The Impact of Affirmative Action Litigation on Police Killings of Civilians*

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Abstract

Although research has shown that court-ordered hiring quotas increase the number of minority police officers in litigated cities, there has been little insight into how workforce diversity, or lack thereof, may impact police violence. Using an event-study framework, we find that the threat of affirmative action litigation reduces police killings of non-white civilians in the long-run. In addition, we find evidence of lower arrest rates for non-white civilians and more diverse police departments 25 years after litigation. Our results highlight the vital role that federal interventions have in addressing police behavior and the use of lethal force.

Keywords: affirmative action, excessive use of force, police shootings, race
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1 Introduction

Questions regarding the efficacy of policing, particularly as it pertains to the disproportionate use of excessive force towards minorities, are of critical concern in the United States. Police violence is a leading cause of death for young men following accidents, suicide, other homicides, heart disease, and cancer (Edwards et al., 2019). Racial minorities have a greater lifetime risk of being killed by police: Black men and women are 2.5 and 1.4 times more likely, respectively, to die because of police violence than their white counterparts (Edwards et al., 2018). After more than fifty years of protests in response to police violence in Black communities, the conversation has shifted from police reform to abolition. While most Americans agree that improvements are necessary to address gross inequities in policing (Shannon, 2020), there is considerable disagreement about how to transform the police. Abolitionists recommend defunding the police and redirecting those funds to social programs devoted to easing the burdens of poverty and disenfranchisement. Reformists argue for greater incorporation of social programs and initiatives within the current structure of policing, which would likely increase funding for public safety (Bell, 2016). For most cities, spending on public safety as a share of total expenditures has remained approximately 11 to 14 percent since the 1960s.[1]

Historically, police reform has centered around three themes: 1) diversity training, 2) transparency and oversight, and 3) more diversity among police officers. For example, the President's Task Force on 21st Century Policing recommended that police departments emphasize diversity in the workplace (Commission and on Civil Disorders, 1968; on 21st Century Policing, 2015). As early as 1968, the Kerner Commission[2] recognized the contentious relationship between the Black community and mostly white police departments as a primary cause of the civil unrest in the 1960s; it explicitly advised local municipalities to "recruit more Negroes into the regular police" as a potential solution (Commission and on Civil Disorders, 1968). Following the Kerner Commission, the National Advisory Commission on Criminal Justice Standards and Goals (1973) advised that "every police agency that has racial or minority groups of significant size in its jurisdiction ensure that the needs of minorities are actively considered in the establishment of police policy

[1] See Appendix Figure B1.

[2] The Kerner Commission Report was published in February 1968, two months before the assassination of Dr. Martin Luther King, Jr. and the Passage of the Civil Rights Act of 1968.

and the delivery of police service. Affirmative action should be taken to achieve a proportion of minority group employees in an agency that is an approximate proportion of their numbers in the population."

Beginning in 1969, municipal police departments experienced one of the most aggressive affirmative action programs across the country in the form of court-ordered hiring quotas. The quotas increased the number of Black officers (and women, to a lesser extent) hired by police departments (McCrary, 2007; Miller and Segal, 2012). Although police agencies became more diverse as a result of affirmative action hiring, there is little evidence that diversity has impacted police productivity (McCrary, 2007; Garner et al., 2019). Nonetheless, prior research has found that police performance may vary by race and gender. Empirical evidence shows that Blacks and Hispanics are less likely to be arrested (Donohue III and Levitt, 2001), and Blacks and Hispanics are subject to fewer stops (Close and Mason, 2007), when there is more diversity within a police force. Although minority police officers conduct a smaller percentage of vehicle searches, they also have greater rates of success, suggesting that minority cops may be able to identify non-white criminals more effectively (Close and Mason, 2007).

Moreover, Hoekstra and Sloan (2020) find that white officers use force 60 percent more than Black officers and use gun force twice as often, particularly when dispatched to neighborhoods with a large fraction of Black residents. Female police officers are similarly less likely to engage in excessive use of force (McElvain and Kposowa, 2008; Rabe-Hemp, 2008; Smith, 2003). Police departments with affirmative action plans are also associated with lower crime victimization rates for Black and white residents (Harvey and Mattia, 2019), with a much larger decrease for Black residents. There is also evidence that affirmative action plans decrease intimate partner homicides for men and women, increase reports of violence against women, and decrease the rate of non-lethal domestic violence (Miller and Segal, 2019).

Despite the potential benefits of affirmative action policies to lower racial disparities in policing outcomes, few studies have rigorously analyzed their effect on racial disparities in the most severe and arguably the most costly use of force—police killings of civilians. This is especially important as affirmative action litigation and police diversity was not a crime prevention strategy but a riot prevention policy, with the goal of reducing police violence, improving police-community relations, and reducing the likelihood of future violent demonstrations in Black communities.

Crime prevention or police productivity was a second-order concern. This study will fill that gap by examining the effect of the threat of affirmative action on excessive use of force, as measured by deaths due to legal intervention.[3] Following McCrary (2007), we analyze the threat of court-ordered affirmative action plans between 1969 and 2000. Although Title VII of the Civil Rights Act of 1964 includes provisions for remediating grievances related to discrimination, a series of executive orders and amendments to the Civil Rights Act expanded the scope of enforcement and the criteria for affirmative action implementation. After the passage of the Civil Rights Act of 1968, there was a sharp increase in the number of civil rights cases brought through private litigation to U.S. District Courts (Farhang, 2010).[4]

Our analysis takes advantage of variation in the location and timing of when a city within a county is threatened with affirmative action litigation. We implement a Difference-in-Difference (DiD) research design within an event-study framework to test if the trends in deaths due to legal intervention change in response to litigation. The data on deaths due to legal intervention comes from the Vital Statistics, which provide information on cause of death and the decedent's county of residence. The event-study design allows us to test the common trends assumption and visualize the effect of police killings of civilians over time. To do so, we confirm previous research findings by providing suggestive evidence that the threat of court-ordered affirmative action quotas increases the employment of Black police officers (McCrary, 2007; Miller and Segal, 2012). Our results are in line with Miller and Segal (2012), which finds that litigated departments increase their Black employment shares, but at a lower rate than those facing court-ordered affirmative action.

The potential effect of affirmative action litigation on police killings of non-white civilians is ambiguous. On the one hand, the threat of affirmative action could change the morale and behavior of currently employed police officers. Historically, police departments have been predominantly white, male, and resistant to change. Black professional police organizations seeking changes in employment opportunities initiated most of the lawsuits associated with affirmative action litigation (McCrary, 2007). Therefore, the threat of litigation could have caused feelings of

[3] We use police killings and deaths due to legal intervention interchangeably.

[4] Although the initial Civil Rights Act was passed in 1968, it was Executive Order 11478 of 1969 that prohibited the federal government from considering race in hiring, and the 1972 amendments to the Civil Rights Act that extended non-discriminatory practices to state and local governments.

animus towards minority groups due to the prospect of a changing work environment. Consequently, threat of an affirmative action lawsuit and/or expected changes in workforce composition may anger a white male-dominated profession (Hidalgo, 2019), leading to more killings of non-white civilians with no change in killings of white civilians. Devi and Fryer Jr (2020) show that police officers become less engaged when police departments are involved in a federal investigation. Similarly, Ba and Rivera (2019) demonstrate that public or known investigations of police departments alter community-police relations. Thus, federal intervention into local police departments could change police behavior, even without changing the racial composition of law enforcement.

On the other hand, such a threat may also lead to the preemptive hiring of more female officers and more racial and ethnic minorities to win lawsuits, leading to greater diversity. To the extent that police departments become more representative of their constituents, we would expect the hostility between police departments and communities of color to decrease, leading to a decrease in negative interactions between the police and the community and fewer non-white deaths. Moreover, research suggests that racial stereotypes are used to determine the presence of threats and whether to shoot when decisions need to be made very quickly (Correll et al., 2011, 2002). Although it is not clear whether racial and ethnic minorities will hold fewer racial stereotypes than white officers, exposure to racially diverse peers may lead to improved perception of, or more favorable interactions with, members of minority groups (Carrell et al., 2019). Thus, we would expect police killings of non-whites by white officers to decrease after their departments grow more diverse. Moreover, a reduction in both black and white police killings may result if racial and ethnic minorities are more likely to view racial and ethnic neighborhoods within a less threatening context (Correll et al., 2011). Relatedly, if minority police officers are better at identifying and deescalating violent situations among non-white criminals, we would also expect police killings of non-white civilians to decline. Lastly, if minority police officers are less likely to use force in general, it is reasonable to expect police killings of white civilians to decrease as well (Hoekstra and Sloan, 2020).

Our findings indicate that the threat of affirmative action results in fewer deaths of non-white civilians from legal intervention in the long-run. Our results suggest that by the year 2000 litigated counties averted roughly 60 non-white deaths at the hands of law enforcement per year.

We consider the changing composition of police departments as a potential mechanism and provide evidence that affirmative action litigation increases the racial and gender representation of police departments in the long-run, consistent with previous research (McCrary, 2007; Miller and Segal, 2012). We also find lower property crime arrest rates for non-white civilians and no impact on violent crime arrests, suggesting changes in the frequency of police-civilian contact as another possible mechanism. In addition, we find evidence of short- and long-run decreases in police killings of white civilians. Although our findings indicate long-run declines in both white and non-white police killings, we also find that the threat of affirmative action leads to a relative increase in non-white killings in the initial year of litigation.

We check the validity of our results using ordinary least squares, weighted least squares, and Poisson estimators. We also execute a series of robustness checks that restrict the sample to litigated counties, large counties, highly urbanized counties, and counties that experienced uprisings. The results are robust to these reasonable sample restrictions. Moreover, we do not find that the decrease in police killings is part of a larger downward trend in homicides in response to affirmative action litigation nor do we find that homicides increase due to changes in recording police-related fatalities. Our findings are consistent even in litigated locations where police departments experienced relatively minor changes in their racial composition. This result suggests that affirmative action litigation, itself, may help reduce police killings of civilians. Additionally, we show that the timing of litigation has little impact on the long-run effects, as counties treated both before and after 1980 report long-run decreases in police killings of non-white civilians. Lastly, we employ alternative estimators proposed by Callaway and Sant'Anna (2020) within an event-study framework to address potentially biased estimated treatment effects associated with the traditional two-way-fixed effects difference-in-difference model (Goodman-Bacon, 2021). We find that both estimators produce similar estimates, indicating that our results are not biased by heterogeneity in the timing of the treatment.

Our research contributes to the growing literature on race and the use of force in general, and specifically police killings of civilians. Overall, the prevailing body of work shows that minority citizens are more likely to experience non-lethal force by the police but provide mixed evidence that a relationship exists between the race of civilians and police killings (Pleskac et al., 2018; Edwards et al., 2018; Fryer, 2019; Kahn et al., 2016; Ross, 2015). However, similar to the litera-

ture on racial profiling, empirical evidence has found that white officers are more likely to use force and to do so more aggressively on minority civilians (Headley and Wright, 2020; Hoekstra and Sloan, 2020). Aggressive policing and higher rates of lethal force stem from protections obtained through the adoption of collective bargaining rights– resulting in higher levels of police violence that disproportionately impact minorities (Cunningham et al., 2020; Dharmapala et al., 2019). However, our results indicate that greater levels of diversity can potentially prevent police killings of civilians. Moreover, we provide evidence of long-term consequences of the threat of federal interventions, via the courts, into local policing. Our paper relates closely to work by Harvey and Mattia (2019), who also examine the impact of affirmative action on the welfare of minority citizens but use a different measure of citizens' welfare, self-reported victimization. Our paper is also linked to research investigating the relationship between federal interventions and racial disparities in local policing outcomes (Cox and Cunningham, 2021; Weisburst, 2019).

As previously mentioned, entrenched hostilities between Black communities and predominantly white police departments were influential in the 1960s racial uprisings. While drawing attention to the problems between the Black community and the police, these uprisings also created a ratcheting effect, leading to an increase in police killings of both white and non-white citizens in the short-run (Cunningham and Gillezeau, 2019). As previously mentioned, one common police reform recommendation in response to these protests was diversifying local law enforcement, a common suggestion even today. Our study extends the work of Cunningham and Gillezeau (2019) by investigating whether increasing diversity of police can improve community-police relations and, thus, have an impact on racial disparities in police killings of civilians.

The remainder of this paper is organized as follows. Section 2 describes our data and methodology. Sections 3 and 4 present our primary findings, along with a series of robustness checks. Section 5 explores potential mechanisms, and Section 6 contextualizes our results. Section 7 concludes the paper.

2 Data and Methodology

2.1 Primary Data Sources

We use a variety of data sources to conduct our analysis. We collect data on civilian deaths involving law enforcement for the period 1960–2012 from the National Vital Statistics System (NVSS) Multiple-Cause of Death files (US Department of Health and Human Services, 2007). The vital statistics classify deaths by cause, age, race, and county where the death occurred. We use the Vital Statistics data to create race-specific mortality rates for deaths due to legal intervention, as well as race-specific aggregates or counts of deaths due to legal intervention.[5] The Vital Statistics data is particularly useful because it captures deaths caused by law enforcement beyond police shootings. Unfortunately, Vital Statistics data is dependent on local municipalities' reporting of police involvement and therefore grossly under-counts the number of deaths due to legal intervention (Fyfe, 2002; Loftin et al., 2003; Sherman and Langworthy, 1979). In general, government-collected data on police killings accounts for roughly 50 percent of the police-involved deaths in other non-governmental data sources on police killings (Barber et al., 2016; Feldman et al., 2017). Although a herculean effort in data collection has recently increased the number of recorded deaths, there is still a debate about the nature in which data on police killings is collected (Fryer, 2018). Recent data collected by Fatal Encounters and The Washington Post began long after the 1960s and is therefore not helpful for this analysis. Although we are unable to use more recent data sources, Appendix Figure A1 plots police killings in the NVSS and Fatal Encounters data from 2000-2016, respectively, and shows that the two closely track each other over this period. This is also true when we compare both series by treatment group status.[6]

One way to proceed with the Vital Statistics data is to assume that measurement error in the dependent variable is captured by the error term and will not bias our estimates.[7] This approach assumes that the recording of deaths due to policing over time is exogenous to the treatment. This is a strong assumption; policy parameters may be associated with the treatment, changing how

[5] We exclude deaths due to legal execution from our calculation of deaths due to legal intervention.
[6] See Appendix Figure A2.
[7] This is true for crime data in general. Myers (1980) shows that between 1970 and 1974, only 1/3 to 2/3 of all crimes were reported. In general, roughly 50 percent of all crimes are reported in the UCR. See Boggess and Bound (1997). Also, Chalfin and McCrary (2018) find measurement error in police employment data, which is more problematic because police are typically an independent variable, and measurement error, in this case, will lead to biased estimates.

local municipalities record deaths due to policing. However, it is unclear if this would lead to an increase or decrease in reporting. Nonetheless, we acknowledge the shortcomings of the Vital Statistics data, and readers should interpret our results considering these caveats.

Information on the litigated departments comes from McCrary (2007). The litigated designation arises from a series of class-action lawsuits filed across the country, beginning in 1969. This movement led to one of the most aggressive implementations of affirmative action, resulting in a substantial number of court-ordered racial hiring quotas. The dates of litigation in our data vary over time and cover the period from 1969-2000.

2.2 Descriptive Analysis

In Figure 1, we use Illinois's Cook County, where Chicago is located, to illustrate how police killings of civilians change in response to the threat of affirmative action litigation. From the figure, we see that Chicago experienced a racial uprising in 1965.[8] The aftermath of this event resulted in an increase in non-white police-related deaths (consistent with Cunningham and Gillezeau (2019)).[9] In 1970, a class-action suit was filed against the Chicago Police Department (CPD) for discriminatory practices in police hiring. The number of non-white deaths declined in the year the lawsuit was filed and continued to substantially decline the following year before the hiring quota was imposed. Once the quota was imposed, non-white police killings declined further and remained substantially lower than their pre-litigation rates. The Cook County example provides suggestive evidence that behavioral responses to federal interventions in policing—in addition to the composition of the police department—are important in understanding the dynamics of police outcomes in general and police killings of civilians in particular.

To investigate changes in police killings due to court-ordered quotas, we merge litigation data and Vital Statistics data with county demographics data provided by the Surveillance, Epidemiology, and End Results and the County and City Data Books consolidated files from the ICPSR. We remove counties that report having no non-white residents in any year between 1960 and 2012. The final sample consists of 2,985 counties, of which 75 counties are treated. Treatment status is

[8] Data on racial uprisings is from Carter (1986). For this source, a particular incident has to meet several criteria to be classified as an uprising, so the data does not capture the entire universe of possible uprisings in the 1960s.

[9] Racial uprisings peaked in 1968 with the assassination of Martin Luther King, Jr. Following the uprisings of the Long Hot Summer of 1967, the Kerner Commission was established to determine the cause of civil unrest. The commission published its report in 1968—almost two months prior to King's assassination—and found that hostile community–police relations were a major contributor to the start of the uprisings.

denoted by having at least one city in the county in which a discrimination suit was filed. The timing of the treatment is observed by the year the first city within a county enters into litigation. Once a city is treated, the treatment status does not change over time. Thus, we do not change the treatment status (1) if another city in the county is treated or (2) if the treated municipality does not implement an affirmative action plan.

This highlights a limitation of our study: the litigation data is at the police department level, but our outcome measures are only available at the county level. Thus, if a county contains a litigated department, we treat the entire county as litigated. Although this is not ideal, from Figure 1, we see that the relationship between litigation, the share of new Black hires, and police killings can persist at the county level. However, we cannot rule out the possibility that other police departments in the county also change their behavior in response to the treatment. Figure 2 presents a map that contains the counties with a litigated city over the time period in our sample. As one would expect, many of the litigated counties are in the South; however, we do see a non-trivial number in the Midwest and Northeast. Between 1969 and 2000, 75 counties are treated (i.e., litigated), amounting to about 2.5 percent of the counties in our sample but covering 32 percent of the U.S. population.[10] Specifically, over 46 percent of the non-white population in 1960 resided in these locations. By 1977, roughly 40 percent of the non-white population lived in a county with at least one city involved in a dispute over minority police employment.[11]

The relationship between federal involvement in local policing and police killings is also evident during the 1970s. In Appendix Figure B2, we see that in the U.S., the number of non-white deaths per 100,000 non-white residents at the hand of police decreased drastically after the Civil Rights Act of 1968, coinciding with subsequent increases in the number of court-ordered racial hiring quotas. There appears to be relatively little change in the killing of white civilians over time. While Figure 1 provides anecdotal evidence that federal involvement influences police behavior, we see stark changes in police killings by treatment status in Appendix Figure B3. Locations that were eventually treated reported much higher police killings of civilians prior to the Civil Rights Act of 1968. This is true for both non-white and white civilians. Both treated and control counties reported rising deaths due to policing prior to 1968; however, the increase is more pronounced

[10] See Table 1 for a distribution of litigation over time.
[11] See Figure 3.

in the treated group. After 1968, there is a decline in police killings resulting in a convergence in police killings between the treatment and control groups. By 1977, the gap had significantly narrowed between treated locations and the control group; the two groups reported the same rate of non-white police killings by the 2000s.

This difference in police killings between the treated and control groups becomes even more evident when we directly compare their relative changes over time. Panel (a) of Figure 4 normalizes per capita non-white deaths due to legal intervention to zero in 1968. This also allows for a more direct comparison between the treated and control groups. It is clear that police killings rose faster in treated locations before 1968 compared to counties in the control group. Although this may have to do with a law-and-order response to racial uprisings, it is highly unlikely since many of the locations where uprisings occurred experienced their first racial uprising in 1968 (Cunningham and Gillezeau, 2019). Moreover, counties in both the treated and control groups experienced a decrease in police killings of civilians after 1968. This is important because it highlights that the control group captures trends in police killings and can plausibly serve as a control group. Lastly, there is a significant drop in police killings of non-white civilians in the treatment group compared to the control group. The decline in police killings becomes starker in panel (b) of Figure 4, which plots the normalized difference in non-white deaths between the two groups. This is consistent with Devi and Fryer Jr (2020) and Ba and Rivera (2019), who find that public or federal intervention influences police behavior. Although they suggest that public or known federal interventions change police performance and decrease productivity, we present suggestive evidence that federal intervention may reduce police killings of non-white civilians in the long-run.

Table 2 presents county-level summary statistics for the variables used in our analysis for the year 1960. Column (1) shows the mean for all counties in the sample. Columns (2) and (3) provide mean characteristics for the treated (i.e., litigated) and non-treated counties, respectively. This basic comparison reveals significant cross-sectional differences between the treatment and control groups, shown by the p-values reported in column (4). Litigated counties are more populous, more educated, and have higher income, and a larger share of their population is non-white. Also, the treated counties are denser and more likely to have experienced an uprising during the 1960s. In fact, roughly 84 percent (63 of the 75) of treated counties experienced at least one violent protest between 1964 and 1971, compared to only about 7 percent (199 out of 2910) counties in the

control group.[12] In addition, treated locations have more police killings. This is as expected, given the fact that the treated counties are larger and denser. Moreover, the treated counties tend to be highly urbanized; at least 47 percent of each treated county's population resides in urban areas.

2.3 Event-Study Framework

Our identification strategy relies on the evolution of police killings of civilians prior to a city in a county experiencing litigation; we do not identify causal effects based on cross-sectional differences between litigated and non-litigated counties. Given that we cannot observe the counterfactual, causality relies on deaths due to legal intervention evolving similarly in litigated and non-litigated counties before the threat of affirmative action occurs. Thus, it would be reasonable to assume that a common trend, pre-treatment, would have persisted in the absence of litigation. Put another way, we require that the timing of the first litigation be exogenous to pre-existing trends in police use of deadly force and that similar trends would exists in the absence of treatment. If this holds, county fixed effects will account for key cross-sectional differences that are time-invariant, and non-litigated counties will capture trends in police killings over time as well as provide a counterfactual for how police killings of civilians are expected to evolve in the absence of litigation.

A preview of the identification strategy appears in Figure 4, which suggests that treated locations would see an immediate decrease in police killings of civilians due to federal involvement. However, many locations were treated well after 1968, so it is plausible that we will not uncover any impact of litigation on deaths due to legal intervention in the short-run. Also, Figure 4 shows that at least before 1968, treated and non-treated locations have different pre-trends. Lastly, the figure shows that in the absence of treatment, police killings of civilians would decrease over time, capturing a general trend of lower deaths due to legal intervention. However, it is possible that the control group is not the ideal comparison group. We proceed by using the entire sample, but supplement our analysis by restricting the sample to 1) counties with a relatively large population, 2) highly urbanized counties, and 3) counties that experienced at least one racial uprising during the 1960s. It is important to note that Figure 4 only exploits variation in location, not in timing.

To check for parallel trends, we run several tests to examine the influence of pre-existing

[12] Both McCrary (2007) and Miller and Segal (2012) highlight the importance of the 1960 uprisings in providing the initial impetus for implementing affirmative action programs.

trends on the timing and location of litigation. Panel (a) of Figure 5 plots the pre-period growth rate in non-white deaths per 100,000 non-white civilians prior to 1969 against the year of treatment. This provides a simple test of whether changes in non-white deaths in the 1960s are correlated with the timing of treatment. It is clear that there is no pattern associated with the timing of treatment and pre-period growth rates. Counties treated relatively early experienced large increases in non-white deaths, while a significant number of counties experienced a decline in police killings prior to 1969.

We test for differences in pre-period trends in non-white deaths between the treatment and control groups in panel (b) of Figure 5. Panel (b) plots estimates of the average differences in non-white deaths between treated and non-treated counties prior to 1969. We regress non-white deaths per 100,000 non-white residents on treatment status, year fixed effects, and treatment-by-year effects. The reference year is 1968, thus comparing the difference in non-white deaths to 1968 (analogous to panel (b) of Figure 4). The triangle markers plot the coefficient for the treatment-by-year effects. Although the pre-trend coefficients are negative, the point estimates are statistically indistinguishable from zero. Neither Figure 5a nor 5b shows a distinct difference in the pre-period growth rates. The lack of a statistical difference provides suggestive evidence that non-white deaths due to police intervention evolved similarly in the litigated and non-litigated counties prior to 1969. An additional test of the common trends assumption is embedded in our analysis.

Our main specification employs the following difference-in-differences (DiD) event-study framework:

$$y_{ct} = \alpha_c + \gamma_{r(c),t} + \sum_{j=2}^{7} \pi_j D_c \mathbb{1}\{t - t_c^* = -j\} + \sum_{j=0}^{26} + \phi_j D_c \mathbb{1}\{t - t_c^* = j\} + v_{ct} \tag{1}$$

where y_{ct} is the number of deaths due to legal intervention in county c in year t for either white or non-white civilians. The term α_c represents county fixed effects, while $\gamma_{r(c),t}$ are region-by-year fixed effects. D_c is an indicator variable equal to one if the county was ever threatened with litigation. $\mathbb{1}\{t - t_c^* = -j\}$ is an indicator variable equal to one if the observation year is $-j$ years from the date of litigation; $\mathbb{1}\{t - t_c^* = j\}$ is equal to one if the observation year is j years after litigation. We omit $\mathbb{1}\{t - t_c^* = -1\}$ due to collinearity and as a reference year for our analysis. Lastly, t_c^* is the year of litigation (threat) for county c.

Our coefficients of interest, π_j and ϕ_j, capture how the relationship between our measure of litigation and police killings varies over time, both before and after litigation. A key assumption of the DiD model is "parallel trends," where trends in an outcome should be common or parallel prior to policy implementation. Given the inability to observe the counties' counterfactual trends, we assume that pre-policy trends persist in the absence of litigation. A test of this is embedded in the event-study framework. Specifically, the difference in pre-policy trends is captured by π_j. Therefore, the common trends assumption is valid if π_j is statistically insignificant and close to zero. The remaining coefficient, ϕ_j, allows us to examine any dynamics resulting from treatment post-litigation. We group event-times for seven years before (i.e., $\pi_{-7} D_c \mathbb{1}\{t - t_c^* \leq 7\}$) and twenty-six years after (i.e., $\phi_{26} D_c \mathbb{1}\{t - t_c^* \geq 26\}$) the policy for the long sample (places treated prior to 1987); we group them for thirteen years after the policy for the full sample.[13] Our primary analysis will focus on the long sample, six years before litigation to twenty-five years afterward.

We consider a variety of specifications to estimate equation (1). Our dependent variable, the number of deaths due to police, takes on non-negative integers with a significant number of zeros. Due to the nature of the dependent variable, ordinary least squares (OLS) can produce biased estimates with the wrong sign or direction. Therefore, we proceed by estimating equation (1) using a Poisson estimator.[14] In addition to the Poisson estimator, we estimate the impact of the threat of litigation on the number of deaths using an ordinary least squares estimator, and we estimate the effect on deaths per 100,000 civilians using weighted least squares (WLS). Police encounters that result in death are rare occurrences. Changes in mortality rates, consequently, will be larger for counties with smaller populations due to the fact that most counties have very few (or zero) deaths. Therefore, we use the 1960 population to give more weight to larger counties that experience wider variations in police killings of civilians.[15]

We summarize event-study estimates with joint treatment effects using the following equa-

[13] Since the last treated unit experienced litigation in 2000, the maximum number of post-treatment years in a balanced panel for this timing group is 12 years (i.e., until the last year of our sample period, 2012). Although the event-study design can estimate treatment effects for an unbalanced panel, due to composition bias, it can lead to misleading estimates. Specifically, the average treatment effect can change over time because of the composition of timing groups are changing. This is even more problematic if there is heterogeneity in the treatment effects across timing groups or heterogeneity over time.

[14] In our Poisson model, we use population (by demographic group) to account for differences in exposure related to county size and demographic make-up.

[15] Population weights also correct for heteroskedasticity related to county size in the error term. The population in 1960 will be used as weights.

tion:

$$y_{ct} = \alpha_c + \gamma_{r(c),t} + \sum_{\tau} \widetilde{\pi}_j D_c \mathbb{1}(t - t_c^* \in \tau) + \sum_{\omega} \widetilde{\phi}_j D_c \mathbb{1}(t - t_c^* \in \omega) + v_{ct} \tag{2}$$

where τ accounts for the pre-period event-years $j \leq -7$ and $-6 \leq j \leq -2$, while ω accounts for the short-run ($0 \leq j \leq 7$), medium-run ($8 \leq j \leq 15$), and long-run ($16 \leq j \leq 25$) event-years, as well as $j \geq 26$. This specification allows for testing the joint significance of pre-period trends and will also be used to summarize our robustness checks results.

3 Results

We start by describing the event-study estimates, which allow us to analyze pre-trends further while also examining the subsequent dynamic effects post-litigation. We plot the pre- and post-treatment effects from equation (1) with a solid line and circle markers for the long sample. The 95-percent confidence intervals are shown with dashed lines and circle markers, and the gray shaded area identifies the 90-percent confidence intervals. Confidence intervals are constructed from robust standard errors, clustered at the county level to address over-dispersion. We also plot the results from the full sample with a solid red line. We interpret our effects as an "intent-to-treat" estimate because we only know when the threat to litigate occurred (we are unable to identify if or when a city in a treated county implemented an affirmative action hiring program), and we are unable to distinguish between the actions of the treated city in the county versus non-treated cities within the same county.

3.1 Primary Findings

Figure 6a graphs the pre- and post-treatment effects of litigation on the number of non-white civilian deaths due to legal intervention from our Poisson model. Appendix Figures B5a and B5c report treatment effects from our OLS and WLS models, respectively. As indicated in Figure 5, we see no pre-trend difference in police killings of non-white civilians. In the year of treatment, the post-treatment effect for event-year 0 is positive but imprecisely estimated in our OLS and Poisson specifications. However, our WLS specification does yield a statistically significant increase in the non-white death rate for police killings of civilians in event-year 0 (see Appendix Figure B5c). This suggests that the threat of affirmative action may have led to an initial backlash effect aimed at non-whites, consistent with the literature on conflict theory and racial threat (Jacobs and O'brien,

1998; Smith, 2003). Afterward, post-treatment effects decrease and become negative by event-year 2 for all specifications. From Figure 6a, we see that the post-treatment effects are negative and statistically significant starting in event-year 5 for the Poisson model and decrease slightly over time. The OLS and WLS models produce statistically significant post-treatment effects in event-year 11 and onwards. On average, 1.457 non-white civilians were killed by police in a treated county in the year prior to the threat of litigation. According to the point-estimate for event-year 11, non-white deaths decreased by 44 percent, which amounts to 45 fewer deaths in 70 treated counties.[16] Our results provide evidence that after litigation, police killings of non-white civilians decrease in the long-run. Appendix Figure B7 plots the number of non-white deaths prevented in litigated counties over time. In the year 2000, these counties prevented roughly 60 non-white deaths due to legal intervention.[17] Although we cannot rule out that changes in the racial composition of police departments may be an important mechanism in lowering police killings of non-white civilians, our findings suggest that the threat of federal intervention in and of itself may have had an effect on police culture.[18]

Figure 6b and Appendix Figures B5b and B5d plot event-study estimates for white deaths at the hands of police. In general, results are similar across the OLS, WLS, and Poisson specifications; however, the common trends assumption is likely violated for white deaths. Estimates plotted in Appendix Figures B5b and B5d provide evidence that the common trends assumption does not hold across specifications. The point estimates for event-years -3 and -4 are statistically or marginally statistically significant, depending on the model. In the Poisson model, event-year -5 is marginally statistically significant. Although we cannot imply causality for all of our models, white deaths immediately decreased after treatment, ruling out the possibility that officers indiscriminately increased force in response to the threat of litigation. The initial increase in racial disparities in police violence could indicate a distinct change in police behavior in response to federal intervention. Appendix Figure B8 plots pre- and post-treatment effects from a Triple Difference

[16] The coefficient in event-year 11 is -0.582, which implies, on average, a 44% difference between the number of deaths in event-year -1 and event-year 11. There are 70 treated counties in the long sample, resulting in an estimated 45 fewer non-white deaths.

[17] Our results suggest that the threat of affirmative action prevented nearly 800 non-white deaths between 1970 and 2000.

[18] Specifically, we do not observe if and when a municipality implements an affirmative action program. Similarly, it is possible that the threat of federal intervention caused other departments in the county to change their behavior, without causing the treated department to do so.

model and shows an initial statistically significant increase in racial disparities in police killings at event-year 0. After the initial increase in racial disparities of police killings, post-treatment effects decrease and eventually become negative but are not statistically significant. The decrease in police killings of white civilians also runs counter to Cunningham and Gillezeau (2019), who find that police killings of both white and non-white civilians move in the same direction after a violent protest, and counter to the finding of Fryer (2019), who finds no racial differences in police use of force. Post-treatment effects for white civilians are negative and statistically significant, or marginally statistically significant, in certain event-years. When using the point estimate for event-year 0, white deaths decreased by approximately 43 percent (36 fewer white deaths). However, these results should be interpreted with caution, given the violation of the pre-trends assumption across our specifications. It is plausible that white deaths increased in the year before treatment but afterward reverted to pre-ligation trends.

We summarize the joint pre- and post-treatment effects in Table 3, which presents estimates from equation (2). Columns 1 through 3 present results for non-white deaths, while columns 4 through 6 refer to white deaths. We present estimates from the OLS, Poisson, and WLS specifications. Columns 1 and 4 estimate equation (2) using OLS for the number of deaths due to policing; columns 2 and 5 present joint effects from the Poisson model; and columns 3 and 6 reflect the WLS model where the dependent variable is a mortality rate. For non-white deaths, the pre-treatment effects are not statistically significant in all three models. This provides evidence that the pre-treatment effects are not jointly statistically significant. In the short-run, post-treatment effects are negative and not statistically significant in all three models. The joint effect masks the initial increase in non-white deaths discovered in Figure 6.[19] The event-study estimates are also negative and statistically or marginally statistically significant in the medium and long-run. If we interpret the long-run coefficients, the threat of affirmative action litigation is associated with a 40 percent decrease in non-white deaths in the Poisson model. The WLS model suggests a 35 percent decrease in non-white deaths, while the OLS model indicates a 46 percent decrease.

Joint pre-treatment effects are marginally statistically significant for white deaths in the Poisson and WLS models but statistically insignificant in the OLS model. White deaths are lower in the

[19] Appendix Figure B5c plots the pre- and post-treatment effects for the WLS model. This model also shows an increase in non-white deaths in event-year 0; however, the point estimate is only statistically significant in the WLS models.

short-run across all three models. If we use the OLS model, we can assume causality, and litigation is associated with a 39 percent decrease in police killings of white civilians in the short-run. The OLS model provides evidence of a long-run reduction in white deaths by 43 percent. In Appendix Table C1, we limit the sample to counties that report at least one non-white death over the sample period to check the validity of our estimated joint effects. This restriction produces post-treatment effects that are similar in magnitude for both non-white and white deaths. However, as before, the joint pre-treatment effect for white deaths is statistically significant in the Poisson and WLS models.[20]

4 Robustness Checks

Our results suggest that in the year of litigation, racial disparities in police killings increase, while several years after the threat of litigation, non-white deaths decrease. This result holds across multiple specification checks. However, it is clear from Table 2 that the treatment group differs drastically from the control group. Although our empirical strategy can identify a causal relationship despite key cross-sectional differences, it is reasonable to be concerned about the interpretation of our results given the control group. Figures 7 and 8 present joint effects for a series of robustness checks using the Poisson model for non-white and white deaths, respectively. The estimated joint effects are denoted by a circle marker, while a bold horizontal line indicates a 95-percent confidence interval. The columns of each figure display pre-treatment effects (2–6 years before treatment), short-run effects (0–7 years after treatment), medium-run effects (8–15 years after treatment), and long-run effects (16-25 years after treatment). The first row presents joint effects from columns 2 and 5 of Table 3; the remaining rows report joint effects from a series of robustness checks.

4.1 Treatment Status

It is reasonable to be concerned that our analysis does not have a proper control group. The current empirical strategy takes advantage of variation in the timing and location of affirmative action litigation. In row 2, we restrict the sample solely to treated counties. Restricting the sample in this manner only exploits variation in timing for estimating treatment effects. For non-white

[20] We find similar results when we limit the sample to counties that report at least one non-white death prior to 1969. See Appendix Table C2.

deaths (Figure 7), pre-treatment effects are statistically indistinguishable from zero, and the short-run effects are negative and close to zero. Post-treatment effects in the medium- and long-run have the same sign as those from the main specification. But they are larger, suggesting that the control group helps capture changes in police killings of non-white civilians over time. For white deaths (Figure 8), the joint pre-treatment effect is statistically significant, and the magnitude of the post-treatment effects is similar to that from the original specification. In Appendix B11, we examine the impact of the threat of litigation on nearby (contiguous) counties. It is possible that, agencies in neighboring counties change behavior, to prevent or avoid future litigation. However, when we assign the treatment status to contiguous untreated counties and compare them to non-contiguous untreated counties, we find little evidence of changes in police killings of civilians.

4.2 County Characteristics

The smallest non-treated county in our sample had 57,000 residents in 1960, while only four treated counties had less than 100,000 residents. In row 3 of Figures 7 and 8, we restrict the sample to counties with a population greater than 100,000 residents. This results in a sample that contains 71 treated counties and 212 control counties. Restricting the sample to only large counties produces pre-treatment effects near zero for non-white deaths. In this specification, post-treatment effects are similar in magnitude and statistical significance to our original specification for non-whites. We also see evidence of short- and long-run decreases in white deaths, although pre-treatment effects are statistically significant.

In Rows 4 and 5, we split the treatment group into counties where the share of the black population is below and above the median share for the treatment group; removing the above-median group in row 4 produces joint treatment effects that are estimated less precisely. For non-white deaths, the medium- and long-run effects are similar to row 1 but only statistically significant in the long-run. For white deaths, post-treatment effects are no longer statistically significant. Alternatively, row 5 displays the results for removing the below-median group. For non-white deaths, the medium and long-run effects are statistically significant. The medium-run effect is similar in size to row 1, but the long-run effect is smaller than our baseline estimate. For whites, the joint pre-treatment effect is statistically significant, and the post-treatment effects are statistically significant and larger in magnitude than the post-treatment effects in row 1. These results show that

locations with a large black population do not drive the decrease in non-white deaths, though the negative post-treatment effects for whites are driven by treated locations with a relatively large non-white population.[21] In these treated locations, however, the common trends assumption is violated for white deaths. This is important when interpreting our main results in Figure 6. As shown in Table 3, the estimate for white deaths is not robust to various specifications. Figure 8 shows that the large negative post-treatment effects are driven by locations where the common trends assumption does not hold.

In Row 6, we limit the sample to counties where the proportion of residents residing in urban areas was higher than the median share of urbanization in 1960. All treated counties had urban populations above the median. Restricting the sample in this manner produces a sample with 75 treated counties and 853 non-treated counties. For non-white deaths, restricting the sample to urbanized locations produces medium- and long-run effects that are slightly larger than when we restrict the sample to only large counties.

Together, these findings highlight the importance of having non-litigated counties as the comparison group: post-treatment effects are smaller when including all non-litigated counties. Specifically, including smaller counties helps capture national trends in police killings. Many of the smaller counties are not treated but are policed by majority-white police departments. Feigenberg and Miller (2018) find that heterogeneity in a county's racial composition is associated with a more punitive criminal justice system; relatedly, Cunningham et al. (2020) also show that heterogeneity in a county's racial composition is associated with more police killings of non-white civilians. Appendix Figure B19 shows that the long-run decrease in non-white deaths due to the threat of affirmative action is driven by treated locations outside of the South. When restricting our analysis to southern counties, statistically significant increases in non-white killings are evident two years after litigation and then again seven years after. The decrease in white killings is consistent across geographic regions. The regional heterogeneity of our findings highlights the importance of using region-by-year fixed effects to capture the cultural differences and heterogeneity

[21] This analysis stratifies the treatment group by the county's non-white population. Likely more important is the percentage of the Black population residing in treated cities within a county. Appendix Figure B9 shows that our results are driven by treated location where a significant proportion of non-white residents reside in treated cities. Treated counties with more than one treated city or counties where the treated cities comprise a significant proportion of the county's population provide similar results to counties with one treated city or where the treated locations comprise a relatively smaller proportion of the county's population. Also see Appendix Figure B10.

in racial composition that contribute to criminal justice outcomes.[22]

4.3 Uprisings and Heterogeneity in Timing

Litigation is highly correlated with uprisings. McCrary (2007) finds that uprisings are associated with the timing of treatment. In row 7 of Figures 7 and 8, we restrict the sample to counties that experienced at least one racial uprising in the 1960s. This limits the sample to 63 treated counties and 199 non-treated counties. Once again, our estimates for non-white deaths are robust to this restriction and are of similar size to the models restricting the sample to highly urbanized counties or counties with a population greater than 100,000. This is not surprising since the restrictions are highly correlated. Bigger counties tend to be urbanized and have experienced uprisings in the 1960s. Nonetheless, the impact of the threat of affirmative action litigation on police killings of civilians is robust to various restrictions to the sample. Estimates in row 7 of Figure 8 show that the decrease in white deaths in rioting counties is similar to the baseline estimates. Similarly, row 7 of Figure 7 shows that restricting the analysis to rioting counties leads to similar decreases in non-white killings as in our main results.

Given that the timing of litigation varies over the study's time period, we also consider the possibility that the estimates obtained from the standard two-way fixed effects (TWFE) difference-in-differences (DiD) model may be biased due to treatment effect heterogeneity. Standard DiD techniques may have biased parameter estimates if the treatment effects change over time (Goodman-Bacon, 2021), which may be true even for dynamic specifications (Sun and Abraham, 2020). Appendix Figure B22 plots the 2x2 difference-in-difference estimates and their associated weights from the conventional two-way fixed effect model (Goodman-Bacon, 2021). Appendix Figure B21 plots the event-study estimates of the effect of the threat of court-ordered affirmative action where the coefficients are estimated using the Callaway and Sant'Anna (2020) estimator (CS) to avoid biases associated with TWFE models. The CS estimator yields results that are very similar to our OLS findings; we see a similar statistically significant long-run decrease in non-white deaths due to police, as well as an increase in non-white deaths immediately after treatment.

Lastly, we test for heterogeneous effects by restricting the treatment group to 1) counties treated prior to 1980 and 2) counties treated after 1980. Under both scenarios, the control group

[22] Pre-treatment effects violate the parallel trends assumption, so the results in Appendix Figure B19 should be interpreted with caution.

includes counties that were never treated. It is reasonable to assume that the increase immediately after treatment occurred in the aftermath of riots, where tensions between the police and non-white community were palpable. Appendix Figure B20 plots event-study estimates for counties treated prior to 1980 and counties treated after 1980. The later-treated group produces estimates that are noisy and volatile, but the pattern of a long-run decrease in police killings exists for both groups. However, in earlier treated counties, we see the pattern emerge from our baseline model of an initial increase in non-white deaths followed by a persistent decrease over time. Therefore, we cannot rule out that the initial rise in police killings of non-whites in the year of treatment is related to uprisings that occurred around the same time, as the later-treated group was treated well after the 1960s.

4.4 Threat to Internal Validity

Police killings of civilians are severely under-reported in government agency data, including our primary source. According to Collaborators et al. (2021), roughly 50 percent of police killings are not reported and under-reporting is more likely to occur for non-white decedents. In our case, measurement error in the dependent variable would lead to imprecise estimates by increasing the variance and the likelihood that we are unable to uncover a statistically significant relationship. However, if treatment changes reporting behavior, our results would be biased. For example, if the threat of litigation increases oversights and decreases under-reporting, then our estimates understate the impact of federal intervention on police killings of civilians. However, if federal intervention increases under-reporting, then our estimates overstate the impact.

Unfortunately, we are unable to test directly for changes in reporting behavior. However, to observe changes in reporting behavior indirectly, we examine the impact of the threat of affirmative action on suicide deaths. Suicides serve as a legitimate test of changes in reporting behavior, as there are no direct linkages between affirmative action litigation and suicides. However, both police killings of civilians and suicides depend on local inter-agency cooperation for reporting in the Vital Statistics and both are under-reported. Appendix Figure B6 displays pre- and post-treatment effects for suicides per 100,000 residents by racial group. In both panels, we see no systematic change in suicides after litigation. Non-white suicide post-treatment effects are statistically insignificant and are both positive and negative. Post-treatment effects are positive and

then negative for white deaths but are statistically significant for 5 out of the 26 post-treatment observations.

If police killings are under-reported, where are the missing deaths? According to Feldman et al. (2017), in 2015, 86 percent of non-accounted police killings were recorded as homicides/assaults. Therefore, if local officials are changing reporting behavior, it could be captured by changes in homicide victimization. Appendix Figure B23 report treatment effects for the impact of the threat of affirmative action litigation on homicides per 100,000 residents by race.[23] According to panel (a) of Appendix Figure B23, non-white homicides decrease four years after treatment; the point estimates are statistically significant in event-years 4 and 5. However, eventually the post-treatment effects begin to increase and eventually become positive. The post-treatment effect twenty years post-litigation is positive and statistically significant. For white homicides, pre-treatment effects are negative and statistically significant, while post-treatment effects are essentially indistinguishable from zero. In general, there is suggestive evidence that litigation may reduce non-white homicides in the short-run, while welfare gains dissipate in the long-run. However, there is little evidence that the decrease in police killings of civilians is reflected in higher homicide victimization rates.

Together these results show no systematic change or pattern with regards to reporting suicides or homicides and provide additional evidence that we are capturing changes in police behavior in response to the threat of litigation and not changes in reporting behavior due to inter-agency cooperation or the lack thereof. It is important to note, although we find little evidence of changes in reporting of suicides, we cannot rule out changes in cooperation with regards to reporting police-related homicides. This is evident as the number of police killings in the Vital Statistics data remains severely under-counted. In addition, the rarity of police killings relative to the number of homicides may result in only a slight variation in the measurement of homicides, plausibly masking any impact from under-reporting.

We attempt to address under-reporting in the vital statistics by using estimates of the bias in

[23] To capture pre-period differential trends in homicides, the analysis includes urban-by-year fixed effects in equation (1). Urban-by-year fixed effects are constructed by interacting urban status indicator variables with year indicator variables. Urban status is defined as the following: percent of the population residing in urban areas $(\mu) : 0, 0 < \mu < 25, 25 \leq \mu < 50, 50 \leq \mu < 75, 75 \leq \mu \leq 100$. Urban-by-year fixed effects capture unobserved heterogeneity that varies across time and urbanicity. Not including urban-by-year fixed effects results in statistically significant pre-treatment effects for non-white homicides.

reporting of police killings by state and race from Collaborators et al. (2021). Using their corrected estimates of police killings from 1980 to 2019, we predict police killings of civilians by race and state for 1960 to 1979. Appendix Figure B12 plots the complete time series of police killings of civilians from 1960 to 2016. We assume that each county's contribution to the state estimate of police killings of civilians by race is constant over time. Therefore, our new measure of police killings at the county is dependent on year-to-year variation in the Vital Statistics data, the estimated bias in police killings by state and race, and the county's contribution to the state level number of police killings in the Vital Statistics data. We do this to avoid recording deaths in counties that never report a police killing between 1960 and 2016. Appendix Figure B13 replicates Figure 1 but includes the updated measure of police killings of non-white civilians. In general, the new estimate mirrors the original, with police killings approximately twice as large as before. Appendix Figure B14 plot pre- and post-treatment effects using the new measure of police killings. Interestingly, panel (a) shows a much larger decrease in police killings of non-white civilians, while panel (b) report similar estimates for white deaths. For non-white deaths, the long run estimates are more than 60 percent larger than the original treatment effects. Although accounting for under-reporting results in larger treatment effects, the new measure of police killings and the estimates are contingent on the treatment being exogenous to reporting. More importantly, this exercise highlights the importance of reforming how data is collected for police activities and the use of force. Currently, there does not exist a governmental source that accurately captures the degree to which police officers engage in lethal and non-lethal use of force. Therefore, making it difficult to conduct in-depth policy analysis or provide serious solutions regarding discriminatory policing, use of force, and police-community relations.

5 Possible Mechanisms

We find that the threat of litigation decreases police killings of non-white civilians. Our findings are robust to a series of reasonable robustness checks, including changes in the sample restriction, accounting for 1960s uprisings, eliminating the bias from TWFE models, and using various models for handling count data. Now, we explore possible mechanisms that may explain the non-trivial decrease in police killings of civilians in response to the threat of litigation.

5.1 Employment Effects

The threat of litigation and the implementation of affirmative action change the racial and gender composition of police departments (McCrary, 2007; Miller and Segal, 2012). Employment effects are important because, as previously mentioned, a police department's demographic composition is a potential mechanism through which non-white police killings decrease over time in counties with at least one litigated police department. Prior research suggests that Black and female officers are less likely to use force (Hoekstra and Sloan, 2020; McElvain and Kposowa, 2008; Rabe-Hemp, 2008; Smith, 2003). Miller and Segal (2012) find that litigated departments increase their Black employment shares, but at a lower rate than departments with court-ordered affirmative action. They also find that litigation leads to increases in the hiring of female officers.

Nonetheless, our litigation data is limited because we do not have information on dispositions of lawsuits, nor do we have data on the imposition of court-ordered mandates. Moreover, we do not have information on the year-to-year racial and gender composition of police departments. However, we can use police employment data from the Law Enforcement Management and Administrative Statistics (LEMAS) and the Law Enforcement Officers Killed and Assaulted (LEOKA) files to examine the compositional effects of police departments over our analysis period. The LEMAS is obtained from the Bureau of Justice Statistics (BJS), and the LEOKA data comes from the Uniform Crime Reporting (UCR) program. While LEOKA data are available after 1971, the UCR only contains police employee data by gender. It does not have information by race. LEMAS data, on the other hand, provides employment information by gender and race, but data collection did not begin until 1987. Appendix Figure B15 plots the share of Black and female officers over time using the LEMAS data. Both groups have gained representation since 1987, with the female and Black share being very similar. Using these two data sources will allow us to get a clearer picture of changes in the police force composition over our sample period.

We estimate multiple models to understand the effect of litigation on police composition. We start with the LEMAS data and regress the county share of Black and female sworn officers, respectively, on three dummies that indicate whether a police department was treated in the 1970s, 1980s, or 1990s along with region and year fixed-effects. We report these estimates in Appendix Table C3, where the reference group is counties that were never treated. These estimates indicate

that counties treated earlier (in the 1970s and 1980s) have on average significantly larger shares of Black and female officers relative to never treated counties. There is no statistically significant difference in the shares of Black and female officers between counties treated in the 1990s and those never treated.

Next, using our main specification (see equation (1)), we use the LEOKA employee data to calculate changes in the share of sworn female officers in a county (as an indication of the changing dynamics or composition of police departments) over our sample period. We plot the point estimates from our event-study approach in Appendix Figure B16. The top figure, B16a, plots the dynamic effects of the threat of litigation on the number of total sworn officers (per 1,000 residents), and panel B16b plots the estimates for the share of female officers. The figures show that pre-treatment effects are insignificant between treated and control counties. In addition, the findings indicate an immediate significant increase in the number of total sworn officers in treated counties relative to control counties following the threat of litigation. While the number of sworn officers seems to be increasing, in general, over the post-treatment period, the increase is not significant until event-year 24. Panel B16b also shows that the share of female officers increases immediately after litigation and continues to do so for all post-treatment years. Together, these results indicate that litigated departments are becoming more diverse over our sample period.

Finally, we consider a method similar to Miller and Segal (2014) in which we regress the county share of Black and female sworn officers on the years since a county experiences litigation. Our results are presented in Table C4 and are similar to those of Miller and Segal (2014), where the share of Black officers increases as the number of years since litigation increases. We find similar results for female officers. We then split up the LEMAS data and run this model for each year of the LEMAS that is available. Figure B17 plots the point estimates and 95-percent confidence intervals on the variable, capturing years since litigation for each cross-section of LEMAS. Years since litigation is not statistically significant until 1993. Beginning in 1993, years since litigation is significantly and positively associated with a larger share of Black and female officers. The lack of a significant association between years since litigation and the LEMAS years 1987 and 1990 coincides with our main event-study estimates that indicate long-run decreases in non-white police killings but no short-run effects.

To see how important diversity is in explaining our results, we use LEMAS data to account

for the racial representation gap in police departments. We compare the racial composition of police departments to the racial demographics of treated cities twenty-five years after treatment. We use the most recent census year, closest to the twenty-five year mark, to obtain the racial composition of treated cities. For example, for a city treated in 1972, we use LEMAS data from 1997 and city demographic characteristics from 2000. Although not ideal, this provides a close approximation of representativeness in police departments at the end of our event-year window.[24] We then rank treated locations by their representation gap and compare locations above and below the median level of representativeness to non-treated locations.[25] Appendix Figure 9 plots event-study estimates for non-white civilian deaths. For both the below and above median groups, we see a decrease in police killings of non-white civilians. Twenty-five years after treatment, both groups report similarly sized post-treatment effects. Interestingly, the decline in police killings of non-white civilians is more drastic in the below-median group. It is important to note, the level of diversity in a police department is endogenous to the treatment, but this exercise suggests that intervention is just as effective–or even more effective–than racial diversity in police departments.

5.2 Arrests

Our findings suggest that the threat of affirmative action leads to long-run declines in the police killing of non-white civilians as well as to changes in the demographic composition of police agencies. Another possible channel for a decrease in police violence is a change in policing due to federal intervention. Changes in policing could be reflected by a decrease in police contact for less serious crimes. Weisburst (2019) shows that racial disparities in police contact drive racial disparities in use of force incidents. Furthermore, McCrary (2007) finds evidence of a negative relationship between the share of Black arrestees and the threat of litigation. This could occur due to changes in police departments' racial and gender composition over time or as a reaction to federal intervention (not necessarily changing racial composition).

Using our main specification, equation (1), we plot event-study estimates for arrests stratified by race in Figure 10 and arrests by race and type of crime in Figure 11. Because the sample is limited to counties identified by McCrary (2007), which is comprised of agencies that report at least

[24] We define representitiveness by the Black share of officers in the police department divided by the Black population share.

[25] Both the above and below-median groups will have the same controls.

52 years to the UCR program between 1960 and 2012 (UCR sample), we re-estimate our primary specification of police killings of civilians by race in panels (a) and (b) of Figure 10. Beginning with police killings of civilians, we find our results for the UCR sample to be very similar to our main results for police killings of civilians for non-whites and whites. However, the magnitudes of the point estimates are larger but less precise. Next, our event-study results for arrests in panels (c) and (d) of Figure 10 provide evidence of a statistically significant decrease in non-white arrest rates in treated counties threatened with litigation beginning in event-year 11, the same year that police killings of non-whites begin to significantly decrease, with no significant change for white arrest rates. Nonetheless, when we disaggregate arrests rates by type of crime and race, a different picture emerges. Figure 11 shows that the reduction in non-white arrests is driven by a reduction in arrests for property crimes. Moreover, white arrests for violent crimes significantly increase after the threat of litigation, while white arrests for property crimes decrease, although not significantly. This suggests that the decrease in arrests and police contact may lead to decreases in police-related fatalities.[26]

5.3 Homicide Victimization

Given the effect on arrests, it is possible that the threat of affirmative action may improve community-relations and reduces crime, therefore reducing police contact and police-related fatalities. Though McCrary (2007) finds little evidence of changes in reported crime, Miller and Segal (2014) and Harvey and Mattia (2019) find higher levels of reporting in treated locations. Moreover, Miller and Segal (2014) find lower rates of intimate partner homicides, and Harvey and Mattia (2019) find lower rates of victimization for Black civilians in locations where affirmative action plans were implemented. It is possible that changes in reporting behavior due to improved police quality mask the impact of affirmative action litigation on actual crime.

As highlighted earlier, Appendix Figure B23 reports treatment effects for the impact of the threat of affirmative action litigation on homicides per 100,000 residents by race. In general, scholars typically focus on homicides, as a proxy for actual crime, due to the accuracy in reporting. However, misclassification of police killings of civilians brings the accuracy into question. Ignor-

[26] In Appendix Table C5, we find suggestive evidence that the treatment is not associated with differences in the demand for police services. We also find no evidence that treatment is associated with differences in access to emergency systems.

ing the misclassification of police killings, homicide victimization rates will capture changes in actual crime and proxy for the general welfare of non-white civilians.[27] Our results provide little evidence that the decrease in police killings is driven by lower homicides. It is important to note that there is evidence of a trend break for non-white and white homicides at the time of treatment. A different research design may uncover a stronger causal relationship between litigation and homicide victimization rates. Nonetheless, there is not enough evidence to support the claim that lower homicide victimization rates drive the long-run decrease in police killings. This does not rule out other forms of victimization, however, such as domestic violence or intimate partner violence, as well as other criminal offenses.

6 Discussion

We present evidence of a causal relationship between the threat of litigation and police killings of civilians. For our main outcome, non-white deaths due to policing, we find no evidence of pre-trend violations. The results are robust to reasonable specification checks, including using a Poisson, weighted least squares, or a standard ordinary least squares estimator. The Callaway and Sant'Anna (2020) estimator produces similar results to our OLS estimator. Our results are robust to heterogeneity in the timing of treatment and to accounting for uprisings, which were a major contributing factor to future litigation. We also show that litigation is not a predictor of suicide deaths. Our results are also not driven by trends in homicides post-litigation. Given the ongoing effort to reform the police and improve police-community relationships, we explore the implications of our results.

6.1 Diversity and Racial Disparities in Policing

In the 1960s, The Kerner Commission Report called for aggressive recruitment of minority police officers to improve police-community relations. However, Black community leaders had been calling for more minority officers well before the Kerner Commission Report. Police diversity was a major platform for the NAACP dating back at least to the 1930s (Forman Jr, 2017). Over the next 40 years, little progress was made in diversifying police departments, but the racial composition of northern cities changed considerably. Between 1940 and 1970, 4 million Blacks migrated north as part of the second wave of the Great Migration. Segregation locked them into

[27] Homicides is the leading cause of death for young Black men (Heron, 2019).

specific neighborhoods, significantly reducing future economic prospects, especially for northern-born Blacks (Derenoncourt, 2021). All-white police departments met new demands to police in Black communities and combat rising crime, which began to reverse a trend in 1959. Police not only symbolized white racism but were an antagonizing public and highly visible entity in the civil rights movement in the South and uprisings in the North. Integrating the police was deemed vital for improving race relations.

As a consequence of migration, Blacks constituted a large voting block in major U.S. cities—resulting in newly elected mayors, city council members, sheriffs, judges, and prosecutors. Recent research has shown that electing black sheriffs reduces racial disparities in arrests (Bulman, 2019), and that newly elected prosecutors implement reforms that reduce police killings of civilians (Stashko and Garro, 2021). Facchini et al. (2020) show that political power and accountability through the 1965 Voting Rights Act reduced racial disparities in arrests for less serious crimes in the South. Moreover, many police departments increased their number of minority officers without court-ordered affirmation action plans. Our results suggests federal intervention, in and of itself, can improve the quality of life for non-white civilians. We find no evidence of changes in homicides, the leading cause of death for young black men, but we uncover a reduction in police contact and police-related fatalities. Therefore, it is possible that federal intervention can improve police quality and police-community interactions in locations where the minority community lacks the political capital to affect change in local policing.

Lastly, the police's racial composition and the communities they serve have both been shown to influence racial disparities in crime-related outcomes. Research has shown that police contact is inversely related to police diversity. Blacks are less likely to be arrested and less likely to be searched and frisked when the minority share of officers increases (Donohue III and Levitt, 2001; Close and Mason, 2007). However, more diversity may not influence police productivity. McCrary (2007) finds no change in crime due to the threat of litigation. This is partly because Black officers may be better at identifying non-white criminals, which indicates an improvement in performance. Yet, Black officers scored lower on entrance exams, suggesting lower productivity in other police tasks. Interestingly, Harvey and Mattia (2019) find lower victimization for Black residents in MSAs that experience affirmative action litigation. In addition, they find that victims of crime are more likely to report incidents to the police. This provides suggestive evidence that

actual crime may decrease with mandated police diversity. Our research differs from Harvey and Mattia (2019) as we focus on outcomes directly associated with policing and police violence, which was the primary impetus for increasing police diversity. We find that our results complement their findings, as well as contribute to the causal research on affirmative action litigation and policing.

6.2 What do our treatment effects mean for policy today?

Our results support recent research showing that diversity in police department matters. White officers have been shown to use lethal force more aggressively on non-white citizens (Ba et al., 2021; Headley and Wright, 2020; Hoekstra and Sloan, 2020). But our results also show that federal intervention is just as important and likely leads to more immediate changes in police behavior. We find a sharp decrease in police killings of non-white civilians even in locations where greater diversity within police departments was not achieved. While the intended purpose of promoting and recruiting minority officers is to offer better police services to minority communities, federal intervention, through the courts, also results in fewer police-related deaths.

However, it is important to note that our outcome is the most extreme measure of police brutality. The events that sparked many of the uprisings in the 1960s were not police killings but aggressive police tactics and excessive use of non-lethal force.[28] This is counter to the #BlackLivesMatters (BLM) protests, which largely resulted in demonstrations across the U.S. in response to a police killing of an unarmed minority citizen in a particular location. The BLM protests crossed geographical boundaries, while the 1960s protests were more localized, confined within a city or region, and predominately took place in Black communities. In addition, the police response to BLM protests differs from the 1960s responses. Cunningham and Gillezeau (2019) find an increase in police killings of civilians in the aftermath of the 1960 protests, while Campbell (2021) finds a decrease in police killings of civilians after the initial wave of BLM protests. Similarly, the call for police reform was quite different. In the 1960s, community leaders and politicians called for more policing and more police officers from minority groups (Forman Jr, 2017), while more recently, there has been growing support to defund the police. The 1960s ushered in police militarization through President Johnson's War on Crime (Hinton, 2017), and now activists push to de-militarize the police. Our results show that federal interventions to increase police diversity reduce police

[28] This does not suggest that police killings were not an important issue in the 1960s. Police use of lethal force and police brutality have always been a central concern and major platform item for police reform.

killings of civilians. However, it is important to note that the historical setting that introduced affirmative action litigation may not apply to more contemporary outcomes and could possibly have no effect on police killings of civilians. Therefore, police diversity should be one action item in conjunction with broader reforms to reduce police use of force.

7 Conclusion

Our results indicate that police departments subject to affirmative action litigation respond by killing fewer civilians. Specifically, we find that in the long-run, police killings of non-white civilians significantly decline as a result of threatened affirmative action litigation. By 2000, counties that experienced litigation were preventing roughly 60 non-white deaths due to legal intervention per year. In the long-run, we find that departments subject to affirmative action litigation reduced non-white deaths due to legal intervention by roughly 40%. We also find suggestive evidence that police in those departments killed fewer white individuals as well. We do not find evidence that such decreases in police killings are driven by an overall decline in homicide victimization post-litigation. Instead, we find evidence of a reduction of police contact with non-white civilians, measured by low-level arrests (i.e., property crimes).

Moreover, as in other studies, we confirm that a potential mechanism driving our main findings may be increased minority representation in police departments (Harvey and Mattia, 2019; Miller and Segal, 2014; McCrary, 2007). We find that litigation is associated with an increase in Black police representation in the long-run, which coincides with the decrease in non-white deaths at the hands of the police. Given the recent call to diversify and restructure police departments, our results highlight the vital role that federal interventions have in addressing excessive use of police force in marginalized communities. However, we also find evidence of racial disparity in police killings in the initial year of the litigation threat. In some of our specifications, we see an outright increase in non-white deaths due to legal intervention in the year that a police department is subject to litigation–though because we only see this pattern among cities treated immediately after racial uprisings in the 1960s, this threat may be a response to protests or uprisings (Cunningham and Gillezeau, 2019). Interestingly, we also see that the decrease in police killings is consistent in litigated locations where police departments experienced relatively fewer changes in their racial composition. This suggests that litigation may help reduce police killings of civilians independent

of the racial composition effect. Our main findings of a decrease in non-white police killings due to affirmative action litigation are robust to various sample restrictions, including counties' size and racial composition. Our results are also consistent across alternate estimators.

Overall, our results show that diversifying the racial composition of police departments, or at the very least affirmative action litigation intended to do so, decreases non-white deaths at the hands of police. However, even with greater diversity within police departments today, Black citizens are still 2.5 times more likely to be killed by the police than white men (Buehler, 2017; Edwards et al., 2018). During encounters with Black citizens, police are also more likely to draw a gun and employ aggressive non-lethal force tactics (Fryer, 2019). If racial disparities stem from structural factors, such as a deep-rooted organizational culture that is in opposition to non-white residents (e.g., racial threat theory (Jacobs and O'brien, 1998)), political power (Gray and Parker, 2020), or other federal programs that encourage greater use of force (e.g., the 1033 program), then simply diversifying police personnel may be insufficient to address apparent racial disparities in police killing. Evidence of a more systemic issue may be present in our findings, given that our results suggest that diversification leads to decreases in non-white deaths in the long-run. Thus, it may be necessary to take other steps to reform police culture for more immediate changes in police behavior.

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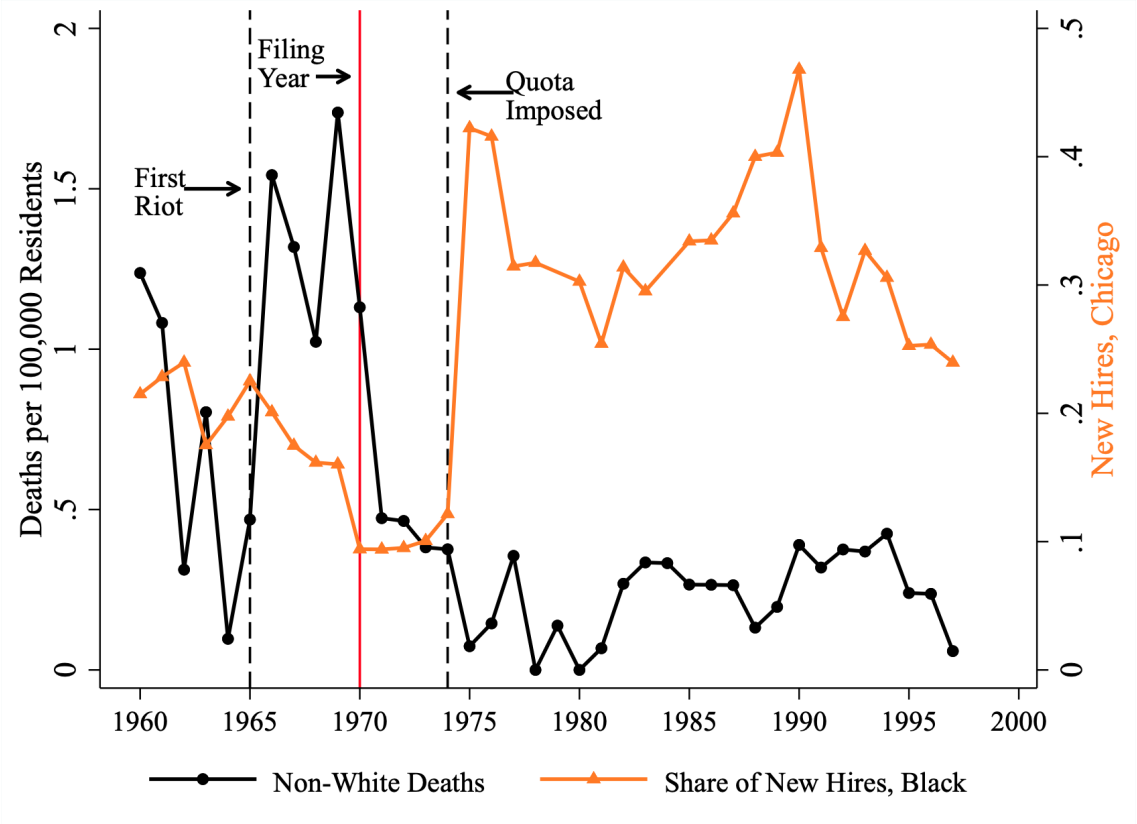
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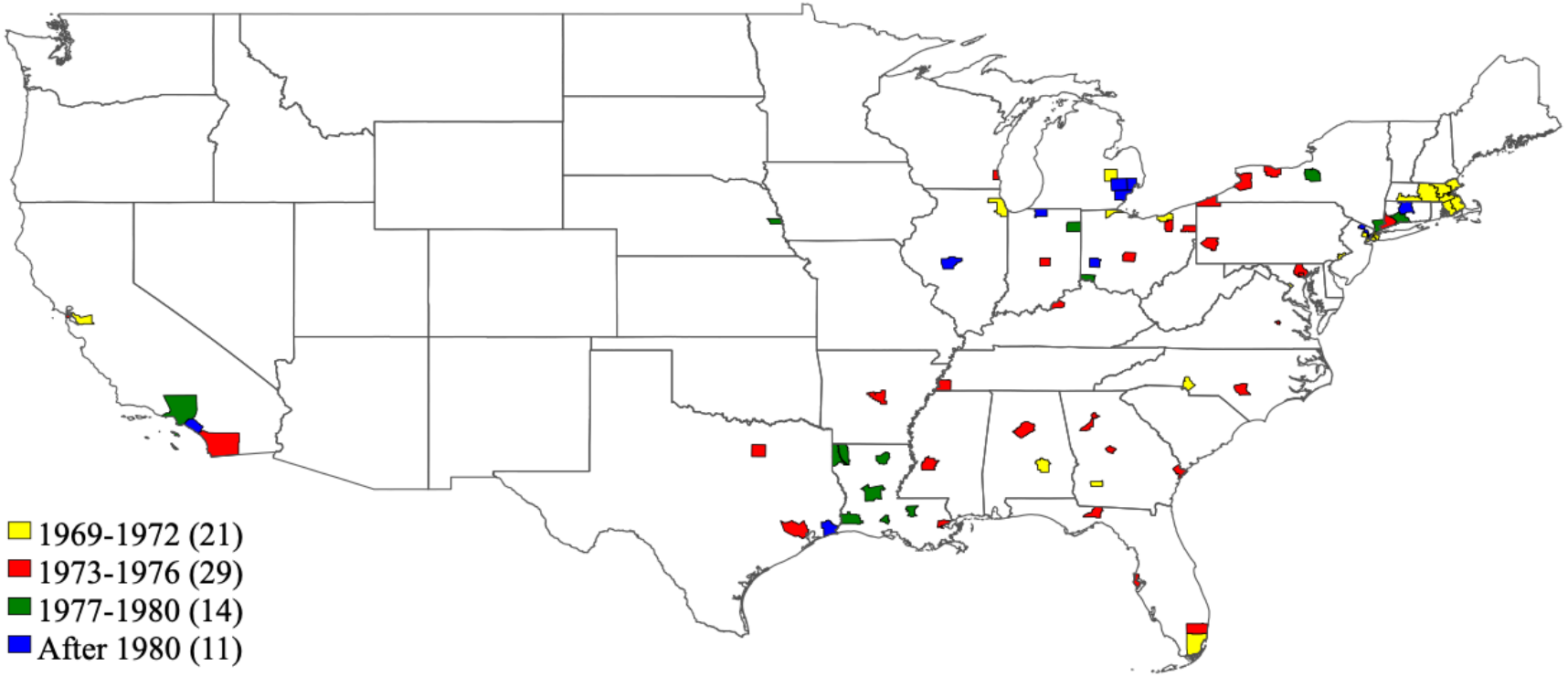
Figures

Figure 1: Cook County Police Killings Over Time



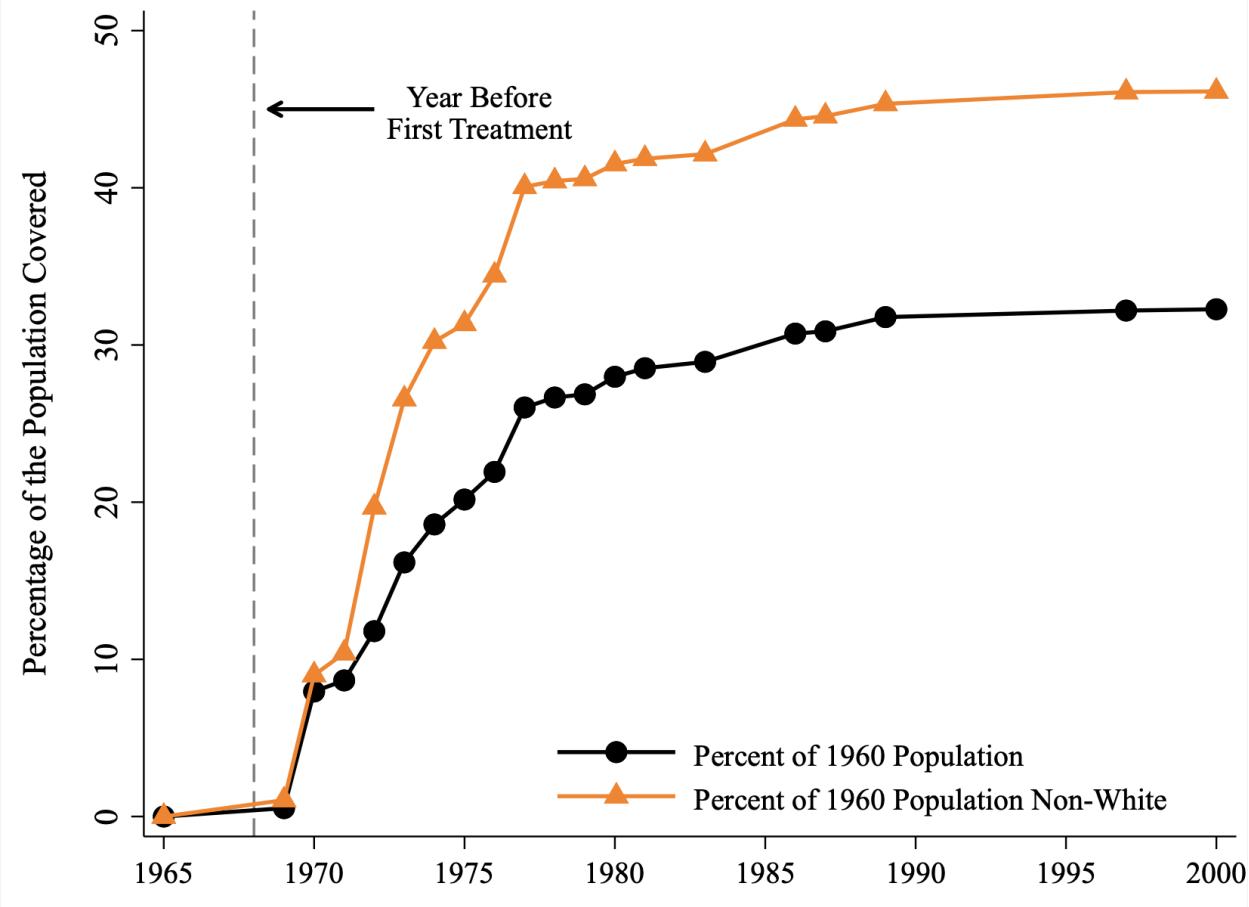
Notes: Data of Chicago Police Department new hires come from McCrary (2007).

Figure 2: Location of Legal Action, 1969-2000



Notes: Legal action dates and locations come from McCrary (2007).

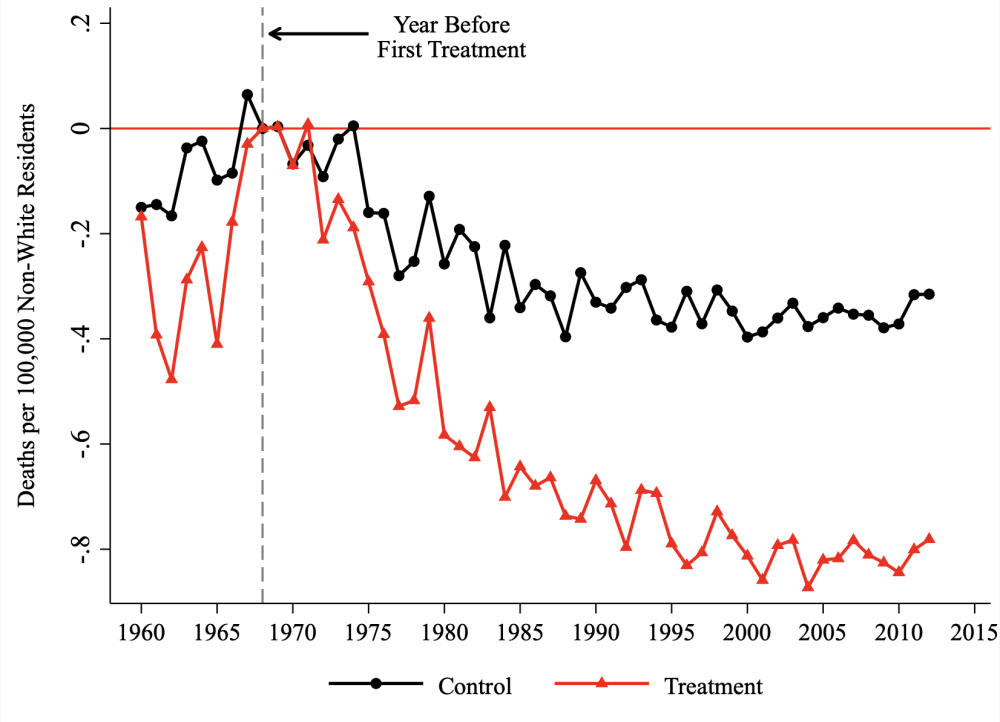
Figure 3: Percent of the Population Residing in Litigated Counties, 1969-2000



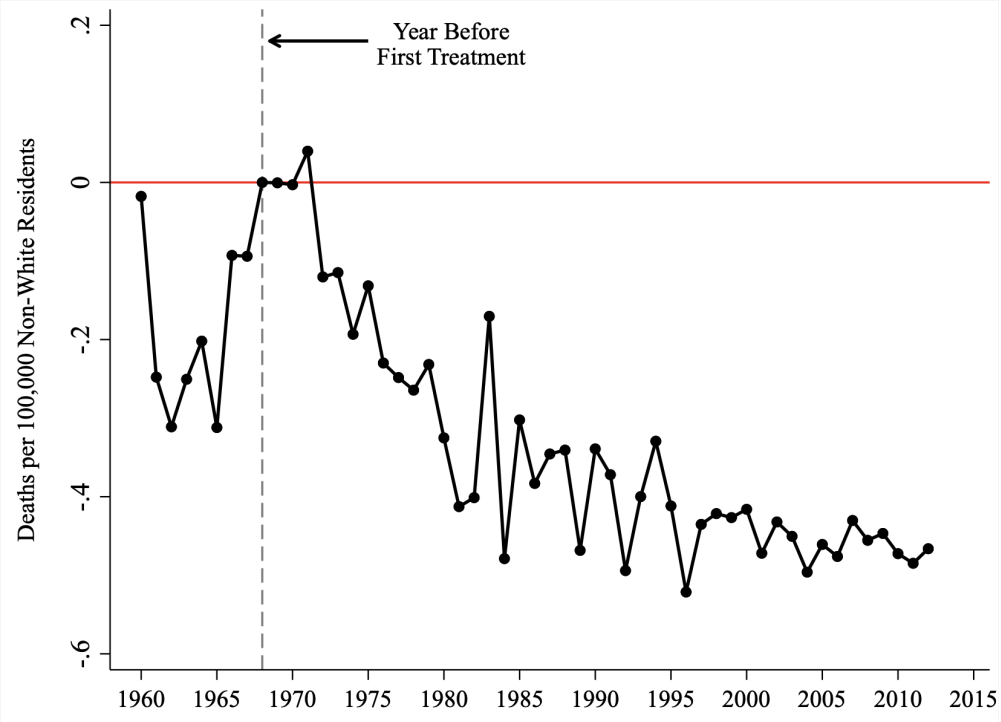
Notes: To calculate the proportion of the population by race, we interpolate the 1960 Census county population to 1968 and use annual county population profiles from the Surveillance, Epidemiology, and End Results (SEER) from 1968-2000.

Figure 4: Police Killings - Normalized Difference

(a) Non-White Deaths



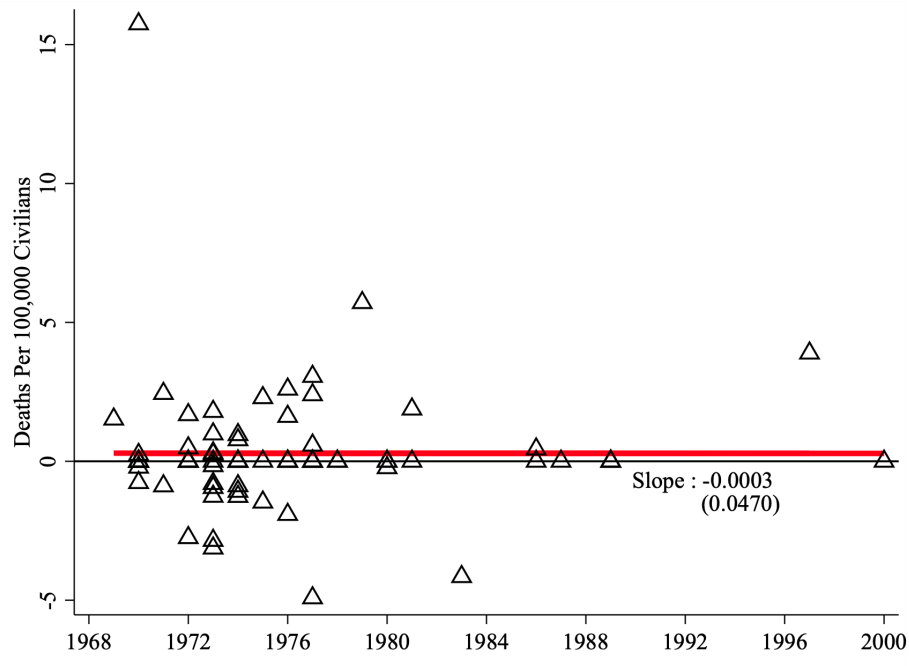
(b) Difference - Treated vs. Control



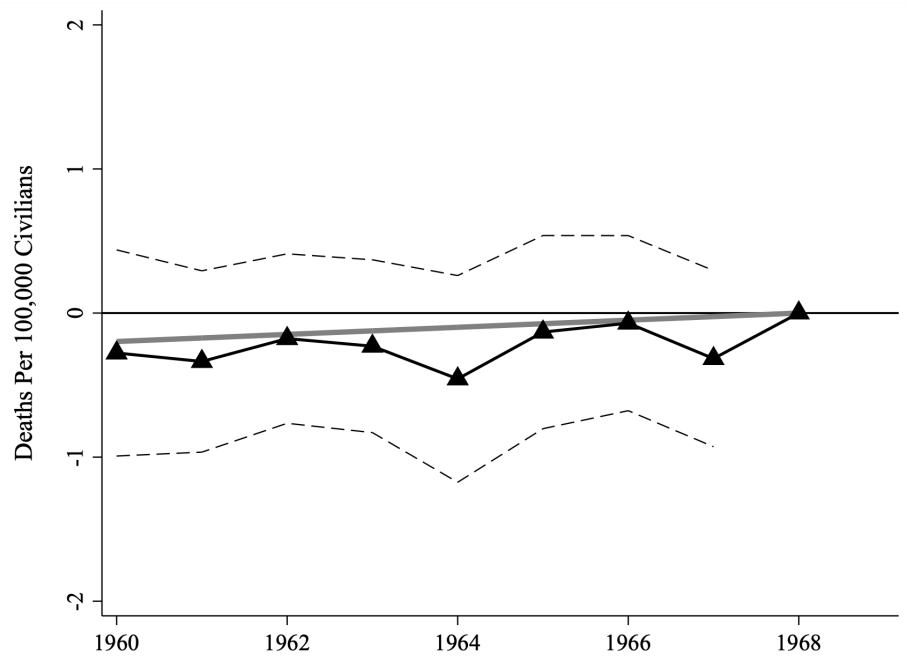
Notes: Panel (a) Police Killings were normalized to zero in 1968. Panel (b) plots the difference in police killings between treated and control groups.

Figure 5: Pre-Period Growth Rates

(a) Pre-Trend Growth Rates in Non-White Deaths (1960-1968)



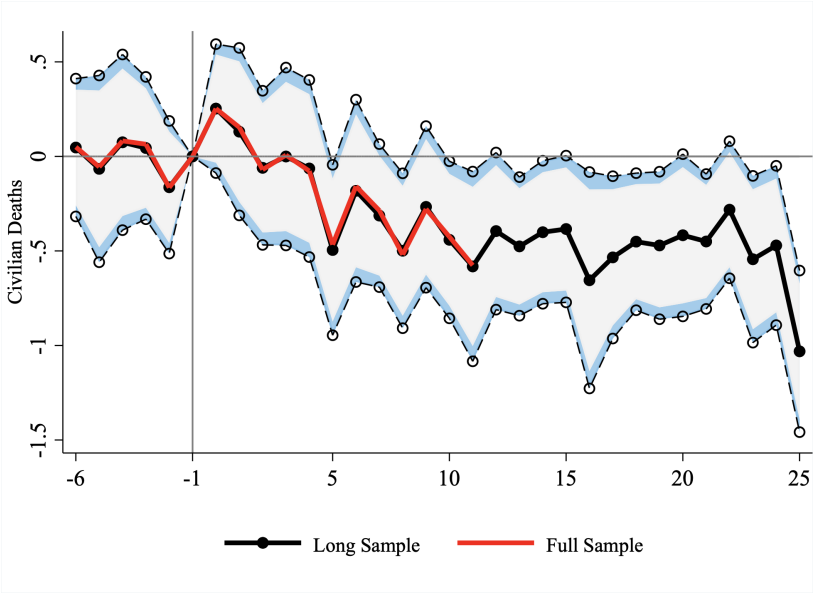
(b) Pre-Period Differences in Non-White Deaths



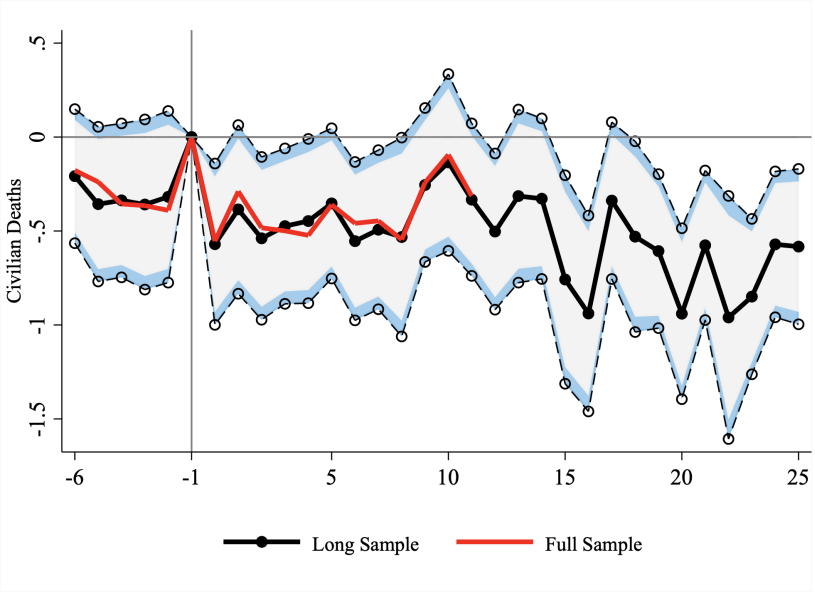
Notes: **[Panel A]** Regression coefficients and predicted values are from a univariate regression of the dependent variable changes in the non-white death rate on the year a county is threatened with litigation. **[Panel B]** The dependent variable is non-white deaths per capita. The independent variables are year fixed effects (1960-1968) – Y, treatment indicator (0/1 if ever litigated) –T, and year by treatment effects $T \times Y$. The coefficients plotted are the coefficients on the interaction terms.

Figure 6: Event Study – Deaths Due to Legal Intervention

(a) Non-White: Poisson

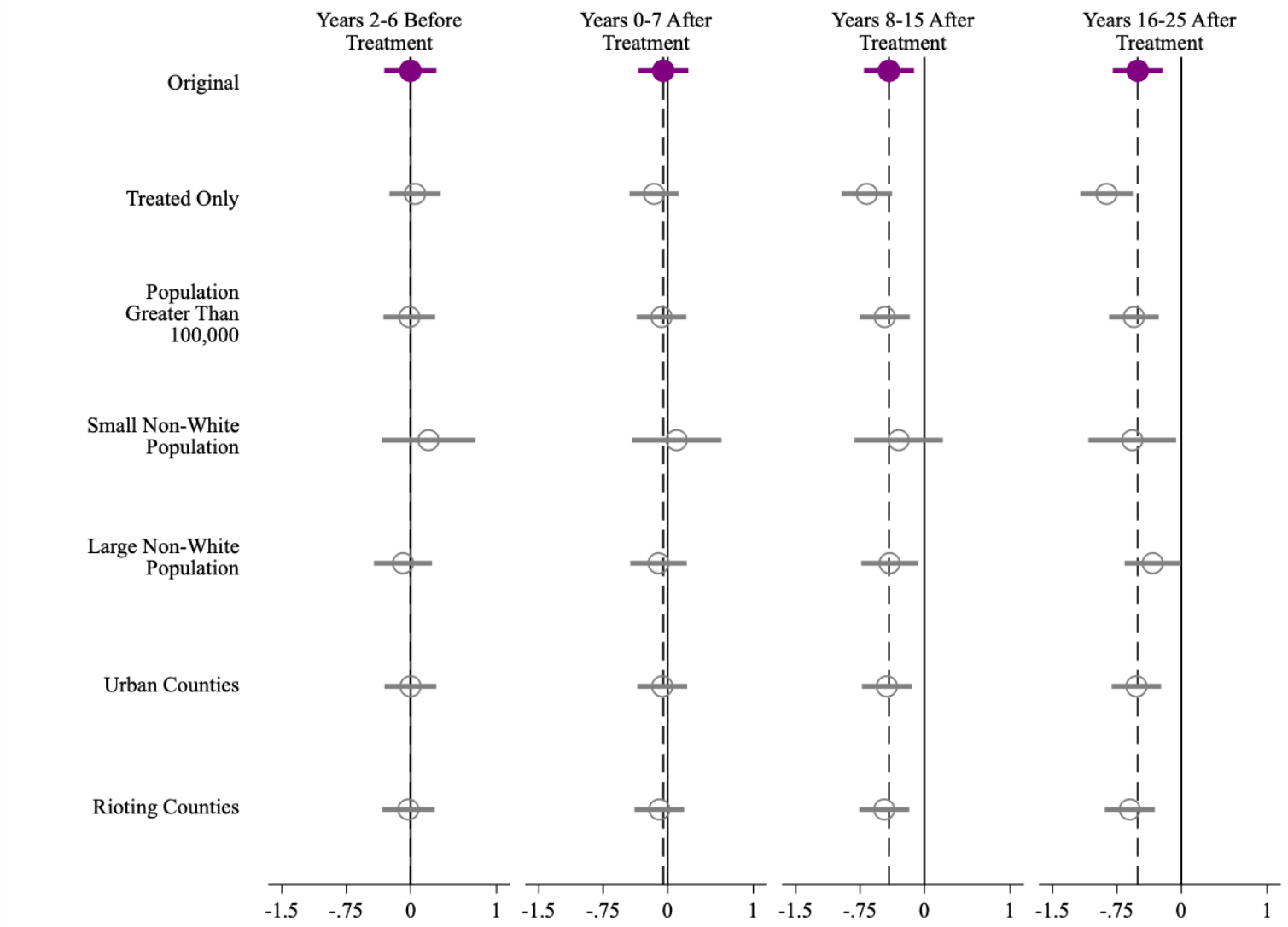


(b) White: Poisson



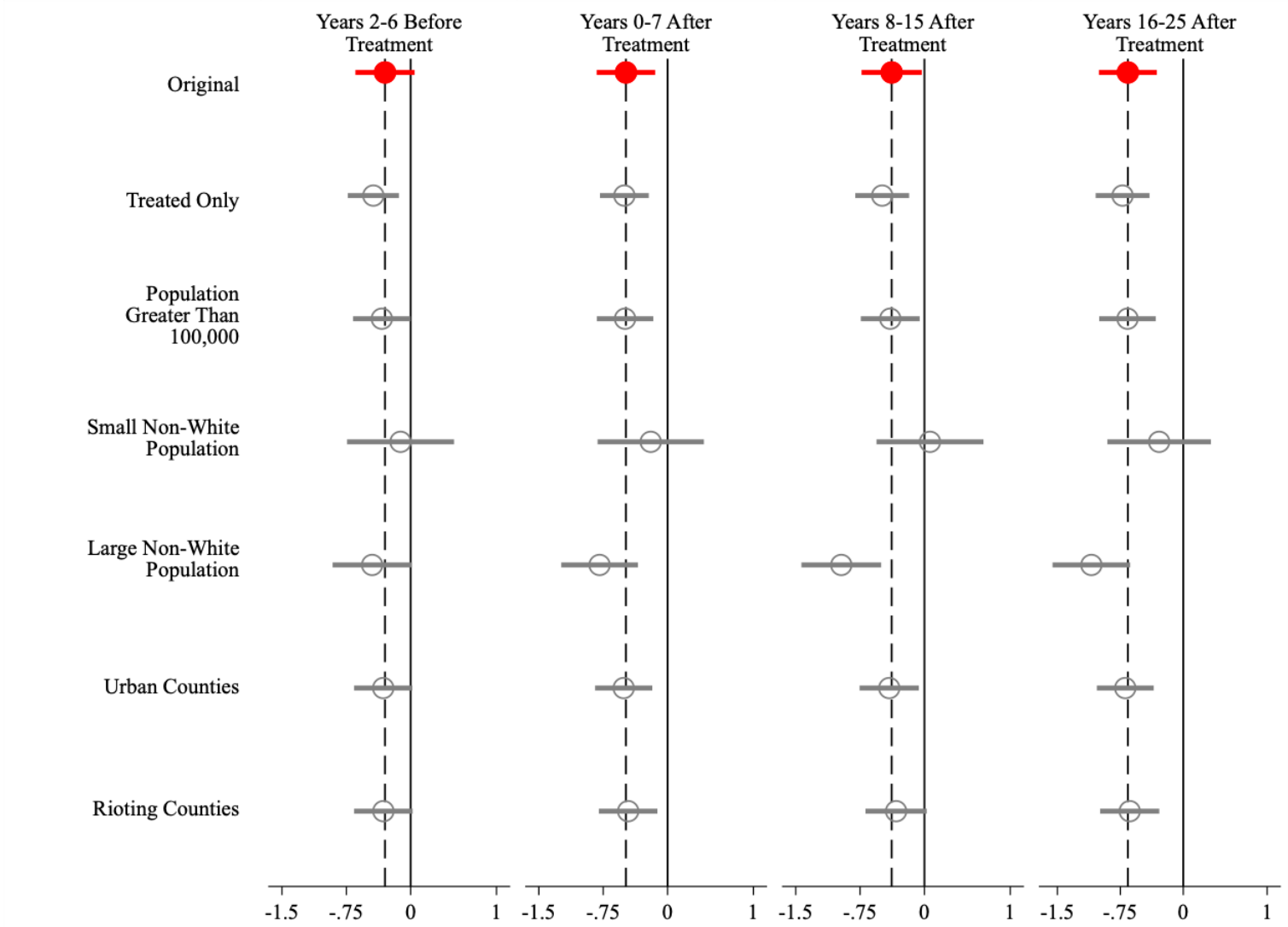
Notes: The Poisson regression specification includes county, C, and region-by-year R-Y, effects. The red line corresponds to the full sample, locations treated between 1969 and 2000. The black line with circle markers correspond to the long-sample, locations treated between 1969 and 1987. Robust standard errors are clustered by county and 95 and 90 percent confidence intervals are presented for the long-sample only. The horizontal axis represents event-years (years before and after litigation).

Figure 7: Robustness Checks - Non-White Deaths



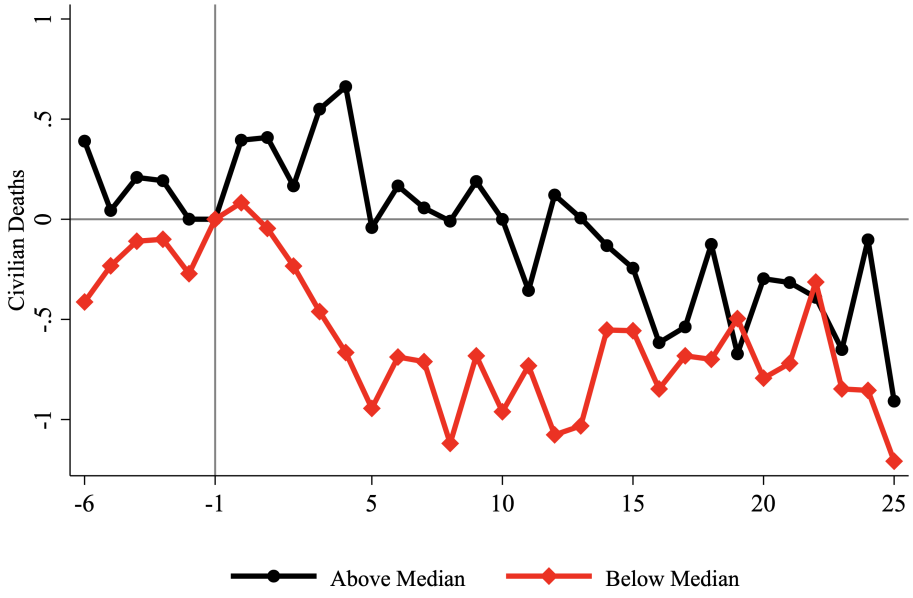
Notes: The figure displays Poisson estimates obtained from estimating Equation 1 by grouping event-years. All rows include county, C, and region-by-year R-Y, effects. Heteroskedasticity-robust standard errors clustered by county are presented by the bold line. Joint lest-square coefficients are presented by circle markers.

Figure 8: Robustness Checks - White Deaths



Notes: The figure displays Poisson estimates obtained from estimating Equation 1 by grouping event-years. All rows include county, C, and region-by-year R-Y, effects. Heteroskedasticity-robust standard errors clustered by county are presented by the bold line. Joint lest-square coefficients are presented by circle markers.

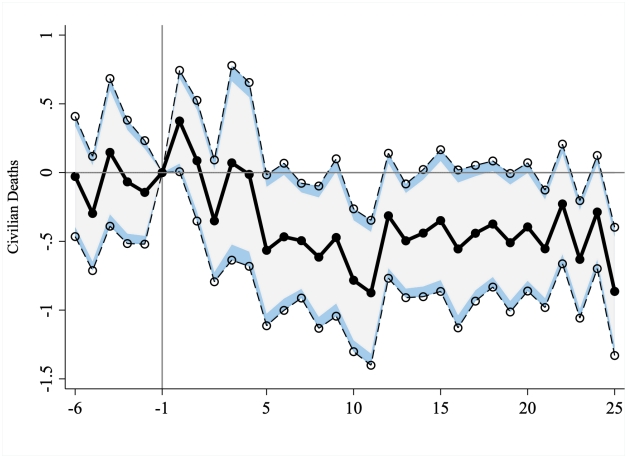
Figure 9: Event-Study Results - Police Representation



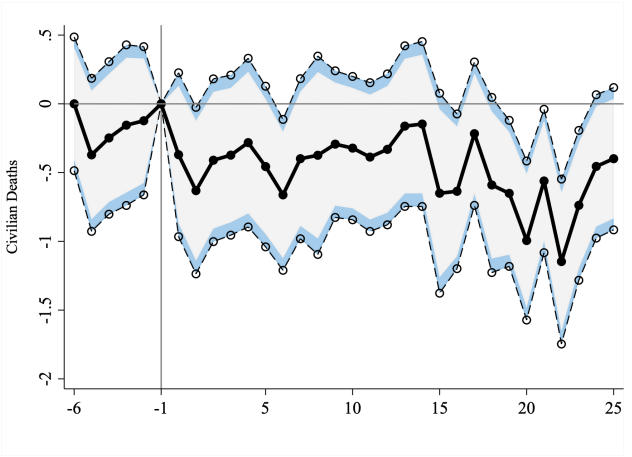
Notes: The regression specification includes county, C, effects and region-by-year, R, effects. We stratify the treated groups to those with police litigated police departments with above/below medain level of representation, 25 years after treatment. Representation is measured as the share of the police of officers that are black divided by the share of the city population that is black. San Francisco and Washington D.C. is omitted from the analysis. Once again, we present robust standard errors. The horizontal axis represents event-years (years before and after litigation).

Figure 10: Event Study Estimates – UCR Arrests Sample

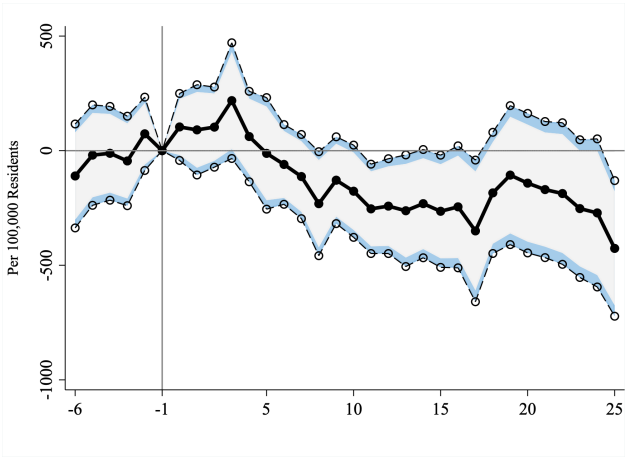
(a) Non-White Deaths



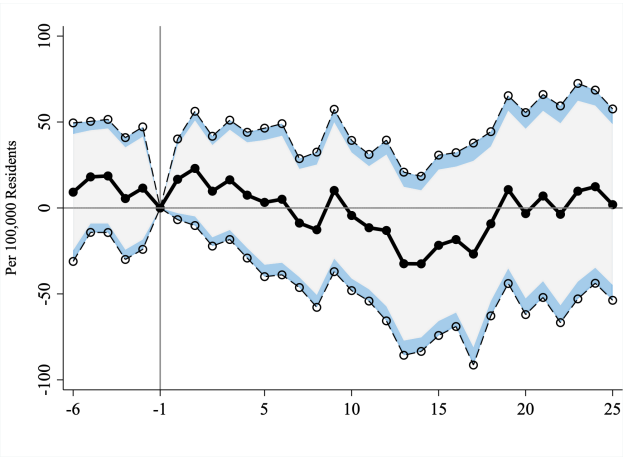
(b) White Deaths



(c) Non-White Arrests



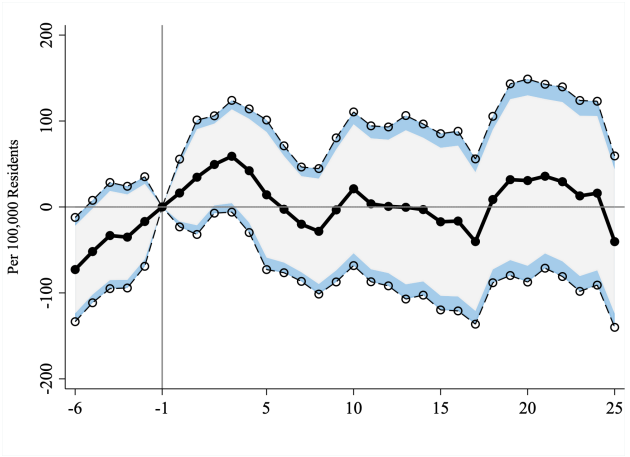
(d) White Arrests



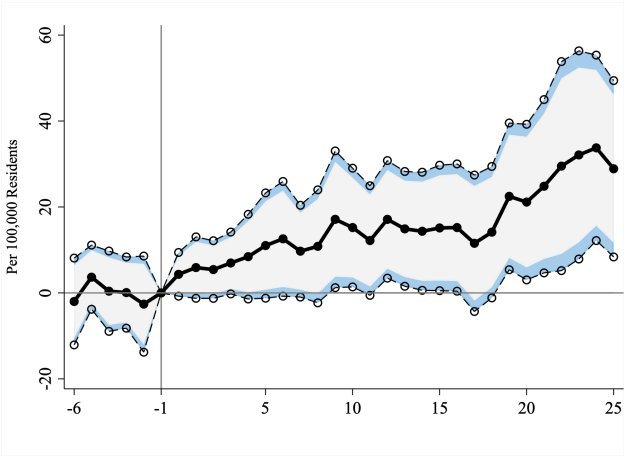
Notes: The regression specification includes county, C, effects and region-by-year, R, effects. Panel (a) and (b) corresponds to civilian deaths by race while panel (c) and (d) report total arrests by race. The sample is limited to counties identified by McCrary (2007) and report at least 52 years between 1960 and 2012. Treatment group corresponds to counties with cities treated prior to 1987. Robust standard errors. The horizontal axis represents event-years (years before and after litigation).

Figure 11: Event Study Estimates – UCR Arrests Sample – by Arrest Type

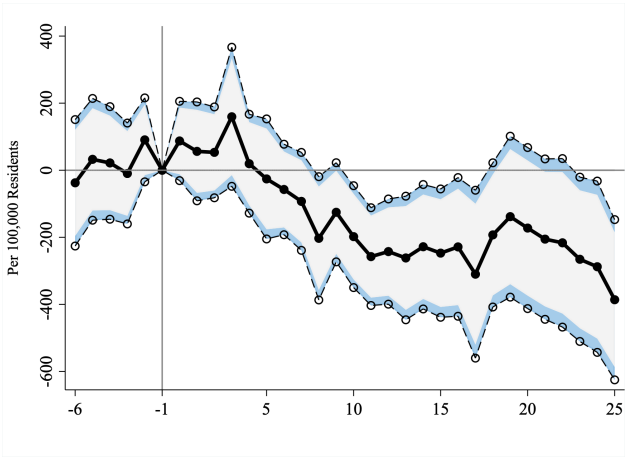
(a) Non-White Arrests – Violent Crimes



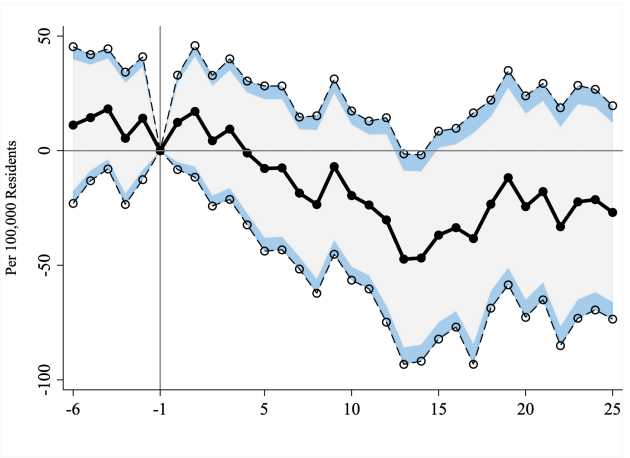
(b) White Arrests – Violent Crimes



(c) Non-White Arrests – Property Crimes



(d) White Arrests – Property Crimes



Notes: The regression specification includes county, C, effects and region-by-year, R, effects. Panel (a) and (b) corresponds to violent crime arrests by race while panel (c) and (d) report property crime arrests by race. The sample is limited to counties identified by McCrary (2007) and report at least 52 years between 1960 and 2012. Treatment group corresponds to counties with cities treated prior to 1987. Robust standard errors. The horizontal axis represents event-years (years before and after litigation).

Tables

Table 1: **Variation in Litigation Over Time**

Treatment Status	Number of Counties	Percent of Counties	Percent of 1960 Population
Treated	75	2.51	32.27
Year Treated:			
1969	1	0.03	0.53
1970	11	0.40	7.95
1971	2	0.47	8.66
1972	7	0.70	11.79
1973	11	1.07	16.17
1974	9	1.37	18.58
1975	3	1.47	20.17
1976	6	1.68	21.93
1977	8	1.94	26.03
1978	2	2.01	26.66
1979	1	2.04	26.86
1980	3	2.14	27.98
1981	2	2.21	28.52
1983	1	2.24	28.93
1986	2	2.31	30.73
1987	1	2.35	30.87
1989	3	2.45	31.78
1997	1	2.48	32.19
2000	1	2.51	32.27
Untreated	**2910**	**97.49**	**67.73**

Note: Data on threats of litigation comes from McCrary(2007).

Table 2: Summary Statistics

1960 Characteristics	(1) Overall	(2) Treatment	(3) Control Group	(4) T-Test of Difference
Population	59,431	815,406	39,947	<0.01
Population per square mile	165.74	3,214.55	87.16	<0.01
% of counties that experienced uprisings	0.09	0.84	0.07	<0.01
Percentage of the Population				
residing in urban areas	32.50	87.21	31.09	<0.01
w/ 12 or more years of education	36.45	43.41	36.27	<0.01
w/ income greater than 10K	7.92	16.95	7.69	<0.01
w/ income less than 3K	35.62	17.95	36.07	<0.01
non-white	10.94	16.61	10.80	<0.01
Deaths Due to Legal Intervention				
white	0.04	0.61	0.03	<0.01
non-white	0.04	1.03	0.01	<0.01
Number of Counties	2,985	75	2,910	
joint F-test				3.58
p-value				<0.01

Note: Authors' calculations.

Table 3: Event Study - Joint Effects

	(1)	(2)	(3)	(4)	(5)	(6)
		Non-White			White	
	OLS	Poisson	WLS	OLS	Poisson	WLS
Pre-Period Effect (Event Years -6 to -2)	-0.0201	-0.00352	0.0272	-0.291	-0.299*	-0.0477*
	[0.217]	[0.155]	[0.0990]	[0.208]	[0.176]	[0.0254]
Shorter-Run Effect (Event Years 0 to 7)	-0.165	-0.0500	-0.0379	-0.457**	-0.485***	-0.0651***
	[0.262]	[0.149]	[0.111]	[0.215]	[0.174]	[0.0246]
Medium-Run Effect (Event Years 8 to 15)	-0.594*	-0.413***	-0.249**	-0.383	-0.382**	-0.0592**
	[0.321]	[0.149]	[0.114]	[0.263]	[0.179]	[0.0244]
Longer-Run Effect (Event Years 16 to 25)	-0.671**	-0.508***	-0.280**	-0.507*	-0.660***	-0.0827***
	[0.319]	[0.148]	[0.121]	[0.269]	[0.176]	[0.0250]
Mean Dependent Variable	1.457	1.457	0.797	1.171	1.171	0.159
Number of Counties	2,980	2,980	2,707	2,980	2,980	2,707

Note: Sample removes counties treated after 1987 from the analysis. All regressions include county and region-by-year fixed effects. Robust standard errors are clustered by county. *** p<.01, ** p<.05, * p<0.1

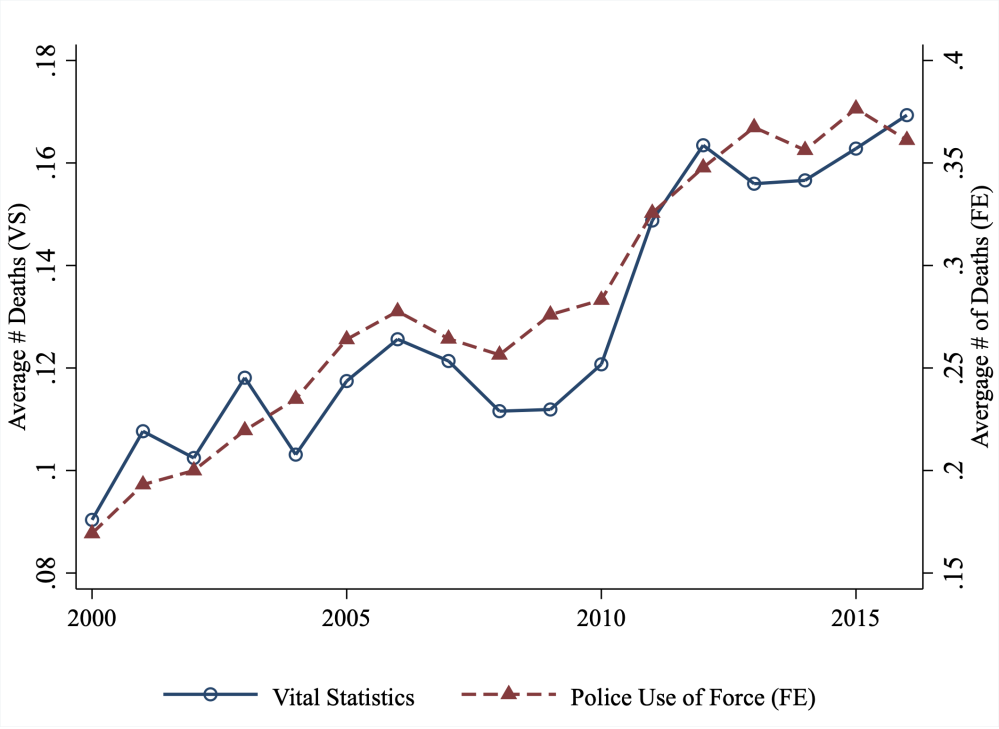
A Data Appendix

We use several data sources in our analysis. Below we describe the data we use and how we obtain the final data set.

A.1 Vital Statistics Data

Our measurement of police killings of civilians comes from the 1959 to 2016 Vital Statistics Multiple Cause of Death Files. The 1958-1988 files are publicly available at the ICPSR at the University of Michigan. For 1989-2016, we use restricted files obtained directly from the CDC. A major shortcoming of using Vital Statistics files to measure police-related fatalities is the severe under-reporting of police-caused deaths. Ideally, we would use newer, crowd-sourced data such as Fatal Encounters to capture police-related fatalities, but the series begins well after locations are treated. Despite the under-count, Vital Statistics does reasonably well with capturing national trends. Below, Appendix Figure A1 shows that the Vital Statistics and Fatal Encounters series on police-related fatalities mirror each other over time. Also, Appendix Figure A2 plot the time-series by treatment status.

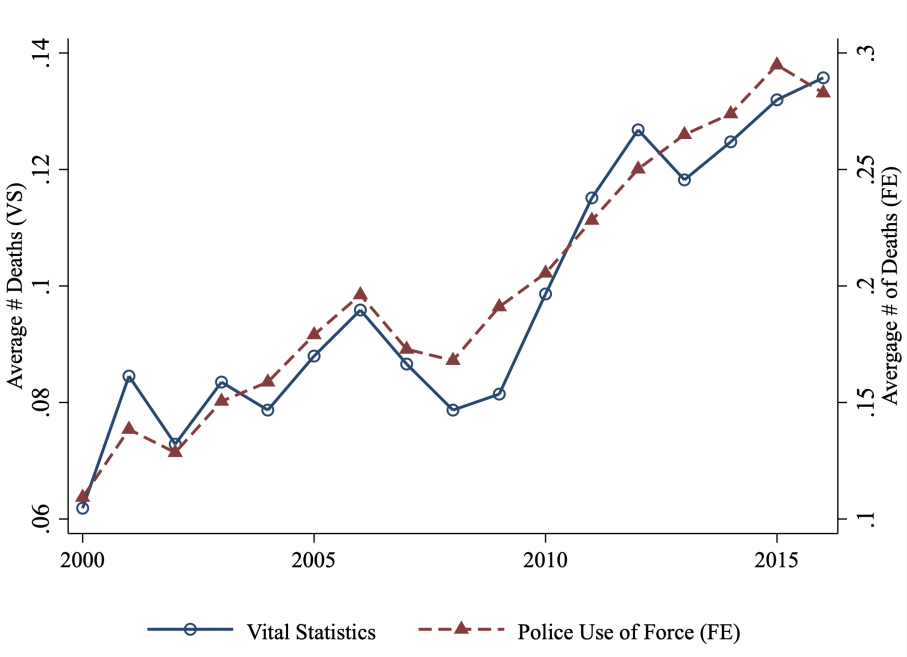
Figure A1: Police Killings, 2000-2016



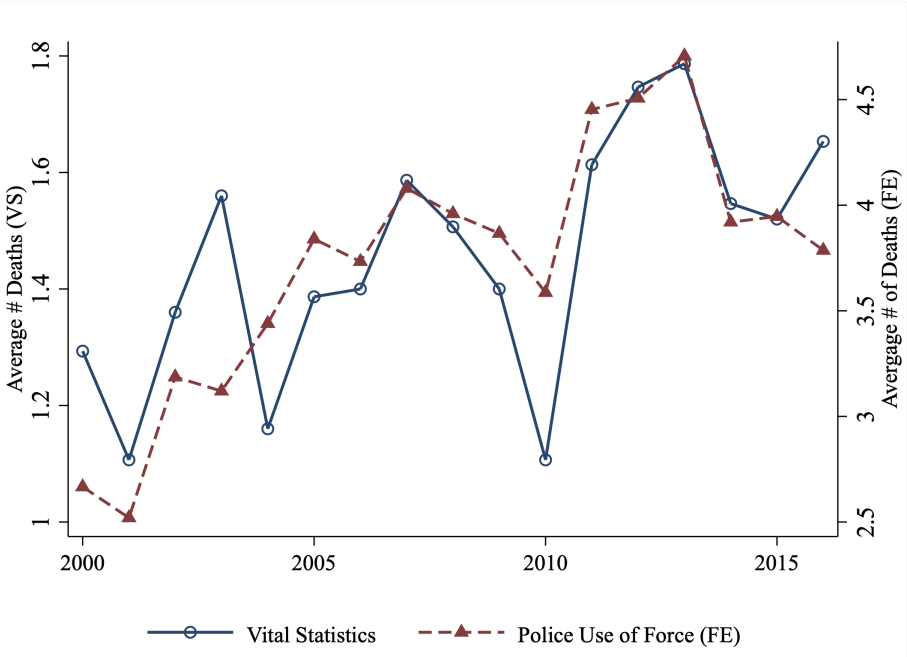
Notes: Data are from the 1998 to 2016 Vital Statistics Multiple Cause of Death Files and the Fatal Encounters Data (see Fatal Encounters Website).

Figure A2: Police Killings by Treatment Status, 2000-2016

(a) Control Group



(b) Treatment Group



The Vital Statistics files cover several decades with many changes to coding police-related fatalities. For years 1999 to 2016 we use the following ICD-10 codes to classify police-related deaths: Y35.0-Y35.4, Y35.6, Y35.7, and Y89.0. For 1968 to 1998, we use the following ICD-9 and ICD-8 codes: E970-E977. Finally, for 1959 to 1967, we use the following ICD-7 code: E984. For suicide deaths from 1999 and 2016, we use the following ICD-10 codes: X60-X84, Y870, U030. For suicide deaths from 1968 to 1998 we use the following ICD-9 and ICD-8 code: E950-E959. For 1959-1967, we use the following ICD-7 codes: E963, E970-E979.

A.2 UCR Data

Information for arrests and police employment comes from the Uniform Crime Report Data Series, which is publicly available at the ICPSR at the University of Michigan.

A.2.1 Police Employment

Uniform Crime Reporting (UCR): Law Enforcement Officers Killed or Assaulted UCR Law Enforcement Officers Killed or Assaulted (LEOKA) contains monthly counts of law enforcement officers killed and assaulted as well as annual law enforcement employment. The UCR reports the number of civilian officers and sworn officers as of October 31st of the reporting year. The UCR LEOKA is available publicly at the ICPSR website for years after 1974. For years 1960 to 1974 data are manually entered from hard copies of UCR LEOKA reports.

A.2.2 Arrest Data

UCR Arrests by Age, Sex, and Race is publicly available at the Inter-University Consortium for Political and Social Research (ICPSR). It provides information on the number of arrests reported by local and state law enforcement agencies to the Federal Bureau of Investigation (FBI). The data compiled for the UCR is submitted voluntarily by city, county, and state enforcement agencies. The FBI or state law agencies directly provide survey forms to local law agencies or state collecting programs, which are collected on a monthly basis. The data on arrests includes yearly information on the number of arrests within 43 categories including property and violent crime as well as drug-related offenses; it also includes information on the age, sex, and race of arrestees.

A.3 Police Diversity

Law Enforcement Management and Administrative Statistics (LEMAS) is publicly available at the Inter-University Consortium for Political and Social Research (ICPSR) at the University of Michigan. LEMAS provides detailed survey data from police agencies on their equipment, personnel, and capabilities. The survey is conducted every three years and includes most large law enforcement agencies. The survey contains information on the demographic composition of law enforcement officers. Our analysis focuses on full-time sworn officers by race.

A.4 Demographic Information

County and City Data Books provide information on city demographics and local government expenditures for counties, cities, and incorporated areas of 25,000 inhabitants or more in the United States. Data on population counts as well as demographic information are from the decennial census conducted in the beginning of each decade. Data on local government operations, revenue, and expenditures are from the Annual Survey of Governments, which is conducted for a sample of governments by the Census Bureau. We use County and City Data Books for 1962 and 1967. We obtain yearly population counts from the 1960 Census (Haines et al., 2010) and the Surveillance, Epidemiology, and End Results (SEER) annual data, which begin in 1968 (interpolated between 1960 and 1968).

A.5 Uprising Data

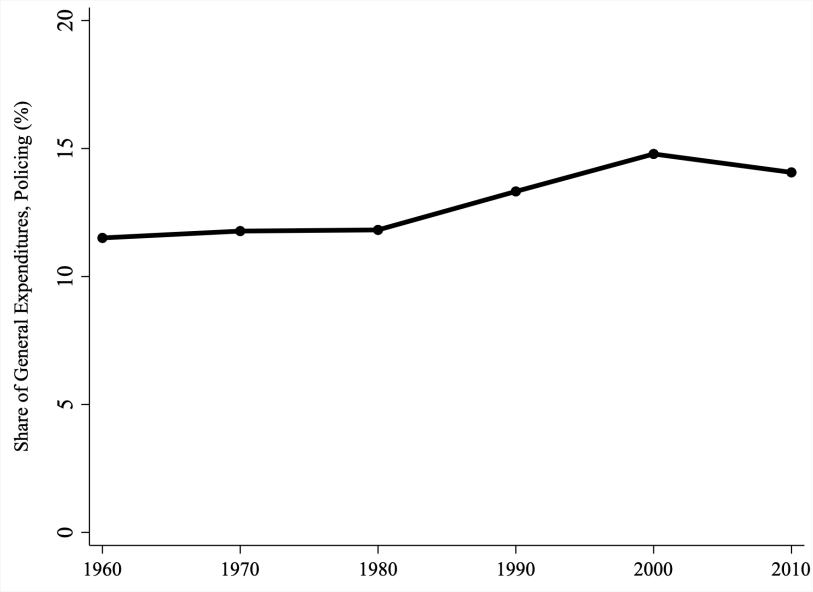
Data for rioting was provided by Collins and Margo (2007) and was originally collected by Carter (1986). The data consist of detailed information on arrests, injuries, and deaths caused by riots in each city where rioting occurred. Using this information we create a dummy variable to identify the year and county in which rioting occurred. We also create an index of severity by summing the number of arrests, injuries, arsons, and deaths in the year and dividing them by the total sum of injuries, deaths, arsons, and arrests due to rioting. The severity index is similar to other rioting measures used in research that analyzes the impact of rioting.

A.6 Final Sample

To compile our final sample we first collapse the litigation data, courtesy of McCrary (2007), and collapse the treated locations to their respective counties. We identify a treated county by the earliest year a city within a county is threatened with affirmative action litigation. We then merge the litigation data with Vital Statistics data using state and county FIPS identifiers. Note, all counties are identified by their 1960 definition. Although San Francisco and Washington D.C. are excluded from McCrary (2007), we include them because we know the litigation dates and outcome of interests. Lastly, we restrict our sample to counties that have a non-white population in every year of record (which excludes 30 counties).

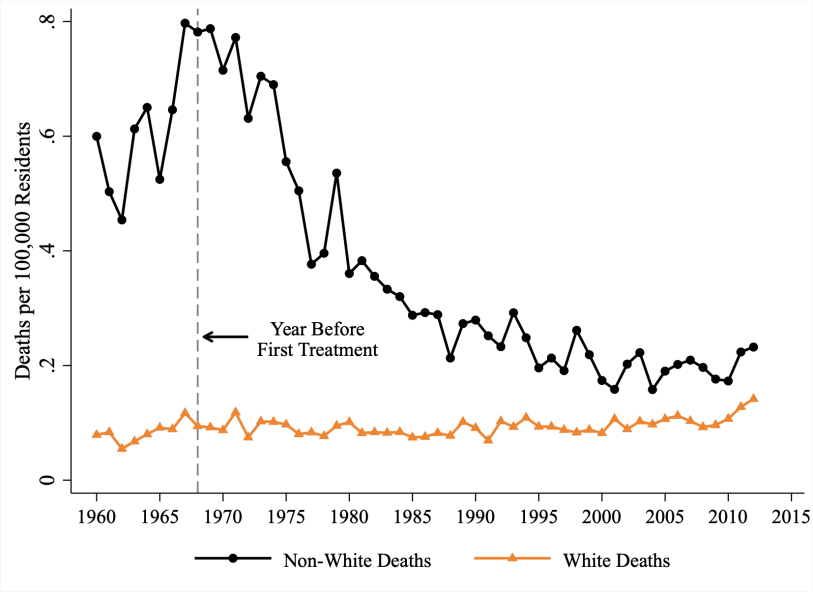
B Appendix Figures

Figure B1: Police Spending Over Time, 1960-2010



Notes: Data comes from City and County Data Books publicly available at the ICPSR and www.census.gov.

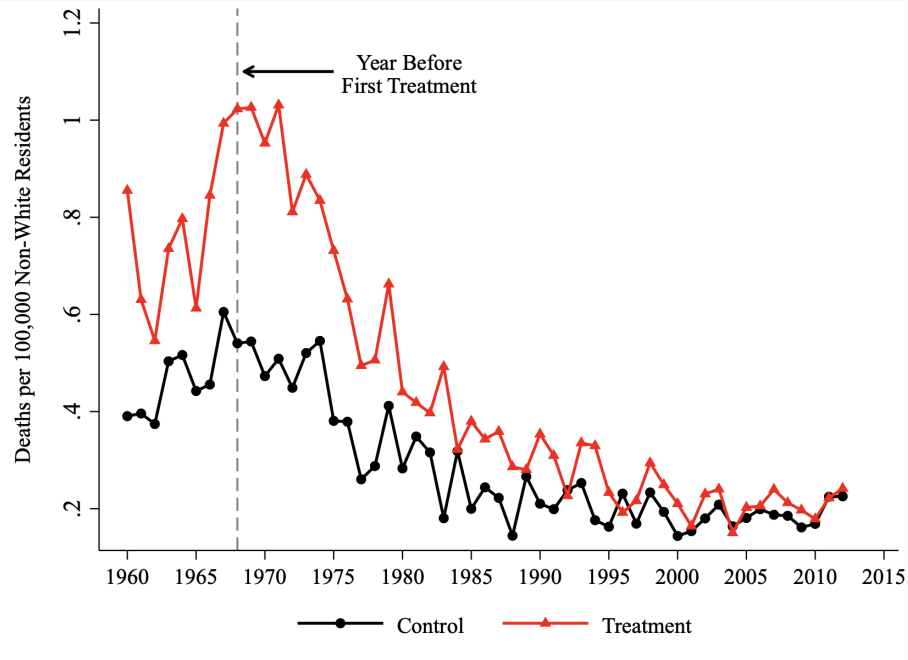
Figure B2: Police Killings Over Time, 1960-2012



Notes: Data comes from United States Vital Statistics

Figure B3: Police Killings Over Time by Treatment Status

(a) Non-White Deaths



(b) White Deaths

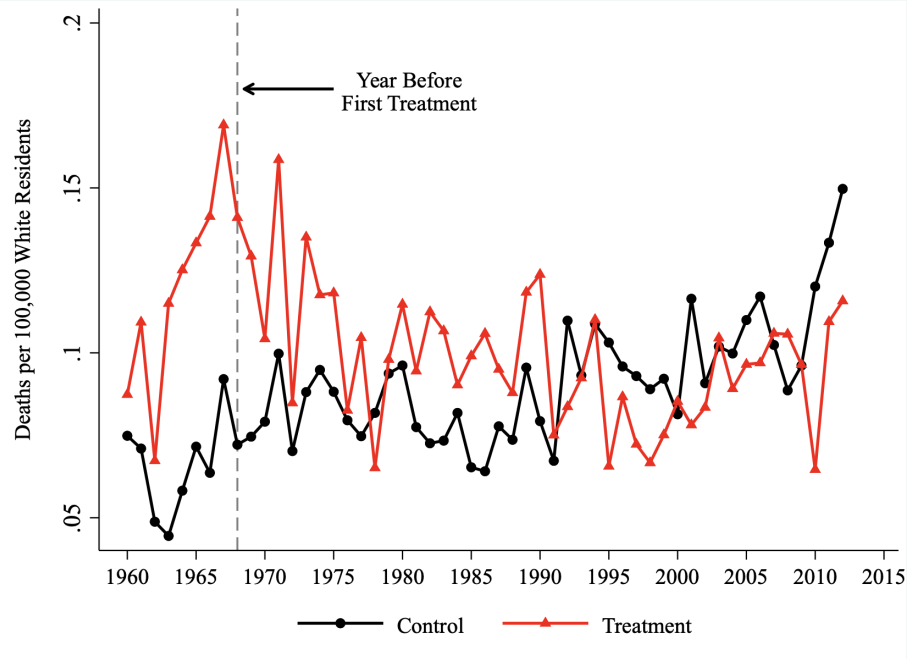
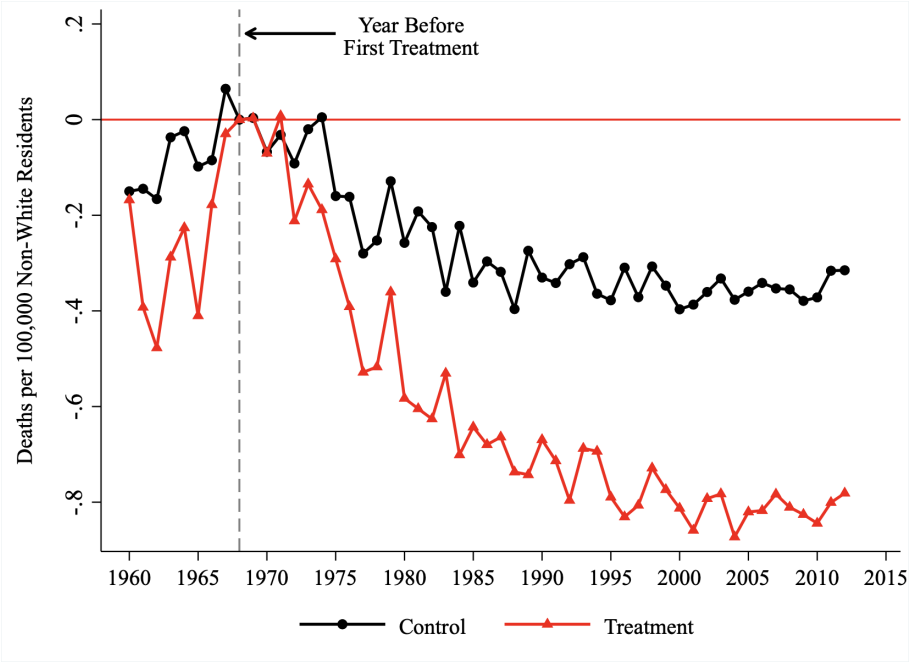
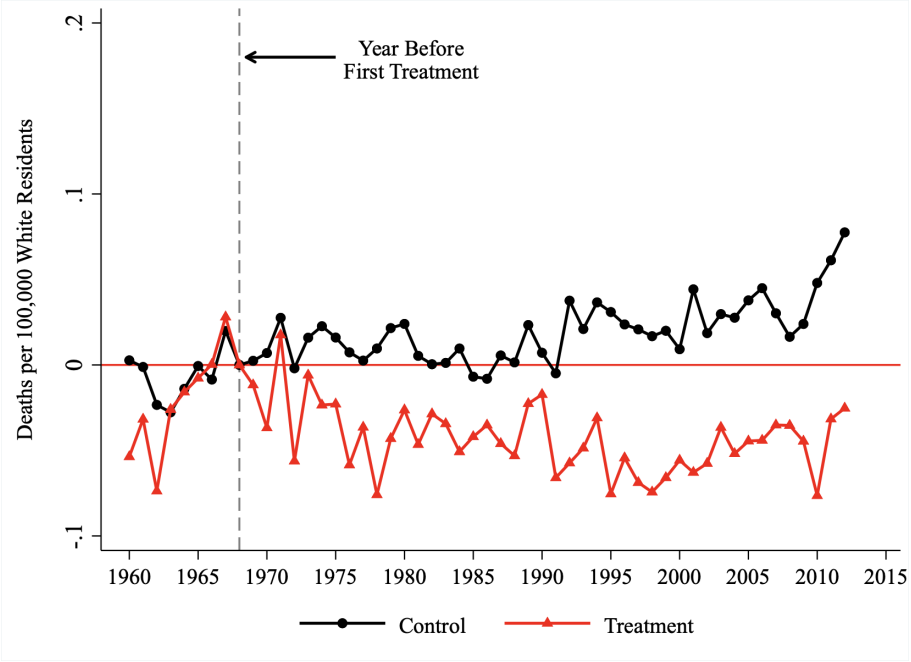


Figure B4: Police Killings - Normalized Difference

(a) Non-White Deaths



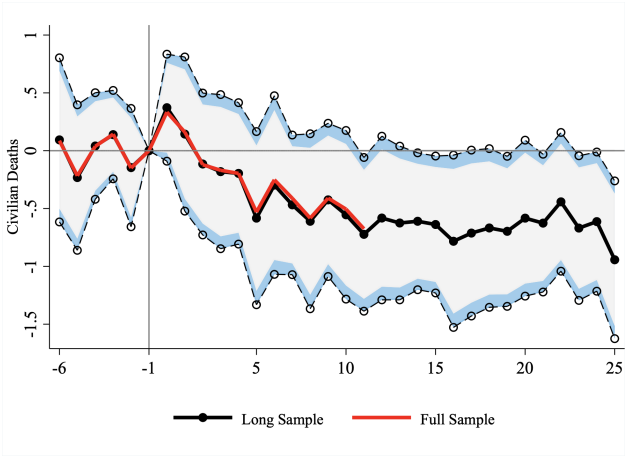
(b) White Deaths



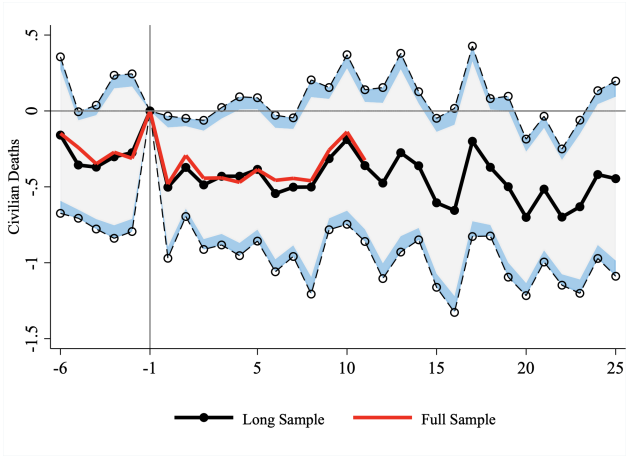
Notes: Police Killings have been normalized to zero in 1968.

Figure B5: Event Study – Least Squares Regressions

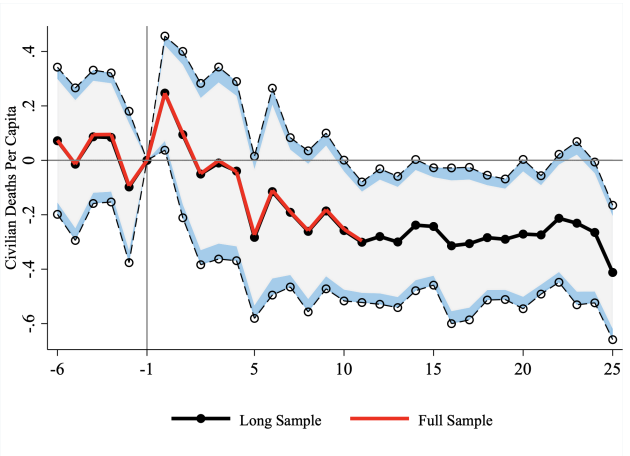
(a) Non-White: OLS



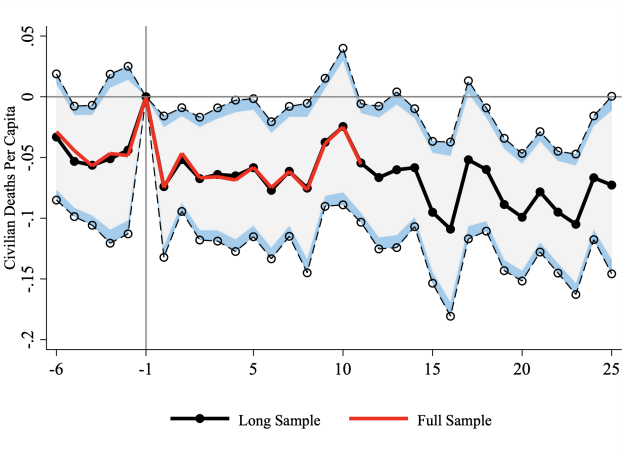
(b) White: OLS



(c) Non-White: WLS



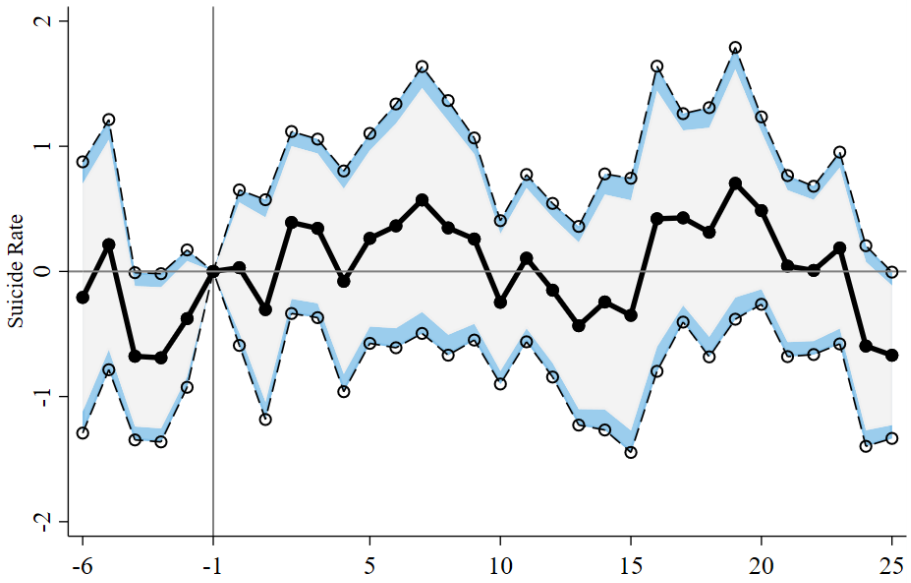
(d) White: WLS



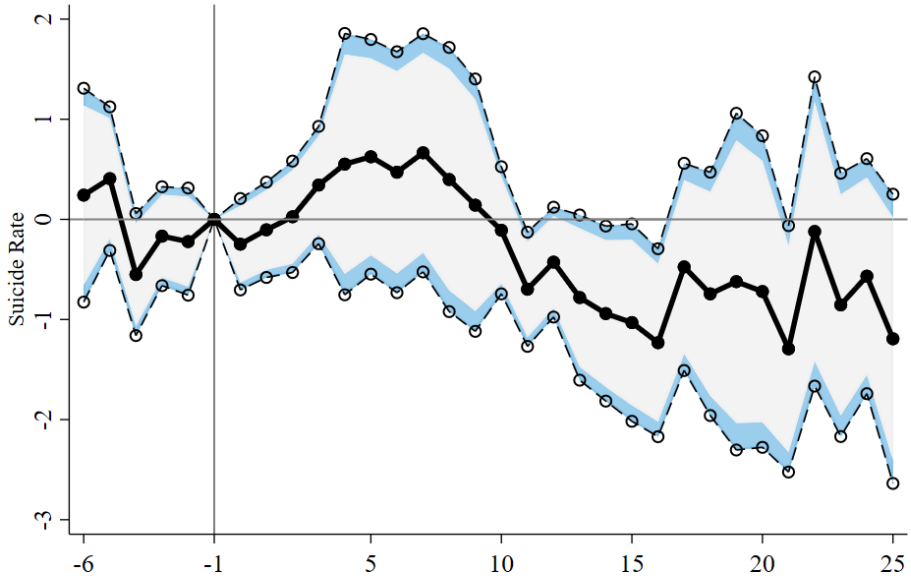
Notes: The ordinary least squares and weighted least squares regression specification includes county, C, and region-by-year R-Y, effects. Number of deaths by race is the dependent variable in panels (a) and (b). The deaths per 100,000 residents by race is the dependent variable in panels (c) and (d). Robust standard errors are clustered by county and 95 and 90 percent confidence intervals are presented for the long sample only. The horizontal axis represents event-years (years before and after litigation).

Figure B6: Event Study Results – Suicides

(a) Non-White

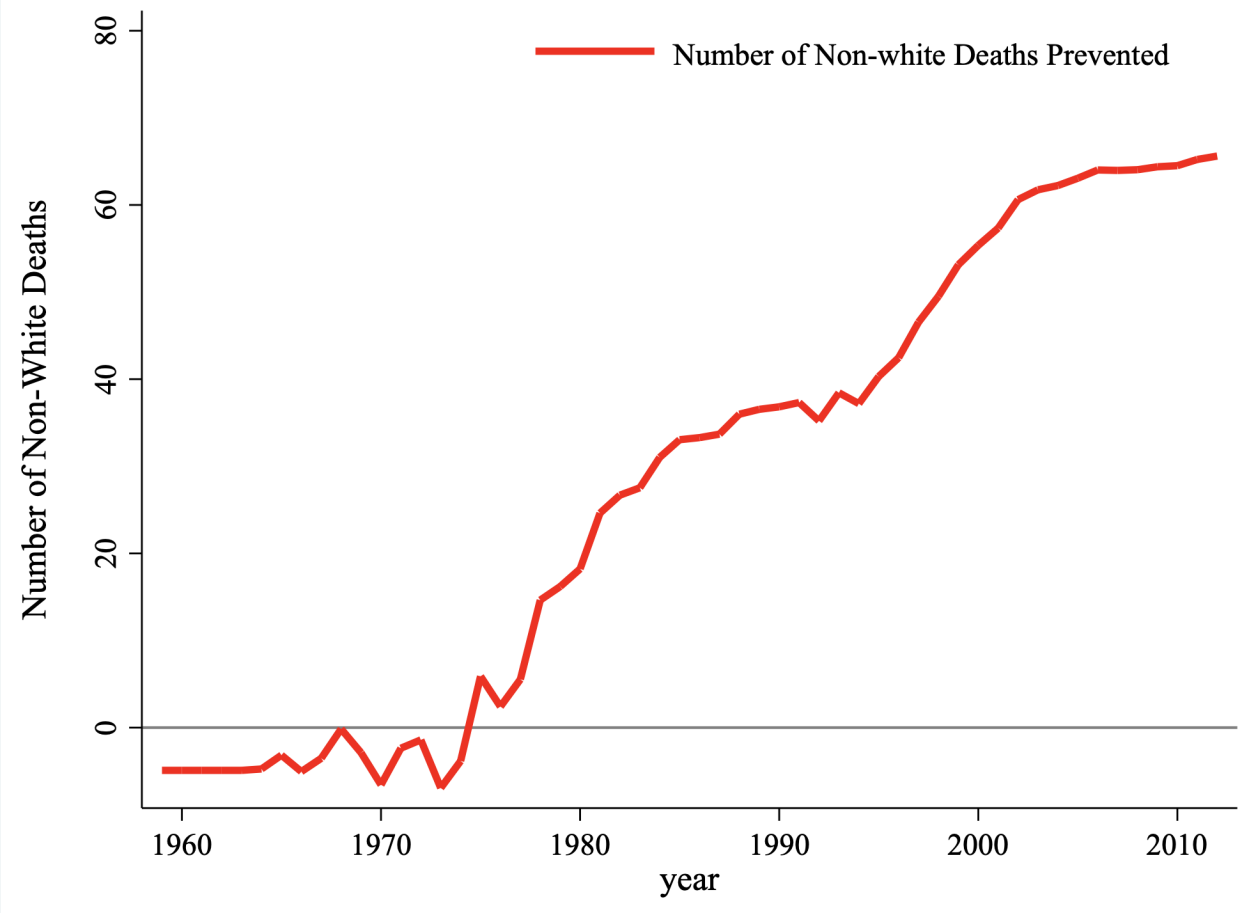


(b) White



Notes: The weighted least squares regression specification includes county, C, and region-by-year R-Y, effects. Robust standard errors are clustered by county. 1960 population by race are used as weights. The horizontal axis represents event-years (years before and after litigation).

Figure B7: Number of Deaths Prevented



Notes: Based on Authors' Calculations.

B.1 Racial Disparities in Police Killings of Civilians

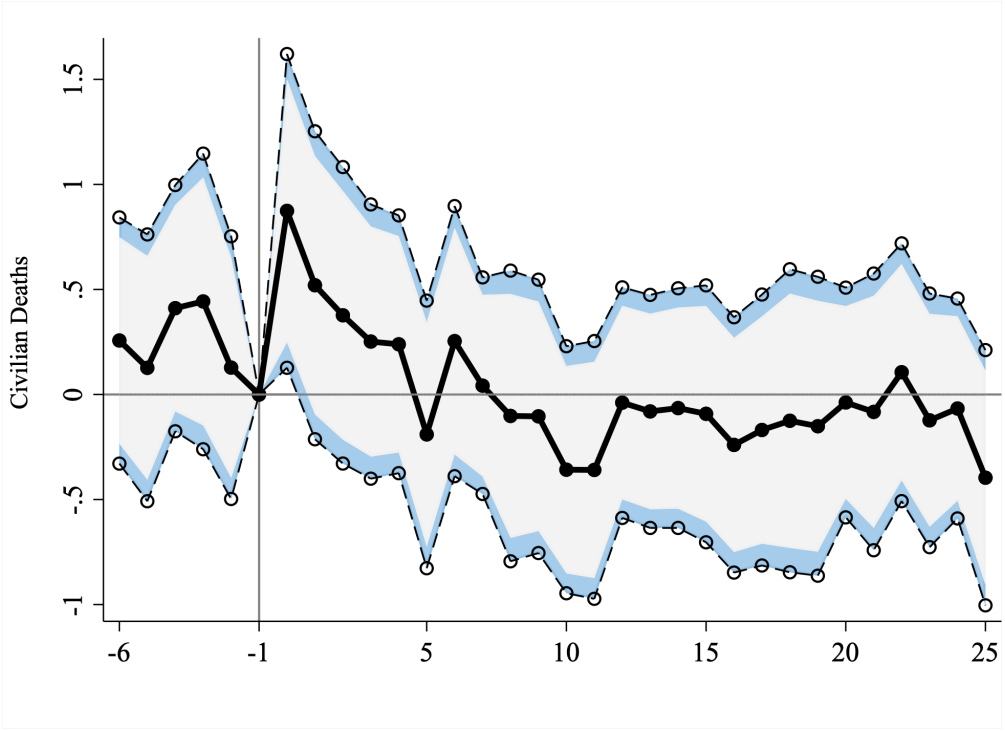
We test for racial disparities in police killings by converting equation (1) into a triple difference strategy (DDD) following Cox and Cunningham (2021). We estimate the following equation using OLS:

$$
\begin{aligned}
y_{ct} = {} & \alpha_c + \gamma_{r(c),t} + NW_k + NW_k\gamma_t + NW_k D_c \\
& + \sum_{j=2}^{7} \pi_j D_c \mathbb{1}\{t - t_c^* = -j\} + \sum_{j=0}^{26} \phi_j D_c \mathbb{1}\{t - t_c^* = j\} \\
& + \sum_{j=2}^{7} \lambda_j D_c \mathbb{1}\{t - t_c^* = -j\} NW_k + \sum_{j=0}^{26} \sigma_j D_c \mathbb{1}\{t - t_c^* = j\} NW_k + v_{ct}
\end{aligned}
$$

where the notation stays as earlier defined in equation 1, and NW_k is equal to one for non-white police killings and zero for white police killings. In this model, we can capture pre-existing trends in racial disparities in police killings of civilians. If the model is correctly specified, the pre-treatment effects will be indistinguishable from zero and any trend break in racial disparities in police killings will be attributed to the threat of litigation. Negative post-treatment effects would suggest that litigation reduces racial disparities in police killings, while positive post-treatment effects would imply that litigation contributes to racial disparities in police killings. If the post-treatment effects are zero, then litigation may not affect racial disparities in police killings.

Appendix Figure B8 plots pre- and post-treatment effects from the OLS model. Overall, pre-treatment effects are not statistically significant. Our model indicates an initial statistically significant increase in racial disparities in police killings at event-year zero. This is not surprising considering the initial rise in non-white deaths in Appendix Figure B5a and the initial decrease in white deaths in Appendix Figure B5b. After the initial increase in racial disparities of police killings, post-treatment effects decrease and eventually become negative, but are not statistically significant. Nonetheless, the initial increase in racial disparities could indicate a distinct change in police behavior in response to federal intervention.

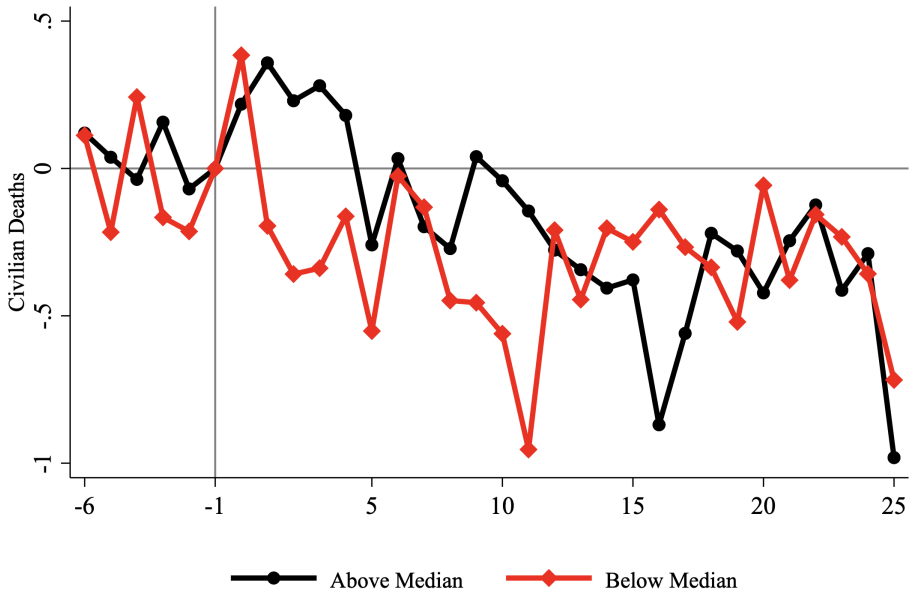
Figure B8: Triple Difference – Racial Disparities in Deaths Due to Legal Intervention



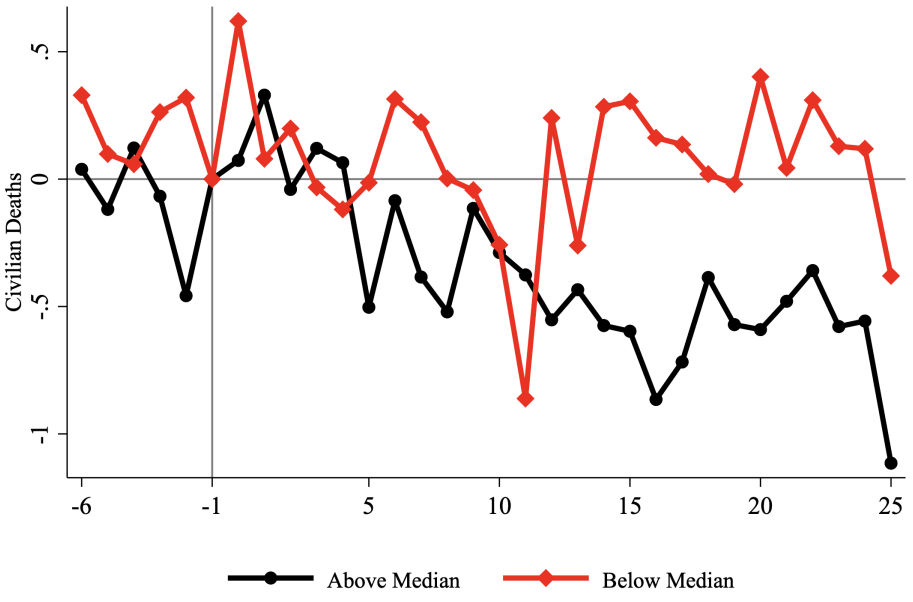
Notes: The ordinary least squares regression specification includes county, C, and region-by-year R-Y, effects. The coefficients are estimated from a Difference-in-Difference-in-Difference model. Marginal effects show the relative change in non-white deaths relative to white deaths in treated counties. Robust standard errors are clustered by county, and 95 and 90 percent confidence intervals are presented for the long-sample only. The horizontal axis represents event-years (years before and after litigation).

Figure B9: Event Study Results – Intensity of Treatment

(a) Total Population Covered

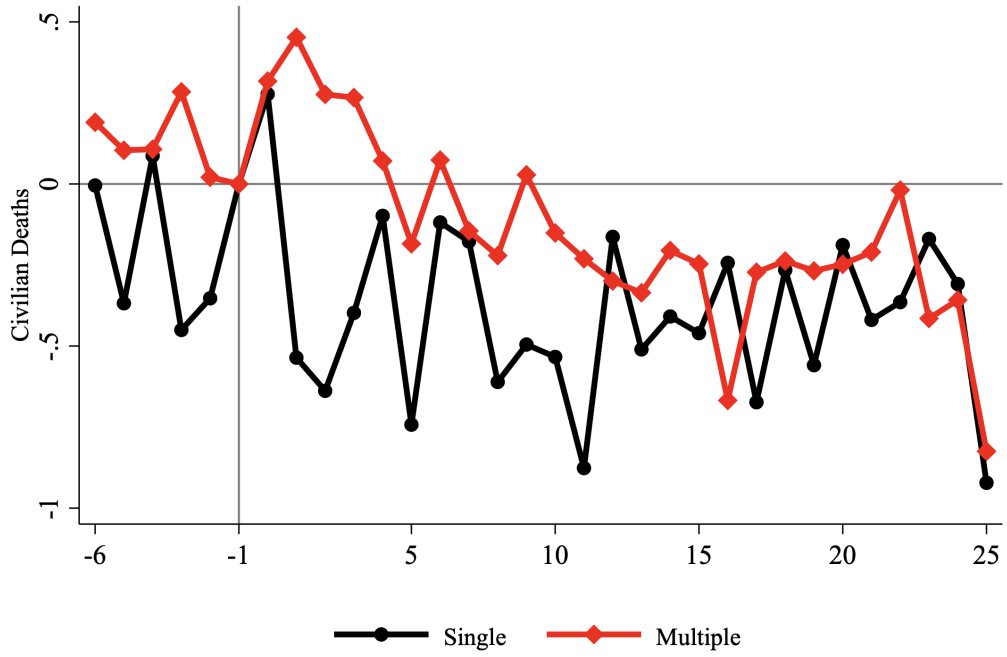


(b) Total Non-White Population Covered



Notes: Panel (a) stratifies treated counties by locations with above-below median coverage. Coverage is defined by the percentage of the county's population resides in treated cities within the county. Panel (b) stratifies treated counties by locations with above-below median non-white population coverage. In both panels, the control group are non-treated locations, irrespective of county demographics. The dependent variable is non-white deaths.

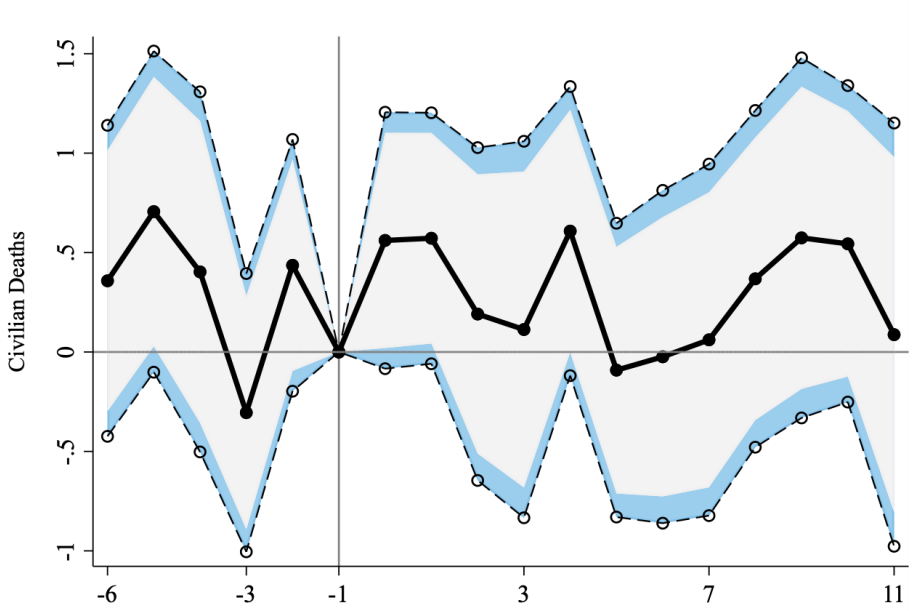
Figure B10: Event Study Results – Multiple Treatments



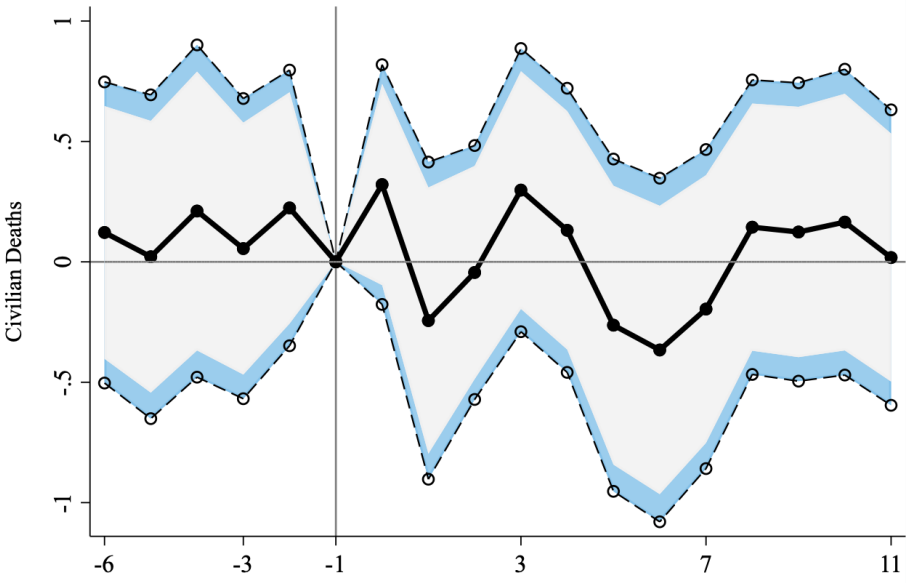
Notes: The figure shows pre- and post-treatment effects when locations with one treated city are compared to control group versus when counties with multiple treated cities are compared to the control group. In both panels, the control group are non-treated locations, irrespective of county demographics. The dependent variable is non-white deaths.

Figure B11: Event Study Results – Contiguous Counties

(a) Non-White

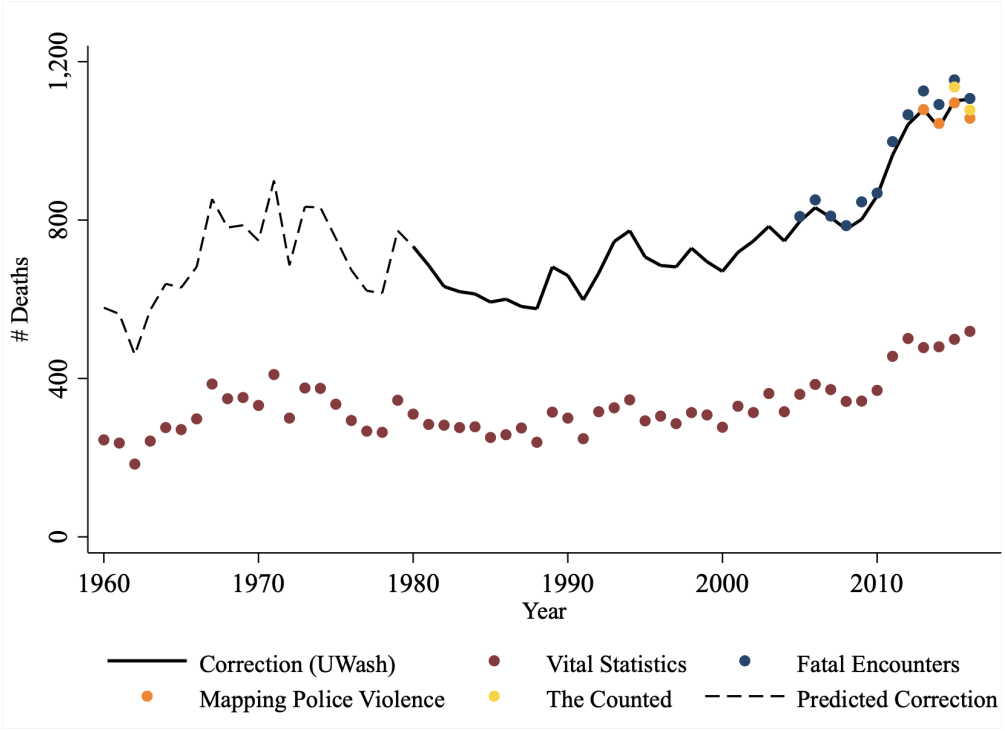


(b) White



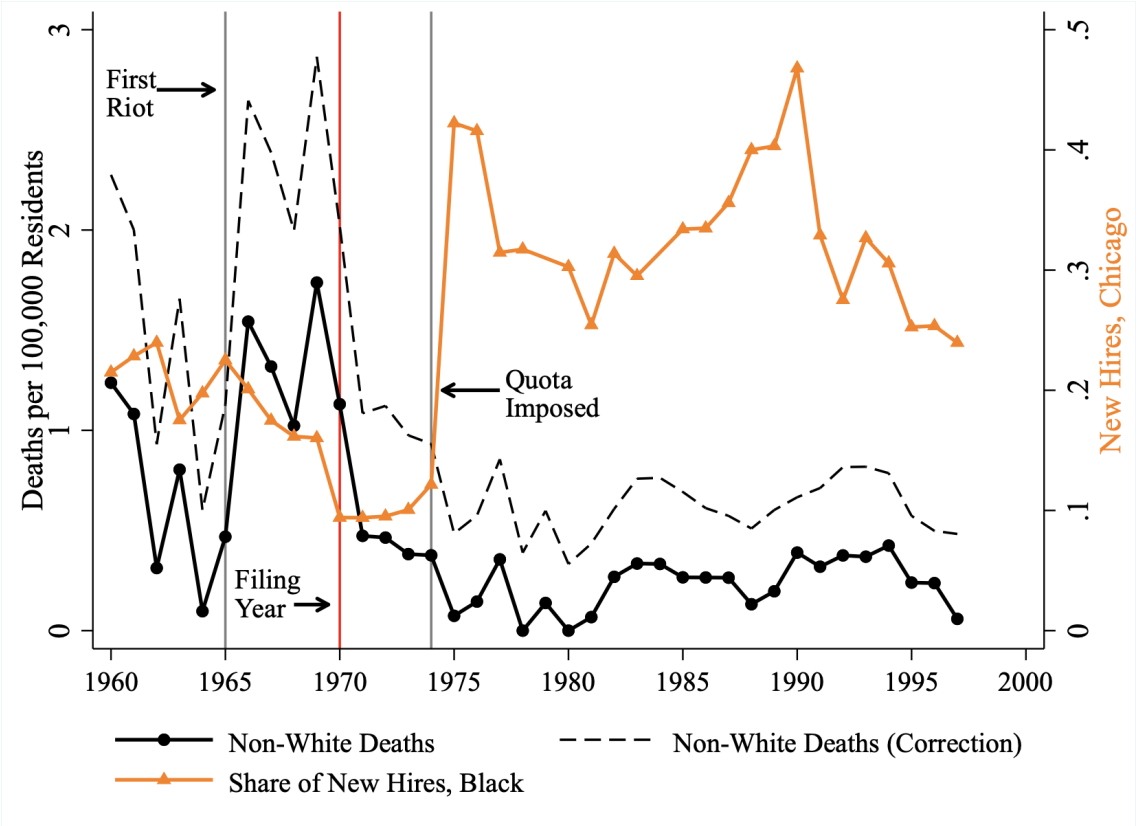
Notes: Figure plot pre- and post-treatment effects for when the treatment status and timing is applied to contiguous untreated counties. The comparison group consists of non-treated non-contiguous counties. Treated counties dropped from the sample.

Figure B12: Police Killings of Civilians, 1960-2019



Notes: The figure replicates Collaborators et al. (2021), showing police killings of civilians from 1960 to 2019. We compute deaths from 1960 to 1979 as a function of reported deaths, bias in reporting, and state-and-year fixed effects.

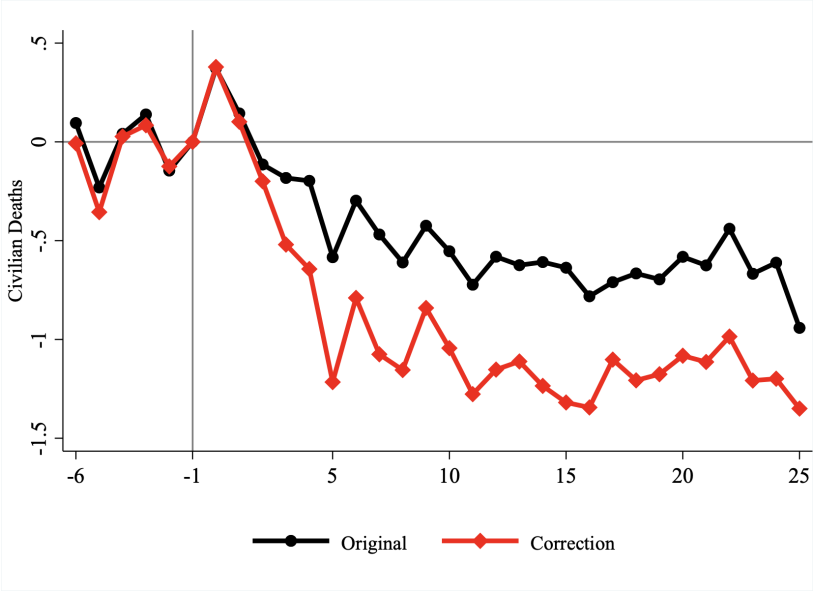
Figure B13: Cook County Police Killings Over Time, Estimated



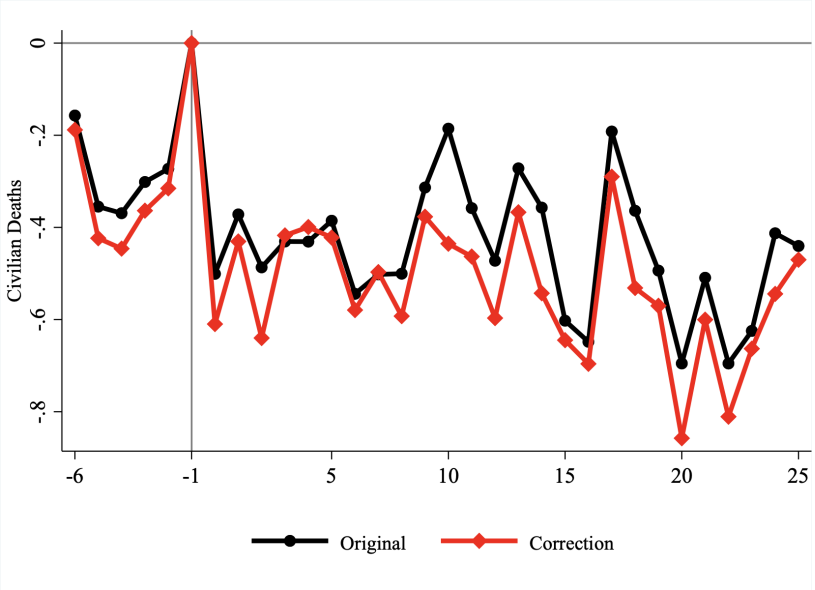
Notes: Estimated police killings displayed by dashed line. We assume that each county's contribution to the state level estimate of police killings is constant over time. The new county level measure of police killings reflects reported deaths and the bias attributed to each county contribution of the state's total.

Figure B14: Event Study – Estimated Deaths Due to Legal Intervention

(a) Non-White: OLS



(b) White: OLS



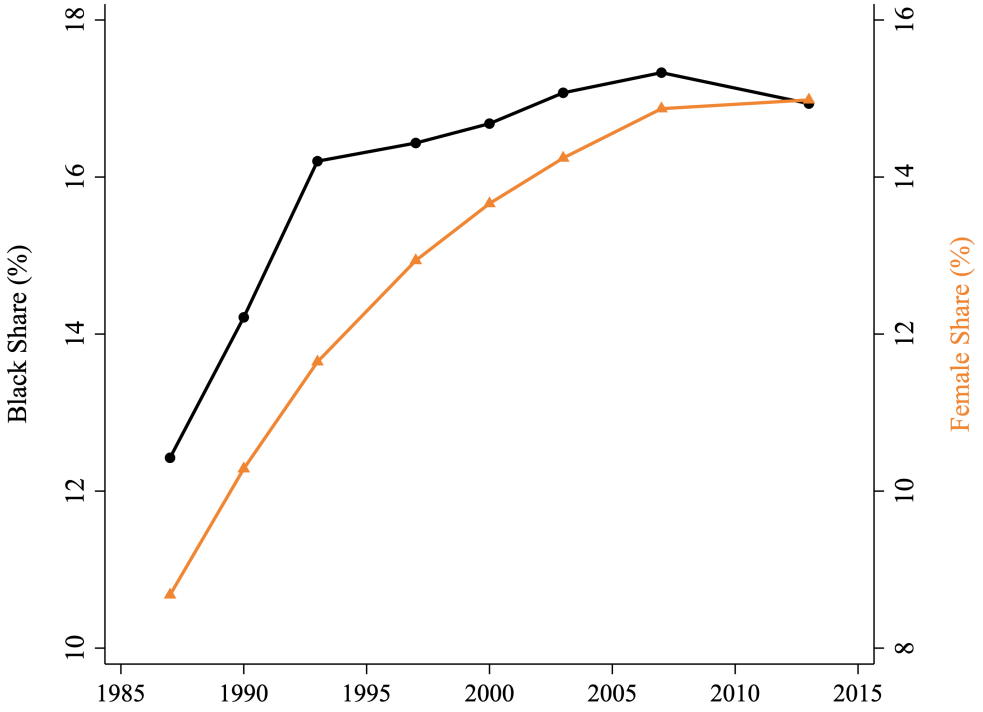
Notes: The specification includes county, C, and region-by-year R-Y, effects. The red line corresponds to the corrected estimated police killings, while the black line refers to the original estimate. The horizontal axis represents event-years (years before and after litigation).

B.2 Diversity vs. Intervention

In the Appendix, Figures B16 and B17 highlight the correlation between litigation and diversity. However, in Appendix Figure B18 we try to explore which mechanism is more important, intervention or diversity. The idea is that by year twenty five, we can observe which cities reduced their representation gap as measured by the share of black police officers relative to the percentage of the population that is black. To obtain the share of the police that is black, we use the LEMAS survey that is closest to event year 25. Then we keep all municipal law enforcement agencies in a county and take the weighted average of the black share of the police in a given county. This is done for 72 treated counties that we can identify in the LEMAS survey. For each county, we calculate representation as highlighted above. We then stratify treated locations to those that are above and below the median representation gap. Lastly, we compare counties in the above-median group to non-treated locations; we do the same for the below-median group. By splitting the sample in this manner we can compare and contrast litigated counties based on representation to examine if police-related fatalities are driven by diversity or intervention.

It is important to note that this proxy for the impact of diversity does not actually capture the true impact of diversity. For one, some places have smaller black populations, potentially making it easier to close the representation gap. Conversely, locations with large black populations may be incapable of recruiting minority officers in large numbers. Lastly, there is great heterogeneity in the share of the non-white population residing outside treated cities but in treated counties; this matters when we are using county populations of non-white civilians. For example, suburban areas in the south may have a larger non-white population relative to suburban areas outside of the south. Therefore, it could be the case that locations in the below-median group made large gains in reducing the representation gap, but that those gains are not reflected in how we split the treatment group. Unfortunately, we have neither the initial representation gap for each police department nor police-related fatalities at the city level. The importance of south vs non-south locations is highlighted in Appendix Figure B19. The decrease in non-white police killings is driven by treated locations outside of the south.

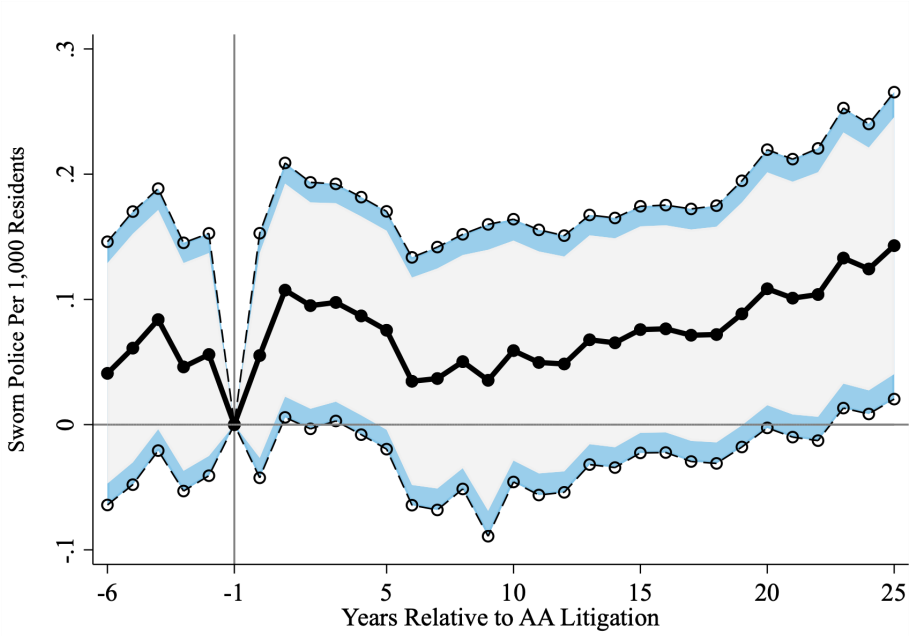
Figure B15: LEMAS Police Composition Over Time



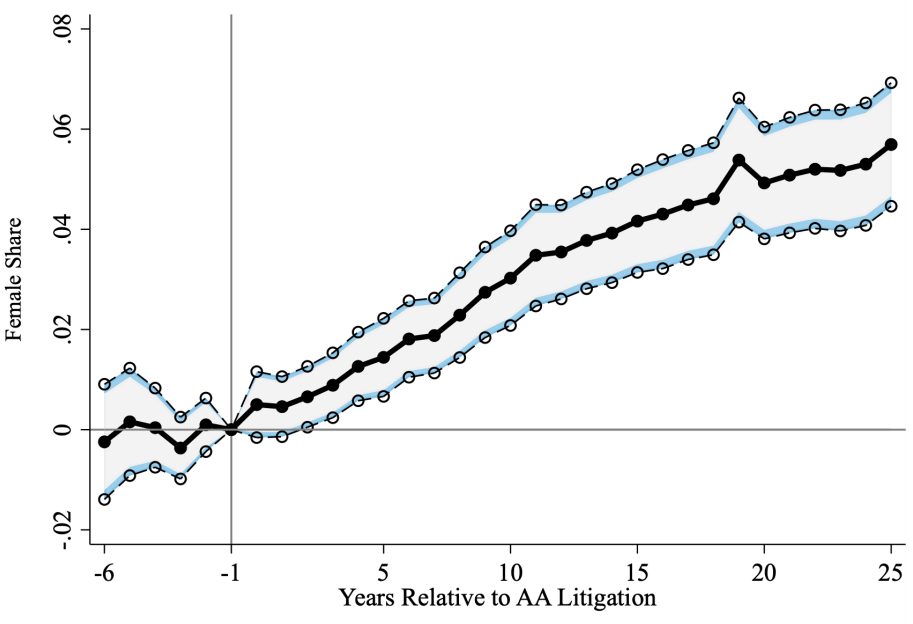
Notes: Data comes from the Law Enforcement Management Statistics (LEMAS), publicly available at the ICPSR.

Figure B16: Event Study Results – Employment

(a) Employment



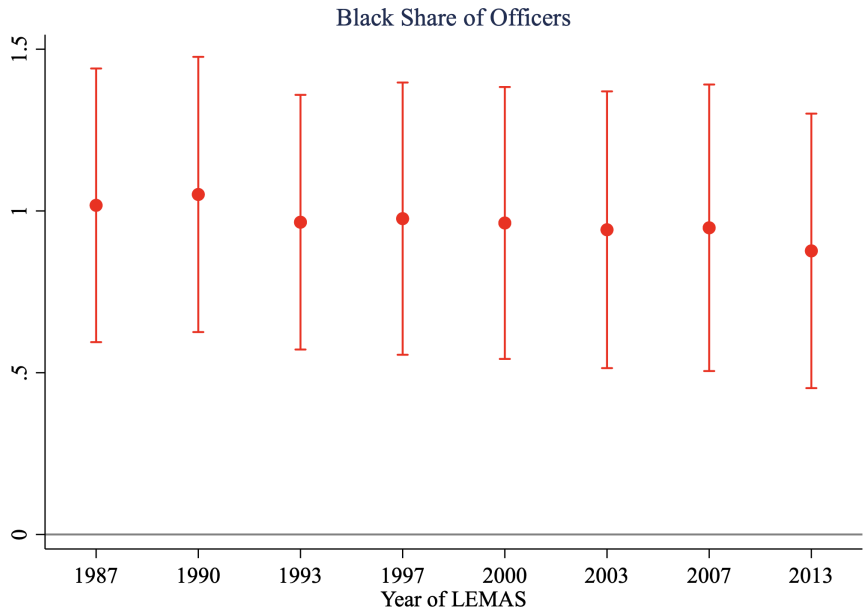
(b) Female Share



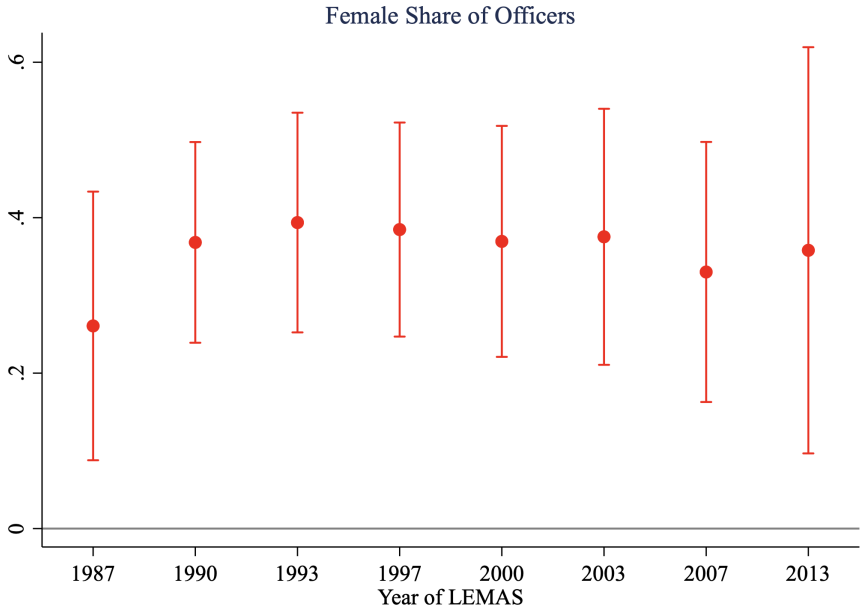
Notes: Data comes from the UCR Law Enforcement Killed or Assaulted files. Employment by gender is available after 1971–therefore the results presented are from an unbalanced panel.

Figure B17: Years Since Litigation and Police Composition

(a) Black Share



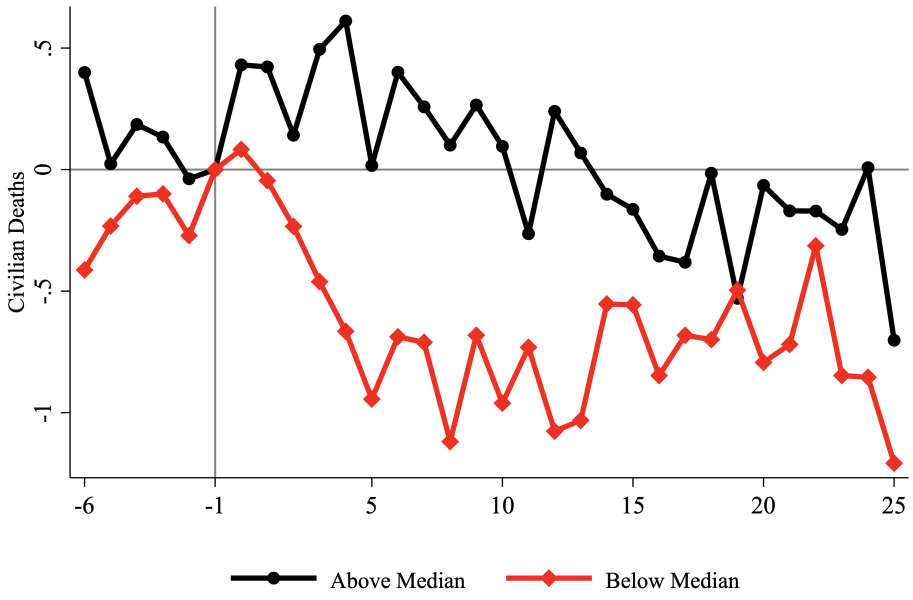
(b) Female Share



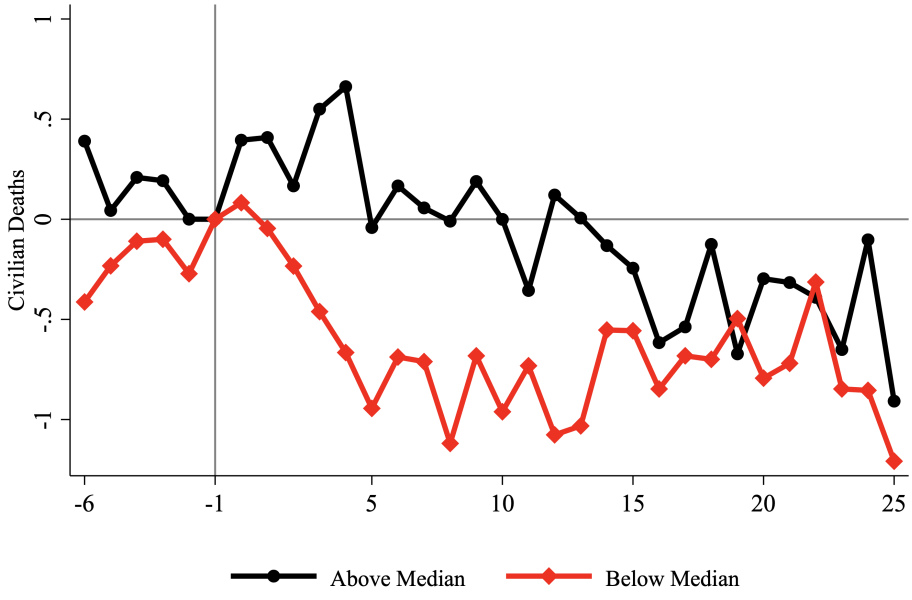
Notes: Each estimate is from a separate cross-sectional regression that includes region fixed effects. Data comes from the Law Enforcement Management Statistics (LEMAS).

Figure B18: Event-Study Results - Police Representation

(a) Main Sample



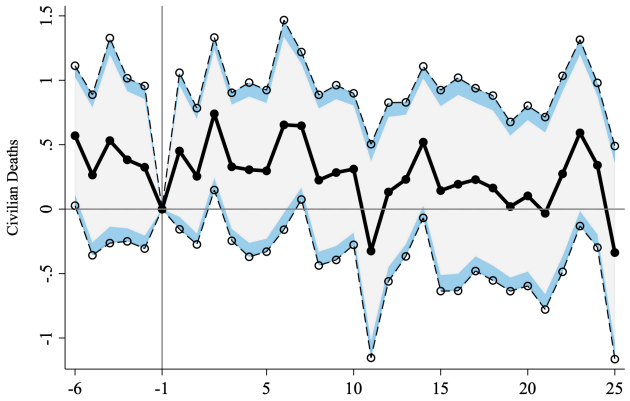
(b) Drop Washington D.C. and San Francisco



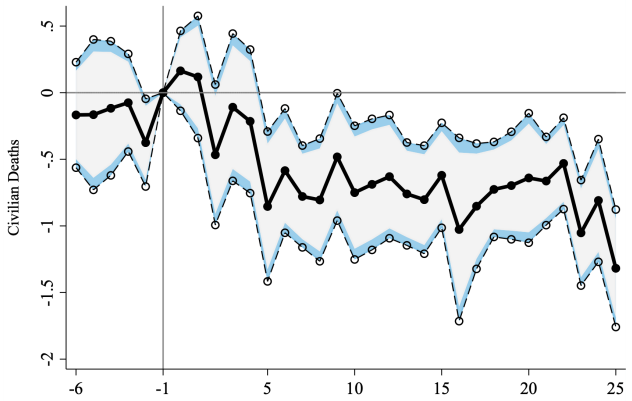
Notes: The regression specification includes county, C, effects and region-by-year, R, effects. We stratify the treated groups to those with police-litigated police departments with above-/below-median level of representation 25 years after treatment. Representation is measured as the share of police officers who are black divided by the share of the city population that is black. Once again, we present robust standard errors. The horizontal axis represents event-years (years before and after litigation).

Figure B19: Event Study Estimates – By Region

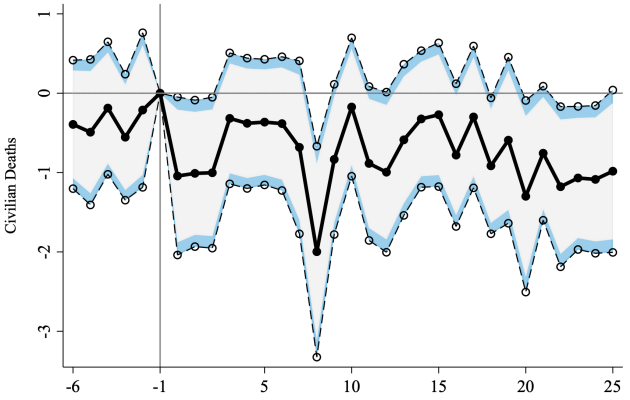
(a) Non-White - South



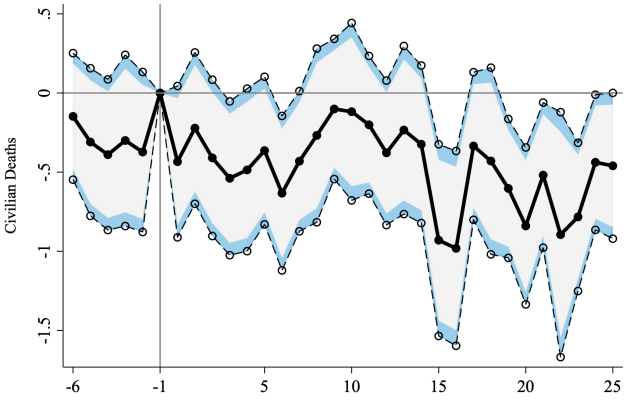
(b) Non-White - Rest of the Country



(c) White - South



(d) White - Rest of the Country



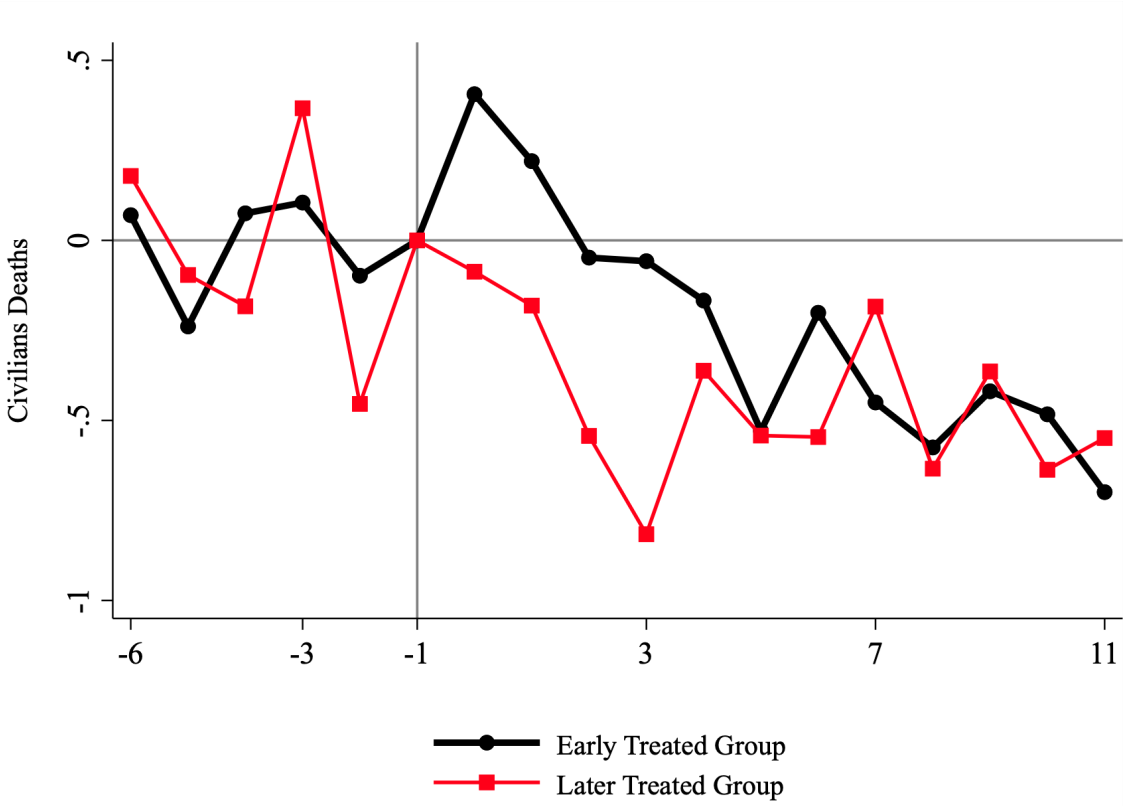
Notes: The regression specification includes county, C, effects. Panels (a) and (c) report event-study estimates for the south for non-white and white civilians, respectively. Poisson model includes year fixed effects. Panels (b) and (d) report event-study estimates for the rest of the country and include region-by-year fixed effects (northeast, midwest, and west). Robust standard errors. The horizontal axis represents event-years (years before and after litigation).

B.3 Heterogeneity in the Treatment Effects Across Timing Groups

We explore heterogeneity in the treatment effects across timing groups for two reasons. One, treatment is a direct response to the 1960 uprisings; therefore, the initial uptick in police-related fatalities may be in response to uprisings and not police actions. Also, the number of newly treated locations drastically slows down during the Reagan Administration. This is especially important because successful litigation depends on involvement from the Equal Employment Opportunity Commission. Secondly, we explore heterogeneity in the treatment effects across timing groups due to potential bias from implementing the standard two-way fixed-effects difference-in-difference model (Goodman-Bacon, 2021).

In Appendix Figure B20, we split treated locations into two groups: 1) those treated prior to 1981 and 2) those treated after 1980. Pre- and post-treatment effects are estimated using non-treated locations as the control group.

Figure B20: Event Study Heterogeneity in Timing – Non-White Deaths



In Appendix Figure B21, we diverge from the traditional two-way-fixed effects model (TWFE) and use the estimator from Callaway and Sant'Anna (2020) for estimating unbiased average treatment effects on the treated (ATET) when there is variation in the timing of treatment.[29] The traditional TWFE model will produce biased ATET when there is heterogeneity in the treatment effect across treatment groups or heterogeneity in the treatment effect over time (equivalent to a change in the slope parameter). The bias in the TWFE is a result of how the ATET is calculated. In general, the DiD estimate is a combination of estimates comparing the treated group to the never-treated

[29]The traditional TWFE model includes both year and location fixed effects and a dummy variable to capture the treatment: $y_{it} = \alpha_i + \gamma_t + \beta^{DD} D_{it} + \varepsilon_{it}$.

group, as well as a comparison between groups treated at different times. Appendix Figure B22 displays the weights applied to each group to produce the difference-in-difference estimate from a generic TWFE model with variation in the timing of treatment. Hence, treated groups serve as both treatments and controls when estimating the ATET. More specifically, the bias occurs when the later treated groups are compared to earlier treated groups. When making this comparison, the parallel trends assumption is likely violated (due to the treatment of earlier units), producing biased estimates (Goodman-Bacon, 2021). Relatedly, Sun and Abraham (2020) show that a similar bias exists in TWFE event-study analyses when there is heterogeneity in the dynamics of the ATET across timing groups. Therefore, TWFE will bias the ATET away from the true effect when there is heterogeneity in the treatment effect over time or across groups.

To avoid the bias associated with TWFE models, we first employ the estimator from Callaway and Sant'Anna (2020) (CS) in an event-study framework. We stack our data so that stack k includes one timing group t_k^* and other counties that will be never be treated. The never-treated group serve as the control group for treatment group k. This method uses calendar time to compare and contrast treated locations with the control group. For example, for those counties treated in 1977, the comparison group will include the calendar years 1971 to 2002 for never-treated counties.

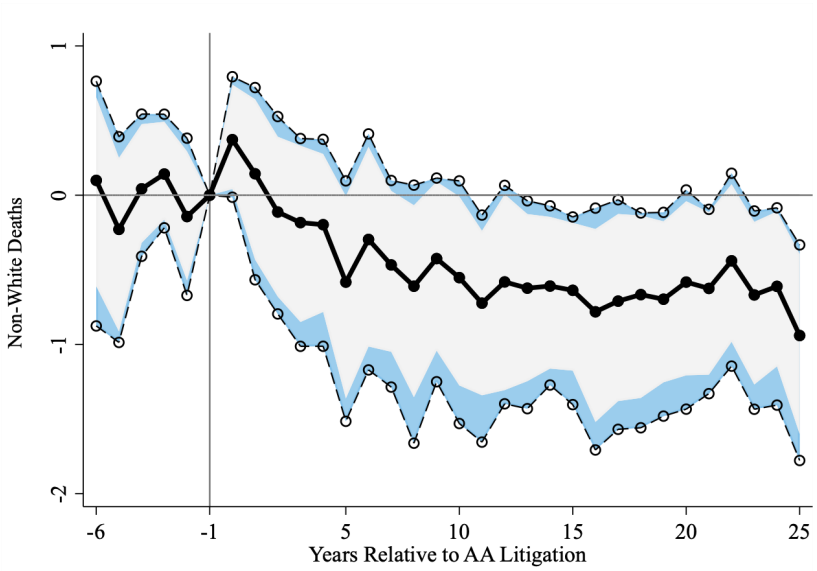
To estimate unbiased pre- and post-treatment effects, we create all possible stacks for timing groups as previously highlighted and calculate means by treatment group (g), time (t), and stack (k). Then we estimate the following equation:

$$Y_{kgt} = \alpha_{kg} + \gamma_{gt} + \sum_{y=-6}^{-2} \pi_y^{CS} D_g \mathbb{1}(t - T_g^* = y) + \sum_{y=0}^{25} \pi_y^{CS} D_g \mathbb{1}(t - T_g^* = y) + \epsilon_{kgt} \tag{3}$$

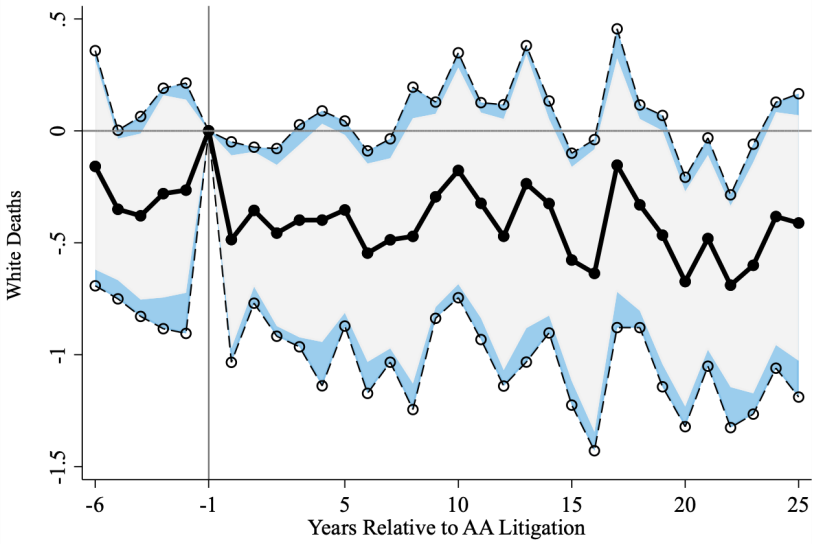
where Y is police-related fatalities by race. We include treatment group fixed effects for every stack α and time fixed effects for every stack γ. The Callaway and Sant'Anna estimator is captured by π^{CS}, which will provide pre- and post-treatment for group g. The estimator will calculate event-study estimates for each group. The reported pre- and post-treatment effects are a weighted average of each group's estimated effects using the number of treated counties in each timing group as weights. We construct 95 present confidence intervals from 250 draws of a block bootstrap that resamples the counties in our analysis. We aggregate the estimated average treatment effects by the number of resampled counties in each timing group and use this distribution for statistical inference. Due to the manner of computation, these results should be compared to the OLS results. If the TWFE model produced unbiased results, the CS estimator should produce similar estimates.

Figure B21: Event Study –Callaway and Sant'Anna Estimator

(a) Non-White



(b) White

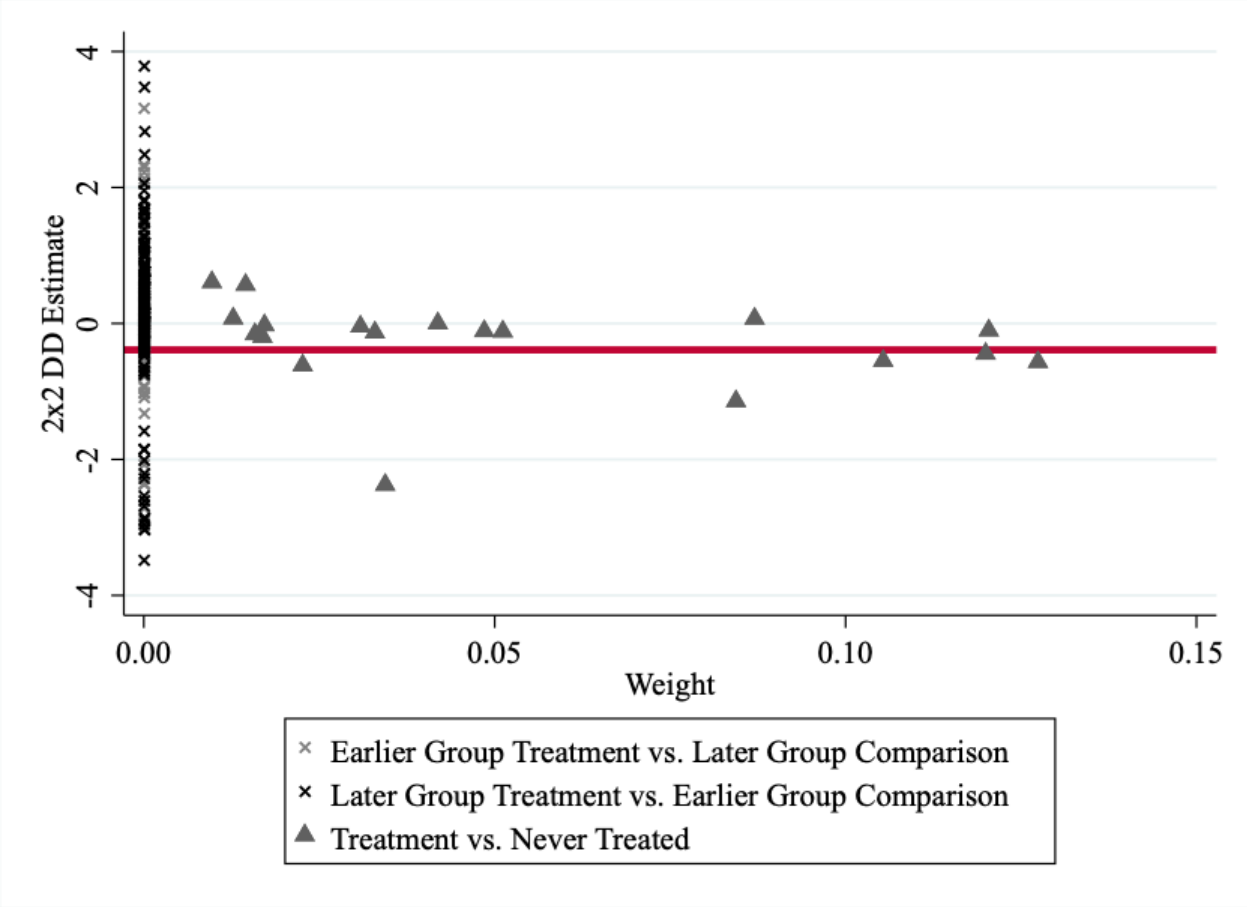


Notes: The figure plots regression estimates of the effect of the threat of court-ordered affirmative action on policing-related civilian fatalities. The dependent variable in Panel (a) is the number of non-white deaths due to legal intervention. Similarly, the dependent variable in Panel (b) is the number of white deaths due to legal intervention. Coefficients are estimated using Callaway and Sant'Anna (2020) estimators to avoid biases associated with two-way fixed effects models highlighted in Goodman-Bacon (2021). The horizontal axis represents event-years (years before and after threat of litigation).

B.4 UCR Arrests by Race Analysis

We explore changes in police contact as a possible mechanism for the reduction in police use of force. We proxy for police contact with arrest rates, measured by the number of arrests

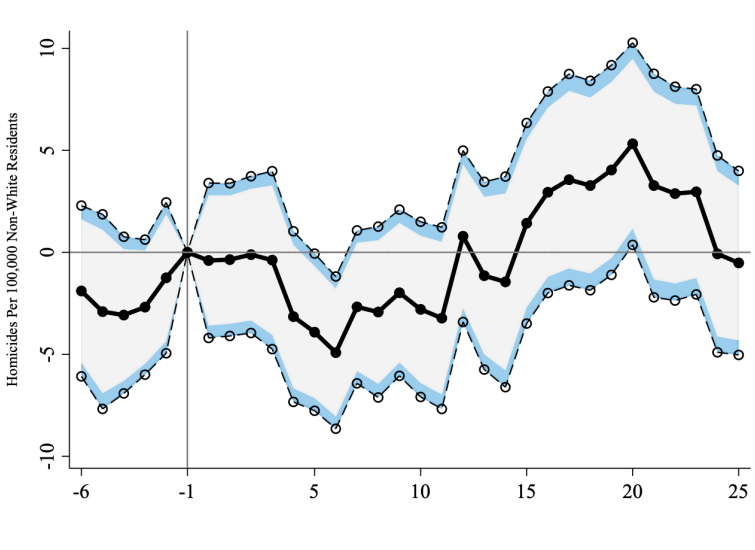
Figure B22: Difference-in-Difference Decomposition



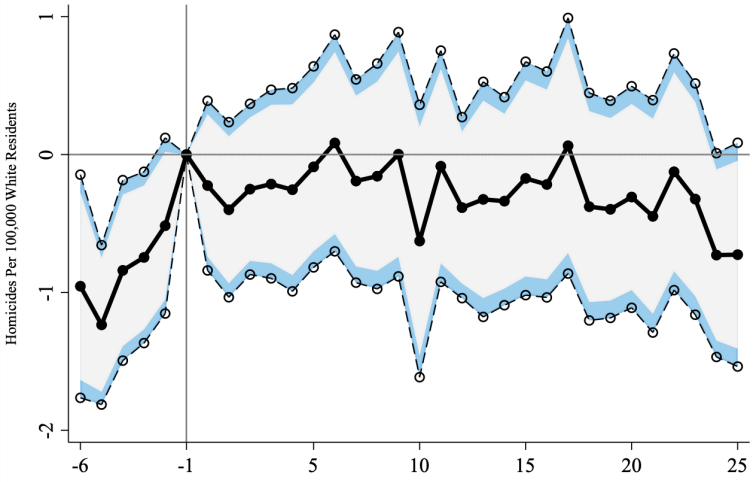
divided by the population. We obtain our arrests sample by only including agencies that report 52 times between 1960 and 2016. We then restrict the sample period to the years between 1960 and 2012. For those cities with incomplete information, most of the missing data occurs in the 1960s. Next, we take the total arrests for all the agencies in a particular county and divide by county population. For the majority of the counties in the sample, there is only one agency reporting, which typically belongs to the largest city in the county. Unfortunately, we do not have city-level population by race dating back to 1960 and therefore cannot do a city-level analysis. It is possible to interpolate population by race for intercensal years and re-estimate Figures 10 and 11. We feel that our analysis closely proxies for the ideal analysis as we find a similar pattern for arrests as McCrary (2007).

Figure B23: Event Study – Homicide Victimization Rates

(a) Non-White



(b) White



Notes: The regression specification includes county, C, effects, region-by-year, R, effects, and urban-by-year, U, effects. Panel (a) and (b) correspond to homicide victimization rates per 100,000 residents by race. Robust standard errors. The horizontal axis represents event-years (years before and after litigation).

C Appendix Tables

Table C1: Joint Effects - Counties With At Least One Non-White Death

	(1)	(2)	(3)	(4)	(5)	(6)
		Non-White			White	
	OLS	Poisson	WLS	OLS	Poisson	WLS
Pre-Period Effect (Event Years -6 to -2)	-0.0301	-0.00352	0.0222	-0.300	-0.336**	-0.0523**
	[0.222]	[0.155]	[0.0990]	[0.210]	[0.167]	[0.0249]
Shorter-Run Effect (Event Years 0 to 7)	-0.155	-0.0500	-0.0250	-0.464**	-0.525***	-0.0685***
	[0.263]	[0.149]	[0.111]	[0.219]	[0.165]	[0.0246]
Medium-Run Effect (Event Years 8 to 15)	-0.575*	-0.413***	-0.216*	-0.387	-0.432**	-0.0616***
	[0.322]	[0.149]	[0.116]	[0.265]	[0.171]	[0.0238]
Longer-Run Effect (Event Years 16 to 25)	-0.657**	-0.508***	-0.239*	-0.527*	-0.699***	-0.0849***
	[0.319]	[0.148]	[0.126]	[0.272]	[0.169]	[0.0250]
Number of Counties	844	844	784	844	844	784
Mean DV	1.478	1.478	0.414	1.188	1.188	0.155

Note: Sample includes counties that report at least one non-white death over the sample period. All regressions include county and region-by-year fixed effects. Robust standard errors are clustered by county. *** p<.01, ** p<.05, * p<0.1

Table C2: Joint Effects - Counties With At Least One Non-White Death in the 1960s

	(1)	(2)	(3)	(4)	(5)	(6)
		Non-White			White	
	OLS	Poisson	WLS	OLS	Poisson	WLS
Pre-Period Effect (Event Years -6 to -2)	-0.0714	-0.116	-0.0516	-0.325	-0.399**	-0.0615**
	[0.282]	[0.153]	[0.105]	[0.255]	[0.165]	[0.0274]
Shorter-Run Effect (Event Years 0 to 7)	-0.214	-0.0646	-0.0371	-0.579**	-0.627***	-0.0819***
	[0.320]	[0.144]	[0.106]	[0.269]	[0.161]	[0.0265]
Medium-Run Effect (Event Years 8 to 15)	-0.707*	-0.409***	-0.224*	-0.517	-0.544***	-0.0742***
	[0.392]	[0.144]	[0.116]	[0.319]	[0.171]	[0.0251]
Longer-Run Effect (Event Years 16 to 25)	-0.798**	-0.472***	-0.239*	-0.685**	-0.790***	-0.0950***
	[0.386]	[0.145]	[0.134]	[0.330]	[0.170]	[0.0273]
Number of Counties	369	369	350	369	369	350
Mean DV	1.800	1.800	0.462	1.327	1.327	0.169

Note: Sample includes counties that report at least one non-white death prior to 1969. All regressions include county and region-by-year fixed effects. Robust standard errors are clustered by county. *** p<.01, ** p<.05, * p<0.1

Table C3: Composition of Police Department, 1987-2013

	(1) Black Share	(2) Female Share
Treated in 1970s	0.131***	0.0511***
	[0.0141]	[0.00477]
Treated in 1980s	0.118***	0.0383**
	[0.0454]	[0.0149]
Treated in 1990s	0.0393	0.0112***
	[0.0245]	[0.00331]
Observations	9,267	9,267
R-squared	0.179	0.036
Mean DV	0.0550	0.0790

Note: Source: Law Enforcement and Administrative Statistics (LEMAS), 1987,1990, 1993, 1997, 2000, 2003, 2007, and 2013. Regression of share of sworn police officers by demographic group relative to the timing of treatment. The reference group consists of counties that were never treated. All columns include region and year fixed effects. Mean dependent variables report the average share of officers for the control group. Robust standard errors are clustered at the county level and presented in brackets.

Table C4: Composition of Police Department, Years Since Litigation

	(1) Black Share	(2) Female Share
Years Since Litigation	0.00437**	0.00159**
	[0.00172]	[0.000675]
Observations	564	564
R-squared	0.276	0.194
Mean DV	0.0810	0.0820

Note: Source: Law Enforcement and Administrative Statistics (LEMAS), 1987,1990, 1993, 1997, 2000, 2003, 2007, and 2013. Regression of share of sworn police officers by demographic group relative to timing of treatment. All columns include region and year fixed effects. Mean dependent variables report average share of officers for the initial year of litigation. Robust standard errors are clustered at the county level and presented in brackets.

Table C5: Service Calls by Treatment Status in 1987

Dependent Variable	(1)	(2)	(3)	(4)	(5)	(6)
	Service Calls Per Capita			Operational 911 System		
Treatment	0.0509	0.00866	0.128	-0.0615	-0.107	0.0627
	[0.105]	[0.109]	[0.192]	[0.0782]	[0.0732]	[0.196]
Add Covariates		X			X	
Treated Only			X			X
Observations	211	211	68	211	211	68
R-squared	0.147	0.242	0.233	0.113	0.189	0.256

Note: Source: Law Enforcement and Administrative Statistics (LEMAS), 1987. Regression of police services to 1987 treatment status. All columns include the percentage of the population black (1980) and region fixed effects. The dependent variable in columns (1) - (3) is service calls per capita and (0/1) operational 911 system in columns (4) - (6). Columns (2) and (5) adds the following covariates from the 1980 census: population per square mile, median age, median income, percentage of the population age 5 and under, percentage of the population age 64 and older, percent of the population with 12 or more years of education, and the percent of households headed by a female. Columns (3) and (6) restrict the sample to cities litigated between 1969 and 2000. Robust standard errors presented in brackets.

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