Using Riot Shocks to Estimate the Consequences of Lower Neighborhood Quality on Adolescent Dropout Rates (DRAFT)*

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Abstract

I study the short and long run effects of diminished community conditions on city level dropout rates by exploiting plausible exogenous variation in city quality through the occurrence of riots across the United States during the 1960s and in Los Angeles during 1992. Using a basic OLS, a regular difference-in-differences, and a new method, synthetic control matching, on decennial census data, I find that riots depressed enrollment rates between 1960 and 1970, with no rebound in the 1970s. While these results indicate both short and long-term effects, using yearly California administrative data, I find only short-term effects on dropout rates in Los Angeles after the 1992 riot. The results indicate that community conditions do have an affect on schooling persistence rates and highlights the relevance of community conditions in education based public policy.

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1 Introduction

At least since the publication of William Julius Wilson's *The Truly Disadvantaged* (1987), which asserts that concentrated poverty engenders and perpetuates social problems, many commentators and policy makers have argued that communities have an independent, measurable effect on the health, social, and economic outcomes of its residents. Community quality is not a single characteristic, but encompasses a bundle of aggregate level traits - percent unemployed, racial diversity, crime rate, to name a few. Wilson hypothesizes that high or low levels of these characteristics create a culture of disadvantage within a community that discourages individual social advancement.

An integral component of a healthy community is the educational advancement of its adolescent youth. The effects of communities on youth outcomes are of particular interest since it is during adolescence a person's social world begins to integrate peers and the larger community (Darling and Steinberg 1997). The purpose of this paper is to identify the effects of community quality on high school dropout rates. The dropout rate reflects the amount of human capital in the population and a community's success in preparing its youth for adulthood. The evidence since Wilson's publication indicates a positive correlation between community quality and adolescent academic success (Johnson 2011).

A major methodological obstacle to estimating the impact of community conditions is the sorting of individuals into neighborhoods for reasons that are likely to be correlated with the underlying determinants of their outcomes. An additional obstacle specific to academic outcomes is the difficulty of separating a pure community effect from strictly school level factors. In an attempt to obviate these problems researchers have relied on social experiments such as the Moving to Opportunity program, which provides housing vouchers to families to facilitate relocation to low poverty neighborhoods. A key limitation to these programs is that they identify community effects that may be conflated with the effect of residential mobility. Disentangling neighborhood effects is further complicated by the fact that programs change children's schools and neighborhood attributes simultaneously. One way to address these limitations is to conduct an experiment randomizing neighborhoods rather than individuals into better conditions. However, such an experiment currently does not exist and conducting one at a large scale would be costly.

When carefully constructed social experiments are not available, we need a naturally occurring mechanism that acts at the community level to exogenously change community conditions while leaving family and school level factors undisturbed. In this study, I use the occurence of an urban riot as a natural experiment for estimating the effects of a negative shock on community quality. In particular, I study the effect on dropout rates in Los Angeles after the 1992 riot and in United States cities affected by the civil rights riots during the 1960s. Here I define the community at the city level and connect shocks to the city to its aggregate level rates. The extent to which I can connect a decrease in city quality due to a riot to subsequent changes in enrollment rates depends on the exogeneity of riot occurences across geography and the size of their impact on cities.

Serious academic inquiry into the causes of riots largely developed after the 1960's, led by Seymour Spillerman (1970, 1971, 1976) and his set of influential studies examining the 1960s riots in the United States. He concluded that riot occurrence and severity are unpredictable after controlling for black population size and region. Building on Spillerman's findings, I show that there exists a small, well-defined set of variables that consistently predicts riot occurrence. After conditioning on these variables, riot shocks are essentially random.

Capturing the exogenous portion of riot occurrence is only half the task. A riot must also affect the entire basket of goods that make up the overall quality of a city. Drawing on studies that examine the impact of riots on city-level housing values and taxable sales, which are established proxies of city quality, I find that a riot's effect on a city is not geographically local, short term, and relevant to only certain aspects of quality, but is widespread across space and time.

To estimate the effect of decreased quality on city dropout rates, I employ three modeling strategies: a regular ordinary least squares (OLS) regression, a traditional difference-in-differences (DID) model, and a new method, synthetic control matching (Abadie and Gardeazabal 2003; Abadie, Diamond, and Hainmueller 2010). I also conduct an analysis at the census tract level using 1992 Los Angeles riots' data to estimate local effects. I use a variety of models primarily to test the robustness of my findings across various specifications and provide a comparative analysis of three popular estimation procedures. My principal finding is that non-enrollment rates decreased more slowly in riot-affected cities between the periods 1960-1970 and 1960-1980, indicating that riot shocks have both short and long-term effects. Using the 1992 Los Angeles riot, I find only short-term effects, revealing that cities experiencing more contemporary shocks may have the infrastructure to rehabilitate their schooling systems downstream.

I begin the paper with an examination of the riot cause literature in the context of both the 1960s and the 1992 Los Angeles riots. In this section, I show that although riots are not entirely unpredictable, previous research has consistently found a limited, well-defined set of variables to be predictive of riot occurrence and severity. I then shift attention to the literature examining the effects of riots. In this section, I establish that riots have such wide, debilitating effects on a city, that the overall quality of a city is diminished in the short and long term. I then outline my empirical strategy followed by separate results for the 1960s and the 1992 Los Angeles riots. I conclude the article with a discussion of the main findings.

2 Riots Background

A riot is generally defined as a group of "people attempting to assert their will immediately through the use of force outside the normal bounds of law" (Gilje 1999). Although this definition has some legal precedent, I am only interested in the type of riots experienced during the 1960s and in Los Angeles in 1992. Conforming to the standards established by Spillerman (1970) and subsequently adopted by others (Carter 1986; Olzak and Shanahan 1996; Olzak, Shanahan, and McEneaney 1996; Myers 1997; DiPasquale and Glaesser 1998), I define a riot as a "spontaneous event" with at least 30 participants that resulted in property damage, looting, or other aggressive behavior

[1].

I exclude riots that have documented origins in civil rights or war demonstrations, school settings, formal protests, or other planned activities because disorders originating from these activities may reflect local based grievances and tensions that are a reflection of local underlying causes of riot occurrence. Although many of these riots had destructive effects on cities, my empirical strategy relies on disconnecting riot occurrence with local conditions. If a riot was largely a function of local context, attempting to use riot shocks to measure the effects of city quality on resident outcomes becomes difficult because I must control for each community's set of conditions, most of which are likely unmeasurable.

2.1 Causes of Riots

In order to use riots to identify the effects of lowered city quality on dropout rates, we must determine whether characteristics that influence both riot occurrence and dropout levels exist. If riot occurrence is not completely exogenous, we must control for the complete set of city-level characteristics that do predict riot activity.

The 1960s riots of the United States were historically unprecedented - within the span of 10 years, hundreds of riots erupted across the country. A number of riots led to levels of violence, theft, property damage, and police mobilization unseen in U.S. history. For example, the 1964 Watts riots led to 34 deaths, $40 million in property

[1] I acknowledge that there are various definitions of a riot, but the purpose of this paper is not to explore the validity of these definitions. Since this is not a study measuring the direct effects of riots, but an analysis of negative shocks on city quality, the precise definition of a riot is not entirely relevant. On the many issues involved in defining riots, see Gilje (1996, 4).

damage, and the mobilization of over 14,000 California National Guard troops in over six days of disorder. The most violent of the riots occurred in 1967 in Detroit, where nine days of rioting led to 7,200 arrests, 1,600 cases of arson, and 43 deaths, the most in any city. There were a total of 752 riots that occurred between 1964 and 1971, yielding nearly 70,000 arrests, 16,000 occurrences of arson, 12,700 injured persons, and 228 deaths.

The most destructive and expensive riot in U.S. history did not occur during the 1960s, but in 1992 in Los Angeles. In response to the not guilty verdicts of four Los Angeles Police Department officers on trial for the beating of Rodney King, protests erupted into what many consider to be the "worst riot the U.S. has seen in modern years" (Hohman 2002). What followed the trial were nearly 3 days of riots resulting in at least 42 deaths, more than 700 businesses burned, over 5,000 people arrested, and approximately one billion dollars in property damage (Webster and Williams 1992).

Once the severity of these riots were assessed, the immediate reaction was to determine the root causes of the violence so that future outbreaks could be avoided. Spillerman was one of the first researchers to publish scholarly work examining the causes of riot occurrence. In his set of influential studies (1970, 1971, 1976), he tested the predictive power of various economic and sociological theories hypothesized to predict riot occurrence and severity in urban areas in the 1960s. He grouped city-level variables into clusters that represent broad theories related to black relative and absolute deprivation (Downes 1968; Gurr 1968), lack of political representation of minority groups (Lieberman and Silverman 1965) and minority expectations of economic and social fairness. He concluded that these theories did not predict the frequency and severity of rioting. In fact, he found that only black population size and U.S. region predict riots. He concluded that rioting was driven by nationwide conditions, instilling a "riot ideology" among city residents, particularly blacks; therefore riots would break out randomly, and when and where were predicted only by the number of available rioters.

Spilerman's findings largely defined the field until more recent scholarship challenged and refined his results. Olzak and Shanahan (1996) and Olzak, Shanahan, and McEneaney (1996) expand Spilerman's data set to include riots up to 1992 and confirm his conclusion that variables related to absolute and relative deprivation, political structure, and competition do not predict riots. However, they find that racial competition as proxied by racial segregation measures are significant predictors. Myers' (1997) findings largely support their results, but he also finds evidence of a diffusion process such that city-level proximity to previous riots predicts future riot occurrence. In these three studies, non-white population and region continue to explain a significant amount of variation in riot occurrence.

DiPasquale and Glaeser (1998) examine the 1960s and the 1992 Los Angeles riots separately and find that non-white population, unemployment, and home ownership, and government expenditures on police predict riots in the 1960s, total population, ethnic diversity and Black and Hispanic unemployment rates predict riots in Los Angeles in 1992, and poverty and migration do not predict riots in either case. Bergesen and Herman (1998) find that in-migration of Asian and Hispanic groups predict census tract level riot occurrence in Los Angeles in 1992, but median income and unemployment do not. Ridland (1993) examines census tract data specific to South Central Los Angeles and finds that riot property damage does not correlate with selected socioeconomic variables, including income, poverty, and overcrowding. Baldasere (1994) claims ethnic tension between Blacks, Whites, Asians, and Latinos was a critical cause in the Los Angeles riots. These later studies confirm that pooling the 1960s and 1992 Los Angeles riots together may be inappropriate because although the spark that potentially started the Los Angeles riot was connected to similar African American related issues present in the 1960s, participation during and the issues emanating after the riots were multiethnic.

In the majority of these studies, many of which included more recent riots, Spilerman's variables, black or non-white population and region, consistently predict riot severity and occurrence. However, there is a lack of consensus on other predictors. Table 1 provides results for major empirical studies investigating the causes of riot occurrence or severity in the 1960s and in Los Angeles in 1992. Measures of black or non-white population, either in absolute size or percent, and census region appear frequently as significant predictors. Although other common themes arise, such as racial competition or mixing, the rest of the table reveals that there is no real consensus on other aggregate or individual level variables that predict riot occurrence or severity. For example, while Spilerman (1970, 1971), Myers (1997), and Olzak and Shanahan (1996) discredit deprivation theory, Lieske (1978), Carter (1986), and Chandra and Foster (2005) find evidence supporting this hypothesis. Several variables, such as Myers' interaction of non-white unemployment and percent foreign born, predict riot occurrence in the opposite hypothesized direction. Despite efforts to expand Spilerman's initial findings, we can firmly conclude that a small set of variables, related to a city's population size, racial mixture, and geographic region, consistently explain riot occurrence and severity in United States cities in the mid to late 20th century. The mixed bag of evidence beyond these variables shows that either other community level factors do not account for riot occurrence and severity or that researchers have simply not constructed the appropriate variables that represent broader theories.

2.2 Effects of Riots on City Quality

While scholarly work has brought considerable insight into the causes of riots, far less attention has been devoted to understanding the effects of riots . If these effects are felt city wide such that it diminishes a city's overall quality, the health of a city's social outcomes (e.g. education, health, fertility) is in danger of deteriorating. In this section, I establish the link between riot occurence and city quality and use these results to generate hypotheses on why diminished city conditions might affect city-level adolescent dropout rates.

Case studies of modern urban riots have described the devastating economic and social costs of a riot on affected cities (Aldrich and Reiss 1970; King 2003; Margo and Smith 2004). A riot's effect on city conditions can be direct and immediate or indirect and long term. Direct effects impact those with an immediate association to the riot: deaths, injuries, arrests, burned buildings, looted businesses, and so on. Although the direct effects can be significant, the percentage of a city's population directly impacted by a riot is relatively small and the immediate economic costs are transitory (Widick 1989). However, a riot's indirect effects, which capture a riot's impact on a city mediated through economic and social pathways, can carry significant and long-term consequences for a city's overall quality.

A riot adversely affects the economic health of a city through various channels. Local entrepreneurs whose businesses were burned or looted during the riot likely leave the city for safer environments. Residents relying on local businesses for employment seek jobs elsewhere or remain unemployed. Wealthier residents seeking safer neighborhoods move out only to be replaced by poorer families. The unsafe social conditions, the deteriorating economic health due to the outmigration of local businesses and wealthier families, and the overall negative stigma attached to a riot-affected area make it difficult for cities to attract new businesses and non-poor residents. The downward spiral continues as the negative decline reinforces itself as tax revenues diminish, crime increases, and the quantity and quality of public services decline.

City quality encompasses more than just economic vitality and the health of local infrastructure. The strength of community norms, neighborhood networks, and social capital demonstrate the ability of residents to productively live and work together and reflect the overall safety and social stability of a neighborhood. Riot-affected cities incur serious social costs. Mobility of residents sever established social ties that help promote trust amongst neighbors and build social capital within a neighborhood. The out-migration of more affluent residents reduces the pool of adult role models and exposure to high achieving students who place considerable value on schooling success. The violence and disorder of a riot reduces a neighborhood's immunity towards deviant behavior, allowing delinquency and disorder to become acceptable community norms. Individuals lose faith in the collective efficacy of their neighborhoods when they witness residents committing crimes against their neighbors and destroying shared local infrastructure.

We can quantify the effects of a riot on city quality by drawing on studies estimating the impact of riots on property values and taxable sales. Property value is a generally recognized proxy measure for many indicators of neighborhood quality, such as crime and poverty rates, because these aspects of a neighborhood are capitalized into the value of its properties (Galster et al. 2004). Reduced housing values could work through a number of channels that feed into the net benefit stream: personal and property risk might seem higher; insurance premiums might rise; taxes for redistribution or more police and fire protection might increase; retail outlets might close; businesses and employment opportunities might relocate; friends and family might move away; burned-out buildings might be an eyesore[2]. In an analysis of large cities affected by riots in the 1960s, Margo and Collins (2007) find that city housing median values decreased in riot-affected cities in 1970 and 1980. They arrive at the same conclusion when looking at city wide black-owned property values and examining census tract data in Cleveland and Newark.

Taxable sales are also a good indicator of economic well-being as they are strongly correlated with many measures of economic activity such as personal income or gross

[2]See Roback (1982) for a lengthier discussion of the connection between amenities and property values

domestic (city) product. Baade, Baumann and Matheson (2007) and Baade and Matheson (2004) find an immediate loss in taxable sales in Los Angeles after the 1992 riots. They also find that in the years since the Los Angeles riots, loss of taxable sales in the city has translated into a cumulative loss of $3.8 billion and over $125 million in direct sales tax revenue losses. Using per capita levels of jobs as a measure of economic health, Johnson, Farrell and Toji (1999) find a two year impact on Los Angeles county while Spencer (2004) finds no impact by 1997 in zipcode defined riot-affected neighborhoods. These findings suggest that the Los Angeles riot had short but no long-term effects.

These results indicate that riots have a negative effect on city quality. The concern in this paper is to estimate the impact of this downturn in city conditions on school enrollment rates. Note that although a riot may have a direct effect on enrollment rates through the destruction of a school or the death or imprisonment of children or their families, these direct effects are likely negligible, and thus a riot works indirectly through its impact on city quality to influence enrollment. Previous literature has established that neighborhood conditions can influence youth well-being through a variety of mechanisms (Harding et al 2011), including the availability of institutional resources (Cook et al 2002; Celano and Nueman 2001), school and neighborhood climate and safety (Woolley and Kaylor 2006; Pong and Hoa 2007), exposure to positive role models in the neighborhood (Crane 1991) and rapid changes in neighborhood composition (Crowder and Teachman 2004). In general, poorer neighborhood level economic health has been linked to lower rates of schooling persistence (Corman 2003; Harding 2003; Jacob, 2004) and lower academic achievement (Ladd and Ludwig 1997; Ainsworth 2002).

Although the breadth of research on understanding the effects of riots is not deep, we can still conclude from the available findings that riots have wide geographic effects that result in a significant downgrade in the overall quality of cities. The effects of a riot are not simply confined to the neighborhoods that experience the most violence and destruction, but are felt by all residents, businesses, and institutions located throughout a city. These effects then translate into diminished community quality. The extent to which communities affect individual outcomes has been researched and debated for decades. This study attempts to answer this question by using plausibly exogenous variation in city quality induced by riot occurrences.

3 Data

3.1 1960's Riots

In studying the causes of the 1960's riots, Spillerman (1970, 1971, 1976) collected data measuring the extent of damage on riot-affected cities. Gregg Carter (1986) subsequently extended Spillerman's data set by including more years and verifying the accuracy of the data by checking alternative sources. Carter's data set includes dates and locations of more than 700 riot related civil disturbances during the time period of 1964 to 1971. Each case is a time by city observation measuring the number of deaths, injuries, arrests, and arsons. Carter excluded disturbances related to organized Civil rights protests and those occurring in schools.

For this analysis, I use a city-level version of Carter's dataset constructed by Margo and Collins (2007). They summed up the days, arrests, deaths, injuries and occurrences of arson for each city to create a city-level dataset containing 316 observations. Since I consider 1970 a post-riot year, I eliminate cities experiencing its first riot after 1969. I merge in a number of control variables, civilian unemployment rates, population total, educational attainment, median housing values, non-white population, and region, into the dataset. Demographic data were obtained from the 1950, 1960, 1970 and 1980 decennial census through the City and County Data Books. I exclude cities with a population size of 50,000 or less since these cities are missing data on at least one of the control variables or the outcome. Given this limitation, we must not extrapolate the results to all cities, but limit the discussion to just large cities in the United States. I am left with a sample of 147 riot-affected cities for my analysis. I include cities experiencing no riot activity during the 1960-1970 time frame to act as control units in the analysis. The addition of these cities brings my final analytic sample to 302.

Margo and Collins (2007) constructed an index measuring the level of severity experienced in each riot afflicted city. Each city is assigned a value $S_i = \sum_j \frac{C_{ij}}{C_{iT}}$, where C_{ij} is a component of severity j (deaths, injuries, arrests, arsons and days of rioting) for city i and C_{Tj} is the sum of the severity component j across all cities. Higher values of S_i indicates greater riot severity. Table 2 presents the distribution of S_i for the 147 riot cities used in the regression analyses. The index is highly skewed as a large number of cities had minor riots, with a handful, such as Hammond, witnessing no deaths, injuries, arrests, or cases or arson. Compare this to the riots experienced in Los Angeles, which totalled over 1,000 injuries, 30 deaths, 4,000 arrests, and 3,000 cases of arson.

I use the non-enrollment rate as my measure of the dropout rate. The non-enrollment rate is defined as the ratio of the number of 16 and 17 year olds not enrolled in either public or private school to the size of the population of 16 and 17 year olds residing in the city. The decennial census reports non-enrollment rates at the city level in 1950, 1960, 1970, and 1980. The non-enrollment rate is a measure of dropout since by and large, if an adolescent is not a dropout, he should be enrolled. It is possible that individuals not enrolled in school could not be dropouts, but early graduates. Conversely, individuals enrolled in school could be early graduates enrolled in post secondary institutions. These concerns should be minimized since the percent of adolescents graduating at age 16 or 17 is small[3].

Table 3 presents summary statistics in pre-riot years 1950 and 1960 for cities affected and not affected by a riot. On average, riot-affected cities are larger, less educated and less white. However, riot-affected and non-affected cities have similar median housing values and unemployment rates. Riot-affected cities have higher pre-riot non-enrollment rates, although the differences are relatively small. Figure 1 maps out the riot-affected and non-affected cities used in the analysis. The red circles represent riot-affected cities with their sizes proportional to the severity of the riot. Heavily affected cities appear to cluster in certain regions of the country, particularly in California, the midwest and in the northeast. Table 3 and Figure 1 reveal the importance of controlling

[3]I considered using the status dropout rate, which measures the percent of 16-21 year olds who are not enrolled in school and have not earned a high school credential. However, the statistic has several flaws, many of which are related to the imprecise measurement of the numerator and misreporting (Warren and Halpern-Manners 2007). Additionally, the wider age range increases the potential that a significant percentage of those counted in the numerator dropped out of secondary schooling in a city other than the current residence. Lastly, the census reports status dropout rates only for cities with a population of at least 250,000, which severely reduces the analytic sample.

for Spillerman's variables, percent non-white and region, as well as population size.

3.2 1992 Los Angeles Riots

I conduct my analysis of the Los Angeles riot at the city and census tract levels. City-level control variables were obtained from the 1980 and 1990 decennial census through the National Historical Geographic Information System. After the Los Angeles riots in 1992, the Los Angeles Department of Building and Public Safety, in conjunction with the Los Angeles City Fire Department, published the Disorder Damage Survey, which contains addresses where commercial and residential structures were damaged during the 1992 Los Angeles Riots. Watts (2010) combined these addresses with an additional data set from Ong and Hee (1993) to construct what can reasonably be considered a near census of riot related damaged structures. Watts geocoded the 1,234 mappable locations onto 1990 defined census tracts.

Bergesen and Herman (1998) used locations of riot related fatalities to determine which census tracts were affected by the riot. The authors located the nearest intersection for each of the 51 reported riot fatalities using reports from the Los Angeles Times and a City of Los Angeles commissioned report summarizing possible causes and effects of the riot. I combine the death locations with the property damage data to construct a data set containing census tracts that either had riot related property damage or a fatality.

While city boundaries remain relatively stable from one census to the next, census tract boundaries have changed significantly since 1980, making the process of comparing tracts over time difficult. In order to make comparisons, standardized tracts were established. In this case, 2000 census tracts were used as the standard and the data from 1980 and 1990 census were converted to their 2000 census tract equivalents given the distribution of the population. Standardized census tract data were obtained from the Geolytics Neighborhood Change Database (GeoLytics 2003).

The non-enrollment rate used in the 1960s riots analysis is a cross sectional measure and thus ignores the timing of dropout. Although the shorter age range minimizes this problem, using a longitudinal measure would decrease potential bias by lining up riot occurrence with the dropout event. The event dropout rate measures the proportion of 9th-12th graders that were enrolled at some point in the previous school year but are not enrolled in the current school year. This longitudinal measure is not collected at the city level during the 1960s , but has become available in recent years. For the 1992 Los Angeles analysis, I calculate event dropout rates using district level data collected by the California Department of Education (CDE) through their October census surveys. This annual measure of dropout occurrence can be used to track yearly changes in dropout behavior and provides important information on how effective educators are in keeping students enrolled in school. Data is available from 1987-88 up to 2010-11. Note that unlike the census data, the CDE does not collect data on private school students, which make up a small percentage of the total student population in California.

Figure 2 shows the location of damaged structures and deaths in Los Angeles during the 1992 riots. The majority of damage and destruction is concentrated in the South Central area, however the riots reached up North towards San Fernando and down

South towards Long Beach. There are several deaths located outside of the city's boundaries, indicating that possible spillover effects may contaminate results. Based on the distribution of these riot-affected indicators across the city, it appears the riot was widespread and affected many neighborhoods in Los Angeles.

4 Using Riot Shocks As a Way to Assess a Neighborhood's Impact

I leverage the evidence of riots having city wide, immediate, and long-term effects on the various mechanisms that connect a community with the educational well-being of its residents to estimate the effects of lowered city quality on high school dropout rates. Through this strategy, we can make the claim that "a city experiencing a sudden shock to its quality at time t has higher/lower aggregate level resident dropout rates at time $t+1$[4]." This analysis does not allow us to identify the specific city characteristics that produce change in dropout rates. A riot changes an entire bundle of neighborhood

[4]A mathematical representation of the empirical strategy comes from a system of equations:

$$\begin{aligned} \text{Structural Model:} \quad & Y_{it} = \beta I_{it} + \mu \\ \text{Reduced Form Equations:} \quad & Y_{it} = \sigma R_{it} + \epsilon \\ & I_{it} = \begin{cases} 1, & \text{if } R_{it} \geq c \\ 0, & \text{if } R_{it} < c \end{cases} \end{aligned}$$

where R_{it} is a measure of riot occurrence at time t for city i, c represents a cutoff determining riot occurrence status, I is a a binary variable indicating whether or not quality decreases ($I = 1$) or remains the same ($I = 0$), and Y is the dropout rate. Note that when $R \geq c$, community quality Q goes down, where $Q = f(X)$, a function of various city characteristics X, or the bundle of goods that make up community quality. Since riots are a one time shock, I in time period t before a riot is 0 and after is 1. Unless we use R as a proxy for Q, our model only estimates the effect of a decrease of Q on Y rather than directly estimating the change in Y caused by a specific unit q decrease in Q. The reduced form equations express the endogenous variables Y and I as a function of the exogenous variable R. In this paper, we are directly estimating the reduced form parameter σ.

characteristics, and I am estimating the average effect of this change.

In the following sections, I present estimates of the average impact of a riot shock on dropout rates for cities that experienced a riot in the 1960s and for Los Angeles in 1992. In the 1960s analysis, I use 1950-1980 Census data to derive estimates from three models: a basic OLS using riot *severity* as the main independent variable, a regular DID regression using riot *occurrence* to separate cities into treated and non-treated conditions, and a synthetic control estimator that matches each riot-affected city with a weighted average of non-affected cities. In the Los Angeles riots analysis, I employ two models: the synthetic control procedure to yearly 1987-2005 California administrative data and a DID regression at the Census tract level to estimate local effects. The extent to which the results are consistent across these specifications ensures the robustness of the overall findings.

4.1 1960s Riots

The 1960s riots analysis is conducted at the city level for two primary reasons. First, most studies on the causes and effects of riots, including Carter (1986) and Margo and Collins (2007) from which the data used in this study are derived from, have generally relied on cities as the units of analysis. I use city-level data to remain consistent with this literature. Second, outcome and control data for spatial units below the city are largely unavailable during the time period. Ideally I would use individual level data but existing public-use microdata from the 1960 census do not contain city-level codes.

4.1.1 Ordinary Least Squares Regression

The variable of interest in this analysis is one that measures the level of riot activity in a city. A city with higher levels of riot severity have lower levels of quality. If we believe that riot occurrence is unpredictable and random across space and time, I can conduct a basic ordinary least squares (OLS) regression with education outcome Y as the dependent variable and riot severity S as the independent variable. The coefficient on the variable S yields the causal effect of lowered city quality on education outcome Y. The identifying assumption is that riot occurrence decreases city quality and is uncorrelated with city-level non-enrollment rates.

Exogeneity of riots across time is justifiable. Chronologies and narratives suggest that riots were sparked by routine events that turn into minor altercations that eventually turn into full blown riots. For example, in the 1965 Watts riot, the arrest of an intoxicated black motorist led to a wider altercation with neighborhood residents and eventually into a riot that killed over 30 individuals. A city doesn't prepare itself for the after effects of a riot much like it does when preparing for a natural disaster. This is important since preparation may alter pre-treatment variables, contaminating the time-specific first-difference estimates.

However, the assumption that S is randomly assigned across space is tenuous. Riots likely occur in cities with specific characteristics. If these characteristics are also associated with lower or higher non-enrollment rates, I will obtain biased estimates of the neighborhood effect. Spilerman (1970, 1971, 1976) argues that only a city's black population size and region predict riot occurrence and severity. Subsequent studies have attempted to refine and extend these results, but have not been able to identify additional variables that consistently predict riot occurrence and severity. Given this, we can claim that rioting is a random function of the number of Blacks and region. City-level Black population levels are not available in the 1950 census. Therefore, I use the percent non-white, which is an appropriate proxy of the Black population since minority populations excluding blacks were relatively small during the mid 20th century.

Although the riot cause literature points to region and Black population size as the only variables to control for, we can include additional variables in the model to ensure the proper identification of the effect. I include control variables for log population size, the percent of the civilian population that is unemployed, the percent of 25-year olds with a high school degree and above, and median housing value for owner-occupied units. These variables should capture otherwise unobservable trends that correlate with city quality, which correlates with riot propensity and the quality of education in a neighborhood (e.g. higher shares of neighbors with superior credentials can serve as positive role models and norm-setters, whereas higher shares of dropouts can have the opposite impact). I also include the 1960 log non-enrollment rate to control for pre-shock levels of the outcome variable.

I begin with the following OLS regression

$$\Delta log(Y_i) = \alpha + \sigma_{ols} log(S_i) + \theta X_i + \epsilon_i \tag{1}$$

where $\Delta log(Y_i)$ is the change in city $i's$ natural log non-enrollment rate from pre to post-riot, X_i is a set of control variables measured pre-riot, and S_i is a continuous variable measuring the severity of a riot as described in section 3.1. The distribution of S is highly skewed as the majority of cities had minor riots while a few had severe ones. In order to correct for this, I use the logarithm of the riot index [5]. The advantage of using a continuous riot variable is that it adds precision to the estimate of the effect. A clear approach to understanding σ_{ols} is to exponentiate it using a percent increase in riot severity as the base. By doing so, we interpret the treatment effect as an elasticity. For example, a 10 percent increase in riot severity yields a ($1.10^{\sigma_{ols}}$-1) x 100% percent change in the non-enrollment or dropout rate. Robust standard errors are calculated to control for potential heteroskedasticity.

The identifying assumption for equation (1) is that conditioned on X_i, cities find themselves in their observed level of riot severity essentially by accident. I assert that this assumption is plausible given that previous research has consistently found only a small set of variables that correlate with riot occurrence and severity. The pre-riot year is 1960 and the post-riot periods are 1970 and 1980. I estimate separate models for changes from 1960 to 1970 and 1960 to 1980, which measure short and long-term effects, respectively.

Table 4 reports OLS results for the 1960-1970 and 1960-1980 periods. In columns 1 and 2, I report the 1960-1970 model controlling for just Spillerman's variables and including the expanded covariate set. The coefficient for the 1960 rate variable is negative,

[5] Since the natural logarithm of 0 is undefined, I added a small value to S_i for cities having no riot activity

indicating that rates generally decreased between 1960 and 1970. Most importantly for the purposes of this study, the positive and statistically significant coefficients on the severity variable indicates that riot shocks to city quality increased non-enrollment rates in the short term. A 10% increase in the severity of a riot increases city non-enrollment rates by approximately 0.10 percent. Columns 3 and 4 report results for the 1960 to 1980 time period. We find that riot-shocked cities experienced long-term effects on their enrollment rates, although the impact is somewhat muted and not statistically significant.

In summary, the OLS models using a continuous measure of shock show that larger shocks generate increases in the non-enrollment rates from 1960 to 1970. However, the results show that the effect was short term as enrollment rates bounced back in 1980.

4.1.2 Difference-in-Differences

The advantage of the OLS model using a continuous specification of riot treatment is that it does not force the researcher to choose treatment and control conditions based on a cut-off. However, there are potential problems with this approach. First, there are several flaws with the riot severity index. For example, counts of destructive events do not necessarily correspond to how severely a riot impacted city quality. Therefore, several important components may be missing from the index. Second, the decrease in city quality produced by a riot shock may not affect non-enrollment rates linearly. Third, the OLS model does not take advantage of the panel nature of the data. Not doing so may introduce bias related to changes in the response variable over time due to secular changes happening concurrently with the riot shock. Lastly, it is unclear what a one-unit increase in severity means in practical terms. Rather than rely on the exact index values to measure a riot's impact on city quality, I follow the general strategy employed by Margo and Collins (2007) and the riot-cause literature by using a categorical specification of riot occurrence.

I employ a difference-in-differences (DID) model to obtain the effect, which requires a dichotomous definition of riot shock. The general idea of this estimator is to compare the change in the non-enrollment rates from pre to post-riot of affected cities to non-affected cities. The behavior change for the control (non-riot) group picks up any naturally occurring changes in behavior while the experimental (riot) group's behavior change reflects both the (same) naturally occurring change in behavior plus the impact of the shock. By comparing the time changes in the means for the riot and non-riot groups, both group-specific and time-specific effects are allowed for. The majority of the riot-cause literature assigns cities to riot and non-riot conditions according to the definition established by Spillerman (1970, 1971, 1976). Following in this tradition, I place cities with a riot severity index equal to 0 into the non-riot group and those with a value greater than 0 into the riot group.

Given the separation of the population into treatment and control cities and pre and post-treatment periods, I use the following DID model to estimate the riot treatment effect:

$$log(Y_{it}) = \alpha + \beta_1 D_i + \beta_2 P_t + \sigma_{did} D_i \cdot P_t + \theta X_{it} + \epsilon_{it} \tag{2}$$

where D_i is an indicator of riot occurence and captures possible differences between the treatment and control cities prior to the riot, P_t gives a value of one if the city-year observation is in the post-riot period and zero otherwise and captures aggregate factors that would cause changes in Y over time even in the absence of the riot, $D_i \cdot P_t$ multiplies the treatment city and year indicators (which is simply a dummy variable equal to one for those observations in the treatment group in the second observation year), and σ_{did} is the DID treatment effect. I calculate robust standard errors to minimize bias related to heteroskedasticity. The interpretation of σ_{did} in equation (2) is a city with a decrease in quality due to a riot experiences a σ_{did} change in non-enrollment rates *relative to a city not experiencing a decrease in quality.* The identifying assumption is that conditional on X_{it}, the non-enrollment rates for the riot-shocked and non-shocked cities must have parallel trajectories over the two time periods, 1960 to 1970 and 1960 to 1980, absent the shock.

Table 5 shows results for the DID models by year. As in Table 4, columns 1 and 2 present results for the 1960-70 period with just Spillerman's variables and including the additional covariates, and columns 3 and 4 present results for the 1960-80 period. Similar to the OLS findings, we find that non-enrollment rates generally decreased over the period as evidenced by the statistically significant negative coefficient on the *Shock Year* variable. The coefficient on the interaction of *Shock Year* and *Shock City* measure the effect of a shock on city-level log non-enrollment rates. We find that the decrease in the non-enrollment rate from 1960 to 1970 was 12 percent lower in shocked cities than in non-shocked cities. We find that the effects do not disappear in the long term, as non-enrollment rates in shocked cities decreased by 12 percent less compared to non-shocked cities from 1960 to 1980.

In summary, results from the DID models indicate that a shock to city quality depresses the decrease in non-enrollment rates in both the short and long term. Although we cannot compare the effect sizes directly since one model uses a continuous version of riot shock and the other uses a binary version, both the OLS and DID models arrive to the general conclusion that a shock to quality has negative effects on the schooling persistence of city residents. Results can only be contested if there is an unobservable difference between the riot-affected and non-affected cities or another change not related to the riot shock and not controlled for in X_{it} affected the groups differently and also happened between the 1960-1970 and 1960-1980 periods. Since the time trajectory of a riot is quite random and largely exogenous, this seems unlikely.

4.1.3 Synthetic Matching

The DID model does not make explicit a comparison group for each individual city. There may be differences between cities within control and treatment groups in their time trends that are not captured by the covariates X_{it}. We can improve upon the DID model by using a less ad-hoc way of selecting control units that avoids the type of extrapolation exercises that regression results are often based on. To address this issue, I employ a strategy - synthetic control matching - developed by Abadie et al. (2003, 2010) that constructs a weighted control group using a data driven procedure.

The main idea of this method is to choose for each city in the treatment group a weighted average of cities in the control group, which Abadie terms a synthetic match.

The weight attached to each control city is based on how closely it resembles the treated unit on the outcome variable and across selected demographic variables during the pre-treatment period[6]. The effect of the riot can be measured as a function of the difference between the behavior of the city and its synthetic match after the riot. Abadie et al. (2011) show that a primary reason to use this method is to control for the effect of unobservable factors that have an effect on the common time trend of samples in the treatment and control groups.

Formally, the synthetic matching procedure is as follows. The observation pool consists of N cities with a population greater than 50,000, separated into treatment and control groups based on D_i, with J treatment cities and $N - J$ control cities. X_{it} is a matrix of covariates and Y_{it} a matrix of outcomes for city i measured at time t. Suppose we observe these units over T time periods, where the riot occurs at time T_0, t_1 designates pre-treatment period (1950 and 1960) and t_2 designates post treatment periods (1970 and 1980). For treatment city 1, let $Z_1 = [X_{1t_1}, Y_{1t_1}]$ be a matrix containing pre-treatment covariates X and pre-treatment outcomes Y. Let Z_0 contain the same variables, but for the entire set of $N - J$ potential control candidates. The synthetic control method identifies a convex combination of the $N - J$ cities in the candidate pool that best approximates the pre-intervention matrix for the treatment city.

The goal is to construct a weight matrix W that will be used to combine all $N - J$

[6] Abadie, Diamond, and Hainmueller (2011) show that a basic regression also uses a linear combination of control units with coefficients that sum up to one. However, regression does not restrict the weights to be between zero and one, therefore allowing extrapolation outside the support of the data. A key difference between the two methods is that in a basic regression all control units factor into calculating the treatment effect for a particular city, while only control units that closely resemble the treated unit are used in a synthetic control regression.

control units into a single unit. I choose W such that it minimizes the distance between Z_1 and Z_0. Specifically, I choose a W that minimizes $\sqrt{(Z_1 - Z_0W)'V(Z_1 - Z_0W)}$, where V is set to minimize the the mean squared prediction error of the outcome variable during the pre-treatment period. The values of W yields a synthetic comparison group that best approximates the pre-intervention period for the treatment city.

After obtaining the weighting matrix W, I construct the control post treatment outcome $Y_{0t_2}W$ and estimate a DID estimate $\widehat{\sigma}_1$ of the riot that occurred in city 1

$$\widehat{\sigma_1} = (Y_{1t_2} - Y_{1t_1}) - (Y_{0t_2}W - Y_{0t_1}W) \tag{3}$$

I follow the same procedure for the rest of the J treatment cities. Previous studies employ synthetic matching for the case of one entity in the treatment group and one intervention (Abadie and Gardeazabal 2003, Abadie et al. 2010; Hinrichs 2011; Montalvo 2011). However, since my sample includes more than one riot-affected city I extend this method for the case of many cities in the treatment group. I take the average of the individual city treatment effects calculated in equation (3) to obtain the final estimate of the average treatment effect

$$\widehat{\sigma_{synth}} = \sum_{i=1}^{J} \sigma_i / J \tag{4}$$

At its heart, the synthetic control model is a combination of matching and difference-in-differences - control units are chosen based on how closely they resemble the treated unit in the pre-treatment periods. The idea is to obtain an appropriate control city per treatment city, where the control city is a weighted average of potential control cities. The method generalizes the DID approach by allowing for time-variant unobserved confounders. In this respect, the estimates are not only robust to time-invariant unobservables, but also unobservables that vary over time.

To formally test the significance of $\widehat{\sigma_{synth}}$, I apply the exact permutation test suggested by Abadie et al. (2010), but modify it to accomodate for multiple treatment cities. I observe a riot in Los Angeles, Newark, Detroit, etc. but not in other cities. I can then map out the distribution of the null hypothesis of a no riot effect by randomly assigning riot treatment to cities that were not afflicted by a riot. I calculate $\widehat{\sigma}_{synth}$ for each randomly selected control city and find where $\widehat{\sigma}_{synth}$ for the treatment cities lies in that distribution. If it lies somewhere in the extreme, I can reject the null since the observed $\widehat{\sigma}_{synth}$ is too large relative to what I would see if control cities were assigned the treatment. The procedure is as follows

1. From the pool of N cities, there are J treatment and $N - J$ control cities. Eliminate the J treatment cities

2. Randomly select a control city i from the $N - J$ cities. Consider this city the treatment city.

3. Randomly select $N - J$ cities with replacement from the remaining $N - J - 1$ control cities. This is city $i's$ control group.

4. Estimate σ_i for the selected control city i using the synthetic matching procedure.

5. Do steps 2-4 J times. Compute $\widehat{\sigma}_{synth}$ using equation (4).

6. Do the above procedure K times.

7. Using the K estimated coefficients, find the confidence intervals of 1%, 5%, and 10%.

The permutation procedure reveals an additional way the synthetic control method augments the regular DID model. The method leverages the large pool of potential controls to obtain inference in a manner that is robust to the possibility of city-by-time period specific shocks. In other words, it accounts for the fact that, even if we observed the entire population for each unit, there would still be some deviation between the treated unit and its synthetic control because there are aggregate shocks that occur at the unit-by-time level.

Figure 3 displays the average log non-enrollment rate trajectory of riot-affected cities and their synthetic counterparts for the 1950 to 1980 period. The blue line, which represents the average log non-enrollment rate for riot-affected cities, matches closely to its synthetic counterpart in 1950 and 1960 but diverges in 1970. Although the non-enrollment rates continue to decrease after 1970 in both cities, it does so at a slower rate in riot-affected areas. The difference between the two lines continues into 1980, indicating that a riot shock has both short and long-term effects. These findings match the conclusions derived from the OLS and DID models. The mean log non-enrollment rates in riot-affected cities and their synthetic matches are presented in rows 1 and 2 in Table 6. We can compute the DID estimator $\widehat{\sigma}_{synth}$ using these synthetic estimates. The synthetic DID estimates for 1970 and 1980 are 0.100 and 0.126, respectively.

The clear break in the riot-affected and synthetic control trajectories depicted in Figure 3 is somewhat deceptive. I constructed the synthetic control unit so that it tracked each treated city closely in the pre-riot period. Consequently, the trajectories are likely to diverge in the post-riot period even if the divergence is not significant. Fortunately, I have a set of control cities to map out a null distribution of no effect that will allow me to determine whether the post-riot divergence is significant or not.

We can formally test the statistical significance of these estimates using the permutation test outlined above. Figure 4 displays histograms of 1,000 random permutations of the DID estimates in 1970 and 1980. The blue portion of the histogram in graph (a) of Figure 4 represents the percentage of the 1,000 permutations that have a value equal to or greater than 0.100, the estimated effect in 1970. The percentage of the total distribution that is blue represents the one sided p-value of a test of no riot effect. Row 4 in Table 6 present these p-values. Using a confidence level of 5%, we can conclude that a riot shock does have a statistically significant effect on log non-enrollment rates in the short and long term. In summary, we find that the synthetic control method corroborates the general findings from the OLS and regular DID models: A riot-affected city in the 1960s experienced higher non-enrollment in the short and long term.

Table 7 summarizes the riot shock effect estimates by model and year. The first row reports the cross-sectional differences in the mean outcome between riot-affected and non-affected cities. We would rely on these estimates if we believe that the occurrence of a riot is entirely exogenous. The second row reports OLS estimates using riot severity as the treatment effect variable. The third row reports regular DID estimates while the last row reports DID estimates using the synthetic matching procedure. All values reported in parentheses are two sided p-values for tests of no significance. The cross-sectional mean difference indicates that a riot shock leads to a 25% difference in the non-enrollment rate in both years. Controlling for potential bias reduces the effect sizes by roughly half. The regular DID and synthetic control DID models report a 10 to 12 percent difference in non-enrollment rates between riot-affected and non-affected cities in both years. These effect sizes are not trivial since the population of 16 and 17 year olds in many of these cities are quite large. The OLS models, which use riot severity rather than occurrence, indicate that a 10 percent increase in severity leads to a fairly modest 0.12 percent increase in the non-enrollment rate in 1970 and a non statistically significant effect in 1980.

With the exception of the 1980 OLS estimate, the riot shock effect sizes are statistically significant across all models and years. The general conclusion from the analysis is that riot shocks on community conditions in the 1960s had a non-trivial negative impact on the non-enrollment rates in cities in the short and long term. I tested the robustness of these findings in the following ways. First, I add controls for higher-degree powers of log population size. The motivation is to assess if results are driven primarily by very large populations. Second, I eliminated non-shocked cities that bordered a shock city to control for potential spillover effects. If there are spillover effects, then the estimates are understated. Lastly, I eliminated three possible outliers with the greatest riot severity (Washington (0.369), Detroit (0.494), and Los Angeles (0.521)). None of these changes significantly altered the estimates in Table 7.

Although the results are consistent across several modelling specifications and passed a battery of robustness tests, we must interpet these findings as suggestive for the following reasons. First, the analysis does not provide estimates for the specific city-level mechanisms that affect non-enrollment rates. Second, the data are not detailed enough to allow for more localized analyses. Although a riot may have an effect on an entire city, the effects I find may be driven by highly segregated, black and poor neighborhoods[7]. Third, Myers (2004) speculates that riots may have an affect on poor black neighborhoods nationwide. Riots may create the general impression that certain neighborhoods are poor prospects for development, thus non-riot affected neighborhoods similar in character to those affected may experience indirect negative consequences, contaminating the control group. Lastly, in response to to the riots, the federal government could have diverted funds towards non-riot affected cities to riot-affected cities, artificially understating the effects.

4.2 1992 Los Angeles Riots

The analysis of the 1960s riots shows that riot severity and occurrence have short and long-term effects on city-level non-enrollment rates. The 1960s were a unique time in U.S. history given the dramatic societal, financial, and political changes sweeping the nation during that era. Would a contemporary riot shock yield such negative effects

[7]In a case study of 1960s Cleveland, Margo and Smith (2004) find greater decreases in property values and higher mobility in census tracts closest to a riot's epicenter

on the educational health of a city? To help address this question, I use the 1992 Los Angeles riot to estimate the effects of decreased city quality on log dropout rates. To determine whether the riot had local effects, I estimate the impact of the shock at the census tract level.

4.2.1 Synthetic Matching

In order to assess the effects of the diminished conditions in Los Angeles after the 1992 riot on dropout rates, I use a comparative event study approach. Unlike the 1960s analysis, I only have one treatment city, Los Angeles, but many potential control cities. Abadie and Gardeazabal (2003) originally formulated the synthetic control method for such instances. Abadie et al. (2011:3) elaborate on the empirical basis for the method: "The synthetic control method is based on the observation that, when the units of analysis are a few aggregate entities, a combination of comparison units (which we term "synthetic control") often does a better job reproducing the characteristics of unit or units representing the case of interest than any single comparison unit alone." The method makes explicit not only which cities are being compared to Los Angeles, but the weight with which each of the cities are factored into the comparison. I construct a synthetic Los Angeles, i.e. a non-riot affected Los Angeles, as a convex combination of other cities chosen to resemble the values of dropout rates and covariates for Los Angeles prior to the riot in 1992.

In addition to providing contemporary results, the Los Angeles case study provides more years of data. Rather than solely relying on the decennial census to obtain dropout rates as I do for the 1960s riots, I use district level administrative data collected by the CDE. Ideally, I would include all U.S. cities in my potential synthetic control pool, but the federal government did not collect dropout data from a significant number of states before 1992. Therefore, I reduce the donor pool to only California cities. Although the restriction reduces the leverage obtained from using a large number of cities to match on, it has the distinct advantage of controlling for state and local policy and administrative factors, such as funding and teacher hiring, that may affect dropout rates. I have data for each school year 1987-88 up to 2004-05. Using yearly data allows me to estimate immediate (e.g. 1992) and longer term effects (e.g. 2002). The pre-treatment years used in the method are from 1988 to 1991 and the post treatment years are from 1992 to 2005.

Similar to the 1960s riots analysis, I use pre-riot values for total population, unemployment rate, the percent 25 years and older with a high school degree, and the median housing value to construct a synthetic match. Following Bergesen and Herman (1998), I break out the percent non-white variable into percent Hispanic, Black, and Asian to capture the multi-ethnic nature of the riots. Although the white-black tension garnered the majority of media coverage, similar levels of racial tension occurred between Koreans and Blacks and Latinos and Blacks (Webster and Williams 1992; Baldasare 1994; Pastor 1995). These variables were obtained from the 1980 and 1990 decennial census. I also match on pre-riot log dropout rates measured each year between 1988 to 1991.

The public school dropout data are collected at the school district level. Since district boundaries do not match up with city boundaries, I use GIS software to match

2000 unified and high school district level boundaries with 1990 city boundaries[8]. The majority of cities contain only one unified and high school district, however few cities contain multiple districts, with several of them crossing city boundary lines. Therefore, I assign a district to a city if the union of their areas is greater than or equal to 50% of the total area for that district[9].

I exclude cities that share a boundary with Los Angeles to control for possible spillover effects. I also exclude cities that do not have dropout data for all years 1988 through 2005. Lastly, I exclude cities that did not report a population of at least 50,000 in the 1980, 1990 or 2000 decennial census to avoid the inclusion of consistently small cities. I am left with a sample of 88 potential control cities.

In a regular DID model, all cities in the control group receive a non zero weight. The synthetic control method provides positive weights to only cities that match up well with the treated city according to a set of pre-treatment values. Additionally, the method makes transparent the contribution of each city to the counterfactual unit. Table 8 shows the weights of each city in the synthetic version of Los Angeles. Synthetic Los Angeles is a weighted average of 3 cities: Rialto and San Bernardino, neighboring cities located east of Los Angeles, and Indio, located 125 miles east of Los Angeles.

[8]Ideally, I would like to match city and district boundaries from the same year , but 1990 school district boundaries are not publicly available. Changes in district boundaries typically occur in smaller cities where population continues to shift and expand. Since I am using large cities whose own boundaries change very little from 1990 to 2000, it is safe to assume the same districts serve these cities in both 1990 and 2000.

[9]For example, the attendance areas for districts Z and Y overlap with the area of city X. The union of the areas of city X and district Z make up 100% of district Z's total area (i.e. all of district Z's area is found within city X's area). However, only 15% of district Y's area contains the union of district Y and city X because it also enrolls students from an adjacent city. Therefore, district Y is not included in the calculation of city X's dropout rate. If the union was greater than or equal to 50%, *all* of its students, even if the district partially serves an adjacent city, is included in the dropout rate for city X

The table also provides the average pre-riot values for the log dropout rate and various demographic variables. While none of the cities match Los Angeles' population size, they compare favorably on other demographic characteristics.

Figure 5 displays the log dropout rate in Los Angeles, its synthetic counterpart and California minus Los Angeles from 1988 to 2005. Using the rest of California would not be an appropriate comparison group since its log dropout rates pre-1992 are significantly lower than Los Angeles. In contrast, we find that Los Angeles and its synthetic match have similar log dropout rates in the pre-treatment period. Using the permutation test outlined in section X, I obtain p-values for one-tailed tests determining whether any differences in log dropout rates between Los Angeles and its synthetic match in the years 1988 to 1991 are statistically significant. The permutation tests yield p-values of 0.098, 0.28, 0.153, and 0.074 for each year between 1988 and 1991. These results indicate that differences between Los Angeles and its synthetic match during the pre-riot period are either marginally or not statistically significant.

Figure 5 shows that dropout rates in Los Angeles and its control diverge quite significantly beginning in 1992. While the synthetic control witnesses a decrease in dropout rates after 1992, Los Angeles maintains its relatively high rate for several years before dropping in the mid 1990's. After 2000, both Los Angeles and its match experience similar rises in dropout rates.

Throughout most of the post-riot period, the statewide (minus Los Angeles) and synthetic control dropout trajectories are similar. Before 1992, statewide dropout rates excluding Los Angeles hovered around 5%. In 1992, the rate is 4.3%, drops to 2.7% by

1998 and rises thereafter. While the Los Angeles' synthetic match follows the general trajectory of the state post-1992, Los Angeles dropout rates remain stagnant before dropping in the mid 1990's. Based on this evidence, the riot shock kept Los Angeles dropout rates at its pre-riot levels, at least until the mid 1990s. Without the shock, the drop we see in Figure 5 starting in 1995 would have occurred earlier.

Figure 6 maps out the difference in dropout rates between Los Angeles (black line) and its synthetic match and all 88 control cities and their synthetic matches (gray lines). The control cities receive a placebo treatment - I use the synthetic control procedure on these cities as if they had a riot in 1992. If we expect the divergence between Los Angeles and its synthetic match to represent a real treatment effect, than the black line should be located in the outer edge of this distribution. The fact that the Los Angeles line is at or near the top after the riot suggests that the effect of the riot shock is not simply due to chance[10] .

The synthetic control DID estimator $\widehat{\sigma_{synth}}$ expressed in equation (4) is defined as the difference in the average log dropout rate in Los Angeles before and after the riot minus the difference in the average log dropout rate in synthetic Los Angeles before and after the riot. The first row of Table 9 reports the DID estimator and indicates that a shock to Los Angeles' neigborhood quality had a negative impact on the schooling persistence of its high school students. The 2nd and 3rd columns show the average log dropout rates in Los Angeles and its synthetic match, respectively. The shock induced

[10] Abadie et al. (2010) suggests eliminating cities with a pre-riot mean squared prediction error (MSPE), which is defined as the mean squared gap between the "treated" city and its synthetic control, that is twice as large as Los Angeles. Eliminating the five cities with poor pre-riot MSPEs does not alter the results

a 64% difference in the average dropout rate in Los Angeles compared to its synthetic match during the post-riot period.

While row 1 presents an estimate of the average effect over the entire post-riot period, the rest of the table presents estimated effects for each post-riot year. Averaging over the entire post-riot period may mask differences between short and long-term effects. The DID estimates are positive for all years, indicating that the shock negatively impacted aggregate level dropout rates in Los Angeles' in the short and long term.

We can formally test the statistical significance of these results by following the exact permutation test procedure applied in the 1960s analysis. The inferential exercise is exact in the sense that regardless of the number of available comparison cities and time periods, it is always possible to calculate the exact distribution of the estimated effect of the placebo riots. More generally, the test examines whether the estimated effect of the Los Angeles riot shock is large relative to the distribution of effects obtained if we randomly pick an untreated city and pretend a riot occurred in 1992.

Figure 7 presents a histogram of 1,000 permutations for the synthetic DID estimate. The blue portion of the histogram represents the probability that we find an effect as large as the one witnessed in Los Angeles *if we assume that the riot had no effect at all.* The distribution represent the null hypothesis of no effect and the blue portion of the distribution represents the p-value of a one-sided test of this null hypothesis. We find that the probability of finding an affect as large as the one in Los Angeles (0.640) is relatively small. Figure 8 presents histograms of 1,000 permutations for the years 1992, 1993, 2002 and 2003. The probability of an equal or larger effect is small in the short term (1992 and 1993), but relatively large in the long term (2002 and 2003).

The last column in Table 9 provides one sided p-values calculated from the 1,000 permutations for the DID estimator for the average of the entire period and each individual year 1992 to 2005. Using a confidence level of 5%, we find that the DID estimate for the entire post-riot period is marginally significant. However, we find that the DID estimates are statistically significant in the two years (1992-1993) after the riot shock, but largely not statistically significant thereafter. These inferential tests formalize the results found in the previous figures, namely that the riot shock had an immediate, short-term impact on dropout rates, but no long-term consequences. Los Angeles dropout rates were impacted the first two years after the riot, but bounced back thereafter.

4.2.2 Census Tracts

A riot shock may affect cities at a more local level. I could not test this hypothesis using the 1960s riots data due to the unavailability of riot occurrence and severity mapped at a lower spatial entity than the city, but I can do so for the 1992 Los Angeles riot. A tract is defined as a recognizable and homogeneous geographic unit with relatively permanent boundaries and an average population of 4,000. Tracts have been traditionally used as the best geographic measurement of neighborhood, although its popularity can be largely traced to their convenience. By using tracts as the unit of analysis, we can determine whether the decrease in community quality caused by the Los Angeles riot had a more localized effect.

Tracts within Los Angeles are categorized as treated if it contains a riot related damaged building or death. I use three sets of control tracts. The first includes tracts within the city of Los Angeles that do not contain a riot related damaged building or death. We may be concerned with possible spillover effects onto non-riot affected tracts, dampening the estimated treatment effect. Baade et al. (2007) find that the riot had a long-lasting negative impact on the economy of the City of Los Angeles but not the County of Los Angeles. Therefore, the second set of control tracts are those not within Los Angeles' city boundaries but within Los Angeles county. The last set of control tracts are those not within Los angeles city but within the state of California[11].

Using census data[12]. I apply the DID model using pre and post-riot years of 1990 and 2000. I use the same set of control variables from the city-level analysis with the exception of median housing values, which are not available at the census tract level and thus I use mean housing values.

Table 10 displays DID estimates of the riot shock on tract level log status dropout rates. Column 1 presents results using only riot and non-riot affected tracts within Los Angeles city. I find that the coefficient on the *Riot Year* x *Riot City* interaction is not significant. We may be concerned that the results are affected by spillover effects. Tracts close to riot treated tracts may also be affected rendering them as inappropriate control units. To alleviate this problem, I exclude all tracts within Los Angeles city

[11] There are a few affected tracts not in the Los Angeles city boundary. These tracts were excluded from the Los Angeles county and California analyses

[12] While the assignment of districts to cities is relatively straightforward, their assignment to census tracts is significantly more difficult. The majority of cities in California generally encompass one district. However, district and tract boundaries intersect. Rather than making potentially flawed assignment assumptions, I use status dropout rate data available at the tract level as collected by the Census. Therefore, I cannot utilize the yearly district data used in the city-level analysis, but restrict the tract analysis to the years 1990 and 2000.

boundaries, and use tracts either in just Los Angeles county or in the entire state of California as my control units. Columns 2 and 3 present these results. A number of the coefficients on the control variables are significant and contain expected signs. For example, higher percent Black increases log dropout rates, while higher percent Asian and high school graduates decrease rates. As in the Los Angeles city model, the estimates for the *Riot Year x Riot City* dummy are not significant in either specification.

In summary, I do not find a localized effect of the riot on log dropout rates. One must be wary about drawing inferences from administratively defined neighborhood boundaries, as the shock may have effects on other local geographic entities. We also cannot determine whether the shock had a short-term effect since yearly dropout data at the tract level is not available. Additionally, I cannot make a direct comparison between the census tract and city-level results since I use related but operationally different measures of dropout rate in each analysis. With these caveats in mind, these results provide support to the claim that a riot shock does not affect local areas in the long term.

5 Discussion and Conclusion

In this article, I study the effects on city-level adolescent dropout rates of two separate shocks on city quality, the civil rights riots of the 1960s and the 1992 Los Angeles riots. Both shocks were largely exogenous, sustained events affecting the various amenities that make up the quality of a city. I find that a riot occurrence in the 1960s generated a 10-13 percent difference in city non-enrollment rates in the 1960-1970 and

1960-1980 periods. I find that the Los Angeles riot depressed the decrease in dropout rates by 60 percent post-1992, but the effects were concentrated in the first two years after the riot when the difference in dropout rates were 52 and 11 percent. The general conclusion from the analysis is that a disruptive shock to city conditions has a negative effect on the educational persistence of adolescent residents.

The results of the 1960s analysis is robust to various model specifications. Nearly all of the evidence I have uncovered points in the same general direction, namely that cities experiencing riot shocks witness higher non-enrollment rates in the short and long term. Margo and Collins (2007) theorize that the consequences of the riots on affected cities were reinforced by the declining influx of wealthier residents and a lack of long-term business revitalization post-riot. Wilson (1996) describes this process as the further ghettoization of a community, which he links to social isolation theory. Poor inner-city neighborhoods are thought to be socially isolated from mainstream or middle class individuals and institutions, leading to cultural isolation and the development of a ghetto specific culture, which orients young people away from schooling by reinforcing norms and values that denigrate the value of education (Harding et al. 2010). Given these severe downstream consequences, overall city quality, and consequently enrollment rates, in riot-affected cities diminished.

The results from the 1992 Los Angeles riots analysis paint a slightly different portrait. While riot shocks in the 1960s yielded short and long-term effects on city-level enrollment rates, the 1992 Los Angeles riot had a short-term impact on dropout rates but no long-term consequences. Potentially, school level factors or education focused rehabilitation programs were able to buffer the educational health of the city from the long-term downturn in city quality. It is also possible that the riot had a short-term effect on the entire city, but a long-term effect on certain smaller sectors of the community, such as the African American community or those socioeconomically disadvantaged. Unfortunately, I cannot test this hypothesis since dropout rates dissaggregated by race are not available before 1992.

Another factor potentially driving the differences in the results is the unique characteristics of the periods in which each riot took place. While the rash of riots in the 1960s were set against the backdrop of nationwide civil unrest, the 1992 Los Angeles riot was less national in scope. Los Angeles did not encounter rising segregation and economic inequality, and dramatic shifts in cultural and societal norms at the levels experienced in cities during the 1960s and 70s. These factors may compound the initial impact of a riot, thus producing deeper, longer lasting effects on the community. These explanations are purely speculative. Future research using a case study approach may help clarify the factors underlying short and long-term effects in both events as well as identify the precise causal mechanisms through which these effects operate.

The study adds to the growing literature revealing the negative societal implications of large negative shocks, such as natural disasters and macro-economic disturbances, on communities (Kirk 2009; Catalano 2010). The study also informs the general debate around the influence of community conditions on social outcomes by providing evidence supporting Wilson's (1987) claim that community quality has a distinct impact on youth well-being. Macro-level shocks are not necessary in order for quality to decrease within a community. The results of this study can apply to cities affected by sudden shocks like an urban riot but also to cities experiencing the type of gradual decay Wilson (1987) describes in his book. Diminished neighborhood conditions, regardless of the causes, do matter and have potentially serious short and long-term consequences for the youth residing within these neighborhoods.

We should also consider how we can apply these results to the current debate surrounding education reform. In this era of greatly increased focus on school accountability, the results of this analysis may compel education policy makers and leaders to be more cognizant of the external factors that can negatively influence student persistence. Large city-level riots, which occur from factors externals to schools, can significantly impact community level mechanisms that influence student persistence and are well beyond the control of teachers and school administrators. The significant effect similar shocks can have on a school's ability to meet accountability goals suggests that policymakers may want to consider community-wide factors when defining whether a school is meeting accountability targets.

Although the substantive findings of the analysis are meaningful in and of themselves, a methodological contribution of this study to the current neighborhood effects literature is its identification strategy. Often researchers interested in determining the impact of neighborhood conditions do not have the type of data required to causally link neighborhood characteristics and resident outcomes. Rather than relying on standard estimation procedures, which carry strict and often unrealistic assumptions, researchers should attempt to utilize more sophisticated methods to draw conclusions from their data. Instrumental variables, propensity score matching, and sibling fixed effects are example methods that may not necessarily solve all the mechanical issues surrounding neighborhood effects estimation but will get us closer to more powerful findings. In this analysis, I use plausibly exogenous negative shocks to city conditions to determine whether the diminished quality within a city leads to higher dropout rates. The strategy is not without problems; it tells us nothing about the effects of community conditions on younger children, who may be more susceptible to environmental conditions than adolescents, and it tells us that community conditions matter, but not *why* they matter. But it offers another angle of attack on a particularly difficult but important problem.

The analysis also illustrates the need to test results across different modelling specifications. In studies of neighborhood effects, robustness of results is desirable since analyses are often based on tenuous assumptions and less than ideal data. The consistency of results across different specifications provides a strong argument against the critique that the findings are merely artifacts of a set of modelling assumptions. I use two common estimation procedures, OLS and DID, on two sets of data from different time periods. I also use a relatively new method, synthetic control matching, that may be of significant use for future analyses of neighborhood effects. Although synthetic matching was developed specifically for aggregate level analyses, it can be extended to individual level data. Furthermore, the procedure may benefit studies where the mechanism altering neighborhood conditions is implemented at the neighborhood level (e.g. enterprise zones). Researchers should aim for randomized data, but in most cases they are not available. Instead of claiming defeat, we can still make reasonably strong claims about the connection between neighborhood conditions and resident outcomes using better analytic approaches.

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7 Figures

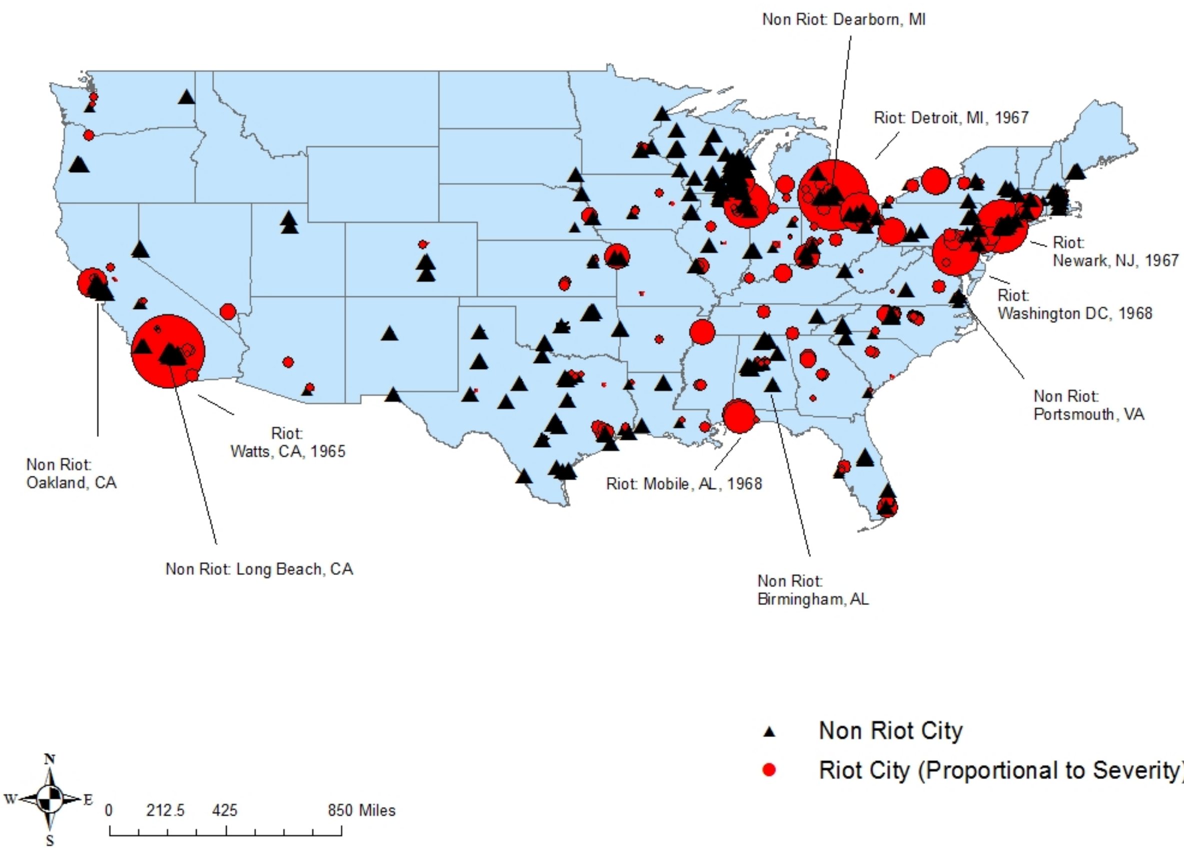


Figure 1: Cities > 50,000 total population affected and not affected during the 1960s Riots

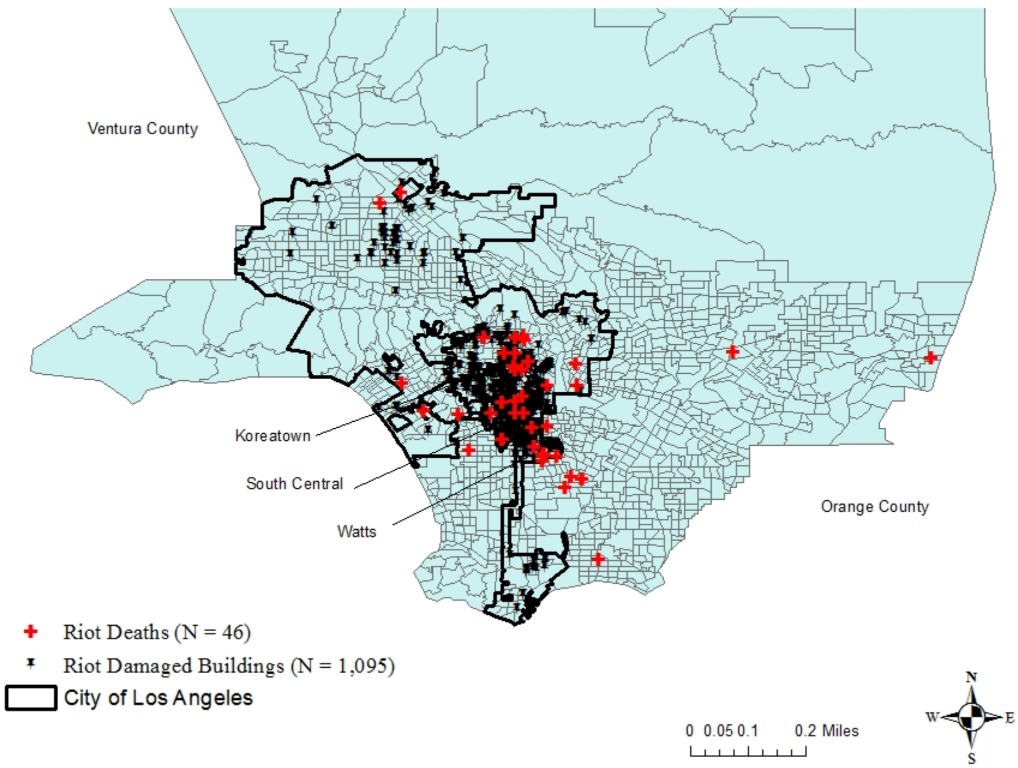


Figure 2: Locations of deaths and damaged structures during the 1992 Los Angeles Riots.

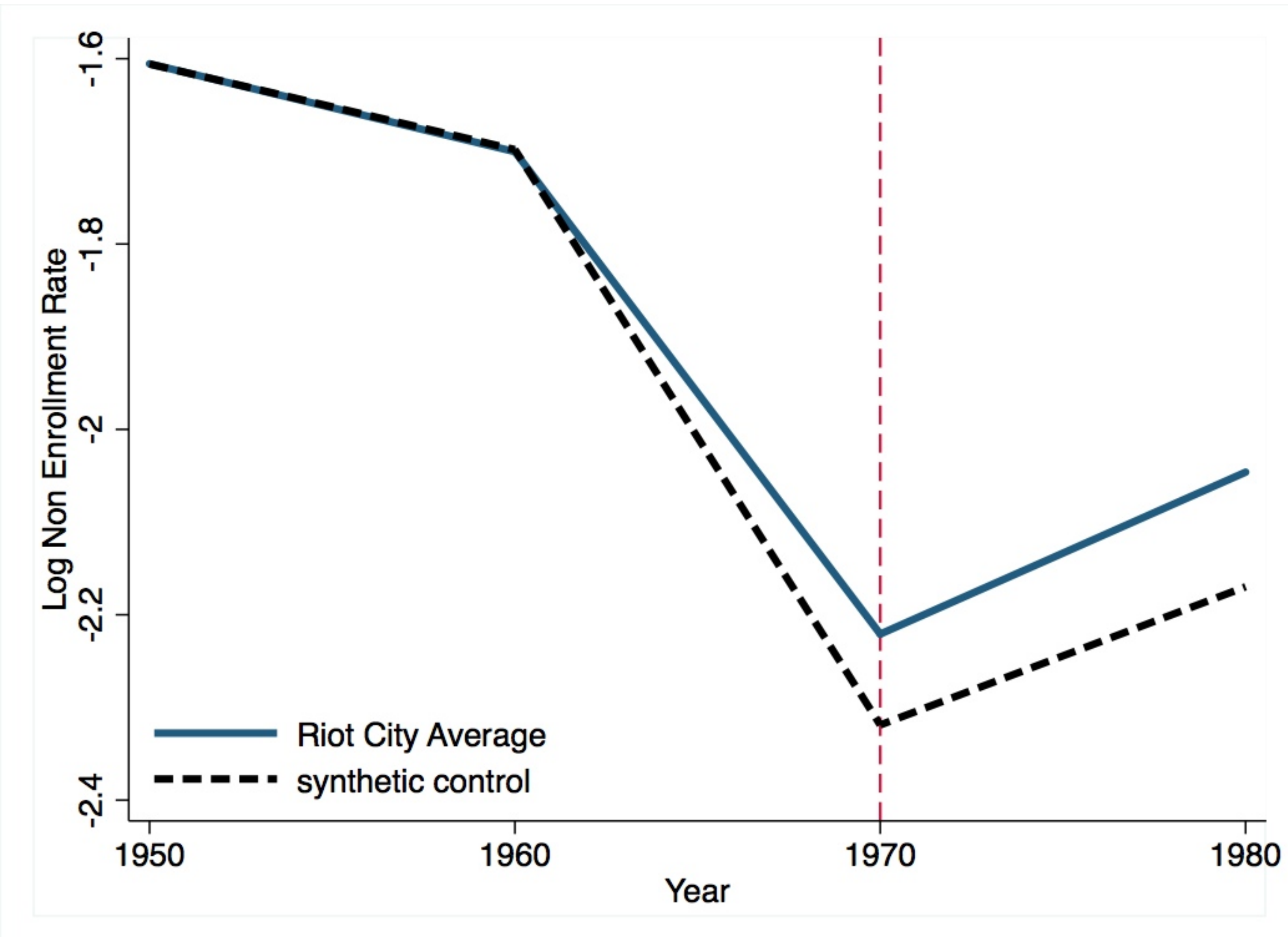
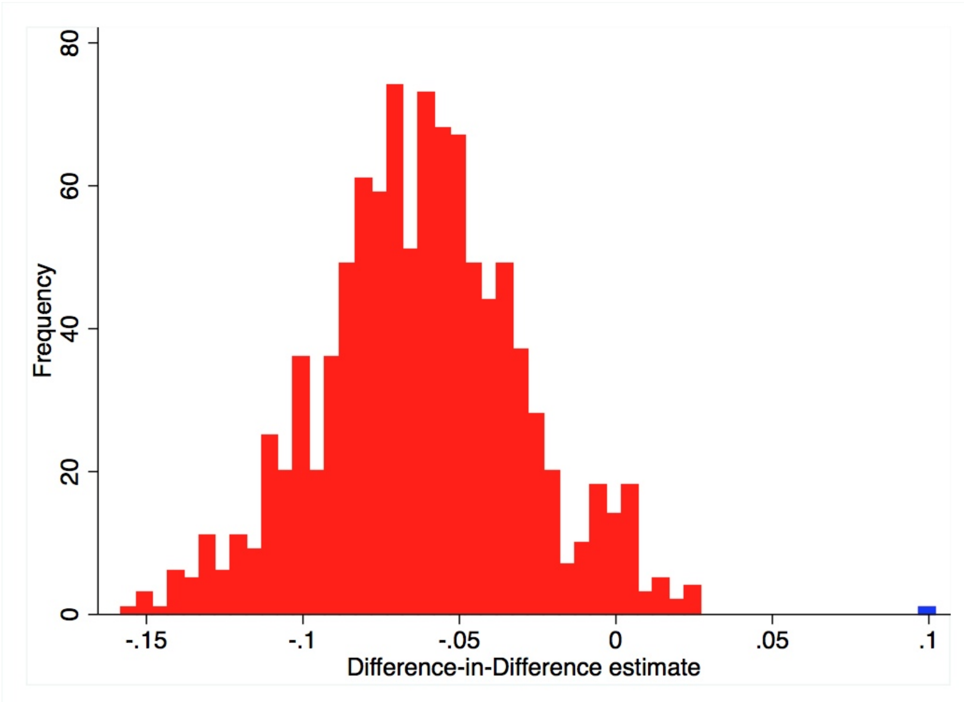
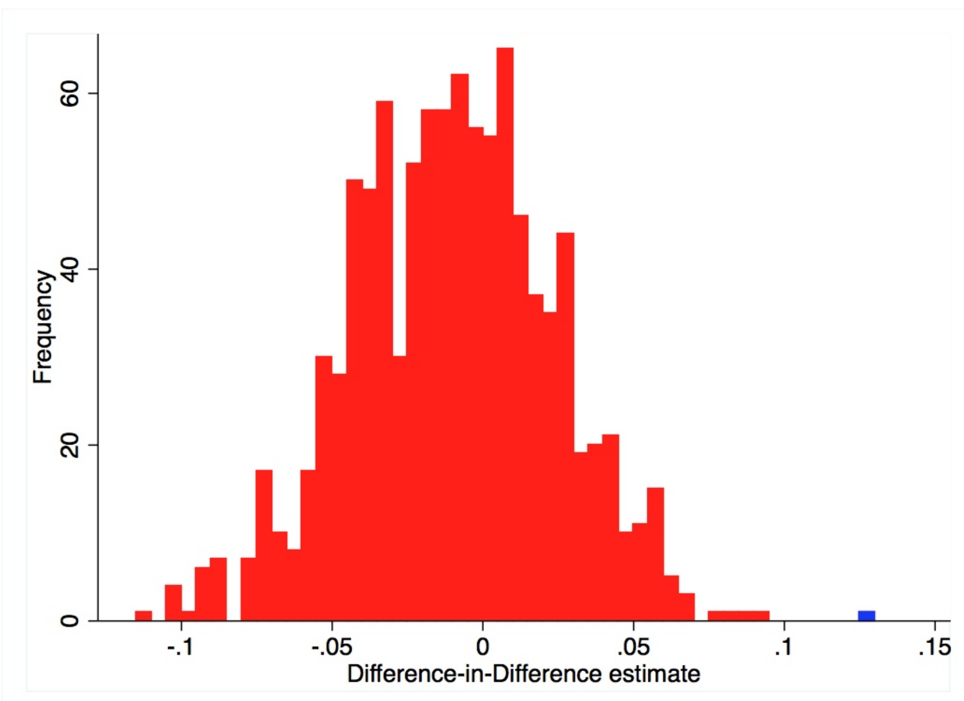


Figure 3: 1960s Riots Synthetic Control Matching: Trends in Log non-enrollment Rates - Riot Treated Cities vs. Synthetic Match



(a) 1970



(b) 1980

Figure 4: 1960s Riots Synthetic Control Matching: Histogram of 1,000 placebo permutations

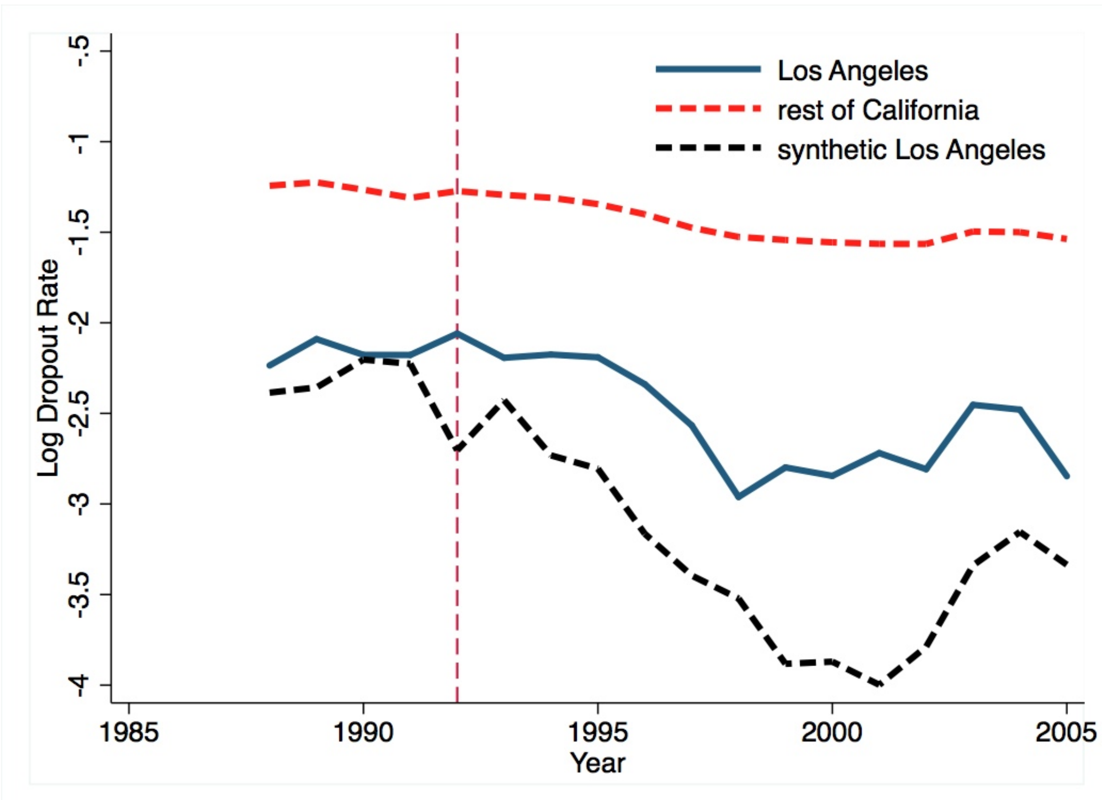


Figure 5: Synthetic Control Matching: Trends in Log Dropout Rates - Los Angeles, California minus Los Angeles, and Synthetic Los Angeles, 1988-2005

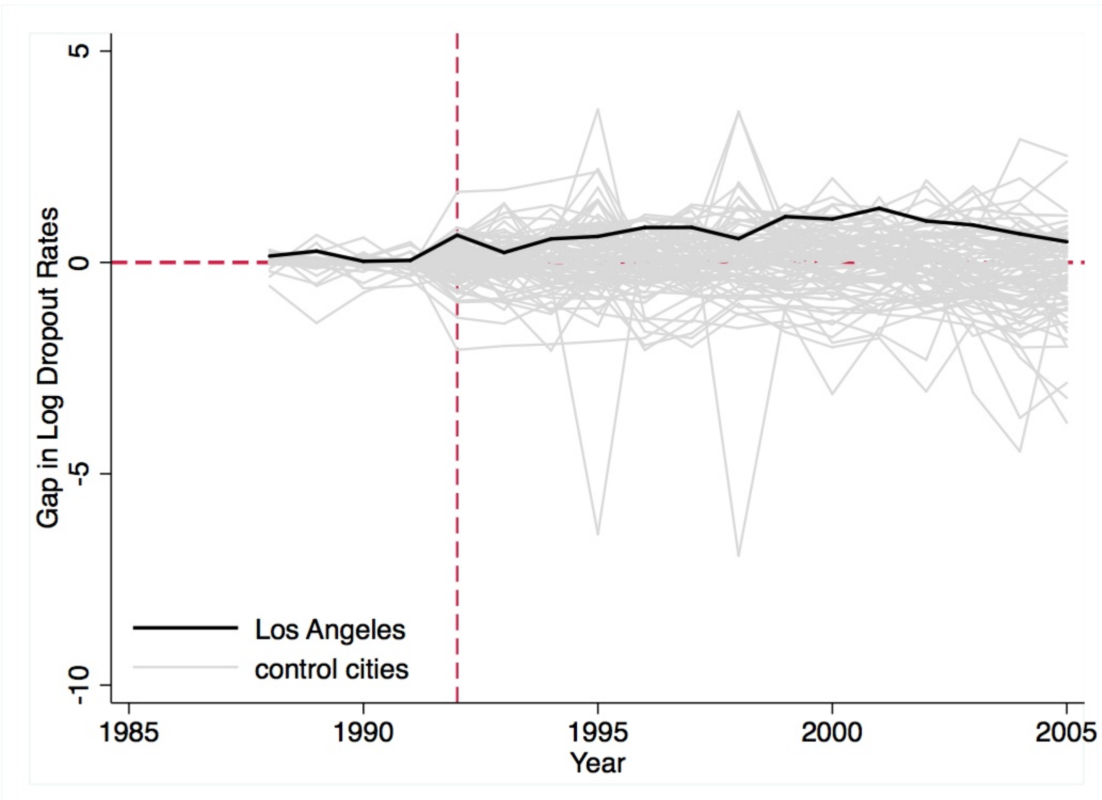


Figure 6: Synthetic Control Matching: Log Dropout Rate gaps in Los Angeles and placebo gaps in control cities, 1988-2005

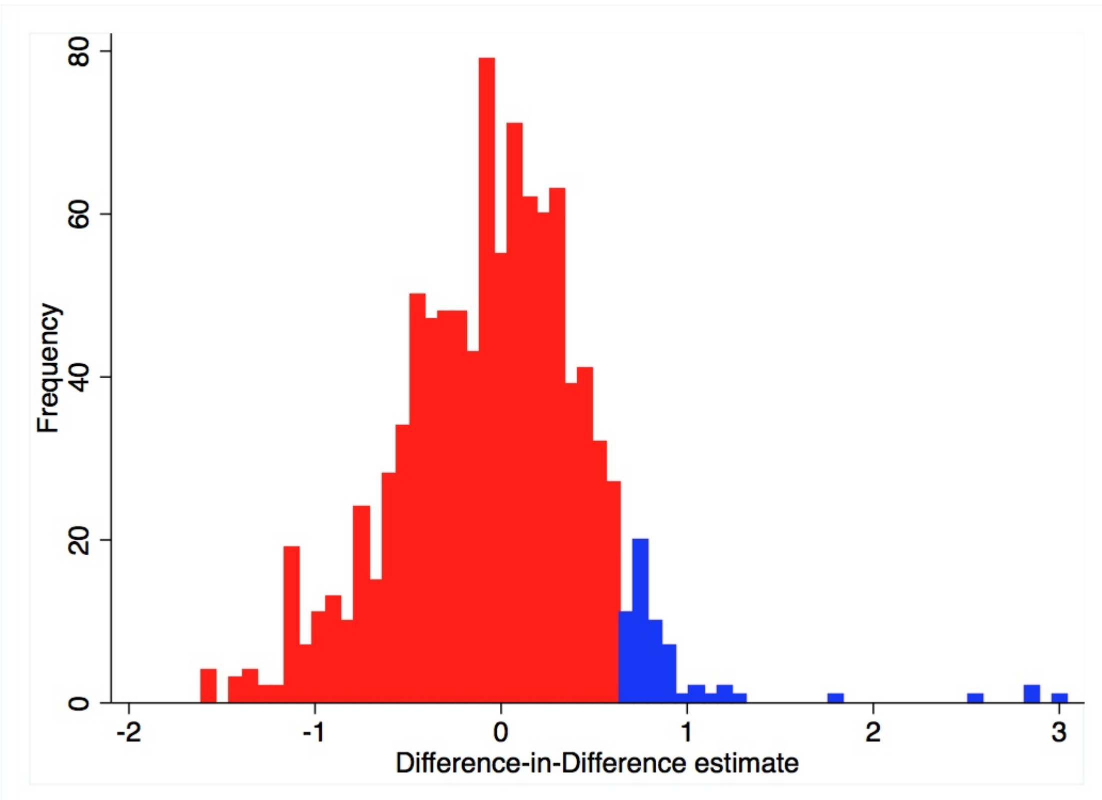


Figure 7: Los Angeles Synthetic Control Matching: Histogram of 1,000 placebo permutations for average post- (1992-2005) minus average pre- (1988-1991) riot period

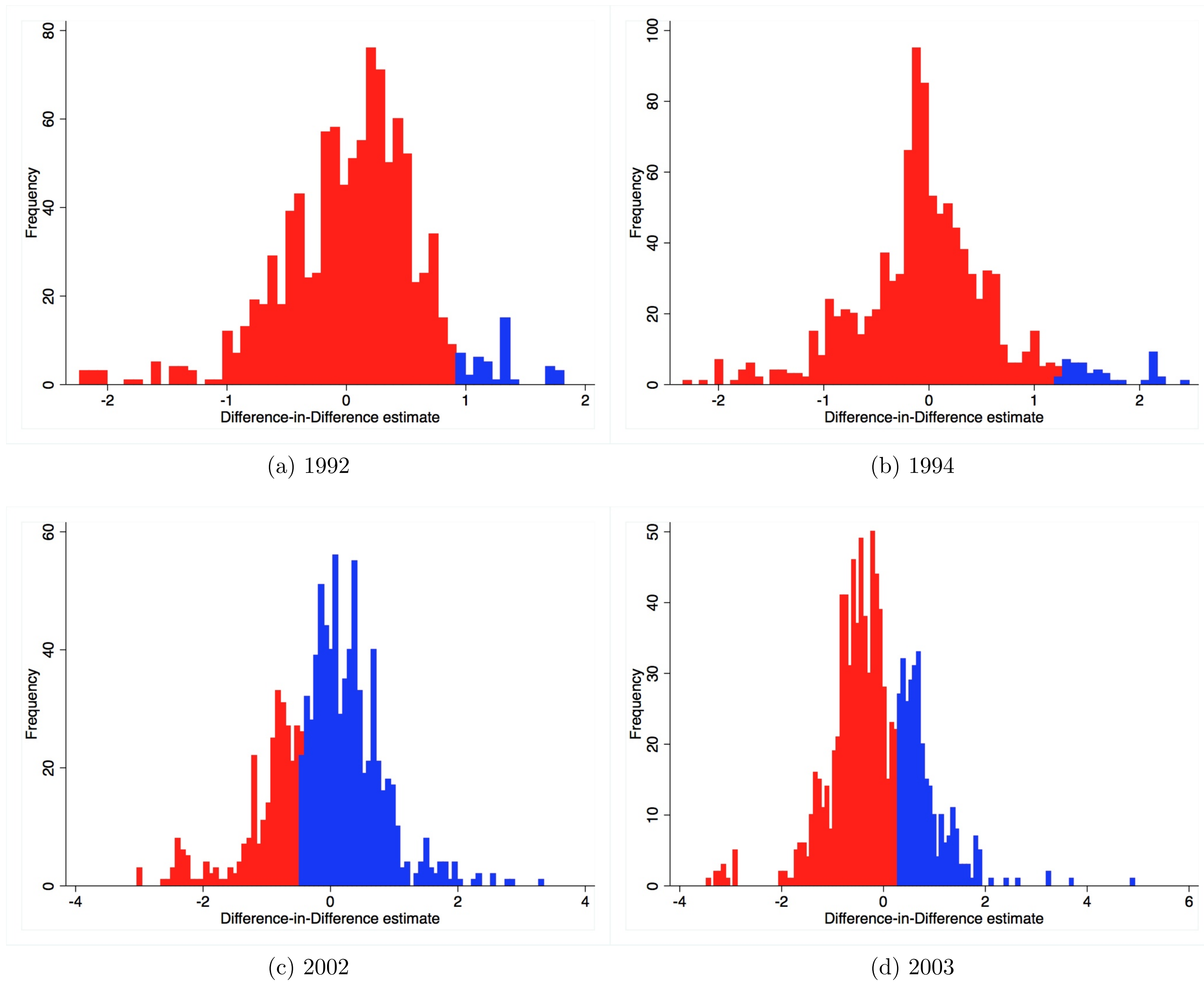


Figure 8: Los Angeles Synthetic Control Matching: Histogram of 1,000 placebo permutations for selected years

8 Tables

Table 1: Causes of Riots Summary

Study Findings	Temporal Scope	Riot Measure	Number of Cities	Method	Significant Findings	Non significant (direction of effect)
Lieberson & Silverman (1965)	1913-1965	occurrence	75	Mean Differences	% Black male holding traditional occ (+), Black unemployment (+), Black police per thousand black (+), population per councilman (+), % council members elected at large (-)	% inc white, % inc black, % self employed black, white unemployment, white and black median income, % black dilapidated housing, Social disorganization

Downes (1970)	1963-1968	Severity & occurrence	676	Correlation	Unemployment (+), population (+), pop growth (-), deaths per 1000 (-), % nonwhite, % change nonwhite, % 25 years old with HS degree (-), % occupied housing units, metro status (-), number of councilmen (+), per capita gen exp (+), per capita sanitation exp (+), per capita debt (+), median age (+)	Median income, % housing with plumbing, % employed in white collar occ, form of govt, type of election, term of office
Spilerman (1970, 1971)	1961-1968	occurrence	410	Regression	Non-white population size (+), region	Geographic contagion, Social disorganization, Absolute Deprivation, Relative Deprivation, Political Structure

McElroy & Singell (1973)	1965-1968	occurrence	129	Mean Differences	Median income (+), education (+), % less than 6 years of educ (+), % change in pop (+), log pop (+), pop per square mile (+), % change in priv household workers (+), % change in clerical workers (+), % change in technical workers (+), % govt workers (-), pop per councilman (+), per capita housing exp (-), % change in racial segregation, % workers in manufacturing	Racial segregation

Lieske (1978)	1967-1969	occurrence	119	Correlation	% poor nonwhite (-), ratio median rental nonwhite to white (-), educational inequality (-), occupational inequality (-), personal income inequality (-), family income inequality (-), % non-white divorced or sep (+), % non-white one parent home (+), non-white illegitimacy rate (+), police density (+), % welfare exp (+), nonwhite to teachers ratio (-), unemployment (+), school segregation (-)	% nonwhite with high school education, % males occupied as crafts/foreman, % nonwhite child in one parent home, nonwhite birth rate, nonwhite fertility ratio, nonwhite age and job inequality
Snyder (1979)	1965-1969	occurrence	241	Regression	Ratio of black to housing units (+), blacks age 15-34 (+), % black unemployed (+), % recent occupants (+), contact between white police and black per capita (+)	% black age 15-34, contact between white police and black

Carter (1986)	1964-1971	Severity	313	Regression	Nonwhite pop size (+), region, relative deprivation (u-shaped), mixed govt (+), police presence (u-shaped)	Mayor council govt, partisan elections
Olzak & Shanahan (1996)	1954-1993	occurrence	204	Regression	Nonwhite pop (+), region, partisan election (+), % dilapidated housing (-), ratio of nonwhite and white median income	Social disorganization, Absolute deprivation, Relative deprivation, Political structure, Competition
Olzak, Shanahan, & McEneaney (1996)	1960-1993	occurrence	55 (SMSAs)	Regression	Dissimilarity index (+), isolation index (+), exposure index (+), change in racial segregation	Black poverty rate, % change in unemployment, crime rate, underclass families on welfare, white-black family income gap, ratio of black and white median income, % change in black poverty, black population size

Myers (1997)	1961-1968	occurrence	410	Regression	non-white population (+), region, non-white unemp*% foreign born (-), log non-white unemp (+), median manufacturing wage (+), total unemp rate (-), % foreign born (+), diffusion (+)	Social disorganization, Absolute deprivation, Relative deprivation, Expectations, Political structure, Competition
Bergesen & Herman (1998)	1992	occurrence	1,591 (tracts)	Regression	% change in black pop, % change in foreign born, % foreign born, % black, change in median household inc, % non Mexican Hispanic, % Asian, interethnic contact	% change in prop unemployed, % unemployed, % Mexican

DiPasquale & Glaesser (1998)	1965-1968, 1992	occurrence	192	Regression	1960s - Nonwhite population (+), region, non-white home ownership (-), non police govt exp (+). Los Angeles - 16-30 black and Hispanic unemployment rate (+), total pop (+), ethnic diversity (+)	1960s - Segregation index, log of total population, median age of nonwhite pop, % poverty nonwhite, police exp per capita. Los Angeles - poverty rate, homeownership rate, self employment rate, female headed households (all vars for white, black, and Hispanic)
Myers (2000)	1964-1971	occurrence	313	Regression	non-white population (+), region, contagion (+), diffusion (+)	-
Chandra and Foster (2005)	1961-1968	occurrence	49 (States)	Regression	Relative deprivation (quadratic), crime rate (+), manager form of govt (+), % black (+)	Occupational segregation, residential segregation, police presence,

Table 2: Cities by Riot Severity Percentiles

Percentile	City	Riot Severity	Year(s)	Days of riots	Killed	Injured	Arrested	Arson
Min	Hammond, IN	0.0005	1968	1	0	0	0	0
25th	Racine, WI	0.0038	1968	3	0	22	23	3
Median	Gary, IN	0.0093	1968	6	0	12	267	20
75th	Toledo, OH	0.0186	1967-68	11	0	57	248	74
Max	Los Angeles, CA	0.5209	1965-68	18	39	1,109	4,061	3,073
Total	N = 147	-	1964-1969	1,281	209	11,196	14,866	63,207

The sample includes cities with a population $\geq$ 50,000 experiencing a riot between 1964 and 1969

Source: Riot severity calculated from Carter (1986) and Margo and Collins (2007)

Table 3: Summary Statistics, city level, by riot occurrence and pre-riot year.

	non-riot		Riot	
	1950	1960	1950	1960
Population	119,729	137,243	252,637	278,541
Percent Non-white	5.7	6	14.2	17.8
Percent 25+ w/ HS Degree	42.6	46.7	40.2	42.4
Median Housing Value	7,978.1	13,556.5	8,861.2	12,600.0
Unemployment Rate	4.9	4.8	5.5	5.4
Non-enrollment rate	12.3	11.7	13.3	13.0
Riot Severity	0.0000		0.0296	
Percent Northeast	27.1		31.3	
Percent Midwest	26.5		21.1	
Percent South	24.5		31.3	
Percent West	21.9		16.3	
N	155		147	

The sample excludes cities with missing values for any of the variables.
All values are reported as averages unless otherwise specified
Source: Data for non-enrollment, population, housing values, unemployment rate, and percent with high school degree are based on census data taken from the U.S. Department of Commerce, County and City Data Book, the National Historical Geographic Information System and the Governmental Units Analysis Data (tabulated in ICSPR 0028). Riot severity derived from Carter (1986) and Margo and Collins (2007)

Table 4: Ordinary Least Squares Regression: Log non-enrollment Rates and Riot Severity, 1960-1970 and 1960-1980

	1960-70		1960-80	
Shock Severity	0.0108**	0.0127**	0.0057	0.0063
	(0.0038)	(0.0037)	(0.0043)	(0.0041)
Log Population	6.47e-09	3.01e-08**	-5.59e-09	2.01e-09
	(1.28e-08)	(1.38e-08)	(1.29e-08)	(1.35e-08)
Percent non-white	-0.0648	-0.0975	-0.9060**	-1.0124**
	(0.2337)	(0.2572)	(0.2694)	(0.2727)
Midwest	-0.1679**	-0.2343**	0.0154	-0.0728
	(0.0622)	(0.0740)	(0.0564)	(0.0634)
Northeast	-0.1335**	-0.1511	-0.0585	-0.1630**
	(0.0614)	(0.0825)	(0.0608)	(0.0769)
West	-0.0960	-0.1366	0.3330**	0.3005**
	(0.0749)	(0.0901)	(0.0656)	(0.0769)
1960 Log non-enrollment Rate		-0.3318**		-0.2250**
		(0.0841)		(0.0810)
Median Housing Value		-0.2026		0.0841
		(0.1098)		(0.1139)
Unemployment Rate		-0.7916		-1.2379
		(1.4876)		(1.4159)
Percentage 25+ w/ HS Degree		-0.5869		-1.0406**
		(0.3530)		(0.3617)
Observations	302	302	302	302
R^2	0.070	0.143	0.227	0.268

** $p < 0.05$. Robust standard errors in parentheses

Source: See Table 3 for sources

Table 5: Differences-in-Differences: Log non-enrollment Rates and Riot occurrence, 1960-1970 and 1960-1980

	1960-70		1960-80	
Shock Year	-0.6554**	-0.4103**	-0.4297**	0.4148**
	(0.0462)	(0.0428)	(0.0500)	(0.0961)
Shock City	0.0822**	0.0600	0.1057**	0.0566
	(0.0407)	(0.0355)	(0.0406)	(0.0361)
Shock Year x Shock City	0.1176**	0.1219**	0.0572	0.1168**
	(0.0598)	(0.0522)	(0.0615)	(0.0545)
Log Population	6.52e-08**	7.49e-08**	6.29e-08**	6.54e-08**
	(2.08e-08)	(1.77e-08)	(2.19e-08)	(1.78e-08)
Percent non-white	0.3134**	-0.1460	0.2052	-0.1219
	(0.1398)	(0.1387)	(0.1120)	(0.1172)
Midwest	-0.3730**	-0.3296**	-0.3137**	-0.2314**
	(0.0446)	(0.0405)	(0.0434)	(0.0436)
Northeast	-0.1068**	-0.1727**	-0.0841**	-0.1756**
	(0.0446)	(0.0461)	(0.0411)	(0.0438)
West	-0.4780**	-0.2467**	-0.2680**	0.0723
	(0.0490)	(0.0541)	(0.0453)	(0.0566)
Median Housing Value		-0.3320**		-0.2068**
		(0.0650)		(0.0631)
Unemployment Rate		-0.2184**		-3.4345**
		(0.9465)		(0.8267)
Percentage 25+ w/ HS Degree		-1.5137**		-2.4211**
		(0.1842)		(0.1991)
Observations	606	606	606	606
R^2	0.517	0.642	0.314	0.505

** $p < 0.05$. Robust standard errors in parentheses

Source: See Table 3 sources

Table 6: Synthetic Matching: Estimated impact on log non-enrollment rates - 1960s Riots

	1970	1980
Riot City Average	-2.319	-2.169
Synthetic Match Average	-2.221	-2.046
Difference-in-Differences Estimate	0.098	0.124
p-value one tailed test	0.001	0.001

The difference-in-differences estimator measures the difference in the average outcome for treated cities before and after the riot minus the difference in the average outcome in their synthetic matches before and after the riot. The one sided test reflects the percent of 1,000 permutations greater than or equal to the DID estimate using riot cities and their synthetic matches

Table 7: Estimated Effects on Log non-enrollment Rates of 1960s Riot Severity and occurrence by Model

	1970	1980
Mean Difference††	0.253**	0.225**
	(0.001)	(0.123)
Ordinary Least Squares†	0.013**	0.006
	(0.004)	(0.004)
Difference-in-Differences††	0.122**	0.117**
	(0.020)	(0.032)
Synthetic Control -	0.098**	0.124**
Difference-in-Differences††	(0.002)	(0.002)

** $p < 0.05$

†Outcome is riot severity. ††Outcome is riot occurrence

two-sided p-values are in parentheses

Table 8: Summary pre-riot Characteristics (1988-1991) of Los Angeles and Control Cities used in Synthetic Los Angeles

		Control Cities		
	Los Angeles	Indio	Rialto	San Bernardino
Weight	-	0.67	0.284	0.046
Log Dropout Rate	-2.2	-2.3	-2.3	-2.5
Population	3,226,124	29,202	54,931	140,827
Unemployment Rate	7.6	8.6	8.1	9.9
Percent Black	15.5	4.4	15.8	15.5
Percent Asian	8.2	1.4	2.4	2.7
Percent Hispanic	33.7	62.1	25.2	29.9
Percent 25+ w/ HS Degree	67.8	52.5	73.7	67.3
Median Housing Value	170,300	71,150	93,150	74,400

The dropout rate is averaged over 1988 to 1991. The other values are for 1990.
Source: Demographic data are based on census data taken from the National Historical Geographic Information System. Dropout data calculated from data obtained through the California Department of Education

Table 9: Log Dropout Rates for Los Angeles and Synthetic Match by Year

Year	Los Angeles	Synthetic Control	Difference-in-differences estimate	p-value from one tailed test
1992-2005	-2.531	-3.294	0.640	0.059
1992	-2.060	-2.704	0.521	0.044
1993	-2.193	-2.427	0.111	0.045
1994	-2.175	-2.732	0.434	0.290
1995	-2.190	-2.805	0.492	0.294
1996	-2.339	-3.164	0.702	0.155
1997	-2.566	-3.395	0.706	0.146
1998	-2.961	-3.522	0.438	0.255
1999	-2.798	-3.882	0.961	0.077
2000	-2.845	-3.871	0.903	0.103
2001	-2.719	-3.998	1.156	0.043
2002	-2.808	-3.788	0.857	0.097
2003	-2.453	-3.339	0.763	0.123
2004	-2.479	-3.154	0.552	0.171
2005	-2.847	-3.335	0.366	0.247

The difference-in-differences estimator measures the difference in the average log dropout rate in Los Angeles before and after the riot minus the difference in the average log dropout rate in synthetic Los Angeles before and after the riot

The one sided test reflects the percent of 1,000 permutations greater than or equal to the difference-in-difference estimate using L.A. and its synthetic match

The pre-riot outcome is the average log dropout rate in 1988 to 1991

Table 10: Difference-in-Differences: 1992 Los Angeles Riot Effect on Log Dropout Rates Riot, Census Tract, 1990-2000

	Los Angeles City	Los Angeles County	California
Shock Year	-0.3566**	-0.4253**	-0.2923**
	(0.0482)	(0.0368)	(0.0168)
Shock Tract	0.2049**	0.2136**	0.0591
	(0.0474)	(0.0411)	(0.0346)
Shock Year x Shock Tract	-0.0158	0.0791	-0.0429
	(0.0627)	(0.0551)	(0.0464)
Log Population	-1.16e-05	-1.28e-05	-1.85e-05**
	(1.28e-05)	(8.65e-06)	(3.96e-06)
Unemployment Rate	0.4224	1.2151**	0.1355
	(0.5636)	(0.4271)	(0.2081)
Percentage 25+ w/ HS Degree	-1.0876**	-1.7825**	-2.2216
	(0.2594)	(0.2000)	(0.1057)
Percent Black	0.1630	-0.0767	0.2921**
	(0.1432)	(0.1171)	(0.0562)
Percent Asian	0.0185	-0.5530**	-0.7230**
	(0.1813)	(0.1329)	(0.0651)
Percent Hispanic	0.5655**	0.0435	0.1128
	(0.2021)	(0.1521)	(0.0690)
Mean Housing Value	-6.07e-08	-4.82e-07	2.27e-07**
	(2.86e-07)	(2.96e-07)	(9.44e-08)
Observations	1454	2076	8393
R^2	0.364	0.466	0.365

** $p < 0.05$. Robust standard errors in parentheses

Source: GeoLytics Neighborhood Change Database, 1970-2010