# Don't Shoot! The Impact of Historical African American Protest on Police Killings of Civilians

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July 26, 2018

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**Abstract:**

The African American community has a long-standing history of protesting excessive use of force by law enforcement. 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 racial uprisings in the 1960s have had on civilian deaths due to law enforcement activity (legal intervention). An event study approach reveals racial uprisings during the 1960s and 70s resulted in an increase in civilian deaths at the hands of police. In the three years following the initial violent protest, there was 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 persist. 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.

**Keywords**

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

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# 1. Introduction:

Multiple *justifiable* police homicides in 2013 and 2014 sparked a national social movement known as #BlackLivesMatter. Police homicides in Cleveland, New York, and Ferguson, Missouri led to a series of protests that spontaneously occurred in urban cities across the United States. At the forefront of these civil demonstrations was the contentious relationship between police and black communities. Tensions between African American communities, police, and the justice system created a volatile environment, sparking uprisings in Ferguson, Baltimore, and many other American cities. The struggle for the fair treatment of the African American population by law enforcement is not a new phenomenon. In 1930s the National Association for the Advancement of Colored People's (NAACP) became active in highlighting police brutality and incorporated police-community relations as a core part of the organization's mission (Santain, 2013). Moreover, the Civil Rights Movement of the 1950s and 1960s had police brutality as a major 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 these protests attracted in the post-Rodney King era, it remains an unanswered question as to whether protest events 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 these 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.

It would be ideal to address this question using data from the current wave of Black Lives Matter protest events; [1] however, it is impossible to determine any long-term protest impact since these events are fairly recent. 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

[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.

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 of all riots compiled by Carter (1986) and Spilerman (1971), we look to determine the impact of the occurrence of various county's first riot and the number of civilians killed by police as derived via the Vital Statistics Multiple-Cause of Death files (US DHHS and ICPSR 2007) from 1959 to 1988. Additionally, we seek to analyze the impact these uprisings had on the number of civilian deaths. With the available data, we will chart 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 primarily focus on the number of police homicides and the number of police homicides per 100,000 by race, we also 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 that the African American protests in the 1960s and early 1970s resulted in an immediate increase in police killings of civilians across racial groupings. 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 11 African Americans are killed by the police in the affected county. Furthermore, uprisings appear to have little impact on police employment levels or crime rates. However, more officers are killed in the line of duty, providing evidence that riots escalate tensions in police-civilian interactions. These results paint a depressing picture in which uprisings represent a structural change in police-civilian relations adversely affecting non-white civilians both in 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 1946 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,[2] 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 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. Martin Luther King Jr. 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. The geographical variation in the timing a county's first uprising is displayed in Figure 1.

A vast majority of evidence indicates that the uprisings were not planned occurrences (Spilerman 1970; Sears and McConahay 1973; Spilerman 1976; Carter 1986; Collins and Margo 2007), but were rather spontaneous protests that resulted from interactions between the police and the African American community (Bauman, 2008).[3] Prominent uprisings such as the Watts Riot and the Detroit Riot of 1967 (Singer et al., 1970) followed this pattern.[4] We know from work by Collins and Smith (2007) and Collins and Margo (2007) that these uprisings were enormously destructive

[2] 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.

[3] 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 favor 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.

[4] 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.

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 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 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) considers 170 cities across the United States and showed racial income inequality is a major determinant of police killings as are factors of interpersonal 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) examined a smaller sample of cities and discovered police killings of civilians may be playing a "deterrence" role since they decrease the incidence of non-violent crime. Fryer (2016) highlighted 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, given its limited data sample for the analysis of officer-involved shootings, it remains an open question as to whether the results are generalizable to other regions of the United States or time-periods.

There is also rich literature regarding the interaction between police killings or misconduct and the use of new technologies for policing. Pang and Valou (2016) found that 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 et al. (2014) who find evidence that the wearing of body cameras substantially reduces complaints filed against the police. Alpert and MacDonald (2001) used data from 256 individual agencies, and discovered that requiring a third party to fill out use of force forms causes a decline in use-of-force rates.

### 2.3 Expected Effects

According to economic theory, institutional or technological factors that increase the potential risk to police officers should result in more civilian deaths due to legal intervention. Moreover, factors that increase the cost of the use of force to police officers should discourage its use. Theoretically, however, it is unclear as to whether African American protests, peaceful or otherwise, will impose consequential costs to police who engage in the use of force which potentially outweigh any risks, real or otherwise, perceived by police. Nonetheless, we expect uprising to have direct effects—via lowering the cost of using lethal force or increasing the potential risk to police officers and indirect effects—changes in community and institutional behavior in response to racial uprisings. An uprising that involves violence, destroying of property, and civilian clashes with the police present inherent risk to policing. Fryer (2016) presented a model of police-community interactions in which lowering the cost of lethal force will increase the likelihood of its use. In addition, violent protests may also change police priors concerning the likelihood civilian compliance during civilian-police interactions. As a result, lower priors will increase the use of lethal force, regardless of the race of the civilian. Relatedly, an uprising may increase the cost of compliance on black civilians, especially if police-community relationships deteriorate. Desmond et al. (2016) highlight how police violence reduces civic engagement and spreads legal cynicism within the black community. Mistrust of public officials and the police can increase the cost of compliance, increasing the likelihood that lethal force is used during civilian-police encounters.

Conversely, changes in community or institutional behavior can increase or decrease use of lethal force. For instance, uprisings may foreshadow changes in civic engagement or political dynamics within a city. Civically engaged citizens can increase voter participation and change policies by electing public officials more sympathetic to their concerns. Many cities elected the first African American mayor after 1960s uprisings.[5] However, embolden citizens can engage in confrontational

[5] Political power gain by African American in large cities is primarily a result of black migration into urban areas and white flight to the suburbs. Both black migration and white flight began before the 1960s but racial uprisings may have accelerated white flight.

interactions with the police, increasing the use of lethal force. Also, uprising may lead to tough on crime strategies to deter civil disorders such as increasing surveillance, hiring additional police officers, or enforcing stricter applications of laws already on the books (Hinton 2016). Also, racial uprisings have been linked to government initiatives providing resources, aid, and funding to riot prone communities as well as the implementation of policies and initiatives to improve police-civilian interactions (Gillezeau, 2015; Cunningham, 2018; Rahtz, 2016). Given the direct and indirect effects, the expected impact of uprisings on civilian deaths due to legal intervention is *ambiguous*.

# 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 occurrences, 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 direct a narrow focus 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 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. In addition, we used state-by-year fixed effects to account for potential unobservable changes in state policy over time or riot contagion effects. We also interacted various point-in-time county demographics with time.

[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.

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 $t$, $\alpha$ is a set of county fixed effects, $\gamma$ us a set of urban status-by-year fixed effects, $\delta$ is set of year or state-by-year fixed effects, and $\varepsilon_{it}$ 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 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 estimates are summarized by using 3-year intervals for post-treatment effects:

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

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

[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, $0 < u < 25$, $25 \quad u < 50$, $50 \quad u < 75$, $75 \quad u \quad 100$. This captures the differential utilization of police resources and changes in use of force with varying degrees of urbanization.

examined 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

Data on racial uprisings were provided by Collins and Margo (2007) and were originally collected by Carter (1986) and Spilerman (1971). The data contained 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 2, 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-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. To calculate the proportion of the population by race, we interpolate the 1960 Census county population to 1968 and use annual county population profiles

[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.
[10] Excludes deaths due to legal execution.

from the Surveillance, Epidemiology, and End Results (SEER) from 1968-1988.[11] Figure 3, plots civilian deaths due to legal intervention by race over time. White civilian deaths are identified with the circle marker, and non-white deaths are identified with the square marker. According to Figure 3, 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.[12]

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 a surprise considering the fact that rioting counties are typically larger and more urban.

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).[13] 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

[11] 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.

[12] It is important to note that although the number of police killings of civilians is quite similar in the 1960s, non-white deaths per non-white population is considerably higher than white deaths per white population.

[13] 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).

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 causality based on cross-sectional differences between the uprising and non-rioting counties. Rather, the identification strategy relies on how deaths due 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 4 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 5 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 tested 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 and 4 refers to the location of racial

uprisings. Columns 2 & 4 includes the change in police killings of non-white civilians between 1960 and 1963 as an independent variable.

As it relates to the timing of the first riot, income greater than $10,000 and the western region are the only predictors of timing. Counties that are more affluent have uprising earlier as well as counties located in the west. As shown in Figure 4 and 5, pre-period growth rates in police homicides do not influence the timing of the first uprising. According to columns 3 & 4, urban areas and income inequality are major determinants of rioting. In addition, counties with a greater percentage of the population non-whiter or between the ages of 25 and 39 are more likely to experience an racial uprising while counties with a larger percentage of the population with more than 12 years of education are less likely to experience an uprising. Once again, pre-period growth rates in police homicides are not correlated with the probability of experiencing a racial uprising.[14] The results in Table 2 further show that there is no statistical relationship between pre-period growth rates in police homicides and the timing or location of uprisings.[15]

These four tests provide statistical evidence that the variation in the timing of a county's first uprising is not influenced by pre-period growth rates in police killings of non-white civilians. 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 6 and Figure 7 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-

[14] Our inability to link riots to growth rates in police killings of nonwhite civilians is contrary to Williamson et al (2018) finding that BLM protest activity is correlated with previous killings of African Americans. This could be a matter of methodology or the fact that BLM protest are starkly different from the 1960s uprisings.

[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.

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]

Figure 6 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 $\pi$ are near zero or slightly more than zero and statistically insignificant. According to the pre-treatment effects, non-white deaths due to legal intervention in rioting counties are evolving in counties similar to non-rioting counties. This provides further statistical evidence that the timing of a county's first uprising is unrelated to pre-period growth rates in police killings of non-white civilians. This also provides additional support to the notion that conditional on a few observables, riots are spontaneous events (Spilerman 1970; Spilerman 1971). Collins and Margo (2007) highlight the idiosyncrasies of 1960 uprisings. Detailing how routine event escalate into wide-spread riots due to unforeseen exchanges or interpretations of exchanges between the police and the black community.

Since each specification captures pre-period trends in police killings of non-white civilians, the trend break that occurs after event year 0 can be attributed to the first riot. This is displayed by the post-treatment effects, $\phi_y$, which shows a statistically significant increase in non-white deaths due to legal intervention. The number of non-white deaths rises sharply after the first event year and continues to rise until the third event year in both specifications. 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. The reweighting estimator provides much larger estimates of the post-treatment effects relative to the fixed effects estimator. 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. Afterwards, post-treatment effects remain large and statistically significant.

Figure 7 plots the treatment effects for uprisings on the number of white deaths due to police intervention. Once again, the pre-treatment effects are essentially zero for both models but appear

[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.

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

to be measured less precisely. This is not surprising because racial uprisings in the 1960s were typically precipitated by clashes between the black community and the police, unlike previous riots which were inter-racial conflicts usually involving violence directed against blacks by white civilians (Lieberson and Silverman 1965). The econometric specification captures pre-existing trends in police killings of white civilians, implying a causal interpretation for post-treatment effects. After the first uprising, the number of white deaths due to police intervention increases sharply and peaks at event year 2 in both models. According to model 1, the fixed effects estimator, 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. Afterward post-treatment effects decrease and revert to pre-uprising trends in model 2.

Our primary results are summarized by joint treatment effects in Table 3, which presents estimates from Equation 2. Panel A report joint treatment effects of the first racial uprising on non-white deaths due to legal intervention and panel B 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 6 and 7, 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 shorter-run joint treatment effects (event years 1-3) are only marginally statistically significant for the fixed effect model, but model 2 produces larger point estimates. The medium run effects (event years 4 to 6) imply an additional 1 to 1.8 nonwhite deaths per year which are 90 percent larger than the shorter-run estimates. The longer-run effects (event years 7-9) captured the aftermath of uprisings in a period when the frequency and severity of riots have abated. The post-riot period corresponds to more deaths of non-white civilians due to legal intervention than the pre-riot period. The longer-run effects imply an additional 1.3 to 1.9 non-white deaths per year.

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 the shorter-run but statistically insignificant in the medium and longer-run. For both specifications, the joint post-treatment effects are never larger than 1. The shorter-run effect implies an additional 0.67 to 0.745 white deaths due to legal intervention per year. The medium and the longer-run effects are similar to the shorter-run effects for the fixed effects estimator but are much smaller for the reweighting estimator. The longer run effect

for the fixed effects estimator imply an additional 0.728 white deaths while the reweighting estimator implies an additional 0.116 white deaths due to legal intervention per year.

### 4.2 Robustness Checks

The additional columns in Table 3 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 a majority the variation in the dependent variable and driving the results. Secondly, the literature on uprisings has identified only three correlates or predictors of racial uprisings – 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 in panel A. 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 in panel B.

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 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.

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.[19] Secondly, white flight from urban centers into suburban communities could exacerbate

[18] This limits us to the 272 counties across the United States that experienced at least one riot over the time period.
[19] As stated earlier, the population variables are created from interpolating from 1960 census data to 1968 REIS data. See text above.

any estimated effect of police use of force against white residents. Lastly, the rarity of the event exacerbates the changes in small relative to large counties when scaled by the population. It is true that larger counties experience more deaths due to legal intervention but pre-period growth rates are similar across county sizes. As in Figure 5, pre-period growth rates in police homicides are statistically indistinguishable from zero for counties in each of the 1960 population quartiles. As a robustness check, we present estimates using mortality rates by race as the dependent variable with the caveats noted above.

Column 5 report joint treatment effects from an examination of the effects of uprisings on deaths per 100,000 residents due to police intervention. 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 show no consistent pattern. 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 4 presents our last set of robustness checks, where we include additional controls for model 2. Panel A present joint treatment effects for non-white deaths and panel B report joint treatment effects for white deaths due to police intervention. It is clear from Table 3 that population dynamics are influencing our results. We attempt to address this issue by adding a measure of the annual population as an independent variable. Instead of population influence the variation in the dependent variable, we use population to control for changes in population over time. According to column 1, adding the population control increases the post-treatment effects. Relative to column 1 of Table 3, the shorter-run effects increases by 171 percent for non-whites although not statistically significant, and increases by 38 percent for whites and is statistically significant. Medium and longer-run effects are much larger for non-whites, but the treatment effects slightly decrease over time for whites.

Column 2 accounts for additional riots that occur after the initial uprising. Previous results capture the local changes in policing and community behavior after the first riot, but the persistent increase in police homicides could be driven by additional riots. We account for additional riots by including a dummy variable to capture if a riot occurs in an event year after the initial riot. When accounting for additional riots, the shorter-run effects (event years 1-3) are much smaller. The shorter-run effects for non-white deaths become negative and are not statistically significant. The medium and longer-run effects are positive, statistically significant, but smaller the marginal effects in column 1 of Table 3. For white deaths due to legal intervention, including additional riots reduce

the post-treatment effects. The post-treatment effects are only statistically significant in the longer-run (event years 7-9). Although additional riots reduces the post-treatment effects, the treatment effects are much larger when we account for the severity of the additional riots. Column 3 interacts additional riots with the severity index, accounting for medium and high severity riots. This approach produces post-treatment effects similar to the baseline fixed effects estimator.

Crime, especially violent crime, began to rise in the 1960s, increasing the interactions between the police and civilians. Column 4 includes a measurement of crime and the number of police officers in a county. Including police and crime controls increase the post-treatment effects for nonwhite deaths and produce larger shorter-run estimates for white deaths due to legal intervention. Interestingly, adding crime controls produce estimates similar to the reweighting estimator. For nonwhites, the shorter-run effects are not statistically significant but increase over time and become statistically significant. For white deaths, the shorter-run effect is marginally statistically significant but the effects dissipate, and the medium and longer-run effects are statistically insignificant.

### 4.3 Heterogeneous Treatment Effects and Other Outcomes

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. The magnitude and the persistence of these effects may be driven by additional riots, but accounting for the severity of the riots produce estimates that are qualitatively similar. Interestingly, crime and police have little influence on the results.

Next, we consider the role of heterogeneous treatment effects. Specifically, we focus on two predictors of riots, size of the black population and the geographical region. Tables 5 report results from a model 2. For each column, all non-rioting counties serve as the comparison group. Column 1 and 2 report pre-treatment and post-treatment effects for deaths by legal intervention in rioting counties with the non-white population above/below the 1960 average non-white population. The average non-white population in rioting counties in 1960 was of 10.7 percent. Roughly 38 percent (103 of 272) of rioting counties had the percentage of the non-white population above this threshold.

In panel A, when limiting the rioting counties only those with an *above* or *below* average non-white population produce estimates that are slightly smaller and not estimated as precisely as estimates in panel A of Table 3. The shorter-run effect in both groups is statistically insignificant,

but in the above average group, the shorter-run effect is 24 percent larger than the below average group. The medium run effect is larger for the below average group and statistically significant while the longer-run effect is larger and statistically significant for the above average group. With regards to white deaths due to legal intervention, both groups report positive post-treatment effects. However, the shorter-run effect is statistically insignificant in the above average group and marginally statistically significant in the below average group. Both groups report medium and longer-run effects that are smaller and statistically insignificant. Although estimates in panel A and B are not measured precisely, column 1 and 2 provide qualitatively similar results to our preferred reweighted specification. This suggests that the treatment effects are not driven by counties with relatively large or small non-white populations in rioting counties.

Columns 3-6 account for regional variation in police use of lethal force after a riot occurs. The comparison group is all non-rioting counties, and each column reports joint treatment effects for when rioting counties in a particular region are removed from the analysis. In Panel A, the pre-joint treatment effects are statistically significant when the Midwest, South, or West is removed from the analysis but positive and statistically significant when rioting counties in the Northeast are removed. Model 1, including covariates and urban-by-year fixed effects, produce similar results and the pre-joint treatment effects are marginally statistically significant when Northeast rioting counties are dropped. This highlights the importance of the region-by-year fixed effects specification, which accounts for regional variation in rioting and police use of lethal force. The pre-joint treatment effects in Panel B are statistically insignificant in each column.

Post-treatment effects in Panel A are positive. Shorter-run effects are statistically significant only when rioting counties in the Northeast are removed. Medium run effects are statistically or marginally statistically significant when rioting counties in the Northeast, South, or West are removed but are not statistically significant when rioting counties in the Midwest are removed. Longer-run effects are statistically significant in all four columns. The shorter-run, medium-run, and longer-run effects are the smallest when the rioting counties in the Midwest are removed from the sample. In panel B, shorter-run effects are positive and statistically significant when rioting counties in the Midwest and the South are removed. Medium-run effects are statistically insignificant in all four columns, and longer-run effects are marginally statistically significant when rioting counties in the west are drop. According to Table 5, the increase in non-white deaths due to legal intervention is driven by counties in the Midwest while the immediate increase in white deaths is driven by rioting counties in the Northeast and West.

## 5. Discussion

Our results show that there is an increase in the use of lethal force toward whites and non-whites after an uprising occurs. However, the effects are persistent for non-whites and subside for whites. The long-run effects are quite devastating considering the rarity of police killings of a civilian. 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 in rioting counties relative to non-rioting counties.

Using estimates from the reweighing specification in Figure 6, we can estimate the total effect of uprisings on non-white deaths due to legal intervention. Figure 8 plots the average number of non-white deaths and the counterfactual average number of non-white deaths due to legal intervention. Pre-1964, the counterfactual estimates of non-white deaths due to legal intervention (1.84) are slightly larger than the actual average (1.74). However, the post-1964 average number of non-white deaths (2.92) is much larger than the counterfactual (1.88). The counterfactual average number of non-white deaths peak in 1967 at 3.2. The counterfactual average gradually declines and is 0.09 in 1975. However, the actual average number of non-white deaths peak in 1969 at 3.8 non-white deaths and gradually decline to 2.2 non-white deaths by 1975. The cumulative difference between the counterfactual and the actual average number of non-white deaths between 1960 and 1975 is 11 non-white civilian deaths.

Our main explanation for the results is that racial uprisings reduce the economic cost (implicit and explicit) of using lethal force. Urban decay and future lawlessness in the wake of a riot may increase the risk or perceived risk to police officers in police-civilian interactions. Relatedly, high crime counties may implement aggressive policing, exacerbating tensions between the police and the black community, which results in additional deaths due to legal intervention. We examined this argument by analyzing the relationship between rioting and lawlessness and reviewing changes in crime rates before and after a county's first uprising. The crime data used in this analysis aggregates agency-level data to county-level data. To account for the voluntary nature of the UCR, we include additional covariates to account for the percentage of the population covered in the aggregate measure of crime. We also included covariates that account for the proportion of agencies reporting consistently in a year as well as over time.

Figure 9 plots pre-treatment and post-treatment effects of the impact of a counties' first racial uprising on the log of total crime per 100,000 residents. Pre-treatment effects in both the fixed effects and reweighted specification are positive but statistically insignificant. As highlighted earlier,

the timing of an uprising is seemingly unrelated to many observed characteristics including crime rates. Total crime is evolving similarly in rioting and non-rioting counties before a county experience it's first uprising. Moreover, there is little evidence of a trend break in either specification. The post-treatment effects are essentially zero in the fixed effects model and negative but statistically insignificant in the reweighting model. According to these results, there is no evidence that uprisings increase crime. There are a few possible explanations for this result. One, there are likely heterogeneous effects. The severity of riots is an important factor when linking riots to urban decay. Collins and Margo (2007) highlight those cities where severe rioting occurs experiences a decrease in property values adversely affecting black-owned property. Secondly, riots have localized effects that influence outcomes at census tract level, which spillover to the entire city. However, we use a county measure of crime which may suppress localized effects by averaging outcomes in rioting cities with non-rioting cities. A city-level analysis does reveal an increase in reported crime after a riot occurs. However, the increase in crime only occurs in the shorter-run and is not persistent.

Relatedly, tough on crime policies in response to riots increased tension between blacks and police officers (O'Reilly, 1988). The War on Crime was introduced by president 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 (LEAA) were dispersed to deal with crime and riots in urban communities. LEAA provided federal funds to state and local municipalities for training, new equipment and hiring additional police officers. According to economic theory, hiring additional police in response to riots may deter future riots (increase total product). However, additional police officers (lowers marginal product) may be more likely to use force or increase the number of negative interactions between police and civilians, resulting in additional deaths due to legal intervention. Figure 10 present pre-treatment and post-treatment effects for the impact of uprisings on the log of police officers per 1,000 residents. Once again, pre-treatment effects generally statistically insignificant in both models and the post-treatment effects are essentially zero. A city-level analysis produces qualitatively similar results. This is not surprising considering the LEAA grant program was criticized for misappropriation of funds, funding ineffective programs, and inefficient use of federal funds by state planning agencies (Halloran 1971; Varon 1975; C.H. 1976; Diegelman 1982, Hinton 2016).

Lastly, an increase in the risk of police officers being injured or killed in the line of duty will reduce the economic cost of using lethal force. The increased risk of police injury could be a result of urban decay, perceived black militancy and radicalization, or embolden civilians (protest, peaceful or violent). Figure 11 presents estimates of the effects of the first uprising on the number of police

officers killed on duty. Pre-treatment effects are negative and statistically insignificant but are jointly marginally statistically significant. After the first uprising, the number of police officers killed on duty increases. The fixed effects model implies an additional 0.582 officers are killed in the line of duty in event year 2. Afterward, the number of police officers killed on duty continue to increase, and post-treatment effects are statistically significant after five event years in both models.[20] Although we are unable to link uprisings to changes in police employment or changes in crime rates, we do find that police officers are policing in more dangerous environments. The increase in the probability of being killed or injured while policing in conjunction with a federal mandate to reduce the number of uprisings is a possible mechanism that lowered the cost of using lethal force resulting in additional civilian deaths due to legal intervention. However, this is merely suggestive because we cannot rule out the possibility that community members created a volatile environment, evident by uprisings, which increased the risk to police officers.

## 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 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 thousands of 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 inciting of community unrest through the organizing of an uprising or protest. 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 phyically push back on police more frequently, the increase in community-police confrontations could produce the empirical pattern observed. Regardless of the mechanism, the results present a dismal picture. The uprisings

[20] 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.

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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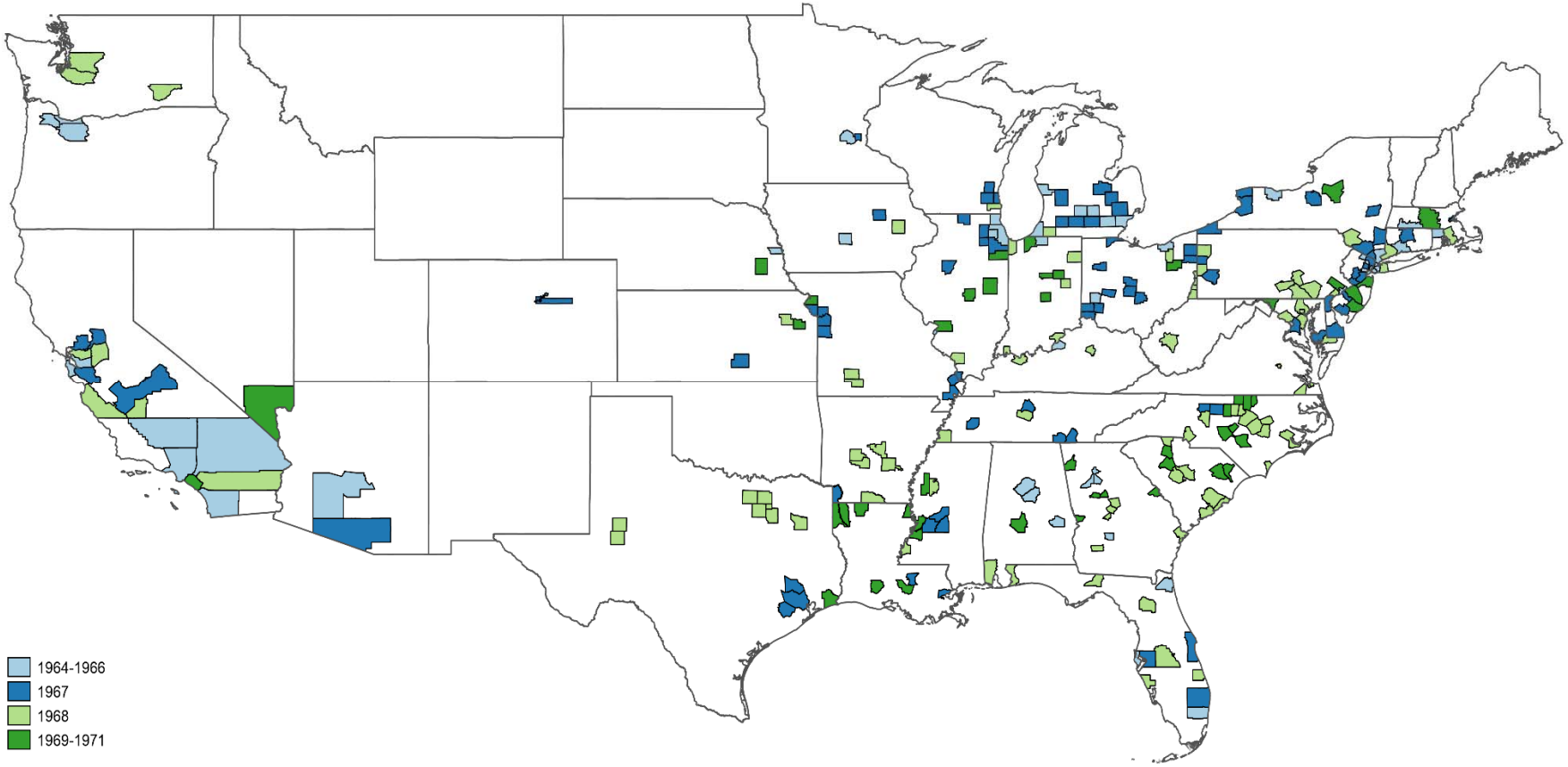
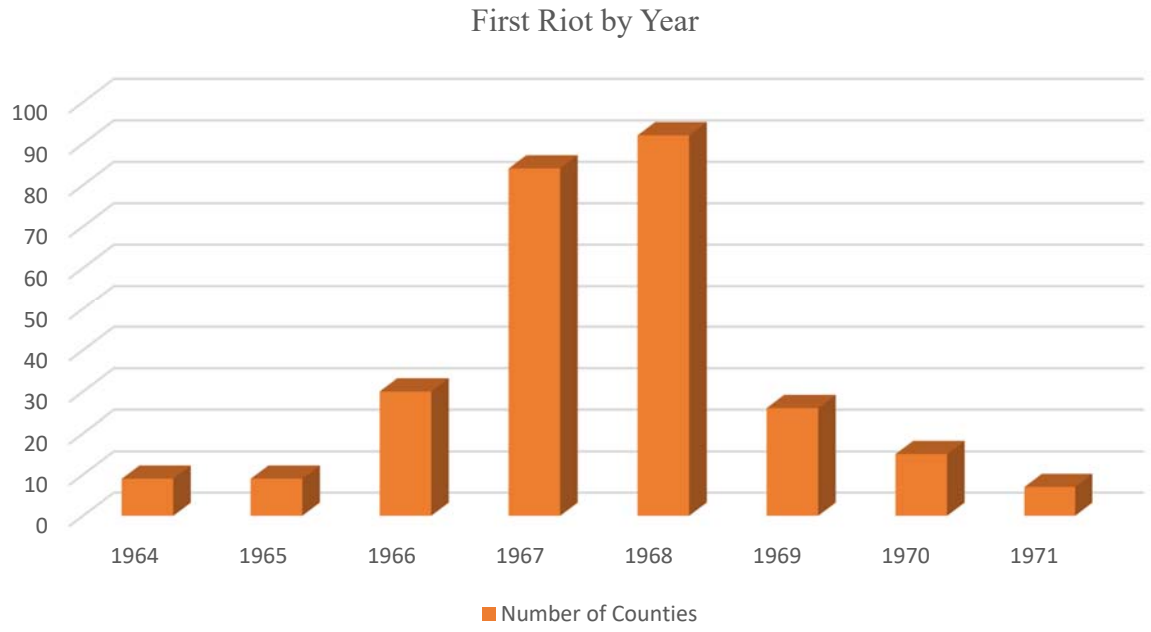
**Figure 1. County-Level First Riot**



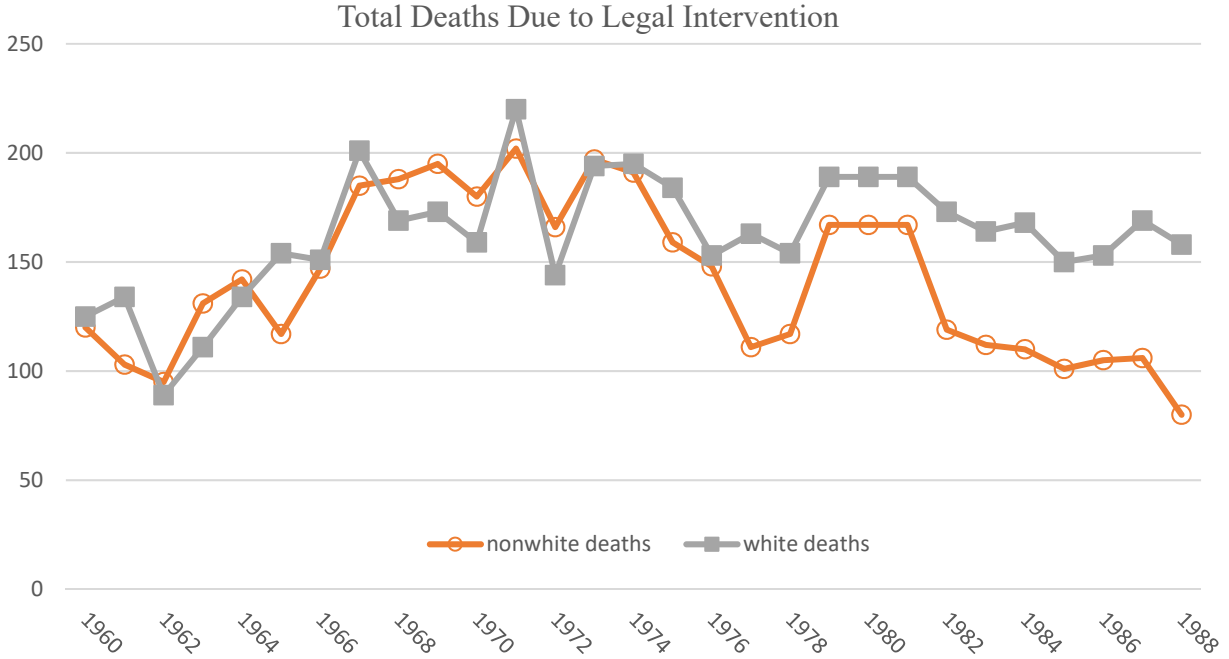

*Source:* Riot data comes courtesy of Carter and Margo (2007). Carter (1986) original source of race riots from 1964 to 1971.

**Figure 2. County-Level First Riot Occurrence by Year**



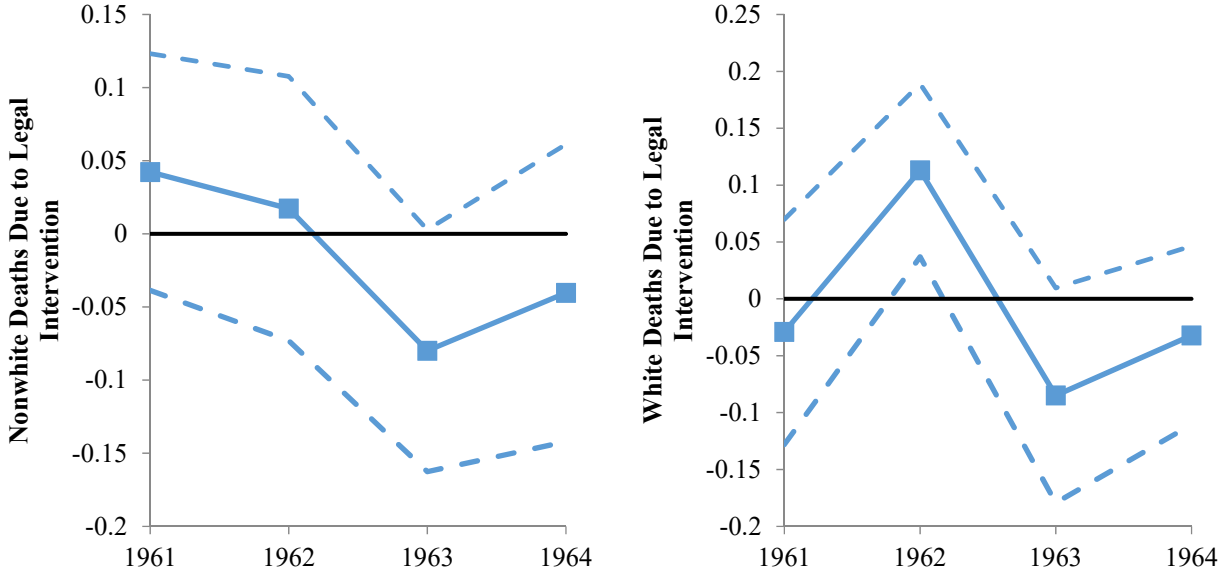

*Source:* Riot data comes courtesy of Carter and Margo (2007). Carter (1986) original source of race riots from 1964 to 1971.

**Figure 3. Annual Civilian Deaths Resulting from Legal Intervention**



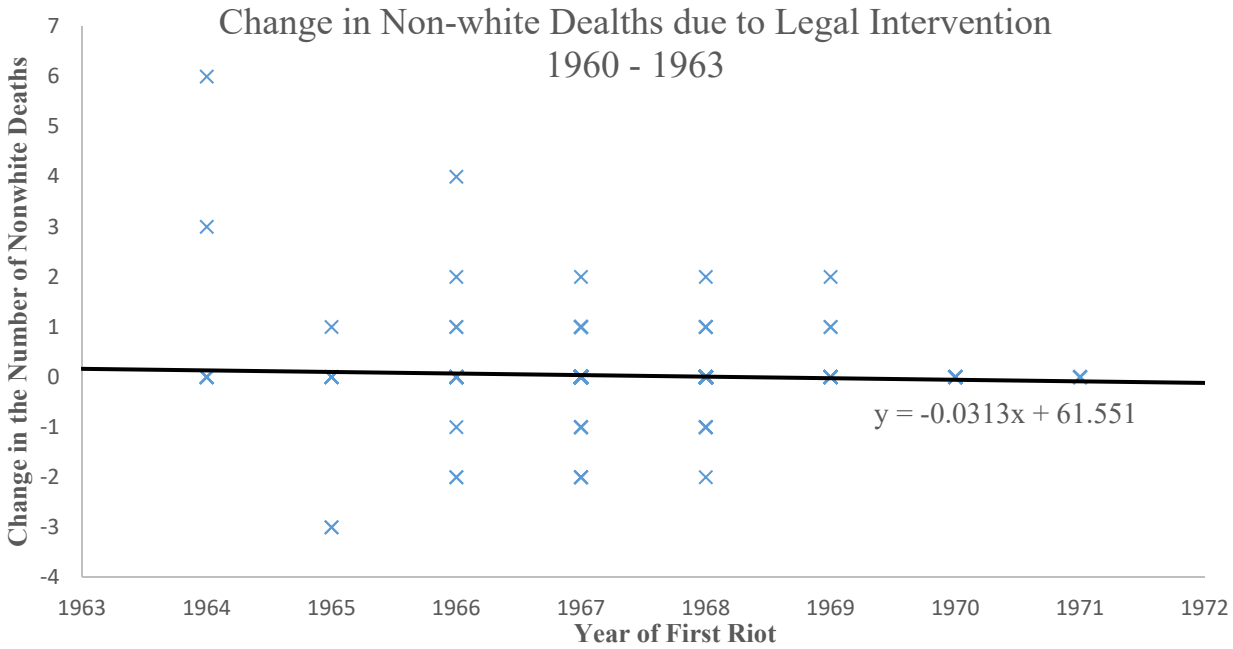

*Source:* Vital Statistics Multiple-Cause of Death Files (US DHHS and ICPSR 2007). The vertical axis corresponds to the total number of deaths due to legal intervention by race.

**Figure 4. The Average Pre-Trend Difference in Civilian Deaths by Legal Intervention, Rioting vs. Non-Rioting Counties – Evidence of Location**



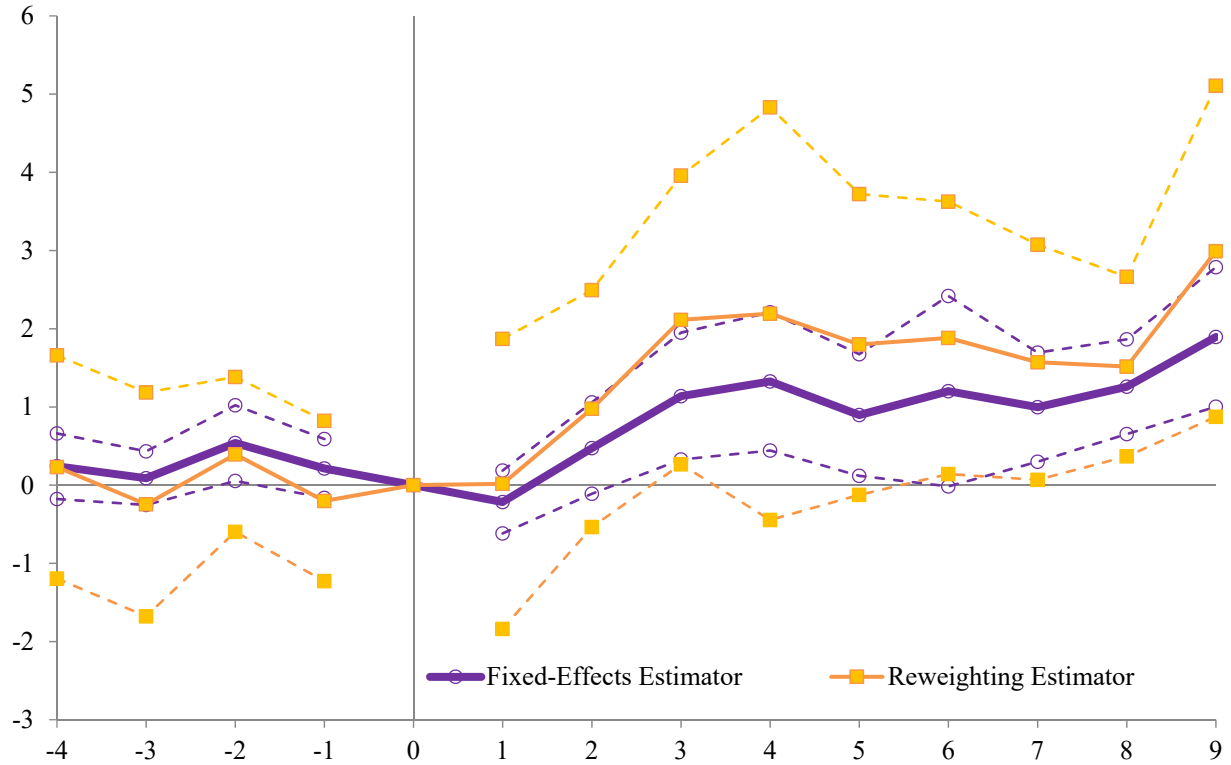

*Notes:* Figure plots the average difference in pre-period growth rates in police killings of civilians between rioting and non-rioting counties.

**Figure 5. 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 6. Estimates of Effects of First Riot on Non-White Deaths by Legal Intervention**

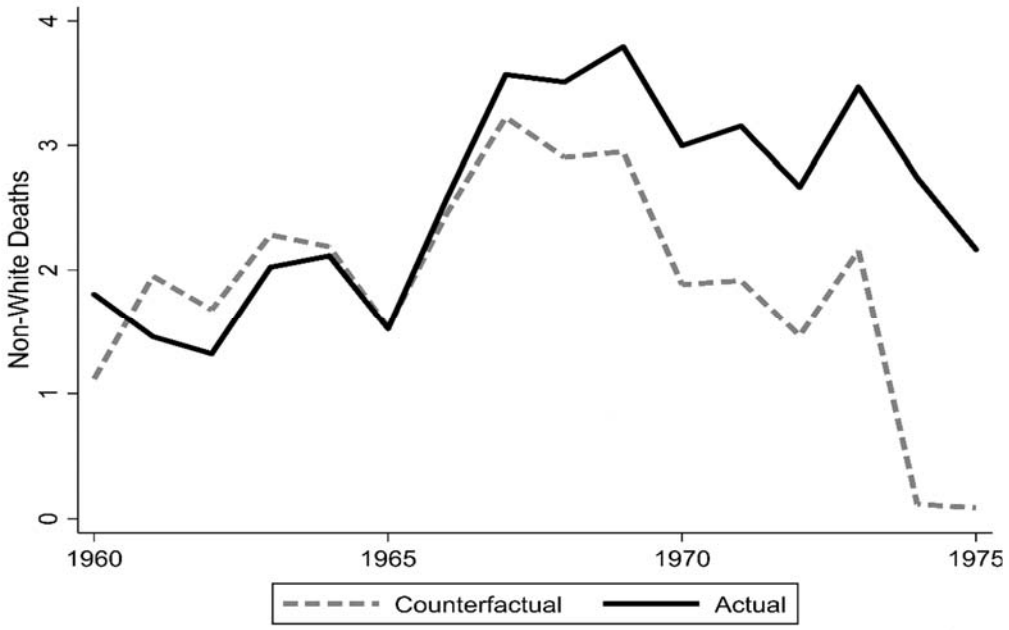



Notes: Figure display weighted least-squares estimates obtained from estimating by Equation 1. The dependent variable is the number of non-white deaths due to legal intervention. Model 1 includes county, C, effects, urban-by-year, U, effects, state-by-year, S, effects and covariates, X, from the 1960 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 7. Estimates of Effects of First Riot on White Deaths by Legal Intervention**

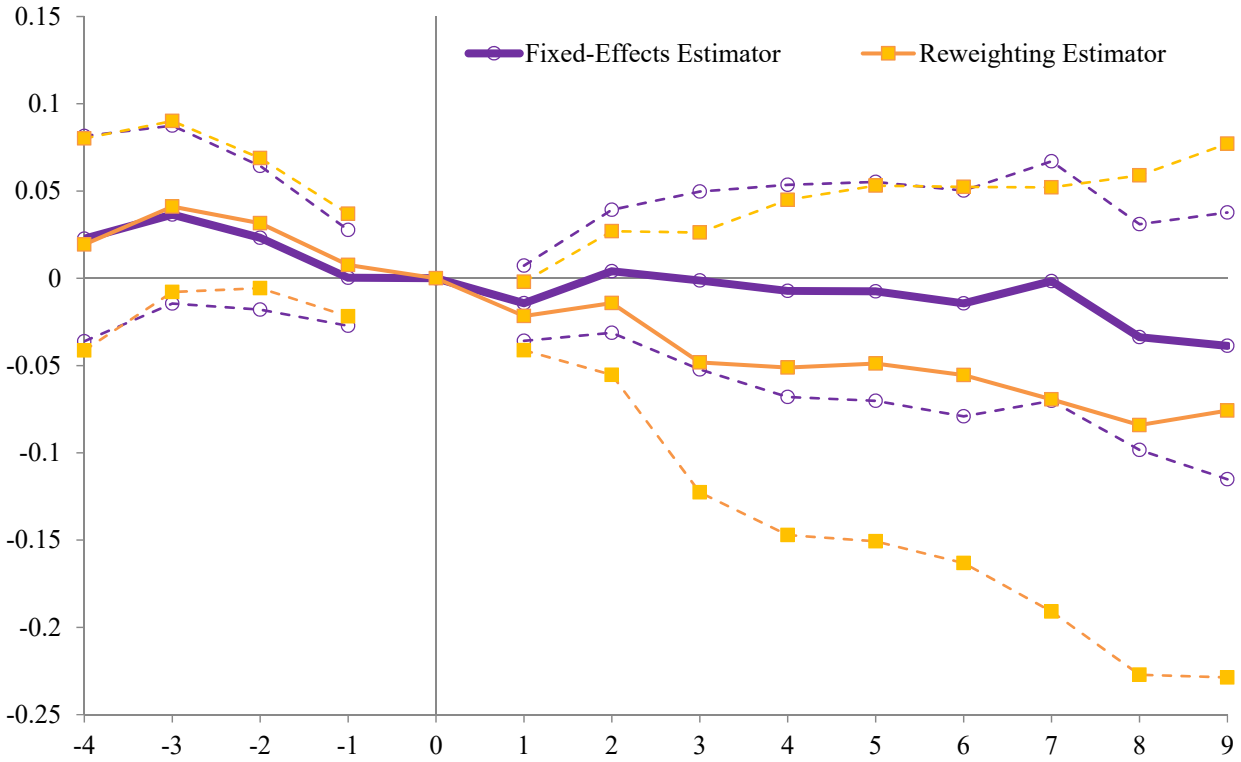

Notes: Figure display weighted least-squares estimates obtained from estimating by Equation 1. The dependent variable is the number of white deaths due to legal intervention. Model 1 includes county, C, effects, urban-by-year, U, effects, state-by-year, S, effects and covariates, X, from the 1960 census 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 8. Actual vs Counterfactual, Average Non-White Deaths due to Legal Intervention**



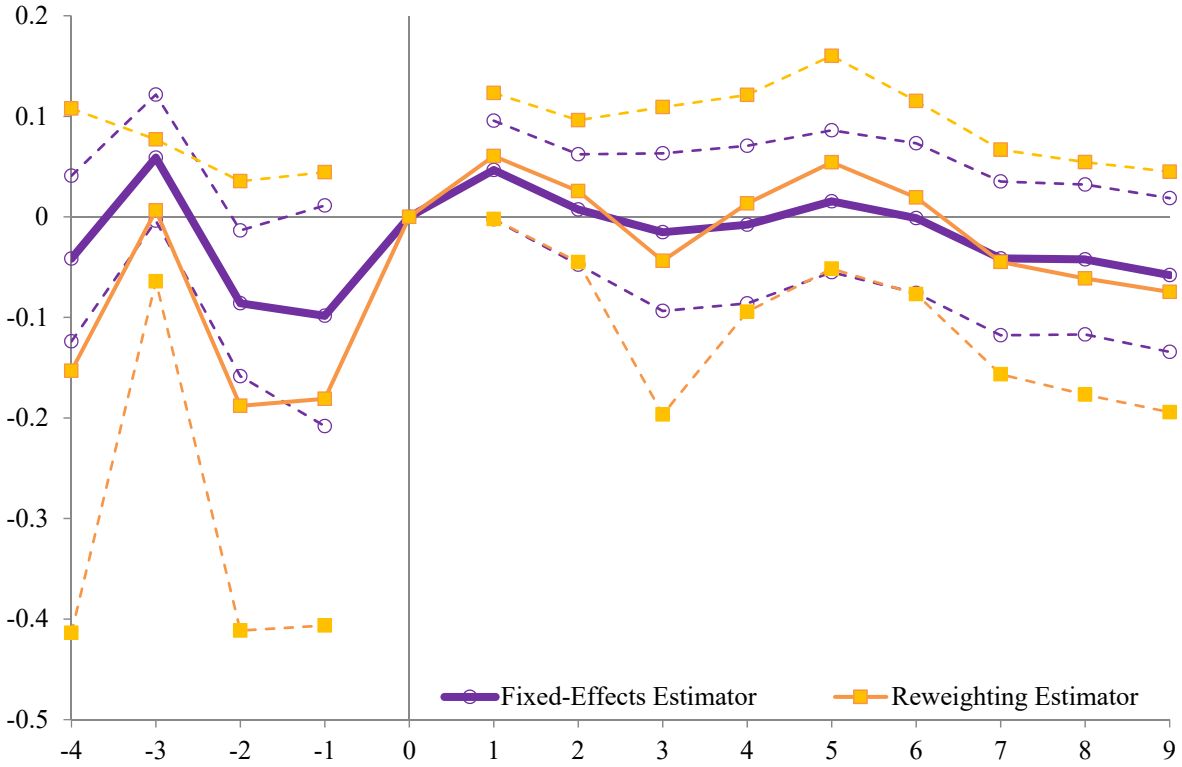

Notes: Figure displays the average number of non-white deaths and the counterfactual average number of non-white deaths due to legal intervention. The counterfactual is calculated using model 2 estimates from Figure 6.

**Figure 9. Estimates of Effects of First Riot on the Log of Total Crime Per 100,000 Residents**



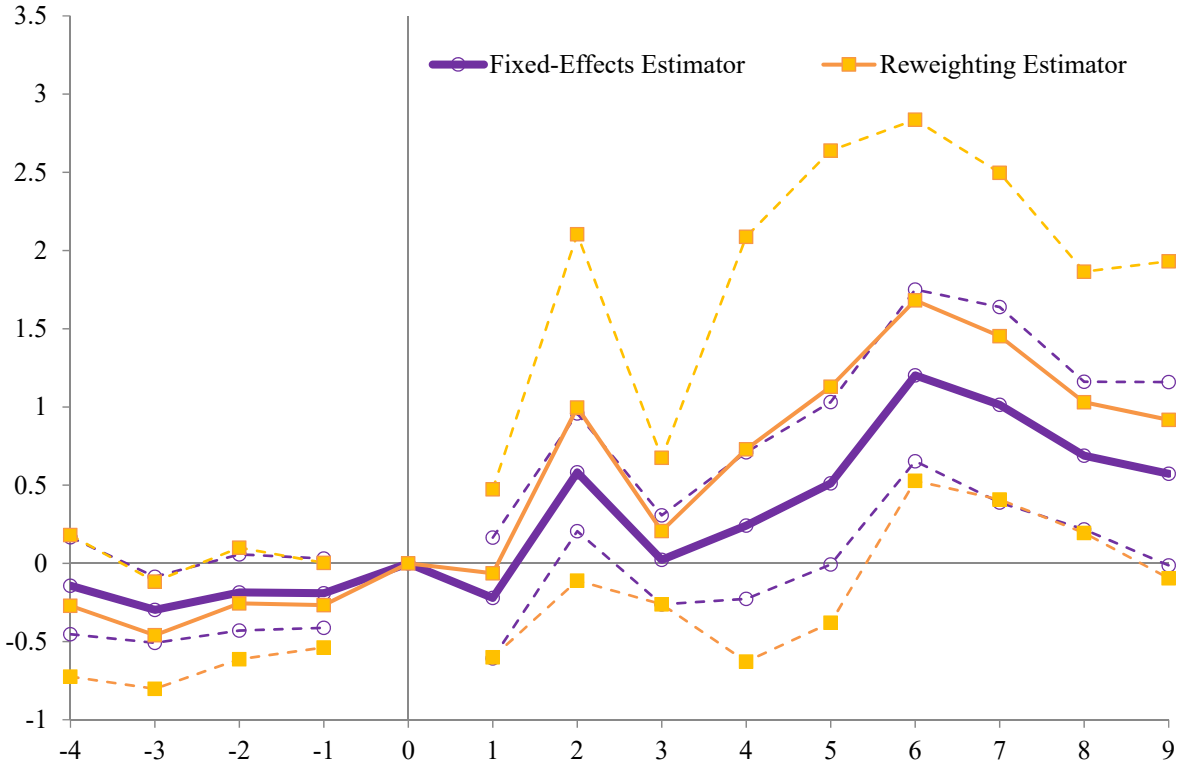

Notes: Figure display weighted least-squares estimates obtained from estimating by Equation 1. The dependent variable is the log of total crime per 100,000 residents. Model 1 includes county, C, effects, urban-by-year, U, effects, state-by-year, S, effects and covariates, X, from the 1960 census interacted 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 10. 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 11. 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* |
| Years -4 to -1 | 0.260 | 0.0352 | 0.538 | 0.357 | 0.474 |
| | [0.171] | [0.449] | [0.349] | [0.221] | [0.410] |
| Years 1 to 3 | 0.399* | 0.983 | 0.986 | 0.628** | -0.257 |
| | [0.222] | [0.685] | [0.604] | [0.271] | [0.240] |
| Years 4 to 6 | 1.048*** | 1.865** | 2.061** | 1.969*** | -0.0142 |
| | [0.391] | [0.917] | [1.005] | [0.465] | [0.210] |
| Years 7 to 9 | 1.286*** | 1.931*** | 2.284*** | 3.079*** | 0.612 |
| | [0.289] | [0.554] | [0.839] | [0.675] | [0.433] |
| R-squared | 0.710 | 0.163 | 0.366 | 0.767 | 0.157 |
| Number of Counties | 3,027 | 3,026 | 3,027 | 272 | 3,027 |
| | *DV: white deaths* | | | | |
| | | | | | *Per 100K* |
| Years -4 to -1 | -0.0914 | -0.436 | -0.0873 | -0.212 | 0.0284 |
| | [0.200] | [0.478] | [0.350] | [0.378] | [0.0679] |
| Years 1 to 3 | 0.670*** | 0.745* | 0.765** | 0.901** | 0.0589 |
| | [0.222] | [0.393] | [0.349] | [0.351] | [0.0420] |
| Years 4 to 6 | 0.680* | 0.393 | 0.641 | 1.071 | 0.0719 |
| | [0.393] | [0.616] | [0.474] | [0.749] | [0.118] |
| Years 7 to 9 | 0.728*** | 0.116 | 0.670 | 1.095* | 0.133 |
| | [0.258] | [0.580] | [0.438] | [0.560] | [0.124] |
| R-squared | 0.465 | 0.090 | 0.286 | 0.561 | 0.008 |
| Number of Counties | 3,064 | 3,063 | 3,064 | 272 | 3,027 |
| Covariates | S, U, & X | Y, P | R, U, & X | S, U, & X | Y, P |

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. Robustness Check: Event Study Regressions of Deaths by Legal Intervention**

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | | | *DV: non-white deaths* | |
| | Population Control | Additional Riots | Additional Riots Interacted with Severity | Total Crime & Police Control |
| Years -4 to -1 | 0.00852 | 0.104 | 0.245 | 0.0954 |
| | [0.424] | [0.162] | [0.172] | [0.420] |
| Years 1 to 3 | 1.083 | -0.193 | 0.369* | 0.984 |
| | [0.733] | [0.205] | [0.219] | [0.694] |
| Years 4 to 6 | 2.130** | 0.627* | 1.026*** | 1.774* |
| | [1.057] | [0.377] | [0.385] | [0.953] |
| Years 7 to 9 | 2.273*** | 0.948*** | 1.265*** | 1.840*** |
| | [0.620] | [0.263] | [0.283] | [0.510] |
| R-squared | 0.170 | 0.713 | 0.710 | 0.168 |
| Number of Counties | 3,026 | 3,027 | 3,027 | 3,022 |
| | | | *DV: white deaths* | |
| Years -4 to -1 | -0.461 | -0.195 | -0.0868 | -0.455 |
| | [0.501] | [0.207] | [0.190] | [0.489] |
| Years 1 to 3 | 0.923** | 0.218 | 0.681*** | 0.748* |
| | [0.426] | [0.143] | [0.196] | [0.392] |
| Years 4 to 6 | 0.884 | 0.373 | 0.687* | 0.414 |
| | [0.624] | [0.364] | [0.385] | [0.627] |
| Years 7 to 9 | 0.754 | 0.498** | 0.735*** | 0.130 |
| | [0.717] | [0.201] | [0.247] | [0.584] |
| R-squared | 0.137 | 0.468 | 0.465 | 0.091 |
| Number of Counties | 3,063 | 3,064 | 3,064 | 3,059 |
| Covariates | S, U, & X | S, U, & X | S, U, & X | S, U, & X |

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 5. Heterogeneous Treatment Effects – Non-White Residents**

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | *% Black Above Average* | *Northeast* | *Midwest* | *South* | *West* |
| | | | *DV: non-white deaths* | | |
| Years -4 to -1 | 0.389 | 0.756** | -0.304 | -0.482 | 0.00118 |
| | [0.590] | [0.326] | [0.461] | [0.760] | [0.486] |
| Years 1 to 3 | 0.966 | 1.635* | 0.0905 | 1.427 | 0.772 |
| | [1.138] | [0.854] | [0.575] | [0.957] | [0.708] |
| Years 4 to 6 | 1.556 | 2.660** | 0.772 | 2.289** | 1.499* |
| | [1.296] | [1.175] | [0.728] | [1.111] | [0.902] |
| Years 7 to 9 | 1.850*** | 1.610*** | 1.473** | 1.716** | 2.211*** |
| | [0.679] | [0.622] | [0.661] | [0.869] | [0.557] |
| R-squared | 0.152 | 0.160 | 0.141 | 0.250 | 0.171 |
| Number of Counties | 2,857 | 2,814 | 1,998 | 1,643 | 2,623 |
| | | | *DV: white deaths* | | |
| Years -4 to -1 | 0.253 | -0.605 | -0.707 | -0.522 | 0.0757 |
| | [0.528] | [0.712] | [0.597] | [0.548] | [0.232] |
| Years 1 to 3 | 0.940 | 0.762 | 0.578** | 0.749* | 0.649 |
| | [1.056] | [0.559] | [0.281] | [0.424] | [0.464] |
| Years 4 to 6 | 0.322 | 0.850 | 0.0428 | 0.280 | -0.0758 |
| | [0.784] | [0.707] | [0.591] | [0.654] | [0.489] |
| Years 7 to 9 | 0.0410 | -0.484 | -0.302 | -0.0680 | 0.628* |
| | [0.223] | [0.734] | [0.846] | [0.717] | [0.377] |
| R-squared | 0.121 | 0.107 | 0.097 | 0.102 | 0.104 |
| Number of Counties | 2,894 | 3,017 | 2,986 | 2,942 | 3,035 |

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.