DON'T SHOOT! THE IMPACT OF HISTORICAL AFRICAN AMERICAN PROTEST ON POLICE KILLINGS OF CIVILIANS

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Abstract:

For decades, African Americans have taken to the streets in both peaceful protests and, at times, violent uprisings to express outrage over police killings of African American citizens. However, there is little empirical evidence as to whether these protests affect the behavior of local police forces. We seek to close this gap by considering the impact of the racial uprisings in the 1960s on deaths by legal intervention. An event study approach reveals that 60s and 70s era racial uprisings resulted in an increase of civilian deaths at the hands of police. In the three years following a protest in a county, there were an additional 2.2 to 2.4 police killings of white Americans in impacted counties and 1.4 to 3.1 killings of non-white Americans. In subsequent years, the impact on killings of white Americans disappears while the impact on killings of non-whites persists. Furthermore, uprisings have little impact on police employment or crime, but the number of police officers killed on duty increases gradually over time. These results paint a depressing picture in which local police forces respond to racial unrest through increased killings of largely non-white civilians.

JEL Classification
N92, R00, K42, J15

Keywords
Protests, Riots, Uprisings, Police Homicides, Police Violence, Killings by Law Enforcement, Black Lives Matters, Civil Rights

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1. Introduction:

For decades, African Americans have taken to the streets in both peaceful protests and, at times, violent uprisings to express outrage over police killings of African American citizens. This long-standing history of protest is now manifesting itself in the hundreds of peaceful uprisings and infrequent violent uprisings that have occurred in the years after the death of Trayvon Martin and the rise of Black Lives Matter. The struggle for the fair treatment of the African American population by law enforcement is not a new phenomenon. In 1930s Baltimore, home of the National Association for the Advancement of Colored People's (NAACP) headquarters, the organization became active highlighting police brutality. This became a core part of the organization's mission (Santain, 2013). The Civil Rights Movement of the 1950s and 1960s had police brutality as a core concern, and the riots that erupted in the late 1960s were largely driven by incidents of perceived police mistreatment of African Americans. Throughout the 1980s, 1990s, and early 2000s incidents such as the beating of Rodney King in Los Angeles and the shooting of an unarmed teenager, Timothy Thomas, in Cincinnati resulted in periods of protest, violence, and calls for reform.

Regardless of the media attention that these protests have attracted in the post-Rodney King era, it remains an unanswered question as to whether protest events have had any impact on police killings of African American civilians or other related outcomes. Certainly, they have not resulted in their desired end goal in as far as the protests continue and young, unarmed, African American men and women continue to be disproportionately killed by law enforcement. However, just because problems remain, it does not mean that protests have necessarily been ineffective in curbing the use of force against civilians. There could have been meaningful subnational variation in the interactions of the African American community and police as a result of protests. It is also possible that theses protests have had the perverse effect of worsening the situation. As such, an empirical investigation is required to determine the exact role of protest on the response of police towards African Americans.

While it would be ideal to address this question using data from the current wave of Black Lives Matter protest event,[1] the data covering these events need to be digitized. Even after collection, these events face the limitation of being recent, making it impossible to determine any long-term protest impacts. As such, it would be impossible to discern whether protest events have any impact on the behavior of police officers over the long-run. Given these constraints, instead, we turn to the next most recent wave of African American protest regarding police violence: the over 700 racial uprisings that occurred in the mid to late 1960s and early 1970s. Although these protests occurred in a different era, they were frequently triggered by incidents of police brutality or other violent interactions between the African American community and police (Bauman, 2008). This protest wave also has the benefit of having occurred long enough ago that it is possible to determine their long-run impacts of these uprisings on a host of outcomes.

Using a dataset compiled by Carter (1986) and Spilerman (1971) of all riots that occurred in the United States between 1964-1971,[2] we look to determine the impact of the occurrence of county's first riot on the number of civilians killed by police as derived from 1959 to 1988 Vital Statistics Multiple-Cause of Death Files (US DHHS and ICPSR 2007). In particular, we seek to determine the impact of these uprisings on the number of civilians killed by ethnicity. With the available data, we are able to consider deaths based on a "white" or "non-white" binary. We pursue this analysis in an event study framework, taking advantage of variation in the location and timing of a county's first uprising to determine the impact of uprisings on police killings of civilians. Our approach controls for cross-sectional differences due to unobserved heterogeneity by using county fixed effects and differences across time by using year fixed effects. We use state-by-year fixed effects to account for potential unobservable changes in state policy over time or contagion effects. To ensure robustness, we interact various point-in-time county demographics with time. We also attempt to account for key cross-sectional differences by using a semiparametric reweighting scheme (Abadie 2005; Baily and Goodman-Bacon 2015). While we focus on the number of police

[1] These data are currently being digitized by a team at the University of Victoria's Racial Uprisings Lab under the direction of the two authors, covering the entire 1992-2017 period.
[2] These data were provided to the authors by William J. Collins and Robert Margo who used this data and developed a severity index in their 2007 paper.

homicides and the number of police homicides per 100,000 by race, we consider a number of related outcomes including incidents of crime, the number of police officers killed in the line of duty, and police employment.

The results indicate the African American protests in the 1960s and early 1970s resulted in an immediate increase in police killings of civilians regardless of race. However, the increases for white Americans disappear after a handful of years while the killing of African American civilians remains elevated into the future. A decade after a county's first uprising, an additional 1.3 – 2.3 African Americans per year were killed by the police in the affected county. In net, the impact of these uprisings appear to have resulted in several hundred additional African Americans killed each year by the police. Over a decade, the total runs into the thousands. Furthermore, uprisings appear to have little impact on the number of police killed, police employment levels or crime. These results paint a depressing picture in which local police forces respond to racial unrest through increased killings of largely non-white civilians in both the short and the long run.

2. Historical and Literature Review:

2.1 The Riots of the 1960s and 1970s

The United States experienced over 700 racial uprisings or riots between 1964 and 1971 in cities of all sizes and all regions. While the most prominent riots occurred in large urban centers such as Detroit, New York City, and Los Angeles, smaller communities with large African American communities such as Benton Harbor, Michigan often experienced uprisings. Some cities had a single riot, and others experienced several riots over the period,[3] although it was generally the case that a city's first riot was the most destructive.

This 7-year period of urban unrest was enormously destructive. Dozens of individuals were killed, thousands were injured, tens of thousands were arrested, and billions of dollars in property damage occurred (Harris and Wilkins, 1988). Uprisings were relatively rare in the early to mid-1960s, but their frequency

[3] For example, Washington DC experienced over a dozen riots over the 7-year period. Even a much smaller community like Benton Harbor, MI witnessed 3 riots.

increased gradually until a massive surge in 1967 and 1968 at which point they began to decline after the election of Richard Nixon on a platform of "law and order." The initial peak in the number and severity of rioting occurred in the "long hot summer" of 1967 and a second peak occurred in April 1968 in the immediate aftermath of the assassination of Dr. King. The vast majority of cities only had a single uprising, and only a handful of counties saw this figure reach double digits. Of direct relevance to the empirical strategy, the pattern of a county's first uprising generally mirrors the distribution of total riots by year as is displayed in Figure 1.

The vast majority of evidence (Sears and McConahay, 1973) indicates that the uprisings were not planned occurrences, but were rather spontaneous protests that resulted from interactions between the police and the African American community (Bauman, 2008).[4] Prominent uprisings such as the Watts Riot and the Detroit Riot of 1967 (Singer et al., 1970) followed this pattern.[5] We know from work by Collins and Smith (2007) and Collins and Margo (2007) that these uprisings were enormously destructive events that harmed African Americans' income, labor market outcomes, and property values over the long run. Additional work by King (2003) has failed to find any substantial long-run positive economic impacts of the riots. They may have also hastened the "white flight" from America's urban cores, although this is uncertain (Boustan, 2010). Even though these uprisings were often grounded in the use of or perceived use of state violence against African Americans, there is no evidence regarding their impact on police killings of civilians. In fact, as far as we are aware, there is currently no work that has empirically evaluated the

[4] Sociologists have advanced a number of theories as to the underlying causes of the 1960s riots, although there is little consensus the causes. Liberson and Silverman (1965) believed that a lack of access to political representation was a driving source of riots as there few other mechanisms to convey grievances. Berkowitz (1968) argued that the progress of the Civil Rights era may have heightened expectations for progress, which when not met resulted in violence. Downes (1968) believed that a lack of integration into society made rioting more acceptable as a path to airing grievances. Myers (1997), building off of earlier work by Spilerman (1970, 1971, 1976) sought to empirically evaluate many of these theories, finding little evidence in their favour and, instead, pointing to ethnic competition and diffusion as causes. Carter (1987) finds that having too few or too many police officers can also increase the likelihood of triggering a riot. Economists such as Gunning (1972) and DiPasquale and Glaeser (1998) have similarly sought to explain the rioting decision in a rational choice framework.

[5] There is also evidence that police treat African American protest very differently than those held by white Americans (Davenport et al, 2011). Police are much more likely to be present at Black protests and to take direct action, which may in turn make an eruption of violence more likely.

impact of racial unrest in the United States on police behavior towards the African American community. This is a gap in the literature that this work seeks to close.

2.2 Police Use of Force on Civilians

Not only has there been no empirical research on the relationship between historical African American protest and police killings of civilians, but there is also remarkably little empirical research in the area of police killings of civilians at all. The limited research that exists tends to be limited to a relatively small set of locations and only considers impacts over a relatively short period.

Some work has been done looking at inter-city variation in the civilian killings by police officers. Jacobs (1998), considering 170 cities across the United States, shows that racial income inequality is a major determinant of police killings as are factors of inter-personal violence. Jacobs also finds evidence of an institutional power effect in that the introduction of a black mayor reduces the number of African American civilians who are killed. However, these results could be subject to a substantial endogeneity problem, and there is no source of exogenous variation that the author can employ. Cloninger (1991), looking at a smaller sample of cities, showing that police killings of civilians may be playing a "deterrence" role in that they decrease the incidence of non-violent crime.

Roland Fryer's (2016) recent work in the area has drawn a great deal of media attention but is relatively limited in its scope. Fryer shows that there is, in fact, a gap in the use of non-lethal force against African Americans and Hispanics relative to the white population, but is unable to identify a similar gap for officer-involved shootings of civilians. However, this research more closely resembles a case study given its limited data sample, and it remains an open question as to whether the results are generalizable to other regions of the United States or other time periods.

There is also a rich literature regarding the interaction between policing killings or misconduct and the use of new technologies for policing. Pang and Valou (2016) find that the regional usage of smartphones and statistical analyses of crime data decrease the number of civilian shootings by police with the effects being most pronounced in the Black and Hispanic communities. However, they find the counterintuitive result that body cameras do not explain any decline in deadly shootings. This result is contradicted by Ariel

at al. (2014) who find evidence that the wearing of body cameras substantially reduces complaints filed against the police Alpert and MacDonald (2001), using data from 256 individual agencies, finds that requiring a third party to fill out use of force forms causes a decline in use-of-force rates.

Across this literature, the results are consistent with priors suggested by economic theory. Institutional or technological factors that increase the potential risk to police officers are found to result in more civilian deaths resulting from legal intervention and factors that increase the cost of the use of force to police officers discourage its use. A priori, however, it is unclear as to whether African American protest, peaceful or otherwise, will impose meaningfully large costs to police for engaging in the use of force to potentially outweigh any risks, real or otherwise, perceived by the police.

3 Methodology

To determine the causal impact of racial uprisings on police killings of civilians, we proceed with an event-study analysis with a robust set of controls. This approach requires county-level data on uprising occurrence, civilian deaths by legal intervention, and control variables interacted with time. We take advantage of variation in the location and timing of a county's first uprising to identify the impact of uprisings on police killings of civilians. The analysis focuses on the total civilian killings by race and for the overall population. Given that many counties had more than a single riot over the sample period, we choose to narrow in on the first riot that occurs in a county.[6] We also engage in a number robustness checks that supplement the main analysis.

3.1 Empirical Strategy

We take advantage of variation in the location and timing of a county's first racial uprising to identify the impact of uprisings on police killings of civilians. In particular, we focus on the first uprising that a county experience as the treatment variable of interest. A county is "treated" after the *first* uprising occurs. Untreated counties are those that never experience a racial uprising in the 1960s. Untreated counties

[6] For robustness, we also consider specifications using the date of a county's most severe uprisings, although we believe these results to be less precisely identified than those surrounding the occurrence of a first riot. More often than not, a county's first riot is also its most severe riot.

help identify how police killings of civilians are changing over time and provide a functional control group for the evolution of police killings of civilians in the absence of any uprisings occurring (counterfactual). Our approach controls for cross-sectional differences due to unobserved heterogeneity by using county fixed effects and differences across time by using year fixed effects. We additionally use state-by-year fixed effects to account for potential unobservable changes in state policy over time or contagion effects. To ensure robustness, we also interact various point-in-time county demographics with time.

The key identifying assumption for this approach is that the timing of the first riot is uncorrelated with other determinants that influence riot behavior. A test of this assumption is embedded in the difference-in-difference model that includes both leads and lags to analyze changes in the outcome variable of interest before and after the treatment occurs, which is commonly called an event study.[7]

Our primary empirical specification appears as follows:

$$(1)\ K_{it} = \alpha_i + \gamma_{u(i)t} + \delta_{s(i)t} + \sum_{y=-5}^{-1} \pi_y D_i 1(t - T_i^* = y) + \sum_{y=1}^{10} \phi_y D_i 1(t - T_i^* = y) + X_{i,t}'\beta + \varepsilon_{it}$$

where K is the number of civilian deaths by police in county *i* in year *t*, α is a set of county fixed effects, γ us a set of urban status-by-year fixed effects, δ is set of year or state-by-year fixed effects, and ε_{it} is an error term.[8] The column vector $X_{i,t}'$, consists of covariates from the 1960 census interacted with a linear time trend. Covariates include the percentage of the population that is non-white, population per square mile, the percentage of the labor force unemployed, the percentage of workers using public transportation, the percentage of the population in households earning less than 3,000 dollars, the median family income of white households, the median family income of non-white households, the number public assistance recipients, and the percentage of black owner occupied homes. D_i is an indicator variable equal to one if a county ever experiences an uprising. The effect of an uprisings on police killings of civilians is then

[7] See Jacobson, LaLonde, and Sullivan, 1993.

[8] Urban-by-year fixed effects are constructed by interacting year indicator variables with five categories of a county's population share in urban areas, *u*: 0, $0 < u < 25$, $25 \leq u < 50$, $50 \leq u < 75$, $75 \leq u \leq 100$. This captures the differential utilization of police resources and changes in use of force with varying degrees of urbanization.

captured in a set of event-year indicators $1(t - T_i^* = y)$, which are equal to one if the observation year is y years after the first uprising occurs. $1(t - T^* = 0)$ is omitted due to collinearity where T^* is the year of the first uprising; to ensure the coefficients are well estimated, event time for $y > 9$ and $y < -4$ are grouped into endpoints, $y = -5$ and $y = 10$.

The event-study design provides a statistical description of the evolution of pre-trends in the outcome variable as well as the dynamics of changes in the outcome variable after the first racial uprising occur. The pre-treatment effects are important as they provide a falsification test of pre-treatment, time-varying, county-level unobservables that influence the outcome, similar to the pre-treatment test in the difference-in-difference literature. Since the econometric model includes county-fixed effects, the pre-treatment effects, π, and post-treatment effects, ϕ, are unbiased even in the presence of pre-existing and permanent differences between counties that experience uprisings and those that do not.

The event-study framework provides an inter-temporal response of the outcomes that are gradual and non-linear. This dynamic model estimates changes in the outcome relative to the date of the first uprising, eliminating potential downward bias from the averaging of outcomes in reactionary locations with those from more volatile locations. This framework uncovers and estimates a causal relationship between uprisings and police use of deadly force. Because the model captures changes in outcome variables of interest that are unrelated to uprisings, the post-treatment effects will capture any *trend break* in an outcome variable of interest due to racial uprisings in black communities.

The event-study estimates are summarized by using 3-year intervals for post-treatment effects:

$$(2)\ K_{it} = \alpha_i + \gamma_{u(i)t} + \delta_{s(i)t} + \sum_q \tilde{\pi}_y D_i 1(t - T_i^* \in q) + \sum_p \tilde{\phi}_y D_i 1(t - T_i^* \in p) + X'_{i,t}\beta + \varepsilon_{it}$$

where the notation remains as defined above; q indexes the group of all years more than 5 years before treatment and years -4 to -1; and p indexes each of the periods for 1 to 3, 4 to 6, and 7 to 9. This specification is less tightly connected with the timing of changes than the event study, but it has the advantage of summarizing the estimates and their joint statistical significance. We also examine heterogeneity in the scale of the treatment effects for a number of subpopulations: counties with above average African

American populations, counties with a share urban between 25%-50%, 50%-75%, or above 75%, and counties split by region (Northeast, Midwest, South, and West). To gain deep a deeper understanding of the mechanisms, we estimate additional specifications with total crime, total police officers employed, and the number of officers killed as the outcome variables of interest.

3.2 Descriptive Analysis

We draw on a number of existing data sources for this analysis. Data on racial uprisings are provided by Collins and Margo (2007) and were originally collected by Carter (1986) and Spilerman (1971). The data contain all racial uprisings from 1964-1971, including both the location and duration of uprisings and various measures of severity including arrests, deaths, injuries, and arson cases. An uprising in this dataset is defined as a demonstration involving at least 30 participants[9] that result in any property damage or violence (Carter 1986). In addition, the event has to occur outside of a school setting or an organized civil rights demonstration. Between 1964 and 1971, over 700 uprisings occurred. The occurrences of these events are summarized in Figure 1, which highlights the number of counties experiencing a racial uprising for the first time. A significant proportion of the uprising is associated with mass collective action in the "long hot summer of 1967," where violence ensued for days in Detroit and Newark and triggered uprisings in surrounding communities in Southeast Michigan and Northern New Jersey. A majority of the uprisings that occurred during our sample period happened in 1968 after the assassination of Dr. Martin Luther King Jr. These uprisings represent the most frequent and severe violent demonstrations in the 1960s. Our analysis will exploit the variation in the severity of uprisings, but the primary analysis relies on the variation in the timing of uprisings.

Information on civilian deaths due to law enforcement intervention is provided by published Vital Statistics Multiple-Cause of Deaths files (US DHHS and ICPSR 2007) from 1959 to 1988. The vital statistics data report deaths by cause, age, race, and county of residence. We use this data to create county-

[9] At least some of the participants in this uprising need to be African American to be included in the dataset. While problematic in general, this does narrow the scope of the treatment effect that we estimate.

race specific mortality rates for deaths due to law enforcement intervention.[10] The dependent variables of interest are the number of police homicides by race and the number of police homicides per 100,000 residents by race.[11] 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 1968-1988.[12] According to Figure 2, white deaths and non-white deaths due to legal intervention move in lockstep with each other until the mid to late 1970s. After 1975, white deaths due to legal intervention remain higher than non-white deaths. It is important to note that although the number of police killings of civilians is quite similar in the 1960s, non-white deaths are considerably high when compared to the percentage of the population non-white.

To create the final sample, riot and vital statistics data are merged with the US County and City Data Book consolidated files for 1944-1977 from ICPSR (1981) to provide control variables that are interacted with a linear time trend. In total, the primary dataset contains over 3,000 counties and super-counties on an annual basis for over 20 years. Of the 3,064 counties available, 272 experience at least one racial uprising during our sample period. As displayed in Table 1, there are clear cross-sectional differences between counties that experience a racial uprising and non-rioting communities. Uprising counties are typically larger and have a greater percentage of the population residing in urban areas. Counties that experience at least one uprising are also more affluent and have a smaller percentage of the population living in poverty. Uprising counties also have a larger share of non-white population. Additionally, counties that experience uprisings have more deaths due to police use of force. This is not surprising considering the fact that rioting counties are typically larger and more urban.

[10] Excludes deaths due to legal execution.

[11] For robustness we have also run specifications in which the number of civilian deaths by legal intervention are scaled by the total number of crimes in a county on an annual basis.

[12] It is important to note that Vital Statistics recording of deaths by law enforcement contain many shortcomings related to completeness and accuracy due to political pressure and heterogeneity in data collection methods as a result of the voluntary nature of ICD coding (Sherman and Langworthy, 1979; Fyfe, 2002; and Loftin et al., 2003). Despite these shortcomings, Vital Statistics remains the most consistent and complete collection of deaths by law enforcement intervention for the time period of interest and a reliable source of police homicides for regression analysis (Sherman and Langworthy, 1979). Moreover, heterogeneity in the recording of civilian deaths due to law enforcement is captured by county fixed effects assuming data collection efforts vary across counties but are time-invariant.

The primary dependent variable in our analysis is the number of deaths due to legal intervention and not a mortality rate (deaths per population). We focus on the aggregate count of deaths due to legal intervention for several reasons. First, black migration into urban areas will attenuate any estimate using mortality rates. Police killings are relatively rare events, but migration patterns to urban areas began well before our sample period and continued throughout the 1970s (Cutler, Glaeser, and Vigdor 1999). While it would be interesting to test if racial uprisings influence migration patterns, data limitation prohibits examination of year-to-year changes in population by race until after 1968.[13] Secondly, white flight from urban centers into suburban communities could exacerbate any estimated effect of police use of force against white residents. As a robustness check, we will present estimates using mortality rates by race as the dependent variable with the caveats noted above.

Although there are meaningful cross-sectional differences, our analysis accounts for these differences with fixed effects as well as the inclusion of covariates interacted with time trends. We also attempt to account for key cross-sectional differences by using a semiparametric reweighting scheme (Abadie 2005; Baily and Goodman-Bacon 2015). Using propensity scores, $p(x)$, we reweight non-rioting counties by inverse propensity scores $\frac{p(x)}{1-p(x)}$, balancing the distribution of covariates across groups (DiNardo et al 1996).[14] Table 1, column 5 reports summary statistics using the weighting scheme. Column 6 reports p-values from t-tests of the differences in demographic characteristics between uprising (unweighted) and non-rioting counties (weighted). Using the weights, the control group closely resembles the treated group. The presentation of our results will include both the fixed effects specification highlighted above as well as the reweighting scheme.

It is important to note that we do not identify a causality based on cross-sectional differences between the uprising and non-rioting counties. Rather, the identification strategy relies on how deaths due

[13] As stated earlier, the population variables are created from interpolating from 1960 census data to 1968 REIS data. See text above.

[14] Propensity scores are calculated using covariates in Table 1. Weights are rescale to sum to one for non-rioting counties and rioting counties all receive the same weight (1/N, where N is the number of rioting counties).

to legal intervention evolve *before* a county experiences their first uprising. To establish a causal relationship between the first uprising and changes in police killings of civilians, deaths due to legal intervention have to *evolve* similarly in uprising and non-rioting counties before an uprising occurs. Simply stated, the timing of the first uprising has to be exogenous to pre-existing trends in police use of deadly force. If this assumption holds, county fixed effects will account for key cross-sectional differences. State-by-year fixed effects will capture regional differences that vary over time. Lastly, urban-by-year fixed effects will capture unobserved urban dynamics that vary over time. Non-rioting counties in our analysis will capture trends in police homicides over time and provide a counterfactual for how police killings of civilians are expected to evolve in the absence of a riot. Non-rioting counties provide a plausible control group if and only if the timing of the first riot is exogenous to pre-existing trends.

If pre-existing trends in police use of force are similar in the uprising and non-rioting counties, our analysis will capture any *trend break* associated with the first uprising. We run several tests for the influence of pre-existing trends on the timing and location of a county's first uprising. Figure 3 plots the average difference in pre-period growth rates in police killings of civilians between the uprising and non-rioting counties. As it relates to both white and non-white deaths due to legal intervention, there is no clear difference in pre-period growth rates. The lack of a statistical relationship provides suggestive evidence that deaths due to police intervention were evolving similarly in the uprising and non-rioting *locations* before 1964. Figure 4 plots the pre-period growth rates for police use of deadly force against the timing of a county's first uprising (uprising counties only). As seen in Figure 4, there is no distinguishable pattern or a statistical relationship. This is strongly suggestive evidence that the *timing* of uprisings is unrelated to pre-existing trends in police killings of civilians.

We further test for pre-existing trends by regressing 1) the year of first uprising (*timing*) and 2) an indicator variable for experiencing at least one uprising (*location*) on pre-period growth rates in police killings of non-white civilians and 1960 demographic characteristics. Table 2 reports the unweighted and weighted estimates from ordinary least-squares (OLS) regressions. Columns 1 and 2 refers to the timing of the first uprising and columns 3 through 6 refers to the location of racial uprisings. Columns 2, 4, & 6

includes the change in police killings of non-white civilians between 1960 and 1963 as an independent variable. Columns 5 and 6 report weighted least squares results using the reweighting scheme. According to Table 2, there is no statistical relationship with pre-period growth rates in police homicides and the timing or location of uprisings.[15]

These four tests provide statistical evidence that pre-period growth rates in police killings of civilians did not influence the variation in the timing of a county's first uprising. As a result, the timing of the first uprising will identify a causal relationship between uprisings and deaths due to legal intervention if one exists. An additional test of pre-existing trends is embedded in our empirical strategy.

4. Results:

4.1 Primary Results

Using the estimates from Equation 1, we plot pre-treatment effects and post-treatment effects from a balanced panel. Figure 5 and Figure 6 plots estimates from the two different specifications discussed above (fixed effects vs. reweighting). Model 1 refers to the fixed effects specification and is plotted with a solid line and circle markers. Model 1 includes county, state-by-year, and urban-by-year fixed effects as well as 1960 demographic characteristics interacted with a linear time trend. Model 2 refers to the reweighting scheme. It includes county and year fixed effects but applies the reweighting scheme to non-rioting counties, and is plotted with square markers. We present 95-percent confidence intervals with dashed lines and include circle and square markers to identify models 1 & 2 respectively. The confidence intervals are constructed from heteroscedastic-robust standard errors clustered by county. Baseline estimates are presented for the dependent variable, number of deaths due to legal intervention, $K_{i,t}$ for Equation 1. All regressions are estimated using the 1960 population as weights to correct for heteroskedasticy related to county size in the error term.[16]

[15] It is important to note, that when reweighting scheme is used, many coefficients switch signs and are counter-intuitive. Although the re-weighting scheme is our preferred specification, we will present both the fixed effects and the weighted regression results.

[16] For non-white deaths, the population weights refer to the 1960 non-white population. Similarly, for the analysis of white deaths, the population weights refer to the 1960 white population. Weighted least squares is used to make error term homoscedastic. Results without population weights are available upon request.

Figure 5 plots pre-treatment and post-treatment effects for racial uprisings on the number of non-white deaths due to legal intervention. For both models, the point estimates for π are near 0 or slightly more than 0 but statistically insignificant. After the first uprising occurs, there is clearly a trend break. The number of non-white deaths due to police intervention rises sharply after the first event year and continues to rise until the third event year. Afterwards, post-treatment effects remain large and generally statistically significant. Model 2 provides a much larger estimate of the post-treatment effects relative to model 1. According to model 1, there is an additional 1.4 non-white deaths due to police intervention over the first three event years. The average treatment effect on the treated is equivalent to a 314 (1.398/0.445)[17] percent increase in non-white deaths due to legal intervention. Using estimates from model 2, the post-treatment effects over the first three event years implies a 698 (3.1096/0.445) percent increase in non-white deaths due to legal intervention.

Figure 6 plot the treatment effects for uprisings on the number of white deaths due to police intervention. Once again the pre-treatment effects are essentially 0 for both models but appear to be measured less precisely. After the first uprising occurs, the number of white deaths due to police intervention increases sharply and peaks at event year 2 in both models. Afterwards post-treatment effects decrease and revert to pre-uprising trends in model 2. According to model 1, there is a 543 (2.159/0.394) percent increase in white deaths over the first three event years. Model 2 estimates imply a 605 (2.403/0.394) percent increase in white deaths due to police intervention. According to Figure 5 and Figure 6, regardless of the model, there is an increase in the use of lethal force against whites and non-whites after an uprising occurs. However, the effects are persistent for non-whites and subside for whites. The cumulative effect over nine event years implies an additional 3.8 to 6.6 white deaths due to police intervention and an additional 9 to 15.1 non-white deaths.

Our primary results are summarized by joint treatment effects in Table 3 & 4, which presents estimates from Equation 2. Table 3 report joint treatment effects of the first racial uprising on non-white

[17] The mean number of non-white deaths due to police intervention at event year zero for uprising counties is 0.445.

deaths due to legal intervention and Table 4 report effects for white deaths due to legal intervention. Column 1 present joint treatment effects for model 1 and column 2 present joint treatment effects for model 2. Similar to Figure 5 and 6, joint pre-treatment effects in columns 1 and 2 of Table 3 and 4 are statistically insignificant. This provides additional evidence that our econometric specification accounts pre-period, time-varying, county-level heterogeneity. With regard to police killings of non-white civilians, the joint post-treatment effects are positive and statistically significant after 3 event years. The immediate joint treatment effects are only marginally statistically significant for the fixed effect model, but model 2 produces larger point estimates. As for white deaths by police intervention, joint post-treatment effects are statistically or marginally statistically significant for model 1. The reweighting scheme produces marginally statistically significant joint treatment effects for event years 1 to 3 but the remaining joint treatment effects are decreasing and statistically insignificant

The additional columns in Table 3 and 4 report results from robustness checks of our analysis. Column 3 report joint treatment effects when replacing state-by-year fixed effects with region-by-year fixed effects. We consider this approach for two reasons. First, state-by-year fixed effects are possibly capturing all the variation in the dependent variable and driving the results. Secondly, the literature on uprisings has identified only 3 correlates or predictors of racial uprising – rain, the percentage of the population black, and geographical region of the city or community. According to column 3, region-by-year fixed effects produce larger estimates of the joint treatment effects for police killings of non-white civilians relative to column 1 and 2 of Table 3. The joint post-treatment effects for white deaths are only statistically significant in event years 1 to 3, but the effects are of similar magnitude as those reported in column 1 of Table 4.

Column 4 report estimates of model 1 for a restricted sample excluding non-rioting counties.[18] Limiting the sample in this fashion shows substantially larger treatment effects for both the number of non-white and white deaths following the first uprising. In this restricted sample we see post-treatment effects rise as high as an additional 3.1 non-white deaths due to police intervention after event year 6. For white

[18] This limits us to the 272 counties across the United States that experienced at least one riot over the time period.

deaths, the fundamental difference is that the effects, while smaller than for non-whites, persist over time and are relatively large compared to the previous columns. The treated-only results highlight the importance of using non-rioting counties to capture trends in police killings of civilians.

Column 5 report joint treatment effects from an examination of the effects of uprisings on deaths per 100,000 residents due to police intervention. In this case, the results show no consistent pattern. The joint post-treatment effects are generally negative as it relates to non-white deaths by legal intervention and positive for white deaths. This is not unexpected given the biases noted earlier, including African American migration into urban areas and white flight from urban centers. However, the results are not consistent. The post-treatment effect for non-whites is positive and large after 6 event years, and the post-treatment effect for whites is negative for event years 4 to 6.

Table 5 presents our last set of robustness checks, where we include additional controls for model 2. Columns 1 through 4 present joint treatment effects for non-white deaths and columns 5 through 8 report joint treatment effects for white deaths due to police intervention. It is clear from Table 3 & 4 that population dynamics are influencing our results. Adding a measure of the yearly population as an independent variable increases the post-treatment effects. The joint treatment effects for event years 1 to 3 increases by 10 percent for non-whites although not statistically significant, and increases by 24 percent for whites and is statistically significant. Accounting for additional riots and the severity of additional riots in columns 2 and 3 does little to alter our general findings for non-white deaths, but the inclusion of these variables eliminate any statistical relationship between the initial uprising and police killings of white civilians. Lastly, according to columns 4 and 8, adding a measure of crime and police presence has little influence on our findings.

To summarize, the baseline results show that counties that experienced a racial uprising witnessed a marked increase in both non-white and white deaths due to police intervention in the years immediately following the initial uprising. However, this initial increase is somewhat larger for non-whites than for white Americans. Moreover, the groups diverge over the medium-to-long run. While non-white deaths

resulting from legal intervention remain elevated after nearly a decade, police killings of whites partially subside.

4.2 Heterogeneous Treatment Effects and Other Outcomes

We consider the role of heterogeneous treatment effects in Tables 6 and 7. These table report results from a regression that reweights observations by propensity scores based on the probability of rioting to reweight non-rioting counties. For each specification, all non-rioting counties serve as the comparison group. Column 1 shows that for non-whites, the treatment effects are not substantially changed when limiting the uprising counties to communities with an above average African American population. When stratifying uprising counties into groups based on the percentage of a county that is urban in columns 2,3, 4, the increase in the number of non-white deaths due to police intervention is observed across all concentrations of urbanicity. The effects, unsurprisingly, are the largest in the most heavily urban areas. As for regional variation in columns 5-8 of Table 6, there appears to be no consistent pattern. Joint post-treatment effects are initially larger in the Northeast and South while post-treatment effects tend to increase in the Midwest and West monotonically. Interestingly, the Northeast post-treatment effects are all statistically or marginally statistically significant while the joint post-treatment effects in the Midwest are only marginally statistically significant after event year 6.

For deaths of whites due to legal intervention, there is no statistical relationship in counties that are heavily populated with African American. The effects, however, do vary by urbanicity. Less urban or more rural counties experience a decrease in police killings of white civilians immediately after an uprising occurs, while counties that are highly urbanized experience an increase in white deaths by police intervention. Regionally, positive joint treatment effect for the first three event years are present across regions while the treatment effects only persist in event years 4 to 6 in the Northeast region and event years 7 to 9 in the West.

Although our results are robust to a number of specification checks highlighted above, there exist the possibility that our results are driven by urban decay or tough on crime policies introduced in the 1960s and 1970 (Hinton, 2016). The War on Crime was the introduced by Lyndon B Johnson and financed by

the Omnibus Crime Control and Safe Streets Act of 1968. The programs and funding allocated by the Law Enforcement Assistance Administration were dispersed to deal with crime and riots in urban communities specifically. Figure 7 and 8 present pre-treatment and post-treatment effects for the impact of uprisings on the log of total crime per 100,000 residents and the log of total police per 1,000 residents. We use crime to proxy for urban decay, and the number of police offers to capture tough on crime funding. Both Figure 7 and Figure 8 indicate that there is no statistical relationship between the first racial uprising and crime or the number of police officers. Surprisingly, uprisings appear to have little effect on either crime or police employment levels. As such, we can reject, urban decay, increased urban violence, or simply the presence of more police officers as the underlying causes of our findings. However, we cannot reject a theory of black militancy, radicalization, or embolden civilians. Figure 9 present estimates of the effects of the first uprising on the number of police officers killed on duty. Although the pre-treatment effects are jointly marginally statistically significant, we do uncover a trend break in both models. After the initial uprising, the number of police officers killed on duty gradually increase and post-treatment effects are statistically significant after five event years. It is important to note that county level crime statistics, police employment levels, and police killed on duty suffers severely from underreporting by local police agencies and inconsistent reporting due to the voluntary nature of the FBI Uniform Crime Reports data collection efforts. Results are to be interpreted with caution.

5. Conclusion

This work presents the first empirical evidence on the relationship between African American protest, in this case, the uprisings of the 1960s and early 1970s, and the subsequent police killings of civilians by race. The results clearly demonstrate that historical African American uprisings resulted in an increase in civilian deaths by legal intervention regardless of race in the short-run and a long-run increase in killings of non-white American residents over the medium-to-long run. As a lower bound on the aggregate effect, it is clear that the uprisings resulted in police killings of several thousand additional non-white American residents over the subsequent decade.

While the direction and scale of the treatment effect are clear, it is less obvious as to what exact mechanisms are driving the results. It is possible that, as a result of the uprisings, police officers felt less "safe" around non-white residents even though uprisings did not lead to an increase in total crime. The story could also be more direct in that police officers are in some sense retaliating against African American protestors for the uprisings. The effect could also be driven by changes in behavior from the public rather than the police. In particular, if citizens of color feel more empowered and push back on police more frequently, the increase in community-police confrontations could produce in the empirical pattern observed. Regardless of the mechanism, the results present a dismal picture. The uprisings of the 1960s were a response to often unjustified police violence targeted towards the African American community and, instead of addressing that valid concern, these protests appear to have only resulted in an increase in the deaths of non-white residents at the hands of law enforcement.

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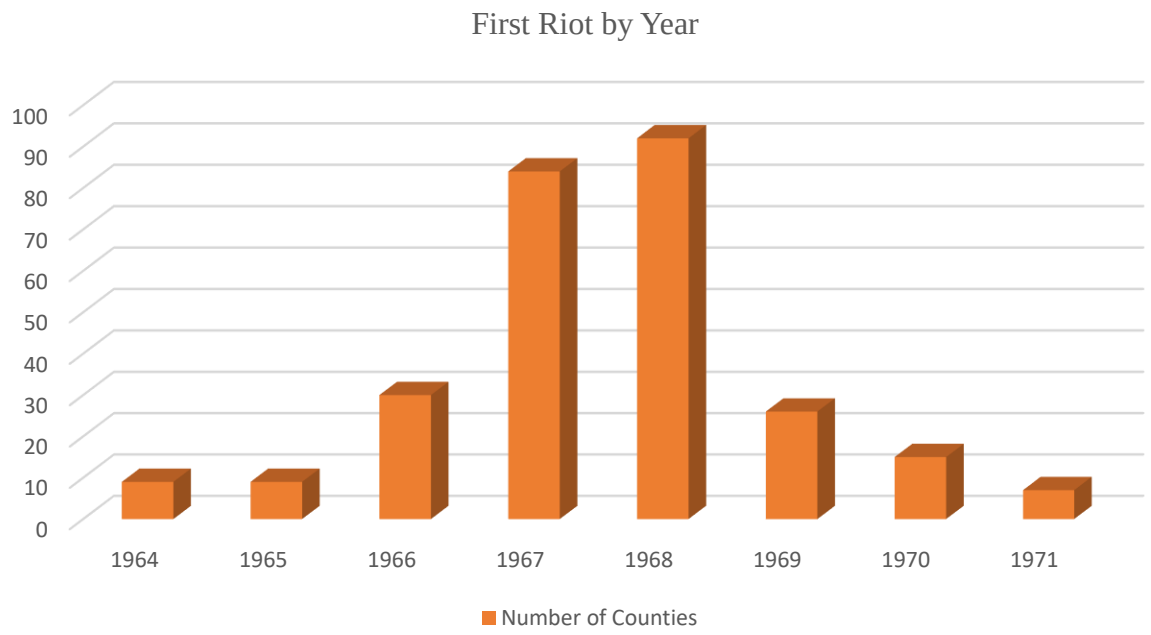
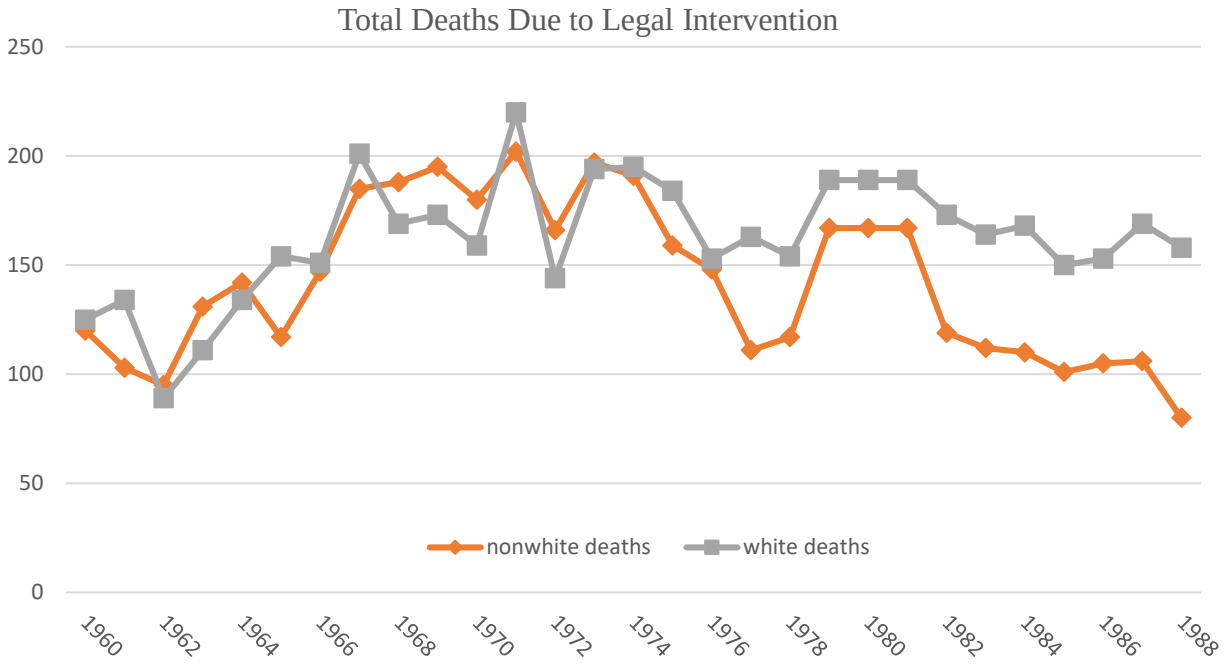
Figure 1. County-Level First Riot Occurrence by Year



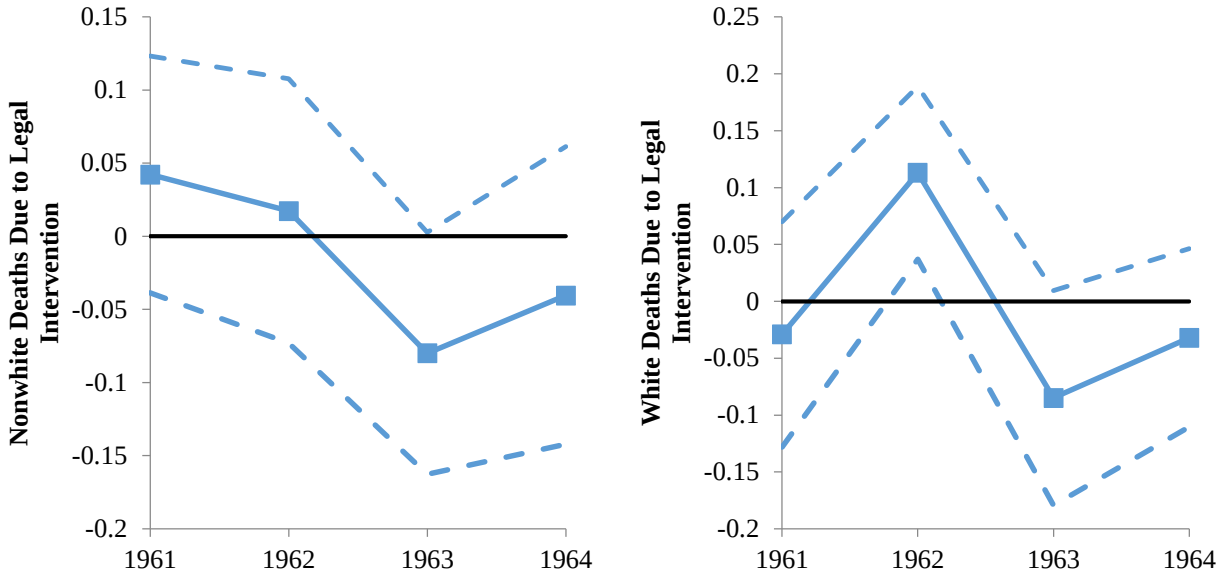
Source: Riot data comes courtesy of Carter and Margo (2007). Carter (1986) original source of race riots from 1964 to 1971.

Figure 2. Annual Civilian Deaths Resulting from Legal Intervention



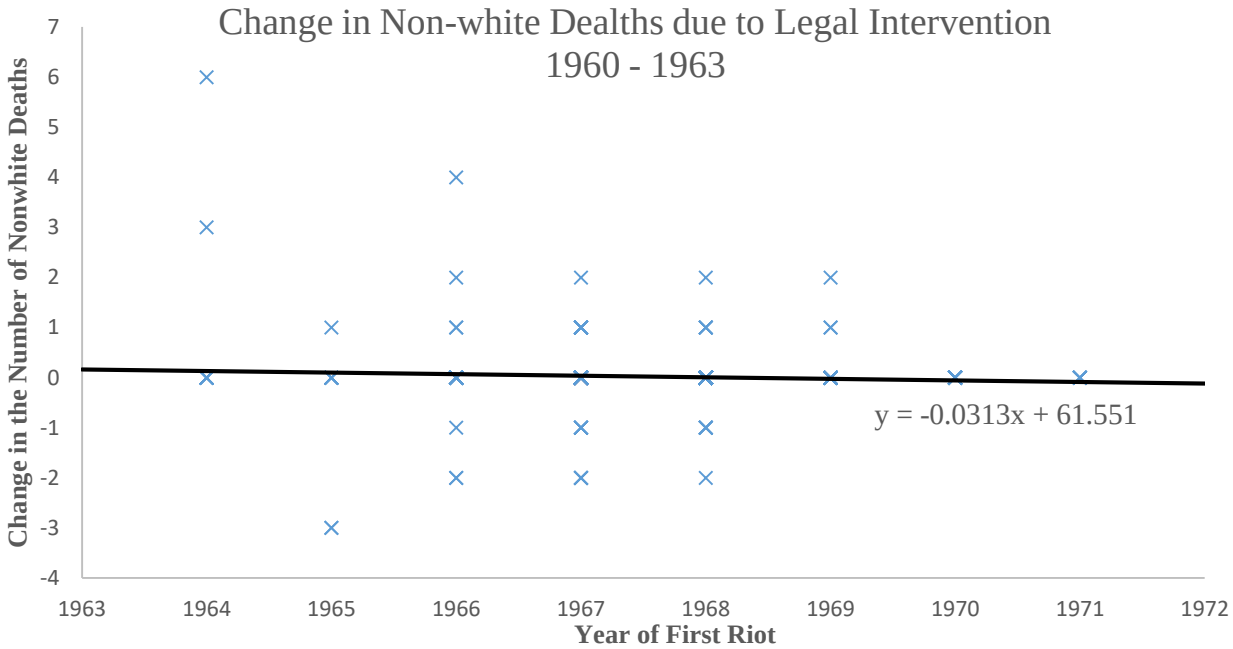
Source: Vital Statistics Multiple-Cause of Death Files (US DHHS and ICPSR 2007). The vertical axis corresponds to the total number of deaths due to legal intervention by race.

Figure 3. The Average Pre-Trend Difference in Civilian Deaths by Legal Intervention, Rioting vs. Non-Rioting Counties – Evidence of Location



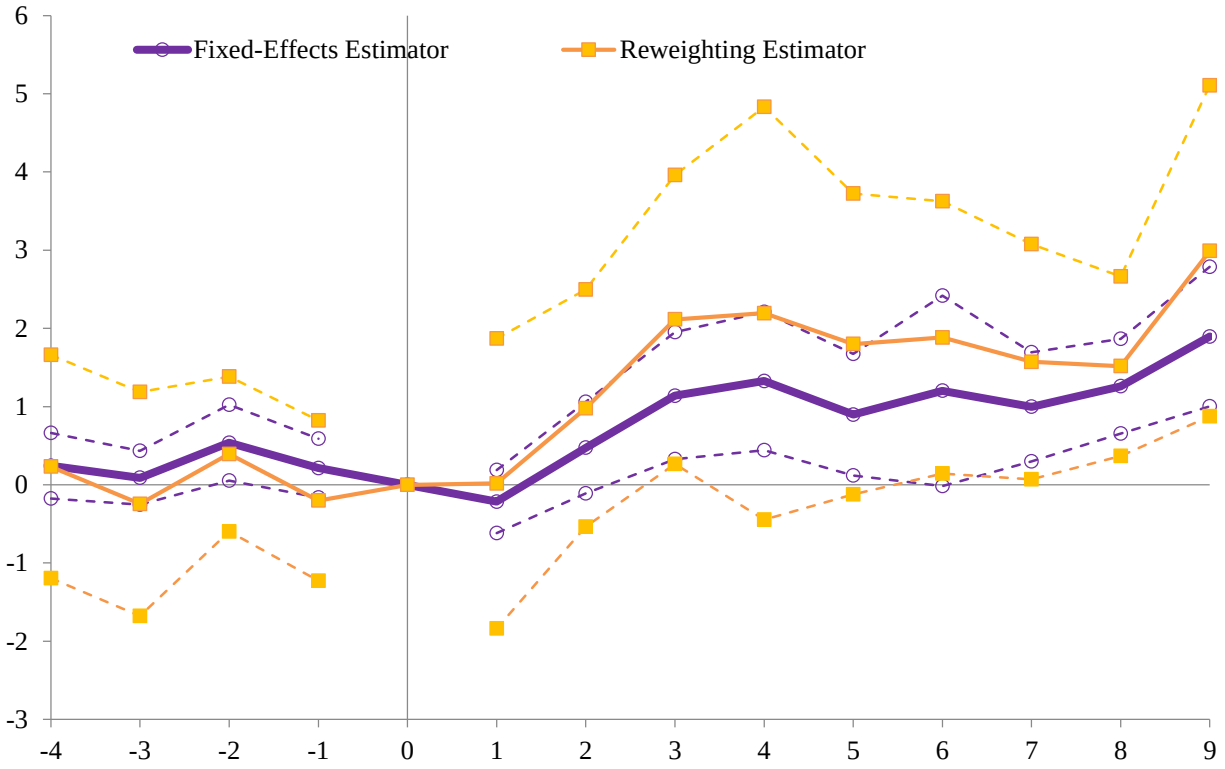
Notes: Figure 3 plots the average difference in pre-period growth rates in police killings of civilians between rioting and non-rioting counties.

Figure 4. Pre-Trend Relationship between Date of First Riot and Deaths by Legal Intervention – Evidence of Timing



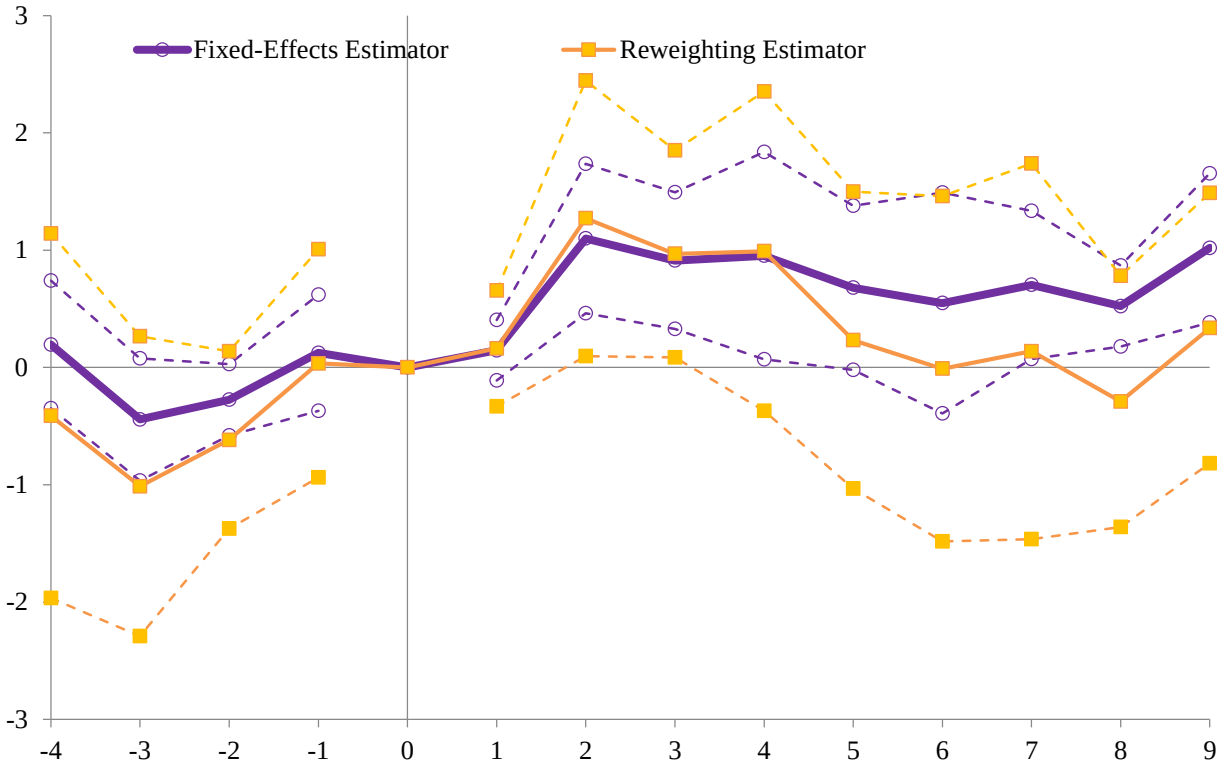
Notes: Regression coefficients and predicted values are from univariate regressions of the dependent variable Δ in non-white deaths due to legal intervention on the year the first racial uprising occurred in a county. The slope in Figure 4 is -0.0313 (0.0535).

Figure 5. Estimates of Effects of First Riot on Non-White Deaths by Legal Intervention



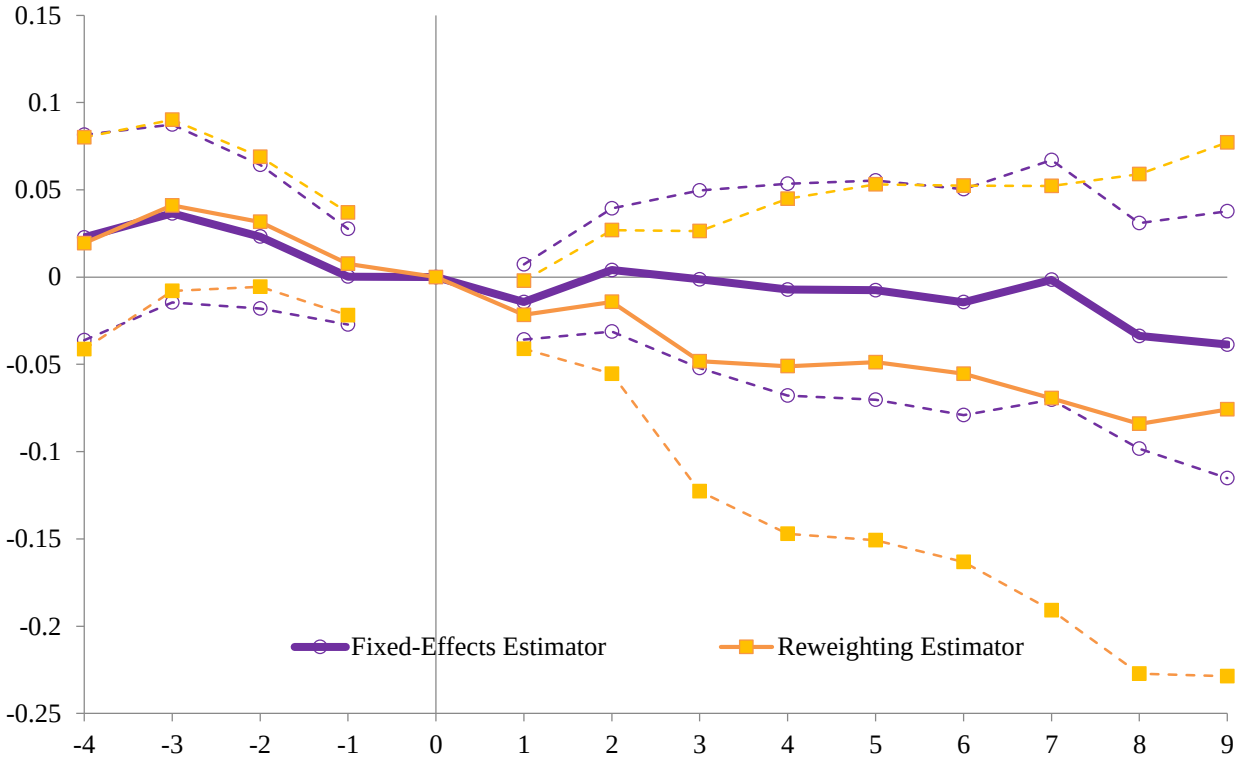
Notes: Figure display weighted least-squares estimates obtained from estimating by Equation 1. The dependent variable is the number of non-white deaths due to legal intervention. Model 1 includes county, C, effects, urban-by-year, U, effects, state-by-year, S, effects and covariates, X, from the 1960 census interacted with a time trend and is plotted with a solid line and circle markers. Model 2 includes county, C, effects, year, Y, effects but reweight observations by propensity scores based on the probability of rioting to reweight non-rioting counties. Heteroskedasticity-robust standard errors clustered by county are presented by dashed lines and circle markers for model 1 and square markers for model 2. Both models use 1960 population by race as weights and use non rioting counties as the comparison group.

Figure 6. Estimates of Effects of First Riot on White Deaths by Legal Intervention



Notes: Figure display weighted least-squares estimates obtained from estimating by Equation 1. The dependent variable is the number of white deaths due to legal intervention. Model 1 includes county, C, effects, urban-by-year, U, effects, state-by-year, S, effects and covariates, X, from the 1960 census interacted with a time trend and is plotted with a solid line and circle markers. Model 2 includes county, C, effects, year, Y, effects but reweight observations by propensity scores based on the probability of rioting to reweight non-rioting counties. Heteroskedasticity-robust standard errors clustered by county are presented by dashed lines and circle markers for model 1 and square markers for model 2. Both models use 1960 population by race as weights and use non rioting counties as the comparison group.

Figure 7. Estimates of Effects of First Riot on the Log of Total Crime Per 100,000 Residents

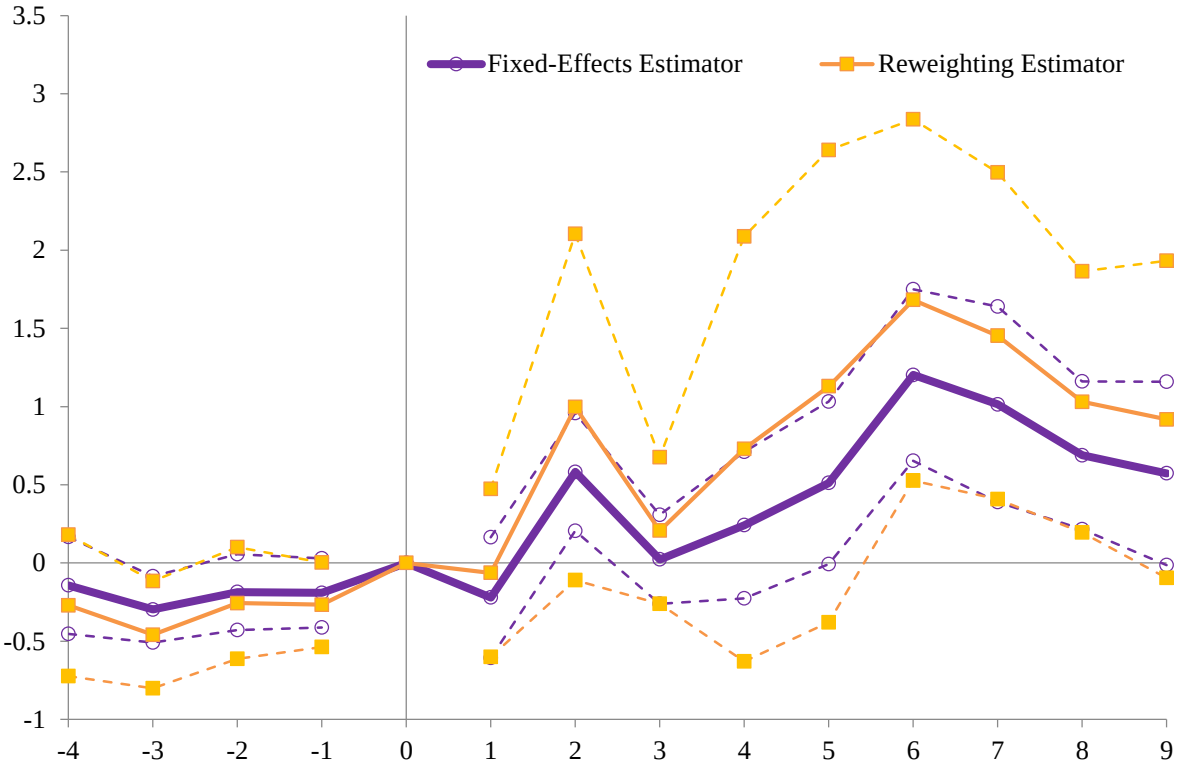


Notes: Figure display weighted least-squares estimates obtained from estimating by Equation 1. The dependent variable is the log of total crime per 100,000 residents. Model 1 includes county, C, effects, urban-by-year, U, effects, state-by-year, S, effects and covariates, X, from the 1960 census interacted with a time trend and is plotted with a solid line and circle markers. Model 2 includes county, C, effects, year, Y, effects but reweight observations by propensity scores based on the probability of rioting to reweight non-rioting counties. Heteroskedasticity-robust standard errors clustered by county are presented by dashed lines and circle markers for model 1 and square markers for model 2. Both models use 1960 population by race as weights and use non rioting counties as the comparison group.

Figure 8. Estimates of Effects of First Riot on the Log of Sworn Police Per 1,000 Residents

Notes: Figure display weighted least-squares estimates obtained from estimating by Equation 1. The dependent variable is the log of sworn police officers per 1,000 residents. Model 1 includes county, C, effects, urban-by-year, U, effects, state-by-year, S, effects and covariates, X, from the 1960 census interacted with a time trend and is plotted with a solid line and circle markers. Model 2 includes county, C, effects, year, Y, effects but reweight observations by propensity scores based on the probability of rioting to reweight non-rioting counties. Heteroskedasticity-robust standard errors clustered by county are presented by dashed lines and circle markers for model 1 and square markers for model 2. Both models use 1960 population by race as weights and use non rioting counties as the comparison group.

Figure 9. Estimates of Effects of First Riot on the Sworn Police Killed on Duty



Notes: Figure display weighted least-squares estimates obtained from estimating by Equation 1. The dependent variable is the number of law enforcement officers killed on duty. Model 1 includes county, C, effects, urban-by-year, U, effects, state-by-year, S, effects and covariates, X, from the 1960 census interacted with a time trend and is plotted with a solid line and circle markers. Model 2 includes county, C, effects, year, Y, effects but reweight observations by propensity scores based on the probability of rioting to reweight non-rioting counties. Heteroskedasticity-robust standard errors clustered by county are presented by dashed lines and circle markers for model 1 and square markers for model 2. Both models use 1960 population by race as weights and use non rioting counties as the comparison group.

Table 1. County Characteristics and Balance

1960 County Characteristics	(1) All Counties (N=3064)	(2) Rioting Counties (N=272)	(3) Non-Rioting Counties (N=2792)	(4) T-Test of Difference (2) – (3)	(5) Reweight Non-Rioting Counties	(6) T-Test of Difference (2) – (5)
Population	57,986	342,799	30,239	*<0.01*	140,960	0.587
Percentage of the population						
in urban areas	31.7	68.3	28.1	*<0.01*	55.5	0.628
with 12 years of education	36.5	40.3	36.1	*0.54*	32.9	0.568
income greater than $10,000	7.9	14.2	7.3	*<0.01*	11.0	0.53
income less than $3,000	35.6	23.1	36.9	*<0.01*	27.7	0.625
nonwhite	10.7	16.5	10.1	*<0.01*	28.6	0.438
Deaths due to Legal Intervention						
white	0.04	0.27	0.02	*<0.01*	0.32	0.935
nonwhite	0.04	0.31	0.01	*<0.01*	0.37	0.924
total	0.08	0.58	0.03	*<0.01*	0.69	0.922

Source: Table displays averages from the 1960 Decennial Census. Census data from 1962 County and City Data Book publicly available at the ICPSR. Riot data comes courtesy of Carter and Margo (2007). Carter (1986) original source of race riots data from 1964 to 1971. Column 5 applies the semi-parametric reweighting. Column 9 *p*-values are based on a parametric percentile-t bootstrap procedure with 1,000 replications (Jeong and Maddala 1993; Horowitz 2001; Bailey and Goodman-Bacon 2015).

Table 2. Determinants of Location Rioting & Timing of First Riot

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	Year of First Riot		0/1 Ever Experience A Riot			
Percentage of the population						
in urban areas	-0.00800	-0.00764	0.00254***	0.00253***	0.00113	0.00111
	[0.00488]	[0.00480]	[0.000220]	[0.000219]	[0.00181]	[0.00181]
with 12 years of education	0.0223	0.0238	-5.74e-05***	-5.74e-05***	0.00921	0.00927
	[0.0164]	[0.0164]	[1.07e-05]	[1.07e-05]	[0.00704]	[0.00693]
income greater than $10,000	-0.0399	-0.0451*	0.0188***	0.0189***	0.0114*	0.0119*
	[0.0249]	[0.0254]	[0.00206]	[0.00207]	[0.00681]	[0.00681]
income less than $3,000	0.0218	0.0216	0.00155***	0.00155***	0.00867**	0.00855**
	[0.0186]	[0.0187]	[0.000584]	[0.000585]	[0.00338]	[0.00338]
nonwhite	-0.00497	-0.00516	0.00323***	0.00322***	-0.00679***	-0.00678***
	[0.0112]	[0.0115]	[0.000342]	[0.000342]	[0.00149]	[0.00150]
male between 15 and 24	5.710	5.413	-0.395	-0.383	3.094**	3.105**
	[3.911]	[3.926]	[0.247]	[0.247]	[1.519]	[1.510]
male between 25 and 39	4.729	4.377	1.291***	1.285***	-2.123	-2.150
	[9.099]	[9.108]	[0.442]	[0.442]	[2.421]	[2.429]
northeast	-0.179	-0.157	0.0439*	0.0432	0.0491	0.0373
	[0.308]	[0.304]	[0.0264]	[0.0264]	[0.0712]	[0.0691]
midwest	-0.281	-0.289	0.0133	0.0133	-0.178	-0.182*
	[0.236]	[0.237]	[0.0129]	[0.0129]	[0.111]	[0.109]
west	-0.661**	-0.657**	-0.0580***	-0.0585***	-0.204**	-0.214**
	[0.307]	[0.307]	[0.0178]	[0.0179]	[0.103]	[0.0992]
Deaths Due to Legal Intervention						
nonwhite		-0.168		0.0252		0.0542
		[0.125]		[0.0285]		[0.0475]
Weights					P-weights	P-Weights
Observations	272	272	3,064	3,064	3,063	3,063
R-squared	0.202	0.210	0.254	0.254	0.189	0.194

Note: Each column reports estimates from separate ordinary-least-squares regressions. The dependent variable in columns 1 and 2 is an indicator equal to 1 if a county ever experiences a racial uprising. The dependent variable in columns 3 through 6 is the year a county experience the initial uprising. All columns include state fixed effects. Heteroskedasticity-robust standard errors are corrected for clustering with state and presented in brackets. County demographic variables are from the 1960 Decennial Census. Columns 5 and 6 reweights observations by propensity scores based on the probability of rioting to reweight non-rioting counties. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Joint Effects of First Riot on Non-white Deaths by Legal Intervention

	(1)	(2)	(3)	(4)	(5)
		DV: non-white deaths			*Per 100K*
				Treated Only	
Years -4 to -1	0.260	0.0352	0.538	0.357	0.971
	[0.171]	[0.449]	[0.349]	[0.221]	[0.768]
Years 1 to 3	0.399*	0.983	0.986	0.628**	-0.485
	[0.222]	[0.685]	[0.604]	[0.271]	[0.485]
Years 4 to 6	1.048***	1.865**	2.061**	1.969***	-0.0231
	[0.391]	[0.917]	[1.005]	[0.465]	[0.450]
Years 7 to 9	1.286***	1.931***	2.284***	3.079***	1.258
	[0.289]	[0.554]	[0.839]	[0.675]	[0.773]
Covariates	S, U, & X	Y, P	R, U, & X	S, U, & X	Y, P
Observations	87,783	87,754	87,783	7,888	87,282
R-squared	0.710	0.163	0.366	0.767	0.144
Number of Counties	3,027	3,026	3,027	272	3,026

Notes: Table display weighted least-squares estimates obtained from estimating by grouping years before and after treatment. The dependent variable is the number of non-white deaths due to legal intervention. All columns include county, C, effects. Columns 1 & 4 include urban-by-year, U, effects, state-by-year, S, effects and covariates, X, from the 1960 census interacted with a time trend. Column 3 replaces state-by-year fixed effects with region-by-year, R, fixed effects. Columns 2 &5 reweights observations by propensity scores based on the probability of rioting to reweight non-rioting counties and include year, Y, fixed effects. Heteroskedasticity-robust standard errors clustered by county are presented beneath each estimate in brackets. All columns use 1960 population by race as weights. All columns used non rioting counties as the comparison group.

Table 4: Joint Effects of First Riot on White Deaths by Legal Intervention

	(1)	(2)	(3)	(4)	(5)
		DV: white deaths			*Per 100K*
				Treated Only	
Years -4 to -1	-0.0914	-0.436	-0.0873	-0.212	-0.0235
	[0.200]	[0.478]	[0.350]	[0.378]	[0.0478]
Years 1 to 3	0.670***	0.745*	0.765**	0.901**	0.00499
	[0.222]	[0.393]	[0.349]	[0.351]	[0.0527]
Years 4 to 6	0.680*	0.393	0.641	1.071	-0.0432
	[0.393]	[0.616]	[0.474]	[0.749]	[0.128]
Years 7 to 9	0.728***	0.116	0.670	1.095*	0.0787
	[0.258]	[0.580]	[0.438]	[0.560]	[0.0730]
Covariates	S, U, & X	Y, P	R, U, & X	S, U, & X	Y, P
Observations	88,856	88,827	88,856	7,888	88,827
R-squared	0.465	0.090	0.286	0.561	0.036
Number of Counties	3,064	3,063	3,064	272	3,063

Notes: Table display weighted least-squares estimates obtained from estimating by grouping years before and after treatment. The dependent variable is the number of white deaths due to legal intervention. All columns include county, C, effects. Columns 1 & 4 include urban-by-year, U, effects, state-by-year, S, effects and covariates, X, from the 1960 census interacted with a time trend. Column 3 replaces state-by-year fixed effects with region-by-year, R, fixed effects. Columns 2 &5 reweights observations by propensity scores based on the probability of rioting to reweight non-rioting counties and include year, Y, fixed effects. Heteroskedasticity-robust standard errors clustered by county are presented beneath each estimate in brackets. All columns use 1960 population by race as weights. All columns used non rioting counties as the comparison group.

Table 5. Robustness Check: Event Study Regressions of Deaths by Legal Intervention (Additional Controls)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	DV: non-white deaths				*DV: white deaths*			
	Population Control	Additional Riots	Additional Riots Interacted with Severity	Total Crime & Police Control	Population Control	Additional Riots	Additional Riots Interacted with Severity	Total Crime & Police Control
Years -4 to -1	0.00852	0.628	0.0385	0.0954	-0.461	0.0599	0.0833	-0.455
	[0.424]	[0.431]	[0.433]	[0.420]	[0.501]	[0.385]	[0.355]	[0.489]
Years 1 to 3	1.083	0.483	0.985	0.984	0.923**	0.344	1.044	0.748*
	[0.733]	[0.577]	[0.702]	[0.694]	[0.426]	[0.319]	[0.720]	[0.392]
Years 4 to 6	2.130**	1.778**	1.868*	1.774*	0.884	0.389	0.804	0.414
	[1.057]	[0.892]	[0.962]	[0.953]	[0.624]	[0.622]	[0.982]	[0.627]
Years 7 to 9	2.273***	2.177***	1.934***	1.840***	0.754	0.331	0.566	0.130
	[0.620]	[0.580]	[0.549]	[0.510]	[0.717]	[0.485]	[0.471]	[0.584]
Covariates	Y, P	Y, P	Y, P	Y, P	Y, P	Y, P	Y, P	Y, P
Observations	87,754	87,754	87,754	87,638	88,827	88,827	88,827	88,711
R-squared	0.170	0.183	0.163	0.168	0.137	0.114	0.110	0.091
Number of Counties	3,026	3,026	3,026	3,022	3,063	3,063	3,063	3,059

Notes: Table display least-squares estimates obtained from estimating by grouping years before and after treatment. The dependent variable is the number of non-white deaths due to legal intervention in columns 1 - 4 and white deaths due to legal intervention in columns 5 - 8. All columns include county, C, effects and year, Y, effects. All columns report results from a regression that reweights observations by propensity scores, P, based on the probability of rioting to reweight non-rioting counties. Heteroskedasticity-robust standard errors clustered by county are presented beneath each estimate in brackets. All columns use 1960 population as weights. Columns 1 through 8 add various controls as independent variables as a robustness check.

Table 6. Heterogeneous Treatment Effects – Non-White Residents

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				DV: non-white deaths				
	% Black Above Average	*% Urban 25-50*	*% Urban 50-75*	*% Urban Above 75*	*Northeast*	*Midwest*	*South*	*West*
Years -4 to -1	0.389	0.806**	0.934**	0.00359	0.765**	-0.454	-0.250	0.00611
	[0.590]	[0.358]	[0.406]	[0.473]	[0.327]	[0.299]	[0.759]	[0.486]
Years 1 to 3	0.966	0.0756	0.381*	0.992	1.635*	0.344	1.260	0.772
	[1.138]	[0.179]	[0.225]	[0.706]	[0.855]	[0.625]	[0.923]	[0.708]
Years 4 to 6	1.556	0.697**	1.122**	1.866**	2.671**	1.132	2.232*	1.501*
	[1.296]	[0.353]	[0.437]	[0.936]	[1.182]	[0.909]	[1.193]	[0.904]
Years 7 to 9	1.850***	1.496**	1.923***	1.864***	1.631***	1.672*	1.875**	2.220***
	[0.679]	[0.597]	[0.721]	[0.568]	[0.623]	[0.996]	[0.915]	[0.558]
Covariates	Y, P	Y, P	Y, P	Y, P	Y, P	Y, P	Y, P	Y, P
Observations	82,853	80,649	81,055	85,782	81,606	57,942	47,647	76,067
R-squared	0.152	0.526	0.517	0.169	0.160	0.141	0.250	0.171
Number of Counties	2,857	2,781	2,795	2,958	2,814	1,998	1,643	2,623

Notes: Table display least-squares estimates obtained from estimating by grouping years before and after treatment. The dependent variable is the number of white deaths due to legal intervention. All columns include county, C, effects and year, Y, effects. All columns report results from a regression that reweights observations by propensity scores based on the probability of rioting to reweight non-rioting counties. Heteroskedasticity-robust standard errors clustered by county are presented beneath each estimate in brackets. All columns use 1960 population as weights. All columns used non rioting counties as the comparison group. Columns 1 refers to treated counties with the black population larger than the sample average; columns 2 through 4 refers to treated counties with 1960 urbanization between the sample percentile upper an lower bound relative to untreated counties. Columns 5 through 8 removes one region at a time.

Table 7. Heterogeneous Joint Treatment Effects –White Residents

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	DV: white deaths							
	% Black Above Average	*% Urban 25-50*	*% Urban 50-75*	*% Urban Above 75*	*Northeast*	*Midwest*	*South*	*West*
Years -4 to -1	0.253	-0.0569	-0.0762	-0.438	-0.614	-0.706	-0.560	0.0720
	[0.528]	[0.0608]	[0.107]	[0.491]	[0.716]	[0.612]	[0.544]	[0.232]
Years 1 to 3	0.940	-0.219**	-0.182	0.754*	0.790	0.810***	0.809*	0.654
	[1.056]	[0.107]	[0.128]	[0.395]	[0.580]	[0.309]	[0.449]	[0.466]
Years 4 to 6	0.322	-0.343	-0.283	0.374	0.918	0.591	0.410	-0.0688
	[0.784]	[0.224]	[0.236]	[0.619]	[0.780]	[0.736]	[0.754]	[0.493]
Years 7 to 9	0.0410	0.0244	0.110	0.0809	-0.424	0.183	0.101	0.639*
	[0.223]	[0.0751]	[0.107]	[0.605]	[0.718]	[0.646]	[0.729]	[0.383]
Covariates	Y, P	Y, P	Y, P	Y, P	Y, P	Y, P	Y, P	Y, P
Observations	83,926	81,722	82,128	86,855	82,650	58,319	48,575	76,937
R-squared	0.121	0.166	0.157	0.092	0.110	0.115	0.107	0.105
Number of Counties	2,894	2,818	2,832	2,995	2,850	2,011	1,675	2,653

Notes: Table display least-squares estimates obtained from estimating by grouping years before and after treatment. The dependent variable is the number of non-white deaths due to legal intervention. All columns include county, C, effects and year, Y, effects. All columns report results from a regression that reweights observations by propensity scores based on the probability of rioting to reweight non-rioting counties. Heteroskedasticity-robust standard errors clustered by county are presented beneath each estimate in brackets. All columns use 1960 population as weights. All columns used non rioting counties as the comparison group. Columns 1 refers to treated counties with the black population larger than the sample average; columns 2 through 4 refers to treated counties with 1960 urbanization between the sample percentile upper an lower bound relative to untreated counties. Columns 5 through 8 removes one region at a time.