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

Robynn Cox† Jamein Cunningham‡ Alberto Ortega§

April 2, 2021

PRELIMINARY DRAFT—PLEASE DO NOTE CITE

Abstract

Although research has shown that court-ordered hiring quotas increased the number of African Americans hired in litigated cities, there has been little insight into how workforce diversity, or the lack thereof, may impact police performance. Recent events have highlighted the importance of diversity as it relates to community-police relations and the police shooting of civilians. Our analysis investigates how the threat of affirmative action (AA) litigation affects police killings of civilians. We find evidence that the threat of AA litigation reduces police killings of non-white civilians in the long run. We also find suggestive evidence that police killings of whites also decrease in response to the threat of AA litigation. Moreover, the threat of AA litigation is associated with an immediate transitory increase in racial disparities of police killings followed by a decrease over time. Finally, we consider the changing composition of police departments as a potential mechanism and provide suggestive evidence that affirmative action litigation increased the racial and gender representation of police departments in the long-run.

Keywords: affirmative action, excessive use of force, police shootings, race
JEL *Classification:*: I28, J15, J78, K42

*We are grateful for funding from the PSI Center for Workplace Diversity and Inclusion at the University of Memphis. We would also like to thank participants in the NEA Sessions at the ASSA Annual Meetings, participants in the Duke Empirical Criminal Law Roundtable, participants in the APPAM Fall Research Conference, participants in the SEA Fall Meetings, participants in the Health Policy Workshop at Indiana University, and our colleagues Andrew Goodman-Bacon, Nic Duquette, Jose Joaquin Lopez, Anthony Yezer, and Patrick Mason for comments and advice. Finally, we thank Justin McCrary for making the litigation data available. Any errors or omissions are ours alone.

†USC Suzanne Dworak-Peck School of Social Work, University of Southern California.

‡Department of Economics, University of Memphis.

§O'Neill School of Public and Environmental Affairs, Indiana University.

1 Introduction

Police violence is a leading cause of death for young men, following accidents, suicide, other homicides, heart disease, and cancer. 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. Black men have the highest risk of death due to police violence: 1 out of every 1,000 Black men are estimated to be killed by police over the life course (Edwards et al., 2018). After more than fifty years of protests in response to police killings in Black communities, the conversation has shifted from police reform to abolition, largely due to the killing of George Floyd. While most 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 towards public safety (Bell, 2016). For most cities, spending on public safety as a share of total expenditures has remained constant at about 4 percent since the 1970s (Auxier, 2020).

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; President's Task Force on 21st Century Policing, 2015). As early as 1968, the Kerner Commission[1] recognized the contentious relationship between the Black community and mostly white police departments as a primary cause of the civil unrest in the 1960s, so 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 insure that the needs of minorities are actively considered in the establishment of police policy and the delivery of police service. Affirmative action should be taken to achieve a

[1]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.

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 across the country experienced one of the most aggressive affirmative action programs 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 when there is more diversity within a police force, Blacks are less likely to be arrested (Donohue III and Levitt, 2001) and Blacks and Hispanics are subject to fewer stops (Close and Mason, 2007). 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. Women 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 both Black and white residents (**?**), 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 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.[2] Following McCrary (2007), we analyze the threat of court-ordered affirmative action plans between 1969 and 1999. Although Title VII of the Civil

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

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).[3]

Our analysis takes advantage of the 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 come from the Vital Statistics, which provide information on the cause of death as well as the decedent's county of residence. The event-study design allows us to test the common trends assumption as well to 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 increased the employment of Black police officers (McCrary, 2007; Miller and Segal, 2012). This is similar to Miller and Segal (2012) that find that litigated departments still increased their Black employment shares, but at a lower rate than those that led to court-ordered affirmative action.

The potential effect of affirmative action litigation on police killings of non-white civilians is ambiguous. On one hand, the threat of affirmative action could change the morale and behavior of currently employed police. 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 animus towards minority groups, due to the prospect of a changing work environment. Consequently, the possibility of losing an affirmative action lawsuit and/or expected changes in the composition of workforce may anger a very white male-dominated profession (Hidalgo, 2019), leading to more killings of non-white civilians with no change in killings of white civilians. Moreover, Devi and Fryer Jr (2020) showed that police officers become less engaged when police departments

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

are involved in a federal investigation. Similarly, Ba and Rivera (2019) demonstrated that public or known investigations of police departments alter community–police relations. Nonetheless, 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 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, which could lead to a decrease in negative interactions between the police and the community and to fewer non-white deaths. Moreover, research suggests that racial stereotypes are used to determine the presence of threats and the decision 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 associated with danger than white officers, to the extent that interacting with racially diverse coworkers helps to decrease bias among white police officers, we would expect police killings of non-whites to by white officers to decrease. Moreover, if racial and ethnic minorities are more likely to view racial and ethnic neighborhoods within a less threatening context, this could also lead to a reduction of both black and white residents in these neighborhoods (Correll et al., 2011). Relatedly, if minority police officers are better at identifying and deescalating violent situations among non-white criminals, then 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 resulted in fewer deaths of non-white civilians from legal intervention in the long run. Our results suggest that by 2000 litigated counties averted roughly 60 non-white deaths at the hands of law enforcement. We consider the changing composition of police departments as a potential mechanism and provide suggestive evidence that affirmative action litigation increased the racial and gender representation of police departments in the long-run, consistent with previous research (McCrary, 2007). We also find some 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 initially leads to a relative increase in non-white killings in the initial year of litigation.

We check the validity of our results using OLS, WLS, and Poisson estimators. We also estimate a series of robustness checks that involve restricting the sample to only litigated counties, large counties, highly urbanized counties, and counties that experienced uprisings. The results are robust to these reasonable sample restrictions. 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.

Our research contributes to the growing literature on race and the use of force in general, and on police killings of civilians in particular. 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 literature 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 lower 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 **?**, who also examined the impact of affirmative action on the welfare of minority citizens but used a different measure of citizens' welfare, self-reported victimization. Our paper is also linked to research investigating the role of federal interventions in racial disparities in local policing outcomes (Cox and Cunningham, 2020; Weisburst, 2019).

As previously mentioned, entrenched hostilities between Black communities and predominantly white police departments were determined to be an influential factor in the 1960s civil unrest. 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). One of the police reform recommendations in response to the 1960s uprisings was to diversify local law enforcement, a common suggestion even today. Our study extends the work of Cunningham and Gillezeau (2019) by investigating how federal and legal interventions impact racial disparities in policing outcomes in general, and racial disparities in police killings of civilians in particular.

The remainder of this paper is organized as follows. Section 2 describes the data and methodology. Sections 3 and 4 present our primary findings, along with a series of robustness checks. Section 5 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.[4] 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 undercounts 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). More recent data collected by *The Guardian* and *The Washington Post* began long after the 1960s and is therefore not useful for this analysis.

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.[5] This approach

[4]We exclude deaths due to legal execution from our calculation of deaths due to legal intervention.

[5]This is true for crime data in general. Myers (1980) showed that between 1970 and 1974, only 1/3 to 2/3 of all crimes

assumes that the recording of deaths due to policing over time is exogenous to the treatment. This is a strong assumption; it is possible that policy parameters are associated with the treatment, changing how 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.[6] The aftermath of this event resulted in an increase in non-white deaths (consistent with Cunningham and Gillezeau (2019)).[7] 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

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) found 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.

[6] 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.

[7] 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.

data and Vital Statistics data with county demographics data provided by the Surveillance, Epidemiology, and End Results and the Count 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 2,712 counties, of which 73 counties are treated. Treatment status is 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 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, 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 regions. Between 1969 and 2000, 73 counties are treated (i.e. litigated), which is about 2.4 percent of the counties in our sample but covers 31 percent of the population.[8] Specifically, over 45 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.[9]

The relationship between federal involvement in local policing and police killing of civilians is also evident during the 1970s. In Figure A.2, 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,

[8]See Appendix Table 1 for a distribution of litigation over time.
[9]See Figure 3.

we can actually see stark changes in police killings by treatment status in Figure A.2. 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 in the treated group. After 1968, there was an immediate convergence in police killings between the treatment and control group. By 1977, non-white deaths in treated locations were only slightly larger than the control group and 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 them. Panel (a) of Figure 4 normalizes per capita non-white deaths due to legal intervention to zero in 1968. This allows for a more direct comparison between the treated and control groups over time. 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 group 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 large drop in police killings of non-white civilians in the treatment group, compared to the control group. The drop in police killings becomes starker in panel (b) of Figure 4, which plots the difference in non-white deaths between the two groups. This is consistent with Devi and Fryer Jr (2020) and Ba and Rivera (2019), who found that public or federal intervention influences police behavior. Although they suggested that public or known federal interventions change police performance and decrease productivity, we present suggestive evidence that federal intervention may have decreased 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, have higher income, and have a larger share of the population that is non-white. Also, the treated counties are denser; and more likely to have experienced a riot during the 1960s. In fact, roughly 84 percent (61 of the 73) of treated counties experienced at least one violent protest between 1964 and 1971, compared to only about 7 percent (199 out of 2989) counties in the control group.[10] 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: all of the treated counties have at least 47 percent of their population residing 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. If this holds, county fixed effects will account for key cross-sectional differences that are time-invariant. If so, the non-litigated counties in our analysis will capture trends in police killings over time and 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 treated locations would see an immediate decrease in police killings of civilians due to federal involvement. However, the majority of 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, at least prior to 1968, that treated and non-treated locations have different pre-trends. Therefore, it is possible that the control group is not the ideal comparison group. 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

[10] Both McCrary (2007) and Miller and Segal (2012) highlighted 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 as it relates to timing of treatment and pre-period growth rates. Places treated relatively early experienced large increases in non-white deaths, while a significant number of places experienced a decline in police killings prior to 1969.

We test for differences in pre-period trends in non-white deaths between the treatment group and control group 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 show 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 were evolving 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:

$$y_{ct} = \alpha_c + \gamma_{r(c),t} + \sum_{j=2}^{7} \pi_j D_c \mathbb{1}\{t - t_c^* = -j\} + \sum_{j=0}^{12} + \phi_j D_c \mathbb{1}\{t - t_c^* = j\} + v_{ct} \tag{1}$$

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 collinerity 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 vary 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 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 coefficients, ϕ_j, allows us to examine any dynamics resulting from treatment, post-litigation. We group event times for 7 years before (i.e., $\pi_{-7} D_c \mathbb{1}\{t - t_c^* \leq 7\}$) the policy and twelve years after (i.e., $\phi_{12} D_c \mathbb{1}\{t - t_c^* \geq 12\}$) the policy and focus our analysis on 6 years prior litigation to 11 years afterwards.

We consider a variety of specifications to estimate equation 1. Our dependent variable, the number of deaths due to policing, 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.[11] 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, then 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.[12]

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

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

where τ accounts for the pre-period event-years $-6 \leq j \leq -2$ and ω accounts for the shorter-run

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

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

($0 \leq j \leq 5$) and longer-run ($6 \leq j \leq 11$) event-years. 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 further analyze pre-trends while also examining the subsequent dynamic effects post-litigation. We plot the pre-treatment and post-treatment effects from equation (1) with a solid line and circle markers. The 95-percent confidence intervals are shown with dashed lines and circle markers and the 90-percent confidence intervals are identified by the gray shaded area. Confidence intervals are constructed from robust standard errors, clustered at the county-level to address over-dispersion. 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

Figures 6a and 6c graph the pre-treatment and post-treatment effects of litigation on the number of non-white civilian deaths due to legal intervention. As indicated in Figure 5, we see no pre-trend difference in the 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 zero (see Figure A.4a). 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 became negative by event year 2 for all specifications. From Figure 6c we see that the post-treatment effects are negative and statistically significant starting in event year 11 for the OLS model (our preferred specification), and continue to decrease and grow in magnitude. On average, 1.456 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 51 percent, meaning 50 fewer deaths in 68 treated counties. [13] Our results provide evidence that eventually, after litigation, police killings of non-white civilians decrease in the long run. Figure 7 plots the number of non-white deaths prevented in litigated counties over time. By 2000, these counties had prevented roughly 60 non-white deaths due to legal intervention. 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.[14]

Figure 6 and Figure A.4b also plot the event-study estimates for white deaths at the hands of police. In general, results are similar across the OLS, WLS, and Poisson specifications; therefore, we focus on our preferred specification, OLS, for the interpretation of the results. Estimates from the OLS specification plotted in Figure 6b provide evidence that the common trends assumption may not hold for white deaths. The point estimates for event years -3 and -4 are 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 were indiscriminately increasing force in response to the threat of litigation. This also runs counter to Cunningham and Gillezeau (2019), which showed 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) of no racial differences in police use of force. Post-treatment effects for white civilians are negative and are statistically significant, or marginally statistically significant, in certain years. Using the point estimate for event year 0, white deaths decreased by approximately 42 percent (33 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 afterwards reverted to pre-ligation trends.

[13]The coefficient in event year 11, which is the difference, on average, between the number of deaths in event year -1 and event year 11, is .743. There are 68 treated counties, resulting in an estimated 54 fewer non-white deaths.

[14]Specifically, we do not observe if and when a municipality implements an affirmative action program. Similarly, it is possible that the threat of legal intervention caused other departments in the county to change their behavior, while the actual treated department does not.

3.2 Alternate Specifications

We summarize the joint pre-treatment 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 1 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.[15] 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 55 percent decrease in non-white deaths in the Poisson model. The WLS model suggests a 79 percent decrease in non-white deaths, while the OLS model indicates a 49 percent decrease.

For white deaths, joint pre-treatment effects are marginally statistically significant in the Poisson and WLS models, but statistically insignificant in the OLS model. White deaths are lower in the short run across all three models. If we use the OLS model we can assume causality, and litigation is associated with a 37 percent decrease in police killings of white civilians in the short run. The OLS model provides marginal evidence of a long-run decrease of white deaths by 41 percent.

We also check the validity of our results by comparing our main specification with alternative fixed-effects models in Appendix Figure A.5. Equation (1) and equation (2) both use region-by-year fixed effects to capture unobserved heterogeneity that varies by region and over time. We do this to limit the number of fixed effects in the analysis, but also to capture differences that vary across location. We run several additional analyses where we replace region-by-year fixed effects with only year fixed effects, only urban-by-year fixed effects, and a model where we add baseline covariates interacted with time. For non-white deaths, all specifications display similar patterns:

[15] Appendix Figure A.4a plots the pre-treatment 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.

an increase in event year 0 followed by a decrease over time. The region-by-year estimate provides the smallest increase in event year 0 and the largest decline in non-white deaths. The urban-by-year fixed effects model reduces the post-treatment effects in the long run. This is not surprising, as only highly urbanized counties are treated. For white deaths, the estimates are also qualitatively similar across specifications. The pre-treatment effects are negative and violate the common trends assumption. The post-treatment effects decrease and then rise, but the magnitude of effects varies with the type of fixed effects included in the analysis.

3.3 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).[16] 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 would be attributed to the threat of litigation. Negative post-treatment effects would suggest that litigation is reducing racial disparities in police killings, while positive post-treatment effects would imply that litigation is contributing to racial disparities in police killings. If the post-treatment effects are zero, then litigation may not be affecting racial disparities of police killings.

Figure 8 plots pre-treatment 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 Figure 6a and the initial decrease in white deaths in Figure 6b. 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.

[16] We follow Cox and Cunningham (2020) and estimate the following equation:

$$y_{ct} = \alpha_c + \gamma_{r(c),t} + NW_k + NW_k\gamma_t + NW_k D_c + \sum_{j=2}^{7}\pi_j D_c \mathbb{1}\{t - t_c^* = -j\} + \sum_{j=0}^{12}\phi_j D_c \mathbb{1}\{t - t_c^* = j\} + \sum_{j=2}^{7}\lambda_j D_c \mathbb{1}\{t - t_c^* = -j\} NW_k + \sum_{j=0}^{12}\sigma_j D_c \mathbb{1}\{t - t_c^* = j\} NW_k + v_{ct}.$$

Where the notation stays as earlier defined in equation 1 and NW_k is equal to one when referring to non-white police killings and zero for white police killings.

3.4 Employment Effects

Analyzing employment effects are important because, as previously mentioned, racial and gender composition is a potential mechanism through which non-white police killings are decreasing over time in counties with at least one litigated police department. As previously mentioned, prior research suggest Black and women 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 increased their Black employment shares, but at a lower rate than those that led to court-ordered affirmative action. Litigation also led to increases in the hiring of female officers (Miller and Segal, 2014). 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 collected through the Bureau of Justices Statistics (BJS) and the Uniform Crime Reporting (UCR) program, respectively, to examine the compositional effects of police departments over our analysis period. While LEOKA data are available after 1971, it 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, however, data collection did not begin until 1987. Using these two data sources will allow us to get a clearer picture of changes in the composition of police force over our analysis period, and, therefore, a better understanding of the role racial and gender composition is playing in our results.

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 Table 4 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 women officers relative to never treated counties. There is no statistically significant difference in the shares of black and women 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 Figures A.7. The top figure, A.7a, plots the dynamic effects of the threat of litigation on the number of total sworn officers (per 1,000 residents) and panel A.7b 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 A.7b also shows that the share of women 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 Harvey and Mattia (2019) where we regress the county share of Black and women sworn officers, respectively, on the years since a county experiences litigation. Our results are presented in Table B.1 and are similar to those of Harvey and Mattia (2019), where the share of Black officers increase as the number of years since litigation increases. We find similar results for women officers. We then split up the LEMAS data and run this model for each year of the LEMAS that is available. Figure A.8 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.

4 Robustness Checks

Our results indicate that in the year of litigation, racial disparities in police killings increase. Several years after the threat of litigation, non-white deaths decreased. This result holds across various specification checks as well as various models of fixed effects. However, it is clear from Table 2 that the treatment group is drastically different 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 9 and 10 present joint effects for a series of robustness checks of our main specification for non-white and white deaths, respectively. The estimated joint effects are denoted by a circle marker, while 95-percent confidence intervals indicated by a bold horizontal line. 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 1 and 3 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 the proper control group. Treated counties are starkly different from counties in the control group. The current empirical strategy takes advantage of the variation in the timing and location of affirmative action litigation. In row 2, we restrict the sample solely to counties that are treated. Restricting the sample in this manner only exploits the variation in timing in estimating treatment effects. For non-white deaths, pre-treatment effects are statistically indistinguishable from zero, the short-run effects are close to zero, and the medium and long-run effects have the same sign as those from the main specification but are not as precise. For white deaths, the pre-treatment effects are not statistically significant, and the magnitude on the post-treatment effects are similar to those form the original specification.

4.2 Large, Black, and Urban Counties

The smallest non-treated county in our sample had a population of 57,000 residents in 1960, while only 4 treated counties had a population of less than 100,000 residents in that same year. In row 3 of Figures 9 and 10, we restrict the sample to counties that had a population greater than 100,000 residents. This results in a sample that contains 69 treated counties and 212 control counties. Restricting the sample to only large counties produces pre-treatment effects that are near zero for both white and non-white deaths. In this specification, post-treatment effects are similar in magnitude and significance to our original specification for non-whites. We also see evidence of short and long-run decreases in white deaths.

In Row 4, we restrict the sample to counties where the share of the black population is below the median in 1960. From Figure 9 we see that the original benefits from the threat of litigation for non-whites and whites are no longer present. There is no significant change in non-white deaths after the threat of litigation. The same is true for row 4 in Figure 10 for white deaths. Alternatively, row 5 of Figures 9 and 10, display the results for restricting the sample to counties that have a black population share above the median in 1960. The decrease in non-white deaths attributed to the threat of litigation is now about twice as large as our original estimates for counties with a larger share of Black residents. We see a similar trend for white deaths, although pre-treatment effects are significant. These results indicate that our main findings are in part being driven by counties where the share of the black population is above the median.

In Row 6, we limit the sample to counties where the proportion of their residents residing in urban areas was higher than the median share of urbanization in 1960. All of the treated counties had urban populations above the median. Restricting the sample in this manner produces a sample with 73 treated counties and 853 non-treated counties. The additional counties produce medium and long-run effects that are slightly larger than when we restrict the sample to only large counties. The aforementioned treated only analysis also led to smaller post-treatment effects, as did the analysis in Rows 3 and 4. Together, these findings highlight the importance of having non-litigated counties in the comparison group: post-treatment effects are larger when including all nonlitigated counties. Specifically, including small counties help 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) found that heterogeneity in racial composition is associated with a more punitive criminal justice system; relatedly, Cunningham et al. (2020) also showed that heterogeneity in racial composition is associated with more police killings of non-white civilians. Appendix Figure A.9 shows that the long-run decrease in non-white deaths due to the threat of legal intervention is driven by treated locations outside of the south. This highlights the importance of using region-by-year fixed effects to capture cultural differences and heterogeneity in racial composition that contribute to criminal justice outcomes.[17]

[17]Pre-treatment effects violate the parallel trends assumption, so the results in Appendix FigureA.9 should be inter-

4.3 Uprisings and Heterogeneity in Timing

Litigation is highly correlated with uprisings. McCrary (2007) found that uprisings are correlated with the timing of treatment. In row 7 of Figures 9 and 10, we restrict the sample to only counties that experienced at least one racial uprising in the 1960s. This limits the sample to 61 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, due to the fact that the restrictions are highly correlated.[18] Bigger counties tend to be urbanized and tend to 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 of row 7 of Figure 10 show that the decrease in white deaths in rioting counties is similar to the baseline estimates. It should be noted that the medium- and long-run effects are larger for non-whites and the long-run decrease in police killings for whites is no longer significant, which shows the importance of federal intervention to decrease police killings among non-whites.

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, 2018), and this may be true even for dynamic specifications (Sun and Abraham, 2020). Figure A.11 plots the event-study estimates of the effect of the threat of court-ordered affirmative action where the coefficients are estimated using (Callaway and Sant'Anna, 2020) estimator to avoid biases associated with TWFE models. Our estimates are very similar to our baseline findings; we see similar long-run decreases in non-white police killings.

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 includes counties that are 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-

preted with caution.

[18] Appendix Figure A.6 reports event-study estimates from equation 1 when we include event-year indicators showing the time before and after a county experienced their first uprising. Post-treatment results are similar to the main specification, showing a long-run decrease in non-white deaths.

white community were palpable. Appendix Figure A.10 plots event-study estimates for counties treated prior to 1980 and counties treated after 1980, respectively. 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 increase in police killings of non-whites in the year of treatment is related to uprisings that occurred around the same time, as the later group was treated well after the 1960s.

5 Conclusion

Our results indicate that affirmative action litigation affects a specific type of excessive use of force—police killings of civilians. We find that in the long run, police killings of non-white civilians significantly declined. We also find some evidence that white killings decreased as well. Given the recent call to diversify and restructure police departments, our results highlight the vital role that legal and federal interventions have in addressing excessive use of police force in marginalized communities. We confirm research from previous studies by finding suggestive evidence that the mechanism driving our results may be the increased minority representation in police departments. However, we also see a racial disparity in police killings in the initial year of the litigation threat. However, further analysis uncovered this threat may be a response to uprisings because we only see this pattern among cities that were treated after racial uprisings.

Overall, our results show that diversifying the racial composition of police departments decreases non-white deaths at the hands of police. However, even with greater diversity within the police department 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 oppositional to non-white citizens (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 the police personnel may be insufficient to address apparent racial disparities in police killing.

References

Auxier, R. C. (2020). What police spending data can (and cannot) explain amid calls to defund the police. Urban Institute.

Ba, B. A. and Rivera, R. (2019). The effect of police oversight on crime and allegations of misconduct: Evidence from chicago. U of Penn, Inst for Law & Econ Research Paper, (19-42).

Barber, C., Azrael, D., Cohen, A., Miller, M., Thymes, D., Wang, D. E., and Hemenway, D. (2016). Homicides by police: comparing counts from the national violent death reporting system, vital statistics, and supplementary homicide reports. American journal of public health, 106(5):922–927.

Bell, M. C. (2016). Police reform and the dismantling of legal estrangement. YAle lJ, 126:2054.

Boggess, S. and Bound, J. (1997). Did criminal activity increase during the 1980s? comparisons across data sources. Social Science Quarterly, pages 725–739.

Buehler, J. W. (2017). Racial/ethnic disparities in the use of lethal force by us police, 2010–2014. American journal of public health, 107(2):295–297.

Callaway, B. and Sant'Anna, P. H. (2020). Difference-in-differences with multiple time periods. Journal of Econometrics.

Carter, G. L. (1986). The 1960s black riots revisited: city level explanations of their severity. Sociological Inquiry, 56(2):210–228.

Chalfin, A. and McCrary, J. (2018). Are us cities underpoliced? theory and evidence. Review of Economics and Statistics, 100(1):167–186.

Close, B. R. and Mason, P. L. (2007). Searching for efficient enforcement: Officer characteristics and racially biased policing. Review of Law & Economics, 3(2):263–321.

Commission, U. S. K. and on Civil Disorders, U. S. N. A. C. (1968). Report of the national advisory commission on civil disorders. US Government Printing Office.

Correll, J., Park, B., Judd, C. M., and Wittenbrink, B. (2002). The police officer's dilemma: Using ethnicity to disambiguate potentially threatening individuals. Journal of personality and social psychology, 83(6):1314.

Correll, J., Wittenbrink, B., Park, B., M., J. C., and Goyle, A. (2011). Dangerous enough: Moderating racial bias with contextual threat cues. Journal of experimental social psychology, 47(1):184–189.

Cox, R. and Cunningham, J. P. (2020). Financing the war on drugs: The impact of law enforcement grants on racial disparities in drug arrests. forthcoming, Journal of Policy Analysis and Management.

Cunningham, J. P., Feir, D., and Gillezeau, R. (2020). The impact of access to collective bargaining rights on policing and civilian deaths. Working Paper.

Cunningham, J. P. and Gillezeau, R. (2019). Don't shoot! the impact of historical african american protest on police killings of civilians. Journal of Quantitative Criminology, pages 1–34.

Devi, T. and Fryer Jr, R. G. (2020). Policing the police: The impact of" pattern-or-practice" investigations on crime. Technical report, National Bureau of Economic Research.

Dharmapala, D., McAdams, R. H., and Rappaport, J. (2019). Collective bargaining rights and police misconduct: Evidence from florida. University of Chicago Coase-Sandor Institute for Law & Economics Research Paper, (831).

Donohue III, J. J. and Levitt, S. D. (2001). The impact of race on policing and arrests. The Journal of Law and Economics, 44(2):367–394.

Edwards, F., Esposito, M. H., and Lee, H. (2018). Risk of police-involved death by race/ethnicity and place, united states, 2012–2018. American journal of public health, 108(9):1241–1248.

Farhang, S. (2010). The litigation state: public regulation and private lawsuits in the US, volume 113. Princeton University Press.

Feigenberg, B. and Miller, C. (2018). Racial divisions and criminal justice: Evidence from southern state courts. Technical report, National Bureau of Economic Research.

Feldman, J. M., Gruskin, S., Coull, B. A., and Krieger, N. (2017). Quantifying underreporting of law-enforcement-related deaths in united states vital statistics and news-media-based data sources: A capture–recapture analysis. PLoS Medicine, 14(10):e1002399.

Fryer, R. G. (2018). Reconciling results on racial differences in police shootings. AEA Papers and Proceedings, 108:228–33.

Fryer, R. G. (2019). An empirical analysis of racial differences in police use of force. Journal of Political Economy, 127(3):1210–1261.

Fyfe, J. J. (2002). Too many missing cases: Holes in our knowledge about police use of force. Justice Research and Policy, 4(1-2):87–102.

Garner, M., Harvey, A., and Johnson, H. (2019). Estimating effects of police force diversity: A replication and extension of previous research. Working Paper.

Goodman-Bacon, A. (2018). Difference-in-differences with variation in treatment timing. Technical report, National Bureau of Economic Research.

Gray, A. C. and Parker, K. F. (2020). Race and police killings: examining the links between racial threat and police shootings of black americans. Journal of Ethnicity in Criminal Justice, pages 1–26.

Harvey, A. and Mattia, T. (2019). Reducing racial disparities in crime victimization. Working Paper.

Headley, A. M. and Wright, J. E. (2020). Is representation enough? racial disparities in levels of force and arrests by police. Public Administration Review.

Hidalgo, C. (2019). Police Officer Data.

Hoekstra, M. and Sloan, C. W. (2020). Does race matter for police use of force? evidence from 911 calls. Working Paper.

Jacobs, D. and O'brien, R. M. (1998). The determinants of deadly force: A structural analysis of police violence. American journal of sociology, 103(4):837–862.

Kahn, K. B., Goff, P. A., Lee, J. K., and Motamed, D. (2016). Protecting whiteness: White phenotypic racial stereotypicality reduces police use of force. Social Psychological and Personality Science, 7(5):403–411.

Loftin, C., Wiersema, B., McDowall, D., and Dobrin, A. (2003). Underreporting of justifiable homicides committed by police officers in the united states, 1976–1998. American Journal of Public Health, 93(7):1117–1121.

McCrary, J. (2007). The effect of court-ordered hiring quotas on the composition and quality of police. American Economic Review, 97(1):318–353.

McElvain, J. P. and Kposowa, A. J. (2008). Police officer characteristics and the likelihood of using deadly force. Criminal justice and behavior, 35(4):505–521.

Miller, A. R. and Segal, C. (2012). Does temporary affirmative action produce persistent effects? a study of black and female employment in law enforcement. Review of Economics and Statistics, 94(4):1107–1125.

Miller, A. R. and Segal, C. (2014). Do female officers improve law enforcement quality? effects on crime reporting and domestic violence escalation. University of Zurich, UBS International Center of Economics in Society, Working Paper, (9).

Miller, A. R. and Segal, C. (2019). Do Female Officers Improve Law Enforcement Quality? Effects on Crime Reporting and Domestic Violence. Review of Economic Studies, 86(5):2220–2247.

Myers, S. L. (1980). Why are crimes underreported? what is the crime rate? does it" really" matter? Social Science Quarterly, 61(1):23–43.

National Advisory Commission on Criminal Justice Standards and Goals (1973). A national strategy to reduce crime. National Advisory Commission on Criminal Justice Standards and Goals.

Pleskac, T. J., Cesario, J., and Johnson, D. J. (2018). How race affects evidence accumulation during the decision to shoot. Psychonomic Bulletin & Review, 25(4):1301–1330.

President's Task Force on 21st Century Policing (2015). Final report of the president's task force on 21st century policing.

Rabe-Hemp, C. E. (2008). Female officers and the ethic of care: Does officer gender impact police behaviors? Journal of criminal justice, 36(5):426–434.

Ross, C. T. (2015). A multi-level bayesian analysis of racial bias in police shootings at the county-level in the united states, 2011–2014. PloS one, 10(11):e0141854.

Shannon, J. (2020). Usa today poll: Americans want major police reform, more focus on serious crime. USA Today.

Sherman, L. W. and Langworthy, R. H. (1979). Measuring homicide by police officers. J. Crim. L. & Criminology, 70:546.

Smith, B. W. (2003). The impact of police officer diversity on police-caused homicides. Policy Studies Journal, 31(2):147–162.

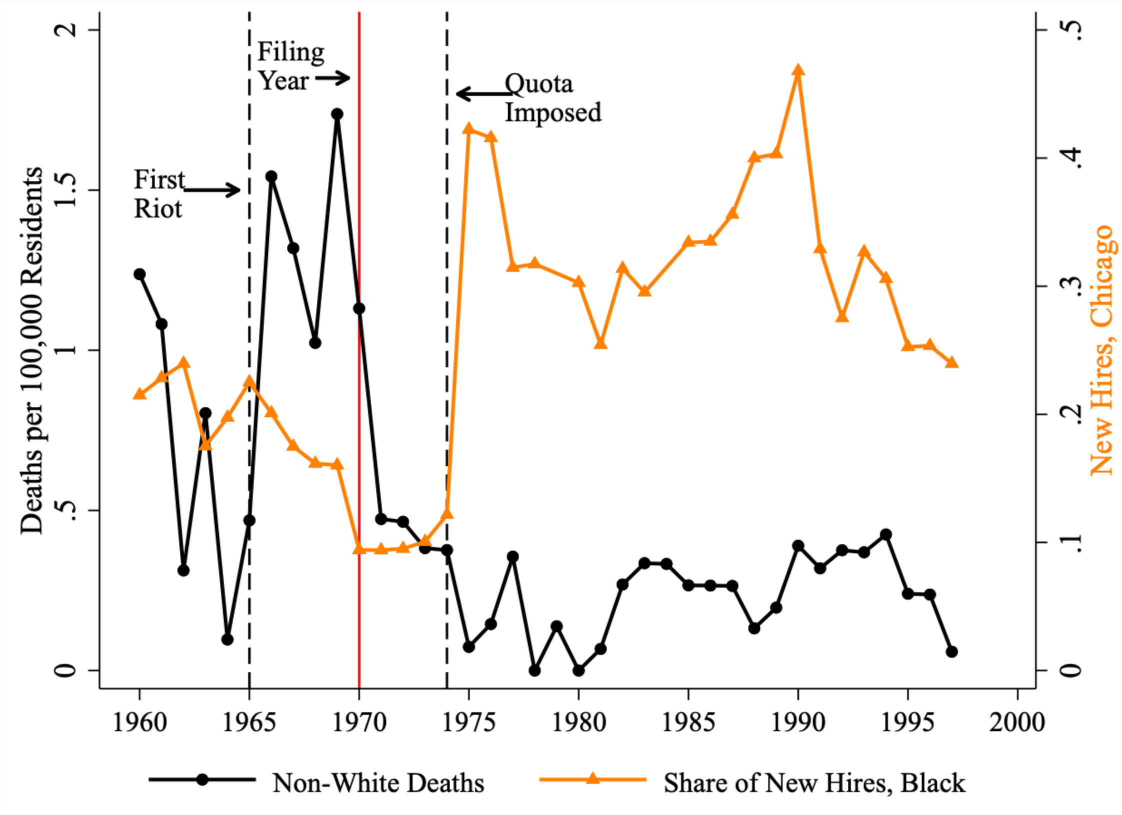
Sun, L. and Abraham, S. (2020). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. Journal of Econometrics.

US Department of Health and Human Services (2007). Multiple cause of death public use files, 2000-2002. https://www.icpsr.umich.edu/web/NACDA/studies/4640.

Weisburst, E. K. (2019). Police use of force as an extension of arrests: Examining disparities across civilian and officer race. In AEA Papers and Proceedings, volume 109, pages 152–56.

6 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

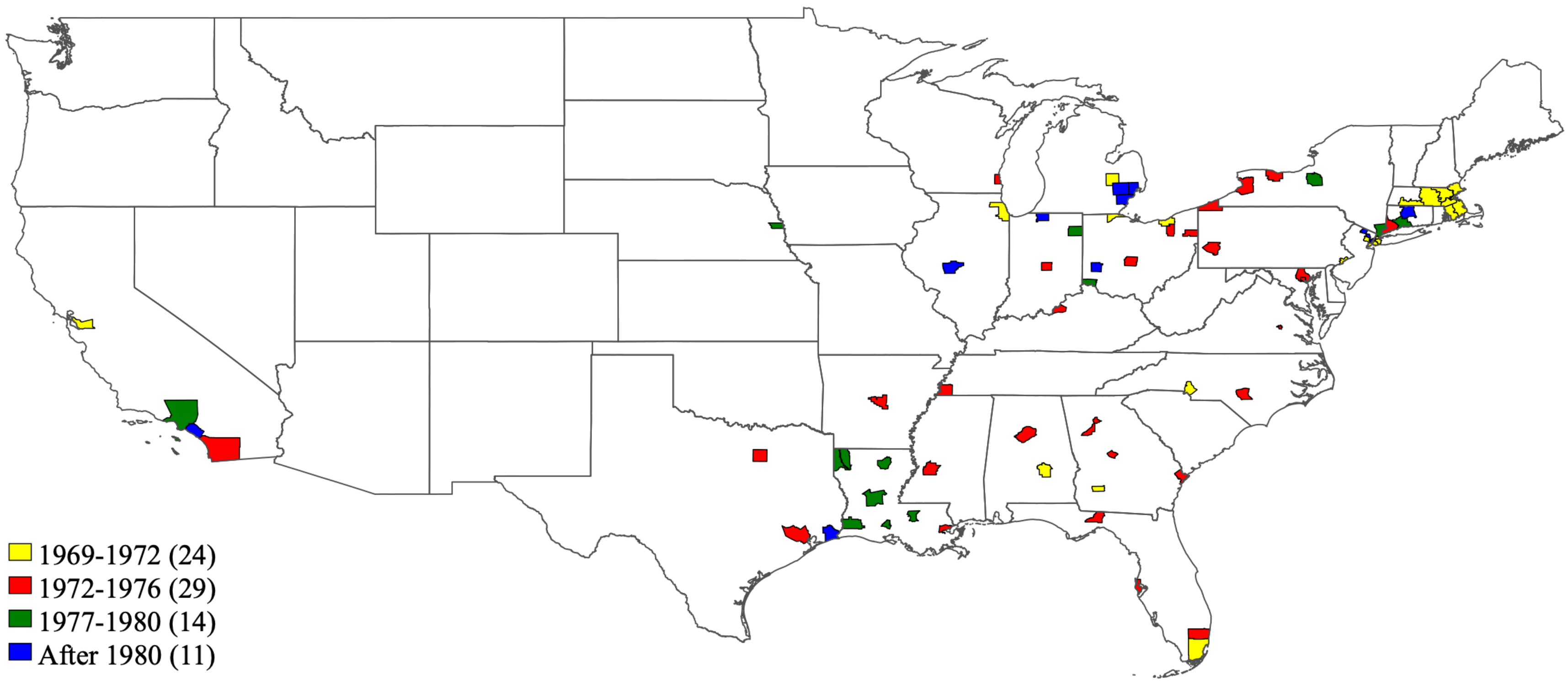


Figure 3: Percent of the Population Residing in Litigated Counties, 1969-2000

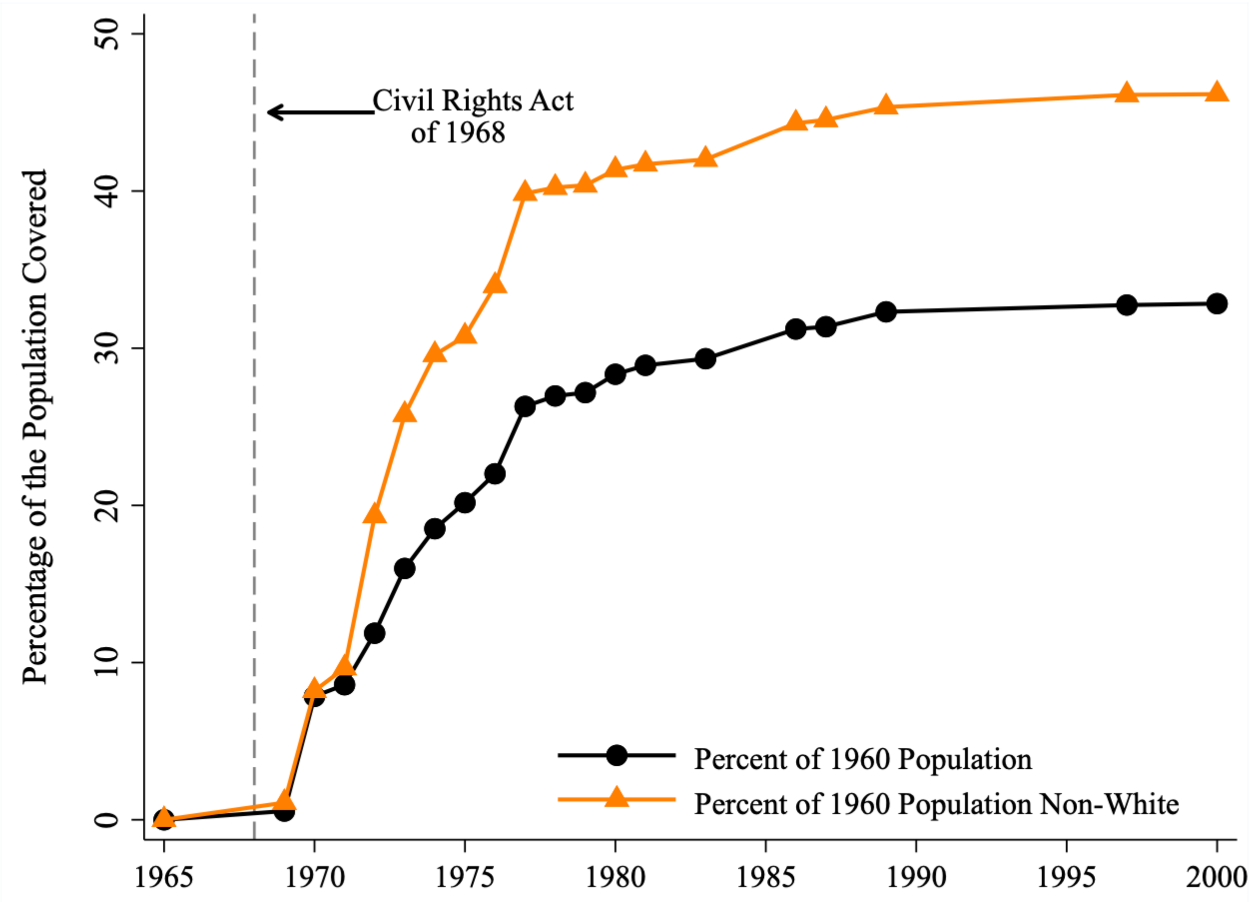
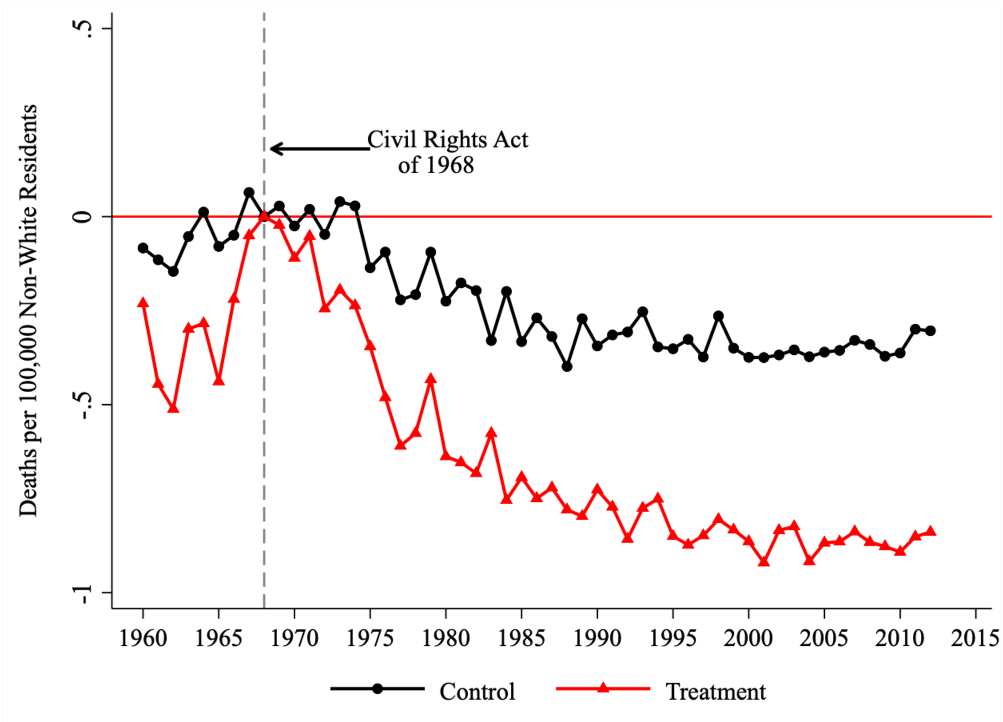
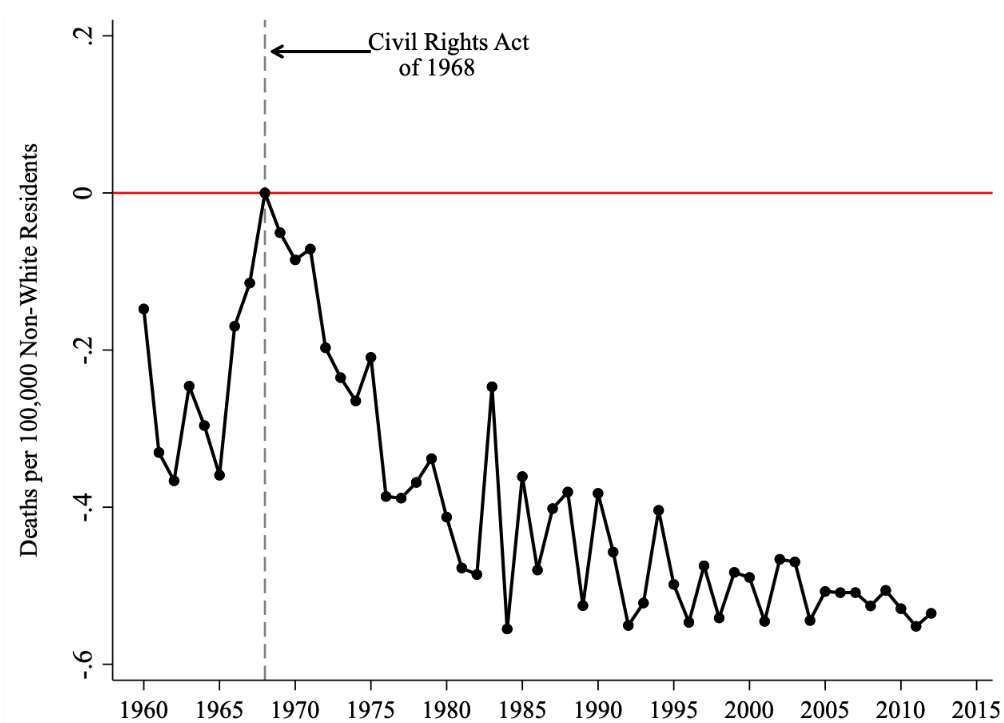


Figure 4: Police Killings - Normalized Difference

(a) Non-White Deaths



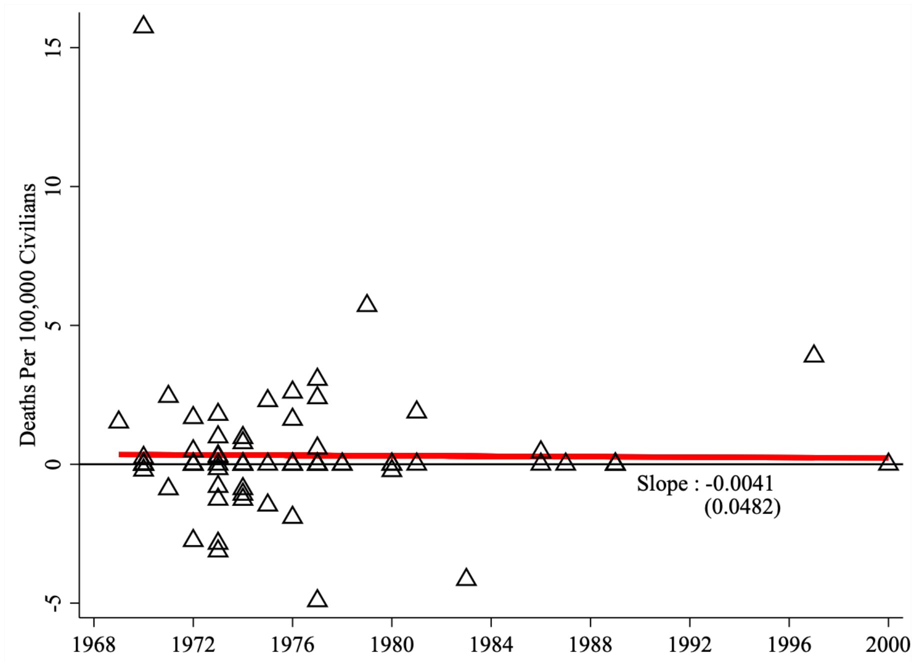
(b) Difference - Treated vs Control



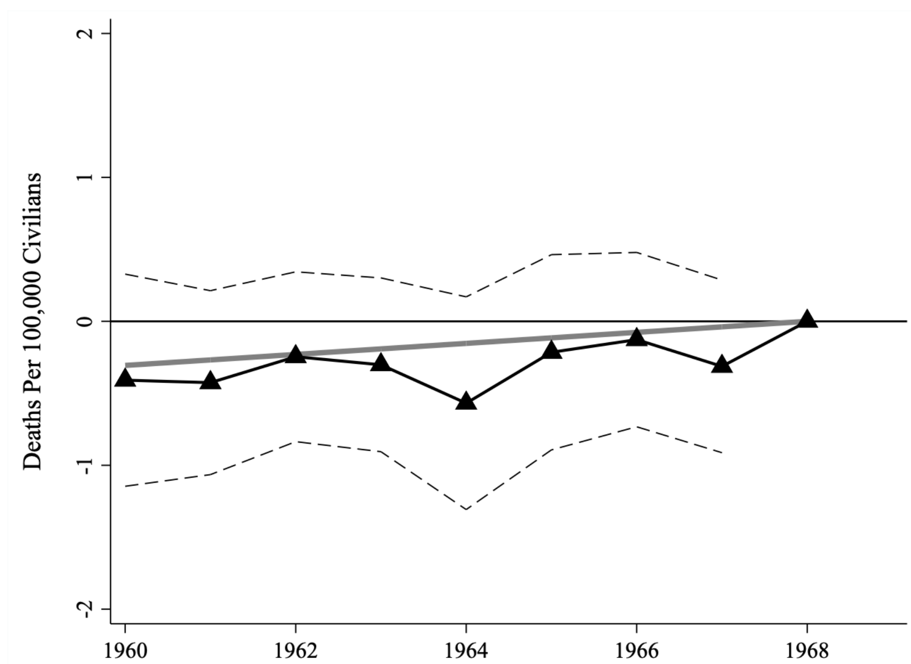
Notes: Panel (a) Police Killings have been normalized to zero in 1968. Panel (b) plots the difference in police killings between treated and control groups.

Figure 5: Pre-Period Growth Rates

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

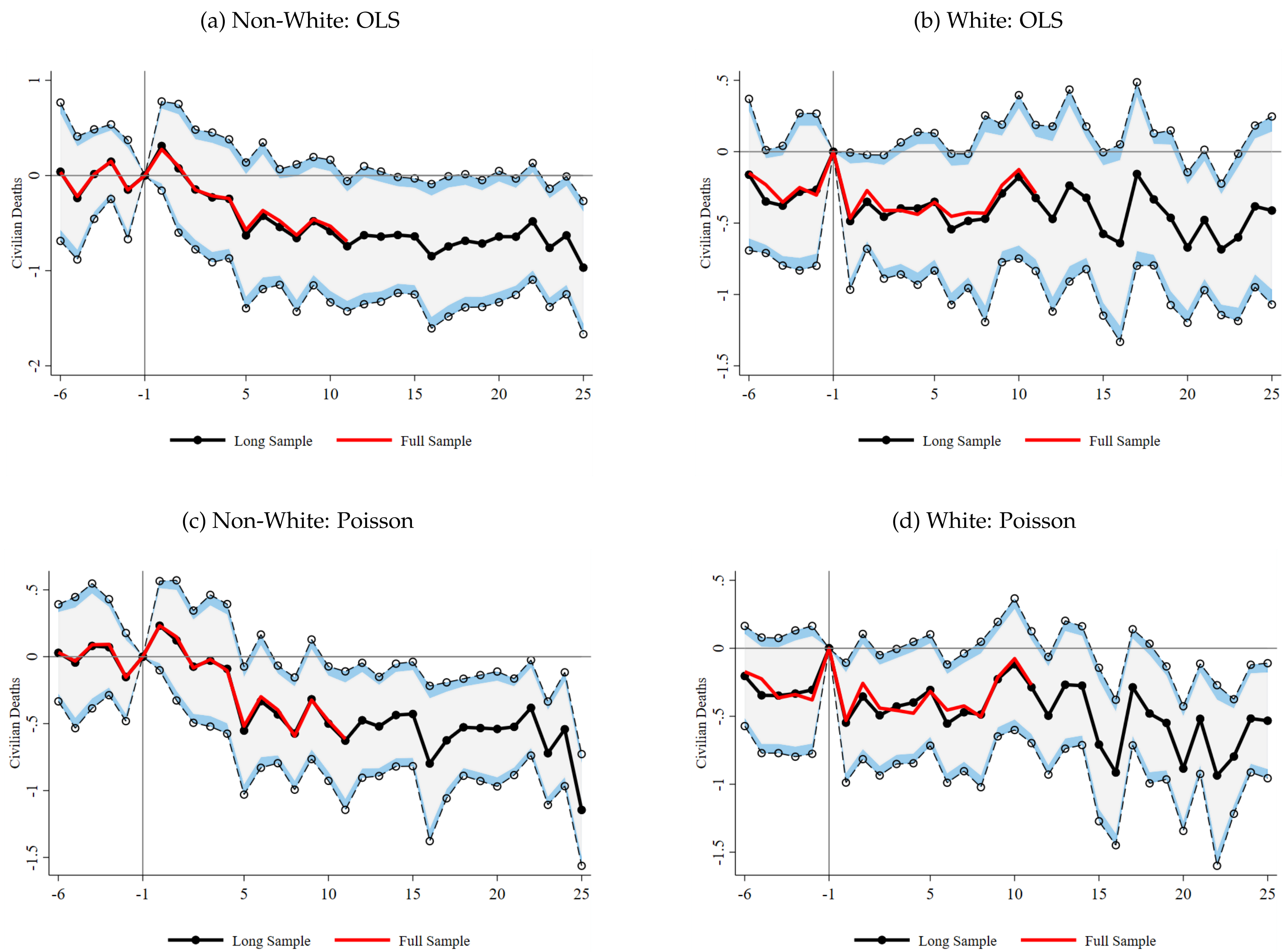


(b) Pre-Period Differences in Non-White Deaths



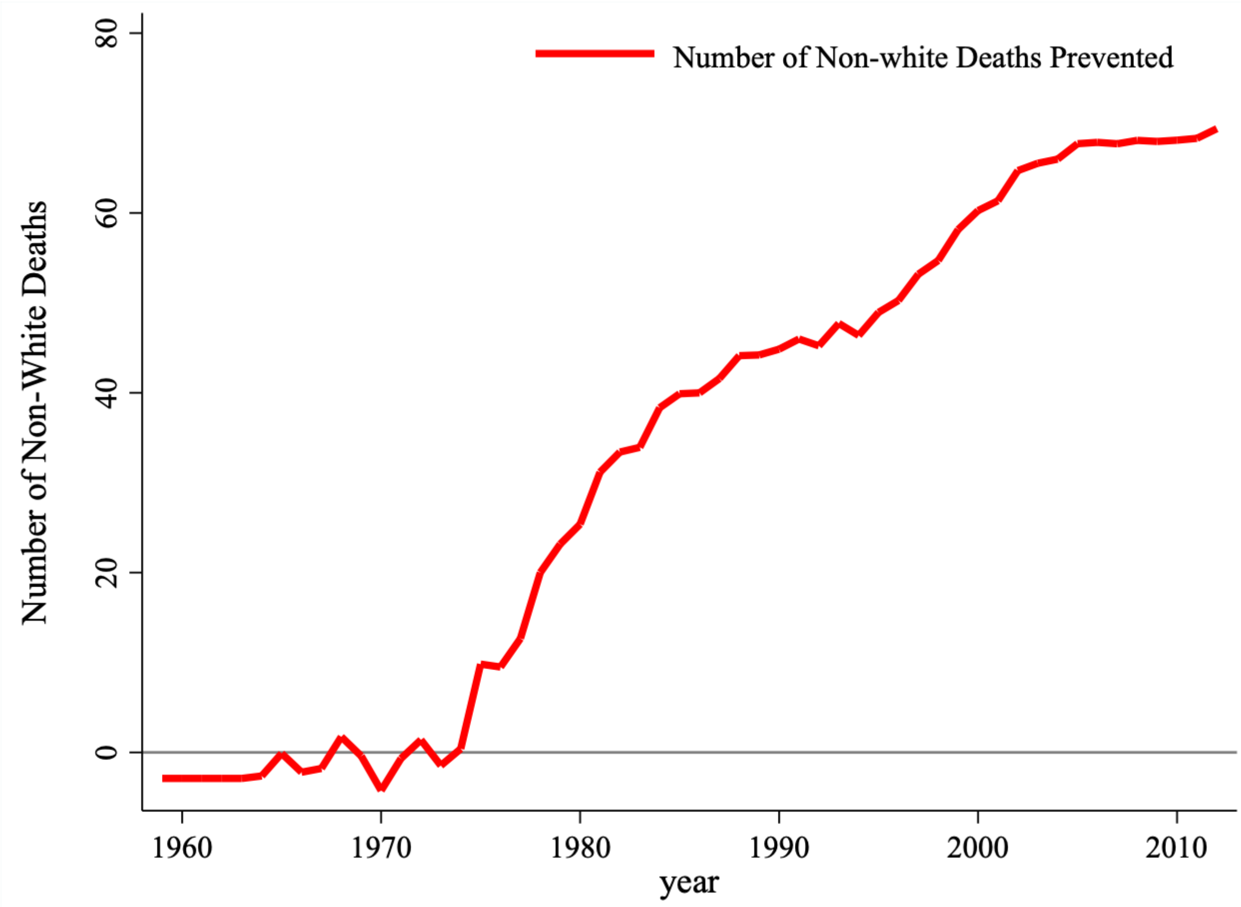
Notes: **[Panel A]** Regression coefficients and predicted values are from a univariate regression of the dependent variable changes in the non-white death rate on the year a county is threaten with litigation. **[Panel B]** The dependent variable is non-white deaths per capita. The independent variables 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



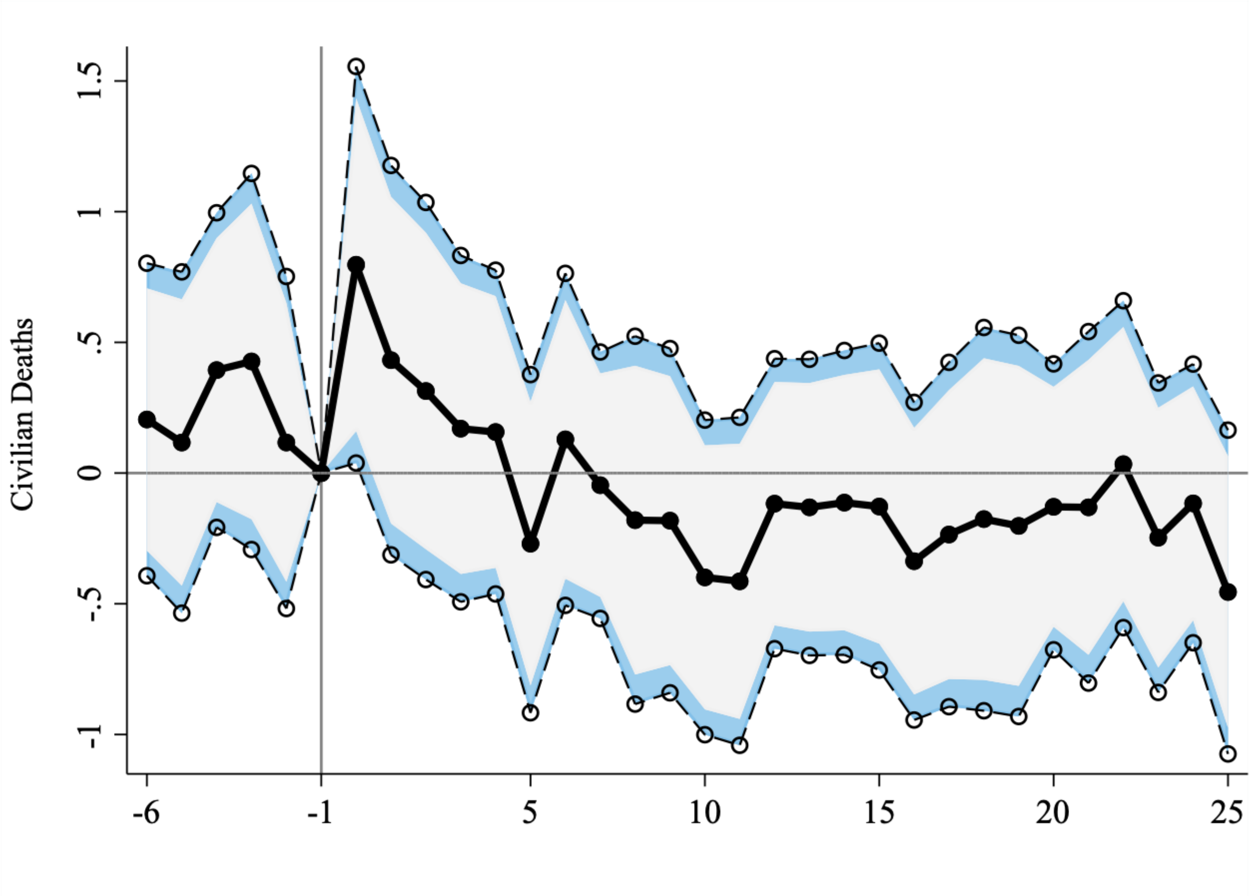
Notes: The ordinary least squares regression specification includes county, C, and region-by-year R-Y, effects. The red line corresponds to the full sample, locations treated between 1969 and 2000. The black line with circle markers corresponds the long-sample, locations treated between 1969 and 1987. 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: Number of Deaths Prevented



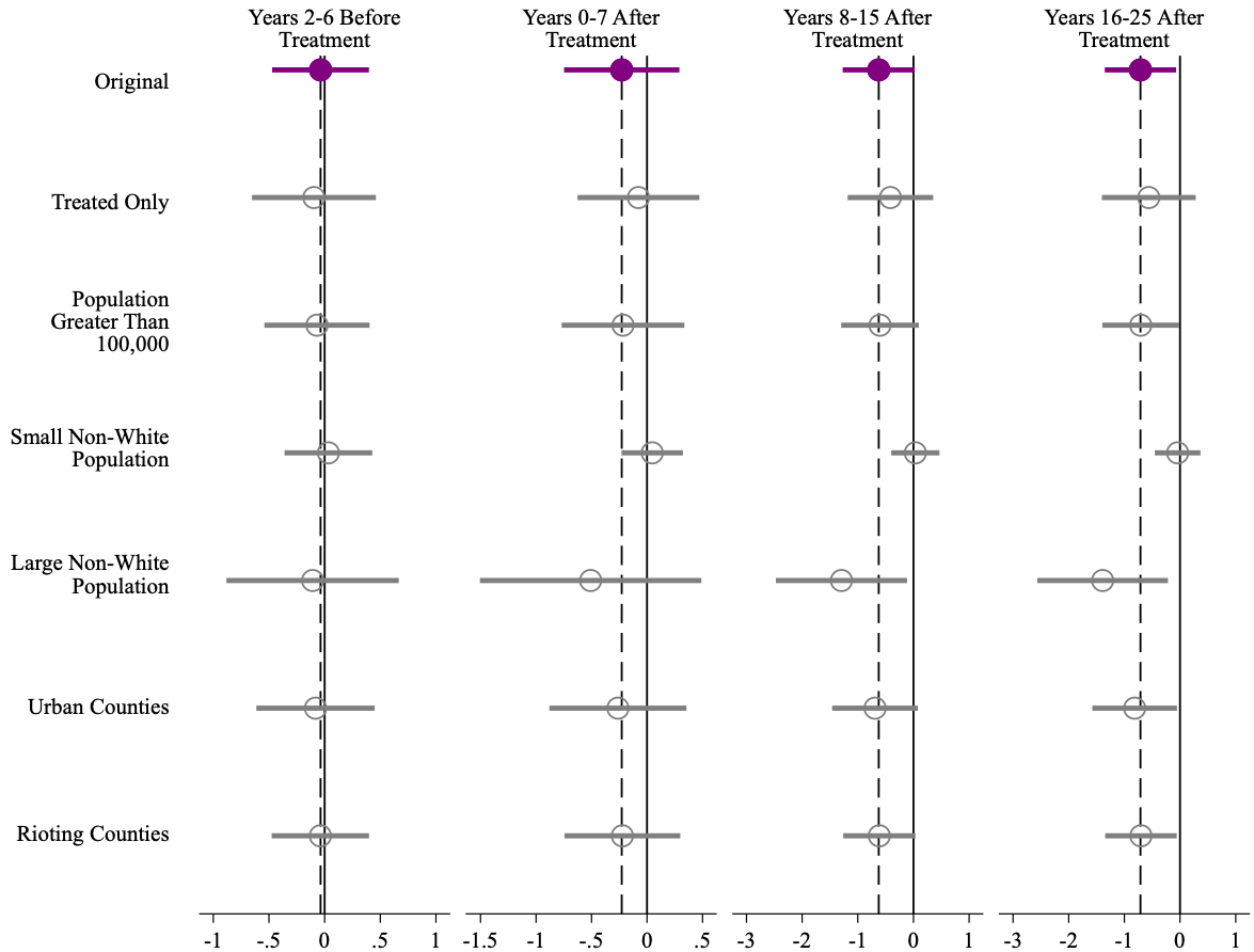
Notes: Based on Authors Calculations.

Figure 8: Triple Difference – Racial Disparities in Deaths Due to Legal Intervention



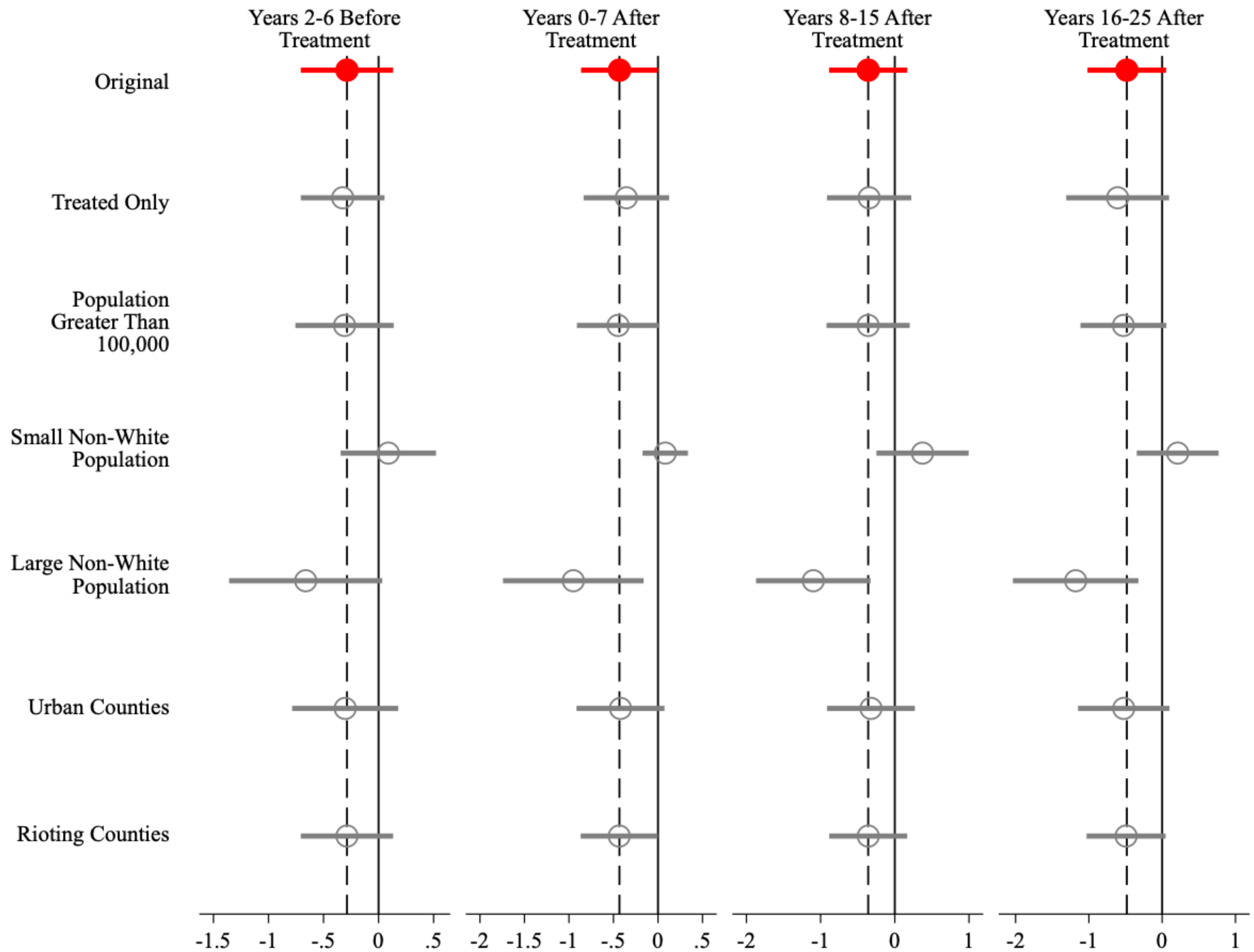
Notes: The ordinary least squares regression specification includes county, C, and region-by-year R-Y, effects. The coeficients are estimated from a Difference-in-Difference-in-Difference model. Marginal effects show the relative change in non-white deaths relative to white deaths in treated counties. 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 9: Robustness Checks - Non-White Deaths



Notes: The figure displays least-squares estimates obtained from estimating Equation 1 by grouping event years. All rows includes county, C, and region-by-year R-Y, effects. Heteroskedasticity-robust standard errors clustered by city are presented by the bold line. Joint lest-square coefficient is presented by the circle marker.

Figure 10: Robustness Checks - White Deaths



Notes: The figure displays least-squares estimates obtained from estimating Equation 1 by grouping event years. All rows includes county, C, and region-by-year R-Y, effects. Heteroskedasticity-robust standard errors clustered by city are presented by the bold line. Joint lest-square coefficient is presented by the circle marker.

7 Tables

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

Treatment Status	Number of Counties	Percent of Counties	Percent of 1960 Population
Treated	73	2.38	31.38
Year Treated:			
1969	1	0.03	0.53
1970	10	0.36	7.50
1971	2	0.42	8.21
1972	7	0.65	11.34
1973	10	0.98	15.28
1974	9	1.27	17.69
1975	3	1.37	19.27
1976	6	1.57	21.03
1977	8	1.83	25.13
1978	2	1.89	25.76
1979	1	1.93	25.97
1980	3	2.02	27.08
1981	2	2.09	27.63
1983	1	2.12	28.03
1986	2	2.19	29.83
1987	1	2.22	29.97
1989	3	2.32	30.88
1997	1	2.35	31.29
2000	1	2.38	31.38
Untreated	**2989**	**97.62**	**68.62**

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	62,477	817,139	41,601	<0.01
Population per square mile	175.26	2,908.79	99.65	<0.01
% of counties that experienced uprisings	0.10	0.84	0.08	<0.01
Percentage of the Population				
residing in urban areas	32.39	86.86	30.88	<0.01
w/ 12 or more years of education	33.44	43.25	33.17	<0.01
w/ income greater than 10K	7.67	16.81	7.42	<0.01
w/ income less than 3K	36.73	18.02	37.25	<0.01
non-white	11.58	16.06	11.46	<0.01
Deaths Due to Legal Intervention				
white	0.04	0.60	0.03	<0.01
non-white	0.04	0.97	0.02	<0.01 5
Number of Counties	2,712	73	2,639	
joint F-test				12.7
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)	-0.0366	0.00893	0.0386	-0.287	-0.306*	-0.0446*
	[0.222]	[0.153]	[0.106]	[0.214]	[0.175]	[0.0260]
Shorter-Run Effect (Event Years 0 to 7)	-0.228	-0.0929	-0.0778	-0.433**	-0.443***	-0.0590**
	[0.265]	[0.151]	[0.113]	[0.220]	[0.172]	[0.0250]
Medium-Run Effect (Event Years 8 to 15)	-0.624*	-0.452***	-0.280**	-0.359	-0.347**	-0.0529**
	[0.330]	[0.148]	[0.114]	[0.269]	[0.177]	[0.0247]
Longer-Run Effect (Event Years 16 to 25)	-0.711**	-0.598***	-0.326***	-0.482*	-0.617***	-0.0745***
	[0.327]	[0.147]	[0.116]	[0.274]	[0.175]	[0.0252]
Mean Dependent Variable	1.456	1.456	.412	1.162	1.162	0.145
Number of Counties	2,707	2,707	2,707	2,707	2,707	2,707

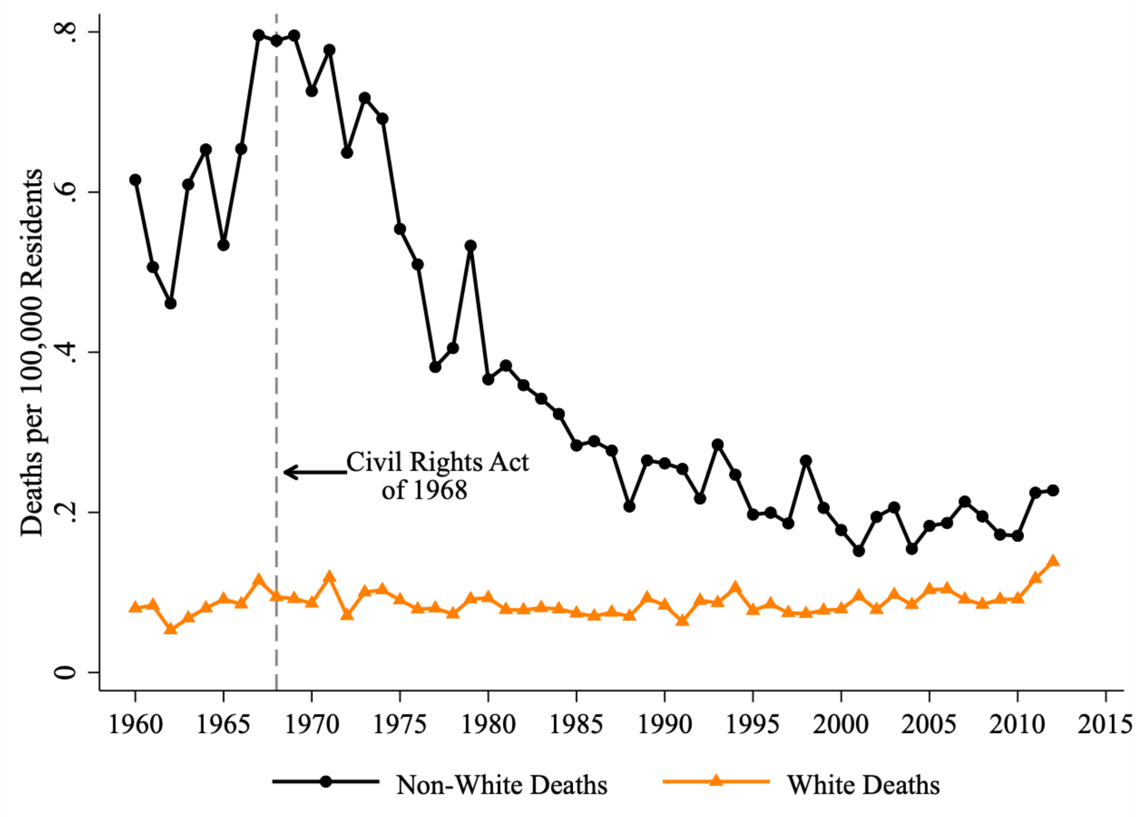
Table 4: Composition of Police Department, 1987-2013

	(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 timing of treatment. The reference group are counties that were never treated. All columns include region and year fixed effects. Mean dependent variables report average share of officers for the control group. Robust standard errors are clustered at the county-level and presented in brackets.

A Appendix Figures

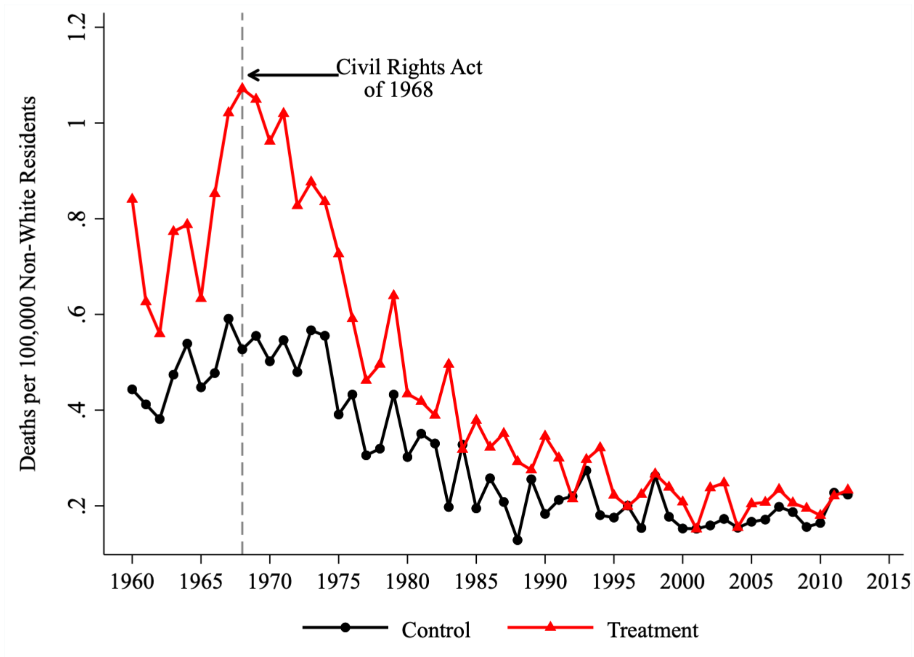
Figure A.1: Police Killings Over Time, 1960-2012



Notes: Data comes from United States Vital Statistics

Figure A.2: Police Killings Over Time by Treatment Status

(a) Non-White Deaths



(b) White Deaths

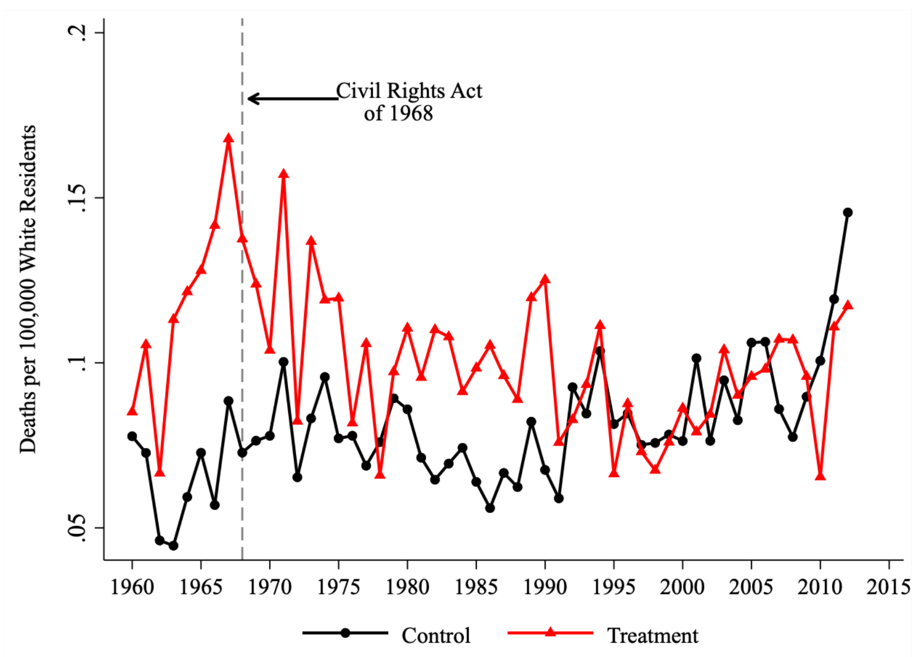
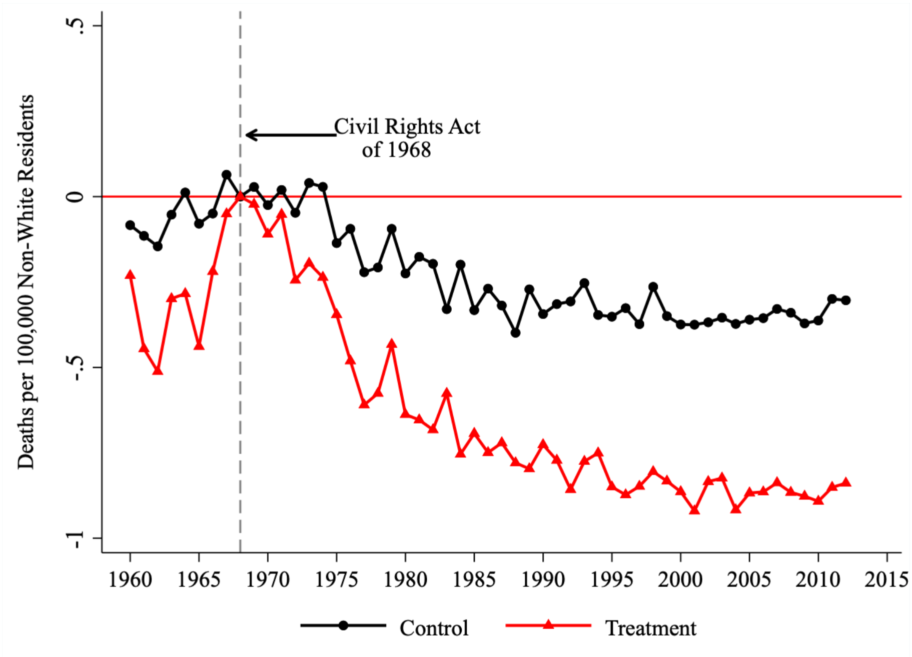
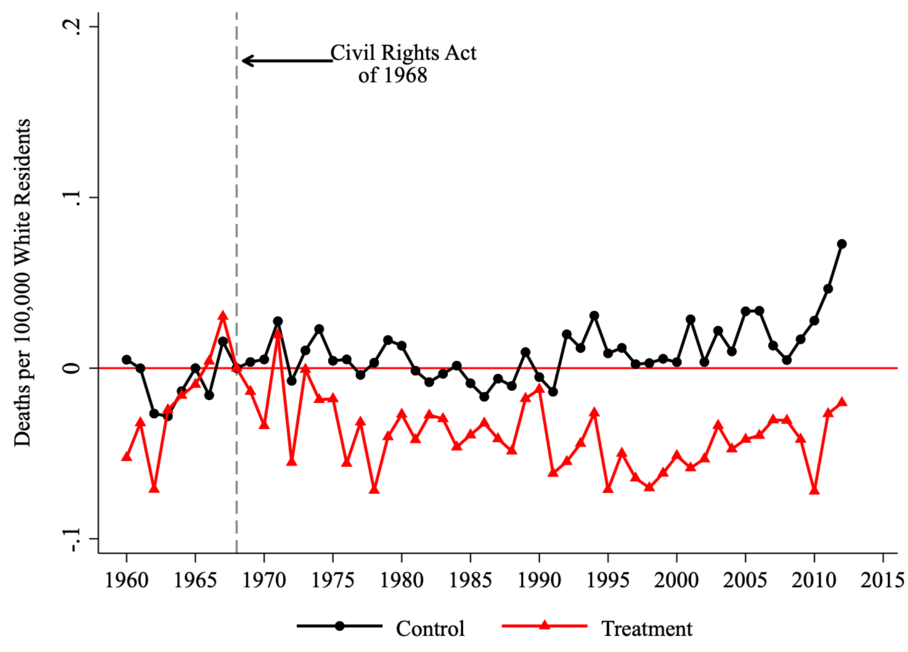


Figure A.3: Police Killings - Normalized Difference

(a) Non-White Deaths



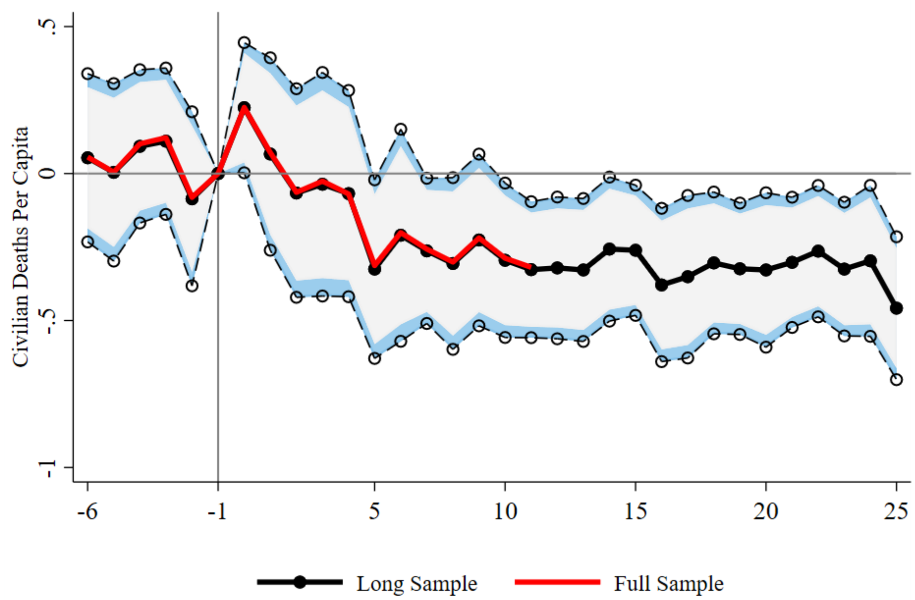
(b) White Deaths



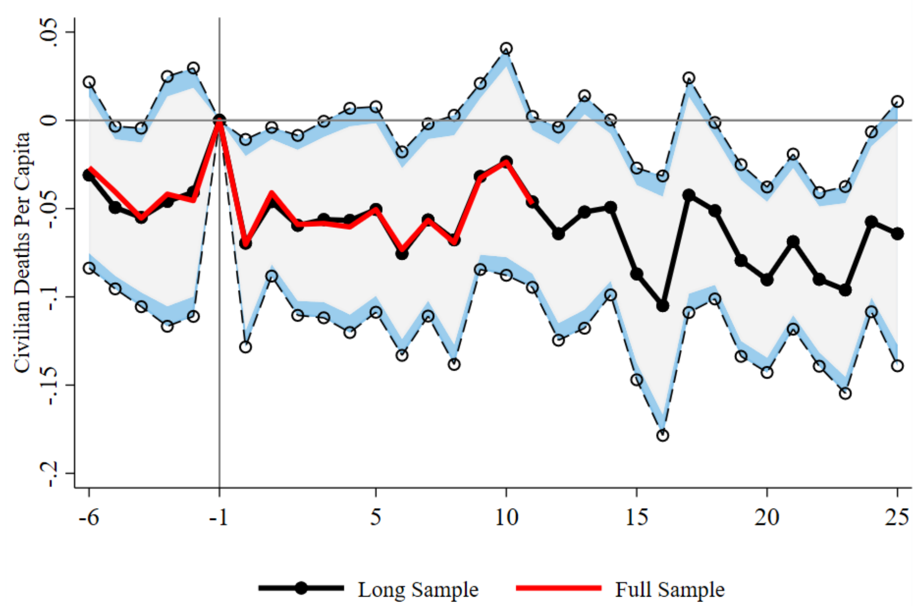
Notes: Police Killings have been normalized to zero in 1968.

Figure A.4: Weighted Least Squares – Deaths Due to Legal Intervention

(a) per 100,000 Non-White Civilians



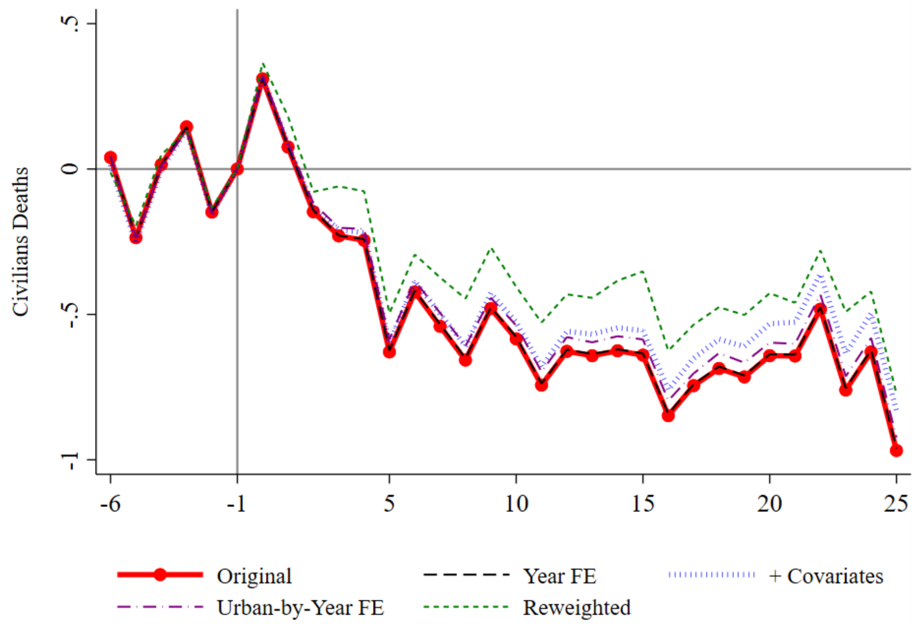
(b) per 100,000 White Civilians



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 A.5: Event Study Specification Check – Non-White Deaths

(a) Non-White Deaths



(b) White Deaths

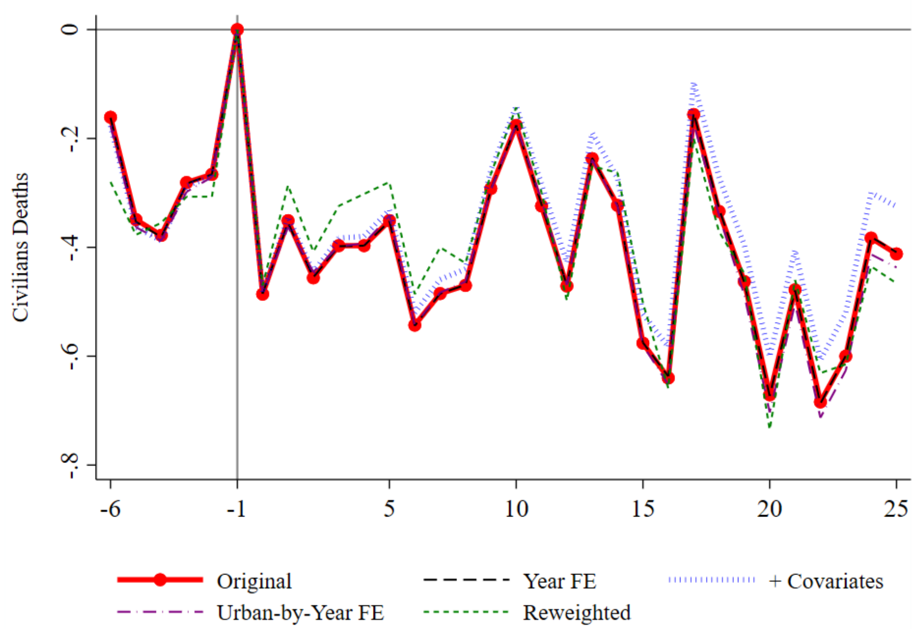
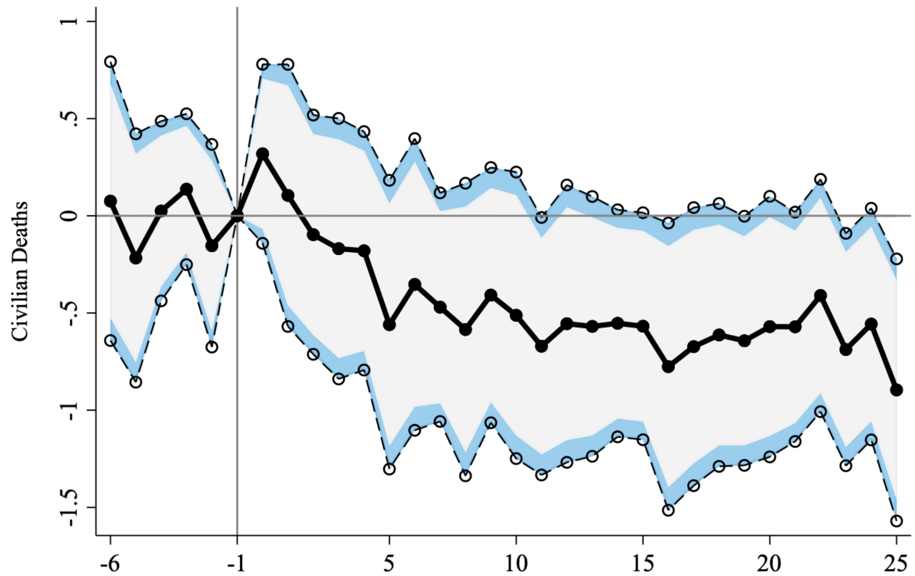
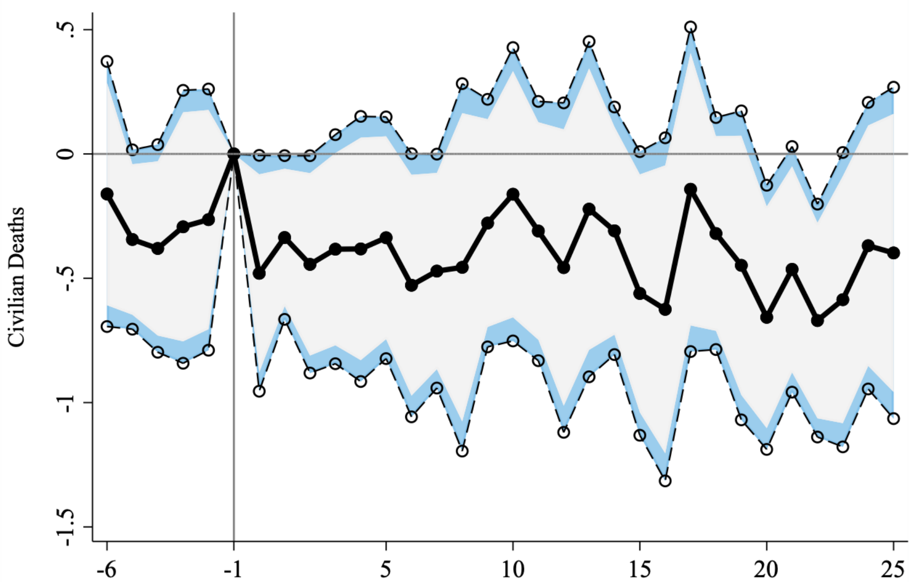


Figure A.6: Event Study – Account for Uprisings

(a) Non-White Deaths



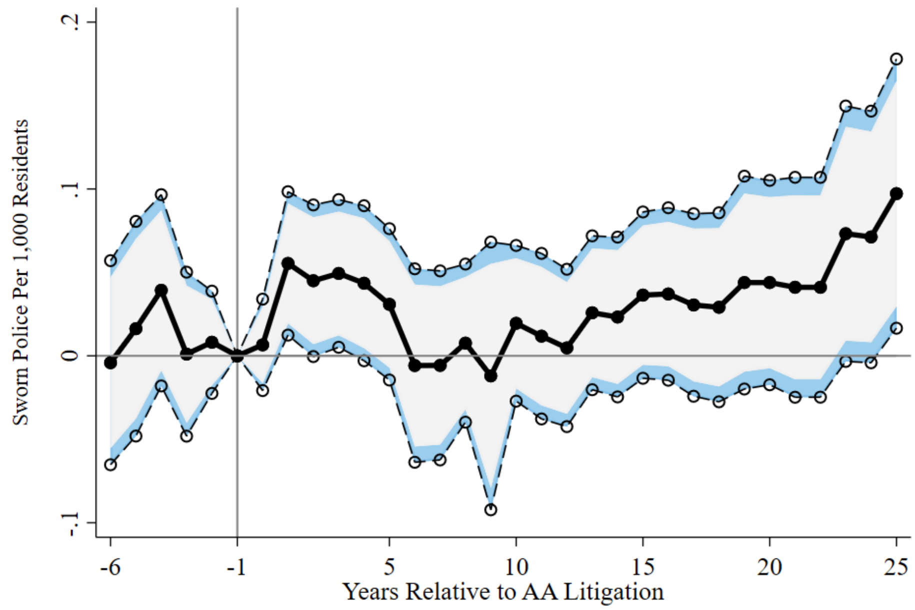
(b) White Deaths



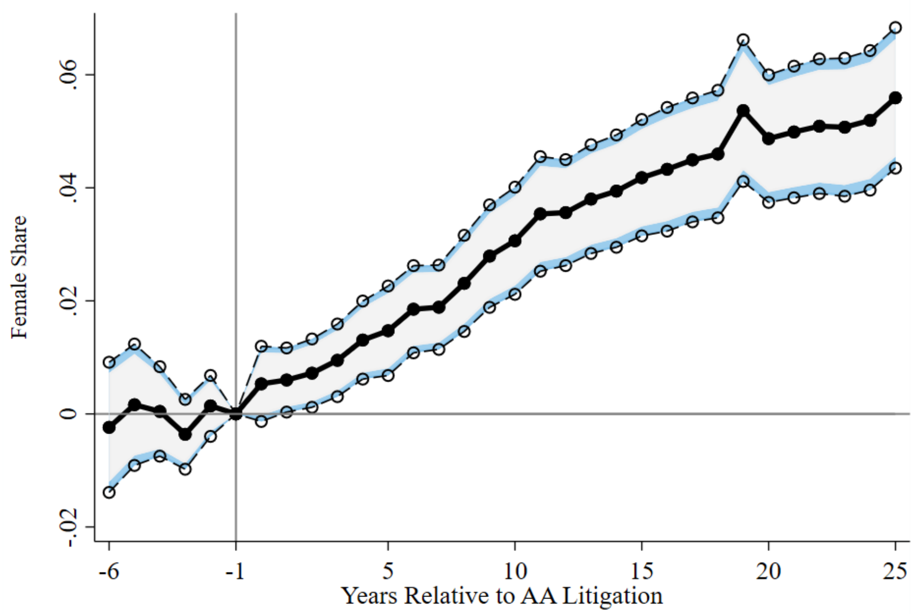
Notes: The regression specification includes county, C, region-by-year, R-Y, effects as well as event-year indicator that captures years before and after the first uprising occurs. Robust standard errors are clustered by county. The horizontal axis represents event-years (years before and after litigation).

Figure A.7: 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 A.8: Years Since Litigation and Police Composition

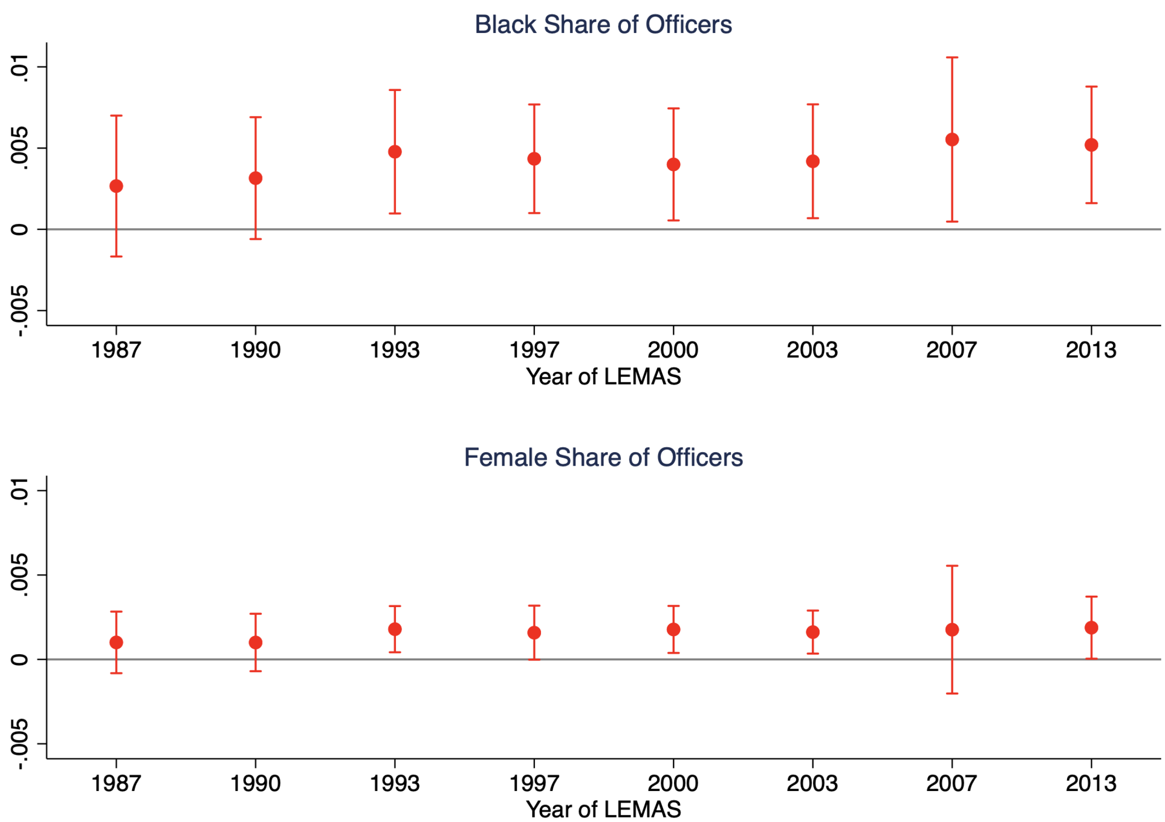
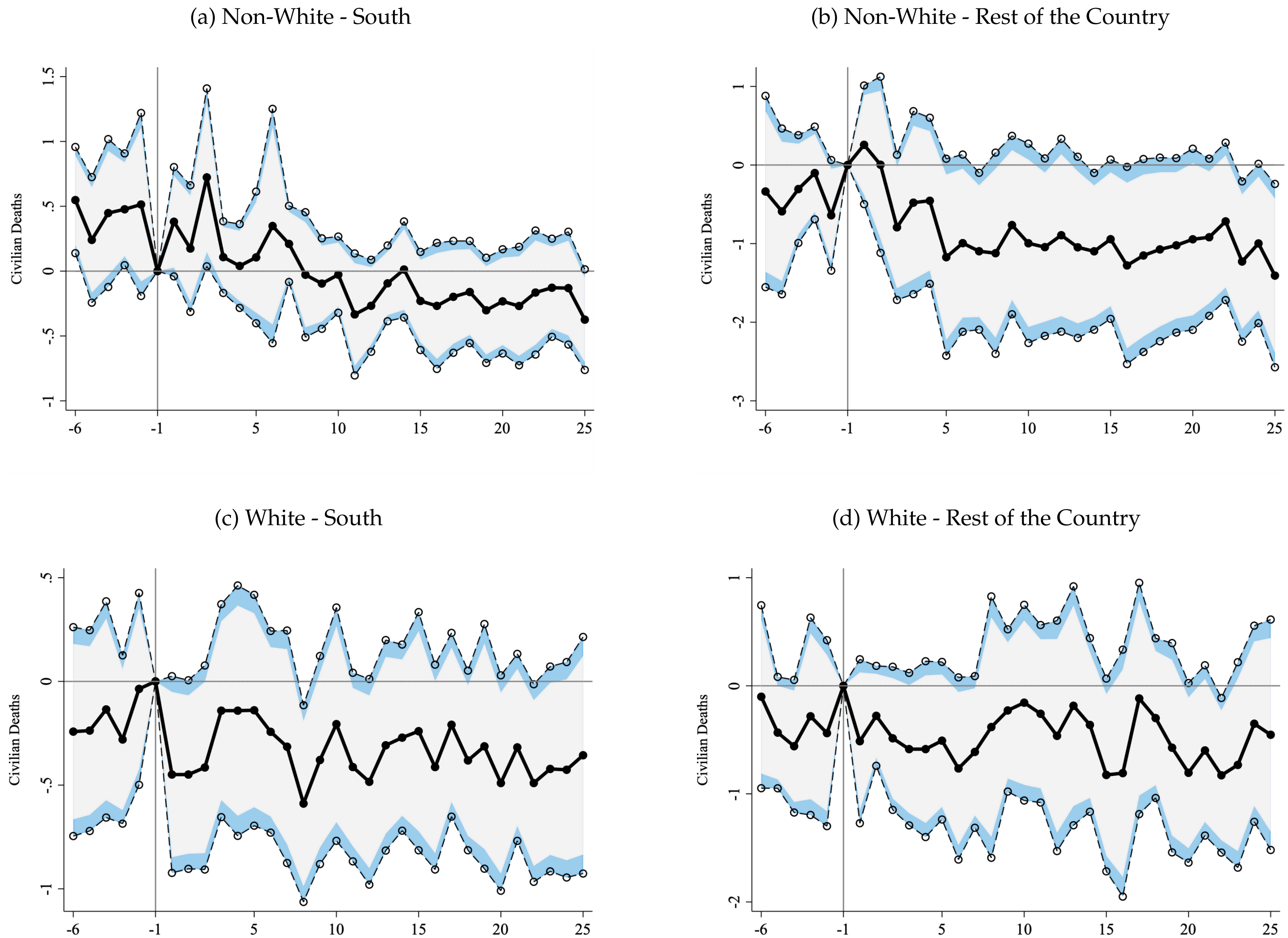


Figure A.9: Event Study Estimates – By Region



Notes: The regression specification includes county, C, effects. Panel (a) and (c) report event-study estimates for the south for non-white and white civilian respectively. OLS model includes year fixed effects. Panel (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).

Figure A.10: Event Study Heterogeneity in Timing – Non-White Deaths

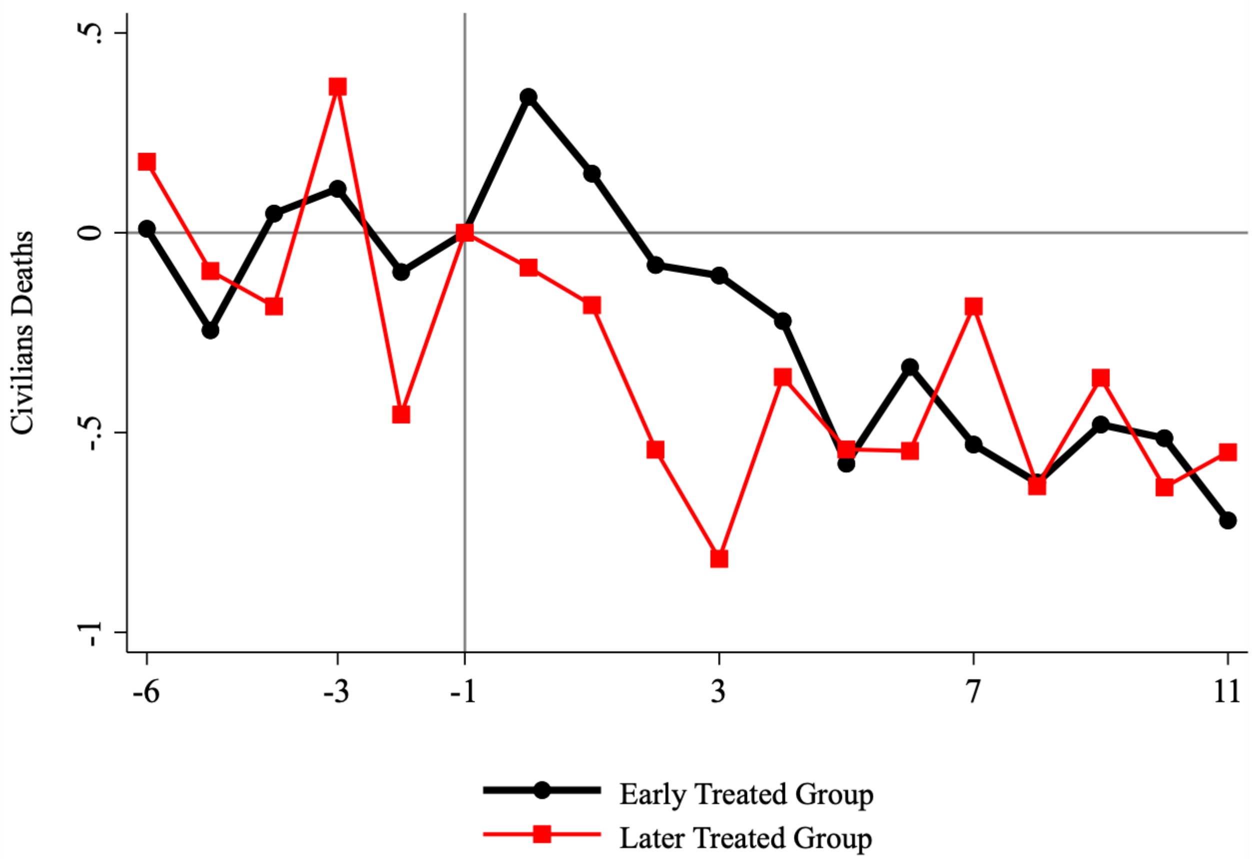
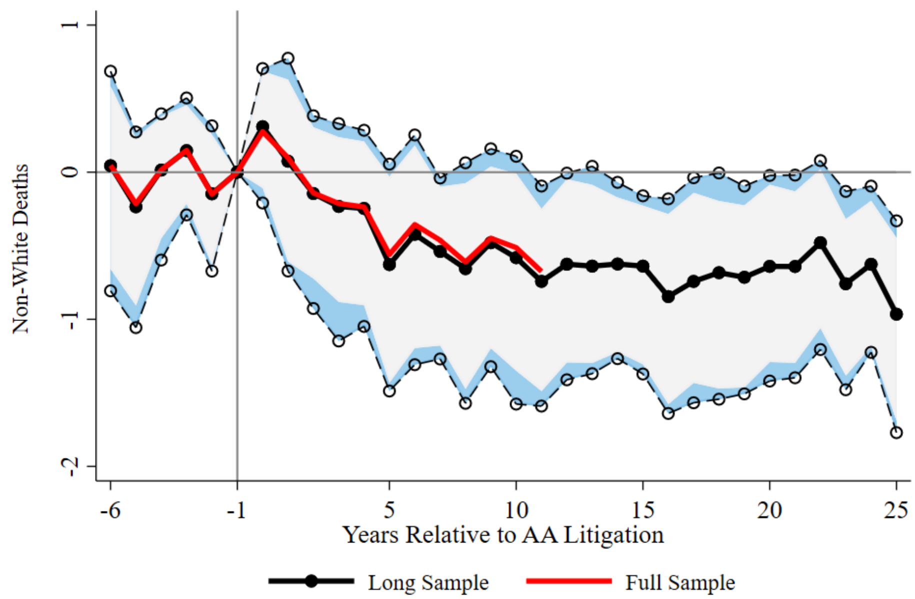
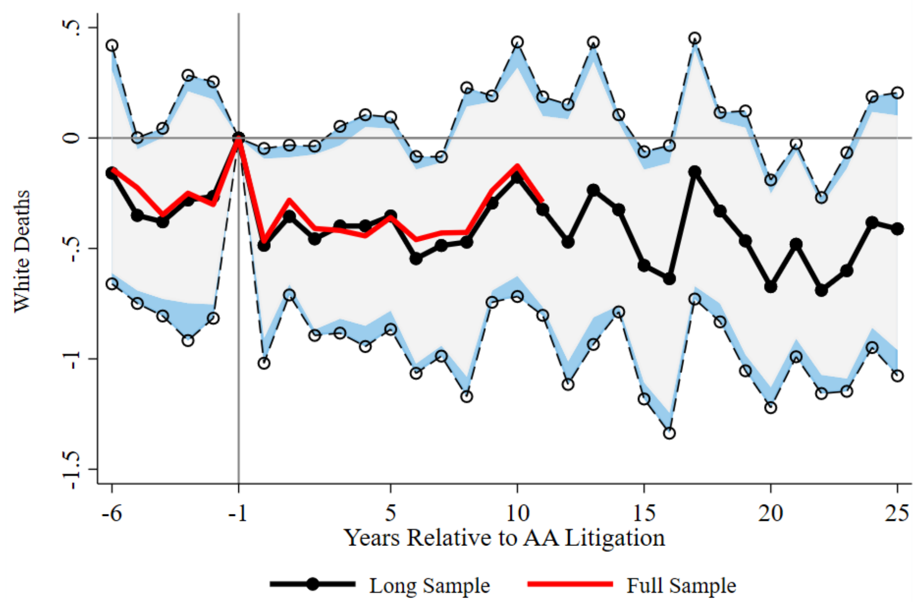


Figure A.11: Event Study – Deaths Due to Legal Intervention

(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) estimator to avoid biases associated with two-way fixed effects models highlighted in Goodman-Bacon (2020). Confidence intervals are constructed from 250 bootstrap draws where we resample counties with replacement and recalculate coefficients using Callaway and Sant'Anna (2020) estimator. 95 percent confidence intervals are reflected by dashed line with circle markers, 90 percent confidence intervals are captured by gray shaded area. The horizontal axis represents event-years (years before and after threat of litigation).

B Appendix Tables

Table B.1: 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.