experienced remarkably similar changes in types of officers employed during this seven-year period. This similarity occurs despite the cities in the smaller sample averaging about 40% more people.

III. EXPLAINING CHANGING CRIME RATES AS A FUNCTION OF THE RACIAL AND GENDER COMPOSITION OF POLICE DEPARTMENTS

The Direct Impact of Consent Decrees

The FBI's Uniform Crime Report allows us to study violent and property crimes, with seven primary crime categories (murder, rape, robbery, aggravated assault, burglary, larceny, and motor vehicle theft), and ten other subcategories (manslaughter, forcible rape, attempted rape, gun robbery, knife

robbery, other robbery, strong-arm robbery, assault with a gun, assault with a knife, and other assault). The results from most of these subcategories will not be reported, because they differ little from the results shown for the primary categories. Data on arrest rates for these broader categories as well as the city populations were obtained directly from the FBI.

The Current Population Survey was used to determine the changing demographic makeup of cities over the 1987–93 period. The percent of the population in different demographic categories was broken down by age (less than 30 years of age, 30–54 years of age, and 55 and older), race (black, white, and other), and sex (male and female), thus yielding 18 demographic categories. This survey also provided information on the average weekly wage and the unemployment rate. The National Conference of Black Mayors provided me with copies of their entire national roster by year so that the race of a city's mayor could be identified. Finally, the LEMAS survey provides information on the racial and gender composition of police departments, as well as on the per capita number of sworn police officers, and other departmental characteristics. The means and standard deviations for these variables are shown in the appendix.

Table III shows simple preliminary regressions that use a simple time trend for the number of years after a consent decree has been imposed and a similar time trend for the years before the decree to pick up changes in before and after trends in crime rates. To do this, I used yearly violent and property crime data for 1985–94 for 495 cities, a longer period than is available for the LEMAS survey. Data prior to 1985 was not included because of severe problems with the consistency between 1984 and 1985 in the city-level crime data. Two sets of fixed effects were used for these simple regressions: city and year fixed effects and city fixed effects along with separate year fixed effects for each state to control for any individual state trends. These regressions use ordinary least squares weighted by city population. The results for both violent and property crime rates imply that crime rates were declining in cities before consent decrees were imposed and were rising thereafter. Violent crimes were rising after the consent

TABLE III
Changes in Crime Rates for Cities with and without Consent Decrees for the Period 1985–94;
Using Only Fixed Effects

	Crime Rates Per 100,000 People		<i>F</i> -test (Prob > F) that before and after time trends are different	Adjacent <i>R</i> ²	No. of Observations
	Time Trend for Years before Consent Decree Went Into Effect (negative values imply that crime was falling until the decree went into effect)	Time Trend for Years after Consent Decree Went Into Effect (positive values imply that crime was rising after the decree went into effect)			
Controlling for City and Year Fixed Effects					
Violent Crime Rate	– 138.6 – 5.3% (4.204)	126.1 4.8% (11.433)	36.35 (.0000)	.7939	4,947
Property Crime Rate	– 593.4 – 9.4% (6.257)	172.2 2.7% (9.346)	57.37 (.0000)	.7719	4,947
Controlling for City Fixed Effects and Separate Year Fixed Effects for Each State					
Violent Crime Rate	– 60.85 – 2.3% (1.195)	86.05 3.3% (7.901)	7.10 (.0078)	.8738	4,947
Property Crime Rate	– 464.0 – 7.4% (5.998)	133.76 2.1% (8.085)	50.94 (.0000)	.8845	4,947

Notes: The first number is the annual change in crimes per 100,000 people, while the second number is the change as a percent of the mean crime rate. Absolute *t*-statistics are shown in parentheses. The regressions use weighted least squares.

decrees by at least 3.3% per year, and for property crimes it was at least 2.1% per year. The differences in trends are all statistically significant at the .01 level.

Given the significant declines in pre-consent decree crime rates, the results for three of the four regressions raise the question about whether the decrees just happen to be imposed when the crime rates were at their low ebb and the post-decree increase is simply a result of mean reversion. At least for the cities with new consent decrees imposed during 1987, the increases in violent crime during the period studied are 2.4 to 3.7 times larger than preceding declines and thus exceed any increase that could simply be attributed to mean reversion.¹⁶ The evidence

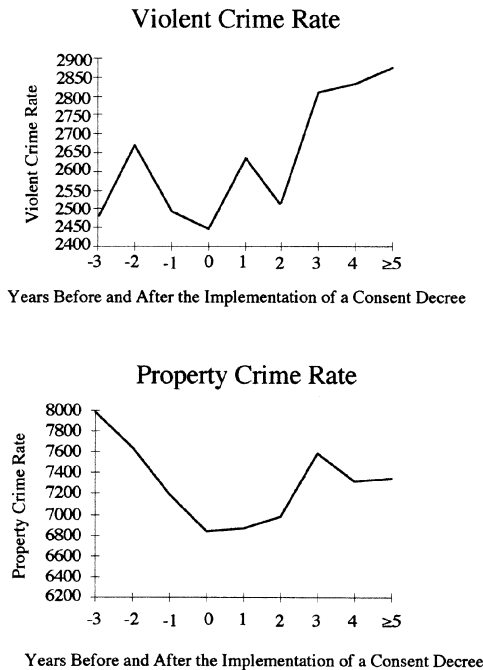
is not clear cut for property crimes, where the declines and increases are of approximately equal size.

Using the smaller sample that matches the LEMAS survey and just the time trend for years after the imposition of the consent decree produces similar, though smaller and less statistically significant increases in crime. Controlling for changing city-level demographics as well as the average weekly wage, unemployment, per capita number of police officers, city population, and population squared, and city and year fixed effects implies: violent crime rises by 1.9% (*t*-statistic = 2.16) and property crime by 2.1% (*t*-statistic = 2.99) for each additional year the consent decree is in effect.

Figure 1 reports this same data slightly differently by including separate dummy variables for the years before and after the imposition of the consent decree. The years included range from three years before the decree to year 4 afterward, with another dummy variable that equals one for years 5 or more after the decree. The estimates use

16. I tried a regression that predicted which cities would have consent decrees imposed on them. The most important factors were city size, whether the city was the largest in a state, and the type of administration. Republican presidential administrations tended to impose consent decrees on relatively Democratic states, whereas Democrat presidential administrations tended to impose consent decrees on relatively Republican states.

FIGURE 1
Using Year Dummies for the Years
before and after the Implementation of
a Consent Decree



Note: Regressions control for city fixed effects as well as year fixed effects by state- and use-population based weighted least squares.

the full sample and correspond to those shown in Table III that account for city fixed effects as well as separate year fixed effects for each state. The pattern is similar to that implied by the table with crime rates falling immediately before the consent decree is imposed and rising after that. (Recently, extending this data set back to 1977 and expanding the number of years studied by seven produced similar results [Lott (2000, chapter 9)].)

An Initial Assessment of How Consent Decrees Affect Crime through the Type of Officers Hired

Consent decrees change hiring policies, and changing hiring policies may affect the crime rate. The consent decrees favor some groups more than others, but the link between these group-specific changes and the

crime rate are unclear. Even if the new hiring procedures introduce randomness, the distribution of skills is not necessarily the same across potential applicants in all groups. To make matters more complicated, the types of officers hired may depend on the crime rate. For example, if departments hired minorities because of growing crime problems in minority areas or hired women because of greater crimes against women, simple ordinary least squares estimates risk improperly blaming some of the higher crime rates on the new police who were hired to help solve the problem.¹⁷ To explain this problem differently, crime rates may have risen even though a city hired black officers, and if they had hired white officers who were less capable of policing minority areas, the crime rate could have risen by even more. Unfortunately, an alternative explanation exists: higher crime rates may signal less concern by city governments about crime and thus a greater willingness to indulge other objectives when hiring police officers.¹⁸

The opposite relationship between crime rates and hiring practices is also possible. Additional law enforcement efforts have a greater effect on crime in high-crime areas (Lott and Mustard [1997, 28, 29]). If affirmative action actually increases crime, high-crime areas would find it more costly to engage in affirmative action, and thus, everything else equal, one suspects that they would engage in less such hiring. Failure to control for why the particular composition of police officers was chosen would underestimate the negative impact from this policy.

To guard against this problem, I initially employ two-stage least squares where the first equation attempts to explain the proportion of black, minority (black, Hispanic, and American Indian), or male officers employed

17. While the hiring of minority officers is motivated by the desire to assign these new officers to minority neighborhoods, the legal prohibition against giving officers assignments based on their race require that any new minority officers be evenly distributed across districts. It is very easy for minority officers to bring discrimination suits if they feel that they are being disproportionately assigned to more dangerous neighborhoods. Black officers have no more desire than white officers to be assigned to dangerous high crime areas. (For another perspective with respect to New York City, see Fyfe [1981].)

18. There are also questions about whether some officers have stronger preferences for policing certain types of communities based on their level of crime.

by a city. As discussed in Section II, I expect that the imposition of a consent decree and, particularly, the length of time that the decree has been in effect to serve as the instruments and help explain the levels of minority or female employment, depending on what type of employment the consent decree deals with. The number of years that a consent decree has been in effect is an excellent instrument since it is extremely unlikely that the causation runs from future crime rates to the number of years since a consent decree has been entered. I also account for the demographic composition of the city's population; the average weekly wage and unemployment rate; whether its mayor was black; the city's population and population squared; and the per capita number of sworn police officers. The second equation that explains each one of the individual crime rates included all the variables except for whether there was the consent decree and the mayor's race. Weighted least squares, where the estimates were weighted by city population, was used to deal with heteroskedasticity.¹⁹

The coefficients on the percent of the police force that is black, minority, or male in the second regression are thus adding the impact on the crime rate of the consent decree together with that particular group being studied. I will disaggregate these two effects later when I report the reduced form regressions in Table VIII and Appendix 2. Because the instruments that I have for racial or gender hiring consent decrees are very heavily correlated, the impacts of the racial and gender compositions of police departments are initially estimated separately for these two-stage least squares regressions.

Admittedly, there are many location-specific and year-specific differences in crime rates that are not captured by the variables controlling for demographic, income, and population differences. One simple way of dealing with this is the use of location and time fixed effects, where a separate dummy variable is used for each city and year. However, this approach also has its drawbacks: although it may correctly measure left-out variables, it may also cause us to falsely

attribute some of the impact of changes in our in our other variables (for example, the impact of changing racial or gender composition of police departments) to these fixed effects. Nevertheless, all the regressions report either city and year fixed effects or county fixed effects with separate year fixed effects for each state.²⁰

As an example, the two-stage least squares estimates examining the percentage of the police force that is black with city and year fixed effects take the following form:

- (1) % Police Force That Is Black
 = $g(\text{Consent Decree in Effect, Number of Years Decree in Effect, Dummy for Whether Mayor Is Black, Per Capita Number of Sworn Officers, City's Demographic Composition, Population and Population Squared, Average Weekly Wage, Unemployment Rate, Fixed Year and City Effects})$
- (2) $\ln(\text{Crime Rate})$
 = $f(\% \text{ Police Force That Is Black, Per Capita Number of Sworn Officers, City's Demographic Composition, Population and Population Squared, Average Weekly Wage, Unemployment Rate, Fixed Year and City Effects})$

The results from the second equation are reported in Table IV separated out by the type of fixed effects employed. All crime

20. I tried three different types of location fixed effects: city, county, and state. Generally, using the broader measures of location produced estimates that agreed in sign with the city fixed effects, but the estimates were larger and more statistically significant. To deal with possible state-level trends in laws, I also tried allowing a separate fixed effect for each state for each year, though when combined with county or city fixed effects this dramatically reduces the degrees of freedom in each regression. Only the time-varying state fixed effects are reported with the county fixed effects because none of the estimates on any of the focus or control coefficients was statistically significant with city fixed effects.

19. Similar estimates are produced if unweighted estimates are employed, but these data exhibit definite heteroskedasticity, with the smaller cities reporting a much greater variation in crime rates over time.

rates are in natural logs, where .1 is added to zero values before the natural log is taken. With the exception of manslaughter, aggravated assault, and motor vehicle theft, an increase in the percentage of a police force that is black is consistently associated with significant increases in crime. The effect is so large that 18 of the specifications imply that a one standard deviation change in the percentage of the police force that is black increases the corresponding crime rates by at least 5% of its mean value (see the percentages listed next to the coefficients). The effects are dramatic no matter how one examines these estimates. For example, increasing black officers' share by one percentage point increases property crimes by 4%, and the same increase raises the murder rate by 1.9% and overall violent crime by 4.8%. As the relative median increase in black officer's share of police departments over this seven-year period because of consent decrees was 2.5 pge points, I conclude that if nothing else had changed, the average city's murder rate would have risen by 4.7%.²¹

One point should be made very clear at this point. We are talking about the impact on crime of hiring "additional" blacks, many of whom would not have been hired without the consent decree. As mentioned in the introduction, changes in testing that are used to encourage hiring more minorities can explain why these blacks are not of the same quality as previously hired blacks. It can still be true that qualified black officers are more effective but that the new less-qualified of-

ficers are associated with more crime. The large impact suggests that more than just the quality of new minority recruits or new minority promotions is affected. Changing tests to employ a greater percentage of blacks can make it more difficult to screen out lower-quality candidates generally, including whites and other racial groups. Independent of the consent decree, the size of the change in black employment may thus proxy for changes in the level of standards used to hire employees in general. Similarly, changing promotion rules that favor seniority over achievement can affect morale and incentives across all categories of police officers.

For the next set of regressions, blacks, Hispanics, and American Indians were combined to represent the share of minorities in a department. The groups included in the minority classification was decided by using a series of reduced-form equations where I tested to see whether the predicted impact of the different racial groups were statistically different from each other. Generally, the coefficients for blacks, Hispanics, and American Indians were not statistically different from each other, and the whites and Asians usually fit together in a separate group.²² More precisely, whites and Asians

21. One concern raised to me by Ed Glaeser is whether the results are being driven solely by time-series changes in the data and whether these results are consistent across the years being studied. To test this, I reran the regressions shown in Table IV with fixed state effects separately on the data for each of the three different years. For blacks the coefficient signs are similar to those already reported, though the results for these smaller subsets of data are not always statistically significant. The results for 1987 are as follows: for violent crimes the coefficient is 4.3 (t -stat = 1.822); property crimes, 3.34 (t -stat = 1.811); and murder 5.52 (t -stat = 1.254). The results for 1990 are as follows: for violent crimes the coefficient is 3.08 (t -stat = 1.071); property crimes, 5.46 (t -stat = 2.358); and murder, 3.45 (t -stat = 0.872). The results for 1993 are as follows: for violent crimes the coefficient is 3.025 (t -stat = 1.900); property crimes, 2.509 (t -stat = 2.847); and murder, 1.711 (t -stat = 0.815). Similar results are also produced for the percentage male and the percentage minority specifications.

22. More precisely, when the omitted group in the reduced form regression (represented by the intercept) is Hispanics, the probability that the coefficients for whites and Asians are statistically significantly different from each other at the following levels as: for violent crimes is 34%; property crimes, 73%; murder, 39%; manslaughter, 41%; rape, 78%; forcible rape, 79%; attempted rape, 5.6%; robbery, 31%; gun robbery, 15%; knife robbery, 4.9%; other robbery, 0%; strong-arm robbery, 79%; assault, 66%; burglary, 77%; larceny, 68%; and motor vehicle theft, 98%. The probability that the coefficients for blacks and Hispanics are statistically significantly different from each other is: for violent crimes it is 36%; property crimes, 51%; murder, 14%; manslaughter, 37%; rape, 77%; forcible rape, 56%; attempted rape, 73%; robbery, 1.6%; gun robbery, 1.5%; knife robbery, 46%; other robbery, 56%; strong-arm robbery, 74%; assault, 99%; burglary, 3%; larceny, 60%; and motor vehicle theft, 22%. The probability that the coefficients for blacks and whites are statistically significantly different from each other is: for violent crimes it is 5%; property crimes, 4.5%; murder, .12%; manslaughter, .01%; rape, 43%; forcible rape, 39%; attempted rape, 57%; robbery, .16%; gun robbery, .37%; knife robbery, 58%; other robbery, 8%; strong-arm robbery, 62%; assault, 3%; burglary, 6%; larceny, 34%; and motor vehicle theft, .17%. State fixed effects were used for these estimates. A related set of regressions is reported in Section VII, though these regressions do not have all these categories included at the same time.

TABLE IV
Using Two-Stage Least Squares to Take Into Account the Impact That Consent Decrees and the Presence of a Black Mayor Have on the Composition of Police Departments

Crime Rates	% of the Police Force that is Black			% of the Police Force that is Minority (Black, Hispanic, and American Indian)			% of the Police Force that is Male			
	City and Year Fixed Effects (1)	County Fixed Effects and Separate Year Fixed Effects for each State (2)	City and Year Fixed Effects (3)	County Fixed Effects and Separate Year Fixed Effects for each State (4)	City and Year Fixed Effects (5)	County Fixed Effects and Separate Year Fixed Effects for each State (6)	Coefficient and <i>t</i> -Statistic	% of mean explained	Coefficient and <i>t</i> -Statistic	% of mean explained
Violent crime	4.79 (2.38)	7.14 (3.57)	3.01 (2.52)	6.068 (3.85)	-8.19 (1.97)	-10.28 (1.67)	6%	13%	9%	12%
Property crime	4.27 (2.51)	3.96 (3.08)	2.58 (2.59)	3.20 (3.18)	-6.42 (1.18)	-6.57 (1.69)	5%	5%	5%	6%
Murder	1.87 (1.64)	9.43 (2.88)	1.46 (1.82)	8.03 (3.20)	-6.41 (0.96)	-8.26 (1.18)	11%	61%	26%	33%
Man slaughter	6.39 (0.78)	6.33 (1.11)	2.50 (0.51)	5.84 (1.31)	-13.06 (0.86)	-3.69 (0.35)	40%	46%	55%	15%
Rape	13.27 (1.78)	4.07 (1.98)	5.97 (1.42)	3.14 (1.96)	-8.98 (0.73)	-6.67 (1.37)	41%	12%	19%	14%
Total robbery	7.86 (3.07)	9.55 (4.08)	4.57 (3.18)	7.67 (4.25)	-11.06 (1.22)	-13.53 (1.78)	16%	20%	15%	19%
Total assault	2.10 (0.90)	6.06 (2.71)	1.83 (1.30)	5.55 (3.13)	-6.64 (1.07)	-8.54 (1.44)	4%	13%	8%	10%
Burglary	7.05 (3.12)	6.19 (3.88)	3.99 (3.23)	4.999 (3.95)	-9.23 (1.22)	-10.45 (1.79)	11%	10%	9%	11%
Larceny	4.83 (2.76)	3.53 (2.77)	3.24 (3.10)	3.05 (3.01)	-8.68 (1.24)	-6.38 (1.69)	6%	5%	8%	6%
Motor vehicle theft	-0.81 (0.34)	6.30 (2.69)	-1.11 (0.76)	4.66 (2.57)	3.79 (0.81)	-7.08 (1.34)	1%	10%	4%	8%
Felonious killings of police officers	-4.26 (1.22)	5.82 (2.23)	-4.39 (1.99)	3.846 (2.14)	11.96 (1.31)	-10.31 (1.58)	20%	22%	29%	35%

TABLE IV
Using Two-Stage Least Squares to Take Into Account the Impact That Consent Decrees and the Presence of a Black Mayor Have on the Composition of Police Departments

Crime Rates	% of the Police Force that is Black			% of the Police Force that is Minority (Black, Hispanic, and American Indian)			% of the Police Force that is Male					
	City and Year Fixed Effects (1)	County Fixed Effects and Separate Year Fixed Effects for each State (2)	City and Year Fixed Effects (3)	County Fixed Effects and Separate Year Fixed Effects for each State (4)	City and Year Fixed Effects (5)	County Fixed Effects and Separate Year Fixed Effects for each State (6)						
	coefficient and <i>t</i> -Statistic	% of mean explained	coefficient and <i>t</i> -Statistic	% of mean explained	coefficient and <i>t</i> -Statistic	% of mean explained	coefficient and <i>t</i> -Statistic	% of mean explained				
Assaults on police officers	78.14 (3.49)	128%	.823 (.095)	1%	61.84 (4.47)	131%	6.35 (1.00)	13%	-18.7 (1.89)	16%	-15.31 (1.69)	18%
Accidental deaths of police officers	-.543 (0.29)	3%	.315 (.242)	1%	-.097 (.083)	1%	-.179 (.190)	1%	-2.00 (.742)	5%	-4.80 (1.60)	16%

Notes: The regression estimates for equation (2) for the two-stage least squares that are reported above account for the same variables controlled for in the first-stage regression except for the consent decree, the number of years since the consent decree went into effect, and whether the city's Mayor is black, which were instead included in the first-stage regression. The other variables controlled for were the percentage of the population in different demographic categories that were broken down by age (less than 30 years of age, 30-54 years of age, and 55 and older), race (black, white, and other), and sex (male and female) so that this information was available for 18 categories, the average weekly wage, the unemployment rate, and city population and population squared. The absolute *t*-statistics are shown in the parentheses below the coefficient estimate, with the percent of the endogenous variable's mean that can be explained by a one-standard-deviation change in the exogenous variable shown in the adjacent column. All regressions use weighted least squares where the variables are weighted by the city population. For the percentage of officers that are black or minority, the sample sizes is 641 and covers the years 1987, 1990, and 1993. For the percent of officers that are male over that period, the sample size is 648.

had different effects on crime in only three of the 19 crime categories (when the broadest set of categories was used), whereas blacks and Hispanics were statistically different in five cases. In the three cases where whites and Asians differed in their impact on crime (attempted rape, knife robbery, and other robbery), Asians had a greater deterrent impact on crime. Hiring additional Hispanics and American Indians did not tend to increase crime by the magnitude shown by hiring additional black officers.

The two-stage least square estimates continue to confirm this pattern. Putting together blacks, Hispanics, and American Indians continued to produce very similar, though smaller, results compared to what I found for blacks alone. The minority portion of a police force in column 3 explains about 75%–80% as much of the percent of the mean violent and property crimes as did the regressions in column 1 for the percent of the police force that is black. Nineteen of the 20 crime regressions imply that increasing the percentage share of minorities in a department increase crime, and the relationship is statistically significant for three-quarters of the estimates.

The last two columns in Table IV imply that increasing the share of males in the police force decreases crime in 19 of the 20 specifications shown, though the aggregate property crime category implies a statistically significant relationship only for the time varying state fixed effects that include the county fixed effects. The specifications for murder, manslaughter, and rape provide no significant evidence that increasing women's share of the police force increases these crimes. Using either simple county or state and year fixed effects produces a much more consistent negative relationship between higher males shares of the police force and crime.²³

23. Limits on the number of variables that could be handled using two-stage least-squares with STATA restricted the regressions on the larger data set to the state fixed effects specifications. (This is the data set that was not restricted to those cities for which demographic data was available.) Estimates using these data remain similar to those already reported in Table IV. The sample size for this larger data set is 1,015 observations for the regressions explaining the percentage of the police force that is black or minority and 1,026 for the percentage of the police force that is male.

Table V reports some of the first equation results from the two-stage least squares estimation of the percent of the police force that is black, minority, or male used in the regressions analyzing violent crime with city fixed effects. The results imply that for the racial components, the number of years that a consent decree has been in effect dramatically increases the percentage of minorities in police forces. Every 10 years after the consent decree goes into effect increases the number of blacks by another 4.1 percentage points and minorities by 4.8 percentage points. These results are comparable in magnitude with those shown in Table II. I also tried these estimates with a squared term for the number of years that the consent decree had been in effect, but including this did not noticeably alter the results. One city in the sample had consent decrees as long as 21 years (with both the sample median and the mean being about 10 years), and the estimates indicate the percentage of the police force that is black is still rising at that time.

Interestingly, the election of a black mayor does not appear to significantly change the number of minority police officers, with the corresponding coefficient even being negative in the minorities specification. There is also surprisingly little relationship between past crime rates and the composition of the police force, and the *t*-statistics are quite small. Only past violent crimes imply more blacks on a police force with a *t*-statistic even greater than 1. The Hausman endogeneity test indicates that the number of years a consent decree is in effect is a valid instrument for the black and minority regressions.

A possible concern with these results is that the consent decree not only directly affects the number of minorities or women who are hired but may also implicitly signal concerns about future crime rates. If one expects that higher crime rates can be best combated with more minority police officers, there is also the concern that this motivated the adoption of the consent decree. While this is possible, it is not clear why the Department of Justice has better information on a particular city's future crime rate than the city itself. In any case, as a check, I reestimated the regressions in Table IV by

TABLE V
 Explaining the Percentage of the Police Force that is Black, Minority, or Male:
 The Regressions for Equation (2) for the City and Year Fixed Effects Regressions
 in Table IV.

	Consent Decree Dummy (a different dummy is used for whether the consent decree involves sex or race cases and is matched with the appropriate regression listed below)	Number of Years that Consent Decree Is in Effect (consent decrees involving race and sex are separated out and are matched with the appropriate regression listed below)	City Has a Black Mayor	ln(Violent Crime Rate per 100,000 people)	F-Statistic	Adjusted R ²
% of the police force that is black	.014 (1.003)	.0041 (3.289)	-.00028 (.049)	-.0100 (1.731)	53.46	.9581
% of the police force that is minority (black, hispanic, and/or american indian)	.0556 (2.150)	.0048 (2.899)	-.0018 (0.237)	-.0089 (1.145)	40.26	.9446
% of the police force that is male	-.031 (0.708)	-.0015 (0.445)	-.0026 (0.192)	-.0061 (0.222)	2.15	.3307

Notes: The regressions listed above represent just three of the different first-stage regressions. In addition to the lagged violent and property crime rates, the regressions from equation (2) also included all the other variables listed for the weighted least squares estimates. The fixed effects used in the different first-stage regressions corresponded with those used in the second stage. The estimates reported above use city and year fixed effects.

including the consent decree dummy variable directly into the second equation that estimates the crime rate. This change has virtually no effect on the results reported previously. As a further test of the sensitivity of the results, I also tried reestimating the results in Table IV by removing the crime rate variables from the first equation and the pattern of results remained similar to those already reported.²⁴

The question of whether more black police officers had a differential impact in more heavily black areas can be examined by interacting the percent of the police force that is black with the percent of the population that is black. The violent and property crime estimates corresponding to the regressions in Table IV, and the estimates that did not include the crime rates in the first regression all imply that *the increase in crime from hiring black officers is greatest in communities with the most blacks*. For example, the violent and property crime estimates that correspond to city fixed effects estimates in Table IV are positive and have *t*-statistics of 4.8 and 4.2, respectively.

Finally, data on whether a police department was unionized and the gross salary paid per sworn officer were available, though for only 1987 and 1990. Using these two variables and the smaller data set, I reestimated the results reported in Table IV and found very little change in results. For the most part neither of these variables was significant in explaining changes in the crime rate.

24. For example, after excluding the crime rates from the first-stage regression, the city fixed effects regressions produced estimates for the percentage of the police force that is black of 2.43 (*t*-statistic = 1.741) for violent crimes and 2.25 (*t*-statistic = 1.864) for property crimes. For the percentage of the police force that is minority, the city fixed effect results were: 1.98 (*t*-statistic = 1.810) for violent crimes and 1.86 (*t*-statistic = 2.055) for property crimes. For the percentage of the police force that is male, the city fixed effect results were: -7.73 (*t*-statistic = 1.012) for violent crimes and -7.9 (*t*-statistic = 1.042) for property crimes. As was true in Table IV, the level of significance tended to be higher for county fixed effect regressions. The first-stage regression results also remain similar to those already reported. For the regression estimating the percentage of the police force that is black, the consent decree coefficient is .017 (*t*-statistics = .899) and the number of years that it is in effect is .0042 (*t*-statistic = 3.376). For the regression for minorities, the consent decree coefficient is .059 (*t*-statistics = 2.274) and the number of years that it is in effect is .0049 (*t*-statistic = 2.962). For the regression for males, the coefficients are again statistically insignificant.

IV. ARE HIGHER CRIME RATES A RESULT OF LESS-EFFECTIVE POLICE OR GREATER REPORTING RATES?

Unfortunately, the FBI's Uniform Crime Report Data relies on reported, not actual, crimes. The problem is potentially critical for this study, because the racial or gender characteristics of the police officers could either be altering the behavior of criminals and/or the rate at which victims report crimes. The problem is made even worse by the fact that both sides of the debate can provide explanations for the preceding results. Those favoring affirmative action can argue that the higher reported crime rates when more minorities are hired implies that the community feels more comfortable about reporting crimes. In contrast, those who believe that lower standards mainly result in less-qualified officers can say that the results confirm the poor performance of the less-qualified officers.

There are several ways of investigating whether the results are being driven by higher reporting rates. The simplest approach is to look at murder and manslaughter, where underreporting is essentially nonexistent. Thus, the race or sex of the police officer does not produce additional reporting. For both murder and manslaughter, the results are very consistent. More minority, black, or female officers are associated with higher murder and manslaughter rates, while more white and male officers imply fewer deaths. These two crimes are also the most accurately reported for another reason: if multiple offenses are perpetrated at the same time, only the most serious offense is reported. Thus, if an armed robbery resulted in murder, only the murder and not the robbery is recorded.

Further, the importance of the reporting problem should vary systematically across crime categories as the loss from the crime varies. For example, suppose that a black person is making a decision on whether to report a theft to a predominantly white police department. His decision to report the crime depends on the value of the item stolen, the probability that the item will be recovered, and the cost involved in going to the police station, including whatever difficulties might arise in how the black man might be treated by white police officers. The victim would only report crimes where

either the value of the item stolen or the probability of recovery is relatively high. Lowering the cost of the black person reporting the crime by introducing more black officers would result in more reporting of relatively low-value, low-probability-of-detection crimes. Since the cost of making the complaint constitutes a much bigger percentage of the return to acting on relatively small harms, actions that reduce those costs have a much bigger effect on reporting minor crimes.

For at least broad categories of property crimes it is possible to make this comparison. Miller, Cohen, and Wiersema (1996) claimed that in 1992 the average larceny involved property loss of \$270, burglary \$970, and auto theft \$3,300. By comparison, the differences in the arrest rates are small: larceny 30%, burglary 21%, and auto theft 25%. These figures would imply that the biggest increase in reporting from changing the racial mix of police should occur for larceny, next for burglary, and least for auto theft. (Auto theft and burglary should also tend to have relatively high reporting rates compared to larceny simply because these crimes must be reported as a condition of obtaining reimbursement from insurance companies.) Yet, all of the two-stage least squares estimates in Table IV indicate that the racial or gender compositions of the police department have always smaller impacts on larceny than on burglary, and half the time the impact on larceny is smaller than on auto theft. None of the estimates are consistent with the earlier results arising from increased reporting rates.

V. DISAGGREGATING FURTHER BY RACE AND SEX

For 1987 and 1990, the Department of Justice survey determined the percent of *each racial group* that was male or female. The two-stage least squares regressions reported earlier were therefore reestimated with two changes: the previous racial or sex breakdowns were replaced one at a time with the eight new race and sex categories and the first equation in the two-stage least squares included a dummy variable that equals 1 when the consent decree dealt with either race or sex.

Table VI reports the county fixed effects with separate year fixed effects for each state.

Despite the sample size being about one-third smaller, the results are similar to those already reported. Gender plays an even smaller role than it did in the earlier results. The effectiveness of different types of police officers lies more along racial than gender lines, though there are notable exceptions for Asians, where males are associated with fewer crimes and females more. Murder divides along racial lines, with more whites (both males and females) coinciding with lower death rates but the reverse being true for blacks and Hispanics. In all but a few of cases, more blacks and Hispanics are associated with higher crime rates.

The variables explaining rape provide very little evidence that the gender of the police officer affects this crime differently. For whites and blacks, the different gender racial groups have the same coefficient signs and are statistically indistinguishable. Although differences do exist for Hispanics and Asians, even here the effects do not suggest a consistent pattern with the relative impacts of male and female officers having the opposite impacts in the two cases. The strength of these results make it very difficult to believe that male and female officers have much of a differential impact on deterring rapes. Although it is still quite likely that male and female officers have different skills in dealing with rape (e.g., female officers may be better able at getting rape victims to reveal details), the tests do not allow us to differentiate what the skill differences are for each gender. Victims or potential victims may also value more than simply deterrence. For example, they may value how they feel going through the process, and that is another dimension that we are unable to measure. However, even if these other attributes are significantly valued, the results presented here allow us to discuss the trade-off between the number of rapes and these other possible dimensions.

As another attempt to control for differences in law enforcement across states, I also reestimated the regressions shown in Table VI with city and year fixed effects and including variables for both the per capita state employment in corrections and the judicial system.²⁵ Including these variables had no discernible impact on the results reported.

25. See Lott and Mustard [1997], for a discussion of these data.

TABLE VI
Using Two-stage Least Squares to Further Examine the Differences by Race and Sex: Controlling for County Fixed Effects and State Fixed Effects that Vary by Year

Category of Crime Being Explained	% of Police Force Black		% of Police Force Hispanic		% of Police Force White		% of Police Force Asian Pacific		% of Police Force Asian Pacific					
	Force Black Male	Force Black Female	Hispanic Male	Force Hispanic Female	Force White Male	Force White Female	Force Asian Pacific Male	Force Asian Pacific Female	Asian Pacific Male	Asian Pacific Female				
Violent crime	10.43 (2.672)	14% (2.657)	13.98 (2.065)	18% (0.890)	234.02 (0.890)	27% (0.890)	-6.28 (2.858)	13% (0.290)	4.545 (0.290)	2% (0.290)	-56.12 (0.735)	31% (2.160)	241.91 (2.160)	10% (2.160)
Property crime	3.95 (1.723)	4% (1.827)	5.366 (1.444)	5% (0.849)	93.94 (0.849)	8% (0.849)	-2.43 (1.883)	4% (0.883)	3.804 (0.389)	1% (0.389)	-28.03 (0.621)	12% (1.554)	97.25 (1.554)	3% (1.554)
Murder	19.72 (2.74)	60% (2.719)	18.46 (1.758)	56% (0.911)	79.28 (0.911)	74% (0.911)	-10.37 (2.738)	78% (2.738)	-2.455 (0.095)	4% (0.095)	-54.34 (0.982)	68% (1.904)	340.94 (1.904)	50% (1.904)
Man-slaughter	26.074 (2.317)	60% (2.571)	101.57 (1.976)	63% (0.874)	148.73 (0.874)	30% (0.874)	-16.47 (2.649)	61% (2.649)	33.60 (0.670)	28% (0.670)	-95.46 (0.839)	64% (2.101)	682.163 (2.101)	49% (2.101)
Rape	4.269 (1.345)	10% (1.354)	6.182 (1.101)	15% (0.732)	90.44 (0.732)	19% (0.732)	-2.588 (1.404)	10% (1.404)	-1.221 (0.088)	1% (0.088)	-6.333 (0.103)	6% (1.090)	97.025 (1.090)	7% (1.090)
Total robbery	11.40 (2.583)	18% (2.578)	13.90 (1.951)	22% (0.887)	252.82 (0.887)	36% (0.887)	-6.665 (2.728)	17% (2.728)	6.35 (0.360)	4% (0.360)	-82.098 (0.910)	56% (2.104)	251.97 (2.104)	13% (2.104)
Total assault	9.802 (2.387)	14% (2.445)	15.49 (2.024)	22% (0.881)	227.57 (0.881)	29% (0.881)	-6.26 (2.642)	14% (2.642)	3.015 (0.174)	2% (0.174)	-21.071 (0.277)	13% (2.047)	251.05 (2.047)	11% (2.047)
Burglary	7.101 (2.444)	8% (2.534)	9.553 (1.879)	11% (0.896)	181.06 (0.896)	19% (0.896)	-4.446 (2.703)	8% (2.703)	12.066 (0.868)	5% (0.868)	-70.765 (1.051)	35% (2.284)	185.99 (2.284)	7% (2.284)
Larceny	2.319 (1.101)	2% (1.534)	6.490 (1.694)	7% (0.843)	86.85 (0.843)	8% (0.843)	-2.096 (1.695)	4% (1.695)	9.061 (0.839)	3% (0.839)	-3.773 (0.096)	2% (1.803)	110.96 (1.803)	4% (1.803)
Motor vehicle theft	13.30 (2.558)	18% (2.170)	3.090 (0.457)	4% (0.853)	201.95 (0.853)	25% (0.853)	-5.3795 (2.018)	12% (2.018)	-6.265 (0.338)	3% (0.338)	-193.24 (1.289)	54% (0.932)	112.87 (0.932)	5% (0.932)
Assault on police officers	-24.16 (1.292)	36% (0.418)	-38.05 (1.28)	27% (0.905)	133.06 (0.905)	8% (0.905)	-3.05 (2.295)	12% (2.295)	16.68 (.280)	14% (.280)	-35.32 (1.671)	3% (0.905)	334.5 (0.905)	2% (0.905)

Notes: The second-stage regression estimates that are reported above account for the same variables controlled for in the first-stage regression except for the consent decree, the number of years since the consent decree went into effect, and whether the city's mayor is black. County fixed effects are used, with additional separate fixed effects for each state by year to pick up any changes at the state level that might explain changes in crime rates over time. All regressions use weighted least squares where the variables are weighted by the city population. The absolute *t*-statistics are shown in the parentheses below the coefficient estimate, with the percent of the endogenous variable's mean that can be explained by a one-standard-deviation change in the exogenous variable shown in the adjacent column. Sample size is 439 and covers the years 1987 and 1990.

While the impact of hiring more people in corrections and the judicial system usually reduced crime, the effect was never statistically significant. Variables to account for concealed handgun laws, waiting periods and the length of those waiting periods in buying a gun, penalties for using guns in commissions of crimes, and cocaine prices were also included,²⁶ but only the variable for the presence of concealed handgun laws reduced crime and none of these variables appreciably altered the other findings. Passage of concealed handgun laws reduced murder rates by about 10.5%.²⁷ Controlling for the use of Lojack automobile anti-theft devices tended to make the results for black and minority officers more positive and statistically significant though the coefficient for Lojack was not significant.²⁸

VI. FELONIOUS KILLING OF POLICE, ACCIDENTAL POLICE DEATHS, ASSAULTS ON POLICE, AND SHOOTING CIVILIANS

Many studies have focused on whether blacks and other minorities civilians have been shot by police at disproportionately higher rates (Matulia [1985, 7]). The standard view is that the higher rates at which

26. See Lott and Mustard [1997], for a discussion of these data.

27. Given the possible relationship between drug prices and crime, I reran the regressions in Table IV by including an additional variable for cocaine prices. One argument linking drug prices and crime is that if the demand for drugs is inelastic and if people commit crimes in order to finance their habits, higher drug prices might lead to increased levels of crime. Using the Drug Enforcement Administration's STRIDE data set from 1977 to 1992 (with the exceptions of 1988 and 1989), Michael Grossman, Frank J. Chaloupka, and Charles C. Brown [1996] estimate the price of cocaine as a function of its purity, weight, year dummies, year dummies interacted with eight regional dummies, and individual city dummies. However, these data are not perfect. Because of the lack of observations for 1993, I used the drug prices for 1992. While the drug price variable was positive it was not statistically significant and its inclusion had very little impact on the relationship between the type of police officer and the crime rate. I would like to thank Michael Grossman for providing us with the original regressions on drug prices from his paper.

28. I followed Ayres and Levitt's [1997] paper, which identifies when Lojack was adopted so that I could control for both a dummy variable for the presence of the law and a time trend for the number of years that the law was in effect. Although both variables implied that auto theft fell when Lojack was adopted, neither coefficient was statistically significant. Unfortunately, neither Ayres and Levitt nor Lojack were willing to share data on the number of Lojack devices sold.

blacks are shot by police can easily be explained by blacks being involved in crimes at higher rates and the observation that black and Hispanic officers are more likely to engage in shootings can result from minority officers patrolling minority areas where the crime rate is highest (Fyfe [1989, 478]).²⁹ Indeed, if one believes that police officers are more likely to shoot civilians accidentally when their own lives are at greater risk, the issues of whether police shoot citizens and whether the police are likely to be shot or assaulted are closely related. Previous work has not examined the differences between male and female officers, and there has been an absence of evidence of the risks that officers face from being shot or assaulted.³⁰

To examine the issue of risks facing different police officers, I use the same two-stage least squares specifications that were used earlier to explain the rate at which police officers are assaulted, killed by attackers, or die in accidents while on the job. As seen in Table IV, increasing the number of women officers is consistently and significantly related to more assaults on police officers. Increasing the number of female officers by one percentage point appears to increase the number of assaults on police by 15%–19%. The breakdown in Table VI is

29. It is important to note that there are legal difficulties in assigning minority officers to specifically patrol minority areas. Such a policy would generate charges of discrimination (e.g., 411 F. Supp. 218, which writes that police department can not "segregate its personnel along black neighborhood lines any more than the City's housing authority can foster racially segregated public housing").

30. The 174 cities that were in the sample every year averaged a felonious killing of an on-duty police officer at the rate of one every ten years, while accidental deaths (from all sources such as traffic accidents and accidental shootings) averaged about one every 27 years. Three cities New York City in 1987 and Chicago and Philadelphia in 1990 had three felonious killings of police officers in a year. Between 1987 and 1993, the number of felonious police killings per full-time sworn officer rose from .010% to .018%, and the number of accidental deaths per officer increased even faster, from .0005% to .0028%. While these are large percentage increases, the amount of variation from one year to another does not imply an overall trend. With 12 observations having more than a thousand assaults in a year, a much more frequent occurrence is an officer being assaulted—though the probability fell from 26% to 22%. The cities with more than a thousand assaults against police in a year are Baltimore (all three years), Chicago (one year), Houston (one year), Los Angeles (three years), Phoenix (one year), Philadelphia (two years), and New York (one year).

similar, with the number of assaults on police officers being statistically significantly different between men and women for all races. Clearly, if a physical attack takes place, it is much more likely to be directed against a female officer. When weapons are involved, as is much more typically the case with felonious killings, criminals do not appear to be making as much of a distinction over whether the officer is male or female.³¹ The evidence from Table IV weakly also suggests that black and minority are more likely to be assaulted. It is difficult to see any consistent pattern for the killing of police officers or accidental deaths, though this might arise because these deaths are so infrequent.³²

Although the regressions that explain attacks on police officers have controlled for the same variables used to explain all the different crime rates, the use of protective body armor could make a significant difference in the number of felonious killings of police. Unfortunately, data on body armor are not available for 1987, and thus there is only one year overlap between these data and the data that break down police personnel by sex for each racial group. Rerunning the felonious killing regressions shown in Table IV with this smaller data set produces very similar results for the racial and sex groupings, and, surprisingly, in all the cases the body armor variable is very statistically insignificant, with a *t*-statistic that is never greater than .4. One possible explanation for these results is that police officers are offsetting the greater security offered them by these protective devices through taking greater risks (Peltzman [1975]).

Finally, it is possible to match evidence on police shootings of civilians with our data on the racial and sex composition of police departments. Geller and Scott [1992] compiled

data police shooting of civilians for 12 cities: Atlanta, Chicago, Dallas, Houston, Indianapolis, Kansas City, (Mo.), Los Angeles, New York, Philadelphia, Santa Ana (Calif.), St. Louis, and San Diego. Although they provide as many as 20 years of data for Chicago and New York, our tests here are limited by the LEMAS to 1987 and 1990, thus leaving us with only 24 observations, so any results must be viewed as very preliminary. The central concern is well summarized by Los Angeles Police Commissioner Bert Boeckmann during a debate before the city's decision to remove the 5-foot height requirement: "Commissioner Bert Boeckmann expressed concern that small-statured officers might rely too much on their guns or partners to compensate for a lack of size and strength in dealing with uncooperative suspects. 'Would there be more of a tendency to reach for a gun as opposed to using some other form for quieting a person she may be having an altercation with?' he asked" (McGreevy [1997, N4]). This argument not only applies to height requirements but also raises the broader question of whether women are more likely to resort to substitute methods, such as guns, to control criminals. To test this, I regressed the per capita number of police shootings of civilians on the percentage of the police force that were black or white males as well as on the per capita number of felonious killings of police and assaults on police, the per capita number of sworn full-time police, officers, the city population, and city and year fixed effects. Felonious killings and assaults on police are used to measure the risks facing officers, with more killings and assaults implying that officers face higher costs to delaying a decision on the appropriate response to possible threats. A similar regression was run using the percentage of the police forces that were black and white females.³³

31. An alternative explanation for the high assault rates on women officers is that the changes in assaults are being driven by a lack of respect for women that just happens to be correlated with the changes in the number of female officers. It is difficult to measure this changing respect for women, but I attempted to do this by including the rape rate in the regressions that use the percent of the police that is male to explain the assault rate. In none of the regressions was the rape rate statistically significant, and its inclusion did not alter the coefficient on the percentage male. I would like to thank Bill Landes for raising this possibility to me.

32. The comparable estimates for Table VI and Appendix Tables B1 and B2 are available on request from the author.

33. While the existing evidence by Fyfe [1989] and others on which types of police officers are more likely to engage in shootings is very interesting, there are several unresolved questions. The primary issues are that the work is purely cross-sectional, uses even smaller samples than I use here from just the largest cities, and only attempts to control for other variables through the use of conditional means. Tests comparing the percentage of police officers by race in different specialties that have engaged in a shooting find statistically significant differences between the races by assignment, but the claim is that the differences are likely to be explained away by such factors such as the different tasks being performed within each type of category (Fyfe, [198]).

TABLE VII
Explaining the Rate at Which Police Shoot Civilians

Endogenous Variable	Exogenous Variables										Adjusted R ²	F-Statistic
	% of Police Force Black Male	% of Police Force Black Female	% of Police Force White Male	% of Police Force White Female	Per Capita Number of Felonious Killings of Police Officers	Per Capita Number of Assaults on Police Officers	City Population	Sworn Officers Per Capita				
Per capita number of civilian shootings	-.000135 (3.671) 1.4%	...	-.0000543 (2.164) 8.3%000142 (2.843) 3.6%	2.31 e-7 (3.093) 2.7%	7.30 e-11 (2.331) 6.7%	.0175 (1.788) 13.4%	.9379	20.29		
Per capita number of civilian shootings	...	-.00005 (.988) 36.9%000085 (2.072) 9.3%	.0000704 (2.109) 8.7%	-2.64 e-8 (.285) 78.7%	-2.57 e-11 (.697) 51.7%	-.00362 (.428) 68.6%	.8420	7.81		

Notes: Using fixed year and city effects. Absolute t-statistics are shown in parentheses and the level of statistical significant for a two-tailed t-test are shown below that. Regressions are run using ordinary least squares.

The results reported in Table VII imply that more black or white male officers lower the number of civilians shot, whereas increasing the number of white females (but not black females) implies an increase. The effects are also quite large with a one standard deviation increase in the black male share of the police force reduces civilian shootings by 1.4 per 100,000 citizens and for white males the reduction is .58 per 100,000 citizens. By contrast, a one standard deviation increase in white females increases shootings by .87. Both regressions also imply that increasing the number of felonious police killings increases the number of accidental shootings of civilians. The other results are more mixed. In the specification that includes the male share of the police force, only the coefficients for assaults and population are statistically significant.

In conclusion, the results for assaults on officers are consistent with women being physically weaker than men. Criminals are more likely to attack if they believe that an attack will successfully allow them to escape. Consistent with the hypothesis, mentioned in the introduction, that female officers have a shorter time to react to perceived threats because they must make a decision before they come into physical contact with the criminal, there is some preliminary evidence that male officers are more likely to avoid shooting civilians. Interestingly, the reduction appears to be greatest for black male police officers. More information is required to draw definitive conclusions for the deaths by police, but, compared to other officers, blacks are the more likely to die from accidents than from a criminal's attack. Additional information on police violence might have provided some important insights. We may be willing to put up with a less-effective police departments if they deal with suspects in less-violent ways.

VII. MIGHT THE HIGHER CRIME RATES BE DUE TO CHANGING RULES LOWERING THE QUALITY OF ALL NEW EMPLOYEES? DOES AFFIRMATIVE ACTION AFFECT THE BEHAVIOR OF EXISTING OFFICERS?

The changing crime rates may be due to additional minorities being hired, but it is also possible that increasing the minority share of police forces may be correlated with a lowering of standards for all new police

officers. Thus, it might not be a greater share of police officers who are minorities that are related to higher crime, but the causation may run from lowering standards for all officers to more crime. Thus, an increasing minority share is merely correlated with higher crime. Rules that base promotion less on merit may also reduce the efforts by all existing officers. This seems most plausible, if only because of the very large impacts that hiring minorities appear to have on crime.

If indeed it is the lowering of overall quality that explains the higher crime rate, the simplest way of detecting it is by examining the relationship between each group's absolute effect on a crime rate and the change in its share of the police force. If the change in a group's share of the police force was merely proxying for the overall change in the entire police force's quality, the largest coefficients would be observed for those groups with the smallest number of new police officers, while those with the largest changes would have the smallest coefficients. However, the Pearson correlation coefficient between each group's effect on murder and the change in their share of the police force is only $-.17$ and is not statistically significant. The corresponding correlations for the other violent crime categories are similar: rape is $-.19$; robbery, $-.24$; and aggravated assault, $-.22$. Although this evidence does not reject the spillover hypothesis, it also does not provide much support. Spillovers may explain a portion, but not all, of the differences in coefficients.

Another test examines quasi-reduced forms corresponding to the regressions shown in Table IV. The difference here is that in addition to the instruments (the consent decree dummy, the number of years that the consent decree has been in effect, the mayor's race) the racial composition of the police department is also included. Given that the percent share of blacks and whites in a police force is highly correlated with the presence of consent decrees and the length of time that they have been in effect, this represents a very conservative test for distinguishing whether rules might have an impact over and above the changing racial composition of police departments. When only the consent decrees are included, they produce consistent significant positive impacts on

crime (analogous to the results using the smaller sample discussed near Table III).³⁴ This test is also imperfect because cities with consent decrees were not the only ones changing their hiring and promotion rules. Other cities that have changed their rules either voluntarily or under the threat of being faced with a consent decree will also be changing their hiring practices. Thus, even evidence that only the racial composition variables matter and that consent decrees have no effect does not allow us to reject the hypothesis that higher crime rates are due to both.

The two different consent decree variables may also help us distinguish whether affirmative action changes the marginal quality of new officers or effects which officers are promoted, thereby altering the incentives of the existing police force. If the variable for the number of years that the decree has been in effect proxies for the percentage of the department that has been hired under the new hiring standards, a positive impact from the number of years provides evidence that general hiring practices are important. The consent decree dummy variable is less clear in either the county or state fixed effects specifications because it could be picking either the type of city on which consent decrees are imposed and/or the immediate impact of the new rules. If the consent decree dummy variable is measuring the immediate effect, any large changes in crime would presumably be attributed to changing the behavior of the existing police force and not simply new hires.

Table VIII reports the results for violent and property crime rates using city or county fixed effects.³⁵ A more detailed breakdown of the county fixed effects when the gender and racial groups are simultaneously included with the consent decree information are reported in Appendix B. Despite the collinearity between the composition of the police

forces and the number of years that the consent decrees have been in effect, certain patterns are evident in Table VIII. Overall, the results imply that consent decrees raise crime rates independently of the changing racial or gender composition of the police force. For both violent and property crimes, there is evidence that consent decrees matter because they alter the behavior of the existing police force. For property crimes, the quality of the new hires produced by consent decrees also appears to matter with each additional year that the decree is in effect raising property crimes by another 1.7%–1.9%. Increasing the number of black officers on a police force independently of the length of time that the consent decree has been in effect is associated with increased violent crimes, though the inclusion of Hispanics and American Indians together with blacks to examine minorities as group produces a much smaller and not statistically significant effect.³⁶

These specifications were also used to examine whether more black police officers had a differential impact in more heavily black areas by interacting the percentage of the police force that is black with the percentage of the population that is black. The specification corresponding to the first row in Table VIII implied that hiring more black officers produces more violent crime in more heavily black areas (the coefficient is 1.864; *t*-statistic = 1.922). Including the interaction has little effect on the other coefficients. These interactions imply that black officers are particularly ineffective at dealing with crime in black communities.

An important question is whether the size of the police force alters the impact of the hiring programs. For example, a large department might be able to reallocate new affirmative action hires to specific jobs where their impact on the functioning of the police force might be relatively small. This effect would presumably be most noticeable if the regressions measured the number rather than the percentage share of minority officers. However, it is not clear why increasing the share of minority officers should be easier

34. These additional results are available on request from the author.

35. By comparison, the violent crime estimates using county as well as different state fixed effects for each year result in a coefficient for the percentage of the police force that is black of 1.721 (*t*-statistic = 3.172) and for the consent decree of .3592 (*t*-statistic = 2.230). Similarly for the percentage of the police force that is a minority the coefficient is 1.495 (*t*-statistic = 3.658) and the coefficient for the consent decree is .3092 (*t*-statistic = 1.921).

36. Evidence from quasi-reduced form regressions when all the additional different racial and gender measures are simultaneously controlled for is mixed (see Appendix B).

TABLE VIII
 Attempting to Disentangle Whether Higher Crime Rates are Due to Lower Quality
 of All New Employees: Using City or County and Year Fixed Effects

Endogenous Variable	Exogenous Variables							Adjusted-R ²	F-Statistic
	Percentage of the Police Force That Is Black	Percentage of the Police Force That Is a Minority	Percentage of the Police Force That Is Male	Consent Decree Dummy	Number of Years That Consent Decree Is in Effect	City Has a Black Mayor			
City fixed effects:									
Violent crime rate	.7873 (1.647)3286 (1.996)	.0077 (0.685)	.0737 (1.407)	.8945	20.51	
Violent crime rate3816 (1.071)3148 (1.896)	.010 (.912)	.077 (1.455)	.8940	20.42	
Violent crime rate	-.038 (.195)	.3412 (2.091)	.011 (1.096)	.078 (1.498)	.8951	20.86	
Property crime rate	.195 (.513)1961 (1.514)	.018 (2.054)	.054 (1.321)	.6832	5.96	
Property crime rate356 (1.275)214 (1.644)	.019 (2.214)	.055 (1.334)	.6844	5.99	
Property crime rate0712 (.466)	.191 (1.491)	.0175 (2.098)	.0534 (1.307)	.6884	6.14	
County fixed effects:									
Violent crime rate	1.425 (3.196)4631 (3.517)	.009 (0.732)	.1532 (3.079)	.8510	15.68	
Violent crime rate	...	1.253 (3.518)4038 (3.044)	.0104 (.862)	.1545 (3.116)	.8518	15.77	
Violent crime rate	-.003 (.014)	.4775 (3.607)	.0173 (1.467)	.166 (3.336)	.8486	15.57	
Property crime rate	.132 (.393)1279 (1.34)	.0293 (3.406)	.082 (2.352)	.6489	5.75	
Property crime rate1286 (.504)1325 (1.430)	.0293 (3.462)	.082 (2.359)	.6490	5.75	
Property crime rate0504 (.316)	.1260 (1.385)	.029 (3.617)	.081 (2.373)	.6550	5.93	

Notes: Although not all the coefficients are reported, these regressions are based on the reduced forms of the regressions used in Table IV. Absolute t-statistics are shown in parentheses.

for large departments to accommodate, and the reverse could even be true if decisions in large departments are driven more by fixed rules and race-based decisions are harder to hide. To test this, I added a new variable that interacted the percentage of the police force that is black with the number of full-time sworn police officers and included this variable in a version of the regressions shown in Table VIII that only included the percent of the police force that is black. I also tried similar specifications for white, Hispanic, and Asian. In none of these cases was the new interaction statistically significant.³⁷

We are thus left with a mixed conclusion. The weight of the evidence indicates that at least a portion of the crime-increasing effects of hiring minorities is picking up more general changes in the way all hiring and promotions are conducted, but the evidence for this is not overwhelming and cannot explain most of the impact that hiring minorities has on crime.

VIII. MEASURING THE COST TO VICTIMS FROM THE CHANGING RACIAL AND GENDER COMPOSITIONS OF POLICE DEPARTMENTS

A recent National Institute of Justice study estimates the victim costs of different types of crime based on lost productivity, out-of-pocket expenses such as medical bills and property losses, and losses for fear, pain, suffering, and lost quality of life (Miller, Cohen, and Wiersema [1996]). Although there are questions about using jury awards to measure losses such as fear, pain, suffering, and lost quality of life, the estimates provide us one method of comparing the

changes in different types of violent and property crimes that arise from the changing composition of police departments and allow us to estimate the total cost of these changes.

To provide a conservative estimate of these changes and provide a simple way of separating out the differential effects of the consent decree from the changing racial composition, I reestimated the regressions shown in Appendix 2 using county fixed effects by replacing the racial and gender breakdowns with the data available for 1987 and 1990 that provided information on the percentage of officers for each race by sex (see also Table VI). Despite the reduced sample size, the results used to produce Table IX were consistent with those shown in the appendix.

Because some of the categories involve such a small number of police officers, Table IX examines the changes for only those race and sex groupings that accounted for at least 1% of all police officers in 1990. The top portion of the table lists out the predicted change in crimes from an additional police officer and compares these changes with the average number of crimes per officer for the sample. Holding constant such variables as the size of the police force and the presence of a consent decree, reducing the number of white male police officers by 6,912 people appears to have increased the number of murders by 1,145 and rapes by over 100. This, however, assumes that the white officers would have been replaced by the average new minority officer. In fact, the actual smaller increase in white female officers more than offset the pernicious effect of losing these white male officers. The actual changes among white officers implies that the number of rapes should have declined by more than 280.

The bottom portion of the table multiplies these estimated changes in crime by the Miller, Cohen, and Wiersema estimates of victim costs from crime in 1996 dollars. The increase in violent crimes represents a loss of \$5 billion (\$4.4 billion loss from murder, \$176 million from robbery, \$453 million from aggravated assault, but a gain of \$51 million from fewer rapes), whereas the increase in property crimes represents a loss of \$442 million (\$333 from motor vehicle theft, \$87 million from burglary, and \$22 million from larceny). However, although \$5.4 billion is

37. I also used the reduced-form regressions to answer whether the impact of the consent decrees differed by either the percentage of the police force that was black or the gap between the percent that was black and the percentage of the over-16-year-old population that was black. To do this, I ran the reduced-form regressions with the percentage of the police force that is black as well as with one of two new sets of variables: either the percentage of the police force that is black interacted with the presence of the consent decree and the length of time that the decree has been in effect or the gap between the percentage of the population and the police force that are black interacted with these two variables. The results for the percent black that were previously statistically significant remain so, but the other variables for the consent decree and the various interactions are usually insignificant.

TABLE IX
Calculating the Total Victim Costs That Arose from the Changing Composition of Police Departments

Categories of Police Officers that Constituted at least 1 percent of the Police Force in 1990	Change in Number of Officers Between 1987 and 1990 for the 189 cities for which population numbers are available for both years (% change from 1987)	Total Change in the Number of Crimes Due to the Changing Composition of Police Departments							Total Cost of Change by Type of Officer
		Murder	Rape	Robbery	Aggravated Assault	Motor Vehicle Theft	Burglary	Larceny	
Black males	950 (5%)	97	300	548	4,699	1,711	-729	-6,571	
Black females	1,135 (23%)	483	96	6,868	13,149	-8,131	34,618	64,160	
White males	-6,912 (-6%)	1,145	111	15,579	37,256	-10,857	28,184	-4,034	
White females	1,067 (12%)	-176	-496	-8,588	-11,639	-1,759	-15,315	-23,024	
Hispanic males	1,283 (13%)	180	-365	8,705	6,119	98	8,130	7,670	
Asian Pacific males	1,542 (171%)	-366	-220	-2,877	-5,672	3,474	2,767	18,164	
Total Crimes Per Police Officer for All Police Officers		1,363	-575	20,235	43,912	-15,464	57,655	56,364	
		0,067	0,34	2,03	3,23	4,82	9,6	24,7	
		Total Cost of the Changing Composition of Police Forces from 1987 to 1990, in Millions of 1996 Dollars							
Black Males		343.8	31.3	5.2	52.9	7.6	-1.1	-2.9	\$436.7
Black Females		1,540.8	9.0	59.6	134.2	-32.6	52.1	25.7	\$1,788.9
White Males		3,654.8	10.4	135.3	380.3	-43.4	42.9	-1.6	\$4,178.6
White Females		-561.4	-46.8	-74.6	-118.8	-7.1	-23.2	-9.2	-\$841.1
Hispanic Males		576.6	-34.4	75.6	62.4	394.2	12.4	3.0	\$1,089.7
Asian Pacific Males		-1,169.4	-20.7	-25.0	-57.9	14.0	4.2	7.3	-\$1,247.5
Totals		4,385.2	-51.3	176.1	453.1	332.7	87.3	22.3	\$5,405.4

Notes: Using the estimates that analogous to the county fixed effect regressions in the appendix, though these use breakdown the each racial category by gender and sex with the control variables that we have used through out the article. The National Institute of Justice Estimated Victim Costs by type of crime are used to calculate these estimates. All values are in millions of 1993 dollars. The regressions were only able to use data for 1987 and 1990 because the breakdown by sex within each racial group was only available for those years. [Miller, Cohen and Wiorsema {1996}]

substantial, to put it in perspective, it equals only about 1.1% of the total aggregate losses from these crime categories. These estimates are probably most sensitive to the value of life used (in the Miller et al. study this was set at about \$3.2 million in 1996 dollars). Higher estimated values of life will increase the net costs from changing the racial and gender composition of police departments, whereas lower values will reduce the gains. To the extent that people are engaging in additional private actions to prevent this increased crime (Philipson and Posner [1996]), these numbers will underestimate the total savings from these changing compositions.

IX. THE IMPACT ON ARREST RATES

The effectiveness of police officers can take several different forms, but surely one of the most measurable is the arrest rate. If certain types of police officers are more productive than others, it may reveal itself in terms of higher arrest rates, though all arrests might not be equally valuable. However, a couple of issues about the arrest rate data should be addressed first. Frequently, because of the low crime rates in some of the smaller cities, it is quite common to find huge variations in the arrest rate both across cities and over years. In this sample, the arrest rate for murder ranges from a low of 5% to a high of 14 times the offense rate. The arrest rates for violent crimes range from 10% to 3.6 times the offense rate. This seeming anomaly arises for a couple of reasons. First, the year in which the offense occurs frequently differs from the year in which the arrest occurs. Second, an offense may involve more than one offender. Unfortunately, the FBI data set allows us neither to link the years in which offenses and arrests occurred nor to link offenders with a particular crime. These problems create significantly more variation in the arrest rate than in the crime rate.

Tables X and XI rerun the regressions shown in Tables IV and VI after replacing the natural logs of the crime rates with their corresponding arrest rates. While the coefficients for the percentage of the police force that is either black or minority are consistently negative, Table X indicates that only the arrest rate for robbery is significantly reduced by black and minority police offi-

cers. A similar pattern holds for Table XI. More white officers are generally associated with higher arrest rates, whereas more black, Hispanic, and Asian female officers are associated with lower arrest rates, but coefficients are usually not statistically significant at the 10% level for a two-tailed *t*-test. As the share of black male officers increases, there are statistically significant drops in arrest rates for violent crime, rape, robbery, and assault. More white male officers produce a statistically significant increase in arrests for murder, and more white females significantly increase arrests for robbery. Increasing the share of Hispanic male officers consistently lowers arrest rates, but the one case where the impact of more Hispanic males approaches statistical significance is for rapes—also the one instance in Table VI where they significantly lowered the crime rate. Overall, however, just eight of the 72 specifications in Table XI are statistically significant at the 10% level for a two-tailed *t*-test, and another 14 coefficients are significant at the 15% level.

Under the assumption that each dependent variable represents an independent test of the hypothesis (which would be appropriate for subcategories that are mutually exclusive), there is a test for significance over all regressions. The inverse chi-square test, known also as the Fisher test, can be used to assess overall significance (e.g., Maddala [1977, 47–48] and Hedges and Olkin [1985]). For the seven subcrime categories shown in Table XI, this test implies that increasing the share of black male police officers significantly reduces arrest rates at the .5% level. For Asian-Pacific male officers, that is true at the 10% level. Black female officers are associated with fewer arrests for violent crime at the 1% level. The results for white male and female officers show that as their shares rise, so do arrest rates (these results are statistically significant at the 10% level).

A different approach is to ask whether the different racial and gender groupings are statistically different from each other. By this weaker standard, most of the violent crime arrest rates for blacks and whites in Tables X and XI are significantly different from each other, and Table X shows that for murder white male officers' arrest rates are significantly higher than those for all other racial groups.

TABLE X

Explaining Changes in the Arrest Rate, Using the Specifications From Table IV:
Using Two-Stage Least Squares to Take Into Account the Impact That Consent Decrees
and the Presence of a Black Mayor Have on the Composition of Police Departments

Arrest Rates	% of the Police Force That Is Black			% of the Police Force That Is Minority (Black, Hispanic, and American Indian)			% of the Police Force That Is Male		
	City	County	State	City	County	State	City	County	State
Violent crime	-2.48 (.729)	-2.72 (1.168)	-4.15 (2.04)	-1.71 (.777)	-1.94 (1.137)	-1.56 (1.472)	6.37 (.942)	7.30 (.995)	1.09 (.409)
Property crime	-7.68 (.584)	-12.33 (.981)	.899 (.094)	-9.268 (1.053)	-7.186 (.847)	3.547 (.769)	33.54 (1.084)	31.42 (.962)	6.77 (.510)
Murder	-4.21 (1.028)	-4.58 (1.355)	-4.33 (1.875)	-5.355 (1.943)	-3.801 (1.597)	-1.21 (.922)	9.489 (1.116)	11.07 (1.135)	-1.037 (.301)
Rape	-5.074 (1.196)	-3.00 (.941)	-3.82 (1.64)	-1.328 (.468)	-1.46 (.681)	-1.76 (1.635)	9.085 (1.170)	7.0265 (1.199)	5.679 (1.781)
Total robbery	-3.98 (1.651)	-3.796 (1.901)	-5.578 (3.287)	-4.382 (2.654)	-3.985 (2.897)	-1.5875 (2.019)	3.077 (.713)	8.681 (1.406)	8.3185 (2.710)
Total assault	-3.176 (.896)	-5.622 (1.724)	-7.66 (3.22)	-2.31 (.995)	-.419 (.225)	-2.077 (1.874)	.77 (.125)	9.568 (1.152)	1.399 (.440)
Burglary	-.735 (.520)	-1.54 (1.151)	-.6945 (.598)	-.3095 (.325)	-.1716 (1.305)	.478 (1.070)	.562 (.225)	4.015 (1.104)	5.252 (2.564)
Larceny	2.22 (1.146)	.0299 (.017)	-2.024 (1.341)	1.231 (.955)	.03115 (.026)	-.6659 (.921)	-2.435 (.642)	.9277 (.228)	9.709 (3.141)
Motor vehicle theft	6.26 (2.132)	2.57 (1.153)	0.400 (.189)	4.01 (1.945)	1.93 (1.176)	1.37 (1.201)	11.38 (1.288)	6.69 (.901)	.73 (.255)

Notes: The second-stage regression estimates that are reported below account for the same variables controlled for in the preceding tables except for the consent decree, the number of years since the consent decree went into effect, and whether the city's mayor is black, which were instead included in the first-stage regression. The absolute *t*-statistics are shown in the parentheses below, with the percentage of a one-standard-deviation change in the endogenous variable that can be explained by a one-standard-deviation change in the exogenous variable. All regressions use weighted least squares where the variables are weighted by the city population. Sample size is 634 and covers the years 1987, 1990, 1993.)

These results are certainly not as strong as those for the crime rates, but they do indicate significant differences in arrest rates between racial and gender groups. They also provide additional evidence to rule out the possibility that the higher reported crime rates shown earlier are a result of victims responding to higher expected payoffs due to higher arrest rates. Further work still needs to be done in evaluating the relative quality of different arrests.

X. HOW DOES THE CHANGING RACIAL AND GENDER COMPOSITION OF POLICE DEPARTMENTS ALTER HOW POLICE DEPARTMENTS ARE ORGANIZED?

The Department of Justice's LEMAS survey provides a wealth of other information

about police departments that can give us some insight into how changing the demographic composition of police officers alters how police departments operate. Among the information available is the percentage of police patrol units (both car and walking patrols) with only one officer, the percentage of police walking patrol units with only one officer, and the number of motorcycles and cars per officer. The most obvious predictions stem from the differences in physical strength of female and male officers. If there are significant differences in strength, it effectively raises the cost of having single-officer patrol units. As long as the percentage of women officers is small relative compared to the number of preexisting two officer patrol units, it is possible that women

TABLE XI

Explaining Changes in the Arrest Rate, Using the Specification from Table VI to Further Examine the Differences by Race and Sex: Using Two-Stage Least Squares to Take Into Account the Impact that Consent Decrees and the Presence of a Black Mayor Have on the Composition of Police Departments

Category of Arrest Rate Being Explained	% of Police Force Black Male	% of Police Force Black Female	% of Police Force Hispanic Male	% of Police Force Hispanic Female	% of Police Force White Male	% of Police Force White Female	% of Police Force Asian Pacific Male	% of Police Force Asian Pacific Female
Violent crime	-8.28 (1.919)	-17.5 (1.01)	-7.10 (.529)	58.7 (1.466)	4.04 (1.38)	32.6 (1.244)	-348.7 (1.295)	197.3 (1.10)
Property crime	-4.04 (0.849)	-10.3 (0.54)	-14.61 (.890)	32.9 (.074)	15.22 (1.02)	89.7 (.851)	-335.3 (1.284)	-3648.6 (1.08)
Murder	-10.78 (1.524)	-40.5 (1.534)	-26.44 (1.091)	-51.7 (.777)	9.10 (1.89)	-5.94 (0.244)	-144.3 (.699)	-539.6 (1.781)
Rape	-16.14 (2.301)	-46.0 (1.597)	25.01 (1.546)	106.8 (1.471)	6.26 (1.311)	22.82 (0.704)	-616.2 (1.610)	205.1 (0.695)
Total robbery	-5.42 (1.732)	-24.6 (1.53)	-22.5 (1.552)	26.27 (0.795)	3.35 (1.395)	22.07 (1.681)	-34.1 (0.603)	62.64 (.447)
Total assault	-8.54 (1.98)	-21.28 (1.16)	-8.05 (0.598)	-69.44 (1.78)	4.11 (1.45)	12.00 (.871)	-137.5 (1.567)	195.63 (1.156)
Burglary	-3.32 (1.45)	-10.65 (1.09)	-10.60 (1.16)	5.74 (0.26)	2.09 (1.36)	12.88 (1.17)	-186.3 (1.269)	-.190 (0.002)
Larceny	-.201 (0.064)	.77 (0.06)	-6.77 (0.64)	99.55 (0.35)	.57 (.261)	7.19 (0.479)	-147.4 (1.497)	35.4 (0.271)
Motor vehicle theft	2.54 (0.804)	19.4 (1.54)	8.87 (0.847)	33.69 (1.19)	-2.86 (.394)	22.46 (0.099)	56.85 (0.863)	204.1 (1.690)

Notes: The Second-stage regression estimates that are reported above account for the same variables controlled for in the first stage regression except for the consent decree, the number of years since the consent decree went into effect, and whether the city's mayor is black. Current rather than lagged crime rates are used in the first-stage regression. County fixed effects are used, with additional separate fixed effects for each state by year to pick up any changes at the state level that might explain changes in crime rates over time. All regressions use weighted least squares where the variables are weighted by the city population. Sample size is 439 and covers the years 1987 and 1990.

may be substituted into one of the “men’s slots” in an existing two officer unit. Yet, even here substitutability might not be perfect because the two-officer unit may have been set up precisely because the physical strength of two male officers was desired. Even though data on the race and sex composition of each single and two-officer patrol unit is not available, it is still possible to examine how the use of these different patrols differs over time in a city as the composition of the racial and sex composition of the police department changes.

Although not a systematic analysis, the data confirm certain regularities. For example, the police departments with the most two-officer patrols tend to be those in the largest cities. For the cities for which data were available, the top ten cities with the

most two-officer patrol units include Detroit, Los Angeles, Chicago, and Buffalo. Only two-city police departments had no women who were full-time sworn officers (Schenectady, N.Y., and Reno, Nev.), and those departments averaged 58% fewer two-officer units (only 5%, compared to the average for the rest of the departments of 12%). Those departments with more than the median percent of male officers were also less likely to have two-officer patrols (10%) than departments with fewer than the median number of male officers (14%).

Table XII uses the same regression specifications employed in Appendix 2 to explain the organization of departments. Because the data for the percentage of police patrols and the percentage of walking patrols with only one officer are only available for one year, it

TABLE XII
How Does the Changing Racial and Gender Composition of Police Departments Alter
How Police Departments Fight Crime?

	% of Police Force				Number of Years that Consent Decree Is in effect				Adjusted R^2 and No. of Observations				Number of Years That Consent Decree Is in effect				Adjusted R^2 and No. of Observations			
	Male	Black	Hispanic	American Indian	Consent Decree Dummy	Consent Decree Is in effect	City Has a Black Mayor	Observations	Adjusted R^2	No. of Observations	Consent Decree Dummy	Consent Decree Is in effect	City Has a Black Mayor	Observations	Adjusted R^2	No. of Observations				
% of police patrol units with only one officer	1.09 (2.08)	-.80 (2.09)	-.377 (2.11)	-5.07 (1.481)	-.357 (2.95)	-.014 (1.471)	-.037 (.619)	.55	.55	103	-.305 (2.54)	-.001 (1.04)	.03 (.525)	.56	.56	103				
% of police walking patrol units with only one officer	2.19 (1.93)	1.734 (1.04)	.169 (.368)	-11.98 (1.623)	.44 (2.49)	-.077 (3.386)	-.033 (.311)	.77	.77	103	.383 (2.319)	-.07 (3.171)	.002 (.024)	.77	.77	103				
Ratio of walking patrols to all patrols	.082 (0.52)	-.214 (2.13)	.065 (1.230)	-.637 (.554)	-.052 (2.72)	.0036 (1.570)	.004 (.224)	.63	.63	203	-.039 (2.05)	.0023 (1.008)	-.007 (.458)	.62	.62	203				
Number of cars per officer	.105 (1.64)	-.1833 (1.12)	-.078 (.877)	-1.347 (.750)	-.040 (.915)	-.005 (1.205)	-.0185 (.844)	.72	.72	639	-.035 (.806)	-.0056 (1.669)	-.021 (.964)	.72	.72	639				
Number of motorcycles per officer	.076 (1.89)	.036 (1.40)	.0061 (.444)	.0296 (.106)	.00026 (.037)	-.00043 (.672)	-.003 (.883)	.51	.51	439	-.0008 (1.21)	-.00031 (.495)	-.002 (.703)	.51	.51	439				
Special operations officers required to wear body armor (yes = 3, sometimes = 2, no = 1)	-1.97 (1.50)	-2.44 (2.70)	.236 (.518)	-16.55 (1.931)	-.187 (.595)	-.0068 (.285)	.0897 (.654)	.46	.46	438	-.0203 (.065)	-.0206 (.875)	.0002 (.001)	.45	.45	438				
Patrol officers required to wear body armor (Yes = 3, sometimes = 2, no = 1)	-1.87 (1.44)	-1.52 (1.70)	-5.399 (1.196)	-3.025 (.357)	-.0002 (.001)	-.0374 (1.587)	-.1375 (1.013)	.53	.53	438	.042 (1.39)	-.041 (1.779)	-.1718 (1.32)	.5356	.5356	438				
Number of sworn officers per capita	.0002 (.224)	.0012 (.236)	-.0011 (2.005)	.0023 (.607)	.00072 (2.251)	-.00003 (3.142)	-.31e - 6 (.071)	.95	.95	641	.00079 (2.465)	-.00003 (2.853)	6e - 6 (.143)	.95	.95	641				

Notes: Controlling for a City's changing Demographic Characteristics, the Per Capita Number of Police Officers, the Unemployment Rate, the Average Weekly Wage, Year Fixed Effects, and City Population and Population Squared. State fixed effects are used in the first three specifications, while county fixed effects are used for the other specifications. The absolute *t*-statistics are shown in the parentheses below, with the percentage of a one-standard-deviation change in the endogenous variable that can be explained by a one-standard-deviation change in the exogenous variable. All regressions use weighted least squares where the variables are weighted by the city population. Data for the first three regressions are only available for 1987, the data for the number of motorcycles and cars per officer as well as information on body armor requirements are available for 1987 and 1993, and the data for the number of sworn officers are for 1987, 1990, and 1993.

is not possible to run these regressions using fixed city or county effects, though there are enough observations here to use state fixed effects. In addition, since the data are only available for 1993, we cannot break down the race categories by sex.

The first two regressions reported in Table XII imply that increasing the female officers' share of the police force dramatically increases the number of two-officer patrol units. The average police department has 88% of all police patrols as one-officer units, but the coefficient on the first regression implies that a one percentage point increase in the share of officers who are female increases the number of two-officer patrol units by 1.1 to 1.3 percentage points. The effect is quite important in explaining the behavior of police departments, with about 46%–55% of a one standard deviation change in the percentage of patrols with one officer being explained by a one standard deviation change in the percentage of a department that is female. I also tried reestimating these results using two-stage least squares along the lines shown in Table IV (but with the crime rate variable in the second regression in the two-stage least squares replaced with a variable measuring single officer patrols). These estimates are similar to those already shown: a greater share of black and minority officers reduce the number of single-officer units, while a higher share of male officers increases the proportion of single-officer units.³⁸

Another concern about the changing composition of police departments involves foot patrols. Possibly because women are relatively less well suited for foot patrols and because it is difficult to exclude women of-

ficers from this task once they are on the police force, the presence of women police officers has an even greater upward effect on the number of walking patrols with two officers. The second regression in Table XII indicates that each one percentage point increase in the percent of police who are women increases the share of two officer foot patrols by two percentage points.

The third regression examines the percentage of all patrols that are foot patrols. If women officers are relatively less desirable as foot patrol officers, more women officers might result in police departments not only increasing the number of police officers assigned to each patrol but also switching from foot patrols to car patrols. The evidence, however, does not support this hypothesis, with changes in the number of officers per patrol apparently offsetting the weaknesses produced by increasing the number of women officers. Although the male share coefficient is indeed positive, it is very small (a one percentage point increase in the female share reduces the foot patrols by only eight hundredths of a percent) and is statistically insignificant.

The LEMAS survey also provided information on the number of cars, motorcycles, bicycles, boats, helicopters, and airplanes used by police departments. As a rough second check on the results supporting the hypothesis that more women officers will reduce the number of single officer patrol units, the number of police cars per officer provides an independent measure of whether officers patrol together. Presumably the more cars per officer, the less likely that multiple officers will be patrolling together in the same car. Consistent with the already reported results, more female officers do reduce the number of cars per officer.

While not related to testing whether female officers are more likely to be paired up with other officers in patrols, the other methods of transportation at least provide a measure of whether the changing demographic composition of police departments alter how they operate. However, with the sole exception of motorcycles, none of the other modes of transportation appear related to any of the race or gender measures. The estimates for motorcycles imply that the increased presence of women officers reduces police departments' reliance on motor-

38. A seminal article by Wilson and Boland [1978] pointed out that it is not simply the total number of police or the size of their budget that matters but also how the police are allocated that determines their effectiveness. They argued that a more "aggressive patrol strategy," one that was associated with more single-officer patrol units, was effective in reducing crime rates. If they are correct and consent decrees make it more difficult for police departments to deploy police officers in single-officer patrol units, this might help identify one of the mechanisms for the increase in crime. However, including the variable measuring the percentage of patrols that are done by single officers in the regressions shown in Table III does not appear to be statistically significant, and it does not alter the significance of the racial or gender composition variables.

cycles and that the size of the effect is about two-thirds to three-quarters the size of women's impact on the number of cars. Undoubtedly, automobiles are the most important portion of police department expenditures on transportation, though except for the number of cars and motorcycles there is no real evidence that altering the race or gender composition of police departments changes how police departments allocate their money for transportation.

Before finishing this discussion of the size of patrol units and modes of transportation, a couple of comments should be made about the other coefficients. The racial composition of police departments only seems to help explain the percentage of police patrols with one unit and the percentage of walking patrols. More black and Hispanic officers increase the number of two-officer patrols, while more whites reduces them. This finding is consistent with minority officers operating in more dangerous areas, but it is also consistent with the desire to pair the officers together to compensate for other deficiencies. In an attempt to separate out these two explanations, I tried including violent and property crime rates as well as the per capita number of felonious police killings and police assaults in all these regressions as measures of greater risks, but these variables had very little effect on the results. Combined with the earlier results on police killings and assaults, it does not appear that the increased reliance on two officer units when more minorities are present can be explained by reference to minority officers operating in more dangerous areas.

Given that we have state-level fixed effects for these regressions, the consent decree variables imply that these decrees are imposed on cities with relatively high reliance on single-officer and walking patrols but that the longer these decrees are in effect, the more these cities switch away from these types of patrols. The estimates further support the hypothesis that changing the racial and gender composition of police departments subject to these consent decrees resulted in lower quality officers, which was compensated for by doubling up officers.

We next examine variables for whether police officers or special operations officers are required to wear body armor. However, these variables have the potential to provide

additional information on whether certain officers face more dangerous tasks, though it also runs into possible difficulties in separating out the question of dangerous risks from issues involving discrimination. The results imply that black officers (both for patrol and special operations) are less likely to be in police departments that require body armor, whereas the reverse is true for white patrol officers. In an attempt to separate out the discrimination story from the lower-risk explanation, I added a variable to the regression that interacted the percentage of the police force that is black with the dummy variable for whether the mayor is black. If the lack of a requirement for body armor arises because of discrimination, one would expect such an effect to disappear when a black mayor is elected; thus, the interaction term would have to be positive. In the specification for patrol officers this new interaction is negative and statistically significant, while for special operations officers, the effect is positive but quite insignificant (the *t*-statistic is .257). The probability that a police department will require police officers to wear protective body armor also increases as there are more women officers on the police force, though the coefficients are only statistically significant at the 10% level for a one-tailed *t*-test. This result provides some evidence that police departments are relatively more concerned in protecting female officers from attacks.

Another important issue involves the changes in police department size and thus changes in police experience that accompany the adoption of consent decrees. Appendix 1 examines this issue but finds no evidence that this can explain the results reported here.

XI. CONCLUSION

A massive experiment has been conducted with law enforcement during the last couple of decades, with more minority and women officers being hired. But does increasing the number of minority and women police officers raise effectiveness by drawing on new untapped abilities, or are standards lowered too far in order to hire large numbers of minorities and women? I have argued here that the effect depends on the type of crime. The evidence for rape is mixed, with most

results implying essentially no difference between male and female officers, though some estimates indicate that the actual changes in the composition of police departments helped reduce the number of rapes. However, for all other crimes, more black officers are associated with more crime, not less. But it would be a serious mistake not to realize that this simple relationship is masking that the new rules reduce the quality of new hires from other groups. This does not say that there are not large potential benefits from minority police officers, but only that the new rules under which new officers have been hired have costs that outweigh the benefits.

So why do we observe different findings for minority and female officers? At least part of the difference appears to arise from how the hiring rules have been altered for the two groups. Physical strength tests involve norming, whereas written tests have been altered so as to produce equal pass rates across different groups. Norming may allow lower-quality applicants in the protected category, but it at least does not lower the quality of all new recruits. The results suggest that if affirmative action is to be practiced, norming is the less costly way to go. This raises a question that economists have thus far ignored: why are different types of affirmative action used in different settings? Why does academia use norming for admissions but police forces choose to alter the testing?³⁹

Changes in the composition of police departments have been accompanied by changes in the organization of police departments. Some of these changes—such as an increasing movement away from single-officer patrol units—is likely due to the presence of more female officers with less physical strength. Women officers are more likely to be assaulted than men, though their overall probability of death on the job is the same. Some preliminary evidence indicates that white women officers are more likely to shoot civilians and that black male officers are the least likely. The evidence is not consistent with the hypothesis that black officers

are more effective at dealing with crime in predominately black areas. Instead, surprisingly, the results suggest that it is the most heavily black communities that are the most at risk from the increased crime produced by affirmative action policies.

Other recent research confirms the basic finding in this paper. While Donohue and Levitt [1998] examine the issue of how nonwhite and white officers impact crime by members of their own group and by the other group, taking their sensitivity estimates of “crime rates to racial composition of the police force” and instead asking what happens to the total crime rate when a white officer is replaced by a nonwhite officer implies a large increase in violent crime in eight of their ten specifications.⁴⁰ While they claim that nonwhite officers relatively reduce white crime and white officers relatively reduce nonwhite crime, the perverse effect that they find of nonwhite officers on nonwhite crime dominates in eight of their ten violent crime specifications.

As a warning for anyone doing future research: the evidence suggests that a great deal of caution needs to be exercised in aggregating different racial and/or gender groups. Not all nonwhite racial groups are the same, and not all men and women in a particular group are the same. Blacks, Hispanics, and Asians do not have the same impact on crime. Many differences between

40. The two-stage least squares estimates for both violent crimes and property crimes imply the largest increases in crime when a white officer is substituted with a nonwhite officer. There are many issues that the authors do not address in their paper. The impact of cross race assignment of officers is puzzling given the prohibitions against assigning patrol officers based on their race (see note 17). It is not clear how two-officer patrol units will favor particular racial groups when the officers are from different racial backgrounds. For example, given the relatively small number of minority officers (see Tables I and II), the rate at which minority officers will be assigned together should be extremely small in the vast majority of police departments. Given the desire to team inexperienced officers with more experienced ones, the probability of that both officers in a patrol unit will be minority officers will lag any changes in the racial composition of the department. The research in this paper suggests that there is some danger in aggregating all nonwhite groups together and in not disaggregating by sex. The current article also goes further in using the amount of time a consent decree is in effect and also in using this instrument to try to differentiate whether the changes in crime rates are due to the nonwhite officers or the affect that the changing rules have on the quality of all new hires.

39. Of course, in recent years, SAT tests have also moved to altering the tests and how the different parts of the tests are weighted so as to equalize the scores of women and men.

men and women on crime also disappear once different racial groups are subdivided by sex. The different results obtained from aggregated and disaggregated classifications strongly suggest that the most disaggregated classifications should be used whenever possible.

This article was initially motivated by the Supreme Court's recent rulings on affirmative action. Prior to consent decrees, the "best" police officers might not always have been hired, but the imposition of consent decrees appears to have increased crime, and the longer the decree was in effect the greater was the increase in crime. The hiring of minority officers thus does not appear to meet the difficult strict scrutiny standard set forth by the Supreme Court. There may be strong moral arguments for affirmative action, but crime reduction is not one of them. The results do suggest that if preferential hiring is to be practiced, changing testing standards is much more costly than norming.

APPENDIX A ACCOUNTING FOR CHANGES IN POLICE DEPARTMENT SIZE AND CHANGES IN LEVELS OF POLICE EXPERIENCE

An important question involves what these consent decrees did to the size of the police departments and what effect that this may have had on police experience.⁴¹ The last set of regressions in Table XII use fixed city effects and imply that the imposition of a consent decree is associated with a large increase in the number of officers of about a third, and it takes about 20–25 years before the city's police force returns to its predecree levels.⁴² Including a squared term for the years after the imposition of the consent decree did not appreciably alter this basic relationship. Since I controlled for the presence of the consent decree and the length of time that it had been in effect in both the two-stage least square and the quasi-reduced form estimates, this finding does not alter any of the earlier regressions, though it does make us ask whether the significant effects of the consent decrees are due to changes that result from testing and/or whether they arise from the lower quality associated with a rapid increase in the size of the police force. A rapidly growing police force with new recruits might face an increase in crime simply because of having police officers with less experience.

41. On average, there are about 2.2 police officers per 1,000 residents in a city.

42. This pattern of growth is consistent with what is mandated by consent decrees which required an usually large number of officers to be hired immediately (e.g., 411 F. Supp. 218).

Unfortunately, I do not have a measure of police officer experience. One substitute method of measuring this change is to reestimate the regressions shown in Tables IV and VI by including a variable for the percentage change in the size of a police force.⁴³ While this reduces our sample size to 386 observations for blacks and minorities and 393 for males, the percentage measures of the composition of police departments remain similar to those already reported and the percentage change in a police force's size is almost always negative, though it is statistically significant about a third of the time. Thus, if anything, there is weak evidence that large percentage increases in the size of police departments appear to reduce crime even after the per capita number of police officers is already controlled for.⁴⁴

Another way to differentiate between these two theories is to examine the changing impact that the consent decree has on crime over time. If the lower initial quality were due to the large sudden increase in the number of officers, the quality of new officers hired after that initial binge should be relatively high. Just as the initial hiring binge would have brought in lower-quality officers, the long period of time over which hiring was below normal, as the city tries to return to its original-sized police force, would result in above-average quality hires. The coefficient on the variable for the number of years that the consent decree has been in effect should thus be negative in the reduced-form regressions. In fact, almost all these coefficients in the preceding tables are either significantly positive or insignificantly different from zero.⁴⁵

43. The percentage change is defined as the change in the number of sworn police officers between two years divided by the average of those two year's number of police officers.

44. I also tried replacing the variables for the percent of the police force that is black with that variable interacted with the percent change in a police force's size. The results were again implied that increasing the percent of a police force that is black increases the crime rate. The t-statistics for this new variable in the violent and property crime regressions with city fixed effects are 3.092 and 2.919.

45. Landy [1992] argues that the increased productivity police officers acquire from experience are essentially produced during their first three years of service. Another method of testing the impact of lack of experience is rerun the regressions in Table IV with a dummy variable equalling one for the first three or four years after a consent decree has gone into effect. While this variable is sometimes positive and significant, the coefficients for the percent of the police force that is black, minority, or male remain essentially unaltered. Finally, in an attempt to measure the actual change in the number of new officers, I contacted the largest 60 police departments and obtained the number of police officers leaving employment by year for Chicago, Cincinnati, Colorado Springs, Columbus, Dallas, Denver, El Paso, Honolulu, Houston, Indianapolis, Memphis, Nashville, New York, Oklahoma City, Philadelphia, Sacramento, San Antonio, San Jose, and Syracuse. For most cities, the data imply a fairly consistent retirement rate for departments across years. For example, New York experienced 506 retirements in 1987, 568 in 1990, and 528 in 1993. Adjusting my estimates of the number of new police officers using these retirement rates leaves those already reported results essentially unchanged.

TABLE B1
 Explaining Crime Rates as a Function of the Racial and Sex Composition of Police Departments:
 Using County Fixed Effects

	% of Police Force			% of Police Force			% of Police Force			Number of Years that Consent Decree is in effect			Number of Years that Consent Decree is in effect			City has a Black Mayor	Adjusted R ²
	Male	Black	Hispanic	American Indian	Hispanic	Black	White	Male	White	Asian	Dummy	Dummy	Consent Decree in effect	Consent Decree in effect	Consent Decree in effect		
Violent crime	2.46 (2.73) 21%	1.65 (3.64) 21%	0.98 (1.842) 12%	-1.04 (2.56) 1%	0.43 (3.17) 25%	0.02 (1.389) 5%	0.15 (3.00) 5%	2.332 (2.61) 20%	-1.435 (3.77) 25%	-1.358 (3.57) 7%	0.403 (3.05) 23%	0.019 (1.64) 6%	0.151 (3.07) 5%	.85			
Property crime	0.79 (1.25) 12%	0.01 (0.27) 0%	-0.13 (2.79) 3%	-0.93 (3.23) 1%	0.14 (1.449) 14%	0.03 (3.586) 18%	0.08 (2.30) 4%	0.78 (1.24) 12%	0.01 (0.33) 0%	0.09 (3.24) 1%	0.13 (1.401) 14%	0.03 (3.68) 18%	0.03 (2.33) 4%	.64			
Murder	0.59 (4.03) 3%	1.39 (1.89) 10%	2.59 (2.42) 19%	3.59 (5.41) 1%	0.54 (2.49) 18%	0.01 (3.11) 4%	0.30 (3.72) 5%	0.80 (5.53) 4%	-1.78 (2.88) 18%	-1.85 (2.99) 6%	0.58 (2.704) 19%	0.004 (1.87) 1%	0.29 (3.67) 5%	.77			
Man-slaughter	-5.23 (1.59) 37%	3.16 (1.91) 33%	-0.39 (1.64) 4%	3.65 (2.45) 2%	0.09 (1.88) 4%	0.03 (7.56) 9%	0.53 (2.96) 13%	-5.65 (1.74) 40%	-2.36 (1.70) 33%	-1.62 (1.16) 7%	-0.04 (0.92) 2%	0.04 (9.44) 11%	0.54 (3.03) 14%	.45			
Rape	3.78 (1.88) 17%	0.62 (1.61) 4%	-1.36 (9.26) 9%	3.72 (4.09) 1%	0.09 (2.95) 3%	0.05 (1.798) 8%	0.12 (1.06) 2%	3.48 (1.75) 16%	0.04 (0.52) 0%	-0.03 (0.4) 0%	0.03 (1.00) 1%	0.05 (1.91) 9%	0.12 (1.102) 2%	.89			
Total robbery	1.77 (1.75) 4%	1.14 (2.25) 2%	1.46 (1.98) 9%	1.12 (2.45) 0%	0.41 (2.76) 15%	0.04 (2.61) 19%	0.24 (4.33) 7%	1.82 (1.83) 13%	-1.24 (2.92) 17%	-1.25 (2.94) 5%	0.42 (2.87) 20%	0.04 (2.59) 9%	0.24 (4.33) 6%	.89			
Total assault	2.73 (2.52) 10%	2.21 (4.05) 3%	1.67 (1.98) 9%	-4.04 (8.22) 2%	0.49 (3.06) 20%	0.01 (4.81) 12%	0.08 (1.32) 1%	2.47 (2.30) 20%	-1.79 (3.90) 29%	-1.53 (3.33) 8%	0.44 (2.78) 24%	0.003 (2.11) 1%	0.09 (1.44) 2%	.81			
Burglary	0.33 (4.34) 4%	0.09 (2.28) 2%	0.48 (8.59) 9%	0.04 (0.12) 0%	0.17 (1.46) 15%	0.04 (3.79) 19%	0.15 (3.64) 7%	0.45 (5.96) 6%	-0.34 (1.06) 9%	-0.09 (2.86) 1%	0.17 (1.52) 15%	0.04 (3.83) 19%	0.15 (3.64) 7%	.70			
Larceny	0.71 (1.16) 10%	0.12 (3.91) 3%	0.43 (9.48) 9%	1.83 (6.58) 2%	0.22 (2.354) 20%	0.02 (2.73) 12%	0.01 (3.11) 1%	0.65 (1.07) 9%	0.22 (.867) 6%	0.26 (.994) 2%	0.21 (2.28) 19%	0.02 (2.85) 12%	0.01 (.361) 1%	.72			
Motor vehicle theft	2.85 (2.59) 23%	0.83 (1.50) 10%	0.68 (.841) 8%	1.87 (3.75) 1%	0.13 (.80) 7%	0.04 (2.881) 13%	0.26 (4.33) 7%	2.56 (2.35) 21%	-0.28 (.606) 5%	-0.32 (.689) 2%	0.09 (.538) 5%	0.05 (3.07) 14%	0.27 (4.42) 8%	.81			
Assaults on police	-4.12 (7.89) 13%	2.49 (9.50) 11%	4.20 (1.11) 19%	-9.75 (4.13) 2%	2.36 (3.043) 49%	-0.05 (.642) 5%	-2.00 (7.00) 22%	-3.54 (.687) 11%	-3.96 (1.80) 24%	-1.88 (.858) 4%	2.34 (3.08) 49%	0.04 (.58) 5%	-1.99 (7.02) 22%	.63			

Notes: Each crime category listed in the first column represents a separate regression. The regressions also controlled for a City's Changing Demographic Characteristics, the Per Capita Number of Police Officers, the Unemployment Rate, the Average Weekly Wage, Year and County Fixed Effects, and City Population and Population Squared. The absolute *t*-statistics are shown in the parentheses, and below these are the percentages of a one-standard-deviation change in the endogenous variable that can be explained by a one-standard-deviation change in the exogenous variable. All regressions use weighted least squares where the variables are weighted by the city population.

TABLE B2
 Explaining Crime Rates as a Function of the Racial and Sex Composition of Police Departments:
 Using State Fixed Effects

	% of			% of			Number			Number			Adjusted	R ²	
	Police Force Male	Police Force Black	Police Force Hispanic	Police Force American Indian	Police Force Male	Police Force White	Police Force Asian	Consent Decree Dummy	Consent Decree in effect	City has a Black Mayor	Consent Decree Dummy	Consent Decree in effect			City has a Black Mayor
Violent crime	-1.07 (-1.79) 9%	.869 (2.25) 11%	.315 (1.48) 4%	4.566 (1.13) 3%	4.566 (1.13) 3%	-1.27 (2.17) 11%	-428 (2.17) 7%	-796 (2.17) 4%	.23 (2.18) 6%	-.03 (2.079) 11%	-.25 (.520) 7%	-.03 (2.079) 11%	-.25 (.520) 7%	.70	.70
Property crime	-1.43 (4.15) 22%	.426 (2.22) 12%	.2585 (2.104) 6%	-4895 (2.210) 1%	-4895 (2.210) 1%	-1.53 (4.64) 24%	-322 (3.07) 10%	-259 (1.23) 2%	-1.03 (1.71) 5%	.0009 (3.07) 1%	.024 (.85) 1%	.0009 (3.07) 1%	.024 (.85) 1%	.51	.51
Murder	1.173 (1.38) 6%	1.845 (3.37) 14%	.2271 (7.53) 2%	7.814 (1.367) 3%	7.814 (1.367) 3%	.57 (7.06) 3%	-588 (2.27) 6%	-1.19 (2.27) 4%	-.04 (.247) 1%	.0003 (.023) 0%	.034 (.495) 1%	.0003 (.023) 0%	.034 (.495) 1%	.65	.65
Man-slaughter	3.548 (.033) 25%	3.939 (3.67) 42%	.7574 (1.279) 8%	3.374 (3.301) 2%	3.374 (3.301) 2%	2.30 (1.44) 16%	-1.57 (3.09) 22%	-1.19 (1.16) 5%	-.16 (.547) 3%	.016 (.656) 5%	-.21 (1.518) 5%	.016 (.656) 5%	-.21 (1.518) 5%	.33	.33
Rape	-1.63 (1.38) 8%	.488 (6.41) 3%	-.3659 (8.72) 2%	5.613 (7.06) 2%	5.613 (7.06) 2%	-1.56 (1.251) 7%	.409 (1.14) 4%	.24 (.333) 1%	.0899 (.431) 1%	-.013 (.757) 3%	.12 (1.208) 2%	-.013 (.757) 3%	.12 (1.208) 2%	.83	.83
Total robbery	-1.32 (2.02) 9%	1.408 (3.34) 14%	.16154 (6.95) 2%	7.8199 (1.776) 4%	7.8199 (1.776) 4%	-1.77 (2.83) 12%	-436 (2.19) 6%	-951 (2.36) 4%	-.006 (.055) 0%	-.009 (.876) 3%	-.032 (.598) 1%	-.009 (.876) 3%	-.032 (.598) 1%	.78	.78
Total assault	-1.09 (1.45) 9%	.6513 (1.35) 8%	.47418 (1.782) 6%	1.6106 (3.19) 1%	1.6106 (3.19) 1%	-1.15 (1.61) 9%	-507 (2.23) 8%	-679 (1.48) 3%	.48 (3.63) 12%	-.05 (4.57) 17%	-.05 (.744) 1%	-.05 (4.57) 17%	-.05 (.744) 1%	.58	.58
Burglary	-1.37 (3.17) 18%	.5810 (2.09) 11%	-.18606 (1.213) 4%	-1.14433 (3.94) 1%	-1.14433 (3.94) 1%	-1.67 (4.05) 22%	-.006 (.044) 0%	.011 (.040) 0%	-.08 (1.06) 3%	.0002 (.033) 0%	.078 (2.22) 3%	.0002 (.033) 0%	.078 (2.22) 3%	.54	.54
Larceny	-1.30 (3.6) 18%	.320 (1.38) 7%	.331 (2.587) 7%	-1.92 (7.9) 2%	-1.92 (7.9) 2%	-1.30 (3.79) 18%	-337 (3.08) 9%	-213 (.967) 2%	-.01 (.172) 0%	-.002 (.35) 1%	-.01 (.34) 0%	-.002 (.35) 1%	-.01 (.34) 0%	.56	.56
Motor vehicle theft	-1.90 (2.75) 16%	1.297 (2.91) 16%	.827 (3.368) 10%	9.306 (1.999) 6%	9.306 (1.999) 6%	-2.05 (3.11) 17%	-936 (4.45) 15%	-1.03 (2.43) 5%	-.41 (3.37) 10%	.007 (.67) 2%	-.04 (.73) 1%	.007 (.67) 2%	-.04 (.73) 1%	.65	.65
Assaults on police	-5.55 (2.06) 17%	.911 (5.24) 4%	.5227 (5.46) 2%	-29.29 (1.613) 7%	-29.29 (1.613) 7%	-5.82 (2.26) 18%	-713 (.871) 4%	-.632 (.383) 1%	.66 (1.40) 6%	-.06 (1.50) 8%	-1.55 (7.05) 15%	-.06 (1.50) 8%	-1.55 (7.05) 15%	.54	.54

Notes: Each crime category listed in the first column represents a separate regression. The regressions also controlled for a City's Changing Demographic Characteristics, the Per Capita Number of Police Officers, the Unemployment Rate, the Average Weekly Wage, Year and County Fixed Effects, and City Population and Population Squared. The absolute *t*-statistics are shown in the parentheses, and below these are the percentages of a one-standard-deviation change in the endogenous variable that can be explained by a one-standard-deviation change in the exogenous variable. All regressions use weighted least squares where the variables are weighted by the city population. Sample size is 641 and covers the years 1987, 1990, and 1993.

**APPENDIX B
QUASI-REDUCED FORM REGRESSIONS,
WHICH ALSO INCLUDE THE
INSTRUMENTS**

The two tables reporting these quasi-reduced form estimates produce rather mixed results. In terms of consistent results, the race measures shown in county fixed effects specifications indicate that a greater share of the police force that is black, the higher the violent crime, murder, manslaughter, robbery, or aggravated assault rates. More Hispanics are associated with higher violent crime rates, though, as shown in previous tables, the effect is less consistent than for blacks. Higher shares that are white or Asian are usually associated with lower violent crime, murder, manslaughter, robbery, or aggravated assault rates. The racial share coefficients for the state fixed effects regressions tend to be largest and the most frequently statistically significant of the three sets of estimates. The estimates of the impact of more male police officers vary a great deal across the three sets of estimates, with the fixed county effects and some of the fixed city effects implying a positive relationship between more male police officers and the various crime rates.

Perhaps these different results are not so surprising when different highly correlated variables are included together in one specification, but the consent decree results provide only strong support for the notion that the new rules are causing crime to rise over and above the increase that is occurring from changing racial and gender composition of the police force in the county fixed effects specifications. Where the number of years that the consent decree has been in effect is significant in state fixed effect regressions, it is the opposite sign and statistically significant in four of the 19 crime categories. Although the racial control variables produce a consistent effect, they do not allow us to differentiate whether it is really race or changing hiring rules that are driving the different crime rates.

One other result should be mentioned. Having a black mayor is associated with more felonious shootings of police officers and fewer assaults against officers. The impact of having a black mayor on crime rates is more mixed, with the county fixed effects regressions implying that the election of a black mayor is associated with more crime for violent and property crimes generally, murder, manslaughter, most types of robbery, burglary, and motor vehicle theft. The state fixed effects regressions indicate that forcible rapes and burglaries rise but that one category of robberies and manslaughter fall. Why the presence of a black mayor is correlated with crime is beyond the scope of this article.

**APPENDIX C
LIST OF CITIES USED
IN STUDY**

Anchorage, Alaska; Birmingham, Ala; Mobile, Ala; Montgomery, Ala; Gadsden, Ala; Florence, Ala; Huntsville, Ala; Tuscaloosa, Ala; Little Rock, Ark; North Little Rock, Ark; Fort Smith, Ark; Fayetteville, Ark; Springdale, Ark; Mesa, Ariz; Phoenix, Ariz; Tucson, Ariz; Oakland, Calif; Chico, Calif; Fresno, Calif; Bakersfield, Calif; Long Beach, Calif; Los Angeles, Calif; Pasadena, Calif; Monterey, Calif; Salinas, Calif; Seaside,

Calif; Napa, Calif; Anaheim, Calif; Santa Ana, Calif; Riverside, Calif; Sacramento, Calif; San Bernardino, Calif; San Diego, Calif; San Francisco, Calif; Stockton, Calif; Lompoc, Calif; Santa Barbara, Calif; Santa Maria, Calif; San Jose, Calif; Santa Cruz, Calif; Fairfield, Calif; Vallejo, Calif; Santa Rosa, Calif; Petaluma, Calif; Modesto, Calif; Yuba City, Calif; Porterville, Calif; Tulare, Calif; Visalia, Calif; Oxnard, Calif; Ventura, Calif; Boulder, Colo; Longmont, Colo; Colorado Springs, Colo; Fort Collins, Colo; Loveland, Colo; Pueblo, Colo; Denver, Colo; Bridgeport, Conn; Danbury, Conn; Hartford, Conn; Meriden, Conn; Middletown, Conn; Milford, Conn, New Britain, Conn; New Haven, Conn; New London, Conn; Norwalk, Conn; Norwich, Conn; Stamford, Conn; Waterbury, Conn; Washington, D C; Wilmington, Del; Gainesville, Fla; Melbourne, Fla; Titusville, Fla; Palmbay, Fla; Fort Lauderdale, Fla; Hollywood, Fla; Pompano Beach, Fla; Hialeah, Fla; Miami, Fla; Jacksonville, Fla; Pensacola, Fla; Tampa, Fla; Fort Myers, Fla; Tallahassee, Fla; Bradenton, Fla; Ocala, Fla; Fort Walton Beach, Fla; Orlando, Fla; Boca Raton, Fla; Delray Beach, Fla; West Palm Beach, Fla; Clear Water, Fla; Saint Petersburg, Fla; Lakeland, Fla; Winter Haven, Fla; Sarasota, Fla; Daytona Beach, Fla; Macon, Ga; Savannah, Ga; Warner Robins, Ga; Columbus, Ga; Augusta, Ga; Atlanta, Ga; Honolulu, Hawaii; Cedar Falls, Iowa; Waterloo, Iowa; Cedar Rapids, Iowa; Des Moines, Iowa; Davenport, Iowa; Sioux City, Iowa; Boise, Idaho; Champaign, Ill; Rantoul, Ill; Urbana, Ill; Aurora, Ill; Elgin, Ill; Kankakee, Ill; Bloomington, Ill; Normal, Ill; Peoria, Ill; Moline, Ill; Rock Island, Ill, Springfield, Ill; Joliet, Ill; Rockford, Ill; Chicago, Ill; Fort Wayne, Ind; Gary, Ind; Hammond, Ind; Anderson, Ind; Misha Waka, Ind; South Bend, Ind; Evansville, Ind; Terre Haute, Ind; Indianapolis, Ind; Wichita, Kans; Topeka, Kans; Kansas City, Kans; Ashland, Ky; Lexington, Ky; Louisville, Ky; Shreveport, La; Lake Charles, La; Baton Rouge, La; Lafayette, La; Monroe, La; Houma, La; New Orleans, La; Attleboro, Mass; New Bedford, Mass; Gloucester, Mass; Haverhill, Mass; Lawrence, Mass; Salem, Mass; Springfield, Mass; Malden, Mass; Brockton, Mass; Boston, Mass; Baltimore, Md; Portland, Maine; Bay City, Mich; Benton Harbor, Mich; Battle Creek, Mich; Flint, Mich; East Lansing, Mich; Lansing, Mich; Lansing Township, Mich; Jackson, Mich; Kalamazoo, Mich; Kalamazoo Township, Mich; Grand Rapids, Mich; Midland, Mich; Muskegon, Mich; Saginaw, Mich; Ann Arbor, Mich; Detroit, Mich; Minneapolis, Minn; Saint Paul, Minn; Duluth, Minn, Columbia, Mo; Springfield, Mo; Joplin, Mo; Kansas City, Mo; Saint Louis, Mo; Biloxi, Miss; Gulfport, Miss; Jackson, Miss; Omaha, Neb; Lincoln, Neb; Asheville, N C; Hickory, N C; Fayetteville, N C; Durham, N C; Winston-Salem, N C; Gastonia, N C; Greensboro, N C; Highpoint, N C; Charlotte, N C; Raleigh, N C; Manchester, N H; Portsmouth, N H; Dover, N H; Rochester, N H; Jersey City, N J; Northbergen Township, N J; Trenton, N J; Middlesex, N J; Passaic, N J; Elizabeth, N J; Newark, N J; Albuquerque, N Mex; Las Vegas, Nev; Reno, Nev; Albany, N Y; Binghamton, N Y; Poughkeepsie, N Y; Buffalo, N Y; Rochester, N Y; New York, N Y; Niagara Falls, N Y; Rome, N Y; Utica, N Y; Syracuse, N Y; Troy, N Y; Schenectady, N Y; Lima, Ohio; Hamilton, Ohio; Middletown, Ohio; Springfield, Ohio; Springfield Township, Ohio; Elyria, Ohio; Lorain, Ohio; Toledo, Ohio; Youngstown, Ohio; Dayton, Ohio; Mansfield, Ohio; Canton, Ohio; Akron, Ohio; Warren, Ohio;

- Cincinnati, Ohio; Cleveland, Ohio; Columbus, Ohio; Lawton, Okla; Oklahoma City, Okla; Tulsa, Okla; Medford, Oreg; Eugene, Oreg; Springfield, Oreg; Salem, Oreg; Portland, Oreg; Reading, Pa; Altoona, Pa; Johnstown, Pa; Carlisle, Pa; Harrisburg, Pa; Erie, Pa; Scranton, Pa; Lancaster, Pa; Lebanon, Pa; Wilkes-Barre, Pa; Williamsport, Pa; Sharon, Pa; Bethlehem, Pa; York, Pa; Philadelphia, Pa; Pittsburgh, Pa; Pawtucket, R I; Providence, R I; Woonsocket, R I; Anderson, S C; Charleston, S C; Florence, S C; Greenville, S C; Columbia, S C; Spartanburg, S C; Rock Hill, S C; Sioux Falls, S Dak; Nashville, T N; Chattanooga, T N; Knoxville, T N; Kingsport, T N; Johnson City, T N; Memphis, T N; Killeen, Texas; Temple, Texas; Brownsville, Texas; Harlingen, Texas; El Paso, Texas; Galveston, Texas; Texas City, Texas; Edinburg, Texas; McAllen, Texas; Mission, Texas; Beaumont, Texas; Lubbock, Texas; Waco, Texas; Corpus Christi, Texas; Arlington, Texas; Fort Worth, Texas; Austin, Texas; Dallas, Texas; Houston, Texas; San Antonio, Texas; Salt Lake City, Utah; Orem, Utah; Provo, Utah; Ogden, Utah; Bristol, V A; Hampton, V A; Newport News, V A; Norfolk, V A; Richmond, V A; Roanoke, V A; Virginia Beach, V A; Burlington, V T; Vancouver, Wash; Tacoma, Wash; Spokane, Wash; Olympia, Wash; Bellingham, Wash; Seattle, Wash; Madison, Wis; Appleton, Wis; Racine, Wis; Neenah, Wis; Oshkosh, Wis; Milwaukee, Wis; Huntington, W VA; Charleston, W VA; Wheeling, W VA;
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