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Reducing racial disparities in crime victimization: Evidence from employment discrimination litigation

Anna Harvey^{a,*}, Taylor Mattia^b

^a Professor of Politics; Affiliated Professor of Data Science and Law; Director, Public Safety Lab, New York University, United States

^b Post-Doctoral Fellow, Center for the Study of Race, Ethnicity, and Immigration, University of Pennsylvania, United States

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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 Black crime victimization. Using data from the National Crime Victimization Survey between 1979 and 2004, and a series of estimators appropriate for difference-in-differences designs with staggered treatment, we find that litigation over racially discriminatory employment practices in law enforcement agencies decreased Black crime victimization by magnitudes ranging between 24 - 27%, but had no discernible impacts on white crime victimization, reducing the pretreatment racial gap in crime victimization by 73 - 82%. Decreases in Black crime victimization appear in the first year after litigation onset, consistent with efforts by litigated departments to address racial disparities in the police response to reported crime.

1. Introduction

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. Several recent studies have established that these racial disparities in enforcement actions are due at least in part to racially disparate practices by police officers (Goncalves and Mello, 2021; Hoekstra and Sloan, 2022; West, 2018). 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 and McCrary, 2018). 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 police departments respond differently to crime experienced by Black victims, relative to crime experienced by white victims. Police officers may spend relatively less time patrolling neighborhoods with more Black residents, leading to relatively less deterrence of crimes committed against Black individuals (Di Tella and Schargrodsky, 2004; Klick and Tabarrok, 2005; Draca et al., 2011). Officers may take longer to respond to reports of crime experienced by Black victims, leading to relatively longer call response times, lower clearance rates, and again relatively less deterrence (Blanes i Vidal and Kirchmaier, 2017). Police officers may exert less effort to encourage Black victims to file reports and to investigate those reports, again leading to relatively less deterrence (Levitt, 1998; Vollaard and Hamed, 2012). Racially disparate police responses to crime victimization would be consistent with racially disparate police uses of enforcement actions (Goncalves and Mello, 2021; Hoekstra and Sloan, 2022; West, 2018).

Racial disparities in crime victimization could also exist for reasons other than racially discriminatory police responses 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).

* Corresponding author.

E-mail addresses: anna.harvey@nyu.edu (A. Harvey), trmattia@sas.upenn.edu (T. Mattia).

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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. Plaintiffs filing employment discrimination lawsuits have strong incentives to report evidence of department-wide practices of intentional race-based discrimination, including in the police response to crime victimization, to support their legal claims. Documentary evidence from employment discrimination litigation indicates that plaintiffs respond to these incentives, including in their initial filings reports of race-based discrimination in the police response to crime victimization. These court filings may then induce command personnel to seek to reduce racially discriminatory practices throughout their departments, including in the police response to crime victimization, for example by directing officers to spend more time patrolling in Black neighborhoods, to reduce response times to reports of Black crime victimization, and to increase investigative effort in cases with Black crime victims. 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 also would not expect the litigation of police departments for race-based employment discrimination to affect the police response to white crime victimization to the same degree that it affects the police response to Black crime victimization.

Using victimization data from the National Crime Victimization Survey between 1979 and 2004, and leveraging idiosyncratic variation in the timing of litigation of law enforcement agencies for race-based employment discrimination between 1970 and 1986, we implement two-way fixed effect difference-in-differences estimation (TWFE DD) as well as the estimators developed by [Borusyak et al. \(2022\)](#), [Callaway and Sant'Anna \(2021\)](#), [de Chaisemartin and D'Haultfœuille \(2022\)](#), and [Sun and Abraham \(2021\)](#) to estimate average treatment effects in settings with staggered treatment timing and potentially heterogeneous treatment effects. We first show that MSAs without law enforcement agencies that were litigated for race-based employment discrimination differ in both levels and trends of pretreatment variables, relative to litigated MSAs, while litigated MSAs experiencing litigation at different points in time are much more similar to each other in pretreatment characteristics. 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 show that, within the sample of treated MSAs, there is no evidence that Black crime victimization was already trending downward prior to litigation onset.

[Goodman-Bacon \(2021\)](#) decomposition of the TWFE DD subgroup estimates in the full sample of treated MSAs, including those MSAs treated before 1979 (always-treated MSAs) and years during which all MSAs had already been treated (1986 - 2004), suggests the potential for downward bias in the TWFE DD estimates of the effect of litigation on Black crime victimization. The estimators developed by [Borusyak et al. \(2022\)](#), [Callaway and Sant'Anna \(2021\)](#), [de Chaisemartin and D'Haultfœuille \(2022\)](#), and [Sun and Abraham \(2021\)](#) all address the potential for bias in the TWFE DD estimator, restricting the sample to those MSAs litigated after 1979, including only those years during which at least some MSAs remained untreated, and allowing for treatment effect heterogeneity. These estimates indicate that litigation reduced total Black crime victimization by 24 - 27%, and reduced the proportion of Black respondents who experienced crime victimization by 18 - 23%, with no discernible effects on white crime victimization. Prior to litigation onset, Black NCVS respondents in these MSAs experienced 50% more total crime victimization, relative to white respondents, and were 38% more likely to experience any crime victimization. Our estimates indicate that litigation reduced the pretreatment racial

gap in total victimization by 73% - 82%, and reduced the racial gap in any victimization by 67% - 83%. While previous work found no effects of employment discrimination litigation on offenses known to law enforcement, as reported in the FBI's Uniform Crime Reporting (UCR) data ([McCrary, 2007](#)), our findings reveal racially heterogeneous effects obscured by the lack of information on victim race in the UCR data.

We also explore heterogeneous treatment effects. Prior to litigation onset, Black and white NCVS respondents were equally likely to experience crime victimization that they deemed sufficiently unimportant to warrant reporting to the police. We find no effects of litigation onset on this category of crime victimization. Instead, effects of litigation onset are localized to the two categories of Black crime victimization which do show pretreatment racial disparities: crime victimization that was deemed sufficiently important by victims to warrant reporting to the police, and crime victimization that was unreported to the police not because it was unimportant to the victim, but rather because the victim did not have trust in the likely police response. These patterns further suggest that pretreatment racial disparities in crime victimization in our sample were driven at least in part by racial disparities in the police response to crime victimization, and that the onset of litigation reduced racial disparities in the police response to reported crime.

Previous work using restricted EEOC data found increases in Black officer shares emerging approximately five years after the onset of race-based employment discrimination litigation ([McCrary, 2007](#); [Miller and Segal, 2012](#)). We see sharp decreases in Black (but not white) crime victimization in the first year after litigation onset, before any likely increases in Black officer shares. The timing of the effects we observe is consistent with attempts by litigated departments to address racially discriminatory practices, including in the police response to reported crime, immediately after litigation onset. This mechanism is also consistent with the responsiveness of police departments to litigation-induced interventions in other contexts ([Mummolo, 2018](#); [Rivera and Ba, 2022](#); [Cox et al., 2022](#)). It is possible that later post-litigation increases in Black officer shares may have contributed to further decreases in Black crime victimization, although, given the limited nature of publicly available data on officer race, we do not explore that hypothesis here.

We are largely unable to explore specific officer behaviors that could account for the effect of race-based employment discrimination litigation on Black crime victimization, including increased patrol deployment to neighborhoods with more Black residents, shorter response times to calls for service from Black crime victims, and increased effort devoted to recording, investigating, and clearing cases with Black crime victims. Similarly, we are unable to explore the contribution of increased reporting of victimization to the decreased incidence of victimization, as suggested by [Miller and Segal \(2018\)](#). The reporting of victimization is a post-treatment outcome that is defined only for those NCVS respondents who would experience victimization under both treatment conditions (litigated and unlitigated), but we lack the information necessary to identify these "always-taker" respondents. We find no evidence to suggest that increases in the numbers of sworn officers per 100,000 in population or increases in clearance and arrest rates, the latter both overall and by race of arrested person, were possible causal pathways.

Our findings indicate that one source of racial disparities in crime victimization lies in a racially disparate police response to crime victimization. Departments facing the threat of judicial intervention as a result of litigation over race-based employment discrimination appear to increase their responsiveness to Black crime victimization, reducing the incidence of Black crime victimization without increasing white crime victimization. Although we lack data on the specific departmental practices that may have changed as a result of litigation onset, our findings do suggest that officers' responses to reports of crime victimization became less discriminatory after litigation onset. These findings indicate a need for further research on police practices that can support greater racial equity in the provision of public safety.

2. Estimating effects of employment discrimination litigation

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, emerging approximately five years after the onset of race-based employment discrimination litigation (McCrary, 2007; Miller and Segal, 2012; see also Miller, 2017) on the effects of federal affirmative action regulations on Black employment shares at firms contracting with the federal government). McCrary (2007) found that litigation of law enforcement agencies for race-based employment discrimination had no effects on offenses known to law enforcement, as reported to the FBI's Uniform Crime Reporting (UCR) program. However, UCR offense data do not identify victim race, potentially obscuring racially heterogeneous effects of litigation. Using the litigation of law enforcement agencies for gender discrimination in employment as an instrument for female officer shares, Miller and Segal (2018) found that litigation-induced increases in female officer shares decreased the incidence of violent crimes against women. To date no study has looked at the effects of litigation of law enforcement studies for race-based employment discrimination on Black crime victimization.

Existing studies have focused on changes in officer racial and gender composition as a consequence of employment discrimination litigation. But we might also expect the onset of race-based employment discrimination litigation to impact police behavior through channels other than increasing the share of Black police officers. The 14th Amendment protects state and local employees from "purposeful or intentional" discrimination by their employers (Washington v. Davis, 426 U.S. 229 (1976)). Section 1983 of the Civil Rights Act of 1871 creates civil liability for state and local employers if employees can show "persistent and widespread discriminatory practices of state officials" that are so "permanent and well settled as to constitute a 'custom or usage' with the force of law" (Monell v. Department of Social Services of the City of New York, 436 U.S. 658 (1978), quoting Adickes v. S. H. Kress & Co., 398 U.S. 144 (1970)). Finally, employee plaintiffs have wide latitude to introduce circumstantial evidence of racial animus on the part of their employers in order to establish intent to discriminate, including evidence that their employers engaged in the discriminatory treatment of individuals beyond those named as plaintiffs (U.S. Postal Service Board of Governors v. Aikens 460 U.S. 711 (1983)).¹ These interacting legal provisions create incentives for officer plaintiffs in race-based employment discrimination cases to report evidence of discriminatory practices throughout a police department, including in the department's response to reported crime.

There are a number of ways in which departments' responses to crime reported by Black victims could be racially discriminatory, with the effect of increasing the relative incidence of Black crime victimization. For example, officers might be allowed or even encouraged to spend less time patrolling neighborhoods with more Black residents. Patrol presence is strongly associated with deterrence (Di Tella and Scharrofsky, 2004; Klick and Tabarrok, 2005; Draca et al., 2011); less patrol time spent in Black neighborhoods could imply less deterrence of Black crime victimization. Officers may also be allowed or even encouraged to take more time to respond to calls for service from neighborhoods with more Black residents. Longer call response times are associated with lower probabilities that victims and witnesses identify suspects and that immediate arrests are made, potentially decreasing deterrence

¹ "[Plaintiff] introduced testimony that the person responsible for the promotion decision at issue had made numerous derogatory comments about blacks in general and [the plaintiff] in particular... As in any lawsuit, the plaintiff may prove his case by direct or circumstantial evidence. The trier of fact should consider all the evidence, giving it whatever weight and credence it deserves" (Aikens, note 3).

(Blanes i Vidal and Kirchmaier, 2017). Longer response times to calls for service from Black victims may then imply less deterrence of Black crime victimization.² Officers may also simply exert less effort to encourage Black crime victims to file formal reports, and to investigate those reports. Increased police effort is associated with increased crime reporting (Levitt, 1998; Vollaard and Hamed, 2012), and increased investigative effort into reported crime is associated with decreased crime incidence (Miller and Segal, 2018). Less police effort devoted to reporting and investigating Black crime victimization could again lead to less deterrence of Black crime victimization.

Case files of employment discrimination complaints brought against police departments reveal that plaintiffs in these cases can and do report evidence of racially discriminatory police responses to Black crime victimization. For example, in December 2018 Black and Hispanic police officers filed a race-based employment discrimination lawsuit in federal court against Prince George's County, Maryland and several individual members of the county's police department. The filing alleged *inter alia* that the department was more likely to ignore civilian complaints brought against white officers, relative to complaints brought against Black and Hispanic officers. The officers' filing cited a complaint made by a Black civilian against the white officer who had responded to the civilian's 911 call: "The complainant alleged that Officer Merkel told her he would not assist her because his job was 'not to assist Black people, only to arrest them' and that he would leave her 'at the mercy of other Black criminals.'" The complainant also stated that the responding officer "accused her of committing a crime by being a Black person loitering in the area" and that he "left the scene without providing the complainant any assistance." The plaintiffs also referenced another complaint brought against a white officer by a Black civilian who had reported drug activity in his apartment complex: "He alleges that P/O laughed at him, attacked him, and knocked him to the ground." The plaintiffs connected the department's racially discriminatory treatment of Black and Hispanic officers with its racially discriminatory treatment of Black and Hispanic civilians, including those reporting crime victimization: "The incidents of racism and retaliation that the Plaintiffs experienced are a part of a long historical pattern of mistreatment directed at People of Color both within PGPD and the broader community."³

Plaintiffs alleging racially discriminatory police responses to crime victimization can seek additional evidence about these practices through discovery. Police command staff thus have incentives to address discriminatory policing practices throughout their agencies as soon as employment discrimination litigation is filed, including in the police response to crime reported by Black victims, in order to undermine plaintiffs' claims. For example, officers in agencies litigated for race-based employment discrimination may be directed to spend more time patrolling in Black neighborhoods, to decrease response times to calls for service from neighborhoods with more Black residents, and/or to increase effort devoted to the reporting and investigation of crime experienced by Black victims. These responses to litigation may increase the deterrence of crimes experienced by Black victims, leading to lower Black victimization rates.

The responses of agencies to litigation may be immediate. For example, Mummolo (2018) reports that, prior to the commencement of the trial in David Floyd et al v. City of New York (2013) over racial disparities in New York Police Department (NYPD) "stop, question and frisk" practices, the plaintiffs filed a memo with the court on March 4, 2013 requesting a significantly increased reporting burden for NYPD

² 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>).

³ https://www.aclu-md.org/sites/default/files/field_documents/pgpd_complaint.pdf, <https://www.washlaw.org/wp-content/uploads/2021/02/HNLEA-v-PGC-Aug-28-Graham-Report-Unsealed.pdf>.

pedestrian stops. One day later, on March 5, 2013, the NYPD issued a directive implementing this reporting policy, apparently in an attempt to preempt judicial intervention in the department's practices, leading to a sharp and immediate decrease in recorded stops. Similarly, [Rivera and Ba \(2022\)](#) found reductions in police misconduct shortly after court rulings increasing the visibility of complaints brought against Chicago police officers, and [Cox et al. \(2022\)](#) found reductions in police killings of nonwhite civilians shortly after the onset of race-based employment discrimination litigation. Changes in police practices can also lead to rapid changes in criminal behavior. [Di Tella and Schargrodsky \(2004\)](#), [Klick and Tabarok \(2005\)](#), and [Draca et al. \(2011\)](#) all document the rapid responsiveness of reported crime to temporal and spatial reallocations of police patrol presence in response to terror attacks and alerts.

In the case of the litigation filed against Prince George's County, within 18 months of litigation onset the police chief had resigned and the county had established a commission on police reform and had committed to diverting funding from the police budget to mental health treatment. These actions were followed within months by the departure of two more command officers named in the litigation, and the hiring of a Black chief publicly committed to racial equity reform in the police department. By July 2021 the county had settled the litigation with a total payout of approximately \$26 million, agreeing to new policies on racially equitable policing monitored by a newly created Office of Integrity and Compliance.⁴ These responses to the filing of the employment discrimination lawsuit may plausibly have led to an increased police responsiveness to Black crime victimization in Prince George's County, and thus to relative reductions in the incidence of Black crime victimization.

These potential effects of litigation onset on the police response to Black crime victimization are also illustrated in Appendix B by the history of the employment discrimination litigation filed against the Chicago Police Department (CPD) in 1973. The litigation effort to end race-based employment discrimination in the Chicago Police Department also shows how plaintiffs can 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 have to address racial disparities in their responses to crime victimization once they face the prospect of judicial intervention in departmental practices.

Over time, employment discrimination litigation leading to affirmative action plans imposed on law enforcement agencies may 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 crime victimization. Black officers may care more about detecting and deterring crime experienced by Black victims, relative to white officers. Black officers may also have better information about the patterns of criminal behavior affecting Black victims, relative to white officers ([Miller and Segal, 2018](#)). Our data do not allow us to investigate the extent to which the impact of litigation on

crime victimization works through increased Black officer shares, but it is a plausible channel for longer-term effects.⁵

Importantly, 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 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.

Finally, we would also not expect race-based employment discrimination litigation to have the same effects on the police response to white crime victimization, relative to its effects on the police response to Black crime victimization. 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 post-litigation decreases in Black crime victimization, relative to changes in white crime victimization.

3. Data

3.1. National crime victimization survey

We source data on crime victimization from the MSA-level release of the National Crime Victimization Survey (NCVS), which reports the MSA of residence for NCVS respondents in core counties of the 40 largest MSAs between 1979 and 2004. The NCVS has been conducted annually since 1973 by the U.S. Census Bureau on behalf of the Bureau of Justice Statistics (BJS); this is the only NCVS release that reports location information for respondents below the level of region. The release is available through the National Archive of Criminal Justice Data (United States Department of Justice, Bureau of Justice Statistics, 2007).

As discussed in more detail in Appendix C, between 1979 and 2004 the NCVS was conducted on a nationally representative sample of approximately 50,000 housing units. Household members aged 12 years and older were interviewed regarding crime incidents twice a year for three consecutive years. 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. Screening questions covered the following types of crimes, including attempts: robbery, burglary, theft, assault, and 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. We report victimization data aggregated to the person/year level.

3.2. 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

⁴ https://www.washingtonpost.com/local/public-safety/prince-georges-police-chief-hank-stawinski-resigns/2020/06/18/c402fb94-b11a-11ea-8758-bfd1d045525a_story.html, https://www.washingtonpost.com/local/public-safety/prince-georges-county-launches-police-reform-task-force/2020/07/03/8cb1c95a-bd62-11ea-8cf5-9c1b8d7f84c6_story.html, https://www.washingtonpost.com/local/md-politics/in-majority-black-suburb-with-history-of-police-brutality-leaders-vow-to-make-reforms/2020/06/16/df28d606-af57-11ea-856d-5054296735e5_story.html, <https://www.washingtoninformer.com/alsobrooks-announces-shake-up-in-police-department-amid-officers-arrest/>, https://www.washingtonpost.com/local/legal-issues/prince-georges-police-lawsuit-settlement/2021/07/20/5953b608-e66b-11eb-a41e-c8442c213fa8_story.html.

⁵ Because employment discrimination litigation may affect outcomes through channels other than the racial composition of police officers, studies estimating the effects of changes in the racial composition of officers as a function of employment discrimination litigation need to separately identify this causal path from other potential effects of litigation onset.

U.S. state and local law enforcement agencies between 1973 and 2005.⁶ 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.⁷ 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 40 largest MSAs in the National Crime Victimization Survey sample. There are 167 such agencies. Each of the 40 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 onset 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. 11 of the remaining MSAs contain no law enforcement agencies that were litigated for race-based employment discrimination between 1969 and 2000; we characterize these as never-treated MSAs. We exclude the remaining three MSAs from the sample.⁸

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. Fig. 1 reports the variation in timing of litigation onset for these 26 treated MSAs, for both numbers of MSAs and numbers of NCVS respondents. Most treated MSAs experienced litigation onset prior to 1979 (the first year in the NCVS survey data), which will have implications for our strategy to estimate treatment effects. Among the set of MSAs treated after 1979, each group of MSAs treated in the same year has at least 30,000 person/year observations in the NCVS.

4. Analysis

We want to estimate the effect of litigation onset (an absorbing binary treatment D_{irt}) on the crime victimization (Y_{irt}) experienced by

⁶ 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.

⁷ The Miller and Segal (2012) data do not include the years in which affirmative action plans were imposed on law enforcement agencies.

⁸ The Dallas and Oakland MSAs 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).

individuals i in MSAs r in periods t . $Y_{irt}(0)$ is the potential outcome without treatment. The causal effect of interest is $\tau_{irt} = Y_{irt} - Y_{irt}(0)$. We will use the crime victimization of individuals in untreated MSAs to estimate the average treatment effect. This is commonly known as a difference-in-differences design.

4.1. Parallel trends assumption

The key assumption in a difference-in-differences design is that outcomes in treated and untreated MSAs would have followed parallel trends in the absence of litigation onset. We cannot directly test the parallel trends assumption (PTA). However, we can assess the likelihood that the PTA holds in two different samples of MSAs: treated and never-treated MSAs, and treated and not-yet-treated MSAs.

One strategy to assess the likelihood of parallel trends is to look at balance on pretreatment characteristics across treated and untreated MSAs. Lack of balance does not imply that crime victimization in these MSAs would not have followed parallel trends in the absence of litigation onset. However, balance on pretreatment characteristics is perhaps suggestive of a greater likelihood that the PTA holds, relative to lack of balance. In our context, MSAs containing law enforcement agencies that were litigated for race-based employment discrimination may have differed on a number of dimensions, relative to MSAs containing only law enforcement agencies that were never litigated. These differences may have affected trends in crime victimization across treated and never-treated MSAs. Within the set of MSAs that would eventually be litigated for race-based employment discrimination in law enforcement, however, those MSAs that were litigated earlier may have been little different from MSAs that were litigated later. Greater balance in pretreatment characteristics across MSAs that were litigated at different points in time may indicate a greater likelihood that the PTA holds across treated and not-yet-treated MSAs.⁹

Table 1 reports descriptive statistics across never-treated and treated MSAs during pretreatment years. 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 for race-based employment discrimination in law enforcement. Perhaps of greatest concern, never-treated MSAs have substantially smaller racial disparities in victimization rates, relative to treated MSAs pretreatment. Table 1 reports averages for overall victimization (both total victimization experienced by a respondent in a given year and an indicator for whether a respondent experienced any victimization in a given year), and subcategories of victimization based on whether the victimization was reported or unreported to law enforcement. Unreported victimization is further differentiated as victimization that was unreported due to a lack of trust in the police response, and victimization that was unreported for other reasons.¹⁰ Relative to white respondents, Black respondents in treated MSAs pretreatment have substantially higher victimization rates

⁹ 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. 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).

¹⁰ Reported victimization is coded as 1 if it was reported to the police; all other responses (both no victimization and unreported victimization) are coded as 0. NCVS respondents who volunteered that they did not report a victimization to law enforcement were asked why; two possible answers were that the "police wouldn't help/not important to police" or that the "police couldn't do anything." These victimizations are coded as unreported due to lack of trust in the police response (1); all other responses are coded as 0. Other reasons for not reporting were that the victimization was "not important to respondent," "dealt with another way," "insurance wouldn't cover," and "other reason." These victimiza-

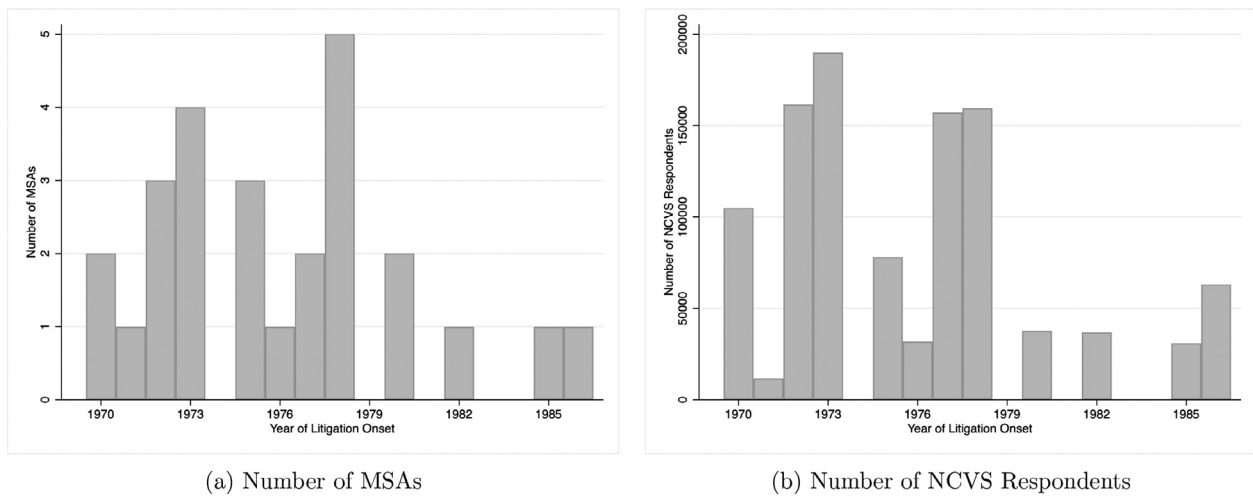


Fig. 1. Timing of litigation onset in treated MSAs 1970–1986.

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/MSA/Yr	712	50	871	129
Proportion White/Black	0.80	0.06	0.83	0.12
Homeownership	0.69	0.55	0.80	0.61
Single Family Home	0.73	0.71	0.83	0.63
Household Income 30K+	0.26	0.11	0.28	0.12
Some College	0.43	0.27	0.35	0.21
Age 18–29	0.27	0.30	0.26	0.27
Married	0.55	0.38	0.56	0.34
Female	0.52	0.53	0.52	0.55
Total Victimization/Year	0.23	0.24	0.22	0.33
Any Victimization/Year	0.17	0.17	0.16	0.22
Reported Total Victimization/Year	0.08	0.08	0.07	0.13
Reported Any Victimization/Year	0.07	0.07	0.06	0.11
Unreported Total Victimization: Police Mistrust/Year	0.06	0.07	0.05	0.09
Unreported Any Victimization: Police Mistrust/Year	0.05	0.06	0.04	0.08
Unreported Total Victimization: Other Reasons/Year	0.09	0.09	0.09	0.10
Unreported Any Victimization: Other Reasons/Year	0.07	0.07	0.08	0.08
N	54,819	3,847	30,487	4,528

Cells report NCVS means for person/year observations between 1979–1985 for all never-treated MSAs, and for treated MSAs during pretreatment years only, by race of respondent. Total Victimization/Year reports the mean of the count of victimizations by person/year; Any Victimization/Year reports the mean of an indicator for any victimization by person/year. Unreported Total Victimization/Year reports the mean of the count of unreported victimizations by person/year; Unreported Any Victimization/Year reports the mean of an indicator for any unreported victimization by person/year. Unreported Total Victimization: Police Mistrust/Year reports the mean of the count of unreported victimizations due to police mistrust (as defined in the text) by person/year; Unreported Any Victimization: Police Mistrust/Year reports the mean of an indicator for any unreported victimization due to police mistrust (as defined in the text) by person/year. 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.

for overall victimization (50% higher total; 38% higher any), reported victimization (86% higher total; 83% higher any), and victimization unreported due to lack of trust in the police response (80% higher total; 100% higher any). These racial disparities in victimization are largely absent in never-treated MSAs. Victimization that was unreported for reasons other than lack of confidence in the police response, including that the victimization was sufficiently unimportant to the respondent to warrant reporting, is experienced approximately equally by Black and white respondents in both treated MSAs pretreatment and never-treated MSAs.

Appendix D reports estimates of the correlations between pretreatment characteristics and treatment onset within the full sample of MSAs,

tions are coded as unreported due to other reasons (1); all other responses are coded as 0.

and between pretreatment characteristics and treatment timing within the sample of treated MSAs, using both county-level demographic measures sourced from the 1970 Census and pretreatment NCVS data. Treated and never-treated MSAs are imbalanced on several pretreatment characteristics for both 1970 Census and NCVS data. Treated and not-yet-treated MSAs show balance on all observable pretreatment characteristics.

We can also look at trends in crime victimization across never-treated and not-yet-treated MSAs. Figs. 2 - 3 report raw victimization data by respondent race for four groups of MSAs: never-treated MSAs, MSAs treated prior to 1979 (always-treated), MSAs treated after 1979 during pretreatment years (not-yet-treated), and MSAs treated after 1979 during posttreatment years (newly-treated). Between 1979 and 1985, Black victimization rates in never-treated and not-yet-treated MSAs are trend-



Fig. 2. Black and white victimization rates by treated MSA status 1979–2004, total victimization/year.



Fig. 3. Black and white victimization rates by treated MSA status 1979–2004, any victimization/year.

ing in opposite directions, suggesting that never-treated MSAs are not likely to provide an appropriate counterfactual for newly-treated MSAs.

Figures 2 - 3 suggest that Black crime victimization was trending in an upward direction prior to litigation onset, reducing concerns that Black victimization rates were already trending downward prior to litigation onset in not-yet-litigated MSAs. We can further explore pretreatment trends in Black crime victimization within the sample of treated MSAs by plotting crime victimization by years before/after treatment. Fig. 4 reports raw victimization data by respondent race and years before/after litigation for only those MSAs treated after 1979, for which we have pretreatment victimization data, and only for the years 1979 - 1985, for which we have data from both treated and not-yet-treated MSAs (all MSAs are treated by 1986). There is no evidence that Black crime victimization was already trending downward prior to litigation onset. After litigation onset, there are immediate discontinuous decreases in Black victimization rates, with no corresponding discontinuous post-litigation decreases in white victimization rates.

Appendix D reports additional tests for the existence of pre-trends in Black crime victimization. Estimated slopes through pretreatment coefficients are positive although not significant at $p < 0.05$ for both the dynamic TWFE DD specification and the dynamic Sun and Abraham (2021) specification. The estimated linear violations of parallel

trends that would be detected only 80% and 50% of the time by standard tests (i.e., a significant pretreatment coefficient in the opposite direction as post-treatment coefficients) are also positive and of very small magnitude (Roth, 2022). There are no significant positive pretreatment coefficients on Black crime victimization in dynamic TWFE DD specifications using a sample restricted to those MSAs and years for which pre-litigation data are available between 1979 and 1985. There is no evidence of pre-litigation downward trends in Black crime victimization.

Our ability to explore pre-trends in Black crime victimization is limited by the relatively short span of time for which we have pre-litigation victimization data. However, the inference of a relative absence of pre-trends before the onset of employment discrimination litigation is supported by other work in similar contexts using data series with longer periods of pre-litigation availability. For example, McCrary (2007) found no pre-trends in Black officer employment shares, crime rates, arrest rates, or clearance rates prior to the onset of employment discrimination litigation. Miller and Segal (2012) likewise found no evidence of pre-litigation trends in Black officer employment during the ten years prior to the onset of employment discrimination litigation. Cox et al. (2022) similarly found no pre-trends in nonwhite police killings prior to the initiation of employment discrimination litigation.

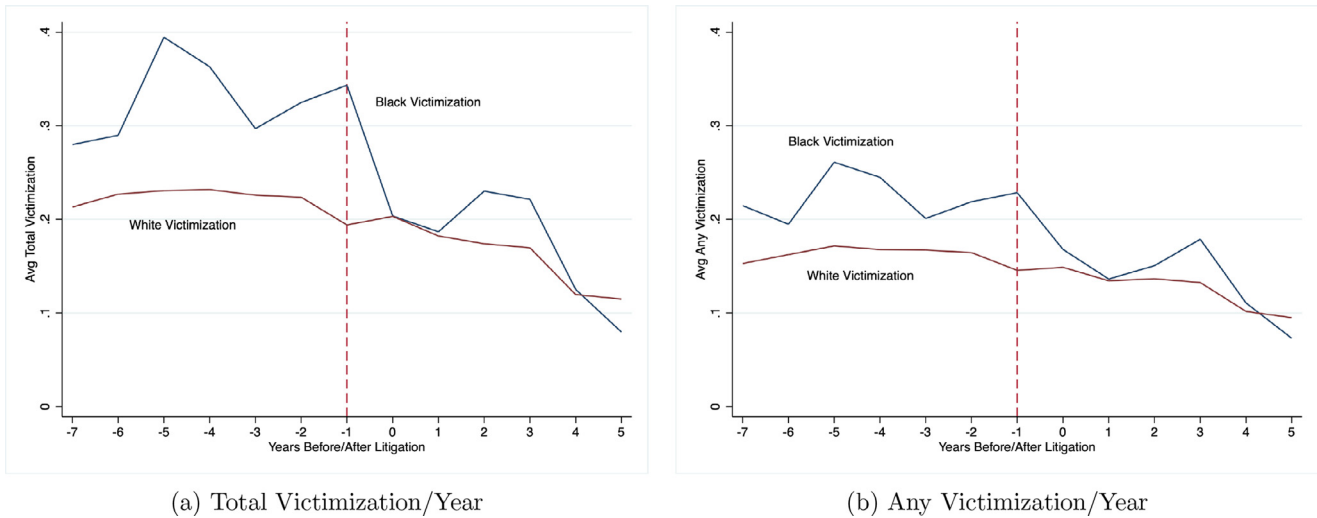


Fig. 4. Black and white victimization rates, treated MSAs 1979–1985.

In short, the lack of observable pretreatment differences in our data (in both levels and trends) across treated MSAs that varied only in treatment timing suggests that not-yet-treated MSAs may serve as a comparable control group for newly-treated MSAs (Johnson, 2015; Deshpande and Li, 2019). The relative absence of pre-trends before the onset of employment discrimination litigation in other work is supportive of this identification strategy.

4.2. Estimating treatment effects

We want to estimate post-litigation changes in crime victimization rates in litigated MSAs, relative to changes in crime victimization rates in MSAs that have not yet experienced 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.

Several recent papers have explored the potential for bias in the two-way fixed effect difference-in-differences (TWFE DD) estimator of average treatment effects on the treated (ATT), particularly in the context of staggered treatment timing and heterogeneous treatment effects (Borusyak et al., 2022; Callaway and Sant’Anna, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2021; de Chaisemartin and D’Haultfœuille, 2022). The variance-weighted ATT 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). The VWATT can differ substantially from the true ATT in the presence of staggered treatment timing and treatment effect heterogeneity, particularly when already-treated units are allowed to serve as controls for newly-treated units (Borusyak et al., 2022; Callaway and Sant’Anna, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2021; de Chaisemartin and D’Haultfœuille, 2022). This is of particular concern in our sample of treated MSAs, most of which were litigated for race-based employment discrimination in law enforcement prior to the onset of the NCVS survey in 1979.

We first explore the potential for bias in the TWFE DD estimator using the diagnostic developed by Goodman-Bacon (2021) and the full sample of treated MSAs (including MSAs treated both before and after 1979), and the full set of years between 1979 and 2004, collapsing the victimization data to the MSA/year level in order to satisfy the diagnostic’s requirement of a balanced panel. Table 2 reports for this sample the subgroup point estimates and weights for the effect of litigation on victimization from the Goodman-Bacon (2021) difference-in-differences decomposition model. Point estimates of the effects of lit-

Table 2

TWFE DD Goodman-Bacon (2021) decomposition estimates, treated MSAs 1979–2004.

	White Victimization	Black Victimization	Racial Gap in Victimization
Total Victimization/Year			
	Beta/Weight	Beta/Weight	Beta/Weight
Timing Groups	-0.02/0.07	-0.11/0.03	-0.06/0.03
Always v. Timing	0.00/0.92	-0.07/0.95	-0.04/0.95
Within	0.06/0.01	-0.08/0.02	-0.09/0.02
Any Victimization/Year			
	Beta/Weight	Beta/Weight	Beta/Weight
Timing Groups	-0.01/0.07	-0.06/0.03	-0.03/0.03
Always v. Timing	0.00/0.92	-0.03/0.95	-0.02/0.95
Within	0.02/0.01	-0.08/0.02	-0.08/0.02
N	724,746	166,770	891,516

Standard errors clustered on MSA. All models include MSA and year fixed effects using the *bacondecomp* Stata 16 package developed by Goodman-Bacon et al. (2019).

igation on both absolute and relative Black victimization for the subgroups using always-treated MSAs as a control group (“Always v. Timing”) are smaller in absolute value and more heavily weighted than the comparisons between MSAs treated after 1979 (“Timing Groups”), indicating the potential for downward bias in the TWFE DD estimator. The subgroup point estimates reported for both absolute and relative Black crime victimization are, however, consistently negative.

Table 3 reports several estimates of average treatment effects, including the TWFE DD estimator as well as estimators developed by Borusyak et al. (2022), Callaway and Sant’Anna (2021), de Chaisemartin and D’Haultfœuille (2022) and Sun and Abraham (2021). The TWFE DD estimator for the full sample of MSAs treated both before and after 1979, for the full range of years available in the NCVS (1979–2004), reported in the top row of Table 3, indicate that litigation reduced total Black victimization per person per year by 21% (p < 0.01) and any Black victimization per person per year by 14% (p < 0.01). There are no effects of litigation on white victimization distinguishable from zero.

The ATT estimators developed by Borusyak et al. (2022), Callaway and Sant’Anna, 2021, de Chaisemartin and D’Haultfœuille (2022), and Sun and Abraham (2021), designed to address the potential for bias in the TWFE DD estimator, exclude always-treated units from the sample. Including those units adjusts the path of outcomes for newly-treated units by the path of outcomes

Table 3
Effect of employment discrimination litigation on crime victimization, treated MSAs.

	Black				White	
	Total Victimization/Year		Any Victimization/Year		Total Victimization/Year	Any Victimization/Year
Full Sample, 1979–2004						
TWFE	-0.07*** (0.02)	21%	-0.03*** (0.01)	14%	-0.00 (0.02)	0.00 (0.01)
N	166,770		166,770		724,746	724,746
MSAs Litigated After 1979, 19 Years Post-Treatment						
TWFE	-0.09** (0.03)	27%	-0.05** (0.02)	23%	-0.02 (0.02)	-0.01 (0.01)
N	17,038		17,038		125,410	125,410
MSAs Litigated After 1979, 1979–1985						
TWFE	-0.06 (0.04)	18%	-0.04* (0.02)	18%	-0.01 (0.02)	-0.01 (0.01)
Borusyak et al (2021)	-0.08*** (0.01)	24%	-0.05*** (0.01)	23%	-0.02 (0.02)	-0.02* (0.01)
Callaway and Sant’Anna (2020)	-0.09*** (0.02)	27%	-0.05*** (0.01)	23%	-0.01 (0.02)	-0.01 (0.01)
de Chaisemartin and D’Haultfoeuille (2021)	-0.08** (0.04)	24%	-0.04* (0.03)	18%	-0.01 (0.03)	-0.01 (0.01)
Sun and Abraham (2020)	-0.08*** (0.01)	24%	-0.05*** (0.01)	23%	-0.01 (0.02)	-0.01 (0.01)
N	5885		5,885		45,866	45,866
Pretreatment DV Mean	0.33		0.22		0.22	0.16

* p<.10, ** p<.05, *** p<.01. Standard errors clustered on MSA. Details on estimators reported in Appendix G.

for already treated units. As such, the counterfactual is not untreated potential outcomes, but treatment effect dynamics, making estimates difficult to interpret. In our sample, excluding always-treated MSAs requires dropping the 21 MSAs litigated before 1979, leaving five MSAs litigated between 1979 and 1986. If we further restrict the sample to the first 19 years post-litigation (inclusive of the year of litigation onset), all post-treatment estimates are constructed using the same sample of five MSAs. The second panel of Table 3 reports the TWFE estimates for this sample of MSAs, indicating that litigation reduced total Black victimization per person per year by 27% (p < 0.05) and any Black victimization per person per year by 23% (p < 0.05). There are again no effects of litigation on white victimization distinguishable from zero.

The estimators developed by Borusyak et al. (2022), Callaway and Sant’Anna (2021), de Chaisemartin and D’Haultfoeuille (2022), and Sun and Abraham (2021) also exclude years after which all units have been treated, as counterfactual outcomes for these years cannot be estimated from not-yet-treated units. Constructing a sample for these estimators thus requires that we further restrict the sample to the four MSAs treated before 1986, observed between 1979 and 1985. The third panel of Table 3 first reports the TWFE estimates for this sample of MSAs, indicating that litigation reduced total Black victimization per person per year by 18% (not significant) and any Black victimization per person per year by 18% (p < 0.10). There are once again no effects of litigation on white victimization distinguishable from zero.

The third panel of Table 3 then reports for this sample the estimates from the four estimators that further take into account potentially heterogeneous treatment effects.¹¹ The Borusyak et al. (2022) estimates indicate that litigation reduced total Black victimization per person per year by 24% (p < 0.01) and any Black victimization per person per year by 23% (p < 0.01). The Callaway and Sant’Anna (2021) estimates indicate that litigation reduced total Black victimization per person per year by 27% (p < 0.01) and any Black victimization per person per year by 23% (p < 0.01). The de Chaisemartin and D’Haultfoeuille (2022) estimates indicate that litigation reduced total Black victimization per person per year by 24% (p < 0.05) and any Black victimization per person

per year by 18% (p < 0.10). The Sun and Abraham (2021) estimates indicate that litigation reduced total Black victimization per person per year by 24% (p < 0.01) and any Black victimization per person per year by 23% (p < 0.01). There are no consistent effects of litigation on non-Hispanic white victimization.

Prior to litigation onset, Black NCVS respondents in these MSAs experienced 50% more total crime victimization, relative to white respondents (33% vs. 22%), and were 38% more likely to experience any crime victimization (22% vs. 16%). These estimates indicate that litigation reduced the pretreatment racial gap in total victimization by 8 - 9 percentage points or by 73% - 82%, and reduced the racial gap in any victimization by 4 - 5 percentage points or by 67% - 83%. As further discussed in Appendix E, while previous work found no effects of employment discrimination litigation on offenses known to law enforcement, as reported in the FBI’s Uniform Crime Reporting (UCR) data (McCrary, 2007), our findings reveal racially heterogeneous effects obscured by the lack of information on victim race in the UCR data.

4.3. Heterogeneous treatment effects

Litigation onset may have had heterogeneous effects on different kinds of victimization. As seen in Table 1, Black respondents in treated MSAs pretreatment had substantially higher rates of both reported victimization and victimization unreported to police due to lack of trust in the police response, relative to white respondents. Police neglect of Black crime victimization during pretreatment years may have been at least partially responsible for these racial disparities in victimization. By inducing greater police responsiveness to Black crime victimization, litigation onset may have reduced these racial disparities in victimization. By contrast, Black respondents in the same MSAs had approximately equal rates of victimization unreported to the police for other reasons, including that the victimization was insufficiently important to the respondent to warrant reporting, relative to white respondents. Victimization that was likely relatively unaffected by racially disparate police behavior during the pre-litigation period may also have been little affected by litigation-induced changes in the police response to Black crime victimization.

¹¹ For details on the construction of these estimators see Appendix G.

Table 4
Effect of litigation on reported victimization, all respondents, treated MSAs 1979–1985.

	Black				White	
	Total Victimization/Year		Any Victimization/Year		Total Victimization/Year	Any Victimization/Year
Borusyak et al (2021)	-0.03*** (0.00)	23%	-0.01*** (0.00)	9%	-0.02*** (0.01)	-0.02*** (0.00)
Callaway and Sant’Anna (2020)	-0.03*** (0.01)	23%	-0.02*** (0.00)	18%	-0.02*** (0.00)	-0.02*** (0.00)
de Chaisemartin and D’Haultfœuille (2021)	-0.03 (0.03)	23%	-0.01 (0.02)	9%	-0.02** (0.01)	-0.01 (0.01)
Sun and Abraham (2020)	-0.03*** (0.01)	23%	-0.02** (0.00)	18%	-0.02** (0.01)	-0.01** (0.00)
N			5,885		45,866	45,866
Pretreatment DV Mean	0.13		0.11		0.07	0.06

* p<.10, ** p<.05, *** p<.01. Standard errors clustered on MSA. Details on estimators reported in Appendix G.

Table 5
Effect of litigation on victimization unreported due to police mistrust, all respondents, treated MSAs 1979–1985.

	Black				White	
	Total Victimization/Year		Any Victimization/Year		Total Victimization/Year	Any Victimization/Year
Borusyak et al (2021)	-0.04*** (0.01)	44%	-0.04*** (0.01)	50%	-0.00 (0.01)	-0.01 (0.00)
Callaway and Sant’Anna (2020)	-0.05*** (0.02)	56%	-0.04** (0.02)	50%	-0.00 (0.01)	-0.00 (0.00)
de Chaisemartin and D’Haultfœuille (2021)	-0.04* (0.03)	44%	-0.03* (0.02)	38%	-0.00 (0.01)	-0.00 (0.01)
Sun and Abraham (2020)	-0.05** (0.01)	56%	-0.04** (0.01)	50%	-0.01 (0.00)	-0.00 (0.01)
N			5,885		45,866	45,866
Pretreatment DV Mean	0.09		0.08		0.05	0.04

* p<.10, ** p<.05, *** p<.01. Standard errors clustered on MSA. Details on estimators reported in Appendix G.

Table 6
Effect of litigation on victimization unreported for other reasons, all respondents, treated MSAs 1979–1985.

	Black				White	
	Total Victimization/Year		Any Victimization/Year		Total Victimization/Year	Any Victimization/Year
Borusyak et al (2021)	-0.01 (0.01)		-0.00 (0.01)		0.01 (0.01)	0.01* (0.00)
Callaway and Sant’Anna (2020)	-0.01 (0.01)		-0.00 (0.01)		0.01 (0.01)	0.01*** (0.00)
de Chaisemartin and D’Haultfœuille (2021)	-0.01 (0.03)		-0.00 (0.02)		0.01 (0.01)	0.01 (0.01)
Sun and Abraham (2020)	-0.01 (0.01)		-0.00 (0.01)		0.00 (0.01)	0.01 (0.00)
N			5,885		45,866	45,866
Pretreatment DV Mean	0.10		0.08		0.09	0.08

* p<.10, ** p<.05, *** p<.01. Standard errors clustered on MSA. Details on estimators reported in Appendix G.

Tables 4 - 6 report estimates of the effect of litigation onset on victimization reported to the police, victimization unreported due to lack of trust in the police response, and victimization unreported for other reasons, using the estimators developed by Borusyak et al. (2022), Callaway and Sant’Anna (2021), de Chaisemartin and D’Haultfœuille (2022), and Sun and Abraham (2021) and the sample of MSAs litigated after 1979 and observed between 1979 - 1985.

As reported in Table 4, total Black victimization that was reported to the police decreased by an estimated 3 percentage points post-litigation, contributing 33% - 38% of the overall reduction in Black crime victimization, and representing a 23% post-litigation decrease in the pretreatment mean of reported Black crime victimization. The proportion of Black respondents who experienced any victimization that was reported to the police likewise decreased by 1 - 2 percentage points post-litigation, contributing 20% - 40% of the overall reduction in the proportion of Black respondents experiencing crime victimization, and rep-

resenting a 9% - 18% decrease in the pretreatment mean proportion of Black respondents experiencing crime victimization that was reported to the police.

As reported in Table 5, total Black victimization not reported because of lack of trust in the police response decreased post-litigation by an estimated 4 - 5 percentage points, constituting 50% - 63% of the overall reduction in Black crime victimization, and representing a decrease of 44% - 56% relative to the pretreatment mean of Black crime victimization unreported because of lack of trust in the police response. This post-litigation decrease eliminated the pretreatment racial disparity in crime victimization unreported due to lack of trust in the police response. Likewise, the proportion of Black respondents who experienced any victimization that was not reported because of lack of trust in the police response decreased by 3 - 4 percentage points, constituting 75% - 80% of the overall reduction in the proportion of Black respondents experiencing crime victimization, and representing a decrease of 39% -

50% relative to the pretreatment mean proportion of Black respondents experiencing crime victimization unreported because of lack of trust in the police response. This decrease likewise eliminated the pretreatment racial disparity in the proportion of individuals experiencing crime victimization unreported due to lack of trust in the police response.¹²

By contrast, as reported in Table 6, there are no discernible impacts of litigation on Black victimization unreported to the police for other reasons. These estimates indicate that victimization that was likely relatively unaffected by racially disparate police behavior during the pre-litigation period was little affected by litigation-induced changes in the police response to Black crime victimization. Overall, these estimates provide further evidence that police neglect of Black crime victimization during pretreatment years may have been at least partially responsible for pretreatment racial disparities in victimization, and that litigation-induced increased police responsiveness to Black crime victimization may have reduced these racial disparities in victimization.

Finally, the estimates reported in Tables 4 - 6 indicate that white crime victimization reported to the police saw small decreases after litigation onset, but that these decreases were offset by small increases in white crime victimization not reported to the police for reasons other than lack of trust in the police response, leading to no overall post-litigation changes in white crime victimization (as reported in Table 3).

4.4. Causal mechanisms

Previous work using restricted EEOC data found increases in Black officer shares emerging approximately five years after the onset of race-based employment discrimination litigation (McCrary, 2007; Miller and Segal, 2012). We see sharp decreases in Black (but not white) crime victimization in the first year after litigation onset, before any likely increases in Black officer shares. The timing of the effects we observe is consistent with efforts by litigated departments to address racial disparities in the police response to reported crime immediately after litigation onset, perhaps in response to plaintiffs' attempts to claim department-wide practices of intentional race-based discrimination. This mechanism is also consistent with the responsiveness of officer behavior to litigation-induced interventions in other contexts (Mummolo, 2018; Rivera and Ba, 2022; Cox et al., 2022). It is possible that later post-litigation increases in Black officer shares may have contributed to further decreases in Black crime victimization, although, given the limited nature of publicly available data on officer race, we do not explore that hypothesis here.¹³

Given the nature of our data, we are largely unable to explore the specific changes in officer behavior that could account for the effect of race-based employment discrimination litigation on Black crime victimization, including increased patrol deployment to neighborhoods with more Black residents, shorter response times to calls for service from Black crime victims, and increased effort devoted to recording, investigating, and clearing cases with Black crime victims. Future work may be able to shed light on the contributions of these mechanisms to racial inequities in policing.

¹² As we discuss below, our data do not permit us to separately identify the effects of litigation on the incidence of victimization and on the propensity of victims to report their victimization to the police. It is possible that some of the decrease in Black crime victimization unreported due to lack of trust in the police response is attributable to increases in the reporting of victimization, rather than to decreases in the incidence of victimization. Likewise, it is possible that we would see even larger decreases in reported Black crime victimization if we could identify increases in reporting due to litigation. Further work with different data sources will be necessary to separately identify the effects of police behavior on victimization and the reporting of victimization.

¹³ The publicly available data on law enforcement employment by race, reported by the Bureau of Justice Statistics' Law Enforcement Management and Administrative Statistics (LEMAS) survey, are not available until 1987, after all MSAs in the litigated sample had been litigated.

Miller and Segal (2018) suggested 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, conditional on an incident of violence occurring, and attributed post-litigation decreases in the incidence of gender-based violence at least in part to this increased reporting. It is likewise possible that employment discrimination litigation, by increasing the responsiveness of police departments to Black crime victimization, increased the trust of Black crime victims in the likely police response, thereby increasing their willingness to report victimization to the police. However, the reporting of victimization is a post-treatment outcome. The treatment effect of litigation on reporting is undefined for those respondents who would experience victimization if not treated but not if treated (compliers), those who would not experience victimization if not treated but would if treated (defiers), and those who would not experience victimization under either treatment condition (never-takers) (Coppock, 2019). The treatment effect of litigation on reporting is defined only for those respondents who would experience victimization under both conditions (always-takers), but we lack the information necessary to identify the always-takers. Our data and design do not permit us to investigate the possible contribution of victimization reporting to post-litigation decreases in Black crime victimization.

A large body of literature indicates that an increased police presence reduces reported crime (Klick and Tabarrok, 2005; Di Tella and Schargrodsky, 2004; Draca et al., 2011). If litigated agencies hired more police officers post-litigation, perhaps because of efforts to hire more Black officers, the additional officers may have contributed to post-litigation decreases in Black crime victimization. We source agency-level data on numbers of sworn officers from the FBI's LEOKA data series, and aggregate agency-level numbers of officers and agency-level population served to the MSA/year level. Table F.1 reports estimates of the effect of litigation on number of officers per 100,000 in population for the treated MSAs. There is no effect of litigation on numbers of sworn officers.

Litigation for race-based employment discrimination may have induced agencies to exert greater effort to make arrests of perpetrators involved in Black crime victimization. Increased arrests and clearances in cases of Black crime victimization may in turn have contributed to increased deterrence and reduced Black victimization rates. However, UCR data largely prohibit us from exploring this causal mechanism. As discussed in greater detail in Appendix E, offenses reported to law enforcement do not report race of victim. Clearances, which include both arrests and clearances through other means, also do not report race of victim in the cleared case. Although UCR arrest data record race of arrested person, they do not record race of victim. We are thus unable to explore heterogeneous treatment effects on arrest and clearance rates by victim race. In addition, increased police effort to patrol Black neighborhoods may have increased deterrence and reduced Black crime victimization even without increased arrest and clearance rates (Di Tella and Schargrodsky, 2004; Klick and Tabarrok, 2005; Draca et al., 2011).

We nonetheless estimate the impact of litigation onset on clearance and arrest rates, the latter both overall and by race of arrested person. We aggregate total numbers of arrests, both overall and by arrested person race, the total number of offenses known to law enforcement, and the total number of cleared offenses (including offenses cleared by arrest and by other means) 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. Arrest rates are defined as total arrests per 100,000 in population, arrests of Black individuals/100,000 in Black population, and arrests of white individuals/100,000 in white population. Clearance rates are defined as total cleared offenses/total offenses. Table F.1 reports estimates of the effect of litigation on arrest and clearance rates for the treated MSAs. There is no discernible effect of litigation on arrest or clearance rates.

5. Conclusion

Black civilians in the United States are in many jurisdictions subjected to the racially disparate use of enforcement actions by police officers, including the use of citations, fines, and force (Goncalves and Mello, 2021; Hoekstra and Sloan, 2022; West, 2018). 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 at least in part to racially disparate police responses to victimization. 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 between 1970 and 1986. 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 and deterring crime experienced by Black victims, leading to lower Black victimization rates.

Using estimators developed by Borusyak et al. (2022), Callaway and Sant'Anna (2021), de Chaisemartin and D'Haultfoeuille (2022), and Sun and Abraham (2021), we find that litigation reduced total Black crime victimization by 24 - 27%, and reduced the proportion of Black respondents who experienced crime victimization by 18 - 23%, with no discernible effects on white crime victimization. Prior to litigation onset, Black NCVS respondents in these MSAs experienced 50% more total crime victimization, relative to white respondents, and were 38% more likely to experience any crime victimization. Our estimates indicate that litigation reduced the pretreatment racial gap in total victimization by 73% - 82%, and reduced the racial gap in any victimization by 67% - 83%. We explore heterogeneous treatment effects, finding that litigation onset reduced Black crime victimization only for categories of crime victimization exhibiting pretreatment racial disparities, further suggesting that these pretreatment racial disparities in crime victimization were driven at least in part by a racially disparate police response.

Prior work has found no effects of the litigation of law enforcement agencies for racially discriminatory employment practices on numbers of offenses known to law enforcement (McCrary, 2007). However, the UCR data on offenses known to law enforcement do not identify victim race. The findings reported here suggest that the absence of victim race in the UCR data on offenses known to law enforcement obscures racially disparate effects of police behavior on crime victimization.

Our findings suggest that one source of racial disparities in crime victimization lies in racially discriminatory police responses to crime victimization. Departments facing the threat of judicial intervention over race-based employment discrimination appear to increase their responsiveness to Black crime victimization, reducing the incidence of Black crime victimization without increasing white crime victimization. Although we lack data on the specific departmental practices that may have changed as a result of litigation onset, including patrol deployment to neighborhoods with more Black residents, response times to calls for service from Black crime victims, and effort devoted to recording, investigating, and clearing cases with Black crime victims, our findings do suggest that officers' responses to reports of crime victimization be-

came less discriminatory after litigation onset. These findings indicate a need for further research on police practices that can support greater racial equity in the provision of public safety.

CRedit authorship contribution statement

Anna Harvey: Conceptualization, Methodology, Data curation, Software, Validation, Formal analysis, Investigation, Writing – original draft, Writing – review & editing, Visualization, Supervision, Project administration. **Taylor Mattia:** Data curation, Software.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jue.2022.103459

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