Reducing Racial Disparities in Crime Victimization: Evidence from Employment Discrimination Litigation

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Abstract

Black Americans are substantially less safe than white Americans, with persistently higher risks of crime victimization. One possible cause of racial disparities in crime victimization may lie in racially disparate law enforcement responses to crime experienced by Black and white victims. We leverage idiosyncratic variation in the litigation of law enforcement agencies for racially discriminatory employment practices to identify changes in the nature of the police response to crime victimization. Using data from the National Crime Victimization Survey between 1979 and 2004, and a series of estimators informed by recent developments in the econometrics literature, we find that successful litigation over racially discriminatory practices in law enforcement agencies substantially reduced both absolute and relative Black crime victimization. We explore possible causal mechanisms, finding that litigation over racially discriminatory employment practices a) increased the reporting of victimization to law enforcement by Black victims, but not by white victims, b) increased trust in the expected police response to victimization by both Black and white victims, but more so for Black victims, and c) consistent with the existing literature, increased Black officer shares and decreased white officer shares. These findings suggest that interventions to reduce racially discriminatory practices in law enforcement agencies can lead to meaningful reductions in both absolute and relative Black crime victimization.

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A Introduction

Several recent studies have documented racial disparities in the use of police enforcement actions, including the issuance of citations, the use of force, and the size of fines [West, 2018, Hoekstra and Sloan, 2020, Goncalves and Mello, 2021]. Relatively unexplored are racial disparities in crime victimization. In the 2019 National Crime Victimization Survey (NCVS), the most recent victimization survey available, Black respondents were 14.2% more likely to be victims of crime, relative to non-Hispanic white respondents. Racial disparities in victimization were larger for more serious crimes. Black respondents in the 2019 NCVS were 87% more likely to experience robbery and 93% more likely to experience motor vehicle theft, relative to non-Hispanic white respondents. Black Americans were approximately three times more likely to be victims of homicide between 1981 and 2018, relative to non-Hispanic white Americans [Chalfin et al., 2021]. As reported in Appendix Figure A.1, Black victimization rates in the NCVS were consistently higher than white victimization rates in the 40 largest metropolitan statistical areas (MSAs) between 1979 and 2004. The racial gap in victimization persists when measured at the city/year level, holding constant the number of police officers employed in a given city/year.

One possible explanation for racial disparities in crime victimization is that at least some police officers respond differently to crime experienced by Black victims, relative to crime experienced by white victims. These officers may exert less effort to respond to crime experienced by Black victims, leading to lower clearance rates and potentially less deterrence for crimes experienced by Black victims, relative to crimes experienced by white victims. Racial disparities in the police response to crime victimization would be consistent with racial disparities in the use of police enforcement actions [West, 2018, Hoekstra and Sloan, 2020, Goncalves and Mello, 2021].

Racial disparities in crime victimization could also exist for reasons other than racial disparities in the police response to victimization. Independently of the nature of the police response, Black victims may be less likely to make formal reports of their victimization to law enforcement agencies, relative to white victims. Witnesses to Black victimization may be less likely to cooperate with police officers, relative to witnesses to white victimization [Leovy, 2015].

Identifying causes of racial disparities in crime victimization is challenging because much of police, victim, witness, and perpetrator behavior is unobserved. We identify causal effects of changes in the police response to victimization by leveraging the timing of litigation of police departments for racially discriminatory employment practices. Departmental command staffs that engage in racially discriminatory hiring and promotion practices may also allow or even encourage racially discriminatory responses to crime victimization. The threat of judicial intervention posed by litigation over race-based discrimination in employment may induce command personnel to seek to reduce racially discriminatory practices throughout their departments. Over time, litigation resulting in judicially-imposed affirmative action plans in hiring and promotion may also increase Black officer shares [McCrary, 2007, Miller and Segal, 2012]. Black officers may be more interested in and/or more effective at policing crime experienced by Black victims, leading to further reductions in Black crime victimization [Miller and Segal, 2018].[1] However, we would not expect employment discrimination litigation brought against police departments to affect victim, witness, or perpetrator behavior independently of litigation's effects on police behavior. We would also not expect the litigation of police departments for race-based employment discrimination to reduce white crime victimization to the same degree that it reduces Black crime victimization.

Using victimization data from the National Crime Victimization Survey between 1979 and 2004, and leveraging idiosyncratic variation in the timing of successful litigation of law enforcement agencies for race-based employment discrimination between 1970 and 1986 (i.e., litigation leading to remedies that included affirmative action plans in hiring and promotion), we implement event study, two-way fixed effect difference-in-differences (TWFE DD), and the Callaway and Sant'Anna [2020a] ATT estimators of treatment effects. We first show that MSAs without law enforcement agencies that were successfully litigated for race-based employment discrimination are different on a number of dimensions relative to litigated MSAs, while litigated MSAs experiencing litigation at different points in time are much more similar to each other. These findings suggest that comparisons that leverage the variation in treatment timing within the sample of treated MSAs may be more credible than comparisons between treated and never-treated MSAs. We nonetheless find that, across estimators, employment discrimination litigation substantially reduced Black crime victimization, with smaller effects on white crime victimization, resulting in large reductions in racial disparities in crime victimization, in samples that include both treated-only and all MSAs.

In the event study analyses, post-litigation average event study estimates range between 23% (all MSAs) and 51% (treated-only MSAs) reductions in Black crime victimization; between 0% (all MSAs) and 33% (treated-only MSAs) reductions in white crime victimization; and between 107% (all MSAs) and 109% (treated-only MSAs) reductions in the racial disparity in crime victimization. The event study estimates indicate that neither absolute nor relative Black victimization were trending downward prior to litigation onset. Estimation of uniform confidence bands [Olea and Plagborg-Møller, 2019, Freyaldenhoven et al., 2018] confirm the absence of negative pretrends in absolute and relative Black crime victimization. The event study estimates also reveal sharp drops in both absolute and relative Black victimization in the year of litigation onset, suggesting that litigation affected the police response to Black crime victimization prior to increases in Black officer shares.

Across samples, TWFE DD estimates indicate an 18% reduction in Black crime victimization ($p < .01$), an insignificant increase of less than 0.1 percentage points in white crime victimization; and a 35% decrease in the racial disparity in crime victimization ($p < .01$). Goodman-Bacon [2021] decomposition of the TWFE DD subgroup estimates suggests potential downward bias in the TWFE DD estimates for both Black crime victimization and the racial gap in victimization.

[1] Police agencies tend to be whiter than the cities they police; as reported in Appendix Figure A.2, Black officer shares have persistently lagged Black population shares in the 40 largest MSAs between 1990 and 2013.

Callaway and Sant'Anna [2020a] ATT estimates, which address the potential for bias in the TWFE DD estimator, range between 9% (all MSAs) and 27% (treated-only MSAs) reductions in Black crime victimization ($p < .05$); between 0% (all MSAs) and 9% (treated-only MSAs) increases in white crime victimization ($p < .05$); and between 44% (all MSAs) and 91% (treated-only MSAs) reductions in the racial disparity in crime victimization ($p < .05$). Estimates are consistent across a series of alternative specifications. We see no comparable effects in simulations assigning placebo litigation years to the never-treated MSAs.

We also explore causal mechanisms that could be generating the large post-litigation decreases in both absolute and relative Black victimization. We find that litigation of law enforcement agencies for race-based employment discrimination a) increased reporting of victimization to law enforcement by Black victims, but not by white victims; b) increased trust in the expected police response to victimization by both Black and white victims, but more so for Black victims; and c) consistent with earlier work [McCrary, 2007, Miller and Segal, 2012], increased Black officer shares and decreased white officer shares. We find no evidence that increases in the numbers of sworn officers or changes in the demographic characteristics of litigated MSAs are plausible causal mechanisms. We find no significant heterogeneity in effects across crime types.

Our findings indicate the potential for meaningful reductions in both absolute and relative Black crime victimization from intervention into the operations of law enforcement agencies engaging in racially discriminatory practices.

B Literature Review

Prior studies estimating the causal effects of litigation of law enforcement agencies for race-based discrimination in employment have found significant post-litigation increases in Black officer shares, and significant reductions in the Black representation gap, or the difference between the percent Black police employment and the percent Black population served [McCrary, 2007, Miller and Segal, 2012]. McCrary [2007] found that litigation of law enforcement agencies for race-based employment discrimination had no effects on reported crime rates. However, if litigation both increases the reporting of crime and decreases its incidence, researchers might find no observable effects on reported crime rates. Using the litigation of law enforcement agencies for gender discrimination in employment as an instrument for female officer shares, Miller and Segal [2018] in fact found that litigation-induced increases in female officer shares increased rates of reporting of domestic violence victimization and decreased the incidence of violent crimes against women.[2]

There are at least two different pathways through which litigation over race-based employment discrimination in police departments might affect racial disparities in crime victimization. First, command staff who practice race-based discrimination in hiring and/or promotion may also tolerate

[2] Miller and Segal [2018] did not estimate the impacts of litigation of law enforcement agencies for racially discriminatory employment practices on racial disparities in reporting and victimization rates.

or even encourage racially disparate responses to crime victimization. Officers in these agencies may be allowed or even encouraged to take more time to respond to calls for service from neighborhoods with more Black residents, relative to calls from neighborhoods with fewer Black residents.[3] As a result of longer response times, officers in these agencies may be less likely to secure formal crime reports from victims, identify suspects, interview witnesses, and/or gather evidence, leading to lower clearance rates (and potentially less deterrence) for crimes experienced by Black victims, relative to crimes experienced by white victims [Blanes i Vidal and Kirchmaier, 2017]. Independently of response times, these police officers may simply exert less effort to induce Black crime victims to make formal reports to the police, and to follow through on investigating those reports, relative to white victims, again leading to lower clearance rates and less deterrence of crimes experienced by Black victims, relative to crimes experienced by white victims [Miller and Segal, 2018].[4].

Plaintiffs bringing race-based employment discrimination cases against police departments may cite racially disparate policing practices such as these as evidence of discriminatory intent on the part of command staff. The filing of these cases may then lead departments to seek to reduce racial disparities in their responses to crime victimization as a means to undermine plaintiffs' legal claims. For example, officers in agencies litigated for race-based employment discrimination may be directed to exert relatively more effort to respond to crime experienced by Black victims. This increased effort may be directed at decreasing response times to calls from neighborhoods with more Black residents, increasing the incidence of formal reports of crime experienced by Black victims, and/or increasing investigatory resources devoted to crime experienced by Black victims. This increased effort in response to litigation may increase both clearance rates and the deterrence of crimes experienced by Black victims, leading to lower Black victimization rates.

Second, over time, employment discrimination litigation leading to affirmative action plans imposed on law enforcement agencies may also increase the shares of Black police officers and decrease the shares of white officers [McCrary, 2007, Miller and Segal, 2012]. Increased Black officer shares may also decrease Black victimization, even without any directives from command staff. Black officers may care more about detecting and deterring crime experienced by Black victims, relative to white officers. Black officers may have better information about the patterns of criminal behavior affecting Black victims, relative to white officers [Miller and Segal, 2018].[5]

Importantly, we would not expect employment discrimination litigation brought against police

[3] In 2011 the ACLU filed a suit against the Chicago Police Department alleging longer 911 call response times for calls originating in neighborhoods with more Black residents; the litigation is ongoing (https://www.chicagotribune.com/news/ct-xpm-2011-10-28-ct-met-aclu-police-lawsuit-20111028-story.html).

[4] A related body of literature documents worse job performance by majority-group employees in the context of workplace discrimination against minority groups [Glover et al., 2017], and finds that firms that discriminate in hiring and promotion are less likely to survive [Weber and Zulehner, 2014]

[5] Because litigation for racially discriminatory employment practices may directly affect police behavior through the first causal pathway discussed above, an estimator using litigation as an instrument to predict Black officer shares would violate the exclusion restriction in our context. We instead estimate the reduced form effect of litigation on outcomes. These estimates will report the total effect of litigation on victimization through both causal pathways.

departments to affect victim, witness, or perpetrator behavior independently of litigation's effects on police behavior. We can thus use variation in the incidence and timing of employment discrimination litigation to isolate changes in the police response to victimization brought about by that litigation.

We would also not expect employment discrimination litigation to have comparable negative effects on white victimization rates. Directives to increase police responsiveness to Black crime victimization may decrease police responsiveness to white crime victimization, leading to increases in white crime victimization rates. Alternatively, efforts to reduce Black crime victimization may have spillover negative effects on white crime victimization, but we would not expect these effects to be as large as the negative effects on Black crime victimization. These positive and negative effects on white crime victimization may also offset each other, leading to no observed effects on white crime victimization rates. In general we would expect to see larger decreases in Black crime victimization, relative to changes in white crime victimization, and thus to see decreases in relative Black crime victimization.

B.1 United States v. City of Chicago (1973)

These potential causal mechanisms are illustrated by the history of the employment discrimination litigation filed against the Chicago Police Department (CPD)in 1973, culminating in a 1976 judicial order to end discriminatory employment practices and to hire and promote more Black police officers (United States v. City of Chicago, 411 F. Supp. 218 (N.D. Ill. 1976)).[6]

In the late 1960s approximately 17% of CPD officers were Black, while approximately 32% of the city's residents were Black [Pihos, 2015]. In a series of surveys of police officers in Chicago, Boston, and Washington D.C. conducted in 1966, Black and Reiss [1967] found that 38% of white officers were "highly prejudiced, extremely anti-Negro" while 34% of white officers were "prejudiced, anti-Negro" [Black and Reiss, 1967]. In surveys of residents of these cities, Black and Reiss [1967] found that white civilians were more satisfied with the policing in their city than Black civilians.

In 1968 a group of Black CPD officers formed the Afro-American Patrolmen's League (AAPL) in response to what they perceived to be the failure of the largely white Chicago Police Department to protect residents of largely Black neighborhoods. The AAPL's leaders decried the response of the CPD to calls for service from Black neighborhoods, including not responding to calls, responding too slowly, refusing to take reports or to investigate crime complaints, and/or using force against those reporting crime victimization.[7] The AAPL sought to achieve greater safety for Black civilians both by changing the behavior of white CPD officers ("They hope to teach their white counterparts that respect for the black community is essential in enforcing the law"),[8] and by advocating for the

[6] The Chicago litigation is also featured in McCrary [2007]. We draw heavily upon the narrative history of these events in Pihos [2015].

[7] Renault Robinson, "A Backwards View of Problems," Chicago Defender, June 26, 1971, 30; Renault Robinson, "Beware the 'Big Trick,"' Chicago Defender, July 17, 1971, 4; Renault Robinson, "What's Wrong with City's Police Boss," Chicago Defender, April 10, 1972, 8.

[8] "Black Police League Has Tough Job Ahead," Sheryl Fitzgerald, Chicago Defender, September 14, 1968.

hiring and promotion of more Black police officers.

By 1973, the AAPL's leaders had not seen the CPD implement the changes they had been seeking. Renault Robinson and other Black AAPL leaders filed a federal employment discrimination claim against the CPD in April 1973; in May 1973 a second employment discrimination suit was filed against the CPD by Black and Hispanic CPD applicants.[9] A key claim of these complaints was that evidence of the CPD's discriminatory practices could be found not only in its hiring and promotion procedures and outcomes, but also in the CPD's lack of responsiveness to crime in Black neighborhoods. Renault Robinson editorialized in 1973 in the *Chicago Defender*, the city's Black newspaper, "The next time your precinct captain has the nerve to knock at your door and ask for your vote for King Daley remember what you are voting for: a police department that does nothing to stop serious crime in the black community – a police department that refuses to hire and promote black police officers – a police department that disrespects black people."[10] In August 1973, the Department of Justice (DOJ) filed its own employment discrimination suit against the CPD, the first time the DOJ had used its newly available powers under the 1972 amendments to Title VII to bring an employment discrimination case against a police department. The three lawsuits were consolidated the following year.

As later recounted in the District Court's 1976 opinion, the 1973 plaintiffs produced testimony not only on the disparate impact of the CPD's hiring and promotion procedures, but also on the Department's failure to protect Black civilians from crime: "We could take judicial notice of the tensions that have existed between white officers and black citizens in predominantly black neighborhoods, but we need not do so for that was the thrust of a good portion of the testimony of the witnesses called by the Robinson plaintiffs. Indeed, one of the avowed purposes of the Afro-American Patrolmen's League is to relieve those tensions through, inter alia, the recruitment and employment of more black officers to serve and protect the black citizenry which is so desperately in need of that service and protection" (United States v. City of Chicago, 411 F. Supp. 218 (N.D. Ill. 1976)).

The city's response to the 1973 filings was immediate. Police Superintendent James Conlisk resigned within two months of the DOJ filing. His interim replacement, James Rochford, was directed by the Chicago Police Board, the CPD's civilian oversight body, to consider the adoption of a variety of policies related to the CPD's hiring and promotion of Black and Hispanic officers, and to the department's policing practices in Black neighborhoods.[11] Upon taking office, Rochford immediately requested letters of resignation from all seventy CPD command officers. At his first press conference, he promised "a safer Chicago, control of street gangs, elimination of police misconduct,

[9] The AAPL plaintiffs had filed an earlier suit in 1970 alleging retaliation for their organizing efforts; their 1973 filing expanded their 1970 complaint to include a generalized employment discrimination claim against the CPD.

[10] Renault Robinson, "Black Watch," Chicago Defender, June 16, 1973.

[11] William Mullen and Pamela Zekman, "Probe Brutality, Police Told: Board Orders Rochford to Take Immediate Steps," Chicago Tribune, November 16, 1973.

a reduction in street crime, police respect for citizens and the public's respect for policemen."[12] Rochford began to meet regularly with Black-led activist organizations.[13] Mayor Richard Daley likewise met with activists and promised to expand and diversify the Police Board.[14] These actions were all taken in 1973 and 1974, well before the 1976 court order imposing a hiring and promotion plan on the Department.

The history of the litigation effort to end employment discrimination in the Chicago Police Department illustrates how plaintiffs could use to their advantage evidence of racial disparities in a department's response to crime victimization. The responsiveness of the CPD to the plaintiffs' allegations in the year that the litigation was filed likewise illustrates the incentives that departments had to address racial disparities in their responses to crime victimization once they faced the prospect of judicial intervention.

C Data

C.1 National Crime Victimization Survey

We source data on crime victimization between 1979-2004 from the MSA-level release of the National Crime Victimization Survey (NCVS). The NCVS has been conducted annually since 1973 by the U.S. Census Bureau on behalf of the Bureau of Justice Statistics (BJS). The standard NCVS releases do not contain identifiers for geographic units smaller than region. However, one NCVS release reported victimization data for the core counties of the forty largest MSAs in the United States between 1979 and 2004. This release is available through the National Archive of Criminal Justice Data (United States Department of Justice, Bureau of Justice Statistics, 2007).

Between 1979 and 2004 the NCVS was conducted on a nationally representative sample of about 50,000 housing units.[15] Household members aged 12 years and older were interviewed regarding crime incidents twice a year for three consecutive years.[16] Participants were asked screening questions to determine if they had been victimized during the six-month period preceding the first day of the month of the interview. Because the NCVS measures both property and personal crime, a separate screening section was administered for each of these crime categories. Screening questions covered the following types of crimes, including attempts: robbery, burglary, theft, assault, and

[12] Philip Wattley, "Rochford Starts Shakeup: 70 Top Cops Asked to Quit," Chicago Tribune, February 16, 1974.

[13] Citizens Alert, "Report to the Illinois Law Enforcement Commission: October 1, 1974 to September 30, 1975," 1975, Citizens Alert.

[14] Alan Merridew and George Bliss, "Daley Rips Police Brutality: Pledges Action to Combat It," Chicago Tribune, Dec. 6, 1973, 1.

[15] Additional detail on the NCVS sampling frame is reported in Appendix B.

[16] Each month during our sample period the U.S. Census Bureau selected respondents for the NCVS using a "rotating panel" sample design. Households were randomly selected and all age-eligible individuals became part of the panel. Once in the sample, respondents were interviewed every six months for a total of seven interviews over a three-year period. The first and fifth interviews were face-to-face; the rest were by telephone. After the seventh interview the household left the panel and a new household was rotated in to the sample.

rape. The household respondent was asked to report on crimes against the household as well as personal crimes against him/herself. Other members of the household were asked only about personal crimes. Positive responses led to additional questions that gathered details about the nature of the incident, including whether it was reported to the police.

C.2 Affirmative Action Data

We source data on litigation alleging race-based employment discrimination by law enforcement agencies from Miller and Segal [2012]. To create this litigation database, Miller and Segal [2012] first collected employment data from confidential EEO-4 reports on 479 of the largest U.S. state and local law enforcement agencies between 1973 and 2005.[17] They then searched the Lexis-Nexis and Westlaw federal databases for employment discrimination cases involving these agencies, finding 140 cases brought by private plaintiffs or the U.S. Department of Justice (DOJ) between 1969 and 2000. They further identified cases among this set that resulted in court orders or settlement agreements imposing affirmative action plans in hiring and/or promotion. Cases were dated by the year in which the litigation was filed.[18] Among the set of cases for which the target group could be identified, 96% involved Black employees.

Following Miller and Segal [2018], we retain from the litigation sample collected by Miller and Segal [2012] only those county and municipal law enforcement agencies located within a core county of one of the forty largest MSAs in the National Crime Victimization Survey sample. There are 167 such agencies. Each of the forty MSAs in the NCVS sample includes at least one department from the Miller and Segal [2012] litigation database. As in Miller and Segal [2018], treatment is defined at the MSA level. We characterize an MSA as having been subjected to litigation if any of the agencies in the litigation sample for that MSA were subjected to litigation between 1969 and 2000. For those MSAs with core county agencies subjected to litigation, litigation date is defined as the earliest year in which any agency in an MSA core county experienced litigation.

Miller and Segal [2012] find that employment discrimination litigation not resulting in affirmative action plans in hiring/promotion induced lower rates of post-litigation nonwhite hiring, relative to litigation leading to externally-imposed affirmative action plans. These unsuccessful litigation efforts may also have induced different immediate responses from law enforcement agencies. In the 40 MSAs in the NCVS sample, 26 MSAs contain at least one law enforcement agency that was litigated for race-based employment discrimination, with the litigation resulting in a post-litigation affirmative action plan in hiring or promotion; we characterize these MSAs as treated MSAs. We characterize the remaining 14 MSAs as never-treated MSAs.[19]

[17] Departments were included in the sample if they had at least 200 full-time workers at some point in the sample period, had at least 200 protective and professional workers at some point in the sample period, and appeared in the sample for at least 10 years.

[18] The Miller and Segal [2012] data do not include the years in which affirmative action plans were imposed on law enforcement agencies.

[19] 11 of the never-treated MSAs contain no law enforcement agencies that were litigated for race-based employment

In the set of 108 agencies located in the 26 treated MSAs, namely MSAs containing at least one agency subject to both litigation and post-litigation affirmative action, the first litigation onset date is 1970; the last is 1986. Figure 1 reports the variation in timing of litigation onset for these 26 treated MSAs.

D Analysis

D.1 Assessing Treatment Exogeneity

MSAs containing law enforcement agencies that were both litigated for race-based employment discrimination, and subjected to post-litigation affirmative action plans in employment, may have been different on a number of dimensions, relative to other MSAs [McCrary, 2007]. These differences may have affected the evolution of racial disparities in victimization across treated and never-treated MSAs.

Among the set of MSAs containing law enforcement agencies that were both litigated for race-based employment discrimination, and subjected to post-litigation affirmative action plans in employment, the assumption of as-if random variation in the timing of litigation is perhaps more plausible. The employment discrimination cases brought against law enforcement agencies, many of which involved multiple parties and multiple actions, typically had lengthy and complex histories. The precise date of litigation onset for that part of a litigation effort resulting in a post-litigation affirmative action plan may in many cases have been plausibly exogenous to factors also affecting crime victimization. Consistent with this hypothesis, McCrary [2007] found smaller differences in pretreatment covariates across agencies litigated at different times, relative to differences across agencies that were either litigated or unlitigated.[20]

We first explore the plausible exogeneity of both the presence and the timing of post-litigation affirmative action plans in the 167 law enforcement agencies in our sample of treated and never-treated MSAs using county-level demographic measures sourced from the 1970 Census. We estimate both the probability of treatment and, conditional on treatment, the year of treatment, as a function of 1970 log population, 1970 percent Black, 1970 median age, 1970 median family income (in thousands), 1970 median years of school, 1970 percent urban, and an indicator for whether an MSA experienced a riot between 1961 and 1968 [Spilerman, 1970, McCrary, 2007]. Specifications

discrimination between 1969 and 2000. Two of the never-treated MSAs (Dallas and Oakland) contain only agencies that were never litigated, or agencies that were litigated for race-based employment discrimination, but the litigation did not result in an affirmative action plan in hiring or promotion. The Tampa MSA contains four agencies that were never litigated, one agency that was litigated with no resulting affirmative action plan (the St. Petersburg Police Department in 1975), and one agency that was litigated in 1980 with a post-litigation affirmative action plan (the Pinellas County Sheriff's Department) We code Tampa as a never-treated MSA; results are qualitatively unchanged if Tampa is coded as a treated MSA.

[20] Work in other contexts has likewise found smaller differences in pretreatment covariates across variation in treatment timing within the set of treated units, relative to differences across treated vs. untreated units [Johnson, 2015, Deshpande and Li, 2019].

and estimates are reported in Appendix C.

As reported in Appendix Table C.1, law enforcement agencies that would eventually be litigated for racially discriminatory employment practices and subjected to post-litigation affirmative action plans are located in counties with larger percentages of Black residents in 1970 ($p < .05$ for agency-, county-, and MSA-level specifications), fewer median years of schooling in 1970 ($p < .10$ for agency- and county-level specifications), and higher median family incomes in 1970 ($p < .05$ for agency-, county-, and MSA-level specifications), relative to agencies that would never be litigated. By contrast, no covariates are significant in the models restricted to treated agencies that predict treatment timing. These estimates are consistent with those of McCrary [2007], who found that covariate differences across cities litigated at different times were much smaller than differences across litigated and unlitigated cities.

We can also use the NCVS data to predict both treatment and treatment timing using the samples of respondents in never-treated MSAs between 1979 and 1985, and in treated MSAs that had not yet been subjected to treatment between 1979 and 1985. Our set of pretreatment observable characteristics includes respondent-level indicators for race, homeownership, residence in a single family home, household income greater than $30,000, some college, age between 18 and 29, married, female, and a respondent's experience of victimization.[21]

Table 1 reports descriptive statistics across never-treated and treated MSAs during pretreatment years. Never-treated MSAs are unlike MSAs containing law enforcement agencies that would eventually be successfully litigated for racial discrimination in employment. Among other differences, never-treated MSAs between 1979 and 1985 have on average fewer NCVS respondents per year, smaller proportions of Black respondents, and smaller racial disparities in homeownership, residence in single-family homes, and marital status, relative to MSAs that would eventually experience litigation and post-litigation affirmative action. Perhaps of greatest concern, never-treated MSAs have substantially smaller racial disparities in victimization rates, reporting rates, and reported victimization rates, relative to treated MSAs pretreatment.

Appendix Table C.2 reports estimates from models predicting treatment status and, conditional on treatment, treatment timing, using the sample described in Table 1. There are several correlations between respondent-level pretreatment covariates and whether an MSA will experience litigation leading to affirmative action in law enforcement; the joint p-values are 0 both for the model without interactions and for the model interacting covariates with respondent race. These correlations disappear, however, for the models predicting treatment timing. The joint p-values for these models are 0.11 for the model without interactions and and 0.33 for the model interacting covariates with respondent race.

[21] Black respondents are defined as those respondents who self-identify as Black, either alone or in combination with other race/ethnicity categories. White respondents are defined as non-Hispanic white respondents. Victimization is defined as whether a respondent reported being the victim of any crime during the six months prior to their NCVS interview.

The many observable pretreatment differences across never-treated and treated MSAs suggest that the former may not be a good comparison group for the latter. By contrast, the lack of observable pretreatment differences across treated MSAs that varied only in treatment timing suggests that later-treated MSAs may serve as a comparable control group for earlier-treated MSAs. As in Johnson [2015] and Deshpande and Li [2019], we place more weight on the estimates from the treated-only sample that leverage variation in treatment timing. However, we also report estimates that include the never-treated MSAs as a comparison group.

D.2 Estimating Treatment Effects

We want to estimate post-litigation changes in crime victimization rates in treated MSAs, relative to pre-litigation victimization rates, relative to changes in crime victimization rates in MSAs not experiencing litigation. The timing of litigation onset varies across MSAs, and we cannot rule out the possibility of heterogeneous treatment effects, including treatment effects that increase over time. Estimates of the effects of staggered treatment onset may be biased in the presence of heterogeneous treatment effects when earlier-treated units are used as controls for later-treated units [Callaway and Sant'Anna, 2020a, Sun and Abraham, 2020, Goodman-Bacon, 2021, Borusyak and Jaravel, 2020].

We begin by reporting event study estimates, which do not use earlier-treated units as controls for later-treated units. Event study estimates also allow us to investigate the presence of pre-litigation trends in crime victimization rates. We report estimates both for the sample that includes only those MSAs that were at some point subjected to employment discrimination litigation leading to affirmative action, and for the sample that includes all MSAs.

We then explore the potential for bias in the two-way fixed effect difference-in-differences (TWFE DD) estimator of variance-weighted average treatment effects on the treated (VWATT) using the diagnostic developed by Goodman-Bacon [2021], and report the TWFE DD estimates of the VWATT. We find smaller estimated magnitudes for comparisons that leverage earlier treated MSAs as comparison units for later-treated MSAs, indicating some potential for bias in the TWFE DD estimates, although all subgroup point estimates are consistently signed for both absolute and relative Black crime victimization. Finally, we report estimates of average treatment effects on the treated (ATT) using the estimator developed by Callaway and Sant'Anna [2020a], which addresses the potential for bias in the TWFE DD estimator. Overall, estimates are consistent across the event study, TWFE DD, and Callaway and Sant'Anna [2020a] ATT estimators.

D.2.1 Event Study Estimates

We estimate event study models of changes in crime victimization relative to the last pre-litigation year, conditioning on fixed differences across MSAs and years, using the estimator specified in Equation 1:

$$Victimization_{imt} = \sum_{\substack{y=-7 \\ y \neq -1}}^{y=24} \beta_y I(t - t_m^* = y) + \boldsymbol{\beta_t} + \boldsymbol{\beta_m} + \epsilon_{imt} \quad (1)$$

In Equation 1, $Victimization_{imt}$ is a binary indicator for whether a respondent i in MSA m interviewed in year t reported having been a victim of a crime in the previous six months. Indicator variables $I(t - t_m^* = y)$ denote pre- and posttreatment years relative to litigation year t_m^*; following best practices, we include the full set of pre- and posttreatment years in the estimation [Borusyak and Jaravel, 2020, Sun and Abraham, 2020]. The omitted category is $y = -1$, the year immediately prior to litigation onset. The estimates of β_y report the changes in victimization in treated MSAs, relative to the year immediately prior to litigation onset. For analyses including all MSAs, the indicator variables in Equation 1 are multiplied by AA_m, a binary variable equal to one for the 26 MSAs subjected to litigation leading to affirmative action between 1970 and 1986, and equal to zero for the 14 remaining MSAs; event indicator variables are zero for the never-treated MSAs.[22] $\boldsymbol{\beta_t}$ are calendar year fixed effects and $\boldsymbol{\beta_m}$ are MSA fixed effects. We estimate Equation 1 with a linear probability model, and report heteroskedasticity-robust standard errors that are clustered at the MSA level.

Figure 2 reports the estimates of β_y for $y = -7$ to $y = 24$ from Equation 1 for Black NCVS respondents, along with 95% confidence intervals. Estimates for the sample restricted to treated only MSAs are reported in Panel (a); estimates for the sample that includes both treated and never-treated MSAs are reported in Panel (b).

The estimates reported in Figure 2 indicate that Black victimization rates were not already trending downward prior to litigation onset, relative to the last year prior to treatment. There is perhaps a slight trend upward in Black victimization rates prior to litigation onset; this pretrend is slightly more pronounced in the sample that includes never-treated MSAs, although no point estimates are significant for the four years prior to the last pretreatment year. In both plots, this slight upward trend reverses sharply with the onset of litigation. In both plots, Black victimization rates decline posttreatment, relative to pretreatment baseline rates, even after removing common time trends through year fixed effects. Averaging over the posttreatment event study estimates, in the sample that excludes never-treated MSAs Black respondents see on average a 10.4 percentage point reduction in crime victimization following litigation onset, or a 51.2% percent decrease relative to the 20.3% average Black victimization rate in yet-to-be-treated MSAs. In this sample, treatment effects appear immediately for Black respondents, grow in magnitude over time, and are significant at the 95% level in all years after litigation onset. In the sample that includes never-treated MSAs, Black respondents see on average a 4.6 percentage point reduction in crime victimization following the onset of litigation, or an average 22.7% percent decrease relative to the baseline rate, with no

[22]Because of the evident imbalances in pretreatment covariates across treated and never-treated MSAs, we also include the vector of respondent-level covariates from Table 1 in models that include never-treated MSAs.

evident increase in the treatment effect over time. These annual changes in Black victimization are significant at the 95% level for the first nine years after litigation onset.

Figure 3 reports the estimates for non-Hispanic white respondents. As expected, the estimated effects of successful employment discrimination litigation on white crime victimization are considerably smaller than the estimates reported in Figure 2. In the treated-only sample, non-Hispanic white respondents experience on average a 4.3 percentage point reduction in crime victimization following litigation onset, a 32% decrease relative to the baseline victimization rate of 13.5% in yet-to-be-treated MSAs. In the sample including never-treated MSAs, non-Hispanic white respondents experience on average an increase of less than one percentage point in crime victimization following the onset of litigation leading to affirmative action, although the annual estimates are not themselves significant at the 95% threshold.

The event study estimates reported in Figures 2 and 3 indicate that litigation of law enforcement agencies for employment discrimination reduced Black crime victimization more than it reduced white crime victimization. We can directly estimate the effects of the onset of employment discrimination litigation on the racial gap in crime victimization using Equation 2:

$$\begin{aligned} Victimization_{imt} = \sum_{\substack{y=-a \\ y\neq -1}}^{y=b} \beta_y I(t - t^*_m = y) + \beta_b Black_{imt} + \\ Black_{imt} \times \sum_{\substack{y=-a \\ y\neq -1}}^{y=b} \beta_y I(t - t^*_m = y) + \boldsymbol{\beta_t} + \boldsymbol{\beta_m} + \epsilon_{imt}, \quad (2) \end{aligned}$$

In Equation 2, the event indicators are multiplied by $Black_{imt}$, an indicator for whether a respondent is Black (1) or white (0). When $Black_{imt} = 1$, the estimates of β_y report the changes in Black victimization in treated MSAs, relative to changes in white victimization, relative to the year immediately prior to plan implementation. For analyses including all MSAs, the indicator variables in Equation 2 are also multiplied by AA_m, a binary variable equal to one for the 26 MSAs subjected to litigation leading to affirmative action between 1970 and 1986, and equal to zero for the 14 remaining MSAs. Event indicator variables are zero for all never-treated MSAs. We estimate Equation 2 with a linear probability model, and report heteroskedasticity-robust standard errors that are clustered at the MSA level.

Figure 4 reports the estimates of β_y from Equation 2 when $Black_{imt} = 1$, for $y = -7$ to $y = 24$, along with 95% confidence intervals, for treated-only and all MSAs. The estimates reported in Figure 4 indicate that, in both samples, racial disparities in victimization rates were not already trending downward prior to litigation onset. There appear to be slight positive pretreatment trends in racial disparities in victimization in both samples, relative to the last year prior to litigation onset, although no single coefficient can be distinguished from zero with 95% confidence in either plot.

The slight positive pretreatment trends in racial disparities in victimization are halted and sharply reversed immediately after litigation onset in both samples, suggesting a law enforcement response to litigation that occurred before substantial changes to Black officer shares. In both samples, post-litigation estimates of decreases in racial disparities in victimization rates, relative to the last year prior to treatment, range between 6.3 and 8.9 percentage points over the 25 years after litigation onset. In both samples, these estimates are all significant at the 95% threshold. The averages of the post-litigation estimates are 7.3 (all MSAs) and 7.6 (treated-only MSAs) percentage point decreases in the gap between Black and white victimization rates, or 107% - 109% decreases relative to the 6.8 percentage point racial gap in victimization in yet-to-be-treated MSAs.

Appendix D reports event study estimates that implement the uniform 95% sup-t confidence intervals recommended by Freyaldenhoven et al. [2018] and Olea and Plagborg-Møller [2019]. Pointwise confidence intervals only allow for the testing of pointwise hypotheses. Because uniform confidence intervals are designed to contain the true path of the coefficients 95% of the time, they are more useful for assessing pretrends [Freyaldenhoven et al., 2018]. We see no significant pretrends in the data using the uniform confidence intervals.

We also explore a number of alternative event study specifications in Appendix D. These include: including a vector of time-varying respondent-level covariates for both the treated-only and the all-MSA samples, using a balanced panel of 5 treated MSAs and 20 years, alternative sets of time and unit-by-time fixed effects, and collapsing the respondent-level data to an MSA/year panel for both the treated-only and the all-MSA samples. Our findings are consistent across these alternative specifications.

D.2.2 VWATT and ATT Estimators

The event study estimators do not report point estimates and standard errors for overall average treatment effects on the treated (ATT). In Appendix E we report two-way fixed effect difference-in-differences (TWFE DD) estimates of the variance-weighted average treatment effect on the treated (VWATT) [Goodman-Bacon, 2021] for the treated-only and all-MSA samples, after collapsing the individual-level NCVS data to an MSA/year panel. Across samples, the VWATT estimate for Black crime victimization is a 3.6 - 3.7 percentage point or 18% decrease relative to the pretreatment mean Black victimization rate of 20.3% in yet-to-be treated MSAs ($p < .01$). The estimate for non-Hispanic white victimization is an insignificant increase of less than 0.1 percentage points. The estimate for the racial gap in victimization is a 2.3 - 2.4 percentage point or 35% decrease relative to the pretreatment racial gap in victimization of 6.8 percentage points in yet-to-be-treated MSAs ($p < .01$).

Several recent papers have explored the potential for bias in the TWFE DD estimator in the context of staggered treatment timing and heterogeneous treatment effects. The VWATT estimated by the TWFE DD estimator is aggregated from a series of DD estimates derived from 2 x 2 treatment/control groups, which themselves compare units treated at the same time to units treated at another time (earlier or later) and to never-treated units. The VWATT can differ substantially from the true ATT in the presence of staggered treatment timing and treatment effect heterogeneity (and may even produce sign reversals, relative to true treatment effects) [Goodman-Bacon, 2021, Callaway and Sant'Anna, 2020a]. Event study estimates that do not account for heterogeneous treatment effects may also incorporate bias [Sun and Abraham, 2020, Borusyak and Jaravel, 2020].

We report in Appendix Table E.1 for the treated-only sample the subgroup point estimates and weights from the difference-in-differences decomposition model developed by Goodman-Bacon [2021] to uncover the potential for bias in TWFE DD estimates. Point estimates for the comparisons between the always-treated MSAs and the MSAs for which treatment status varies are smaller in absolute value than those for the comparisons between the MSAs for which treatment status varies, indicating the potential for downward bias in the TWFE DD estimator. The subgroup point estimates reported for both Black crime victimization and the racial gap in victimization are, however, consistently negative, suggesting that bias in the TWFE DD estimator is likely playing a relatively small role in the TWFE DD estimates. By contrast, DD estimates for the effect of litigation on white crime victimization are both smaller in magnitude and inconsistently signed across subgroups.

The ATT estimator developed by Callaway and Sant'Anna [2020a] addresses the potential for bias in the TWFE DD estimator. The estimator excludes always-treated units from the sample, because including those units adjusts the path of outcomes for newly treated units by the path of outcomes for already treated units. As such, the counterfactual is not untreated potential outcomes, but treatment effect dynamics, making estimates difficult to interpret.[23]

Table 2 reports the Callaway and Sant'Anna [2020a] overall ATT estimate θ_S^O for multiple samples. The NCVS respondent-level data are collapsed to the MSA/year level. All estimates use the Callaway and Sant'Anna [2020b] doubly-robust estimator and cluster standard errors on the MSA.

In the sample of MSAs with agencies that would eventually be subjected to successful race-based employment discrimination litigation, the Callaway and Sant'Anna [2020a] overall ATT estimate is a 5.5 percentage point decrease in Black victimization, a 27% decrease relative to the 20.3% average Black victimization rate in yet-to-be-treated MSAs ($p < .05$). Non-Hispanic white victimization is estimated to increase by 0.8 percentage points (not significant). The racial gap in victimization is estimated to decrease by 6.2 percentage points, a 91% decrease relative to the pretreatment racial gap in victimization of 6.8 percentage points ($p < .05$). In the samples that include all MSAs, the Callaway and Sant'Anna [2020a] overall ATT estimate is a 1.7 - 1.8 percentage point decrease in Black victimization, a 9% decrease relative to the 20.3% average Black victimization rate in yet-to-be-treated MSAs ($p < .05$). Non-Hispanic white victimization is estimated to increase by

[23] For details on the construction of the Callaway and Sant'Anna [2020a] ATT estimator see Appendix E.1.

1.2 percentage points, a 9% increase relative to the 13.5% average white victimization rate in yet-to-be-treated MSAs ($p < .05$). There is an estimated 2.9 - 3 percentage point decrease in the racial gap in victimization, a 44% decrease relative to the 6.8 percentage point racial gap in victimization in yet-to-be-treated MSAs ($p < .05$).

The Callaway and Sant'Anna [2020a] ATT estimates are consistent with both the event study and the TWFE DD estimates. Successful litigation of law enforcement agencies for employment discrimination led to decreases in Black crime victimization. These estimates are larger for the treated-only sample, our preferred sample. Successful litigation of law enforcement agencies for employment discrimination did not similarly lead to reductions in crime victimization for non-Hispanic white respondents. As a consequence, the racial gap in crime victimization decreased post-litigation.

Finally, in Appendix E we report TWFE DD estimates of the effects of placebo treatment years on the 14 MSAs containing no law enforcement agencies that were subjected to litigation leading to affirmative action plans. We assign a placebo litigation year between 1970 and 1986, with replacement, to each never-treated MSA; we iterate this random assignment 10 times. For each set of randomly assigned placebo years, we estimate a TWFE DD model that interacts respondent race with the posttreatment indicator. Appendix Figure E.1 reports the coefficients on the interaction terms; none are significant at conventional thresholds.

D.3 Reporting Rates

Miller and Segal [2018] found that litigation leading to gender-based affirmative action in law enforcement increased the rate at which female victims reported gender-based violence to law enforcement agencies, and attributed post-litigation decreases in gender-based violence at least in part to this increased reporting. We can likewise use the NCVS data to ask whether employment discrimination litigation leading to race-based affirmative action in law enforcement disproportionately increased rates of reporting by Black victims. If so, this may have been a mechanism contributing to decreases in Black crime victimization.

Appendix Figure F.1 plots NCVS reporting rates by race for respondents in treated MSAs between 1979 and 2004. There is suggestive evidence that, after all MSAs in this sample were treated, reporting rates began to rise more steeply for Black respondents, relative to white respondents.

We estimate the effects of employment discrimination litigation leading to affirmative action on changes in the reporting of crime victimization using Equation 1. For these models we restrict the sample to NCVS respondents who reported having experienced a crime in the six months prior to their interview. The outcome of interest is $Reported_{imt}$, which is 1 if the respondent reported the crime to law enforcement, and 0 otherwise. Figure 5 reports the estimates of β_y for both Black and white respondents, for the set of 26 treated MSAs.

Prior to the onset of litigation leading to affirmative action, reporting rates for Black respondents appear to be decreasing in the yet-to-be-treated MSAs. After litigation onset, Black reporting rates reversed their prior downward trend and began to steadily increase, with the changes relative to the baseline year becoming distinguishable from 0 at the 95% confidence level after approximately 10 years. Post-litigation estimates of increases in Black reporting rates average 6 percentage points over the 25-year post-litigation reporting period in these MSAs. By contrast, reporting rates for non-Hispanic white respondents were already trending upward in yet-to-be-treated MSAs. After the onset of litigation leading to affirmative action, reporting rates for white respondents initially dropped, then continued their prior upward growth but at a gradually decreasing rate.

Table 3 reports Callaway and Sant'Anna [2020a] estimates of the ATTs for reporting rates by race, for treated-only and all MSAs. In the treated-only sample, the ATT estimates indicate an increase in Black reporting rates and a decrease in white reporting rates, but the confidence interval for Black reporting rates is very large. In the all-MSA sample, there are again post-litigation increases in Black reporting rates and decreases in white reporting rates; these treatment effects are now more precisely estimated. The ATT estimates for the average Black reporting rate are 5.3 - 5.6 percentage point or 13 - 14% post-litigation increases relative to the 40% average reporting rate for Black respondents in yet-to-be-treated MSAs ($p < .05$). The ATT estimate for the average white reporting rate is a 2 percentage point or 6% decrease relative to the 34% average reporting rate for white respondents in yet-to-be-treated MSAs ($p < .05$).

The divergent effects of employment discrimination litigation on Black and white reporting rates, which are consistent with the findings in Miller and Segal [2018], provide further support for the hypothesis of reductions in discriminatory law enforcement practices following successful litigation. Black victimization reporting rates increased following litigation onset, while white reporting rates decreased slightly. The increased reporting rates for Black crime victimization following litigation onset may have contributed to decreases in the incidence of that victimization. By contrast, the slight decreases in reporting rates for white crime victimization may have contributed to the small increases in the incidence of that victimization.

As discussed in more detail in Appendix F, post-litigation changes in reporting rates help to reconcile the post-litigation reductions in victimization reported here with the absence of evidence of impacts of litigation on offenses known to law enforcement reported by McCrary [2007]. In the NCVS data, overall post-litigation increases in reporting rates mask overall post-litigation decreases in crime victimization, leading to no changes in post-litigation reported victimization, consistent with the findings of McCrary [2007].

D.4 Reasons for Not Reporting

The NCVS asks respondents who experienced a crime, but who did not report the crime to the police, the reasons they did not report to the police. The most frequently cited reasons for not reporting are separately identified. These are: "police wouldn't help/not important to police,"

"police couldn't do anything," "not important to respondent," "dealt with another way," "insurance wouldn't cover," and "other reason." Of these listed reasons for not reporting, the first two ("police wouldn't help/not important to police"; "police couldn't do anything") reflect a lack of trust in the nature of the police response to reported victimization. The remaining reasons for not reporting are unrelated to trust in the police response.

We distinguish reasons for not reporting victimization into two categories: Not Reporting: Mistrust Police, which is coded as 1 if a victim cited "police wouldn't help/not important to police" or "police couldn't do anything" as a reason for not reporting, and zero otherwise; and Not Reporting: Other Reasons, which is coded as 1 if a victim cited any of the other reasons for not reporting, and zero otherwise. The sample is restricted to those respondents who experienced victimization. Appendix Table G.1 reports pretreatment means in yet-to-be-treated MSAs by respondent race.

Figure 6 reports event study estimates of the effects of litigation leading to affirmative action on changes in not reporting because of mistrust in the police response, separately for Black and non-Hispanic white respondents. For Black respondents, there appears to be a slight positive pretrend in not reporting because of mistrust in the police prior to litigation onset. This pretrend reverses upon litigation onset, and we then see large and increasing decreases in the proportion of Black respondents not reporting crime victimization because of mistrust in the nature of the police response. The average post-litigation decrease in not reporting victimization because of mistrust in the police response, relative to the baseline year, is 13.2 percentage points for Black respondents, or a 47% decrease relative to the baseline rate of 28.1% in yet-to-be-treated MSAs. Not reporting because of mistrust in the police response also decreases for white respondents after litigation onset, but these decreases are smaller than those for Black victims. The average post-litigation decrease in not reporting victimization because of mistrust in the police response is 7.5 percentage points for white respondents, or a 31% decrease relative to the baseline rate of 24.1%. In the interaction event study model, reported in Appendix G, nonreporting of victimization because of mistrust in the police response decreases by 4.3 percentage points more for Black respondents, relative to white respondents, after the onset of litigation leading to affirmative action. These decreases are significant at the 90% threshold in a majority of post-litigation years.

Appendix Figure G.2 reports estimates of the effects of litigation leading to affirmative action on changes in not reporting victimization for reasons other than mistrust in the police response, relative to the baseline year. There are no evident post-litigation decreases in not reporting due to reasons other than mistrust in the police response, for either Black or white victims.

Table 4 reports Callaway and Sant'Anna [2020a] ATT estimates for the effects of litigation onset on the proportions of NCVS respondents who did not report their victimization because of mistrust in the police response, by respondent race. As is evident in Figure 6, the proportion of NCVS respondents who did not report their victimization because of mistrust in the police response decreases for both Black and white respondents after litigation onset. However, these decreases are much larger for Black respondents than for white respondents. In the treated-only sample, the ATT estimate is positive for Black respondents but the confidence interval is very wide. In the sample including all MSAs, the ATT estimate for Black respondents is a 15.9 - 16.1 percentage point or 34% decrease in the proportion of respondents not reporting victimization due to mistrust in the police ($p < .05$). The ATT estimate for white respondents is a 7.8 percentage point or 21% decrease in the proportion of respondents not reporting victimization due to mistrust in the police ($p < .05$); the ATT estimate for the racial gap in not reporting due to mistrust in the police is an 8.2 percentage point post-litigation decrease ($p < .05$). As reported in Appendix Table G.2, we see no post-litigation changes in the proportion of Black respondents who did not report crime victimization for reasons other than mistrust in the police.

These estimates suggest that Black respondents became more confident in the nature of the police response they could expect after reporting, after the onset of litigation leading to affirmative action, both in absolute terms and relative to white respondents.

D.5 Agency Racial Composition

The pattern of observed treatment effects reported above is consistent with an immediate post-litigation increase in the police responsiveness to Black crime victimization, with persistent effects potentially due in part to increasing Black officer shares.

Post-litigation increases in Black officers have been previously reported by McCrary [2007] and Miller and Segal [2012] using a different set of municipalities and different data. We extend this prior work to our sample of MSAs using the Law Enforcement Management and Administrative Statistics (LEMAS) survey, conducted by the Bureau of Justice Statistics periodically since 1987. The LEMAS survey reports demographic personnel data for a sample of local law enforcement agencies, including all agencies that employ 100 or more full-time sworn officers, and a nationally representative sample of smaller agencies. We matched the agencies in our sample to the agencies in the LEMAS survey for the years 1987, 1990, 1993, 1997, 2000, 2003, 2007, and 2013. All agencies in our litigation sample are also represented in the LEMAS survey. We aggregated agency-level LEMAS data on the numbers of Black and white sworn officers and the total numbers of sworn officers to the MSA level, weighting the MSA means by the sizes of the populations served by each agency. For each MSA/year, we constructed the proportions of sworn officers that are Black and white.

Because the LEMAS data are only available as of 1987, they record only posttreatment variation in agency racial composition. To estimate the effects of litigation leading to affirmative action using only these posttreatment data, we constructed the treatment variable $AA\ Duration_{mt}$, which records the number of years a treated MSA has been subjected to litigation leading to an affirmative action plan.

Appendix Figure H.1 reports the scatterplots and bivariate relationships between litigation duration and the proportions of Black and white officers, for the 26 treated MSAs. Litigation duration is positively correlated with the proportion of sworn officers that are Black, and negatively correlated with the proportion of officers that are white.

We then estimate Equation 3 separately for the proportions of sworn officers that are Black and white:

$$Pct\ Black/White\ Officers_{mt} = \alpha + \beta AA\ Duration_{mt} + \lambda AA\ End_{mt} + \theta_m + \gamma t + \epsilon_{mt} \tag{3}$$

Equation 3 includes a covariate $AA\ End_{mt}$, which is 1 if MSA m had an affirmative action plan AA that terminated after year t, and is 0 otherwise. θ_m are MSA fixed effects; γ_t are year fixed effects. Robust standard errors are clustered on the MSA level.

Appendix Table H.1 reports these estimates. Litigation duration has clear effects on agency racial composition. We see an average 0.2 percentage point increase in the proportion of Black officers per year posttreatment, and an average 0.8 percentage point decrease in the proportion of white officers per year posttreatment; both estimates are significant at the 95% confidence level. Over the course of 25 years after the imposition of litigation leading to affirmative action, the average agency sees a five percentage point increase in Black officer shares, a 42% increase in Black officer shares relative to the 1987 baseline rate of 12% Black officer shares. These findings are consistent with those reported by McCrary [2007] and Miller and Segal [2012].

D.6 Other Causal Mechanisms

Finally, we consider a number of other causal mechanisms that may explain the observed post-litigation decreases in both absolute and relative Black victimization. We estimate the effects of litigation leading to affirmative action on numbers of sworn officers per capita (Appendix I), the demographic composition of treated MSAs (Appendix J), and offense clearance rates as reported by the UCR (Appendix K). We find no evidence of any systematic relationships between these potential causal mechanisms and litigation leading to affirmative action. We also explore heterogeneous effects of litigation leading to affirmative action on victimization by type of crime (L). We find no pattern of heterogeneous effects.

E Discussion

Black civilians in the United States are disproportionately subjected to enforcement actions by police officers, including citations, fines, and the use of force, relative to white civilians. Numerous studies have documented that these racial disparities in enforcement actions are due at least in part to racially discriminatory practices by police officers [West, 2018, Hoekstra and Sloan, 2020, Goncalves and Mello, 2021].

Black Americans are also less safe than white Americans, consistently experiencing relatively higher rates of crime victimization. Racial disparities in crime victimization may likewise be due to racially disparate responses to victimization by law enforcement agencies. Police agencies may allow or even encourage officers to exert less effort to detect and deter crime experienced by Black victims, relative to crime experienced by white victims. Alternatively, racial disparities in crime victimization may be due to differences in levels of reporting and cooperation with the police, independently of the nature of the police response.

We identify changes in the police response to crime victimization using idiosyncratic variation in the timing of litigation of law enforcement agencies for race-based discrimination in employment. Leaders of agencies that are litigated for racially discriminatory employment practices may seek to reduce racially discriminatory practices throughout their agencies in an effort to avoid adverse litigation outcomes. After litigation onset, officers may be directed to exert relatively more effort to responding to, clearing, and deterring crime experienced by Black victims, leading to lower Black victimization rates. Over time, employment discrimination litigation leading to affirmative action in law enforcement may also increase the shares of Black police officers, and decrease the shares of white officers. Black officers may care more about detecting and deterring crime experienced by Black victims, relative to white officers, leading to further reductions in Black victimization.

Leveraging the idiosyncratic variation in the timing of litigation of law enforcement agencies for race-based discrimination in employment between 1970 and 1986, we find that successful litigation of police agencies for racially discriminatory employment practices substantially reduced rates of both absolute and relative Black victimization.

We also explore causal mechanisms that could be generating the large post-litigation decreases in both absolute and relative Black victimization. We find that successful litigation of police agencies for racially discriminatory employment practices a) increased reporting of victimization to law enforcement by Black victims, but not by white victims; b) increased victim trust in the expected police response to victimization by both Black and white victims, but more so for Black victims; and c) consistent with earlier work [McCrary, 2007, Miller and Segal, 2012], increased Black officer shares and decreased white officer shares. We find no evidence that increases in the numbers of sworn officers or changes in the demographic characteristics of litigated MSAs are plausible causal mechanisms. We find no significant heterogeneity in effects across crime types.

Prior work has found no effects of the litigation of law enforcement agencies for racially discriminatory employment practices on agency performance [McCrary, 2007]. The estimates reported here indicate that, once we take into account increases in reporting induced by litigation of law enforcement agencies for racially discriminatory employment practices, we find that litigation in fact reduces both absolute and relative Black crime victimization.

Since Richmond v. Croson (1989), the Supreme Court has held that affirmative action plans in public sector hiring must survive strict scrutiny, and will be invalidated by courts unless they serve a "compelling" governmental interest. One compelling governmental interest is the performance of an agency's core function [Lott, 2000]. The core function of law enforcement agencies is to provide protection from crime victimization. The estimates reported here indicate that litigation leading to affirmative action in law enforcement leads to net decreases in crime victimization, or to net increases in agencies' performance of their core function. These findings may provide renewed support for intervention into the operations of law enforcement agencies engaging in racially discriminatory practices.

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Tables and Figures

Table 1: Descriptive Statistics
Never-Treated and Treated MSAs Pretreatment
1979-1985

	Never-Treated 1979-1985		Treated Pretreatment 1979-1985	
	White	Black	White	Black
Avg # Respondents/Yr	1,129	82	2,205	462
Proportion White/Black	0.80	0.06	0.83	0.12
Homeownership	0.70	0.55	0.81	0.61
Single Family Home	0.74	0.71	0.83	0.63
Household Income 30K+	0.26	0.11	0.28	0.12
Some College	0.44	0.29	0.35	0.22
Age 18-29	0.26	0.30	0.25	0.27
Married	0.56	0.39	0.56	0.34
Female	0.53	0.53	0.52	0.55
Victimization Rate	0.14	0.15	0.13	0.20
Reporting Rate	0.35	0.34	0.34	0.40
Reported Victimization	0.05	0.05	0.05	0.08
N	86,909	6,304	49,623	7,399

Cells report NCVS means between 1979-1985 for all never-treated MSAS, and for treated MSAs during pretreatment years only, by race of respondent.

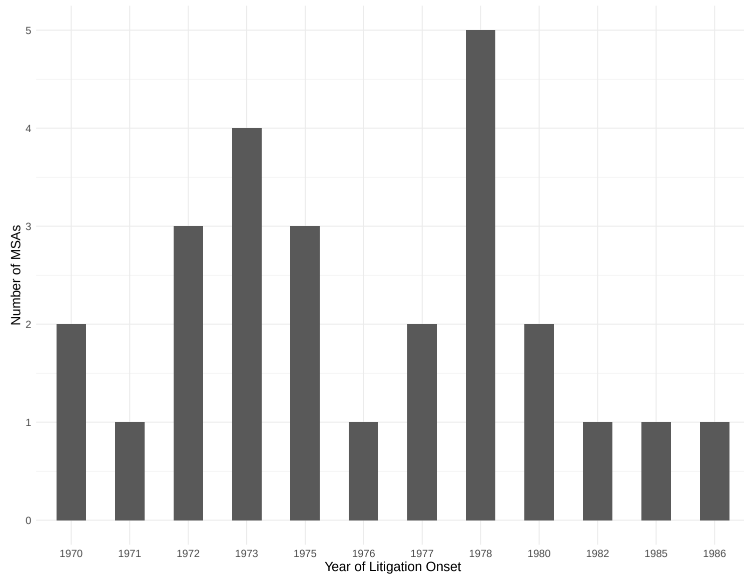
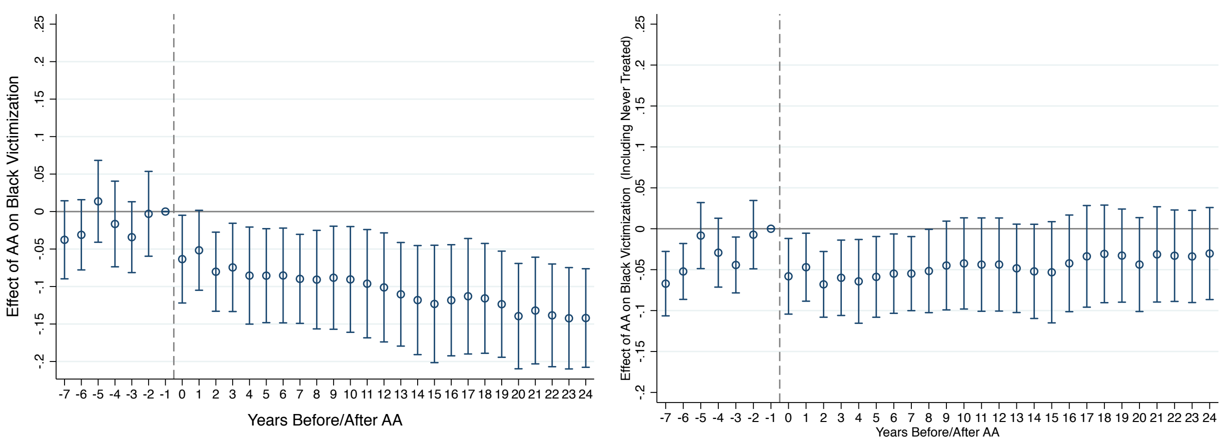


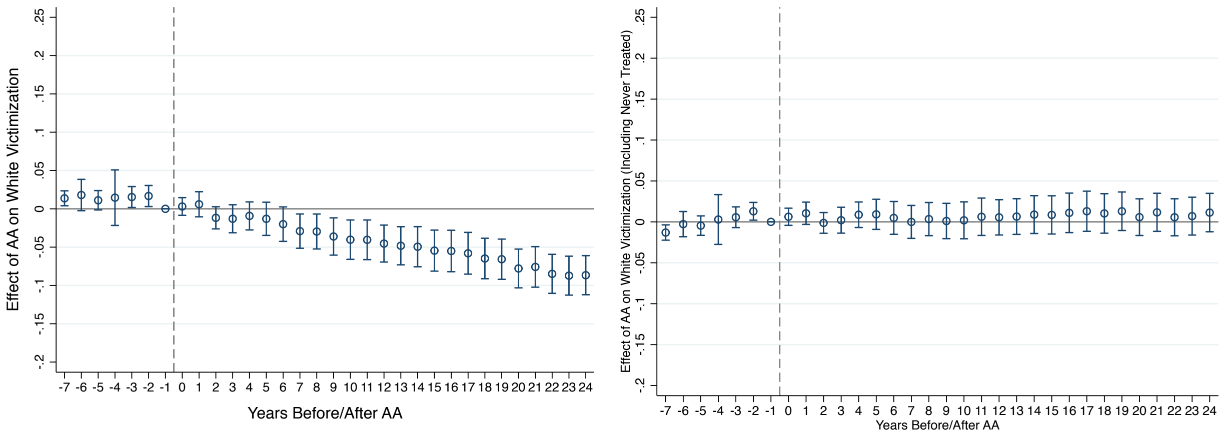
Figure 1: Timing of Litigation Onset in Treated MSAs, 1970-1986



(a) Treated-Only MSAs

(b) All MSAs

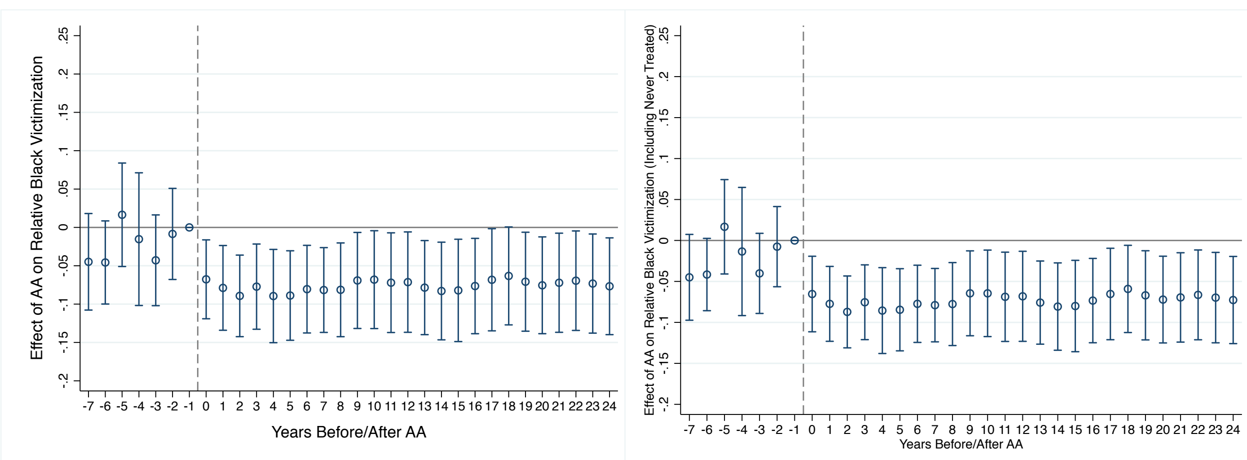
Figure 2: Estimated Effects of Litigation Leading to Affirmative Action on Changes in Black Victimization Rates, 1979-2004



(a) Treated Only MSAs

(b) All MSAs

Figure 3: Estimated Effects of Litigation Leading to Affirmative Action on Changes in White Victimization Rates, 1979-2004



(a) Treated Only MSAs

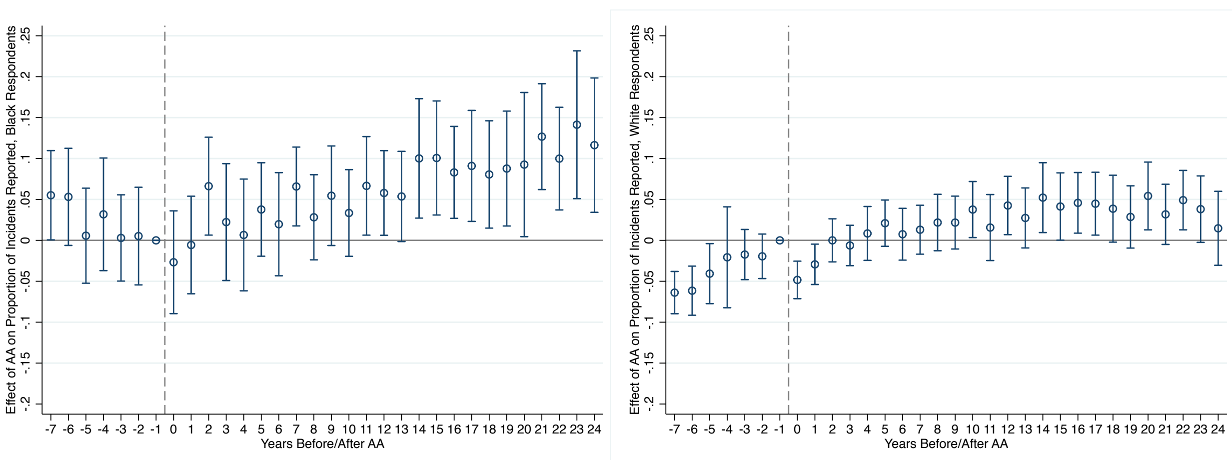
(b) All MSAs

Figure 4: Estimated Effects of Litigation Leading to Affirmative Action on Changes in Racial Disparities in Victimization Rates, 1979-2004

Table 2: Callaway and Sant'Anna [2020a] ATT Estimates
Crime Victimization Rates

	Black Victimization	White Victimization	Racial Gap in Victimization
Treated MSAs	-0.055**	0.008	-0.062**
	(0.016)	(0.012)	(0.019)
N	670	670	670
All MSAs	-0.017**	0.012**	-0.029**
Control = Never-Treated	(0.008)	(0.004)	(0.006)
N	1,032	1,032	1,032
All MSAs	-0.018**	0.012**	-0.03**
Control = Never + Not Yet Treated	(0.008)	(0.006)	(0.01)
N	1,032	1,032	1,032
Pretreatment DV Mean	0.203	0.135	0.068

* p<.10, ** p<.05, *** p<.01. Standard errors clustered on MSA. Callaway B. and Sant'Anna P. (2020), "did: Difference in Differences," R package version 2.0.0, https://bcallaway11.github.io/did/.



(a) Black Respondents

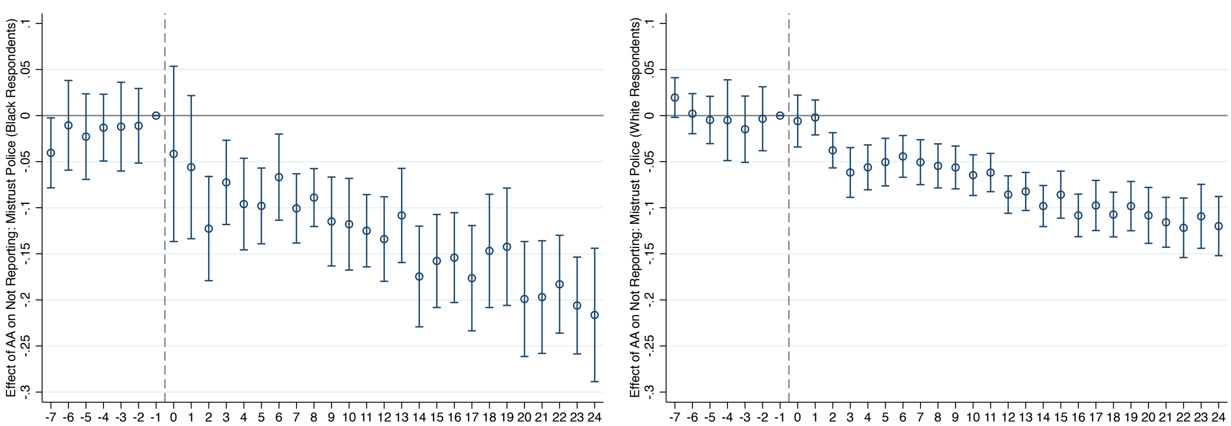
(b) White Respondents

Figure 5: Estimated Effects of Litigation Leading to Affirmative Action on Changes in Black and White Victimization Reporting Rates, 1979-2004

Table 3: Callaway and Sant'Anna [2020a] ATT Estimates
Victimization Reporting Rates

	Black Reporting Rate	White Reporting Rate
Treated MSAs	0.290	-0.080**
	(0.257)	(0.031)
N	670	670
All MSAs	0.056**	-0.020**
Control = Never + Not Yet Treated	(0.019)	(0.009)
N	1,032	1,032
All MSAs	0.053**	-0.020**
Control = Never-Treated	(0.019)	(0.010)
N	1,032	1,032
Pretreatment DV Mean	0.400	0.338

* p<.10, ** p<.05, *** p<.01. Standard errors clustered on MSA. Callaway B. and Sant'Anna P. (2020), "did: Difference in Differences," R package version 2.0.0, https://bcallaway11.github.io/did/.



(a) Black Respondents

(b) White Respondents

Figure 6: Estimated Effects of Litigation Leading to Affirmative Action on Changes in Not Reporting: Mistrust Police, 1979-2004

Table 4: Callaway and Sant'Anna [2020a] ATT Estimates
Not Reporting Due to Mistrust in Police

	Black Respondents	White Respondents
Treated MSAs	0.025	-0.066**
	(0.169)	(0.017)
N	670	670
All MSAs	-0.161**	-0.078**
Control = Never + Not Yet Treated	(0.036)	(0.015)
N	1,032	1,032
All MSAs	-0.159**	-0.078**
Control = Never-Treated	(0.014)	(0.010)
N	1,032	1,032
Pretreatment DV Mean	0.468	0.364

* p<.10, ** p<.05, *** p<.01. Standard errors clustered on MSA. Sample comprises respondents who did not report victimization to the police. Callaway B. and Sant'Anna P. (2020), "did: Difference in Differences," R package version 2.0.0, https://bcallaway11.github.io/did/.

A Additional Figures and Tables

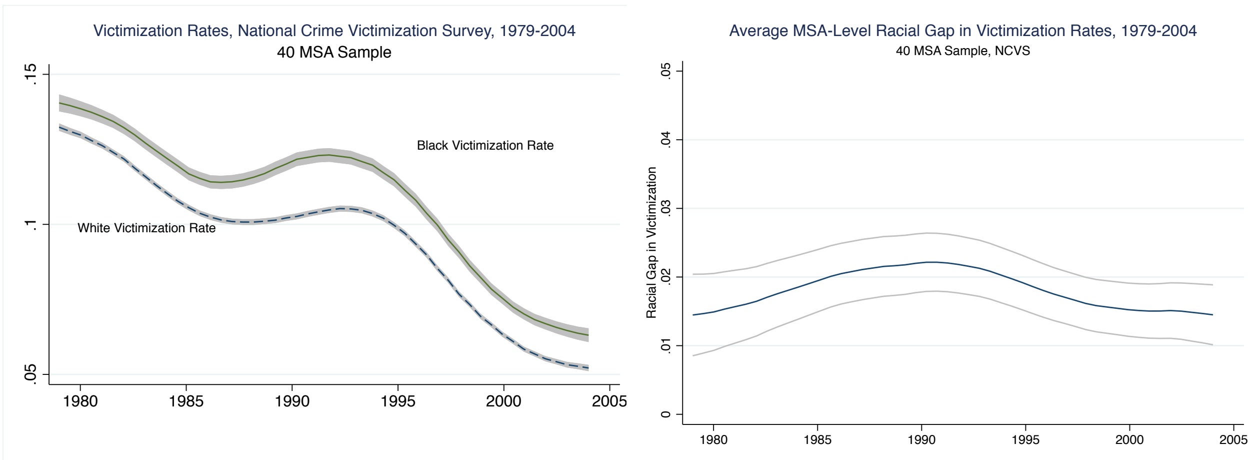


Figure A.1

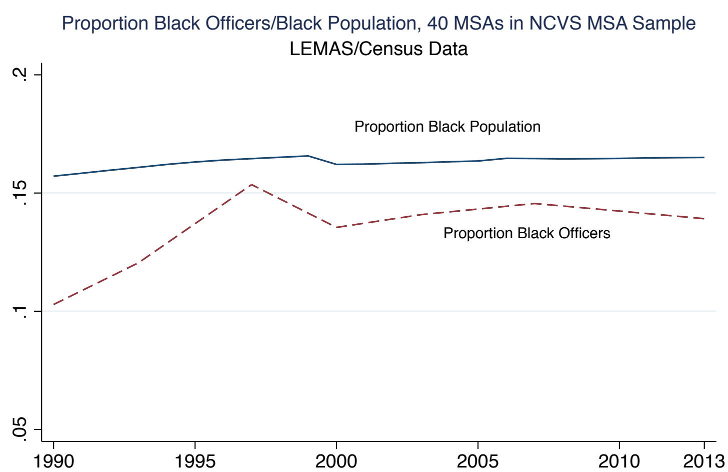


Figure A.2

B Construction of NCVS Sample

The NCVS data used here include respondents from "core counties" within the forty MSAs included in the sample. The 50,000 housing units included in the NCVS are selected using a stratified, multi-stage cluster design. The Primary Sampling Units (PSU's) composing the first stage of the sample are counties, groups of counties, or large metropolitan areas. Large PSU's were included in the sample automatically, and each large PSU was assigned to its own stratum. These PSU's are considered to be self-representing (SR) since all of them were selected. The remaining PSU's, called non-self-representing (NSR) because only a subset of them were selected, were combined into strata by grouping PSU's with similar geographic and demographic characteristics, as determined by the Census, to design the sample. The design consists of 84 SR PSU's and 153 NSR strata [United States Department of Justice. Office of Justice Programs. Bureau of Justice Statistics, 2007, pg. 8].

Housing units within a PSU are selected into the sample in two stages. The stages were designed such that, prior to any weighting adjustments, each sample housing unit had the same probability of being selected. The first stage involved selecting a sample of Enumeration Districts (ED's), geographic areas established for each decennial Census encompassing a population of 750 to 1,500 persons, from designated PSU's. ED's were systematically selected proportionate to their 1980 or 1990 population size. In the second stage, each selected ED was divided into segments using clusters of about four housing units each, and a sample of segments was selected. From this sample was compiled a list of addresses recorded during the 1980 and 1990 Censuses [United States Department of Justice. Office of Justice Programs. Bureau of Justice Statistics, 2007, pg. 8-9].

Beginning in 1992, the NCVS categorized crime as personal or "property" covering the personal crimes of rape and sexual attack, robbery, aggravated and simple assault and purse-snatching/pick-pocketing; and the property crimes of burglary, theft, motor vehicle theft, and vandalism. [United States Department of Justice. Office of Justice Programs. Bureau of Justice Statistics, 2007, pg. 6]. Therefore, in 1992 and 1993, the sample was split in half. Fifty percent of sample households were assigned to receive the continuing questionnaire and the other half was revised to the new questionnaire. The half-samples were designed to be as comparable as possible in terms of crime statistics. The 50-50 split with an 18-month overlap was designed to permit comparative analyses between both methods, to provide a basis for measuring the impact of the new methods on crime rates, and to lay the foundation for statistical adjustments to connect the historical and the new time series of crime rates [United States Department of Justice. Office of Justice Programs. Bureau of Justice Statistics, 2007, pg. 9].

C Treatment Exogeneity

We estimate the probability of treatment using Equation 4:

$$Treatment_i = \alpha + \beta \boldsymbol{Pretreatment}_i + \epsilon_i \tag{4}$$

where the vector $\boldsymbol{Pretreatment}_i$ includes pretreatment demographic variables sourced from the 1970 census and an indicator for whether an MSA experienced a riot between 1961 and 1968

[Spilerman, 1970, McCrary, 2007]. We estimate the timing of treatment using Equation 5:

$$Treatment\ Year_i = \alpha + \beta \boldsymbol{Pretreatment}_i + \epsilon_i \tag{5}$$

using only those agencies that will eventually be subjected to treatment, where the vector $\boldsymbol{Pretreatment}_i$ contains the same covariates included in Equation 4. We report estimates at three levels of aggregation: agency, county, and MSA. Standard errors are clustered on MSA for estimates reported at the agency and county level.

Table C.1 reports the estimates. Law enforcement agencies that would eventually be litigated for racially discriminatory employment practices are located in counties with larger percentages of Black residents (p < .05 for agency, county, and MSA specifications), lower median years of schooling (p < .10 for agency and county specifications), and higher median family incomes (p < .05 for agency, county, and MSA specifications). By contrast, no covariates are significant in the models restricted to treated locations. These estimates are consistent with those of McCrary [2007], who found that differences across cities litigated at different times were much smaller than differences across litigated and unlitigated cities.

Table C.1: Predicting Treatment and Treatment Timing

	Treatment			Treatment Timing		
	Agency	County	MSA	Agency	County	MSA
Log Population	0.09	0.06	0.11	0.35	-0.05	-0.04
	(0.07)	(0.07)	(0.11)	(0.68)	(0.65)	(1.44)
Pct Black	0.02**	0.02***	0.02**	-0.10	-0.03	-0.11
	(0.01)	(0.01)	(0.01)	(0.11)	(0.09)	(0.25)
Median Age	0.01	-0.00	0.01	-0.23	-0.20	-0.17
	(0.02)	(0.02)	(0.03)	(0.18)	(0.18)	(0.28)
Median Yrs School	-0.18**	-0.16*	-0.07	1.10	0.76	-0.11
	(0.08)	(0.08)	(0.17)	(1.17)	(0.92)	(3.04)
Median Fam Income	0.11**	0.14***	0.14**	-0.98	-0.41	-0.46
	(0.05)	(0.04)	(0.06)	(0.72)	(0.65)	(0.98)
Pct Urban	-0.00	-0.00	0.00	0.03	0.02	0.10
	(0.00)	(0.00)	(0.01)	(0.05)	(0.04)	(0.15)
Riot 1961-1968	0.13	0.03	0.15	-1.63	-1.62	-1.03
	(0.21)	(0.18)	(0.20)	(1.79)	(1.38)	(2.08)
N	149	81	37	108	60	26

* p<.10, ** p<.05, *** p<.01. Median family income in 1,000s. Standard errors clustered on MSA for agency and county models.

We also estimate models using the NCVS sample described in Table 1, both with and without interactions with race of respondent, predicting a) whether an MSA would experience treatment, and b) conditional on an MSA experiencing treatment, the year of treatment. We cluster standard errors on the MSA. Table C.2 reports these estimates.

There are several correlations between respondent-level pretreatment covariates and whether an MSA will experience litigation leading to affirmative action in law enforcement. MSAs are more likely to experience the latter if they have more respondents who: own homes, are white and aged 18-29, are Black and do not own a home or live in a single family home, have household income below $30,000, are older, have no college, and are not married, are Black and female, and are Black and have experienced victimization. MSAs are less likely to experience litigation leading to affirmative action in law enforcement if they have more respondents who have some college, are Black and own a single family home, and are Black and aged 18-29. These correlations disappear, however, for the models predicting treatment timing.

Table C.2: Predicting Treatment and Timing of Litigation Leading to Affirmative Action NCVS Sample, Respondent Level

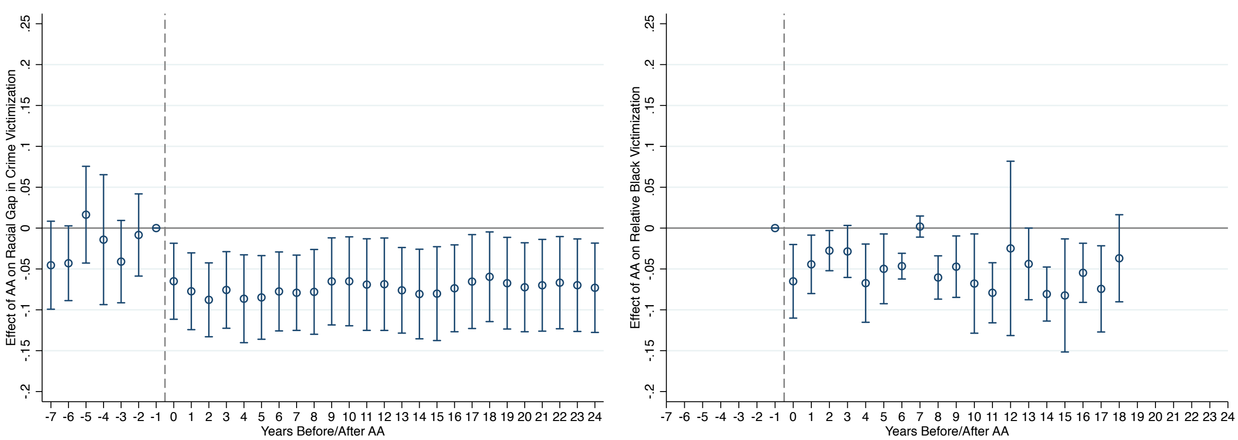
	DV: Treatment		DV: Treatment Timing	
Black	0.21	0.34*	0.81	1.33
	(0.13)	(0.16)	(0.56)	(0.75)
Homeownership	0.13**	0.12**	-0.19	-0.19
	(0.05)	(0.05)	(0.25)	(0.25)
Single Family Home	0.02	0.05	0.55	0.68
	(0.04)	(0.05)	(0.36)	(0.40)
Age 18-29	0.02**	0.03***	-0.05	-0.04
	(0.01)	(0.01)	(0.04)	(0.04)
Household Income 30K +	0.02	0.01	0.50	0.49
	(0.06)	0.05)	(0.27)	(0.26)
Some College	-0.07**	-0.07**	-0.07	-0.07
	(0.03)	(0.03)	(0.05)	(0.05)
Married	-0.02	-0.01	-0.04	-0.03
	(0.01)	(0.01)	(0.05)	(0.04)
Female	-0.01*	0.01**	0.01	0.00
	(0.00)	(0.00)	(0.01)	(0.02)
Victimization	0.02	0.01	0.08	0.08
	(0.02)	(0.02)	(0.05)	(0.05)
Black X Homeownership		0.08		-0.01
		(0.08)		(0.24)
Black X Single Family Home		-0.27***		-0.70
		(0.04)		(0.40)
Black X Age 18-29		-0.05**		-0.04
		(0.03)		(0.09)
Black X Household Income 30K+		0.04		-0.07
		(0.03)		(0.32)
Black X Some College		-0.03		0.01
		(0.03)		(0.09)
Black X Married		-0.04		-0.14
		(0.03)		(0.16)
Black X Female		0.03***		0.02
		(0.01)		(0.05)
Black X Victimization		0.08***		0.07
		(0.02)		(0.15)
Constant	0.26*	0.24*	1984.51***	1984.38***
	(0.13)	(0.12)	(1.01)	(1.02)
N	167664	167664	59554	59554
Joint F-Statistic	19.78	232.23	3.79	1.63
P-Value for Joint F-Statistic	0.00	0.00	0.11	0.32

* p<.10, ** p<.05, *** p<.01. Standard errors clustered on MSA.

D Event Study Extensions

Figure D.1, Panel (a) replicates the interaction estimates from Equation 2 for the treated-only sample, but includes a vector of time-varying respondent-level covariates from Table 1, including homeownership, residence in a single-family home, annual household income above \$30,000, age between 18 and 29, marital status, female, and whether the respondent has some years of college education. Estimates are of nearly identical magnitudes but are somewhat more precisely estimated after the inclusion of covariates.

Figure D.1, Panel (b) replicates the interaction estimates from Equation 2, but restricts the sample to a balanced panel of five treated MSAs and 20 years (one pretreatment year, 19 post-treatment years) for which NCVS data are available in all periods for all treated MSAs. Covariates are included. The estimates, while noisier than those reported for the full unbalanced treated-only sample, continue to indicate that racial disparities in victimization dropped immediately after litigation. The average of the post-litigation event study estimates indicates a 5.2 percentage point reduction in the racial gap in victimization rates in the balanced panel.

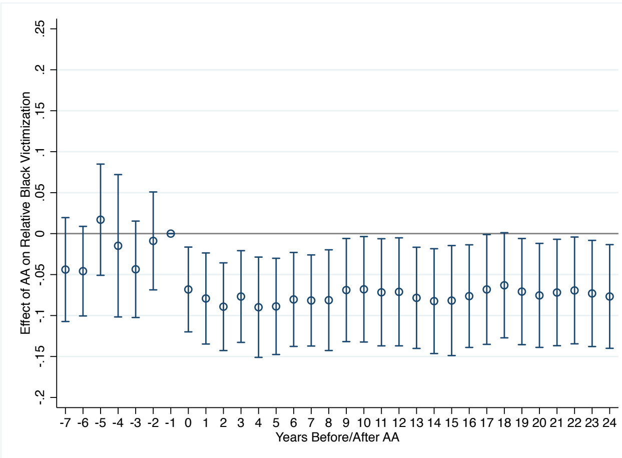


(a) Treated-Only; Including Covariates

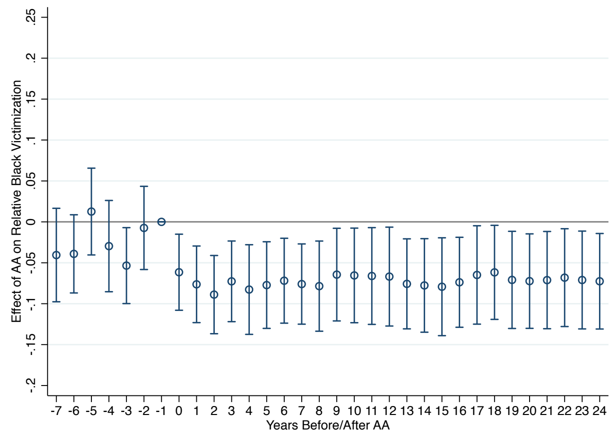
(b) Balanced Panel; Including Covariates

Figure D.1: Estimated Effects of Litigation Leading to Affirmative Action on Changes in the Racial Gap in Victimization, 1979-2004

Figure D.2, Panel (a) reports estimates of the effects of litigation leading to affirmative action on changes in the racial gap in victimization rates in the treated-only sample, including year-by-month fixed effects. Figure D.2, Panel (b) reports estimates of the effects of litigation leading to affirmative action on changes in the racial gap in victimization rates in the treated-only sample, including year-by-month-by-MSA fixed effects. Results are qualitatively unchanged.



(a) Incl. Covariates, Year x Month FE

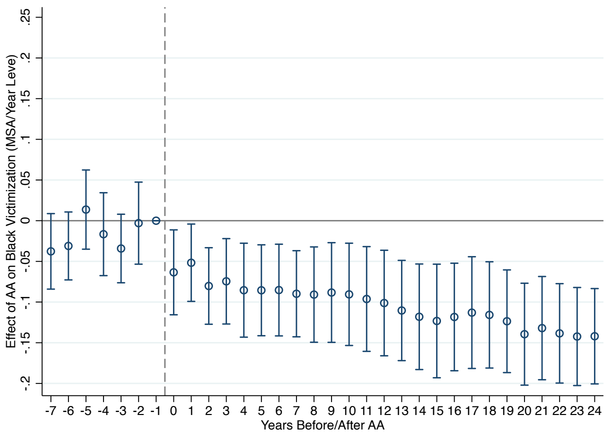


(b) Incl. Covariates, Year x Month x MSA FE

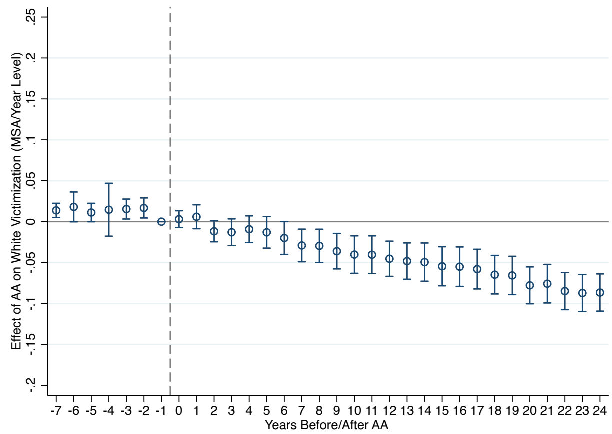
Figure D.2: Estimated Effects of Litigation Leading to Affirmative Action on Changes in the Racial Gap in Victimization, 1979-2004
Treated-Only Sample

Figures D.3 and D.4 report event study estimates for the treated-only MSAs after collapsing the respondent-level data to the MSA/year level.[24] Post-litigation decreases in Black victimization are increasing over time, and are significant at the 95% level immediately after litigation onset. We see an average post-litigation decrease in Black victimization of 10.4 percentage points, similar to that observed using the respondent-level data. Post-litigation decreases in white victimization are also increasing over time, and are significant at the 95% level approximately 8 years after litigation onset. We see an average post-litigation decrease in white victimization of 4.3 percentage points, similar to that observed using the respondent-level data.

[24]MSA-level estimates are weighted by the number of respondents per MSA/year.



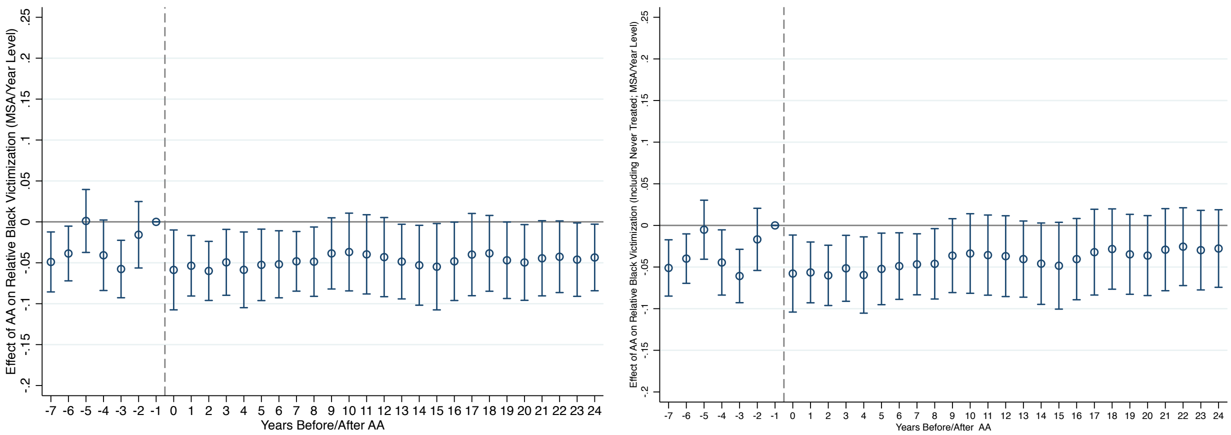
(a) Black Respondents



(b) White Respondents

Figure D.3: Estimated Effects of Litigation Leading to Affirmative Action on
Changes in Black and White Victimization, 1979-2004
Treated-Only Sample; MSA/Year Level

Figure D.4 reports estimates of the effects of litigation leading to affirmative action on changes in the racial gap in victimization after collapsing data to the MSA/year level, for both the treated-only and the all-MSA samples. We weight the MSA/year level estimates by the numbers of respondents per MSA/year. In Panel (a), we see an average post-litigation decrease in the racial gap in victimization of 4.8 percentage points in the treated-only sample, a somewhat smaller average decrease than that observed using the respondent-level data. Post-litigation decreases in the racial gap in victimization are relatively stable over time, and are generally significant at the 90% threshold. In Panel (b), estimates of post-litigation changes in the racial gap in victimization for the all-MSA sample are consistently negative and are significant or close to significance at conventional thresholds. The average post-litigation decrease in the racial gap in victimization in this sample is 4.2 percentage points.

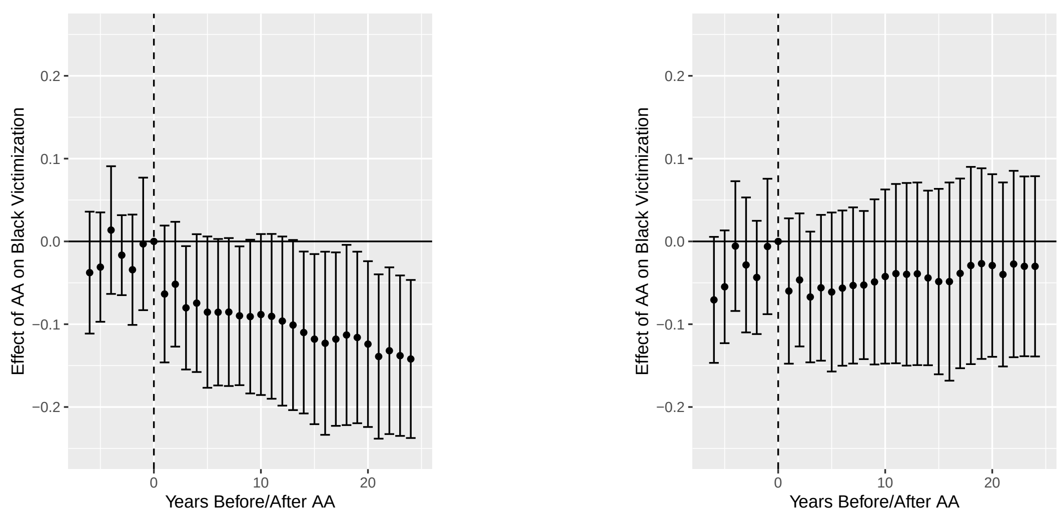


(a) Treated-Only MSAs

(b) All MSAs

Figure D.4: Estimated Effects of Litigation Leading to Affirmative Action on Changes in the Racial Gap in Victimization, 1979-2004; MSA/Year Level

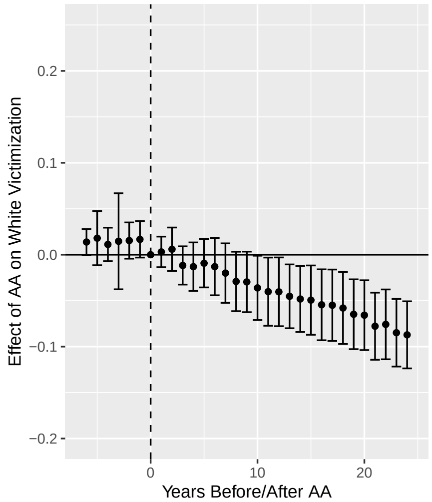
Finally, we report event study results using uniform rather than pointwise confidence intervals [Olea and Plagborg-Møller, 2019, Freyaldenhoven et al., 2018]. Because uniform confidence intervals are designed to contain the true path of the coefficients 95% of the time, they are useful for assessing the presence of pretrends in the data. We report results for white, Black, and relative victimization rates for both the treated-only and all-MSA samples. There is no evidence that either absolute or relative Black victimization were trending downward prior to litigation onset.



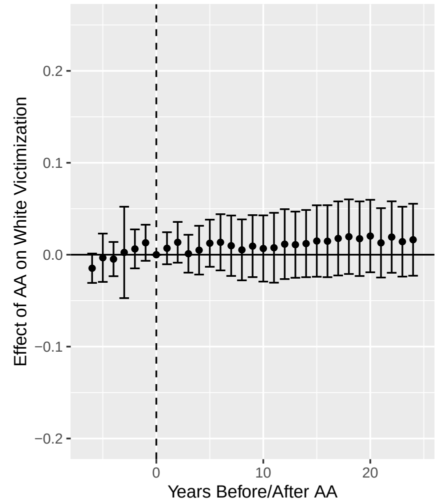
(a) Treated-Only MSAs

(b) All MSAs

Figure D.5: Estimated Effects of Litigation Leading to Affirmative Action on Changes in Black Victimization, 1979-2004
Uniform Confidence Intervals

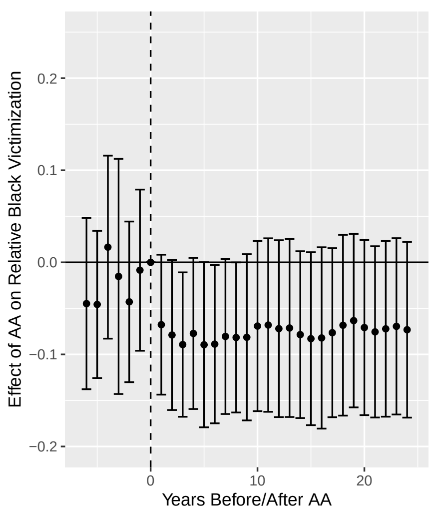


(a) Treated-Only MSAs

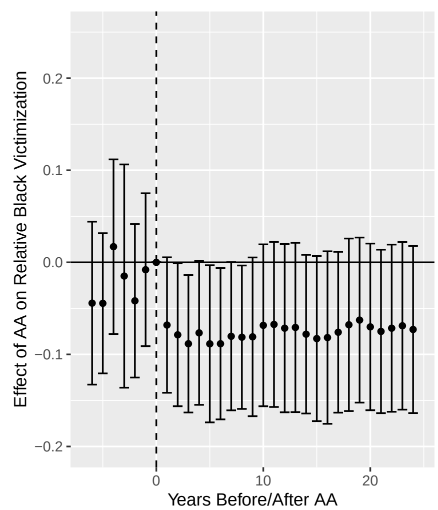


(b) All MSAs

Figure D.6: Estimated Effects of Litigation Leading to Affirmative Action on Changes in White Victimization, 1979-2004
Uniform Confidence Intervals



(a) Treated-Only MSAs



(b) All MSAs

Figure D.7: Estimated Effects of Litigation Leading to Affirmative Action on Changes in Relative Black Victimization, 1979-2004
Uniform Confidence Intervals

E TWFE DD Estimates and Decomposition

We implement the diagnostic developed by Goodman-Bacon [2021] to detect bias in the two-way fixed effect difference-in-differences (TWFE DD) estimator. The TWFE DD estimator estimates the variance-weighted average treatment effect on the treated (VWATT), aggregated from a series of DD estimates derived from 2 x 2 treatment/control groups, which themselves compare units treated at the same time to units treated at another time (earlier or later) and to never-treated units. The VWATT can differ substantially from the true ATT in the presence of staggered treatment timing and treatment effect heterogeneity [Borusyak and Jaravel, 2020, Callaway and Sant'Anna, 2020a, Goodman-Bacon, 2021, Sun and Abraham, 2020].

To implement the Goodman-Bacon [2021] diagnostic we collapse the data to the MSA/year level. We first estimate the VWATT for both Black and white crime victimization, and for the racial gap in victimization, using Equation 6:

$$Victimization_{mt} = \beta_y Post\text{-}AA_t + \boldsymbol{\beta_t} + \boldsymbol{\beta_m} + \epsilon_{mt} \tag{6}$$

In Equation 6, the indicator variables $I(t - t^*_m = y)$ from Equation 1 have been replaced by a single indicator variable denoting whether a treated MSA was posttreatment in year t ($Post\text{-}AA_t = 1$) or not ($Post\text{-}AA_t = 0$). For all TWFE DD models we estimate a linear probability model and cluster standard errors at the MSA level. Estimates are weighted by the number of respondents in each MSA/year. We report estimates for treated-only and all MSAs.

Table E.1 reports these estimates, along with the DD estimates and weights for the categories of treatment/control comparison groups from which the TWFE DD estimates are derived for the treated-only sample.[25]

The TWFE DD estimates using NCVS data collapsed to MSA/year means are comparable albeit slightly smaller in magnitude, relative to the averaged posttreatment event study coefficients. We see no significant posttreatment changes in non-Hispanic white victimization rates in any sample. Estimated posttreatment decreases in Black victimization are 3.6 - 3.7 percentage points in all samples ($p < .01$), an 18% decrease relative to the pretreatment Black victimization rate in treated MSAs of 20.3%. Estimated posttreatment decreases in the racial gap in victimization are 2.3 - 2.4 percentage points in all samples ($p < .01$), a 35% decrease relative to the pretreatment racial gap in victimization in treated MSAs of 6.8%.

The decomposition weights and point estimates reported in Table E.1 for the treated-only sample reveal that almost all of the weight in the TWFE DD estimates is being placed on comparisons between MSAs with agencies subjected to litigation leading to affirmative action before 1979 (always treated), and those subjected to litigation leading to affirmative action after 1978 (timing

[25] For the decomposition estimates we use the *bacondecomp* Stata 16 package developed by Goodman-Bacon et al. [2019]. To enable the use of weights in the *bacondecomp* package we include a covariate for whether an affirmative action plan has terminated.

Table E.1: TWFE DD Estimates, 1979-2004
MSA/Year Level

	White Victimization	Black Victimization	Racial Gap in Victimization
		Treated MSAs Only	
Post-AA	0.000	-0.037***	-0.024***
	(0.012)	(0.011)	(0.008)
Constant	0.131***	0.169***	0.031***
	(0.01)	(0.009)	(0.006)
N	670	670	670
		All MSAs	
Post-AA	0.002	-0.036***	-0.023***
	(0.011)	(0.011)	(0.008)
Constant	0.138***	0.174***	0.032***
	(0.009)	(0.009)	(0.006)
N	1,032	1,032	1,032
	DD Decomposition Estimates (Treated MSAs)		
	Beta/Weight	Beta/Weight	Beta/Weight
Timing Groups	-0.013/0.07	-0.06/0.03	-0.04/0.03
Always v. Timing	0.003/0.92	-0.04/0.95	-0.02/0.95
Within	0.029/0.011	-0.06/0.02	-0.06/0.02

* p<.10, ** p<.05, *** p<.01. Population-weighted OLS; standard errors clustered on MSA. All models include MSA and year fixed effects.

groups). Much smaller weight is given to comparisons between MSAs subjected to litigation leading to affirmative action between 1979 and 1986 (the timing group only comparisons). The DD estimates for the effects of litigation on both Black victimization and the racial gap in victimization for the always treated v. timing group comparisons are smaller in magnitude than the estimates for the timing group comparisons, indicating potential downward bias in the TWFE DD estimates. However, all subgroup DD estimates for these outcomes are negative, including the "within" group estimates (estimates derived from comparisons between MSAs in the same timing group but with different values on the covariate of whether and when a court-imposed affirmative action plan ended). The consistently negative DD subgroup estimates for both Black crime victimization and the racial gap in crime victimization indicate that sign reversal in the TWFE DD estimates is likely not an issue. By contrast, DD estimates for the effect of litigation on white crime victimization are of smaller magnitude and are inconsistently signed across subgroups.

E.1 Callaway and Sant'Anna [2020a] ATT Estimator

The Callaway and Sant'Anna [2020a] ATT estimator is defined for T periods, where $t = 1, ..., T$, and D_{it} is a binary variable equal to 1 if a unit is treated and 0 otherwise. G_g is defined as a binary variable equal to 1 when a unit is first treated in period g, and C is a binary variable equal to 1 for never-treated units. For each unit, exactly one of $\{G_1, ..., G_T\}$ or C is equal to 1. The generalized probability score is defined as $p_g(X) = P(G_g = 1|X, G_g + C = 1)$, which is the probability that a unit is treated conditional on having covariates X and on being a member of a group g or a control group C.

Timing group/time average treatment effects are defined as:

$$ATT(g,t) = \mathbb{E}\left[\left(\frac{G_g}{\mathbb{E}[G_g]} - \frac{\frac{p_g(X)C}{1-p_g(X)}}{\mathbb{E}\left[\frac{p_g(X)C}{1-p_g(X)}\right]}\right)(Y_t - T_{g-1})\right] \tag{7}$$

Timing group average treatment effects are defined as:

$$\theta_S(g) = \frac{1}{\tau - g + 1}\sum_{t=2}^{\tau} 1\{g \leq t\} ATT(g,t) \tag{8}$$

This parameter reports treatment effect heterogeneity with respect to treatment adoption period. Callaway and Sant'Anna [2020a] then construct θ_S^O, which is the overall average treatment effect on the treated, as:

$$\theta_S^O = \sum_{g=2}^{\tau} \theta_S(g) P(G = g) \tag{9}$$

E.2 Placebo Litigation Years

We use a series of TWFE DD models to estimate the effects of placebo treatments on the 14 MSAs containing no law enforcement agencies that were subjected to litigation leading to affirmative action plans. We assign a placebo litigation year between 1970 and 1986, with replacement, to each never-treated MSA; we iterate this random assignment 10 times. For each set of randomly assigned placebo years, we estimate a TWFE DD model that interacts race of respondent with the posttreatment indicator. Figure E.1 reports the coefficients on the interaction terms; none are significant at conventional thresholds.

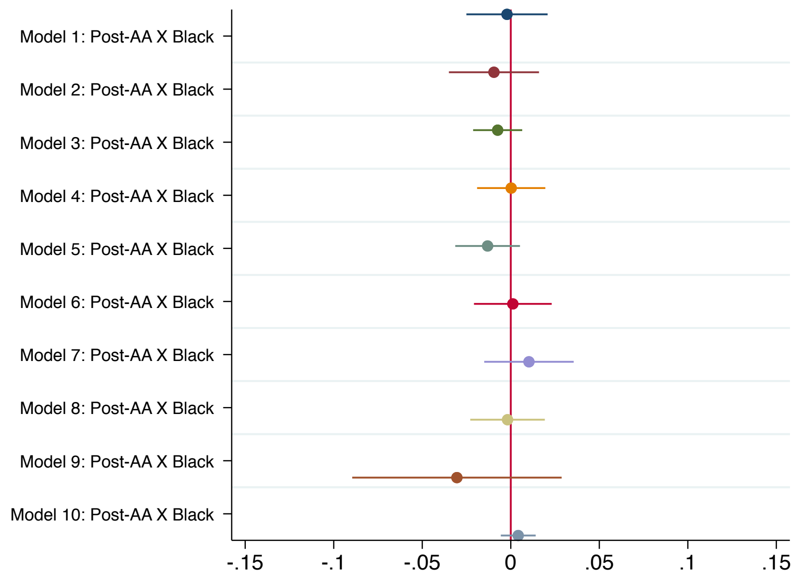


Figure E.1: TWFE DD Estimates of Effects of Placebo Litigation Years on Relative Black Victimization; Never-Treated MSAs; 1979-2004

F Reporting Rates

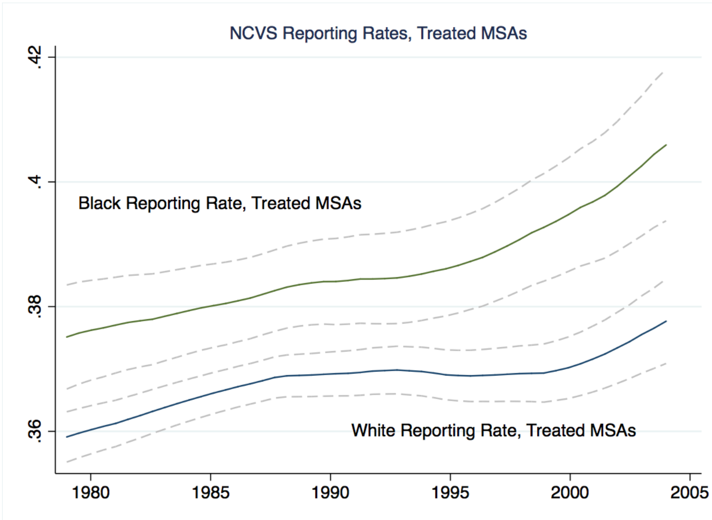
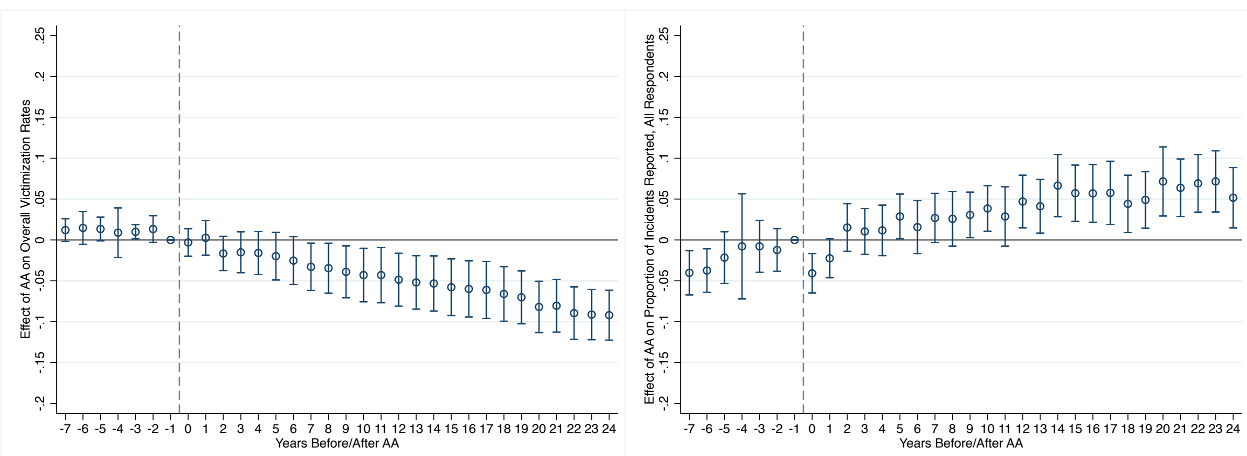


Figure F.1: Reporting Rates by Race in Treated MSAs, NCVS 1979-2004

The post-litigation changes in reporting rates reported in the text help to reconcile the substantial evidence of negative impacts from litigation on crime victimization reported here with the absence of evidence of impacts of litigation on offenses known to law enforcement reported by McCrary [2007]. As reported in Figure F.2 using the treated-only sample, overall crime victimization rates decreased after the onset of litigation leading to affirmative action, while overall reporting rates increased.

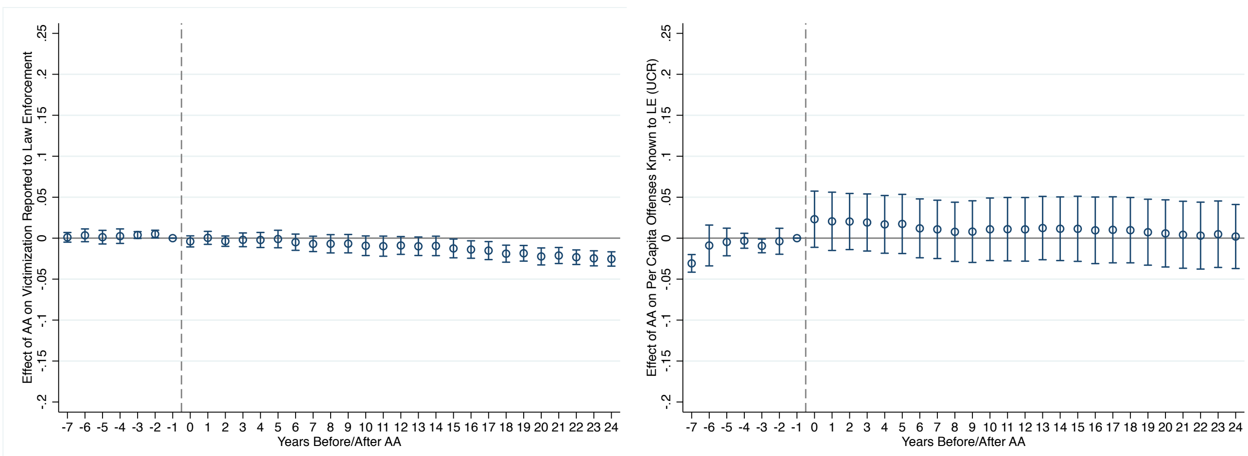


(a) Victimization, All Respondents

(b) Reporting, All Respondents

Figure F.2: Estimated Effects of Litigation Leading to Affirmative Action on Changes in Overall Victimization and Reporting Rates, 1979-2004
Treated-Only Sample

As reported in the first plot of Figure F.3 for the treated-only sample, these trends largely offset each other for victimization reported to law enforcement. This plot reports estimates of the effects of litigation leading to affirmative action on changes in crime victimization *reported to law enforcement*, using data aggregated to the MSA/year level to enable comparisons with aggregated administrative data reported by the FBI's Uniform Crime Reporting program (UCR). The outcome in the event study model used for the first plot in Figure F.3 is the proportion of respondents in an MSA/year who experienced a victimization and reported that victimization to law enforcement. These estimates are indistinguishable from zero for approximately 15 years after the imposition of litigation leading to affirmative action, and are negative and significant but small in magnitude after that. The average of the post-litigation event study coefficients is a decrease in reported victimization of approximately one percentage point.



(a) NCVS Reported Victimization

(b) UCR Reported Offenses

Figure F.3: Estimated Effects of Litigation Leading to Affirmative Action on Changes in Reported Victimization (NCVS) and Offenses Known to Law Enforcement (UCR), 1979-2004

The second plot in Figure F.3 reports estimates of the effects of litigation leading to affirmative action on changes in offenses known to law enforcement, using the FBI s Uniform Crime Reporting (UCR) data for agencies located in the core counties of the 26 treated MSAs. The FBI's UCR program is voluntary; not all agencies participate, or participate consistently, in the program. Offenses known to law enforcement in the UCR data include homicide, rape, robbery, aggravated assault, burglary, theft, and motor vehicle theft. We eliminate the homicide counts in order to better approximate the offenses that could have been reported to law enforcement by victims. We match the 108 agencies located in our sample of 26 treated MSAs to the agencies reporting monthly offense data in the UCR, as cleaned and aggregated to the yearly level by Kaplan [2019]. 87 of these agencies consistently report UCR data.[26] We aggregated total offenses and population served from

[26] Agencies that report zero crimes in any year are treated as missing data in that year. We interpolated isolated

the agency level to the MSA level, and constructed per capita offenses known to law enforcement at the MSA/year level from these aggregated data.

The UCR data are potentially subject to multiple sources of systematic measurement error that may bias estimates of the effects of litigation leading to affirmative action. For example, the UCR includes property offenses involving commercial establishments in its offense series, but the NCVS does not. We cannot separately identify and remove commercial property offenses from the UCR offense series; this may lead to positive bias in estimates of the effects of litigation leading to affirmative action on offenses known to law enforcement. If agencies shifted effort to responding to reports of non-commercial property offenses after litigation onset, non-commercial property offenses may have decreased, but commercial property offenses (recorded in the UCR but not the NCVS data) may have increased. Post-litigation increases in commercial property offenses would generate upward bias in estimates of the effects of litigation leading to affirmative action on offenses known to law enforcement.

Law enforcement agencies also do not record all victimization reports as criminal offenses. In one study in which independent observers accompanied law enforcement officers responding to 911 calls, by the time that officers arrived on the scene, victims were no longer present for approximately 33% of calls. These calls were not recorded as criminal offenses. Of the calls coded by observers as involving criminal incidents in which a victim was present, officers failed to report the incident as a criminal offense in another 36% of cases [Black, 1970]. Calls with longer response times may be less likely to be recorded as criminal offenses [Asher, Jan 29, 2018]. Officers may be slower to respond to calls originating in less white neighborhoods, leading to fewer victimization reports from less white neighborhoods being recorded as criminal offenses, relative to victimization reports from more white neighborhoods.[27]

The discretion of law enforcement agencies to record complaints as criminal offenses (or not) may also generate positive bias in estimates of the effects of litigation onset on offenses known to law enforcement. If, after the onset of litigation leading to affirmative action, agencies decreased response times to calls involving Black victims, and/or were more likely to record incidents involving Black victims as criminal offenses, then a larger number of victimization reports may have been recorded as criminal offenses post-litigation.[28] Post-litigation increases in the proportion of victimizations recorded as "offenses known to law enforcement" would also generate upward bias in estimates of the effects of litigation leading to affirmative action on offenses known to law enforcement.

As reported in Plot 2 of Figure F.3, estimates of the effects of litigation leading to affirmative action on changes in offenses known to law enforcement are very similar to the estimates from

missingness in the annual total offense series between 1979 and 2004.

[27] https://www.aclu-il.org/en/press-releases/newly-released-data-shows-city-continues-deny-equitable-police-services-south-and.

[28] In 1983, the FBI began asking law enforcement agencies to report complaints that were determined by agencies not to involve criminal offenses as "unfounded" complaints. Unfortunately, we do not have enough pretreatment years of data on these unfounded complaints to systematically analyze this potential source of measurement error.

the NCVS data, albeit noisier and slightly more positive on average. The somewhat less negative estimates of the effects of litigation on changes in offenses known to law enforcement, relative to changes in reported victimization, are consistent with positive bias generated by a larger share of reported victimizations being recorded as criminal offenses after litigation onset. Consistent with the estimates reported by McCrary [2007], all of the post-litigation estimates are indistinguishable from zero at the 95% confidence level. Without taking into account the positive effects of litigation leading to affirmative action on reporting rates, and potentially on the share of reported victimizations recorded as offenses known to law enforcement, we would miss its large negative effects on victimization.

G Reasons for Not Reporting

Table G.1 reports pretreatment means for the reasons given for not reporting victimization to law enforcement, by race, for the treated MSAs during pretreatment years only, using only respondents who experienced victimization. During this pretreatment period, Black respondents were more likely to not report victimization because they believed that the police either would not or could not help them; 28% of Black respondents cited at least one of these as reasons for not reporting victimization, relative to 24% of white respondents. White respondents were more likely to not report victimization to the police for reasons unrelated to trust in the police response, relative to Black respondents.

Table G.1: Descriptive Statistics
Reasons for Not Reporting Victimization, by Race
Treated MSAs Pretreatment; Respondents Who Experienced Crime Victimization

	White Respondents	Black Respondents
Not Reported: Mistrust in Police Response		
Police Wouldn't Help	0.078	0.112
Police Couldn't Do Anything	0.177	0.185
Not Reported: Mistrust Police	0.241	0.281
Not Reported: Other Reasons		
Not Impt to Rspndt	0.268	0.176
Dealt With Another Way	0.141	0.111
Other Reason	0.113	0.090
Not Reported: Other Reasons	0.390	0.273
N	6694	1504

Cells report NCVS means between 1979-1985 for treated MSAs during pretreatment years only.

Figure 6 reports event study estimates of the effects of litigation leading to affirmative action on the racial gap in not reporting victimization because of mistrust in the police. There is an average post-litigation decrease of 4.3 percentage points in the racial gap in not reporting because of mistrust in the police. A majority of these estimates are significant at the 90% level or higher.

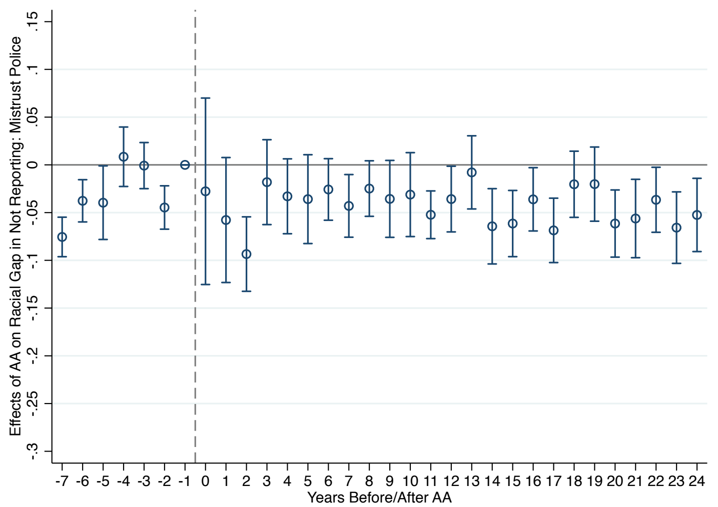
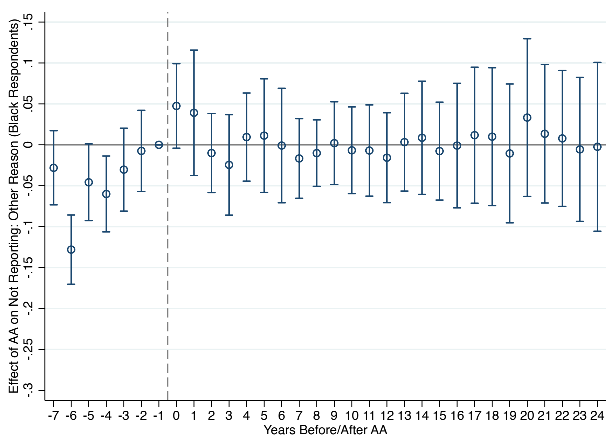
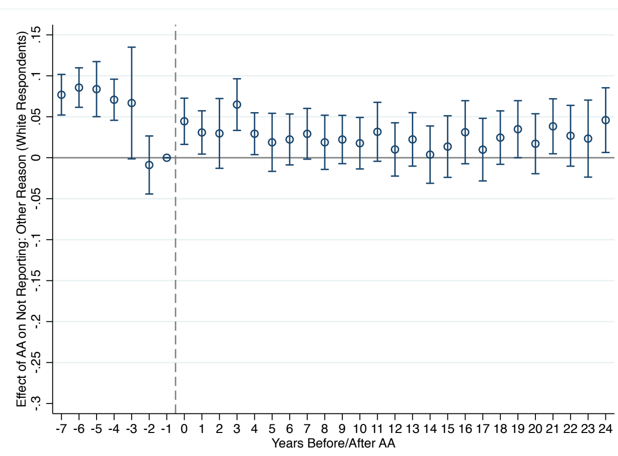


Figure G.1: Estimates of Effects of Litigation Leading to Affirmative Action on Changes in Racial Gap in Not Reporting Victimization: Mistrust Police; 1979-2004
Treated-Only Sample

Figure G.2 reports estimates of the effects of litigation leading to affirmative action on changes in not reporting victimization for reasons other than mistrust in the police response, relative to the baseline year. There are no post-litigation decreases in not reporting due to reasons other than mistrust in the police response, for either white or Black victims.



(a) Black Respondents



(b) White Respondents

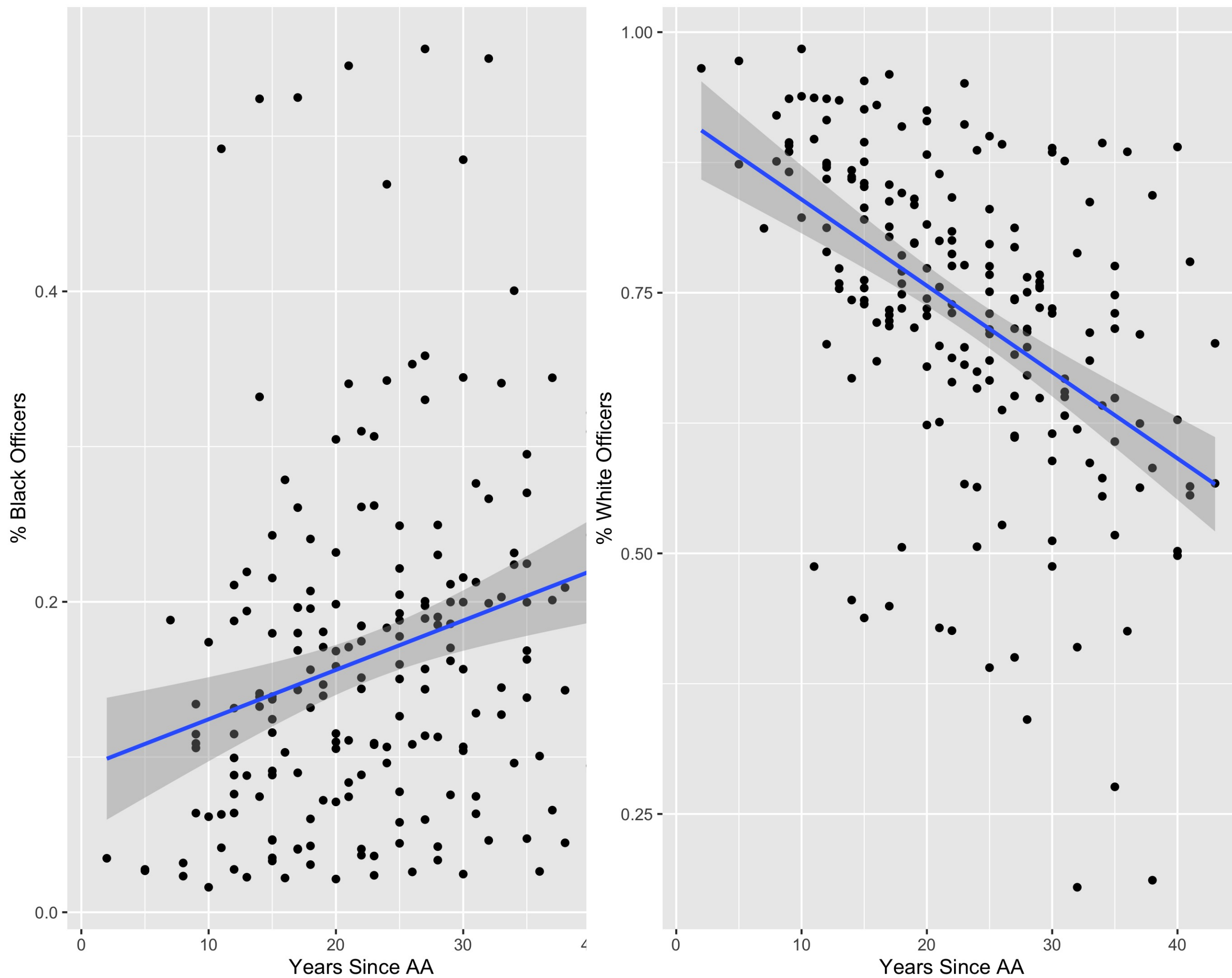
Figure G.2: Estimated Effects of Litigation Leading to Affirmative Action on Changes in Not Reporting: Other Reasons, 1979-2004
Treated-Only Sample

Table G.2: Callaway and Sant'Anna [2020a] ATT Estimates
Reasons For Not Reporting Other Than Mistrust in Police

	Black Respondents	White Respondents
Treated MSAs	0.078	0.030
	(0.195)	(0.044)
N	670	670
All MSAs	0.007	0.069**
Control = Never + Not Yet Treated	(0.027)	(0.019)
N	1,032	1,032
All MSAs	0.006	0.069**
Control = Never-Treated	(0.027)	(0.016)
N	1,032	1,032
Pretreatment DV Mean	0.559	0.710

* p<.10, ** p<.05, *** p<.01. Standard errors clustered on MSA. The sample comprises respondents who did not report victimization to the police. Callaway B. and Sant'Anna P. (2020), "did: Difference in Differences," R package version 2.0.0, https://bcallaway11.github.io/did/.

H Agency Racial Composition



(a) Black Officer Shares

(b) White Officer Shares

Figure H.1: Litigation/AA Plan Duration and Agency Racial Composition, 1987-2013
Treated-Only Sample

Table H.1: Estimates of Effects of Litigation Leading to Affirmative Action on Proportions of Black and White Officers, 1987-2013
Treated-Only Sample

	Prprtn Black Officers	Prprtn White Officers
Litigation Duration	0.002**	-0.008***
	(0.001)	(0.002)
1987 Mean	0.12	0.87
N	200	200
MSA FE	Yes	Yes
Year FE	Yes	Yes

* p<.10, ** p<.05, *** p<.01. Standard errors clustered on MSA.

I Number of Police

We source agency-level data on numbers of sworn officers from the FBI's LEOKA data series. We aggregate agency-level numbers of officers and agency-level population served to the MSA/year level. Figure I.1 reports event study estimates for the 26 treated MSAs. There is no effect of litigation leading to affirmative action on changes in the numbers of sworn officers, relative to the baseline year.

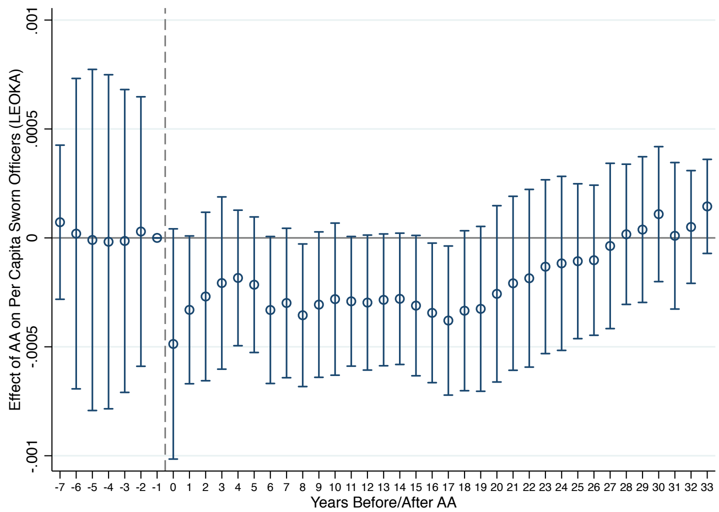


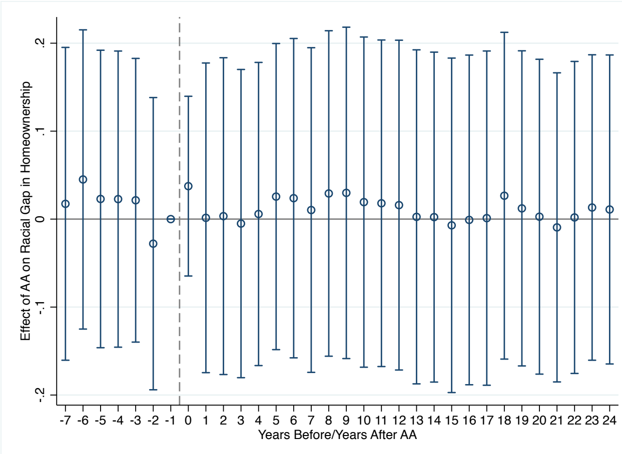
Figure I.1: Estimated Effects of Litigation Leading to Affirmative Action on Changes in Number of Sworn Officers Per Capita, 1979-2004
Treated-Only Sample

J Socioeconomic Characteristics

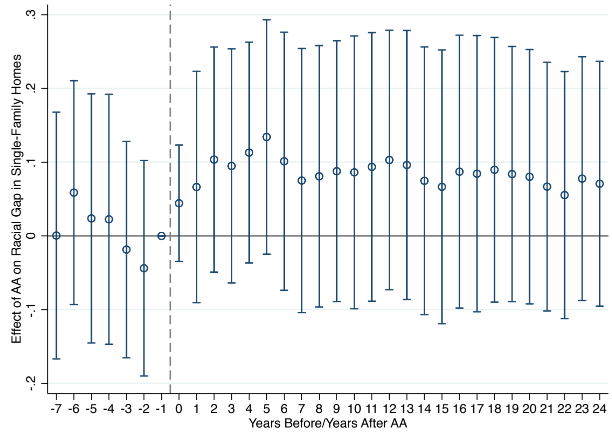
As reported in the second two columns of Table 1, sizable racial disparities in a variety of socioeconomic characteristics are evident in treated MSAs during pretreatment years. Black respondents in these MSAs are less likely to own homes, to live in single family residences, to have household incomes of at least $30,000, to have some college, and to be married, relative to white respondents; a higher proportion of Black respondents in these MSAs are also in the 18-29 year age cohort, relative to white respondents. These pretreatment racial disparities in socioeconomic attributes may have contributed to pretreatment racial disparities in crime victimization. One possibility is that litigation leading to affirmative action may have caused decreases in the racial disparities in socioeconomic characteristics observed in treated MSAs pretreatment, leading to decreases in racial disparities in victimization.

We estimate Equation 2 using the respondent-level outcomes of homeownership, residence in a single-family home, age 18-29, household income of at least $30,000, some college, and marital status, using only the 26 MSAs that will eventually become subject to treatment. Figures J.1-J.3 report the results. There is no evidence of posttreatment changes in racial disparities in socioeconomic characteristics.

Figures J.1-J.3 also reveal no pretreatment trends in respondent socioeconomic characteristics in the 26 MSAs that will eventually be treated. The lack of pretreatment trends in respondent socioeconomic characteristics further supports the premise that the timing of litigation leading to affirmative action was as-if random among these MSAs.

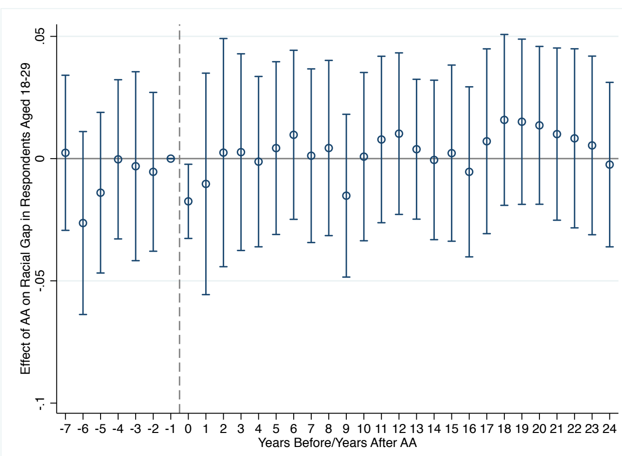


(a) Homeownership

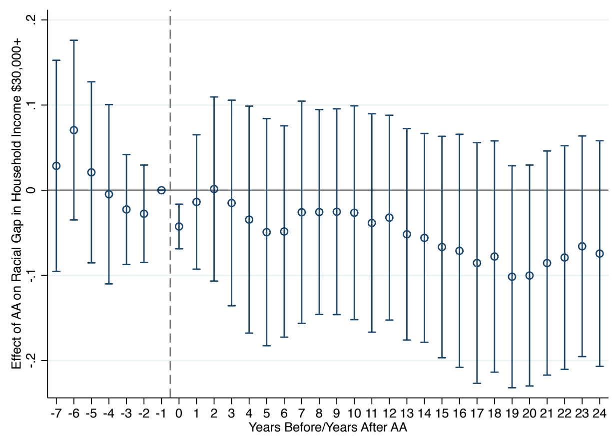


(b) Single Family Home

Figure J.1: Estimated Effects of Litigation Leading to Affirmative Action
on Changes in the Racial Gap in Homeownership and Single Family Homes, 1979-2004
Treated-Only Sample

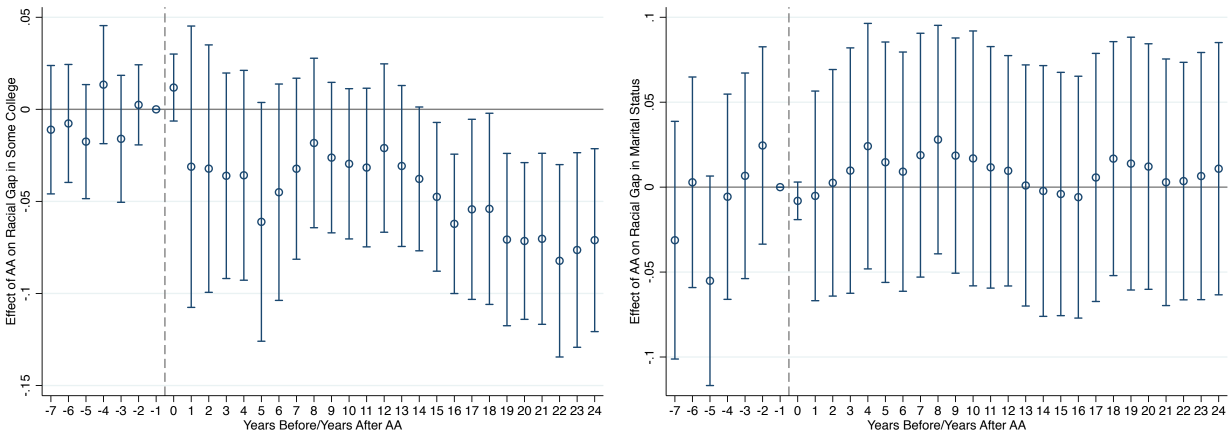


(a) Age 18-29



(b) Household Income $30,000+

Figure J.2: Estimated Effects of Litigation Leading to Affirmative Action
on Changes in the Racial Gap in Presence of Respondents Aged 18-29 and Household Income $30,000+, 1979-2004
Treated-Only Sample



(a) Some College

(b) Married

Figure J.3: Estimated Effects of Litigation Leading to Affirmative Action
on Changes in the Racial Gap in Respondents With Some College and Married, 1979-2004
Treated-Only Sample

K UCR Clearance Rates

The FBI UCR data report the numbers of offenses known to law enforcement that were "cleared" by law enforcement agencies. As noted in the body text, litigation leading to affirmative action may have induced agencies to exert greater effort to solve or "clear" victimization complaints recorded as criminal offenses; this greater effort to solve crime may have both reduced victimization rates and increased clearance rates.

However, declaring offenses as "cleared" in the UCR data is at an agency's discretion. Agencies may report to the UCR that an offense has been cleared if either an arrest of a suspect has been made, or the agency has simply declared the offense to have been cleared even when an arrest has not been made. Although there are UCR guidelines for the use of these "exceptional" clearances, there does not appear to be any monitoring of agency practice. Further, there have been media reports of agencies strategically using exceptional clearances in order to increase clearance rates for offenses to which the agencies either do not want to devote effort, or for which genuine clearances are harder to achieve.[29]

Agencies' discretion to simply declare offenses as cleared may bias effect estimates. Prior to the onset of litigation leading to affirmative action, agencies may have been more likely to use strategic clearances for crimes reported by Black victims, relative to crimes reported by white victims. After the onset of litigation leading to affirmative action, agencies may have reduced the frequency with

[29] "When It Comes to Rape, Just Because a Case Is Cleared Doesn't Mean It's Solved," ProPublica/Reveal from The Center for Investigative Reporting, Nov. 15, 2018; "How We Analyzed Rape Clearance Rates," ProPublica/Reveal from The Center for Investigative Reporting, Nov. 15, 2018.

which they strategically (and misleadingly) declared these offenses to have been cleared. This change in practice would be observed as a decrease in clearance rates after the onset of litigation leading to affirmative action. Even if agencies also increased effort devoted to sincerely clearing these offenses, leading to increases in (true) clearance rates, these two trends might offset each other.

Further, even in the absence of strategic manipulation of clearance rates, clearing recorded offenses is only one mechanism by which agencies could have reduced victimization rates. Increased effort devoted to patrolling higher crime neighborhoods could have deterred criminal victimization, thereby reducing victimization rates, even in the absence of post-victimization law enforcement effort. Even if clearance rates were sincerely reported by law enforcement agencies, the absence of an effect of litigation leading to affirmative action on clearance rates need not imply the absence of greater responsiveness by law enforcement agencies to reports of criminal victimization.

We aggregate total numbers of cleared offenses and total numbers of offenses to the MSA/year level for the sample of 87 agencies located in the core counties of the 26 treated MSAs that consistently report UCR data between 1979 and 2004. Clearance rates are defined as total clearances/total offenses. Figure K.1 reports estimates from Equation 1. Estimates are too noisy to be able to make reliable inferences.

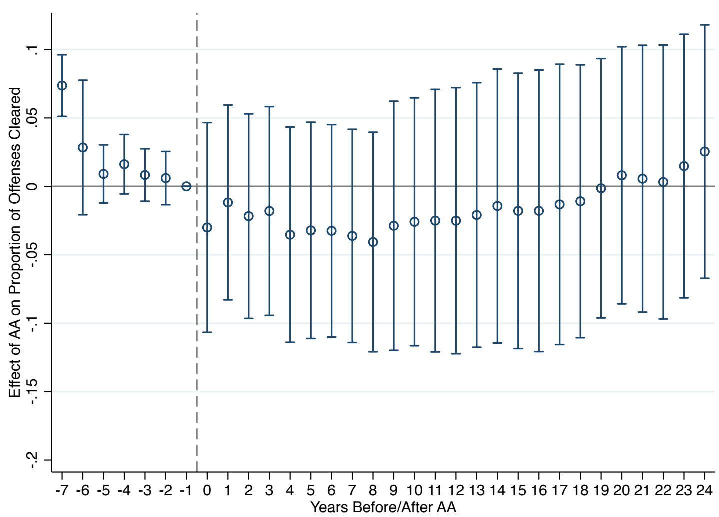


Figure K.1: Estimated Effects of Litigation Leading to Affirmative Action on
Changes in UCR Clearance Rates, 1979-2004
Treated-Only Sample

L Heterogeneity by Type of Crime

We explore whether the impacts of litigation leading to affirmative action on relative Black victimization are heterogeneous by kind of crime. Table L.1 reports victimization means, by race and category of crime, for treated MSAs during pretreatment years only.[30] Black respondents report higher pretreatment victimization rates, relative to white respondents, for nearly all categories of crime. The exceptions are assault without a weapon (simple assault), both with and without injury, for which white respondents report slightly higher victimization rates during the pretreatment period. Most crime victimization during the pretreatment period, for both white and Black respondents, occurs in the category of burglary/theft, where there is approximately a 5 percentage point (50%) racial gap in victimization (11% victimization rate for white respondents; 16% victimization rate for Black respondents).

[30]Crime categories are reported as defined by the NCVS.

Table L.1: Descriptive Statistics
Victimization by Race and Type of Crime
Treated MSAs Pretreatment

	White Respondents	Black Respondents
Attptd/Cmpltd Robbery/Injury/Serious Assault	0.05	0.19
Attptd/Cmpltd Robbery/Injury/Minor Assault	0.08	0.11
Attptd/Cmpltd Robbery/No Injury	0.24	1.09
Attptd/Cmpltd Robbery/No Contact	0.10	0.30
All Robbery	0.47	1.69
Burglary	1.68	3.38
Attptd Forcible Entry	0.48	1.23
Attptd/Cmpltd Motor Vehicle Theft	0.51	1.93
Attptd/Cmpltd Theft	8.43	9.65
All Theft/Burglary	11.09	16.19
Attptd/Cmpltd Aggravated Assault	0.59	1.26
Simple Assault/Injury	0.28	0.19
Simple Assault/No Injury	0.99	0.88
All Assault	1.86	2.32
Attptd/Cmpltd Rape	0.06	0.12
N	49,623	7,399

Cells report NCVS means between 1979-1985 for treated MSAs during pretreatment years only.

Because there are fewer incidents within each crime category, relative to the full sample, to achieve greater precision we report pooled TWFE DD estimates that interact race of respondent with the post-litigation indicator, by type of crime. We report estimates for the 26 MSAs eventually subjected to treatment, exploiting only the variation in treatment timing.

Tables L.2 and L.3 report the results. Effects are largely consistent across crime categories. Litigation leading to affirmative action decreases racial disparities in victimization for robbery with serious injury, robbery with no injury, robbery with no contact (e.g., pickpocketing), all categories of robbery pooled together, burglary, attempted forcible entry, motor vehicle theft, other theft, all categories of burglary and theft pooled together, aggravated assault, and all categories of assault pooled together. For all of these categories of crime, Black respondents were also significantly more likely to be victimized during pretreatment years, relative to white respondents. The largest reduction in the racial gap in crime victimization is realized in the pooled category of burglary/theft, or property crimes committed without the personal involvement of the victim.

We see no negative effects of litigation leading to affirmative action on racial disparities in robbery with minor injury, simple assault with no injury, simple assault with injury, and rape. However, these categories account for few crimes, for both white and Black respondents.

Table L.2: Estimated Effects of Litigation Leading to Affirmative Action on the Racial Gap in Victimization, 1979-2004; By Type of Crime
Treated-Only Sample

	Robbery Injury Serious	Robbery Injury Minor	Robbery No Injury	Robbery No Contact	Robbery All	Burglary	Attmptd Forcible Entry	Theft Motor Vehicle	Theft Attmptd/ Completed	Burglary/ Theft All
Post-AA	0.000	-0.000	0.001	0.001***	0.002**	-0.000	0.000	0.001	-0.000	0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.006)	(0.007)
Black	0.001***	0.000	0.008***	0.002***	0.012***	0.017***	0.007***	0.014***	0.015***	0.053***
	(0.000)	(0.000)	(0.001)	(0.000)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.003)
Post-AA X Black	-0.001***	0.000	-0.006***	-0.001**	-0.008***	-0.013***	-0.005***	-0.011***	-0.015***	-0.044***
	(0.000)	(0.000)	(0.001)	(0.000)	(0.002)	(0.001)	(0.001)	(0.001)	(0.003)	(0.004)
Constant	0.001***	0.001***	0.002***	0.002***	0.006***	0.015***	0.004***	0.004***	0.081***	0.104***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)	(0.001)	(0.004)	(0.005)
N	1,419,674	1,419,674	1,419,674	1,419,674	1,419,674	1,419,674	1,419,674	1,419,674	1,419,674	1,419,674
MSA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

* p<.10, ** p<.05, *** p<.01. Standard errors clustered on MSA.

Table L.3: Estimated Effects of Litigation Leading to Affirmative Action on the Racial Gap in Victimization, 1979-2004; By Type of Crime
Treated-Only Sample

	Assault Aggravated	Assault Simple Injury	Assault Simple No Injury	Assault All	Rape
Post-AA	0.000	-0.000	-0.001	-0.002	0.000
	(0.000)	(0.001)	(0.002)	(0.002)	(0.000)
Black	0.007***	-0.001***	-0.001	0.005***	0.001***
	(0.001)	(0.000)	(0.001)	(0.001)	(0.000)
Post-AA X Black	-0.005***	0.001***	0.001	-0.003***	-0.000
	(0.001)	(0.000)	(0.001)	(0.001)	(0.000)
Constant	0.006***	0.002***	0.009***	0.017***	0.001***
	(0.000)	(0.000)	(0.002)	(0.002)	(0.000)
N	1,419,674	1,419,674	1,419,674	1,419,674	1,419,674
MSA FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

* p<.10, ** p<.05, *** p<.01. Standard errors clustered on MSA.