

# Estimating Effects of Affirmative Action in Policing: A Replication and Extension of Previous Research

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## Abstract

Many police departments in the United States have experienced externally-imposed affirmative action plans designed to increase the shares of nonwhite and female police officers. This paper examines whether externally-imposed affirmative action plans have impacted the rates of reported offenses and/or offenses cleared by arrest, seeking to replicate and extend Lott [2000] and McCrary [2007]. Using a series of modern econometric strategies, including difference-in-differences decomposition and generalized synthetic controls, we do not find a significant effect of court-imposed affirmative action plans on the rates of reported offenses or reported offenses cleared by arrest, a finding consistent with McCrary [2007]. We also consider whether unlitigated agencies change their practices due to the threat of litigation, but, like McCrary [2007], are unable to identify causal evidence of such threat effects. We suggest that, in the spirit of Miller and Segal [2018], future research seek to estimate the potentially racially heterogeneous treatment effects of race-based affirmative action plans on public safety outcomes.

## A Introduction

In the aftermath of Michael Brown’s death in Ferguson, Missouri, renewed attention was directed to the demographic composition of law enforcement agencies. In the summer of 2015, 2 out of 3 residents of Ferguson, but only 3 out of its 53 police officers, were black [U.S. Department of Justice, 2016]. This disparity gave rise to questions about whether increasing the proportion of black officers on the Ferguson police force, or in law enforcement agencies more generally, would lead to different public safety outcomes. On the one hand, increasing the proportion of nonwhite police officers may lead to an increased acceptance of policing, fewer instances of the use of force, and fewer citizen complaints in nonwhite communities [Tyler, 2005], leading in turn to increases in the reporting of crime by nonwhite victims, and in the willingness of civilians to cooperate with police investigations into these crimes [Miller and Segal, 2018]. Nonwhite officers may also exert greater effort, or be better equipped, to clear crimes in largely nonwhite neighborhoods. These mechanisms might lead to decreases in nonwhite crime victimization. On the other hand, increasing the proportion of nonwhite police officers may lead to decreases in policing quality if nonwhite applicants to police agencies are less qualified than white applicants, and/or if the effort of white officers is reduced due to morale effects [Lott, 2000]. These mechanisms might lead to increases in nonwhite and/or white crime victimization. Detecting the presence of these potentially offsetting and racially heterogeneous effects is a challenging exercise.

Existing work on this question has generally not attempted to identify the specific causal pathways through which agency racial composition may affect policing outcomes, seeking instead to estimate average effects of agency racial composition on outcomes such as reported offense and arrest rates [Lott, 2000, McCrary, 2007]. Yet analyzing the effect of the racial composition of law enforcement agencies on these outcomes is complicated by the endogeneity of hiring and retention practices to other agency-specific factors that may also affect outcomes. To achieve identification, researchers have sought to leverage the incidence and timing of affirmative action litigation [Lott, 2000, McCrary, 2007]. This work has yielded inconsistent results regarding the effects of racial diversity on policing outcomes. Existing work has not, however, implemented recent econometric advances in difference-in-differences estimation.

In this paper, we replicate and extend the work of Lott [2000] and McCrary [2007] using modern difference-in-differences methods, including the difference-in-differences decomposition method developed by Goodman-Bacon [2019] as well as the generalized synthetic controls method developed by Xu [2017]. Ultimately, like McCrary [2007], we do not find a

significant average effect of court-imposed affirmative action plans on the rates of reported offenses or reported offenses cleared by arrest. We also extend the effort of McCrary [2007] to analyze whether unlitigated agencies change their practices due to the threat of litigation, raising concerns of stable unit treatment value assumption (SUTVA) violations. We estimate whether the total number of agencies receiving court-imposed affirmative action plans at time  $t$  has a differential impact on the rates of reported offenses or offenses cleared by arrest for agencies that will never be litigated, relative to agencies that will eventually be litigated. Like McCrary [2007], we find little evidence of spillover effects. Finally, we return to the question of identifying the specific and possibly racially heterogeneous causal mechanisms linking affirmative action litigation and public safety outcomes, suggesting that this would be a productive avenue for future research.

## B Background

Police departments have experienced some of the most aggressive affirmative action programs ever implemented in the United States [McCrary, 2007, Miller and Segal, 2012]. Beginning in the late 1960s with a number of employment discrimination lawsuits, federal courts began mandating affirmative action plans with the intended effect of increasing the shares of non-white and female police officers. Court-imposed affirmative action plans often take the form of hiring quotas, but also may affect standards for promotion. Some police departments are still under affirmative action plans today, often from court-imposed plans going back to the 1970s.

The justification for such affirmative action plans may be to rectify past discrimination, and/or to promote the compelling government interest in increasing the effectiveness of police departments at detecting and interdicting crime. There are several reasons one might expect more racially diverse police departments to be more effective at policing. It has long been recognized that minority groups tend to be suspicious of police and the criminal justice system more generally [U.S. Kerner Commission, 1968].<sup>1</sup> Further, as Donohue and Levitt [2001] point out, “conflicts between police and citizens have been the flashpoint for virtually every recent urban riot.” This statement remains true in more recent years (for

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<sup>1</sup>As early as 1968, the U.S. Kerner Commission’s Report of the National Advisory Commission on Civil Disorders expressed this sentiment as follows: “To some Negroes police have come to symbolize white power, white racism and white repression. And the fact is that many police do reflect and express these white attitudes. The atmosphere of hostility and cynicism is reinforced by a widespread belief among Negroes in the existence of police brutality and in a ‘double standard’ of justice and protection—one for Negroes and one for whites.”

instance, the 2014-2015 Ferguson riots and the 2015 Baltimore riots). Lack of trust may lead nonwhite civilians to be less likely to report crimes to police, to cooperate with investigations, and to take instructions from law enforcement, in the presence of a largely white police force. Increasing the proportion of nonwhite officers in racially diverse communities may lead to nonwhite victims becoming more likely to report crimes, and to members of these communities becoming more likely to cooperate with the police to help solve crimes reported by nonwhite victims.

Nonwhite police officers may also exert greater effort to detect and clear crimes occurring in nonwhite neighborhoods. Nonwhite officers may also better understand the cultural norms of predominantly nonwhite communities. One might also expect police officers to be less likely to racially discriminate against members of their own race. Nonwhite police officers may also perceive largely nonwhite neighborhoods to be less hostile, relative to white police officers.<sup>2</sup> These mechanisms may contribute to increased reporting by nonwhite crime victims, and to decreased incidence of nonwhite crime victimization.

On the other hand, there are potential adverse effects of more racially diverse police departments. In order to implement court-imposed affirmative action plans, some police departments have had to change entrance standards. Police force entrance requirements vary by location, but generally contain several basic components, including criminal background checks and physical examinations. Prior to the wave of employment discrimination lawsuits in the 1970s, it was standard to use entrance examinations that tested the cognitive abilities of prospective officers. These exams tested aptitudes pertaining to reading comprehension, verbal reasoning, analogies, and, in some cases, I.Q. Black applicants historically performed worse than white applicants on these entrance exams [McCrary, 2007]. Federal courts have intervened in the examination process since the 1970s, often mandating that entrance standards be changed if they have a disparate impact on a particular group and the standards are not shown to relate to job performance. To deal with pressures from the federal judiciary, some police departments have removed cognitive tests altogether in order to increase nonwhite recruitment. Others have simply reduced standards [Lott, 2000]. The relevant question to ask is whether these aptitude tests reliably screen applicants for qualities important to police work. If affirmative action litigation leads to the lowering of entrance standards, and those standards are reliable indicators of future job performance, we would expect affirmative action to lead to worse policing outcomes.

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<sup>2</sup>Groves and Rossi [1970] suggest that the perceptions of hostility toward police are projections of the fears and prejudices of white police officers themselves.

Many employment discrimination lawsuits were brought by private litigants beginning in the late 1960s. The U.S. Department of Justice (DOJ) became involved in these discrimination lawsuits after 1972. If a court found that a police department entrance exam disproportionately affected black applicants and was not reliably related to job performance, the court would order that the department devise a new test that either did not disparately affect black applicants, or was job-related. There would typically be a one- to three-year lag before a hiring quota was imposed [McCrary, 2007]. In some cases, hiring quotas would end once a goal had been reached (for instance, a police department might be required to hire a certain percentage of black officers each year until the agency reached a target proportion of black officers in its force). In other cases, hiring quotas lasted until terminated by the judiciary.

Several papers have considered the employment effects of affirmative action litigation. Lott [2000] and McCrary [2007] both find significant increases in black police employment following affirmative action litigation, with McCrary finding a post-litigation 14 percentage point gain in the fraction of black officers among newly hired officers. Miller and Segal [2012] also find persistent and significant employment effects for black police officers as a consequence of litigation. Their finding holds even for departments for which affirmative action is eventually terminated. Miller and Segal [2012] also find a significant divergence in black employment between agencies that continued affirmative action and those that ended it, with larger persistent gains in black employment in the former agencies. Additionally, Miller and Segal [2012] find that there is an important distinction between simply experiencing litigation, and actually having a court-imposed affirmative action plan. Departments that are litigated but not required to implement affirmative action see increases in black employment, but at a lower rate than those with court-imposed affirmative action plans.

In addition to looking at the impacts of affirmative action on employment, several papers discuss how increased diversity affects policing outcomes. Donohue and Levitt [2001] examine how the racial composition of police departments affects racial patterns of arrest, using the racial composition of fire departments as an instrument for the racial composition of police departments. They find that increases in the percentage of black police officers lead to higher arrest rates for whites but not for nonwhites. Similarly, increases in the percentage of white police officers lead to higher arrest rates for nonwhites, but not for whites. These patterns particularly hold for minor offenses.

Lott [2000] and McCrary [2007] consider how affirmative action litigation affects reported crime and arrest rates. Using logged annual rates of per capita reported violent and property

crime between 1987 and 1994 for a sample of 495 cities, Lott [2000] estimates the impacts of 19 consent decrees signed by the Department of Justice and a city's policing agency and still in force by 1987. In both reduced form and instrumental variable models, Lott finds that consent decrees lead to substantial increases in reported crime rates, and weakly lead to decreases in arrest rates. He interprets these effects as being the result of lower hiring standards.

However, McCrary [2007] suggests that this mechanism is implausible for several reasons. First, consent decrees typically led to hiring practices in which applicants were evaluated relative to other applicants of the same race. Under these hiring practices, no reduction of standards was required for nonblack applicants. Second, while hiring standards may have been reduced in some cases to eliminate disparate impact, in many cases entrance exams were modified to be more closely related to job performance. Third, looking at test score distributions for the New York City Police Department, McCrary [2007] finds that hiring quotas impacted test scores of new hires "only minimally." Further, the consent decrees used in Lott's sample include neither affirmative action plans resulting from private litigation, nor externally-imposed affirmative action plans that were terminated prior to 1987.

McCrory [2007] estimates event study models of the effects of affirmative action litigation on reported offense and arrest rates. Using a sample of 314 large municipal police departments, he finds little evidence that affirmative action litigation impacted reported city-level crime and arrest rates. He suggests that there may be "a complex series of effects that offset one another." However, McCrary [2007] does not distinguish between two types of litigated cities: those that implemented court-imposed affirmative action plans, and those that did not. In addition, difference-in-differences methods developed after the publication of McCrary [2007], including the difference-in-differences decomposition method developed by Goodman-Bacon [2019], as well as generalized synthetic controls developed by Xu [2017], may lead to different insights.

Miller and Segal [2018] use affirmative action litigation, data from the National Crime Victimization Survey, and FBI data on intimate partner homicides to examine how increasing female representation among police officers impacts the incidence of domestic violence and intimate partner homicide, and the reporting of domestic violence. They find that violent crimes against women are reported at higher rates when female representation increases in law enforcement agencies. Further, greater female representation in policing agencies leads to significant declines in the rates of intimate partner homicide and non-fatal domestic abuse. In both instrumental variables and reduced form models, they find similar effects

from affirmative action litigation. They find no effects of affirmative action litigation or increased female officer shares on the reporting or incidence of crimes committed against male victims.

Our paper primarily focuses on replicating and extending the estimates of the impacts of affirmative action litigation on crime reported by Lott [2000] and McCrary [2007]. However, in the discussion section we return to the question of identifying the causal pathways by which race-based affirmative action may affect policing outcomes, including possibly racially heterogeneous treatment effects.

## C Data

### C.1 Affirmative Action Litigation Data

Our data on affirmative action litigation are sourced from Miller and Segal [2012], who constructed the most complete currently available legal database of affirmative action litigation involving police departments. Previous data sets, such as the one used by McCrary [2007], did not distinguish between unsuccessful litigation and successful litigation leading to court-imposed affirmative action plans. Additionally, the data set constructed by Miller and Segal [2012] looks at affirmative action litigation specifically addressing the employment of police officers, rather than the employment of all police department employees (which includes clerical and janitorial positions).

Miller and Segal constructed their litigation data by first looking at employment data from confidential EEO-4 reports filed with the Equal Employment Opportunity Commission (EEOC) between 1973 and 2005. They examined reports from 479 of the largest state and local law enforcement agencies in the United States. Miller and Segal then searched for legal records pertaining to discrimination in employment for each agency using the LexisNexis and Westlaw federal case databases. They gathered information on the actual litigation, including whether affirmative action was implemented and when it ended (if applicable), and the protected group. This information was cross-referenced with data from the U.S. Department of Justice (DOJ) and the databases used in McCrary [2007] and Lott [2000].

Miller and Segal [2012] find that of the 479 agencies examined, 140 affirmative action cases were brought either by private plaintiffs or by the DOJ between 1969 and 2000. Of the 140 agencies which experienced litigation, 117 saw the implementation of affirmative action plans, and 23 saw litigation which did not result in court-imposed affirmative action. Among the 117 agencies that experienced court-imposed affirmative action, 67 of these agencies saw

the eventual termination of the program. The mean duration of the 67 plans that terminated was 16 years. Miller and Segal [2012] also report that 96% of the affirmative action plans for which the protected group can be determined involve the employment of black officers.

## C.2 UCR and CPS Data

We match the agencies in Miller and Segal’s affirmative action database to the agencies reporting annual crime and arrest data in the FBI’s Uniform Crime Reporting program (UCR).<sup>3</sup> Agencies that report zero crimes or arrests in any year are treated as missing data in that year; all agencies missing annual data are dropped from the sample. We identify the county within which each agency is located, and match counties to annual metropolitan statistical area (MSA) population and demographic data sourced from the U.S. Census Bureau’s Current Population Survey (CPS).<sup>4</sup>

We are left with a sample of 99 agencies in the Miller and Segal affirmative action database that are located within an MSA and that consistently report annual crime and arrest data in the UCR between 1964 and 2011. Of these 99 agencies, 27 agencies implement court-imposed affirmative action programs at some point. Among these 27 agencies, 12 agencies have programs that terminate during our sample period, and 15 have programs that do not. The mean time treated is approximately 25.7 years (20.4 years for agencies whose affirmative plans end; 30.8 years for agencies whose affirmative action plans do not end). Figure 1 reports a histogram of the years in which affirmative action plans were imposed. Figure 2 shows where treated and untreated agencies are located.

### C.2.1 Reported Offenses and Reported Offenses Cleared by Arrest

Following Lott [2000] and McCrary [2007], we focus on two sets of outcome variables: the natural logarithms of rates of reported violent and property crime offenses per 100,000 in population, and arrest rates, defined as the numbers of violent and property crime offenses cleared by arrest, divided by the numbers of violent and property crime index offenses, between 1964 and 2011. Violent crime is the aggregation of four index crimes in the UCR data: murder, rape, robbery, and aggravated assault. Property crime is the aggregation

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<sup>3</sup>UCR data, as cleaned and compiled by Kaplan [2019], were downloaded from ICPSR. The UCR program is voluntary; not all agencies participate, or participate consistently, in the program.

<sup>4</sup>This data was compiled by Flood et al. [2018]. The CPS data are not publicly available on an MSA level prior to 1977. We assume that the demographic data for 1964-1976 resemble the 1977 CPS data. CPS data are also not available for the MSAs in our data from 1990 to 1999, so we use the closest previous year of data for the missing year observations.

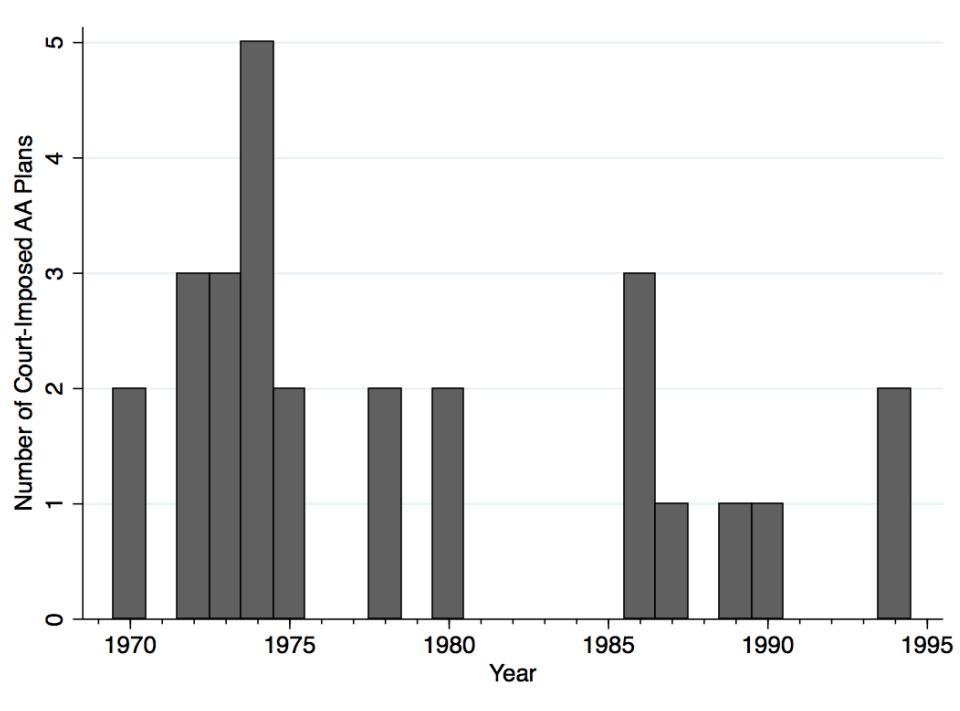


Figure 1: Court-Imposed AA Plans Over Time

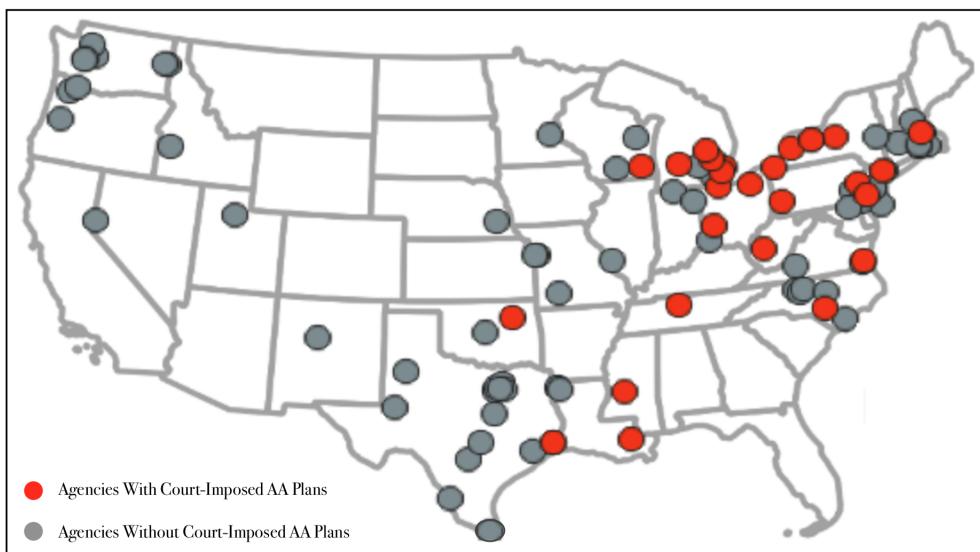


Figure 2: Location Of Agencies

of burglary, theft, and motor vehicle theft. Summary statistics for the departments in our sample are presented in Table 1.

Table 1: **Summary Statistics, Offense and Arrest Rates**

	All Agencies	Treated Years	Untreated Years
ln(Violent Offenses Per 100,000)	6.13 (1.00)	6.70 (0.72)	6.03 (1.00)
Violent Crime Arrest Rate	0.51 (0.34)	0.44 (0.15)	0.52 (0.36)
ln(Property Offenses Per 100,000)	8.48 (0.57)	8.66 (0.48)	8.45 (0.58)
Property Crime Arrest Rate	0.18 (0.08)	0.18 (0.10)	0.18 (0.08)
Observations	4,752	722	4,030

This table reports the means for each annual per capita offense/arrest rate variable from 1964 to 2011; standard deviations in parentheses. *Treated Years* reports means for agencies which had court-imposed AA plans during that year. *Untreated Years* reports means for agencies that did not have court-imposed AA plans during that year.

### C.2.2 Location Demographics

We use the MSA-level demographic data from the CPS only for the purpose of matching departments in the generalized synthetic control analyses. Summary statistics for the demographic variables for all, treated, and untreated agencies are presented in Table 2.

## D Models and Results

### D.1 Two-way Fixed Effect Difference-in-Differences Models

The canonical difference-in-differences model compares pre-post changes in outcomes in treated units to pre-post changes in outcomes in untreated units, for a single treatment [Goodman-Bacon, 2019]. In our data, 72 agencies are untreated units not subject to externally imposed affirmative action. 27 agencies are treated units subject to externally imposed affirmative action. However, treatment timing varies across treated units.

The widely accepted empirical strategy in this context is the two-way fixed effect difference-in-differences model (2WFE DD), as in Equation 1:

Table 2: **Summary Statistics of Demographic Variables**

	All Agencies	Treated	Untreated
Proportion White	0.84 (0.09)	0.82 (0.11)	0.85 (0.08)
Proportion Black	0.12 (0.08)	0.15 (0.10)	0.12 (0.08)
Proportion Other	0.03 (0.03)	0.03 (0.03)	0.03 (0.03)
Proportion Male	0.48 (0.02)	0.47 (0.02)	0.48 (0.02)
Proportion Female	0.52 (0.02)	0.53 (0.02)	0.52 (0.02)
Proportion in Labor Force	0.49 (0.04)	0.49 (0.04)	0.50 (0.04)
Proportion Not in Labor Force	0.27 (0.04)	0.28 (0.04)	0.27 (0.04)
Share of Population Age 18-21	0.05 (0.01)	0.05 (0.01)	0.05 (0.01)
Share of Population Age 21-24	0.06 (0.02)	0.06 (0.02)	0.06 (0.02)
Percent Above Poverty Line	0.89 (0.04)	0.89 (0.04)	0.89 (0.04)
Percent Below Poverty Line	0.11 (0.04)	0.11 (0.04)	0.11 (0.04)
City Population	210,723 (236,833)	324,626 (335,717)	190,317 (207,920)
Number of Observations	4,752	722	4,030
Number of Agencies	99	27	72

This table reports the means for each demographic variable between 1964 and 2011; standard deviations in parentheses. The *Treated* group consists of agencies which had court-imposed AA plans at least for some part of our sample period. The *Untreated* group consists of agencies that had did not have court-imposed AA plans during the sample period. The *Untreated* group includes agencies that faced unsuccessful litigation as well as agencies that never faced litigation.

$$Offense\ Rates/Arrest\ Rates_{it} = \beta_0 + \beta_1 Treat_{it} + \boldsymbol{\theta}_t + \boldsymbol{\alpha}_i + \epsilon_{it}, \quad (1)$$

where  $Treat$  is a binary variable equal to one when a unit is subject to a court-imposed affirmative action plan, and equal to zero otherwise;  $\boldsymbol{\theta}_t$  is a time vector containing indicators for the 48 years from 1964 to 2011; and  $\boldsymbol{\alpha}_i$  is a unit vector containing indicators for the 99 agencies. Standard errors are clustered by agency. The average treatment effect on the treated (ATT) is given by  $\beta_1$ . We estimate the ATT of externally-imposed affirmative action on logged violent and property crime rates per capita, and violent and property crime arrest rates, between 1964 and 2011.

The 2WFE DD model specified above captures average treatment effects on the treated, but does not allow us to consider time-varying treatment effects. There are several reasons to expect the effects of affirmative action plans to vary over time. First, court-imposed affirmative action plans often take time to implement. Further, once implementation begins it takes time for the racial composition of police departments to change significantly, due to the nature of hiring quotas. In order to account for potentially time-varying treatment effects, we implement difference-in-differences decomposition [Goodman-Bacon, 2019].

## D.2 Difference-in-Differences Decomposition

The 2WFE DD estimate is composed of a weighted average of treatment effects estimated from a series of 2x2 treatment/control groups, some of which compare agencies treated at the same time to untreated agencies, and some of which compare agencies treated at the same time to agencies treated at another time (earlier or later).<sup>5</sup> As reported in Figure 1, there are 12 timing groups in our data, or groups of agencies which experience the imposition of externally-imposed affirmative action in the same year. There are thus 144 distinct 2x2 treatment/control comparison groups from which the 2WFE DD estimate is constructed: 132 groups in which earlier-treated agencies are compared to later-treated agencies, or vice versa, and 12 groups in which treated agencies are compared to untreated agencies. In the presence of time-varying treatment effects, comparisons between earlier and later treated units may introduce bias into the 2WFE DD estimate. The extent of the bias depends on the share of the 2WFE DD estimate that is derived from these earlier-later comparisons, which in turn depends on group size and the variance of the treatment [Goodman-Bacon,

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<sup>5</sup>Comparisons may also be made to always-treated units; however, we have no always-treated agencies in our data.

2019].

Goodman-Bacon [2019] has developed a method to decompose the 2WFE DD estimate into the 2x2 weighted estimates from which it is derived. Using this difference-in-differences decomposition model, we can uncover the extent to which the 2WFE DD estimate depends on 2x2 DD estimates which compare earlier to later treated agencies. The Goodman-Bacon decomposition model is currently only available for strongly balanced panels in which treatment only changes from 0 to 1 over time. To estimate the decomposition model, we define treatment as a binary variable that is equal to one in all years after an affirmative action plan is imposed on an agency, and is equal to zero otherwise.<sup>6</sup> We report both the 2WFE DD estimate for this model, as well as the DD estimates and weights for the three categories of treatment/control comparison groups from which the 2WFE DD estimate is derived.

### D.3 Results for DD Models

Results for our difference-in-differences models for offense and arrest rates are presented in Table 3. The DD Model reports the 2WFE DD estimate where treatment is defined as years in which an agency is subject to an externally-imposed affirmative action plan. The GB Model reports the 2WFE DD estimate where treatment is defined as all years subsequent to the imposition of an affirmative action plan. For the GB Model, we also report average DD decomposition estimates and weights for 2x2 treatment/control groups.<sup>7</sup> Each model is estimated separately using four different dependent variables: the natural log of per capita violent crime offenses, the natural log of per capita property crime offenses, violent crime arrest rates, and property crime arrest rates.

Neither model yields any statistically significant results, for any outcome variable. These results support the findings of McCrary [2007] and contrast with the findings of Lott [2000].

The Goodman-Bacon decomposition estimates also allow us to see that there is little evidence that bias introduced by time-varying treatment effects is driving the null 2WFE DD estimates. The latter are largely driven (total weight = 89%) by comparisons between treated and untreated agencies, as in the canonical 2x2 DD model. Although there is some evidence of time-varying treatment effects, with 2x2 DD estimates signed in the opposite direction for some timing groups, relative to other treatment/control groups, little weight is placed on the timing group 2x2 DD estimates in the construction of the 2WFE DD estimate.

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<sup>6</sup>We also estimated a version of the decomposition model that includes an indicator for years after the termination of an affirmative action plan; results were qualitatively similar to those reported here.

<sup>7</sup>For the 2WFE models we use *xtreg* in Stata 16; for the decomposition model we use the *bacondecomp* Stata 16 command developed by Goodman-Bacon et al. [2019].

Table 3: Treatment Effects for DD Models; Offense and Arrest Rates

	Ln(Violent Crime PC)	Ln(Property Crime PC)	Violent Crime Arrest Rate	Property Crime Arrest Rate
DD Model	0.088 (0.067)	0.035 (0.046)	0.004 (0.026)	0.013 (0.015)
GB Model	0.02 (0.09)	0.02 (0.05)	-0.001 (0.027)	0.011 (0.016)
Avg DD Decomp Estimates:				
Earlier T vs. Later C (Weight = .03)	0.01	-0.05	0.01	-0.01
Later T vs. Earlier C (Weight = .08)	0.16	0.01	0.05	0.05
T vs. Never Treated (Weight = .89)	0.01	0.03	-0.01	0.01
Number of Agencies	99	99	99	99
Observations	4,752	4,752	4,752	4,752
Year Fixed Effects	Yes	Yes	Yes	Yes
Agency Fixed Effects	Yes	Yes	Yes	Yes

This table summarizes average treatment effects for two different DD models of four different dependent variables. Each cell in the table represents the treatment effect for one model. Standard errors are clustered on agency. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

## D.4 Duration Models

We also estimate models of duration or dosage effects of externally imposed affirmative action plans. For these models we replace the variable *Treat* with the variable *Years of Treatment* ( $YOT_{it}$ ) in our 2WFE DD model. In our first duration model we control for agency and year fixed effects:

$$Offense Rates/Arrest Rates_{it} = \beta_0 + \beta_1 YOT_{it} + \alpha_i + \theta_t + \epsilon_{it}, \quad (2)$$

where  $\beta_1$  is the effect of an agency being subjected to one more year of affirmative action.

In our second duration model we add the vector  $\lambda_i t$ , which captures agency-specific linear time trends in offense and arrest rates. This model takes the form:

$$Offense Rates/Arrest Rates_{it} = \beta_0 + \beta_1 YOT_{it} + \alpha_i + \theta_t + \lambda_i t + \epsilon_{it}. \quad (3)$$

## D.5 Results for Duration Models

Table 4 reports estimates from Equations 2 and 3 for logged offense rates. We find no effects of years of affirmative action exposure on logged offense rates, in any model.

Table 4: Duration Models; Offense Rates

	Duration Model 1		Duration Model 2	
	Ln(Violent Crime PC)	Ln(Property Crime PC)	Ln(Violent Crime PC)	Ln(Property Crime PC)
Years of Treatment	0.005 (0.003)	0.003 (0.003)	0.006 (0.008)	0.005 (0.007)
Number of Agencies	99	99	99	99
Observations	4,752	4,752	4,752	4,752
Year Fixed Effects	Yes	Yes	Yes	Yes
Agency Fixed Effects	Yes	Yes	Yes	Yes
Agency-Specific Time Trends	No	No	Yes	Yes

Standard errors clustered on agency. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

Table 5 reports estimates for Equations 2 and 3 for violent and property crime arrest rates.

Table 5: Duration Models; Arrest Rates

	Duration Model 1		Duration Model 2	
	Violent Crime Arrest Rate	Property Crime Arrest Rate	Violent Crime Arrest Rate	Property Crime Arrest Rate
Years of Treatment	0.0003 (0.0015)	0.0002 (0.0007)	0.0002 (0.0019)	-0.0002 (0.0007)
Number of Agencies	99	99	99	99
Observations	4,752	4,752	4,752	4,752
Year Fixed Effects	Yes	Yes	Yes	Yes
Agency Fixed Effects	Yes	Yes	Yes	Yes
Agency-Specific Time Trends	No	No	Yes	Yes

Standard errors clustered on agency. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

We again find no statistically significant effects of externally imposed affirmative action on either violent crime or property crime arrest rates. These estimates are generally consistent with our other DD estimates, as well as with the results reported by McCrary [2007].

Even though we are able to control for an extensive amount of exogenous variation using agency and year fixed effects, there may still be selection effects confounding our estimates. The parallel trends assumption, on which DD models depend, states that offense and arrest rates within treated agencies should have changed at the same rate as offense and arrest rates within untreated agencies, had treatment not occurred. Yet if there was nonrandom selection into treatment, and treated agencies differ from untreated agencies, the parallel trends assumption may be violated. Since the 1990s, synthetic control methods have been used to construct more appropriate comparison units for treated units that differ from untreated units [Abadie et al., 2010, Card, 1990]. We likewise use the generalized synthetic control method [Xu, 2017] to construct more appropriate control units for treated agencies.

## D.6 Generalized Synthetic Control Model

The generalized synthetic control (GSC) method introduced by Xu [2017] addresses the case when treatment is imposed at different times for different units. This approach allows for multiple treated units and variable treatment periods.<sup>8</sup> The GSC method allows us not only

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<sup>8</sup>This method also has several other advantages. It includes a built-in cross-validation procedure and is easier to implement than other synthetic control methods.

to match units on pretreatment observables, but also to model unobserved time-varying heterogeneities using interactive fixed effects.

GSC first estimates an interactive fixed effects (IFE) model using only the police departments that were never treated, and obtains a fixed number of time-varying coefficients (latent factors). It then estimates department-specific intercepts (factor loadings) for each treated police department by linearly projecting pretreatment outcomes for treated units onto the space spanned by the factors. Finally, it generates synthetic control units based on the estimated factors and factor loadings. The method is described as a “bias correction procedure for IFE models when treatment data is heterogeneous across units.”<sup>9</sup>

The *gsynth* package requires at least seven years of pretreatment data for each treated agency, and two of our treated agencies did not meet this requirement. We are left with 25 treated agencies for the GSC models. The pretreatment covariates used in constructing the synthetic controls are the following: proportion black, share of population age 18-21, share of population age 21-24, proportion in the labor force, and percent below the poverty line.

## D.7 Results for Generalized Synthetic Control Models

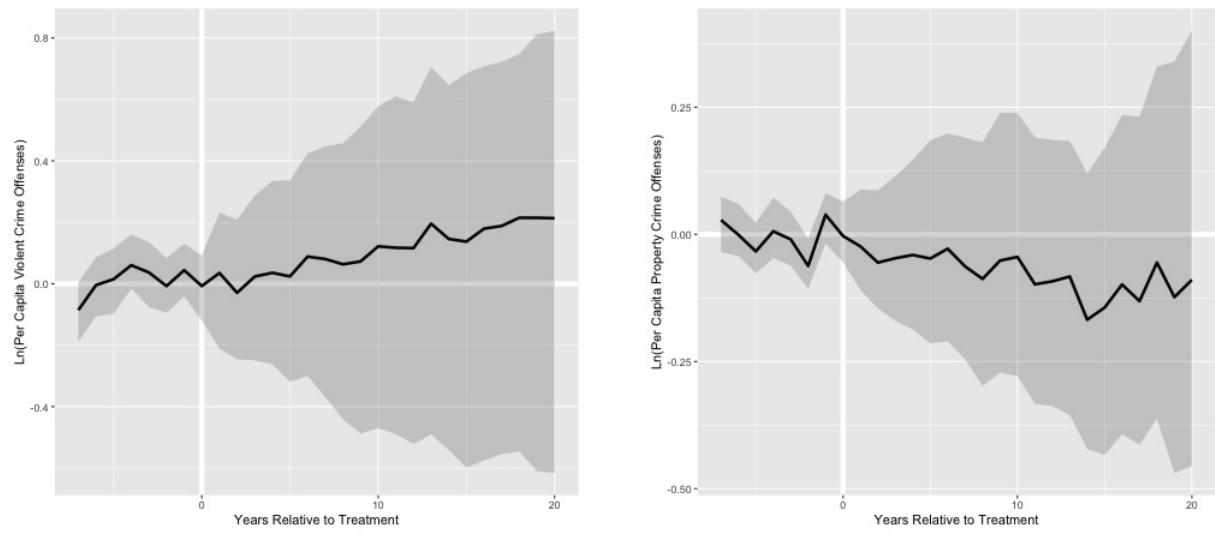
GSC models allow us to estimate treatment effects as they evolve over time, without imposing linearity. We display our results as graphs of the average treatment on the treated (ATT) over time. Each ATT is calculated by taking the difference between the treated unit and the synthetic control for a given unit, and then averaging the difference across all treated units. We do this each year over a 27 year period. Results are displayed in Figures 3 and 4. Each figure shows 7 years of pretreatment effects and 20 years of posttreatment effects. The 90% confidence interval is in gray. Confidence intervals are constructed using parametric bootstrapping.<sup>10</sup>

Figure 3 reports the estimates for the natural logs of violent and property crime per 100,000 in population; Figure 4 reports the estimates for violent and property crime arrest rates. From these estimates, we cannot infer that court-imposed affirmative action plans led to changes in either offense or arrest rates. Overall, these results corroborate the findings from our DD models, which are generally in agreement with McCrary [2007].

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<sup>9</sup>To estimate this model we use the *gsynth* package developed by Xu and Liu [2018].

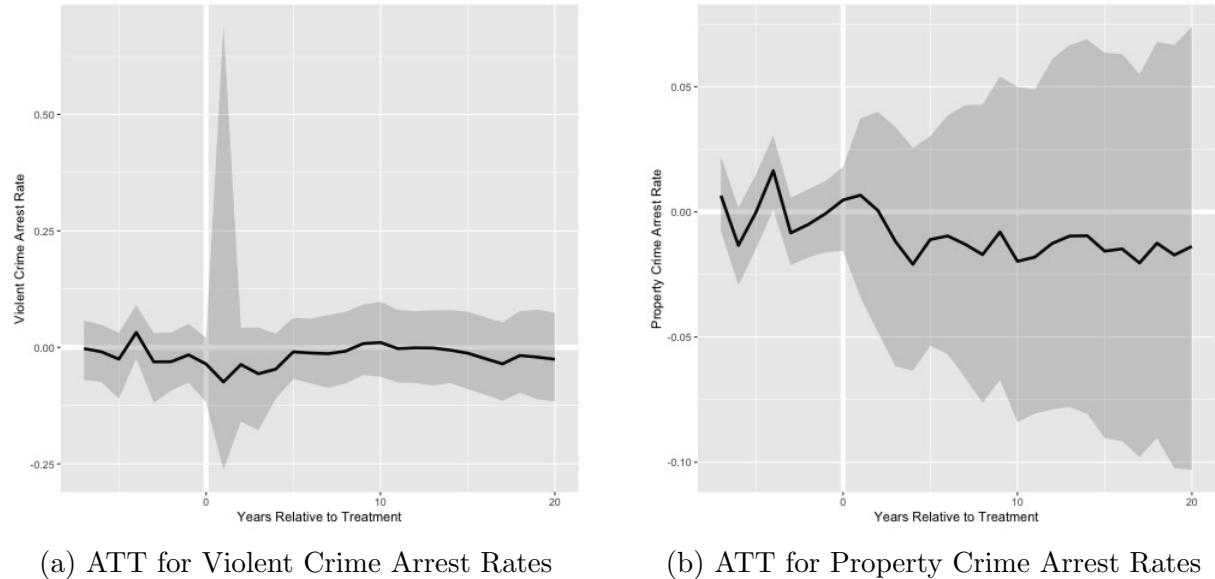
<sup>10</sup>We do not include tables showing the relative weights assigned to each agency included in the construction of each synthetic control, because these tables would be unwieldy. In most cases, the synthetic control group for a treated agency is comprised of a large number of agencies, each with a small assigned weight.



(a) ATT for  $\ln(\text{PC} \text{ Violent Crime Offenses})$

(b) ATT for  $\ln(\text{PC} \text{ Property Crime Offenses})$

Figure 3: Treatment Effects For Logged Per Capita Violent and Property Crime Offenses



(a) ATT for Violent Crime Arrest Rates

(b) ATT for Property Crime Arrest Rates

Figure 4: Treatment Effects For Violent and Property Crime Arrest Rates

## E SUTVA Violation Test

A final concern is that the law enforcement reaction to the threat of potential affirmative action litigation may have violated the stable unit treatment value assumption (SUTVA).  
18

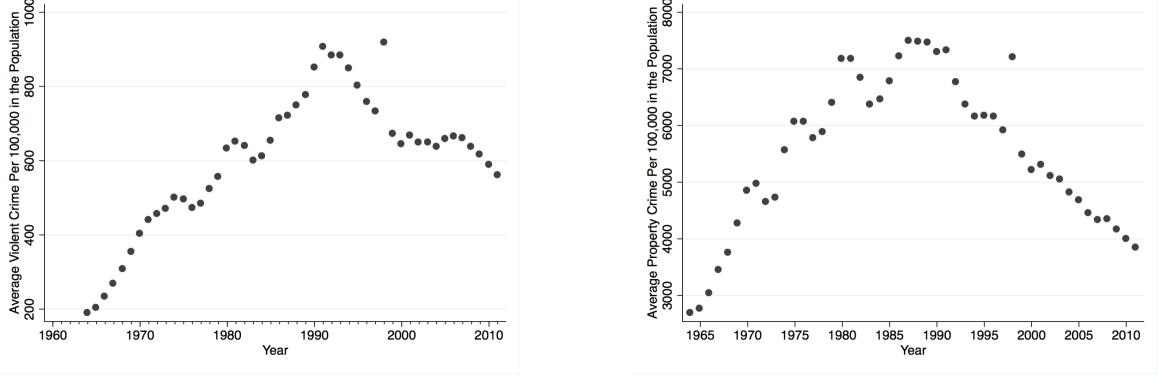
As police departments across the country began experiencing litigation for discriminatory hiring practices, other departments may have made preemptive changes to avoid litigation. Miller and Segal [2012] find that police departments that were unsuccessfully litigated for affirmative action experienced an increase in black employment prior to litigation. This suggests that departments whose leaders believed they were likely to experience litigation may have changed their behavior, implying that the treatment of affirmative action may have affected the untreated units as well as the treated. This would be a direct violation of SUTVA.

McCrory [2007] estimated whether agencies were adjusting their behavior due to the threat of litigation by matching unlitigated agencies with litigated agencies within the same federal district. He assumed the matched unlitigated agencies to be threatened agencies. He then assigned to the threatened agencies neighbor-litigation dates which were equivalent to the litigation dates of their counterparts. He identified unthreatened agencies as those agencies that had no litigated agencies within their federal district. Using an event study, he estimated the effect of neighbor-litigation dates on the minority employment gap for both threatened and unthreatened agencies. He found that there was not a significant difference in the hiring behavior of threatened and unthreatened agencies as a consequence of neighbor-litigation dates. However, McCrary [2007] looked only at litigation, not at successful litigation resulting in court-imposed affirmative action. Including unsuccessful litigation in these analyses would likely attenuate the results. Second, geographical proximity might not be a good matching mechanism.

To further explore the possibility of a SUTVA violation, we examine the effect of the total number of agencies receiving court-imposed affirmative action plans on offense and arrest rates both for agencies that will never be litigated, and for agencies that will eventually be litigated.<sup>11</sup> In order to isolate the pretreatment effect, we drop litigated agencies from our sample once they have been litigated. Since the total number of agencies receiving court-imposed affirmative action plans at any point in time does not vary across agencies, we cannot use time fixed effects to control for the trends in offense and arrest rates over time. Looking at Figure 5, we can see that there are obvious non-linear time trends in both violent and property crime rates. To control for these time trends, we implement polynomial transformations of our year variable, with the optimal number of degrees determined by minimizing the Bayesian Information Criterion (BIC) for each model. We also control for

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<sup>11</sup>We assume that agencies that will one day face litigation are aware that they have a higher probability of litigation, and thus are more likely to adjust their behavior.



(a) Average Violent Offenses PC

(b) Average Property Offenses PC

Figure 5: Offenses per 100,000 in Population

agency-specific linear time trends in crime and arrest rates.

Our model takes the following form:

$$\begin{aligned}
 \text{Offense Rates/Accident Rates}_{it} = & \beta_0 + \beta_1 AAtotal_t + \beta_2 (AAtotal * EverLitigated)_{it} + \\
 & + \sum_{p=1}^P \xi_p t^p + \boldsymbol{\alpha}_i + \boldsymbol{\lambda}_i \mathbf{t} + \epsilon_{it} \quad (4)
 \end{aligned}$$

where  $AAtotal_t$  is the total number of police departments with court-imposed affirmative action plans at time  $t$ , calculated from the data reported by Miller and Segal [2012]; *EverLitigated* is a binary variable that is equal to one (in all time periods) for agencies that will eventually experience affirmative action litigation; and  $P$  is the optimal number of degrees of the polynomial transformation of the year variable  $t$ , as reported in Table 6.  $\beta_1$  is the effect of increasing the total number of court-imposed affirmative action plans on agencies that will never be litigated, while  $\beta_1 + \beta_2$  estimates the effect for agencies that will eventually be litigated, and  $\beta_2$  is the difference in the effect.

Table 6 reports these estimates. We do not find evidence of a possible SUTVA violation. The number of agencies subjected to court-imposed affirmative action plans at any point in time does not appear to be associated with significant changes in offense or arrest rates among unlitigated agencies, either in absolute terms or relative to litigated agencies.

Table 6: Treatment Effects on the Untreated

	ln(Violent Crime PC)	ln(Property Crime PC)	Violent Crime Arrest Rate	Property Crime Arrest Rate
AA Total	-0.0004 (0.0029)	-0.0011 (0.0015)	-0.0005 (0.0006)	0.0009 (0.0005)
AA Total*Ever Litigated	-0.0120 (0.0067)	0.0000 (0.0025)	-0.0002 (0.0021)	-0.0009 (0.0011)
$\beta_1 + \beta_2$	-0.0124 (0.0059)	-0.0010 (0.0021)	-0.0006 (0.0020)	0.0000 (0.0009)
Number of Agencies	99	99	99	99
Agency Fixed Effects	Yes	Yes	Yes	Yes
Agency-Specific Time Trends	Yes	Yes	Yes	Yes
Time Polynomial Degree	3	3	1	3

Standard errors clustered on agency; \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

## F Discussion

In this paper, we replicated and extended the work of Lott [2000] and McCrary [2007] using modern difference-in-differences methods. These two studies sought to estimate the average effects of race-based affirmative action litigation on outcomes such as reported offense and arrest rates. In our replications and extensions, which also focused on offense and arrest rates, we found results generally consistent with the null results reported by McCrary [2007]. Also like McCrary [2007], we do not find evidence that agencies changed their behavior in anticipation of affirmative action litigation.

Yet it is not clear that analyzing only the average effects of court-imposed race-based affirmative action is the most productive empirical strategy. These effects may be heterogeneous by victim race. As noted previously, race-based affirmative action leading to increased proportions of nonwhite police officers may lead to increases in reporting by nonwhite crime victims, and in the willingness of civilians to cooperate with police investigations into crimes committed against nonwhite victims, while having little effect on crimes experienced by white victims. Nonwhite officers may also exert greater effort, or be better equipped socially and culturally, to clear crimes in largely nonwhite communities. Race-based affirmative action may then lead to decreases in the incidence of crimes experienced by nonwhite victims, and/or increases in the reporting of crimes experienced by nonwhite victims, while having little effect on the incidence and reporting of crimes experienced by white victims.

These potentially racially heterogeneous treatment effects may also offset each other. For example, race-based affirmative action leading to increases in the share of nonwhite officers may both increase the reporting of offenses experienced by nonwhite victims, and decrease the number of offenses experienced by these victims, with a net null effect on reported crimes experienced by nonwhite victims. Likewise, race-based affirmative action leading to increases in the number of nonwhite officers could both increase the effort devoted to clearing offenses experienced by nonwhite victims, but simultaneously decrease the number of these offenses through deterrence effects, again leading to a null effect on offenses cleared by arrest for nonwhite victims.

In order to identify and disambiguate these potentially racially heterogeneous and offsetting effects, researchers will need to look for data beyond the widely used UCR data. For example, using the National Crime Victimization Survey, which allows for the identification of the race and gender of crime victims, and for the measurement of crimes both unreported and reported to law enforcement, Miller and Segal [2018] are able to identify gender-specific causal effects of affirmative action on both the actual incidence of violent crimes and the reporting of those crimes. Likewise, the identification of the potentially racially heterogeneous and offsetting effects linking race-based affirmative action to public safety outcomes is a productive avenue for future research.

## G Conclusion

Affirmative action was aggressively implemented in police departments beginning in the 1970s. This implementation usually took the form of court-imposed hiring quotas. We examined how court-imposed race-based affirmative action has impacted both offense and arrest rates, seeking to replicate and extend the results of Lott [2000] and McCrary [2007]. Our estimates from difference-in-differences, DD decomposition, duration, and generalized synthetic control models generally support the null results of McCrary [2007].

We then analyzed potential spillover effects, to examine whether untreated agencies were changing their behavior as they observed other agencies receiving court-imposed affirmative action plans. Like McCrary [2007], we did not find evidence supporting the claim of spillover effects of treatment on the untreated.

Finally, we concluded with a discussion of the importance of identifying the possibly racially heterogeneous and offsetting causal effects of race-based affirmative action on public safety outcomes.

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