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Measuring the Eects of the Community on Education Outcomes Using Natural Experiments

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by

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Committee in charge:

Professor Michael Hout, Chair
Professor Kenneth Wachter
Professor Paul Waddell

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Abstract

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The purpose of this dissertation is to examine the effects of environmental conditions on graduation and dropout rates in the United States. Neighborhood effects studies are not uncommon, however they typically fail to account for serious selection issues that potentially bias results. The contribution of this dissertation is to offer an alternative approach, natural experiments, to deal with such methodological problems. The dissertation consists of one theoretical chapter and two analytical chapters. In chapter one, I establish the econometric difficulties of estimating neighborhood effects and outline how a natural experiment can obviate these problems if applied correctly. This chapter sets up the rest of my dissertation, which includes two empirical chapters using major urban riots and county mass layoffs as natural experiments.

In chapter one, I describe the econometric difficulties of estimating neighborhood effects within the context of youth educational well-being. I outline the three levels of selection bias, individual, family and school, that researchers normally confront when attempting to examine the effects of community conditions on education outcomes. While most studies attempt to minimize individual and family level bias, they often neglect to account for school level context. Doing so will lead to biased results because model parameters will fail to reflect the lack of independence between units due to clustering within schools and the deep selection issues caused by the interwoven relationship between neighborhoods and schools. I then outline the conditions under which a natural experiment can solve these problems. This chapter sets the stage for the two empirical articles that make up the rest of my dissertation.

In the first analytical chapter, I examine the effects of large urban riot shocks to city wide conditions on aggregate dropout rates. Using decennial census data, I examine the 1960s civil rights riots on city level school non enrollment rates. I find that the 1960s urban riots had negative short- and long-term consequences on school enrollment rates. In order to test the robustness of these results to a more contemporary setting, I estimate the effect

of the 1992 Los Angeles riots on city and census tract level dropout rates. I find evidence of a short-term effect, but no long-term impact.

In the second empirical chapter, I estimate the effects of diminished county conditions due to mass layoff occurrences in the U.S from 1996 to 2010 on county level graduation and dropout rates. Although I find no evidence of a negative single year effect on schooling persistence rates, I uncover lagged and cumulative effects. Specifically, I find that a large two-year lagged shock has a negative effect on current graduation rates. I also find that minor and large four-year average shocks have a significant effect on graduation and dropout rates. These results indicate that point-in-time measurements may not capture the temporal effects of neighborhood disadvantage, especially for processes like schooling persistence that may not be sensitive to minor, short-term shocks.

Contents

List of Figures

List of Tables

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The process of earning a doctorate is a microcosm of life - the journey is filled with many peaks and valleys. I am grateful that I had so many supportive people to be there with me throughout this journey and without them I wouldn't have gotten myself back up during the lows and I wouldn't have laughed as hard during the highs.

Chapter 1

Introduction

Although the philosophical inquiry into the role of neighborhoods in shaping individual outcomes dates back to the early 20th century (Park and Burgess 1925), it wasn't until much later, through the pioneering work of Wilson (1987), Jenks and Mayer (1990) and urban sociologists of the classical Chicago school (Sampson and Morenoff 1997), that researchers began developing a theoretical framework for studying neighborhood effects. A cottage industry emerged in which researchers have attempted to determine how and to what degree neighborhoods affect a wide range of individual outcomes. The study of neighborhood effects initially developed under the auspices of sociological inquiry, but it cuts across a wide gamut of social science discplines, including economics, public policy, anthropology, geography and public health.

A survey of the literature yields the general conclusion that low-quality neighborhoods, where quality is defined by an array of characteristics relevant to the outcome being tested, have pernicious effects on an individual's emotional, physical and social welfare, including economic self-sufficiency, crime, drug use, teenage pregnancy, low birth weight and cognitive ability, to name a few. Researchers have also found that children are more susceptible to neighborhood conditions. Adolescence is the period in the life course in which neighborhood effects would become visible. It is the stage in which a young person's social world begins to integrate peers and the larger community (Darling and Steinberg 1997). This finding carries significant consequences since children today compared with their predecessors are more likely to be raised in extremely poor families as well as live in significantly poor neighborhoods.

An important indicator of child well-being is academic success at school. Since schooling performance and educational attainment has been linked to such important outcomes as health and economic success and the probability and timing of marriage, divorce and fertility, it is important to understand the potential factors that influence a child's schooling achievement. This motivation has become even more pressing today as policymakers place increasingly strict accountability targets for teachers, students and schools. If neighborhoods have an influence on a child's schooling success, then they should also be held accountable for how their youth residents perform academically. The growing disdvantages children face in their neighborhoods will likely create disadvantages in the classroom.

There are a number of pathways by which the neighborhood might influence educational outcomes for children. Figure 1 presents a conceptualization of these pathways. Better neighborhoods are expected to provide students with access to neighborhood schools that have higher quality teachers, smaller class sizes, and higher expectations for learning and achievement. On the other hand, while an increase in school quality may develop stronger commitment to education, it might also elevate standards which in turn might lead children to become discouraged if these standards differ appreciably from their old schools.

Community-level mediators such as safer neighborhoods carry over into children's academic and personal lives. Better neighborhoods offer access to academic enriching opportunities such as after-school programs and tutoring, as well as to activities, such as youth recreational leagues, that deter students from engaging in behavior detrimental to their academic success. A final community hypothesis is that families will have access to greater economic resources (like better paying jobs) and this may allow the family to invest more in educational items like books, computers, etc.

On the individual and family level, the model predicts that changes in attitudes and behaviours of both students and parents serve as key mediators in improved educational outcomes. Academic achievement will be influenced by students' beliefs and attitudes (e.g., ideas about themselves as successful students, whether teachers care about them, and their peers' and parents' school beliefs). Also important are students behaviors like showing respect towards teachers, committing to studying, and participating in school activities. The hypothesized parental mediators (attitudes and behaviors) include expectations about school success, the level of support for and active involvement in the school, involvement in homework completion, and parenting practices around students' actions and consequences.

Researchers have generally found that neighborhoods do affect youth academic achievement, specifically finding that those in better neighborhoods are more likely to graduate high school, attend college and have higher test scores (Ludwig, Ladd and Duncan 2001, Harding 2003, Morenoff 2003). Most of the empirical evidence supporting neighborhood effects on children's educational outcomes come from observational studies and quasi experimental settings like the Gautreaux initiative, a Chicago based program that provided housing mobility assistance to poor families to move into low minority areas. Many of these studies rely on cross-sectional measures of neighborhood quality to make such correlational claims as "children living in neighborhoods with lower quality at time t have higher/lower school outcomes at time t." More sophisticated studies institute a longitudinal component, typically measuring the characteristics of neighborhoods at a point in time and measuring their effect on individual outcomes at some future period. These types of studies make such claims as "children living in neighborhoods with low quality at time t have higher/lower school outcomes at time $t + 1$."

Hanging over much of this research is the problem of selection bias, or the possibility that unobserved characteristics of individuals and families may jointly correlate with selection into a specific type of neighborhood and the outcome of interest. Observational studies of neighborhood effects, in particular, have been challenged repeatedly for failing to deal with selection bias adequately (Jencks and Mayer 1990; Ludwig et al. 2008).

In response to these obstacles, more recent neighborhood effects models have relied on experimentally based strategies that explicitly change the composition of an individual's neighborhood and measure the effects of those changes on various outcomes over time. The Moving to Opportunity (MTO) housing mobility program is an example of such a strategy. In the MTO, poor families were randomly selected to receive assistance to move out of their disadvantaged neighborhoods and into wealthier communities. Studies using this strategy often make the claim that "residents who move from a low to a high quality neighborhood at time t experience lower/higher outcomes at time $t+1$." Studies examining the MTO found no detectable evidence on children's achievement test score. Burdick-Will et al. (2010) attempt to reconcile these differences using a new housing voucher lottery in Chicago and find that neighborhood effects are relevant, but may not always matter.

Randomized social experiments push the field towards more powerful findings. However, they have several non-trivial issues. First, randomized social experiments are expensive and require a significant amount of political will to get off the ground. Local communities are concerned about fair selection processes and residents from receiving communities are concerned about the impact of low income residents moving into their neighborhoods. The high internal validity obtained from a social experiment like the MTO must be weighed against the resource cost of such an undertaking. Second, significant design issues may impact the interpretation of the findings. For example, control group MTO families, which were suppossed to stay in their old neighborhoods, ended up leaving their communities on their own for better neighborhoods. Additionally, many treatment group families ended up moving back to their old neighborhoods a year after their initial move.

A third problem relates to the difficulty in disentangling independent school and neighborhood effects from one another. Scholars have traditionally theorized the school as a largely independent institution in which practices, such as curriculum and teacher hirings, and policies were largely governed by school level actors with little influence from outside factors. However, some believe that the school is largely an institution within a neighborhood. The school effects literature generally finds an association between school quality and child schooling success conditioned on individual and family characteristics (Johnson 2011). However, how much of this effect can be attributed to the neighborhood? Conversely, significant effects obtained from neighborhood effects studies may be partially or entirely due to independent school level factors. In the case of experiments like the MTO, school enrollment is not controlled for. Some families enroll their children in the schools of their new neighborhoods, but other families choose to keep their children enrolled in their old schools. We can't distinguish between a pure neighborhood effect from a pure school effect because school choice is not controlled for.

If properly constructed social experiments are not readily available, researchers should not simply rely on traditional observation based statistical procedures or give up their quest for stronger findings since the topic of neighborhood effects, particularly in the context of child well being, carries significant consequences for future policy making and sociological inquiry. In this dissertation, I offer an alternative strategy to routine observation based methods and randomized social experiments to estimate the effect of communities on schooling outcomes. Specifically, I apply a natural experiment approach. In a natural experiment, the treatment (the independent variable of interest) varies through some naturally occurring or unplanned event that happens to be exogenous to the outcome (the dependent variable of interest). In the social sciences, this approach has been used to study the relationship between residential change and propensity towards crime, the effect of stress of in-utero selection, the impact of quotas for women village councilors on public goods provision in India, and many other topics. Therefore, the natural experiment strategy is not new as it has been applied to other contexts, but it has largely been ignored in the neighborhood effects literature.

Natural experiments share two crucial attributes with true experiments. First, outcomes are typically compared across subjects exposed to a treatment and those exposed to a control condition. Second, subjects are often assigned to the treatment not at random, but rather as-if at random. Given that the data come from naturally occurring phenomena that often involve social and natural processes, the manipulation of the treatment is not under the control of the analyst; thus, the study is observational.

However, a researcher carrying out this type of study can often make the claim that the assignment of subjects to treatment and control conditions is as good as random. In the context of measuring neighborhood effects on schooling outcomes, a properly identified natural experiment can disentagle independent school and neighborhood effects since the randomizing is solely enacted at the neighborhood level. Therefore, individual, family and school level confounding factors are controlled for. Researchers without access to ideal data can utilize natural experiments to make causal interpretations of social processes. The strategy is not without problems, but it offers another angle of attack against the methodological problems social scientists frequently encounter when using observational data.

1.1 Organization of the Dissertation

The dissertation is organized in three chapters. The first chapter offers a theoretical delineation of the problems I attempt to answer in the dissertation and my proposed solution, i.e. natural experiment, to these problems. The second and third chapters offer results from empirical applications of natural experiments.

In the first chapter, I outline in more detail the econometric difficulties of identifying neighborhood effects, particularly in the context of controlling for school level context. In this chapter, I determine the conditions that must be met in order to identify causal neighborhood effects and outline the consequences of failing to meet any of these conditions. I then describe how a natural experiment can meet these conditions, specifically outlining the five identifying characteristics underlying a natural experiment event. The chapter sets up the rest of my dissertation, which provides two empirical applications of a natural experiment. In chapter 2, relying on Decennial Census and California Department of Education data, I use major urban riots in the 1960s and in 1992 in Los Angeles to measure the effects of diminished conditions on city level dropout rates. In chapter 3, I use nationwide county level mass layoffs from 1996 to 2010 as my natural experiment and determine the effects of shocked county conitions on adolescent dropout rates. Finally, I end the dissertation with some concluding remarks.

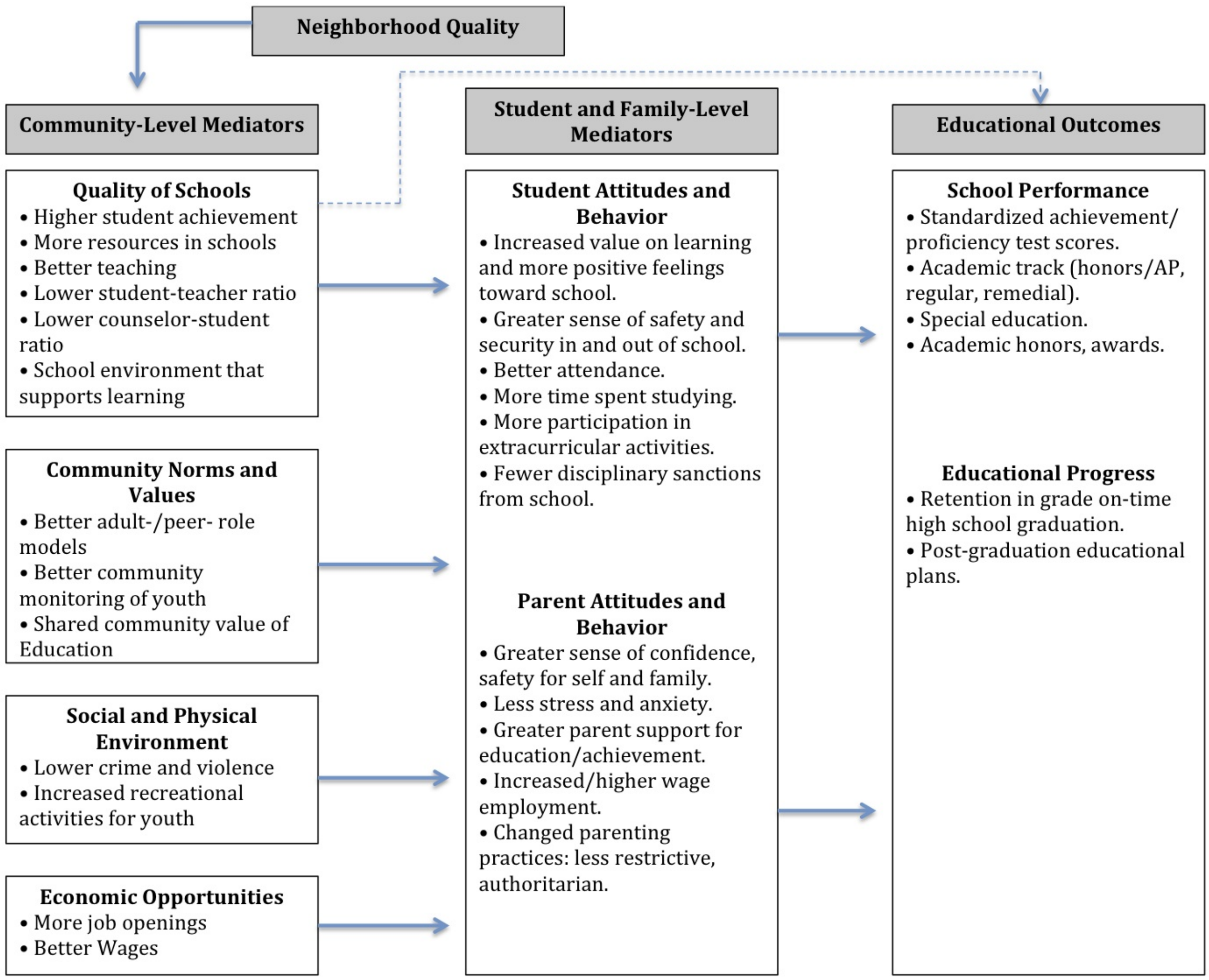


Figure 1.1: Mediating Pathways Between Neighborhoods and Educational Outcomes. (Adapted from Orr et al. 2003, p.102)

Chapter 2

Disentangling Neighborhood and School Effects Using Natural Experiments

2.1 Introduction

Debate over education reform typically focuses on individual, family and school level inputs into the educational process, such as per-pupil funding and parental income. However, a growing body of evidence has shown that conditions independent of the individual and outside the family and school have important consequences for individual health, education, and socioeconomic well-being. In particular, researchers have found non-trivial associations between neighborhood conditions and individual outcomes. These findings carry significant consequences since poor children today are more concentrated in disadvantaged neighborhoods compared with their predecessors. After declining in the 1990s, the population in extreme poverty neighborhoods rose in the following decade (Kneebone, Nadeau and Berube 2011). The growing disadvantages poor children face in their neighborhoods, such as higher crime rates, absence of role models and weaker social institutions, will likely translate into disadvantages in the classroom.

Policymakers have been slow to acknowledge the relationship between youth well-being and neighborhood conditions when drafting child specific policies, particularly within an educational context. For example, the federal government's No Child Left Behind (NCLB) act places strict accountability targets for teachers, students and schools, but largely ignores the role the neighborhood may play in how well a student performs. Many commentators routinely blame individual, family and school level disadvantages for engendering performance gaps in academic performance. However, if neighborhoods have an influence on a child's schooling success, then neighborhood stakeholders, such as community leaders and locally elected officials, should also be held accountable for how their youth residents perform academically.

The estimation of neighborhood effects on schooling success compared to other oft-studied outcomes such as health or juvenile crime carries a unique difficulty in that how well a child does academically is directly influenced by not just his individual traits or family characteristics, but also by his school. If schools were simply institutions under the umbrella of the neighborhood, then neighborhood effects models need not control for school context. However, we can conceive of schools as distinct entities since school actors (e.g. students, teachers, counselors, administrators) may reside in different neighborhoods and school practice and policy are governed by legislation crafted at non-local levels (e.g. the district, county, state, federal government).

Many studies modelling the effects of the neighborhood on educational outcomes include individual and neighborhood level covariates, but ignore the school level. Given that school composition often mirrors neighborhood composition, models that fail to control for school context run the risk of biased parameter estimates. These models implicitly assume that either variation between schools is insignificant or is entirely a function of the organizational characteristics of the neighborhood (Arum, 2000). However, if schools do have an effect independent of the neighborhood and some of that effect is highly correlated with community characteristics, then estimating neighborhood effects in the absence of school level controls leads to potentially misleading results. Conversely, studies of school effects that do not control for neighborhood characteristics may be misattributing some or all of the neighborhood effect to the school.

The studies that have jointly examined neighborhood and school effects on educational outcomes have largely relied on multilevel models to separate school and neighborhood level variation. Although standard multilevel modelling minimizes problems associated with clustering, it does not solve many of the underlying econometric issues that make separating school and neighborhood context methodologically difficult. Selection processes function to place individuals with certain characteristics into certain types of schools. Furthermore, these schools are likely located in certain types of neighborhoods. Controlling for observed school variables and modelling neighborhood and school variation separately may not be enough to minimize these complex multilevel selection and interaction processes, especially when relying on observational data (Bingenheimer and Raudenbush, 2004; Shieh and Fouladi, 2003). The problems of ignoring the school level when measuring neighborhood effects mirror the problems of ignoring the neighborhood level when measuring school effects. The details may differ slightly since the hierarchy - schools are nested *within* neighborhoods - is not symmetric, but the general issue - ignoring a context that may account for some of the variation in the outcome variable - applies to both settings.

One way to address these limitations is to conduct a randomized intervention that exogenously changes neighborhood or school conditions. Randomization alters the neighborhood or school characteristic of interest while controlling for individual, school and neighborhood level confounding. Unfortunately, such interventions are rare since they require a great deal of resources and political capital. In cases where large-scale social experiments are prohibited, researchers often rely on a natural experiment approach, which utilizes identifiable discrete shifts in the environment due to an external event or institutional condition, such as the passage of a new law or a natural disaster, that happens to be exogenous to the outcome. The external event is analogous to the researcher randomly assigning treatment and control conditions to experimental units. Thus, the observed values of the variable of interest in a natural experiment are as good as randomly assigned. Although we should not treat a natural experiment as a silver bullet to the problems detailed above, they are tools that researchers relying on observational data should utilize when traditional strategies fail and data from randomized trials do not exist.

The purpose of this paper is to outline the methodological problems of ignoring a level of context in the estimation of neighborhood and school effects on educational outcomes. Using the potential outcomes framework (Rubin 1974), I outline the use of natural experiments to circumvent these obstacles. I then delineate the conditions that must be met in a natural experiment in order to estimate causal neighborhood or school effects. Note that the paper focuses on neighborhood effects, but the issues raised and the solutions proposed also apply to the school effects literature since the problems associated with ignoring neighborhoods in a school effects analysis is similar to the problems of ignoring schools in a neighborhood effects study.

The remainder of this paper is organized as follows. Section 2 provides a summary of the school and neighborhood effects research. Section 3 outlines the general identification problems underlying most of these studies, specifically muliticollinearity and endogeneity between levels. Section 4 outlines the use of natural experiments to obviate these problems and the conditions that must be met in order to utilize this strategy. The final sections provide a discussion and some concluding remarks.

2.2 Neighborhood and School Influences on Educational Success

The research literature examining the effects of environmental context on individual educational success has traditionally focused on the influence of the schooling environment. Local communities were implicitly assumed to be inconsequential since school practices were argued to reflect the schooling environment and macro level conditions divorced from local characteristics. Theoretical explanations of school effects emphasize four mechanisms through which the school can affect the academic success of a student: organizational process, teacher attributes, resources, and student body composition. Researchers have examined the impact of these school mechanisms on a bevy of educational outcomes, including dropout and graduation, standardized test scores, grades, and delinquency. Most school effects studies find that schools make significant contributions to educational outcomes after controlling for individual and family level variables (Bryk and Driscoll 1988; Mayer, 1991; Zimmer and Toma, 2000; Hanushek et al., 2003; Espenshade, Hale, and Chung, 2005; Johnson, 2011).

Several studies have argued that schools have less of an impact on educational outcomes than what is suggested in the literature. Most notable of these studies is the Coleman Report

(Coleman et al., 1966), which concludes that schools make little difference in explaining student gaps in achievement and attainment. Wilson (1987) further fueled the debate by hypothesizing in his seminal book The Truly Disadvantaged that community social and economic conditions play crucial roles in engendering individual disadvantage. Subsequent neighborhood effects research has attempted to augment Coleman and Wilson's claims by studying the impact of neighborhood context on educational success.

Drawing from the theoretical work of Wilson (1987), Jenks and Mayer (1990) and Sampson, Morenoff, and Earls (1999), we can classify the mechanisms that neighborhoods act through to affect student outcomes in five categories: collective socialization, social control, social capital, institutional resources, and local labor market conditions. Research has produced a mixed bag of results, with some studies finding large neighborhood effects (Evans, Oates, and Schwab, 1992; Harding, 2003) and others finding modest (Brooks-Gunn et al., 1993; Aaronson, 1998) to no effects (Plotnick and Hoffman, 1995, Jacob, 2004). Based on this literature, we can reject the assertion that neighborhoods never matter.

Both the neighborhood and school effects literature have established lines of inquiry examining the effects of each context on educational success. However, these two streams of research are often done in isolation. The few studies examining neighborhood and school effects simultaneously yield results that indicate independent neighborhood and school effects or significant interactions of both contexts (for a complete review, see Johnson, 2011 and Owens, 2011). Typically, these studies rely on fitting multilevel models on observational data to separate school and neighborhood level variation. More sophisticated studies rely on social experiments, such as the Moving to Opportunity (MTO) program, which randomly assigned low-income families to receive moving assistance vouchers. While these neighborhood effects studies acknowledge the need to control for school influence, their models may not fully satisfy the conditions required to separate school context from neighborhood level variation. Including school level variables in a multilevel framework does not necessarily control for school level context, nor does instituting a randomized social trial if the experimental mechanism is not precisely defined (Sampson, 2008).

Ignoring neighborhoods in a school effects analysis also carries significant methodological problems, but the concern in the rest of this paper focuses on ignoring the school level in a neighborhood effects analysis. However, both settings share similar problems and potential solutions, although it will be noted when non-trivial differences do arise. The general problem in both modes of analysis is that a level of context explaining some of the variation in the outcome variable is missing in the model and thus upwardly biasing the explanatory power of the specific level of interest. The following section outlines in more detail the consequences of ignoring school context when estimating neighborhood effects.

2.3 Consequences of Ignoring School Context

Suppose we want to estimate the effect of some neighborhood level k characteristic D_k on an individual level i educational outcome Y_{ik}, such as student test score. The variable D_k can be a compositional characteristic, such as mean neighborhood level income, or an indicator for a neighborhood level intervention like the MTO. In the potential outcomes literature, D_k is typically regarded as a treatment indicator, taking on a value of 1 if neighborhood k is treated and 0 otherwise. We can estimate the effect of D_k on Y_{ik} using the following ordinary least squares (OLS) regression:

$$Y_{ik} = \beta_1 X_{ik} + \beta_3 Z_k + \theta D_k + z_k + \epsilon_{ik}, \tag{2.1}$$

where X_{ik} denote individual-level pre-treatment characteristics for individual i in neighborhood k, Z_k denote neighborhood level pre-treatment characteristics for neighborhood k, z_k is the unobserved neighborhood effect and ϵ_{ik} is the unobserved individual effect.

Statistically, the presence of z_k creates a correlation between individuals within the same neighborhood, breaking the Gauss-Markov assumption that units are independent. If a correlation exists between sampled units, there is less information compared to a simple random sample of non-clustered individuals of a similar size. Ignoring this autocorrelation can result in an increased risk of finding a relationship where none exists (Skinner et al., 1989). A common way of dealing with such issues is to use a multilevel model, which treats z_k as a random term with a (normal) probability distribution. Rather than treating z_k as a nuisance to be controlled away, a multilevel framework models the term by using between neighborhood variation, which assumes that neighborhoods are independent units, and thus leveraging the correlated structure of the data to produce coefficient estimates with correct standard errors.

Many neighborhood effects analyses estimate a two-level (individual and neighborhood) multilevel model and stop there. But, problems may still arise because certain levels of this structure are ignored. Since we are dealing with educational outcomes, we must be specifically worried about clustering within schools[1]. Since children are typically not randomly sorted into schools, students in the same school are likely similar across a broad range of characteristics. If students within schools are not independent, a school level error term s_{jk} enters equation (1), which causes correlation between individuals within the same school. To solve these issues, we can use a three-level model to estimate the effects of D_k on Y_{ijk}. The first level is at the individual:

$$Y_{ijk} = \beta_1 X_{ijk} + s_{jk} + \epsilon_{ijk} \tag{2.2}$$

We assume that ϵ_{ijk} is normally distributed with mean s_{jk} and variance σ_i. The second level is at the school:

$$s_{jk} = \beta_2 S_{jk} + z_k + \lambda_{jk} \tag{2.3}$$

We assume the school specific mean is distributed normally with mean z_k and variance σ_s. The third level is at the neighborhood:

[1] If we are running a school effects analysis, the same general worry applies, but the concern is clustering within neighborhoods

$$z_{jk} = \alpha + \beta_3 Z_k + \theta D_k + \mu_k \tag{2.4}$$

Furthermore, we assume the neighborhood specific mean is also distributed normally with mean α and variance σ_n. Substituting equations (3) and (4) into (2), we obtain the following:

$$Y_{ijk} = \alpha + \beta_1 X_{ijk} + \beta_2 S_{jk} + \beta_3 Z_k + \theta D_k + \mu_k + \lambda_{jk} + \epsilon_{ijk}, \tag{2.5}$$

where μ_k is the neighborhood-level random error, λ_{jk} is the school-level random error, and ϵ_{ijk} is the individual-level random error.

With a multilevel model, we separate variation stemming from the three levels into distinct components: the between individual (within neighborhood and school) variance σ_i, the between school (within neighborhood) variance σ_s, and the between neighborhood variance σ_n. By decomposing the total variance component into three levels, we not only adjust the standard errors for clustering, but we can also determine the degree to which the neighborhood level variable of interest D_k explains variation in outcome Y_{ijk} independent of the variation found between schools and individuals.

Many studies of neighborhood effects on schooling related outcomes do not control for school level context. By not controlling for school context, we face two serious estimation problems. First, ignoring the school level will lead to an overestimate of the individual and neighborhood level variances. Assuming a balanced sample, no random slopes and using a General Least Squares (GLS) estimation procedure, the estimates of the individual and neighborhood level variance components when ignoring the school level are (Moerbeek 2004):

$$\begin{aligned} \sigma_i &= \sigma_i + \frac{n_i - 1}{n_i n_s - 1}\sigma_s \\ \sigma_n &= \sigma_n + \frac{n_i n_s - n_i}{n_i n_s - 1}\sigma_s, \end{aligned}$$

where n_i and n_s are the number of students and schools, respectively. Ignoring the school level distributes the school level variance to the individual and neighborhood levels according to the number of units at each level that are in the sample. The math for an unbalanced sample is not as straight forward. However in either case the general conclusion is that the explanatory power attributed to the neighborhood is inflated when ignoring the school level. The school explains some of the variation in Y_{ijk} and by ignoring it we misattribute some of that explanatory power to the neighborhood.

Similarly, ignoring the neighborhood level in a school effects analysis also affects the proportion of variance explained at the level of interest, in this case the school level. The variance component at the individual level remains the same, but the variance at the school level is biased upwards (Moerbeek 2004):

$$\begin{aligned} \sigma_i &= \sigma_i \\ \sigma_s &= \sigma_n + \sigma_s \end{aligned}$$

Ignoring school context distributes the school level variance to the individual and neighborhood levels because individuals are directly nested within schools and schools are directly nested within neighborhoods. In the case when the neighborhood level is ignored, the variance at the neighborhood level is completely added to the school level since it is only schools that are directly nested within neighborhoods. In either scenario, the variance component at the level of interest is inflated when a nesting or nested level is ignored.

Ignoring levels will also alter coefficient estimates and their standard errors. Van den Noortgate, Opdenakker and Onghena (2005) show that ignoring an intermediate level leads to an increased probability of type I error. Although the coefficient estimates at the neighborhood levels remain consistent, their standard errors are artificially lowered and thus increasing the size of the test statistic and lowering the associated $p - value$, leading researchers to designate an explanatory (potentially causal) interpretation of the effects of specific neighborhood characteristics on an individual outcome when in reality there are either weaker or no relationships present. If poor test scores have a strong dependence at the school level but the analysis only considers neighborhood clustering, incorrect inferences would be made at both the individual and neighborhood levels. Moerbeck (2004) shows that ignoring a higher level such as the neighborhood results in greater variance of the coefficients at the next lowest level, such as the school, and thus in a lower statistical power.

Accounting for the correlation between individuals within a neighborhood and recognizing the importance of controlling for school level context are the first steps to obtaining reliable findings. However, merely adding a school level in the statistical model may not be enough to obtain an unbiased estimate of θ. A multilevel model minimizes the correlation problem that arises when children are nested within schools and neighborhoods, but it does not solve the endogeneity problems that have plagued many neighborhood effects studies, regardless of the context or outcome of interest. As with any regression model, a multilevel model still assumes that all individual characteristics that correlate with D_k and Y_{ijk} are controlled for. However, by explicitly modelling school and neighborhood level context, a multilevel model must also assume that all school and neighborhood level variables that impact T_k and the outcome are accounted for. The following section discusses these conditions and explains how failing to meet them severely biases estimates of neighborhood effects.

2.4 Conditions for Estimating Causal Neighborhood Effects

While multilevel models minimize the issues related to clustering, we still need to meet further important assumptions in order to properly identify θ in equation (5). These assumptions are not related to the statistical model fitting the data, but to the data generating process itself. Therefore, the following section is not necessarily a critique of multilevel models per se, but an exposition clarifying the important concept that statistical models can only do so much in providing causal estimates when the data are flawed.

First, we need to define a few terms. Assume D_k takes on only two values. Define random variables representing what child i would score on a test had he lived in a neighborhood with $D_k = d^0$ and what the individual would score had he lived in a neighborhood with $D_k = d^*$. Denote these two potential outcomes by $Y_{ijk}(d^0)$ and $Y_{ijk}(d^*)$ and regard neighborhoods with a value of $D_k = d^*$ as treated and a value of $D_k = d^0$ as untreated. For each individual we observe only $Y_{ijk} = Y_{ijk}(d^0) + (Y_{ijk}(d^*) - Y_{ijk}(d^0))D_k$, so $Y_{ijk}(d^0)$ is not observed for those in neighborhoods with $D_k = d^*$ and $Y_{ijk}(d^*)$ is not observed for those in neighborhoods with $D_k = d^0$. We might nevertheless still identify certain averages of $Y_{ijk}(d^*) - Y_{ijk}(d^0)$. For example, we can decompose the difference in average test scores by D_k as (Angrist 1998):

$$\begin{aligned} E\left[Y_{ijk}(d^*)|D_k = d^*\right] - E\left[Y_{ijk}(d^0)|D_k = d^0\right] \;=\;& E\left[Y_{ijk}(d^*) - Y_{ijk}(d^0)|D_k = d^*\right] \\ &+ \left(E\left[Y_{ijk}(d^0)|D_k = d^*\right] - E\left[Y_{ijk}(d^0)|D_k = d^0\right]\right) \end{aligned}$$

This shows that average comparisons of test scores are equal to $E\left[Y_{ijk}(d^*) - Y_{ijk}(d^0)|D_k = d^*\right]$ plus some bias term that reflects the fact that test scores in neighborhoods where $D_k = d^0$ are not representative of what individuals in neighborhoods where $D_k = d^*$ would have scored had they been in $D_k = d^0$ neighborhoods. We can see this bias more explicitly with respect to the individual, school, and neighborhood unobservables in the multilevel model specified in equation (5). A generalized least squares regression of (5) yields the following estimator of θ:

$$\widehat{\theta} = (\bar{Y}_{ijk|D_k=d^*} - \bar{Y}_{ijk|D_k=d^0}),$$

where the right hand side is the observed difference in conditional sample means. Expressing this equation in terms of the causal parameter and the unobservables yields the following:

$$\widehat{\theta} = \theta + (\bar{\mu}_{k|D_k=d^*} - \bar{\mu}_{k|D_k=d^0}) + (\bar{\lambda}_{jk|D_k=d^*} - \bar{\lambda}_{jk|D_k=d^0}) + (\bar{\epsilon}_{ijk|D_k=d^*} - \bar{\epsilon}_{ijk|D_k=d^0}), \qquad (2.6)$$

which shows that the estimator equals the causal effect θ and bias terms related to the unobservables at the individual, school, and neighborhood levels. If treated and untreated individuals are similar with respect to their unobservable individual, school, and neighborhood characteristics, then these bias parameters disappear. Otherwise, the general least squares estimator is biased.

In order to eliminate the bias terms, we need to make several assumptions. These assumptions fall under the umbrella of ignorability (Rubin 1974), which states that conditional on covariates, the potential outcomes $Y_{ijk}(d^0)$ and $Y_{ijk}(d^*)$ are independent of D_k. More formally, we will say that neighborhood variable D_k is ignorable given the covariates X_{ijk}, S_{jk}, and Z_k if the following assumptions are satisfied:

Assumption 1: $E\left[\epsilon_{ijk}|X_{ijk}, S_{jk}, Z_k, D_k, s_{jk}, z_k\right] = 0$
Assumption 2: $E\left[\lambda_{ik}|X_{ijk}, S_{jk}, Z_k, D_k, z_k\right] = 0$
Assumption 3: $E\left[\mu_k|X_{ijk}, S_{jk}, Z_k, D_k\right] = 0$

Assumption 1 is typically known as the strict exogeneity assumption, which states that unobserved individual level variables are uncorrelated with the explanatory variables at all levels. Assumption 2 is referred to as the uncorrelated school effects assumption, which implies that unobserved characteristics of the school that influence test scores are not correlated with pupil, school, or neighborhood characteristics that are included in the model. Assumption 3 is referred to as the uncorrelated neighborhood effects assumption, which suggests that unobserved neighborhood characteristics are uncorrelated with all explanatory variables included in the model. The validity of variance estimators and the efficiency of the random effects parameters rely on meeting all three conditions (Halaby 2004). These assumptions also must hold in a school effects analysis.

These conditions imply that in order to obtain a causal neighborhood effect of D_k, for every neighborhood k, school j and each individual i the covariates X_{ijk}, S_{jk}, and Z_k include all relevant variables that affect both neighborhood level treatment D_k and the potential outcomes $Y_{ijk}(d^0)$ and $Y_{ijk}(d^*)$, i.e. that X_{ijk}, S_{jk}, and Z_k contain all individual, school, and neighborhood-level confounding variables. While we typically see neighborhood effects studies address conditions 1 and 3, which imply that the assignment of neighborhood level treatment D_k is independent of any individual or neighborhood level factors, assumption 2, which states that D_k is independent of school context, is normally ignored. In fact, as reviewed in section 2, most neighborhood effects studies on educational outcomes do not even include S_{jk} to control for observable school level factors, which may not completely satisfy assumption 2 since there may be unobservable school characteristics that are jointly correlated with D_k and the outcome.

If assumptions 1-3 are met, we can estimate the average causal effect of D_k. By law of iterated expectations we obtain the following:

$$
\begin{aligned}
E\left[Y_{ijk}(t)\right] &= \sum_{x,s,z} E\left[Y_{ijk}(d)|X_{ijk}=x, S_{jk}=s, Z_k=z, D_k, \mu_k, \lambda_{jk}, \epsilon_{ijk}\right] P(X_{ijk}=x, S_{jk}=s, Z_k=z) \\
&= \sum_{x,s,z} E\left[Y_{ijk}(d)|X_{ijk}=x, S_{jk}=s, Z_k=z, D_k=d, \lambda_{jk}, \epsilon_{ijk}\right] \\
&\qquad\qquad P(X_{ijk}=x, S_{jk}=s, Z_k=z) \qquad by(1) \\
&= \sum_{x,s,z} E\left[Y_{ijk}(d)|X_{ijk}=x, S_{jk}=s, Z_k=z, D_k=d, \epsilon_{ijk}\right] \\
&\qquad\qquad P(X_{ijk}=x, S_{jk}=s, Z_k=z) \qquad by(2) \\
&= \sum_{x,s,z} E\left[Y_{ijk}(d)|X_{ijk}=x, S_{jk}=s, Z_k=z, D_k=d,\right] \\
&\qquad\qquad P(X_{ijk}=x, S_{jk}=s, Z_k=z) \qquad by(3)
\end{aligned}
$$

We can then estimate the average treatment effect of D_k as:

$$\begin{aligned} E\left[Y_{ijk}(d^*)\right] - E\left[Y_{ijk}(d^0)\right] \\ = \sum_{x,s,z}\big(E\left[Y_{ijk}(d)|X_{ijk}=x, S_{jk}=s, Z_k=z, D_k=d^*\right] \\ - E\left[Y_{ijk}(d)|X_{ijk}=x, S_{jk}=s, Z_k=z, D_k=d^0\right]\big)P(X_{ijk}=x, S_{jk}=s, Z_k=z) \end{aligned} \quad (2.7)$$

This equals the parameter θ in equation (5), which represents the average causal effect of neighborhood level variable or treatment D_k on the test scores of individuals in the neighborhoods in which the treatment occurs. We minimize individual selection into neighborhoods through observable controls X_{ijk} and Z_k and conditions 1 and 3. We minimize the influence of school context by controlling for school level variables S_{jk} and fulfilling assumption 2.

In the previous section I outlined a possible route, multilevel modeling, to control for school context in the estimation of neighborhood impact on individual schooling outcomes. If we can control for school context through observable school characteristics S_{jk} and be sure that any unobservable school characteristics are not correlated with both test scores and D_k (assumption 2), then we can estimate θ with the assurance that independent school level factors are controlled for.

However, although fitting a multilevel model that includes a school level minimizes the effects of within school clustering, it does not necessarily control completely for the independent impact of the school since school and neighborhood level characteristics may be deeply confounded. We can include etiologically important individual and school level variables in the model to control for this confounding. However, some of these variables may correlate with etiologically significant neighborhood level covariates, causing σ_n to be underestimated because some of the variance related to D_k has been inadvertantly absorbed by the included covariates. The problem, in substantive terms, is that we have a multicollinearity issue since individual, school and neighborhood level processes are very much intertwined. Bingheimer and Raudenbush (2004) describe this problem as one of composition versus context, where the complexity of multilevel selection and interaction processes makes it difficult to isolate independent effects using observational data.

A particularly thorny problem arises when individual and school level covariates may play the role of confounder and mediator simultaneously. For example, low school funding could negatively impact a student's test score and also reflect the poor conditions of a neighborhood; it might therefore create a spurious relationship between neighborhood poverty and individual academic success. Low school funding could also play a mediating role. A poor neighborhood fails to attract wealthy residents; this could lead to lower locally sourced school funding, which in turn could have consequences for academic success. In situations where an individual or school level covariate plays both confounding and mediating roles, including the covariate leads to an overadjusted estimate of θ, whereas omitting the covariate leads to an estimate that is biased by confounding.

These issues are not specific to multilevel models. Indeed, this technique explicitly recognizes that each neighborhood has idiosyncratic factors and that neighborhoods contain subgroups (schools) also possessing distinct place effects. But, the easy access to software capable of running multilevel models and the visual and methodological appeal of measuring variation at separate levels increase the risk of researchers haphazardly using multilevel models and making questionable inferential leaps (Oakes 2003). We must be careful about relying on any statistical model without understanding the processes that generate the data used to fit the model. Any model regardless of its complexity and methodological sophistication cannot yield causal estimates if the data are inherently flawed. Researchers wishing to obtain causal explanations of social processes should use multilevel modelling in conjunction with research designs and estimation techniques that yield unbiased estimators.

Given these obstacles, we can turn to randomized control trials (RCTs) to ensure that assumptions 1-3 are satisfied. However, RCTs are expensive and thus have rarely been conducted in the context of estimating neighborhood or school effects on schooling outcomes. Therefore, we turn to another route - natural experiments - in an attempt to fulfill assumptions 1-3 and circumvent the other methodological problems related to neighborhood effects estimation in the presence of school influence. Natural experiments attempt to capture the exogenous characteristics of the data derived from a randomized trial. The following section describes natural experiments and outlines the conditions they must meet in order to yield causal estimates of neighborhood effects.

2.5 Natural Experiments

We can decompose the variation of D_k into two components: a systematic component entirely predicted by a set of variables and a random or exogenous component. The goal in any causal analysis is to isolate the exogenous component and use it to estimate the effect of D_k on the outcome. We can do this by eliminating the systematic component by directly controlling for it. Some of the variables making up the systematic component are observable and thus we can insert them into the model. However, some of the variables may be *unobservable* and thus cannot be included; therefore, the variation in D_k is still contaminated. For example, if D_k is the mean income level in a neighborhood, we may be able to control for variables that jointly correlate with D_k and the outcome, such as the neighborhood's ethnic distribution and mean educational level. However, we may be missing certain variables, particularly those that are hard to measure. For example, a child's level of competence or initiative may affect education outcomes. If these factors also help determine where a person lives but are omitted, then the estimated effect will capture not only the impact of D_k but also the impact of the omitted variables that are correlated with both D_k and the outcome.

Instead of using all of the exogenous component, we can just focus on isolating a part of it. In many cases, this is difficult since the exogenous component is largely due to measurement or sampling error and thus is relatively small and difficult to identify. However, what if we make that component larger and its source more explicit? We can do that through an experiment in which we randomly select neighborhoods into different values of D_k. But, such experiments in the social sciences are rare. However, in some cases, differences in D_k are caused by some naturally occurring phenomenon that mimics the randomization that occurs in a social experiment. In this scenario, if we can determine the mechanism that exogenously altered the values of D_k, we can use it to measure a causal estimate of D_k's impact on the outcome of interest. In other words, we want a mechanism T_k that has no connection to unobservable individual, school and neighborhood level characteristics and alters the values of D_k for all neighborhoods k at time t.

The conditions under which the natural experiment approach properly identifies the effect of interest are quite stringent. Specifically, the "natural" event or mechanism must satisfy five conditions: 1.) The event cannot be anticipated by the community, 2.) Event occurrence is not correlated with community level characteristics, 3.) The event has community wide effects, 4.) The event cannot have spillover effects, and 5) The event has a monotonic relationship with D_k. The following section describes these conditions, thus providing for a more precise understanding of when a natural experiment is appropriate for obtaining causal estimates of neighborhood effects. A more technical delineation of the conditions can be found in the appendix.

Conditions

The data used in natural experiments come from phenomena that are often the product of natural, social or political forces. Weather shocks and natural disasters are common applications of natural experiments. For example, Miguel, Satyanath and Sergenti (2004) use poor weather conditions to study the effects of economic growth shocks on civil conflict in Africa. Kirk (2009) uses Hurricane Katrina to examine the effects of place of residence on criminal recidivism. For purposes of illustration, I will refer to natural disasters throughout this section as I outline the conditions that must be met in order to use certain phenomena as an instrument for a natural experiment. Note that these conditions also apply when using natural experiments to estimate school effects.

I must first define in more precise terms the basic characteristics of a candidate phenomena. Define the variable T_k as an indicator assigning a value of 1 if an event, such as a natural disaster, occurs in neighborhood k and a value of 0 otherwise. Some neighborhoods m are affected by the event (thus $T_k = 1$), while some neighborhoods n are not ($T_k = 0$). I will refer to the former as the treatment group and the latter as the control group. Given this setup, I define the first condition as follows:

Condition 1: Exogeneity Across Space The population of individuals in neighborhoods m are similar to the population of individuals in neighborhoods n with respect to individual, school and neighborhood characteristics that jointly affect outcome Y_{ijk} and the probability of event T occurring.

Many studies attempt to satisfy condition 1 by comparing a statistical summary of pre-event characteristics between neighborhoods m and n. A weaker version of this condition states that neighborhoods m and n are similar *conditioned* on observable individual X_{ijk}, school S_{jk} and neighborhood Z_k level variables. This weaker condition fully encompasses assumptions 1-3. Dunning (2008) notes that controlling for characteristics diminishes the inherent value of a natural experiment since the fundamental assertion is that the phenomena in and of itself yields "as-if" randomization. Adding controls opens doubt to this assumption.

We must also consider the temporality of the candidate event. A natural disaster doesn't occur in any neighborhood from time $t - s$ to time t, but occurs in some neighborhoods m but not other neighborhoods n at time t. The pre-treatment time period spans $t - s$ up to t while the post-treatment period spans t to $t + s$, including t. I now define the next condition.

Condition 2: Exogeneity Across Time The trend in pre-event individual, school and neighborhood characteristics that jointly affect the probability of event T_k occurring and outcome Y_{ijk} for individuals in neighborhoods m and n are not altered in expectation of the treatment.

We must consider the trend because those in neighborhoods m may expect the event and thus prepare for the shock. Both treatment and control groups may be similar across all characteristics at a certain time $t - s$, but dissimilar from time $t - s$ to t as individuals in neighborhoods m adjust their behavior in preparation for the event. Conversely, the two groups may be dissimilar at time $t - s$, but similar at time t. If the preparation also affects outcomes, then the effects of the shock are minimized and we obtain downwardly biased estimates.

There are interpretation problems even in scenarios where *both* the treated and control groups prepare for the event in a parallel manner. In this scenario, individuals in both groups alter their behaviors similarly in expectation of the event, thus their characteristics are comparable at times $t - s$ and t. Conditions 4 and 5 are met since group characteristics and trends are identical. However, we must change the interpretation of the estimated effect. For example, if we restrict our population to counties in the Gulf Coast and use a hurricane as a natural experiment, all counties likely implement similar measures to prepare for the hurricane even though it may only affect some counties but not others. Let's say we want to use this natural experiment to measure the effects of destroyed physical infrastructure on juvenile delinquency. As a part of their preparation for a natural disaster, government officials likely increase emergency fire and police support and allocate specific funds for the restoration of public infrastructure. The interpretation is not simply the effect of destroyed infrastructure on juvenile delinquency, but the effect of destroyed infrastructure on juvenile delinquency *in the presence of pre-event conditions that minimize risk*. In other words, the natural experiment achieves high internal validity since characteristics across treatment and control groups are similar across time, but it does not achieve high external validity since counties across the United States may not enact the types of programs and policies, vis-a-vis protection against hurricane damage, found in Gulf Coast counties.

We can verify condition 2 by comparing the trend in treatment and control outcomes and characteristics across several pre-event time points. If we find trend differences, we control for them directly in the regression model. Many studies compare characteristics months to even years before the event takes place, leaving ample time for preparation. We want to make sure we measure characteristics as close to the event as possible to verify that units did not alter their behavior in expectation of the event.

The next condition ensures that the event produces variation in the variable of interest and limits the causal pathway to include just the event indicator, the outcome and the variable of interest.

Condition 3: The event has an effect only on the variable of interest D_k. It does not affect the potential outcomes. This effect is not mediated through any individual, school or neighborhood characteristics.

The first statement in condition 3 ensures that the event creates variation in D_k. In other words, the event has a specific and large enough effect to alter the variable of interest. In the context of neighborhood effects, the event must be a large enough shock that it impacts the entire neighborhood. For example, if D_k is the neighborhood poverty rate, the shock, perhaps a natural disaster, must be large enough such that it decreases the poverty rate for the entire neighborhood and not just certain sectors. This condition is commonly known as the inclusion-restriction assumption in the IV literature.

The rest of condition 3 ensures that the event works only through D_k to affect the outcome. We can violate this assumption in three ways. First, the event affects the outcome directly. This is shown in pathway (a) in Figure 1. Second, the event affects the outcome through unobservable variable U. This is shown in pathway (b) in Figure 1. These two conditions combined is commonly known as the exclusion-restriction assumption. Third, the event affects D but through some unobservable variable S. This is shown in pathway (c) in Figure 1. If the effect of the event on D goes through another variable, we obtain upwardly biased estimates. For example, an event may directly impact some aspect of the school S, which then affects the poverty rate D in the neighborhood. The estimated effect captures variation that is a function of changes at the school level. We want variation solely as a function of the changes in the neighborhood, particularly through D.

The final two conditions are standard requirements in IV regression that also apply to natural experiments.

Condition 4: Stable Unit Treatment Value Assumption (SUTVA). The potential outcomes for each person are unrelated to the treatment status of other individuals.

The condition follows the basic regression assumption of no interference or the independence of units. Individuals in neighborhoods not experiencing a natural disaster may be affected by surrounding affected neighborhoods due to spillover effects. Individuals from non-affected neighborhoods may rely on employment from businesses destroyed in nearby affected areas. Residents from affected neighborhoods might migrate to surrounding non-affected neighborhoods, increasing congestion and putting stress on limited resources in those

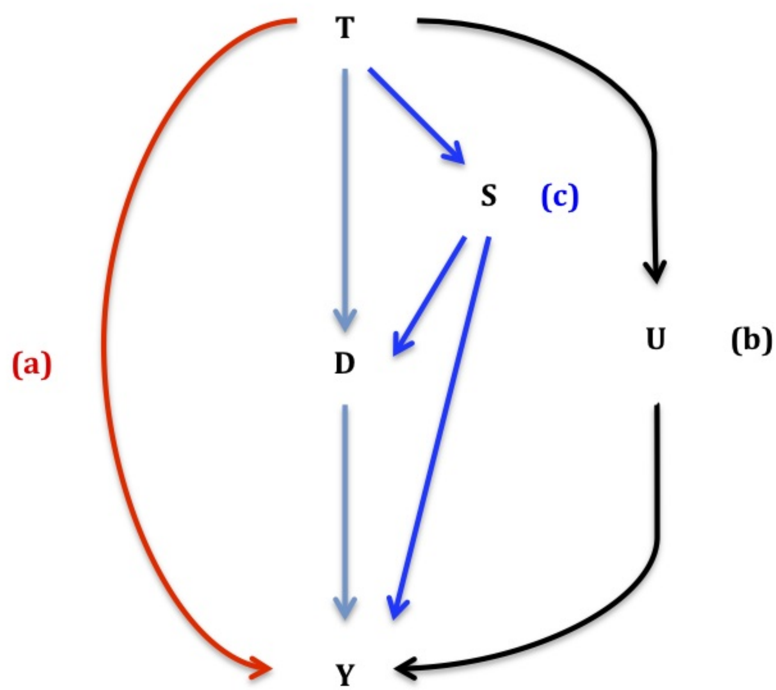


Figure 2.1: Natural Experiment Pathways: T is the event indicator, D is the variable of interest, Y is the outcome, S is an unobserved mediating variable between T and D and U is an unobserved mediating variable between T and Y.

neighborhoods. Consequently, these spillover effects likely impact outcomes in non-affected communities. One can detect violations of SUTVA by examining correlations between non-affected areas and spatially adjacent affected units. If spatial correlation is detected, one can control for this directly in the model (Anselin 1988) or exclude geographically adjacent units from the analysis.

Condition 5: Monotonicity. A hypothetical change in the treatment status either has no impact on a unit's value of D_k, or changes it in the same direction as it does for all other units for which it has an impact.

Monotonicity eliminates the possibility of heterogeneous effects and is also known in the IV literature as the *no defiers* assumption. This is an assumption on the unobserved counterfactuals: Let's say you observe an individual with $T_k = 0$ and $D_k = 1$. If his T_k were to change to 1, then the monotonicity assumption says that his D_k must stay at 1; it cannot go down to 0. Let's say a combination of wealthy and poor neighborhoods are not affected by a natural disaster. Condition 5 states that if a natural disaster did occur in these neighborhoods, they would experience either no changes in poverty levels or changes in the same direction, which we presume to be an increase in levels. A wealthy neighborhood that experiences a decrease in poverty levels given a natural disaster occurrence breaks condition 5. This scenario can happen if wealthy residents from nearby poor and affected neighborhoods decide to migrate to wealthier areas since these neighborhoods have the appropriate infrastructure to minimize the effects of a disaster and rebuild quickly.

If a natural experiment meets the above 5 conditions, a researcher carrying out this type of study can make the credible claim that the assignment of non-experimental subjects to treated neighborhoods m and control neighborhoods n is as good as random. Using a natural experiment, we can estimate the coefficient of interest θ a number of ways. We can measure the coefficient directly from D_k as expressed in equation 7. We don't include any direct measure T_k of the event since the underlying assumption is that the variation in D_k is exogenous. Black, McKinnish and Sanders (2003) use macro economic downturns in the coal and steel industries to estimate the effect of long-term changes in demand for low skilled workers on welfare expenditures. They regress county welfare expenditures directly on county earnings, which is their measure of worker demand. Cohen et al. (2006) use the 1992 Los Angeles riots to study the effects of neighborhood alcohol outlet density on gonorrhea rates. Using a multilevel framework, they estimate the effect of annual alcohol density before and after the riots in Los Angeles census tracts. In both of these examples, the models include the variable of interest D_k, county welfare expenditures in the Black et al. study and alcohol outlet density in the Cohen et al. analysis, but no direct measure of T_k, downturns in he coal and steel industries and the 1992 Los Angeles riots.

Some natural experiments do not have a direct measure of D_k available. Instead, these studies insert T_k into the model and interpret its coefficient as the effect of interest. In this case, reduced form estimates are presented. In the example examining the effects of damaged infrastructure on juvenile delinquency, let's say we dont have a variable D_k that directly measures damaged infrastructure. Define T_k as an indicator variable assigning a value of 1 if the neighborhood was affected by a natural disaster and 0 otherwise. By using the coefficient on T_k as our effect of interest, we bypass the direct measure of D_k and make the claim that areas affected by natural disasters experience damaged infrastructure and areas not affected do not. Torche (2011) uses a major earthquake that occurred in Chile in 2005 to measure the effects of environmentally induced prenatal maternal stress on child birth weight. Using a differences-in-differences regression, Torche's T_k variable categorizes women into three categories based on the level of earthquake intensity experienced in their respective regions. The author does not use a variable D_k to directly measure maternal stress (e.g. cortisol levels), but uses an indicator of earthquake intensity as a proxy.

The most common method for estimating effects from a natural experiment is instrumental variables (IV) regression. Through this framework, the analyst makes explicit use of the variation caused by the event to estimate the relationship between D_k and the outcome. We estimate θ using a two-stage least squares estimation, where we would regress D_k on T_k, obtain the fitted values $\widehat{D_k}$ from this regression and regress Y_{ijk} on $\widehat{D_k}$. Many applications of this strategy can be found in the natural experiment literature, including Angrist (1990), Angrist and Lavy (1999), Miguel, Satyanath and Sergenti (2004), Kirk (2009), Brady and McNulty (2004) and McClellan et al (1994). Note that the above procedures can be utilized through a multilevel framework (see Yu et al. 2008 as an example).

2.6 Discussion

The conditions outlined in the previous section encompass the basic requirements that must be met in order to obtain causal estimates from a natural experiment. Failing to meet any of these conditions considerably weakens the validity of as-if randomization. There are several other issues that researchers must be aware of when constructing and interpreting natural experiments.

First, the type of subjects exposed to the event might be less like the populations in which we are most interested in. For example, hurricanes may occur randomly in Gulf Coast neighborhoods, but we highly doubt that neighborhoods in the Gulf Coast are generally like other populations. As discussed earlier, in many cases a natural experiment trades off high external validity for high internal validity.

Second, if we are interested in the effects of damaged infrastructure on juvenile delinquency, levels of damage from a natural disaster may or may not have similar effects on juvenile delinquency as, say, damage from civil conflict or general urban decay. Therefore, not only may the study population be non-generalizable, but the particular randomizing mechanism used by the event may have effects that are distinct from the effects of greatest interest.

Lastly, a common criticism of the natural experiment approach is that it does not spell out fully the underlying theoretical relationships (Rosenzweig and Wolpin, 2000). Natural experiments typically bundle many distinct mechanisms. Not only does this prevent researchers from understanding the processes that link neighborhoods or schools to individuals, it also prevents them from determining whether the treatment in fact yields causal estimates about the real hypothesis of concern.

Meeting the five conditions outlined above is a non-trivial task. Some of these conditions can be examined and tested empirically, for example presenting summary statistics of pre-event characteristics and trends to verify conditions 1 and 2. However, the bulk of the work that will go into satisfying the conditions will be largely based on a qualitative understanding of the candidate event. Dunning (2010) makes the case that qualitative methods make the most crucial contributions to constructing and executing natural experiments. Not only does indepth substantive knowledge provide compelling evidence of the event's randomness, but it also leads the researcher to a richer understanding of the mechanisms through which treatments exert their impact. Therefore, a strong empirical analysis using a natural experiment approach requires a combination of quantitative and qualitative methods. For exemplary examples, see Galiani and Schargrodsky (2004) and Miguel, Satyanath and Sergenti (2004).

Dunning (2010) presents a useful framework for evaluating the effectiveness of a natural experiment. He identifies three dimensions along which natural experiments vary: plausibility of as-if randomization, the credibility of the statistical models used to estimate the effects from the natural experiment and the substantive relevance of the event. Higher plausibility of as-if randomization can be assessed through a full quantitative and qualitative assessment of the candidate event. Dunning asserts that since the value of natural experiments is their ability to mimic the exogeneity produced in randomized trials, the statistical models employed in both designs should be similar. If natural experiments are like randomized experiments, then simple statistical methods, such as comparisons of means and percentages across the treatment and control groups, should suffice. The use of more sophisticated and complicated regression procedures only tacks on additional assumptions to the analysis and creates doubt surrounding the randomizing feature of the natural experiment.

The last dimension in Dunning's framework underscores the tradeoff between generalizability and internal validity in a natural experiment. In some studies, a natural experiment allows researchers to study treatments that are not amenable to true experimental manipulation. However, some of these interventions are too broad in scope such that exogeneity is likely not possible. In cases where internal validity is high, the treatment is too narrow such that that the findings benefit only a limited population and adds value to a restricted set of hypotheses. Researchers should aim for high external and internal validity, which produces greater substantive relevance. However, in many cases this goal is not feasible and researchers should adjust the interpretation of the findings given the placement of the experiment on Dunning's scale.

We must also distinguish natural experiments from two separate but routinely linked concepts: instrumental variables and quasi experiments. Natural experiments and instrumental variables are routinely defined in conjunction and are taken as synonymous procedures. However, they attend to separate stages in the analysis process. Natural experiments relate to the *research design* of a study while IVs relate to the *modelling procedure*. While a natural experiment is a means for obtaining data, an IV is a means of analyzing that data. We don't consider an RCT as a statistical procedure, but a research design for generating data. We must think of a natural experiment in the same manner. An IV and a natural experiment are often confused to be one and the same because it is common to use an IV to estimate the effects from a natural experiment. But, as discussed in the earlier section, we can analyze a natural experiment using other analytic techniques.

Similar to a natural experiment, a quasi experiment defines the data generating process in an analysis. However, while a natural experiment claims exogeneity, quasi experiments are planned or intentional treatments or events that resemble randomized field experiments but lack full random assignment. An example of a quasi experiment is the Gautreaux Project, a Chicago based housing desegregation project that provided assistance to residents living in highly segregated neighborhoods to move to more racially diversified communities. The project resembles an experiment in the sense that some residents were treated (received vouchers) while others were not. But, the project was not a true experiment; residents were not randomly assigned to receive assistance. The benefits of a quasi experiment are that it establishes the precise assignment mechanism and distinguishes between treated and control units. However, a quasi experiment does not produce exogeneity in the variables of interest and thus has limited value compared to a valid natural experiment.

We must also emphasize that natural experiments are observational studies. With natural experiments, data come from naturally occurring phenomena that are often the product of social and political processes. However, unlike in a RCT, the manipulation of treatment variables is not generally under the control of the researcher. But, if the conditions outlined in the previous section are satisfied, unlike other nonexperimental approaches, a researcher carrying out a natural experiment can make a credible claim that the assignment of non-experimental subjects to treatment and control conditions is as-if random. This is an attractive feature to researchers attempting to make causal interpretations about a social process that cannot be tested experimentally through an RCT.

Lastly, although we primarily frame natural experiments in the context of estimating neighborhood effects, the strategy can also be used to estimate school effects. In this case, the candidate event creates as-if randomization at the school level, controlling for school level confounding and individual and neighborhood level context. For examples of uses of natural experiments in estimating school level effects, see Angrist and Lavy (1997), Case and Deaton (1999) and Hoxby (2000), all of whom take advantage of variation in class sizes due to exogenous circumstances to examine the effect of class size on educational outcomes, Imberman, Kugler and Sacerdote (2012), who use Hurricane Katrina to estimate the effects of school peer groups on education outcomes, and Gould, Lavy and Paserman (2004), who use the mass emigration of Ethiopian immigrants to Israel to measure the effects of school quality on dropout rates and test scores.

2.7 Conclusion

I began this paper with an overview of the current research examining the effects of neighborhood and school conditions on youth schooling outcomes. From this review, I found that a majority of neighborhood effects studies control for potential individual and neighborhood level confounders, but ignore school level context. I also find that most school effects studies fail to control for neighborhood level context. Failing to account for either context carries severe consequences, including an overestimate of neighborhood and school explanatory power. A common solution to this problem is to use a multilevel framework to model individual, school and neighborhood levels explicitly. Researchers have come to increasingly rely on multilevel modelling to estimate neighborhood and school effects because of its intuitive appeal, explicit formalization of school and neighborhood level variation, and easy and ubiquitous access in standard statistical software packages. However, fitting a multilevel model and adding controls at the individual, school and neighborhood levels may not be enough to meet the relatively stringent conditions needed in order to make causal claims about neighborhood (or school) influence. The three conditions outlined in section 4 are non-trivial and are rarely met in observational studies using multilevel models or any statistical procedure. The primary goal in this article is to offer a strategy - natural experiments - for meeting these conditions without having to set up an expensive randomized experiment. Natural experiments are not new, as they have been applied in other fields for many years[2], but they have rarely been applied in the context of estimating neighborhood and school effects, particularly when the outcome of interest is schooling related.

The goal of this paper is not to denigrate multilevel modelling as an analytic tool, but to emphasize that regression models can only do so much to bring the field closer to causal estimates of neighborhood and school effects if the variation in the variable of interest is not exogenous and its source not clearly defined. We can gain some traction on this problem by not getting hung up on the statistical models that fit the data, but instead focus more on the research design in the first place. Randomized control trials can provide researchers the data they seek to make causal claims, but experiments in a social science setting are expensive. Natural experiments can afford school and neighborhood effects researchers with powerful inferential tools for improving the quality of their substantive inferences at a relatively light cost.

Natural experiments do not provide a quick fix to the challenges of causal inference. In every study, analysts are challenged to think carefully about the correspondence between the assumptions of models and the empirical reality they are studying. Natural experiments are valuable to the extent they build on real substantive knowledge, social theory and appropriate methodological facility, and an awareness of the trade-offs in this style of research.

[2]For a review, see Dunning (2008); Angrist and Krueger (2001)

Chapter 3

Riot Shocks and Adolescent Dropout Rates

3.1 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 Rodney Kings riots 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 1960s, led by Seymour Spilerman (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 Spilerman'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.

3.2 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 Spilerman (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.

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 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

[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).

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 important assumption behind my identification strategy is that there were unharmed cities in the 1960s that were similar demographically to cities affected by riots. For example, Youngstown, Ohio, a midwestern city with a population of 166,689 and a percent non-white of 19.1 percent experienced riots in 1968 and 1969, totaling 290 arrests, 42 cases of arson and 34 injuries. However, Kansas City, Kansas, a city with a population of 121,901 and a percent non-white of 23.2 percent was not affected. Jackson, Mississippi, with a population of 144,422 and a percent non-white of 35.7 percent experienced several riots in the late 1960's that led to 13 total days of riots, 27 arrests, 18 cases of arson and 26 injured. But, Savannah, Georgia, with a similar population size and percent non-white did not experience a riot.

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).

The federal government formed a national commision in 1968 to investigate the potential causes of the 1960s riots. The Kerner Commision Report (as it would come to be known) linked riot violence to a lack of economic opportunities for African American residents and recommended desegregation efforts at the community level to alleviate this problem. Similarly, the Federal Bureau of Investigation released the Webster Commision Report, a comprehensive, two-volume overview of the 1992 Los Angeles riots, which includes a chronology of events, responses by law enforcement, and an analysis of underlying causes.

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. Spilerman 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 results confirmed earlier findings by Lieberson and Silverman (1965) in their analysis of 76 black-white race riots between 1913 and 1963. They found no important connection between the occurrence of riot violence and a number of community level variables, including change in white or black populations, the percentage of black males who are self-employed, white unemployment, white and black median income and the percent of dilapidated houses. Horowitz (1983) dismisses variables measuring city-level and black specific disadvantage and discredits theories of marginality. Similar to Spilerman, He asserts that because of national currents there was enough racial tension in the air that a riot "could occur almost anywhere."

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) expanded Spilerman's data set to include riots up to 1992 and confirmed 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.

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 out-migration 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.

Table 3.1: Causes of Riots Summary

Study	Temporal Scope	Riot Measure	Number of Cities	Method	Significant Findings (direction of effect)	Non significant Findings
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,

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 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 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 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 employment 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

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

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).

I focus on the effects of neighborhood quality on high school dropout rates because of the importance of educational attainment to the development of human capital and a school's holding power on the future socio-economic health of a city and its adolescent residents. In the short term, high school dropout rates may be particularly affected since youths make up a non-trivial percentage of riot participants. In the long run, dropout rates reflect a riot's cumulative damage on various neighborhood level mechanisms linked to child well-being. On an aggregate level, in order to rebuild after such a debilitating, city-wide shock, a community must rely on, amongst a number of institutions, its schools to resuscitate its social and economic health. The greater rate at which a city graduates its children, the larger pool of highly skilled individuals it can draw from for future economic and social capital. High school dropouts also increase competition in a riot depressed labor market. If a high school dropout can't find a job, he is added to the expanding pool of unemployed residents, increasing stress on a city's already fragile economy. Dropout rates are a reflection of neighborhood school quality (Brasington 1997, Ries and Somerville 2010). Lower school quality may detract wealthier families, who help the local economy through such externalities as a larger tax base, from moving in.

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 within 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. The current study attempts to answer this question by using plausibly exogenous variation in city quality induced by riot occurrences.

3.3 Data

1960's Riots

In studying the causes of the 1960s riots, Spilerman (1970, 1971, 1976) collected data measuring the extent of damage on riot-affected cities. Gregg Carter (1986) subsequently extended Spilerman'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 riot related deaths, injuries, arrests, and arsons. Carter excludes disturbances related to organized Civil rights protests and those occurring in schools[3].

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 into the dataset the following control variables from the 1950, 1960, 1970 and 1980 decennial census through the City and County Data Book: civilian unemployment rates, population total, educational attainment, median housing values, non-white population, and region. 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 an analytic sample of 147 riot-affected cities. 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 the final sample size 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 of 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.

[3]Carter collected information from the Congressional Quarterlys Civil Disorder Chronology, the New York Times Index, the Report of the National Advisory Commission on Civil Disorders, Brandeis University Lemberg Center for the Study of Violences Riot Data Review, unpublished material from the Lemberg Center, the U. S. Senates compilation reported in Riots, Civil, and Criminal Disorders, and original newspaper articles from the New York Times and the Washington Post

Table 3.2: 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)

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[4]

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

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

[4]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.

Table 3.3: 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)

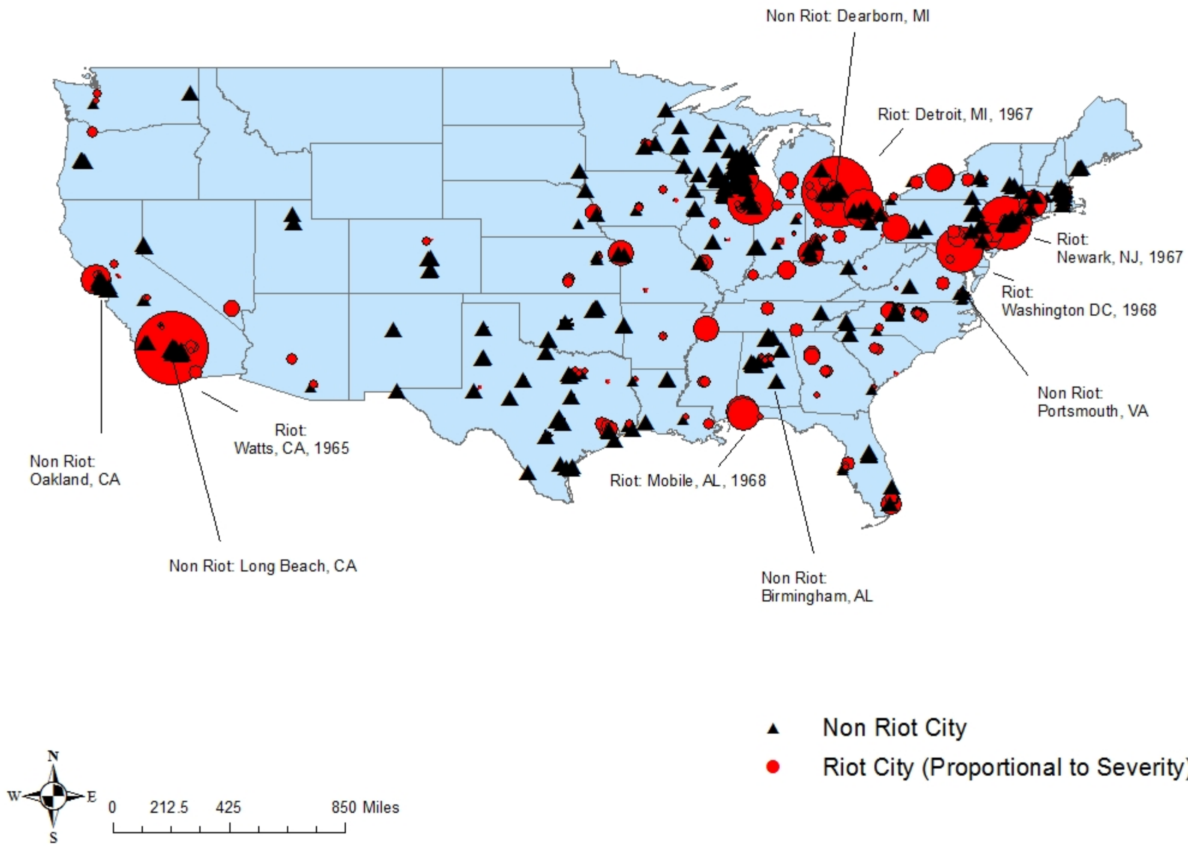


Figure 3.1: Cities > 50,000 total population affected and not affected during the 1960s Riots

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 are 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.

3.4 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 to the educational well-being of its residents to estimate the effects of lowered city quality on high school dropout rates. Through this strategy, I 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 $t + 1$[5]." This analysis does

[5] 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, I am directly

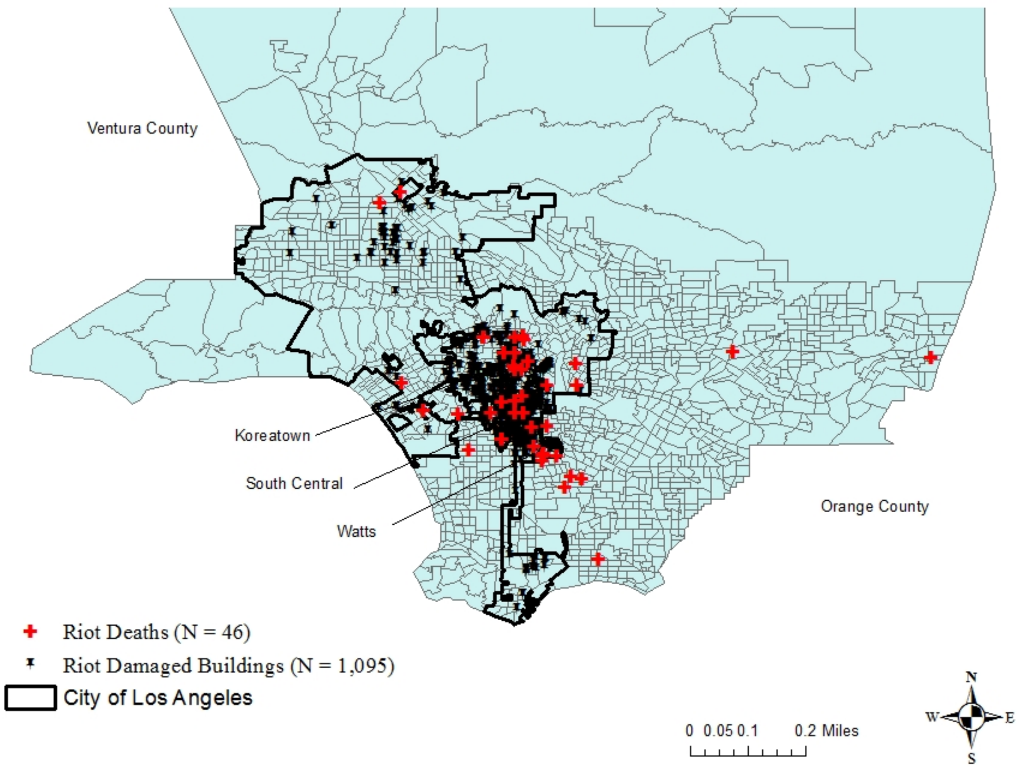


Figure 3.2: Locations of deaths and damaged structures during the 1992 Los Angeles Riots.

not allow us to identify the specific city characteristics that produce change in dropout rates. A riot changes an entire bundle of neighborhood characteristics, and I am estimating the average effect of this change.

The riot treatment negatively affects the bundle of goods that make up a city's quality. In neigborhood effects literature, the bundle of goods represent the mechanisms that neighborhoods work through to affect residents. A primary concern in the study of neighborhood effects is the identification and measurement of these mechanisms (Galster 2010). Instead of explicitly measuring the distinct mechanisms that affect education outcomes, I sidestep this problem and claim that riot treatment is city-wide and negatively affects the collective set of mechanisms that potentially influences education outcomes. The findings that housing values and taxable sales, typical proxies of quality, decrease post-riot treatment support this

estimating the reduced form parameter σ.

claim.

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.

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 relevant time period. Ideally I would use individual-level data but existing public-use microdata from the 1960 census do not contain city-level codes.

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. The more we know about the mechanisms underlying riot occurrence, the more confident we can be in obtaining unbiased estimates of the riot treatment effect after controlling for these mechanisms. 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, I 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

$$\Delta log(Y_i) = \alpha + \sigma_{ols} log(S_i) + \theta X_i + \epsilon_i \tag{3.1}$$

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 [6]. 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

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

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 Spilerman's variables and including the expanded covariate set. The coefficient for the 1960 rate variable is negative, 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 indicate 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. I 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.

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 between 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 Spilerman (1970,

Table 3.4: 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

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:

$$log(Y_{it}) = \alpha + \beta_1 D_i + \beta_2 P_t + \sigma_{did} D_i \cdot P_t + \theta X_{it} + \epsilon_{it}, \tag{3.2}$$

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 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 Spilerman's variables and including the additional covariates, respectively, and columns 3 and 4 present results for the 1960-80 period. Similar to the OLS findings, I 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. I 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. I 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 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.

Table 3.5: Difference-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

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[7]. 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 $N-J$ 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, which is calculated

[7]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.

as:

$$\widehat{\sigma_1} = (Y_{1t_2} - Y_{1t_1}) - (Y_{0t_2}W - Y_{0t_1}W) \tag{3.3}$$

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

$$\widehat{\sigma}_{synth} = \sum_{i=1}^{J} \sigma_i / J \tag{3.4}$$

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 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 formal 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 groups, 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. I 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.

Table 3.6: 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

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

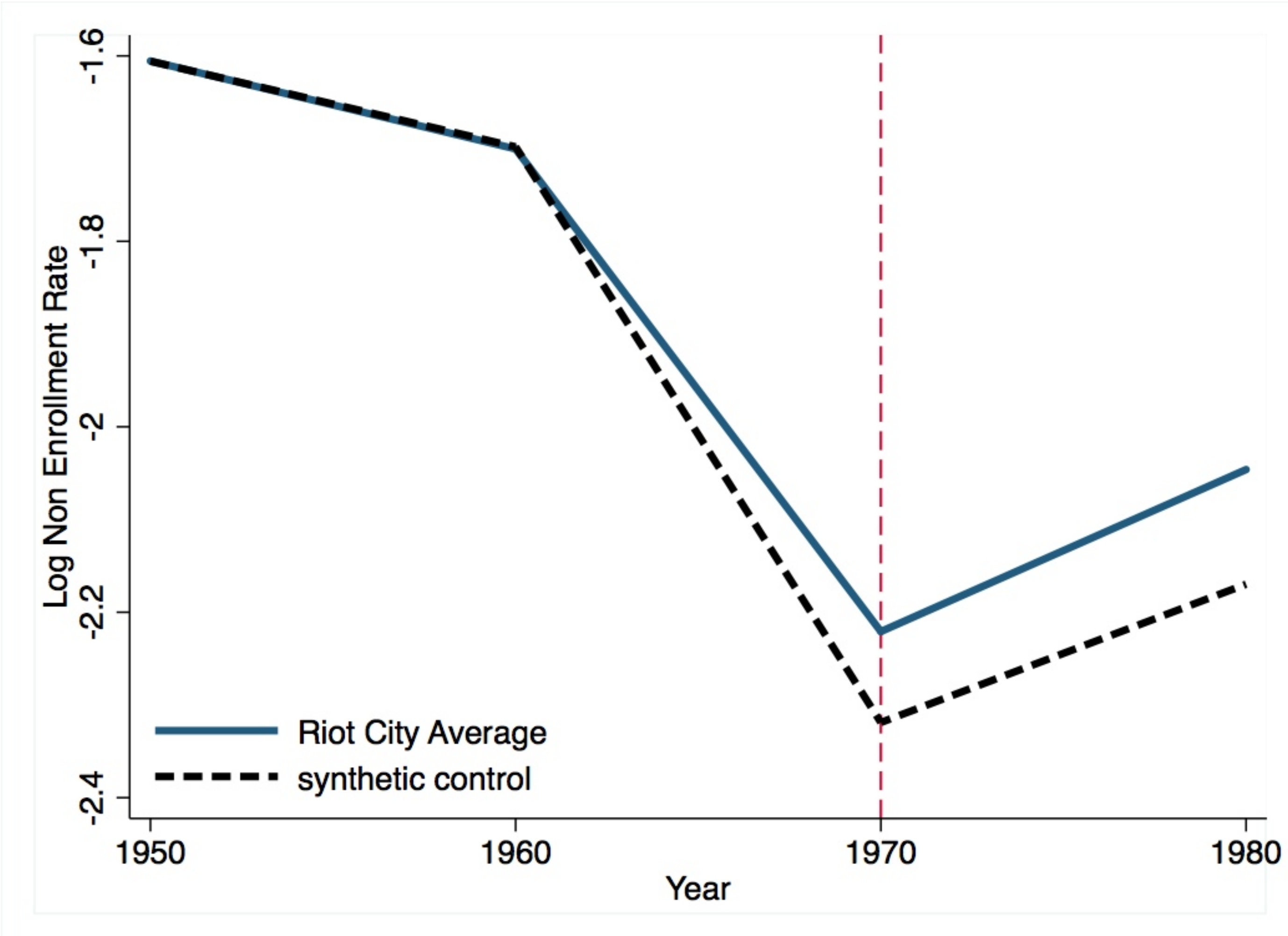


Figure 3.3: 1960s Riots Synthetic Control Matching: Trends in Log non-enrollment Rates - Riot Treated Cities vs. Synthetic Match

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.

I 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%, I conclude that a riot shock does have a statistically significant effect on log non-enrollment rates in the short- and long-term. In summary, I 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 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 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.

Table 3.7: Estimated Effects on Log non-enrollment Rates of 1960s Riot Severity and occurrence by Model

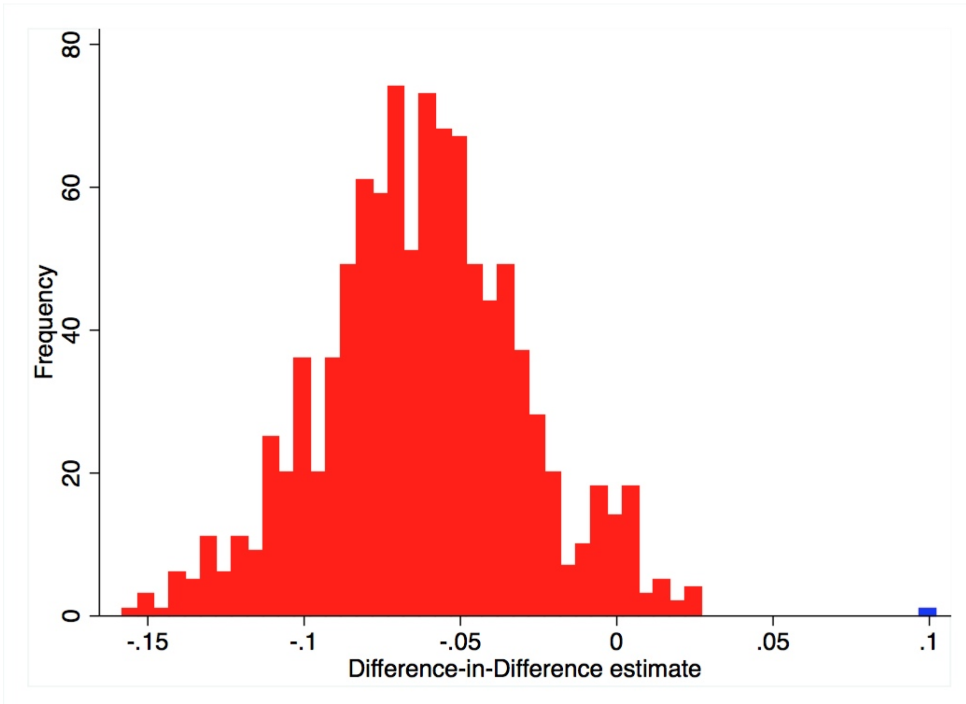
	1970	1980
Mean Difference††	0.253**	0.225**
	(0.001)	(0.123)
Ordinary Least Squares†	0.013**	0.006
	(0.004)	(0.004)
Difference-in-Differences††	0.122**	0.117**
	(0.020)	(0.032)
Synthetic Control -	0.098**	0.124**
Difference-in-Differences††	(0.002)	(0.002)

** $p < 0.05$

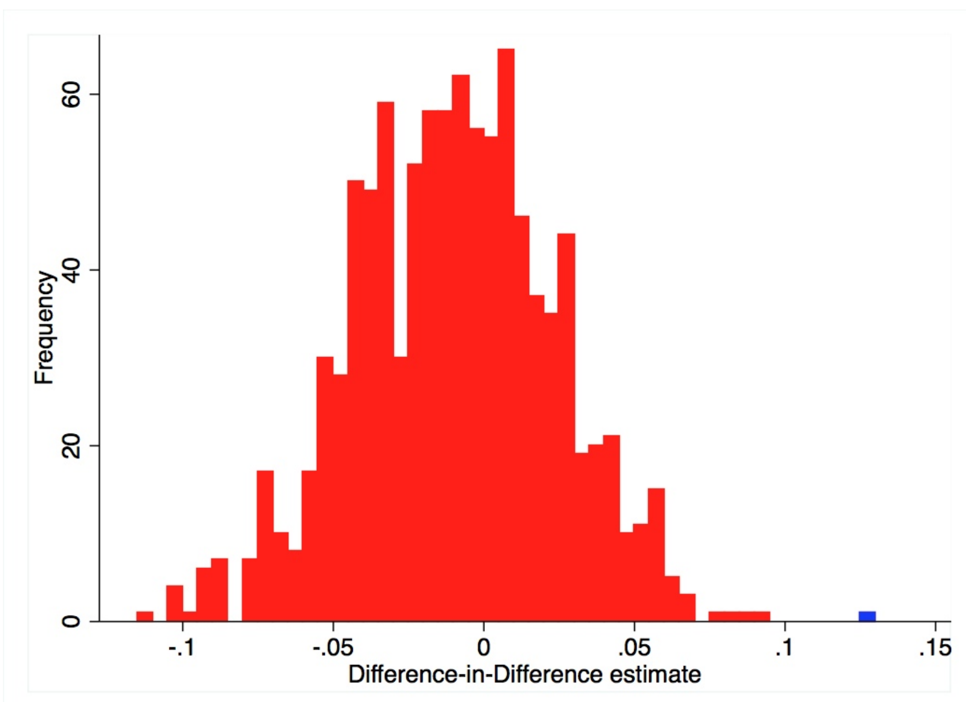
†Independent variable is riot severity. ††Independent variable is riot occurrence

two-sided p-values are in parentheses

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 city non-enrollment rates 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.



(a) 1970



(b) 1980

Figure 3.4: 1960s Riots Synthetic Control Matching: Histogram of 1,000 placebo permutations

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)). See Appendix B for tables of these results. 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[8]. 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.

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 have such negative effects 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.

Synthetic Matching

In order to assess the effects on dropout rates in Los Angeles after the 1992 riot, 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

[8] 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

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[9]. 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[10].

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.

[9]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 with boundaries that change very little from 1990 to 2000, it is safe to assume the same districts serve these cities in both 1990 and 2000.

[10]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

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. 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, I find that Los Angeles and its synthetic match have similar log dropout rates in the pre-treatment period. Using the permutation test outlined in the previous section, 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.

Table 3.8: 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

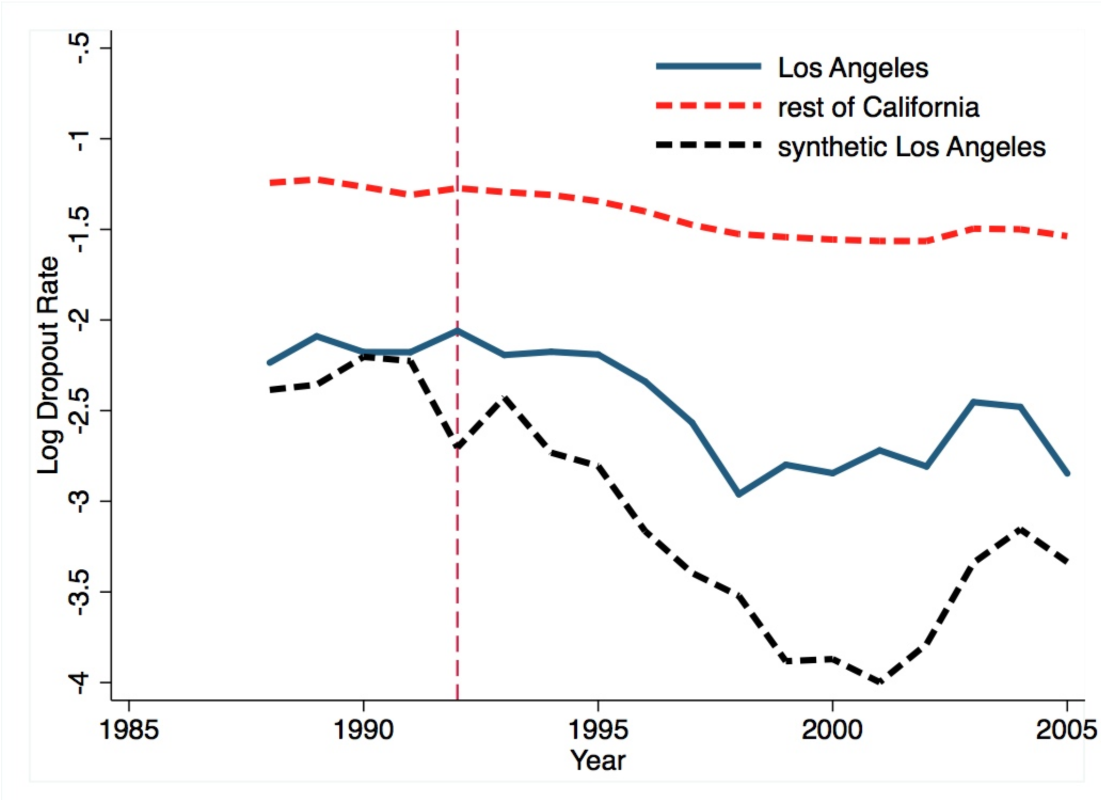


Figure 3.5: Synthetic Control Matching: Trends in Log Dropout Rates - Los Angeles, California minus Los Angeles, and Synthetic Los Angeles, 1988-2005

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, then 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[11].

[11] Abadie et al. (2010) suggests eliminating cities with a pre-riot mean squared prediction error (MSPE),

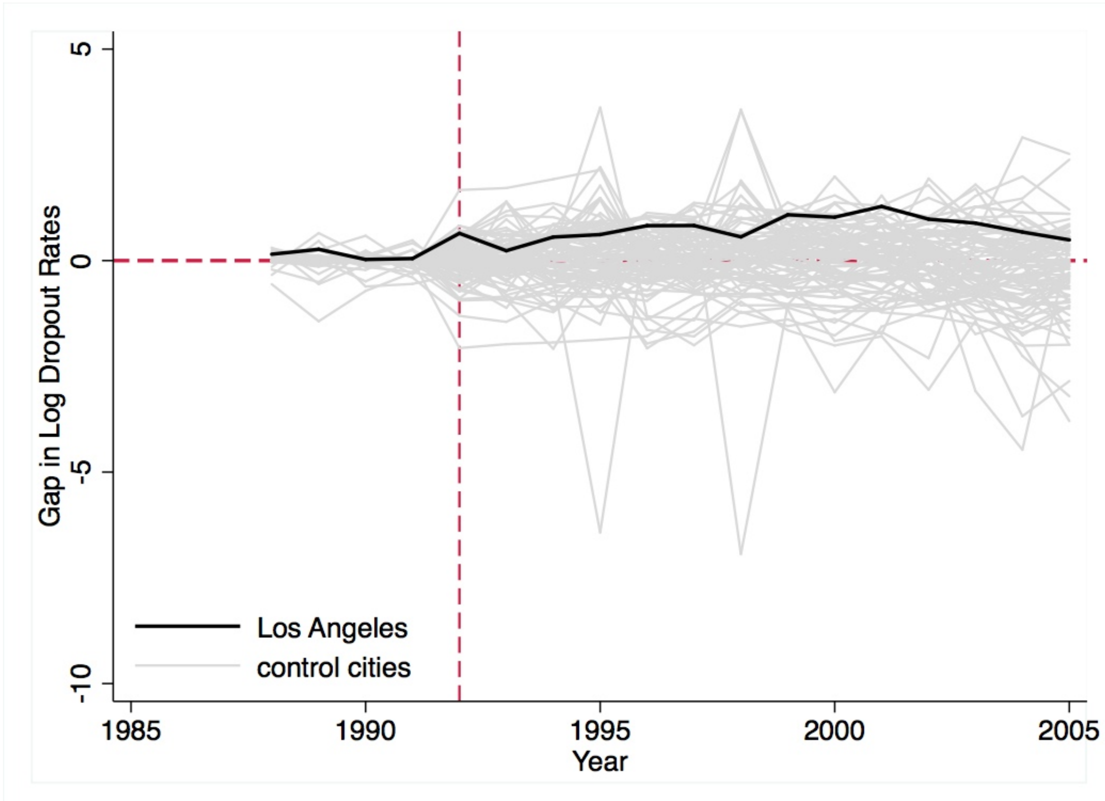


Figure 3.6: Synthetic Control Matching: Log Dropout Rate gaps in Los Angeles and placebo gaps in control cities, 1988-2005

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 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.

I formally test the statistical significance of these results by following the exact permu-

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.

Table 3.9: 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

tation 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. I 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%, I find that the DID estimate for the entire post-riot period is marginally significant. However, I 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.

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, I 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[12].

Using decennial census data[13], 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.

[12] 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

[13] 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.

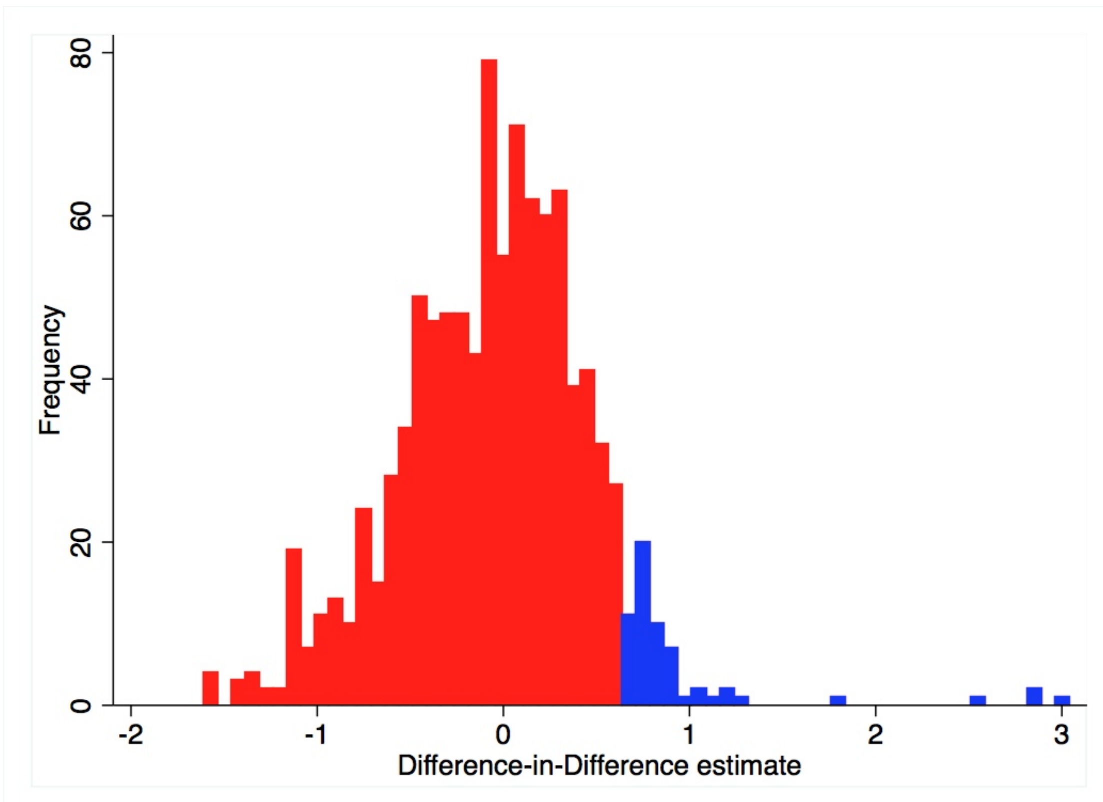
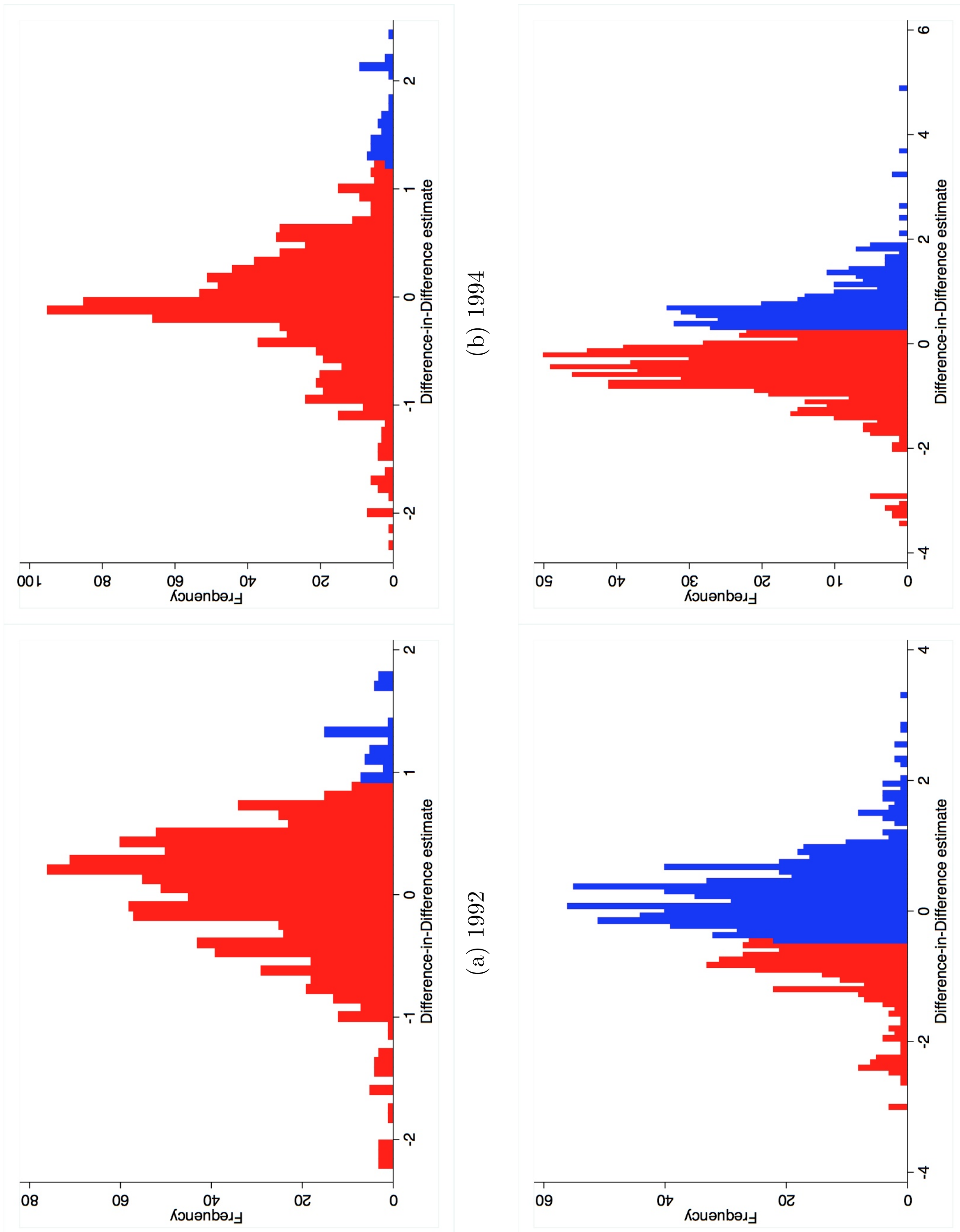


Figure 3.7: Los Angeles Synthetic Control Matching: Histogram of 1,000 placebo permutations for average post- (1992-2005) minus average pre- (1988-1991) riot period



(a) 1992

(b) 1994

(c) 2002

(d) 2003

Figure 3.8: Los Angeles Synthetic Control Matching: Histogram of 1,000 placebo permutations for selected years

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 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 whereas 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. I 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.

3.5 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,

Table 3.10: 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 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 that 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 methodological 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.

Chapter 4

County Mass Layoff Shocks and Adolescent Dropout and Graduation Rates

4.1 Introduction

State and federal policymakers have historically taken a school-centric approach to crafting educational performance accountability legislation and developing programs intended to improve academic achievement. These laws and initiatives reflect the long-standing belief that academic performance is largely governed by schooling institutions (Chenoweth 2007). However, advocates of community-focused approaches to education reform argue that schools do not operate in a vacuum, but reflect the conditions of their macro environments. More recent initiatives, such as the United States Department of Education's Promise Neighborhoods program and the Harlem Children's Zone, reflect this approach, recognizing that it takes both effective schools and strong social and community services to support the educational achievement of children. Indeed, Coleman et al. (1966), in their study on equality of educational opportunity, argue that schools alone cannot solve the problem of chronic underachievement in urban schools.

The purpose of this study is to identify the effects of community quality, which encompasses the physical, social and economic resources, such as safe neighborhoods, recreation programs for children, and local parks and playgrounds, that residents rely on for sustaining and advancing their well-being, on adolescent schooling persistence rates, which I define as high school dropout and graduation. The strong connection between the community and school makes it difficult to parse out independent effects. Many models fail to control for school characteristics thus implicitly assuming that either variation in schools do not matter or are entirely a function of the community (Arum, 2000). Studies must also contend with the problem of residential sorting, where the attributes of families may be correlated with the types of neighborhoods they choose to live in. These methodological issues are formidable obstacles in obtaining causal estimates of community effects on schooling outcomes.

In order to circumvent these problems, we need a mechanism that acts at the community level to exogenously change community conditions. By operating on the community independent of the individual, family and school settings, we can attribute any effects on schooling outcomes solely to community context. In this study, I use nationwide county level mass layoff data from 1996 to 2009 to estimate the effects of decreased community quality on dropout and graduation rates. I define the community as the county and the lever to decrease quality as the occurrence of a mass layoff. Any differences in dropout and graduation rates after the mass layoff event is evidence of a county effect.

Downey, Broh, and von Hippel (2004) use a similar strategy in their examination of school effects on adolescent cognitive skills. They compare test scores taken during summer when the school has little to no effect on the child to test scores taken during the school year. Any difference is evidence of a school effect. I employ a similar strategy for estimating community impact by using a mass layoff as a lever to decrease community quality, which is synonymous to Downey et al's use of the seasonal nature of schooling as a lever to turn the school off and on.

In order to use a mass layoff event as a lever for changing community quality, it must satisfy three conditions: 1.) The community cannot anticipate it. 2.) Its occurrence is not correlated with community level characteristics, and 3.) It has community wide effects. The first two conditions satisfy the exogeniety assumptions built into the econometric modelling. Previous studies have found that mass layoffs can be viewed as random shocks to communities after controlling for community fixed effects (Sullivan and von Wachter, 2007; Annanat et al 2011, 2012). The third condition requires that the change in a child's environment occurs directly at the community level. Mass layoff shocks have been found to affect communities beyond the unemployment of residents given the extensive ties of businesses to local economic and social structure (Greenstone, Hornbeck and Moretti 2010; Greenstone and Moretti 2004). The unique characteristics of a mass layoff event fulfill the above conditions, allowing us to use their occurrence to measure the effects of lowered community quality on schooling persistence.

My estimates indicate that a large shock occurring three years prior decreases current county level graduation rates by 1.2 percent. Additionally, low and high four-year average shocks decrease graduation rates by 4.5 and 9.9 percent, respectively, and increase dropout rates by 10.7 and 5.0 percent, respectively. These results are robust to an instrumental variables specification that further isolates exogenous variation in mass layoff occurrence. The results show that the size, timing and persistence of shocks play an important role in how diminished neighborhood conditions affect county level persistence rates. Counties are not affected by minor shocks and do not experience the effects of a large shock immediately, but only after the shock has time to filter through the various complex mechanisms that tie community conditions to adolescent schooling rates. Furthermore, it's not just one-time shocks that have an effect, but shocks, minor or large, persisting within a county over a four-year period. Education outcomes are often analyzed in relation to contemporary measures of the community, as if there is likely to be an instantaneous impact of levels of community quality on aspects of adolescent schooling success (Brooks-Gunn et al. 1993; Harding 2003). The findings from this study suggest that measuring community conditions at a single point in time may mask the effect of duration on schooling persistence rates.

The body of the article is organized as follows. The first two sections of this chapter provide the background required to understand the use of mass layoffs as an instrument for estimating community level effects, namely exogeneity across time and space and widespread effects on a community. In the following section, I describe the data and motivate the empirical analysis with descriptive statistics. I then outline my empirical strategy followed by a presentation of the results. I conclude with a discussion of the findings.

4.2 Defining Mass Layoffs

The novelty of Downey, Broh, and von Hippel's (2004) strategy in estimating school effects is the use of a mechanism, the seasonal nature of schooling, that turns the influence of a school on child learning off and on. The authors argue that the institutional reach of a school disappears once the school year concludes. Any changes in learning during the summer can be attributed to non school factors. The attractive feature of their strategy is that the change from school to no school is not a direct function of the community or family, allowing the authors to link any significant effects to the influence of the school. The community or family can affect test scores, but only indirectly through and as a consequence of the school disruption.

Although the basic concept behind their empirical strategy is attractive, it does run into several methodological issues. First, families know well in advance the start date of the new school year and may take measures at the end of summer to prepare their children for school, thus possibly understating the school effect. Second, schools may not be the only context turned off and on during and after summer. For example, community context may change because weather tends to cool down during the fall and winter, and there is evidence of a correlation between temperature and juvenile crime which, in turn, impacts schooling success (Baron and Bell 1976; Anderson and Anderson 1984; DeFronzo 1984, Harries et al. 1984; Harries and Stadler 1988; Rotton and Cohn 2000a, b, 2003, Butke and Sheridan 2011).

In examining the impact of communities, I want to adopt Downey et al's basic strategy, but ensure that it meets certain additional conditions. Barring a randomized trial, finding a similar lever to Downey et al's seasonal mechanism at the community level is difficult. We may not find a mechanism that completely turns off the community, but a lever that turns its quality up or down, all else remaining constant, would still allow us to estimate the impact of community context. An appropriate lever must meet several vital conditions[1]: 1.) it must be exogenous across time. In other words the county cannot readily prepare for the shock. 2.) It must be exogenous across space. In other words, certain counties are affected by the shock, but not others, and these two groups are similar across other dimensions. 3.)

[1] A formal mathematical exposition of these conditions is found in Appendix A

The shock must be strong and widespread enough to make an identifiable impact on the entire county.

In this analysis, I use a mass layoff occurrence as the lever that signals a decrease in county quality. The following two sections define mass layoffs and describe how they fulfill the conditions of an appropriate lever as described in Chapter 1.

Timing and Location of Mass Layoff Events

An oft studied issue in the field of Economics is the effects of unemployment on individual outcomes. The difficulty of isolating exogenous individual separation events has led the literature to use separations that encompass multiple worker layoffs to base estimates of the earnings loss associated with an involuntary change of employers. There are two key features of mass layoffs that plausibly justify this exogeneity argument: the unpredictability of the timing of mass layoff events and their disconnect from local conditions.

A mass layoff refers to any reduction in work force that, within any 30-day period, results in an employment loss at a single site of employment of a third or more of the site's employees but at least 50 employees, excluding part-time employees.

Mass layoff events typically occur over a short period of time. Under the Worker Adjustment and Retraining Notification Act, employers of more than 100 employees must notify its employees 60 days in advance of a pending permanent layoff due to a plant closing. Notification is intended to provide employees some transition time to adjust to the loss of employment and to seek and obtain alternative employment. Given notification, both workers and management have time to react strategically (Addison and Portugal, 1987; Ruhm, 1994; Kuhn, 1995; Schwerdt; 2011). Notification is also given to local and state government officials. Although two months may provide enough time for laid off individuals to prepare for unemployment, the community, which will have to bear the effects of an increased unemployment rate, the social and economic negative stigma attached to a mass layoff, and the loss of measured and unmeasured economic externalities, may require even more advanced notice since local administrative units typically act in reaction to, rather than in anticipation of, downturns in the economy (Mattoon and Testa 1992).

My identification strategy also relies on a mass layoff being exogenous across *space*. The argument behind this strategy is as follows. When a firm fires a single individual, it may reflect observable and unobservable characteristics of that individual. If the job change is voluntary, workers may leave for reasons we cannot measure. In both cases, we run into selection issues: the firm *selects* the worker to fire or the worker *selects* into a state of job displacement.

When workers are let go through a mass layoff event, their firing cannot typically be explained by their background characteristics since they make up such a significant proportion of the work force. Mass layoffs are tied to national level conditions, such as recessions or the decline in the demand for a product, or firm level factors, such as top level managerial changes and significant loses in company revenues. If mass layoffs are caused by macro-level factors, then their occurrence is not tied to just worker characteristics, but also local community conditions. Since a mass layoff event typically involves a large percentage of a company's workforce, this produces a considerable shock to the firm. Generally, companies tend to avoid such shocks, choosing to gradually trim employment or services to accommodate economic fluctuations. Therefore, a large shock is a signal that the company is likely going through extensive changes, which can be traced to firm or aggregate level conditions. In times of a recession or organizational change, the company may choose the lowest performing firm for a mass layoff, but the motivation for the layoff in the first place is due to factors beyond local conditions.

In an analysis of 42 firms, Stafford (1991) finds that the most significant reasons for specific plant closings were difficulties related to internal profitability and management. Local conditions were secondary. In a case study of a plant closing at General Motors, Rubenstein (1987) finds the reasons for plant shutdown are inability to expand property, inadequacy of structures to accommodate truck deliveries, relatively lower labor productivity, and poor labor-management relations. He concludes that location factors do not play a role.

Several studies have utilized the exogeneity of mass layoffs, plant closings, or severe economic downturns to obtain estimates of the effects of various characteristics on a number of aggregate level outcomes (Black et al (2003, 2005a, b; Catalano et al 2010; Ruhm 2000, Tapia Granados 2005). Ananat and coauthors (2011, 2012) use mass layoffs and business closure shocks to specifically estimate the effects of unemployment on academic achievement. They find that county level business closings and state level mass layoffs affect student test scores. Their analysis is similar to the one conducted in this paper, but they are interested in the direct effects of unemployment on test scores and utilize county data only in North Carolina or nationwide data measured at the state level, whereas I utilize nationwide county level data to determine the effects of reduced quality induced by an economic shock on graduation and dropout rates.

Table 1 compares counties experiencing a significant ("High") mass layoff event with counties experiencing a minor ("Low") or no ("None") mass layoff event. If mass layoffs are truly random across space, these three groups should have similar profiles. On average, low mass layoff affected counties have substantially larger populations as their no and high mass layoff affected counterparts. However, all sets of counties have similar percent non-white, percent living at or under poverty, and median income averages. While it appears low affected counties are much larger in population size than their high- and non-affected counterparts, their similarity in other demographic variables lends support to the geographic exogeneity of mass layoff events.

Table 4.1: Mean Graduation, Dropout Rates and Demographic Characteristics by Neighborhood Quality Shock for select years 1998-2008

	1998	2000	2002	2004	2006	2008
Neighborhood Quality Shock: None						
%Mass Layoffs	0.000	0.000	0.000	0.000	0.000	0.000
Graduation Rate	80.181	78.309	79.324	78.618	80.629	80.621
Dropout Rate	-	-	-	4.031	2.882	2.957
Population	25,850	22,832	13,334	21,563	21,836	24,621
%Non-white	16.398	16.436	18.064	19.408	18.633	18.555
Median Inc.	32,290.02	33,933.90	32,510.69	35,492.48	37,987.62	41,946.65
% Poverty	15.139	14.009	14.679	14.06	15.628	15.328
Neighborhood Quality Shock: Low						
%Mass Layoffs	0.081	0.078	0.103	0.084	0.071	0.079
Graduation Rate	74.303	74.407	74.781	76.367	75.892	77.521
Dropout Rate	-	-	-	3.671	3.437	3.429
Population	139,786	142,326	127,974	154,041	167,810	174,738
%Non-white	19.604	19.172	19.158	20.422	22.267	21.941
Median Inc.	32,290.02	38,843.80	37,892.68	40,553.60	43,893.27	47,075.70
% Poverty	15.139	12.362	13.063	13.374	14.616	14.597

	1998	2000	2002	2004	2006	2008
Neighborhood Quality Shock: High						
%Mass Layoffs	0.762	0.862	0.779	0.689	0.741	0.822
Graduation Rate	73.041	72.804	72.151	73.804	73.145	71.951
Dropout Rate	-	-	-	3.603	3.218	3.652
Population	76,523	37,883	71,680	56,326	33,694	49,419
%Non-white	20.426	20.195	19.748	20.358	19.256	25.086
Median Inc.	30,259.76	32,499.21	33,943.81	34,521.83	36,186.67	39,326.25
% Poverty	16.891	15.238	14.607	15.713	17.314	17.822

The graduation rate in year t is defined as the number of graduates in year t divided by the average enrollment in grade 8 at time $t-4$, grade 9 at time $t-3$ and grade 10 at time $t-2$. The dropout rate is defined as the number of dropouts in grades 9-12 over total enrollment in grades 9-12

Source: Bureau of Labor Statistics (BLS) Mass Layoffs Department, the National Center for Education Statistics, U.S. Census, and the Census Small Area Income and Poverty Estimates

The arguments offered above provide the foundation for my empirical strategy, but we can take the modelling one step further to reduce the probability that unobserved variables are driving the results. Previous research (McKinney and Vilhuber, 2006; Lengermann and Vilhuber, 2002; Abowd, McKinney and Vilhuber, 2008; Bowlus and Vilhuber, 2002) has found some evidence that a mass layoff event is related to characteristics of the workers employed at the firm. As will be discussed in the methodology section, I take advantage of the panel nature of the data by including county and time fixed effects, which eliminates selection issues based on time invariant characteristics. I also utilize an instrumental variables strategy to further narrow mass layoff occurrence and severity to their exogenous components.

Effects of Mass Layoffs

The final condition for an appropriate community lever is that it must affect the entire bundle of goods that make up the quality of a community. In other words, the event must be widespread and felt throughout the community. Just as the summer turns off the school entirely for all children in Downey et al's study, a mass layoff decreases all aspects of quality for the entire community. If a mass layoff has an effect only on a restricted subset of the community, estimates of environmental influence are severely biased downwards. The purpose of this section is to use previous research and an analysis of housing values to determine whether mass layoffs do in fact alter county quality.

There is abundant evidence showing that unemployment has deleterious effects on individuals, including their physical and psychological health (Sullivan and Till von Wachter 2009, McKee-Ryan et al. 2005; Browning et al., 2006; Kuhn et al, 2009; Salm, 2009; Schmitz, 2011; Classen and Dunn, 2012), future earnings (Jacobson, LaLonde and Sullivan 1993; Stevens 1997, Morissette et al 2007), overall life satisfaction (Blustein 2008), family welfare (Jones 1988; Conger and Elder Jr. 1994; McLoyd et al. 1994), resident mobility and propensity to engage in violent crime (Rege et al 2009). Although these results apply to only those who were laid off, we can imagine that if the displaced workforce makes up a large proportion of the resident population of a county, these effects will spillover to the rest of the community. For instance, higher propensity of crime will make a community less safe, increased out-migration causes a deterioration of social capital, and marital instability might induce increases in divorce rates in the community through social network effects.

Local labor opportunities are potentially more fundamental and causally prior to community level disadvantage than conditions that are often given greater importance in empirical work (Bellair, Roscigno and McNulty 2003). Black and colleagues (Black, McKinnish and Sanders 2003; 2005a, b) examined the effects of boom and bust cycles in the steel and coal industries in the 1970's and 1980's and found that downturns led to lower employment for other industries, underscoring the importance of cross industry spillovers within a local community. Similarly, Greenstone, Hornbeck and Moretti (2010) found that large plant openings provided increases in productivity of other plants residing within the same counties. Using the same identification strategy, Greenstone and Moretti (2004) found that housing values, which are considered in the economics literature to be strong indicators of overall community quality (Galster 2004), decreased in communities where plants were not built.

In an analysis of the impact of a local plant closing in Wisconsin, Ginsburg (1994) found that a mass layoff event had several negative community wide effects, including severe costs to tax payers, decreased tax revenues, indirect business tax losses, increased welfare costs, and non-trivial indirect job losses in neighboring businesses. Blanchflower and Oswald (1994) find that those who remain employed in areas experiencing significant job losses experienced decreased earnings. Several studies have found that an increase in regional unemployment rates is correlated with low self-reported measures of resident life satisfaction (Clark, Knabe, & Rtzel, 2010; Luechinger, Meier, & Stutzer, 2010) and high psychological distress (Dooley & Catalano, 1984; Dooley, Catalano, & Rook, 1988; Fenwick & Tausig, 1994). Carroll (2003) and Malley and Moutos (1996) find that the announcement of intended mass layoffs gauges the degree to which the larger population perceives a threat to its economic security. Mass layoff announcements induce a stress response in the population (Cobb and Kasl, 1977; Carroll, 2003), which Catalano et al. (2010) find to affect selection in-utero.

Housing sale prices are the generally recognized proxy measure for many indicators of community quality, such as crime and poverty rates, because these aspects of neighborhood are capitalized into the value of its properties. A house in a neighborhood with good quality parks, schools, recreational and retail facilities, and physical appearance will fetch a higher price than an identical home in a neighborhood without such amenities.

Using county level panel data from the American Community Survey for the years 2005 to 2010, I explore the relationship between mass layoffs and housing values. I define the variable $\%MassLayoffs_{it}$ as the number of individuals living in County i experiencing a mass layoff event divided by County i's labor force population at time t. I categorize counties according to their level of mass layoff severity at time t: the "None" category includes counties that experience no mass layoff shock, the "Low" category includes counties that experience a $\%MassLayoff$ greater than 0 but less than the 90th percentile of the distribution, and the "High" category contains counties with a $\%MassLayoff$ greater than the 90th percentile. The cutoffs were based on an examination of natural breaks in the distribution, which is highly skewed to the right. These cutoffs strike a reasonable balance between parsimony and flexibility in describing the data.

I use a first-differences regression where the dependent variable is the natural log of median housing value and the main independent variable is a measure of mass layoff severity. I also include controls for a county's median income, population, percent non-white and year fixed effects. The results of this regression are shown in Table 2. The model directly including $\%MassLayoffs$ (column 1) yields a β of -0.097, with a standard error of 0.46 ($p-value$ of 0.034), which translates into a 9.7% decrease in housing value for a one percentage point increase in mass layoff severity. Replacing the continuous variable $\%MassLayoffs$ with a trichotomous definition of shock in column 2 yields a similar although somewhat muted result: low- and high-level shocks are associated with a 1.6% and 2.7% decrease in housing values, respectively.

These results should be interpreted with a certain amount of caution since the analysis is

Table 4.2: The impact of neighborhood level shocks on county log median housing values, first-differences model, 2005-2010

	(1)	(2)
Δ%Mass Layoffs	-0.0975**	
	(0.4604)	
$\Delta LowShock_{it}$		-0.0162**
		(0.0042)
$\Delta HighShock_{it}$		-0.0277**
		(0.0073)
Δ Log Pop	0.6151**	0.5434**
	(0.0974)	(0.1060)
Δ% Non-white	-1.9963**	-1.8177**
	(0.3501)	(0.3580)
Δ Median Inc.	4.89e-06**	4.78e-06**
	(5.00e-07)	(5.02e-07)
N	3921	3924
R^2	0.324	0.326

** $p < 0.05$

Clustered standard errors are reported in parentheses

Source: U.S. Census, American Community Survey

limited to the years 2005 to 2010, a period when the United States was suffering through a severe housing bust, and to counties with relatively large populations. However, the recession was not a localized phenomenon, but had nationwide effects, and the time variant covariates and fixed effects included in the model minimize bias due to unobservable county and year characteristics. In summary, mass layoff severity and occurrence decrease median housing values at the county level. Since housing values are tied to the underlying quality of an entire community, the results also support the conclusion that mass layoff shocks are not localized and have deep, far reaching effects.

4.3 Effects on High School Dropout and Graduation Rates

In the previous section, I established that mass layoff shocks have negative effects on the overall conditions within a county. The concern in this paper is to estimate the consequences of these diminished conditions on schooling persistence rates. Community quality reflects the collection of aggregate level economic and social goods that potentially influences individual well-being. Previous literature has established that community conditions can influence youth schooling success through a variety of mechanisms, including the quality of local institutions, particularly schools, community collective norms, neighborhood social capital, labor market conditions and criminal activity (Sampson, Morenoff, and Earls 1999; Ainsworth 2001; Harding et al 2011).

To provide preliminary evidence of the effects of decreased county quality, I refer back to Table 1 to illustrate the effect on aggregate schooling persistence rates for counties impacted by mass layoffs in the years 1998, 2000, 2002, 2004, 2006, and 2008. Here I place counties into the same three categories of shock defined in the previous section. Each cell in the table represents the mean value of the given variable by year.

The distribution of mass layoff shocks is highly skewed. In any given year, the majority of counties experience no or minimal shock. On average 0.1% of the labor force population in low mass layoff counties experience a mass layoff event. In high mass layoff affected counties, this percentage is between 0.7 and 0.8. The average masks incidences of high mass layoff severity in some counties. For example, 12.5% of the labor force population in Buena Vista county in Virginia experienced a mass layoff in 2003.

I find that in each year, counties with greater shocks have on average lower graduation rates. These differences are not trivial. For example, a county with a severe shock in 2008 has a graduation rate five percentage points lower than a county with a minor shock, which amounts to an average of 60 less ninth grade students not graduating by 12th grade. Compared to counties with no shock, counties with severe shocks have an average graduation rate that is nearly 9 percentage points lower. In 2004 I find that counties with no shocks have a *higher* dropout rate compared to counties experiencing high and low shocks, however the direction reverses in 2006. I also find no difference in the dropout rate between low- and high-shock counties in all years.

Figures 1 and 2 graph the graduation and dropout rates by shock category and all years 1997 to 2010. In Figure 1, I find that in all years the greater the severity of the mass layoff, the lower the graduation rate. Similar to the dropout findings presented in Table 1, counties experiencing high and low shocks have *lower* dropout rates prior to 2006. However, the relationship changes after 2006: counties experiencing a shock find that their dropout rates are greater than those experiencing no shock. The dropout rates for high- and low-shock counties track very closely over this time period.

The preliminary evidence indicates that negative shocks to the county caused by mass layoffs yield lower graduation rates. However, I find that shocks have the opposite effect on dropout rates prior to 2006: rather than experiencing greater dropout rates, shocked counties have lower dropout rates. The relationship reverses after 2006, indicating that period effects may be driving the results shown in Figure 2.

Although we decipher some general relationships between mass layoff shocks on county quality and schooling persistence rates, looking at these graphs is not a systematic way of determining whether there is a statistically significant relationship. I turn to regression

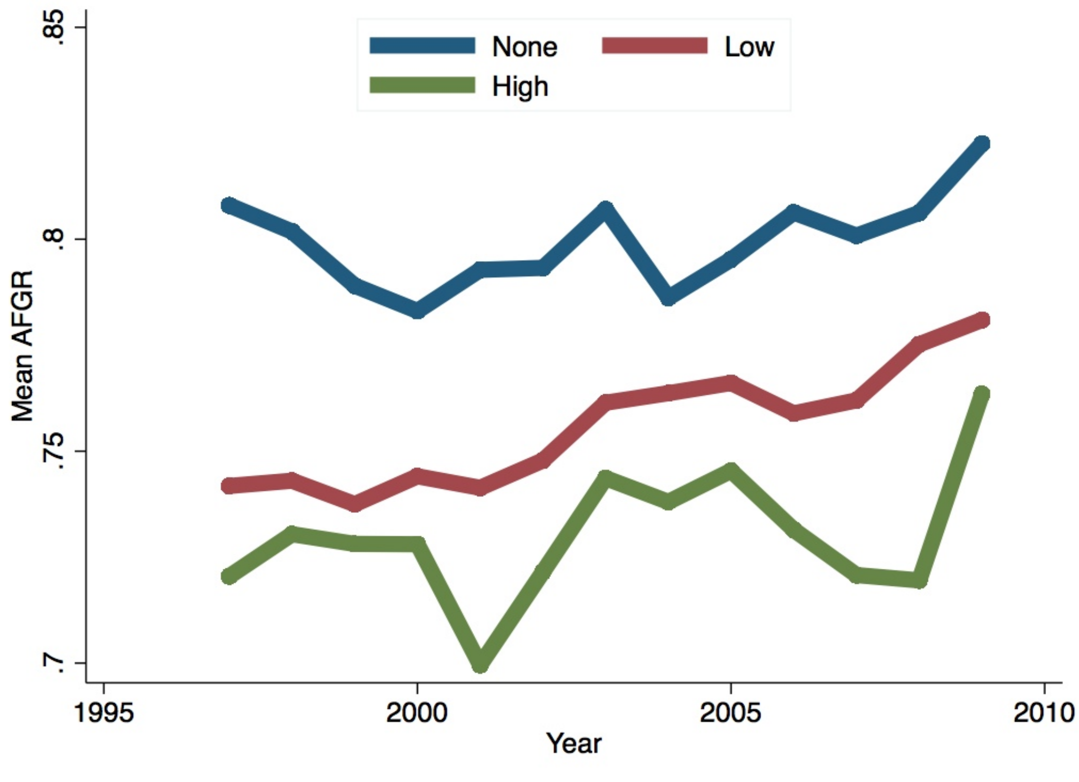


Figure 4.1: Mean Graduation Rates by Neighborhood Quality Shock and Year: 1997-2010

analysis to quantify the effects of community stress on schooling persistence.

4.4 Empirical Strategy

The goal of this study is to estimate the effect of a widespread shock on community level dropout and graduation rates. My identification strategy relies on mass layoff events inducing plausibly exogenous variation in quality across counties. In other words, the strategy relies on the unpredictability of mass layoff events across time and geography. Quasi-experimental variation relies on changes over time in the explanatory variables occurring in some locations but not in other places. Counties that experienced no change act as a quasi-control group for counties in a quasi-treatment group that experienced a change. Comparing differences in outcomes over time between the two groups provides a means to identify the effect of the change. Additionally, mass layoffs should have reasonably widespread effects on the community such that overall county quality diminishes. Therefore, instead of measuring just the impact of unemployment, I am measuring the impact of lowered county quality.

To investigate the effects of reduced county quality caused by a mass layoff shock, one needs only reduced form estimates of the determinants of schooling persistence rates at

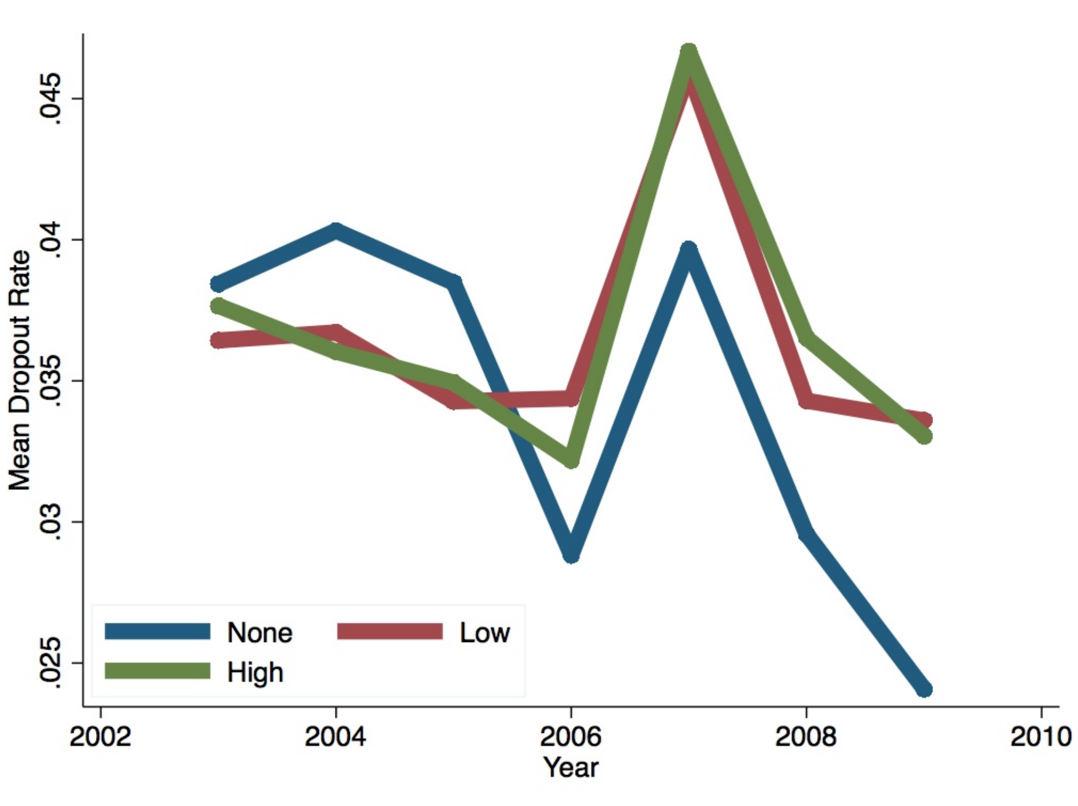


Figure 4.2: Mean Dropout Rates by Neighborhood Quality Shock and Year: 2002-2008

different points in time:

$$SP_{it} = \theta_t(NQ_{it}), \tag{4.1}$$

where SP_{it} is a measure of schooling persistence, either dropout or graduation rate, and NQ_{it} is a measure of county quality in county i during year t. The variable NQ is a function of an assortment of variables, such as local institutional resources, quality of schools, and access to welfare programs.

If we believe mass layoffs are exogenous across time and space, we obtain unbiased, consistent estimates by running a basic ordinary least squares (OLS) regression where the dependent variable is the dropout or graduation rate and the independent variable is a measure of a mass layoff shock. If we assume that θ_t is log linear:

$$ln(SP_{it}) = NQ_{it} + \mu_{it} \tag{4.2}$$

The identifying assumption is that there are no county level characteristics that correlate with the severity and occurrence of a mass layoff event *and* dropout or graduation rates. I am interested not in the direct effect of mass layoffs per se, but the impact of decreased county quality caused by the mass layoff. Here I define NQ_{it} as a non linear function of mass layoff severity:

$$NQ_{it} = \begin{cases} 0, & \text{if } \%MassLayoff_{it} = 0 \\ 1, & \text{if } \%MassLayoff_{it} \geq 0 \ \& \leq c \\ 2, & \text{if } \%MassLayoff_{it} > c, \end{cases} \tag{4.3}$$

where $\%MassLayoffs$ is defined as previously and c is some threshold separating low and high negative shocks, which I define as the value at the 90th percentile of the distribution. County-year observations with an NQ of 0 experienced no shock, a value of 1 a low shock and a value of 2 a high or severe shock. I enter NQ_{it} as a series of dummy variables indicating low ($NQ_{it} = 1$) and high ($NQ_{it} = 2$) shock with no ($NQ = 0$) shock as the baseline.

A more thorough investigation requires that we reduce the probability that any unobserved factors are driving the patterns depicted in Table 1 and Figures 1 and 2. Researchers employing mass layoffs or general unemployment shocks typically include fixed effects in their estimation models (Jacobson, LaLonde, and Sullivan 1993; Stevens 1997; Black, McKinnish and Sanders 2003 2005a,b; Ananat et al. 2011, 2012; Classen and Dunn, 2012). By using a fixed effects approach, each county acts as its own control by making comparisons within counties and then averaging those differences across all counties in the sample. In line with this previous literature, I estimate the following highly parameterized model that controls for a series of fixed effects:

$$log(SP_{it}) = \beta_1 Low_{it} + \beta_2 High_{it} + \sigma_1 log(Pop_{it}) + \psi_1 County_i + \psi_2 Year_t \tag{4.4}$$

Note that dropout and graduation data reflect school rather than calendar year, which means SP_{it} measures the percent of students dropping out (graduating) between the end of summer in year $t-1$ and the end of spring in year t. Since graduation and dropout data are typically collected during the Spring and reflect the school rather than calendar year, I define the time period for a given year as the four quarters prior to the Spring of that year. For example, the total number of mass layoffs in 1996 equals the sum of mass layoffs in the 3rd (July-Sept) and 4th (Oct-Dec) quarters of 1995 and the 1st (Jan-Mar) and 2nd (Apr-Jun) quarters of 1996. The coefficient of interest is β, which measures the percent change in schooling persistence rates due to a shock. Since a one person change has greater or lesser impact depending on the size of the population, I include the log county population Pop as a control variable[2].

The model includes the parameters $County$ and $Year$, which are county and year fixed effects. County fixed effects purge the data of anything that does not vary over time at the county level that affects both county quality and schooling persistence rates, such as regional affiliation and local labor market characteristics. In general, county fixed effects account for the possibility that counties that have higher quality on average may also have higher or lower dropout or graduation rates. By including a county fixed effect, we eliminate

[2] I use the natural log because the distribution of county population is non-normal

any concern that counties with certain fixed characteristics that are correlated with dropout and graduation rates are more or less prone to mass layoff shocks.

Year fixed effects control for potential period effects, such as recessions and federal schooling or labor legislation. These fixed effects control for events in a particular state that occur between 1997 and 2009 that caused a decrease in county quality and below average schooling persistence rates. Examples include Hurricane Katrina in Louisiana, the U.S. recession beginning in 2008 and changing state standards or education policies, which increased in variation over time after the passing of the No Child Left Behind act in 2001. In sum, for any remaining omitted variables to explain my findings, they would need to change systematically within a county over time, while covarying with changes in dropout and graduation rates.

I add several specifications to the model in equation (4) to test for alternative hypotheses. First, the identifying assumption in equation (4) is that unobservable factors that might simultaneously affect schooling persistence rates and mass layoff occurrence are time invariant. I control for time variant county characteristics to capture changes occurring in the county that may decrease quality and concurrently affect schooling rates. I control for county median income and percent non-white as these characteristics have both been relatively volatile over the study's time period (see Table 1) and have been found by previous literature to affect both aggregate schooling rates and neighborhood quality (Johnson 2011).

Second, I include one-, two- and three-year lags of NQ, NQ_{it-1}, NQ_{it-2} and NQ_{it-3}[3], to capture the lagged effects of a shock on persistence rates. I use three years of lagged shocks to capture the typical length of time a student is enrolled in high school, with NQ_{it-3} occurring during ninth grade, NQ_{it-2} during tenth grade, NQ_{it-1} during eleventh grade and NQ_{it} during twelfth grade. We can hypothesize the various ways a lag variable may affect future rates. For example, a mass layoff event may take time to affect a community and thus its impact would be measured several time periods post shock. Therefore, we would not find a significant effect on the coefficient NQ_{it}, but on NQ_{it-s}.

Estimates based on point-in-time measurements of county context may substantially understate the effect of sustained disadvantage. While the lags separate shocks by single years, I test a variable that measures the persistence of shocks over an extended period. I define this variable as the average percent affected by a mass layoff in a county over an s-year period

$$\frac{\%MassLayoffs_{it-1} + ... + \%MassLayoffs_{it-s}}{s} \tag{4.5}$$

where $\%MassLayoffs_{it}$ is the percent of individuals losing employment due to a mass layoff event in County i at year t and s is the time lag, which I specify as four years to represent the number of years (9-12 grades) typically spent in high school. Similar to the yearly shock models, I define a variable NQ^p_{it} that assigns a value of 2 to counties with a high four-year *average* shock, a value of 1 to counties with a low four-year average shock and a value of

[3] Lags beyond three years do not alter the results

0 to counties with no shock. The low-shock category includes all counties below the 90th percentile in the four-year average distribution but above 0, and the high-shock category includes all counties above the 90th percentile. We can interpret shock persistence as a measure of the average conditions of a county over 9th through 12th grades for a given high school cohort. I am not investigating the effects of lifetime community disadvantage, but the effects of shocked county conditions during high school.

While separate lags measure individual prior yearly shocks, equation (5) measures shock persistence. While a lag determines whether conditions s years ago impact a county's rates at current year t regardless of the conditions in the county between s and t, a measure of persistence determines whether the average conditions between s and t influence rates at year t. Previous research measures community context only once and does not account for the length of exposure to neighborhood disadvantage. Recent work (Crowder and South 2011; Jackson and Mare 2007; Sampson et al. 2008; Sharkey and Elwert 2011) has emphasized the need to account for the temporal dimensions of neighborhood effects. Wodtke, Harding, and Elwert (2011) find that a longer duration of exposure to disadvantaged neighborhoods reduces the probability of graduation. A county may be able to withstand a one-year shock, but a prolonged state of disadvantage may prove to be too detrimental to the long-term educational health of its residents. By focusing on the effect of long-term changes in county quality and purging out much of the variation that reflects more transitory fluctuations in general conditions, the estimates from the four-year average model should have a larger effect on schooling persistence rates.

Because we observe graduation and dropout rates in each county every year, standard errors must be corrected for the possibility of a common random effect occurring in a county at some given time period (Moulton, 1990). Therefore, I correct the standard errors to account for clustering at the county level.

4.5 Data

County level mass layoffs data for the years 1995 to 2010 are from the Bureau of Labor Statistics (BLS) Mass Layoffs Department, which report the number of workers in a year who are affected by a mass layoff event [4]. Thirty days after a mass layoff event has triggered, the employer is contacted to determine the duration of the layoff and the number of workers involved. If the layoff has been at least 31 days in duration and at least 50 workers involved, other information pertaining to the layoff is obtained, including reason for the separation.

Data at a more refined geographic level, such as the census tract, are not available. However, the county level is appropriate since it is large enough to encompass the social and economic macro effects of a large unemployment shock, since they adequately capture the geographic boundaries of labor market areas, but small enough not to dilute the effects (Bellair, Roscigno and McNulty 2003). Moreover, individuals and families are much less likely to move across county boundaries than would be the case for smaller spatial units,

[4] BLS does not collect data on layoffs of less than 50 employees

minimizing the possibility that compositional changes within the population are driving the results. Additionally, when more refined spatial information is not available, previous literature (Black et al. 2002; Ananat et al. 2012) studying macro-level effects of unemployment shocks use county level information. Due to the Bureau of Labor Statistics' (BLS) policy that protects the confidentiality interests of respondents, county-year observations with less than ten claimants are denoted by a "<10." For these observations, I imputed a value of 5 as their mass layoff count.

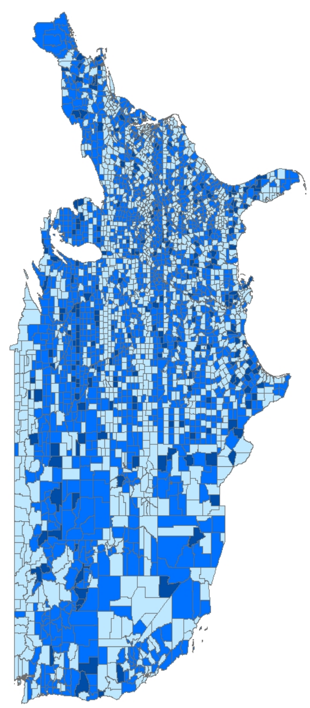
There are two characteristics of the data that possibly affect the results. First, the BLS reports the total number of initial claimants, which are the workers who file unemployment claims after a mass layoff event. This measure excludes workers affected by a mass layoff but not filing unemployment. In order to obtain this information, the BLS contacts businesses directly to obtain the number of workers who lost jobs in the layoff event. Although a total separation count is preferable, it contains considerable measurement error (Ananat et al. 2012).

Second, the claims are residency-based rather than workplace-based. If County i contains 50 laid off workers employed in a firm located in County j, the shock at that county comes from two sources: the unemployment of those 50 residents and the macro social and economic spillover effects of the shock on the firm in County j. Compare this to the scenario if the business was located in County i: the shock comes from the unemployment and the *direct* macro economic and social effects on the county. Given these limitations to the data, results presented in this paper should be considered conservative.

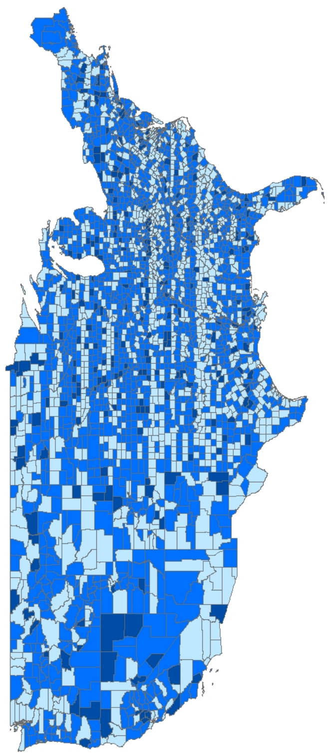
Figure 3 maps the distribution of none, low- and high-shock counties for the years 1998, 2000, 2004, and 2008. Any clustering of shock counties within a year opens up the possibility of bias due to spatial correlation, which arises because location proximity is typically accompanied by value similarity. In the case of mass layoff events, spillover effects may cause clustering. Although small clusters appear from year to year, there is no clear visual evidence of major clustering of shocks in certain regions of the country.

The visualized patterns in the maps can be reduced to a summary statistic that indicates the extent and direction of spatial correlation. The Moran's I (Moran, 1950) offers a global measure of spatial pattern. It measures the clustering of mass layoff events in space and assesses the significance of the cluster. I compute the statistic using first-order contiguity between counties, that is, units having a common boundary are considered neighbors. The statistic itself can be considered the spatial counterpart to the nonspatial Pearson's correlation coefficient. The Moran's I is generally small for all years of data, ranging from a low of 0.01 to a high of 0.08. The only years indicating a statistically significant Moran's I was 1998 and 2008, with correlations of 0.065 (pvalue = 0) and 0.084 (pvalue = 0), respectively. Overall, the maps and the formal tests indicate no or minor spatial clustering.

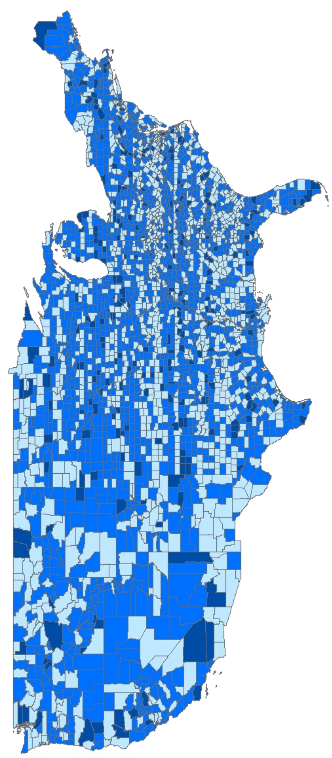
The maps also provide a visual test of the exogeneity across space assumption required to use mass layoff shocks as a lever for assessing the effects of county quality. If mass layoff events are random, then we should not find the same counties experiencing a shock every year. The maps show variation in shock occurrence across space from year to year. Of the approximately 3,000 counties in the sample, 12% experience a shock in all 13 years of



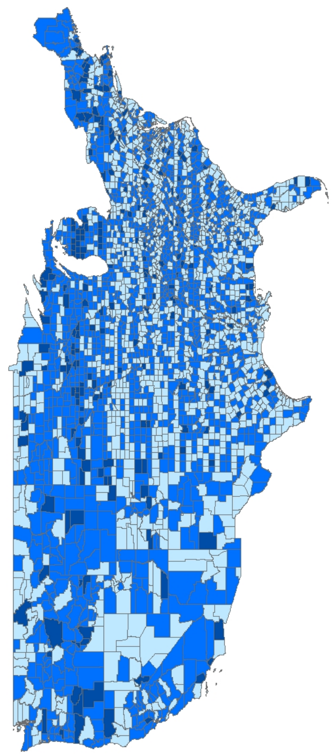
(a) 1998 (Moran's I = 0.065 pvalue = 0.00)



(b) 2000 (Moran's I = 0.012, pvalue = 0.22)



(c) 2004 (Moran's I = 0.02, pvalue = 0.051)



(d) 2008 (Moran's I = 0.08, pvalue = 0.00)

Figure 4.3: None (Light Blue), Low (Blue) and High (Dark Blue) Mass Layoff Shock Counties

available data while 32% of the counties experience a shock in 10 or more years. Only 6% experience no shock in any year. On average, a county experienced a shock in 8 years between 1997 and 2009. The probability of a county changing shock status (shock to no shock or vice versa) is 33%. In other words, in any given year, 33% of counties are expected to be in a different shock status compared to the previous year.

Data on county population counts and percent non-white are from the Census Population Estimates Program for the years 1997 to 2009. Data on county median income levels are from the Census Small Area Income and Poverty Estimates (SAIPE) for the years 1997 to 2009. The SAIPE did not collect county level data prior to 1997.

Data on dropout and graduation rates are from the State Nonfiscal Survey, which is collected by the National Center for Education Statistics (NCES) Common Core of Data (CCD) . The NCES collects data from states after a school year. These data are routinely collected at the district level. The roll-up aggregation to the county level is based entirely on the location of the school district and does not take into account school district boundaries. This aggregation strategy works well in much of the country as school districts and counties are one and the same, but falls apart in some states, especially in New England, where boundaries are determined by city and township boundaries [5]. Both measures are reported by the NCES in their annual Digest of Education Statistics.

Data on dropout rates are available from 2003 to 2008. Dropout rates reflect the percentage of public school students who were enrolled in grades 9-12 at some point during the previous school year but are not enrolled in school in October of the current school year and has not earned a high school diploma or completed a state- or district-approved education program. The measure provides information about the rate at which U.S. high school students are leaving school in a given year without a successful outcome. An event dropout rate measures a county school system's holding power and a young person's ability to successfully navigate the school system.

There are several issues with the dropout data that are worth highlighting. First, the definition of a dropout has changed over time and varies by state. Determining whether a student leaves school is deceptively straightforward, considering that transfers, reenrolled students, and out-of-state or private school enrollees complicate identification. Second, a significant number of states are missing dropout data for the time period covered by the mass layoff data. Reasons for missing data include non compliance to NCES dropout definitions and the suppression of data due to small sample sizes. Given non-trivial percentages of states not reporting dropout data in years prior to 2003, I limit the dropout analysis to the years 2003 to 2008, while the AFGR analysis covers a much wider time span (1997 to 2010).

The NCES measure for graduation rate is the average freshman graduation rate (AFGR), which provides an estimate of the percentage of public high school students who graduate on time-that is, four years after starting 9th grade-with a regular diploma. The rate uses aggregate student enrollment data to estimate the size of an incoming freshman class and

[5] The exclusion of the New England states from the analytic sample does not significantly alter the main findings

aggregate counts of the number of diplomas awarded four years later. The incoming freshman class size is estimated by summing the enrollment in 8th grade for 1 year, 9th grade for the next year, and 10th grade for the year after and then dividing by three. The averaging is intended to account for higher grade retention rates in the 9th grade. Graduates include only diploma recipients [6]. There are differences in what a high school diploma represents across states. The NCES defines a regular diploma as the high school completion credential awarded to students who meet or exceed coursework and performance standards set by the state or other approving authority. However, some states award regular diplomas to all students who meet completion requirements, regardless of the extent to which these requirements address academic standards. The AFGR is available for the years 1996 through 2009.

The key difference between the dropout and graduation rates is the time frame in which a student fails to persist in school. The graduation rate captures the attrition of a student cohort up to four years in high school while the dropout rate is a one-year non-persistence measure. The AFGR attempts to smooth out errors in data collection and dropout identification by measuring persistence over the course of a student's typical tenure in high school. If a student drops out, but returns later on to graduate in four years, he will count as a success under AFGR, but a failure in the dropout rate for the year the student drops out. Hence, a one-year shock to county quality may have an impact on a student's short-term schooling persistence, but no impact on his ability to eventually graduate. However, a student may not be impacted by a single shock, but by several shocks over time. In that case, we may find no significant effect on yearly dropout rates, but on the long-term ability of a student to earn his diploma.

Both measures are derived from data with several problems. The measures do not include information about individuals outside the public school system. Therefore, results will be biased if the private school population is affected by a shock differently. Some states collect data from nontraditional agencies, such as Departments of Corrections and alternative completion programs. Although the NCES requires state agencies to confirm compliance to CCD definitions, it does not formally audit state or school district dropout data. Many states have gone through significant changes in their data systems, especially since the passing of NCLB in 2001, making comparisons across time difficult. Although the NCES has minimized these problems with data adjustments and imputations, results from this study's analysis must be interpreted cautiously.

There are a total of 3,148 counties in the United States. Of these counties, 35 are missing data on the AFGR or one of the covariates for all years 1997-2009, yielding a total of 39,361 country-year observations. The sample size reduces to 33,269 observations after including lagged variables. Similarly, 41 counties are missing dropout data or one of the covariates, yielding a final sample of 3,107 counties and 19,286 country-year observations [7].

[6] The measure excludes other high school completers, such as those who earn high school equivalency credentials such as the GED

[7] With lagged variables, the sample size reduces to only 19,265 since mass layoff data are available two years prior to 2002. A greater decrease occurs in the lagged AFGR analysis since mass layoff data are not available prior to 1996

4.6 Results

I report estimates of the effect of decreased county quality caused by mass layoff shocks on log graduation rates in Table 3. Column 1 of the table reports the results of a model assuming the exogeneity of mass layoff occurrence without fixed effects and any additional covariates. I find that a low negative shock on county quality decreases the graduation rate by 5.2 percent. The effect of a high negative shock is much larger (8.7 percent). These effects are statistically significant.

The second column includes time-variant covariates and county and year fixed effects. The total population and median income variables yield positive, statistically significant effects (the greater the county population and median income, the higher the graduation rate). However, I find that not only does the absolute effect sizes on the shock variables decrease substantially, the signs reverse and the statistically significant effects disappear. The third column reports results including lags on county shock for the past three years. These multiple lags capture the historical yearly effects of county mass layoff shocks on graduation rates. I find no effect on the one- and three-year lag variables, but a statistically significant negative effect on the two-year lag variable. Notice that the significant effect is only for the large-shock coefficient and not the minor-shock variable. The results indicate that a shock will have an effect on graduation rates if it is exceptionally large. Furthermore, the effect doesn't affect current rates, but rates two years after the shock's occurrence. In other words, a large shock during a student's tenth grade year affects his probability of graduating by the end of his twelfth grade year.

Column 4 presents the results using the shock indicator on the four-year average of mass layoff severity. We may expect *a priori* a significant effect on this variable since the AFGR measures long-term persistence, which is more susceptible to accumulated community disadvantage rather than short-term shocks. I find a statistically significant negative effect on the persistent shock coefficients. Experiencing a low average negative shock on county quality over the past four years decreases the graduation rate by 4.5 percent. Experiencing a high average negative shock decreases graduation rates by 10 percent. These results indicate that a student's chances of graduating on time reduces if its county experiences diminished conditions over the four years he is enrolled in high school. The effect sizes on the four-year average variable are much larger compared to those on the individual yearly shock variables. We expect this since the average captures the long-term cumulative effects of community conditions while purging out much of the variation that reflects more transitory fluctuations.

Table 4 presents results using the log dropout rate as the dependent variable. Column 1 shows that counties experiencing low and high shocks witnessed a 54% and 49% increase in the dropout rate relative to no-shock counties. In column 2, the statistical significance of the shock indicators disappears and their signs change after including time varying covariates and county and year fixed effects. Column 3 shows that one, two and three year lags have no effects on current dropout rates. Finally, similar to graduation rates, column 4 shows that longer term exposure to diminished conditions increases the dropout rate by 10% for counties experiencing a low four-year average shock and 5% for counties affected by a high

Table 4.3: The impact of neighborhood level shocks on county log average freshman graduation rate, non-linear shock, 1996-2009: OLS Results

	(1)	(2)	(3)	(4)
$LowShock_{it}$	-0.0521**	0.0012	0.0014	
	(0.0042)	(0.0024)	(0.003)	
$HighShock_{it}$	-0.0866**	0.0067	0.0067	
	(0.0054)	(0.0035)	(0.0041)	
Log Population		0.2054**	0.2339**	0.2342**
		(0.0219)	(0.0281)	(0.0281)
Percentage Non-white		-0.0914	-0.2284**	-0.2276**
		(0.0566)	(0.1035)	(0.1036)
Median Income		2.95e-06**	3.98e-06**	3.99e-06**
		(5.46e-07)	(6.83e-07)	(6.86e-07)
$LowShock_{it-1}$			0.0230	
			(0.0032)	
$HighShock_{it-1}$			0.0230	
			(0.0063)	
$LowShock_{it-2}$			-0.0610	
			(0.0030)	
$HighShock_{it-2}$			-0.1180**	
			(0.0036)	
$LowShock_{it-3}$			0.0190	
			(0.0029)	
$HighShock_{it-3}$			0.0520	
			(0.0034)	
Four-year Avg. Low Shock				-0.0451**
				(0.0044)
Four-year Avg. High Shock				-0.0990**
				(0.0053)
County Fixed Effect	-	X	X	X
Year Fixed Effect	-	X	X	X
N	39,361	39,361	33,269	33,269
R^2	0.018	0.012	0.005	0.005

** $p < 0.05$

Clustered standard errors are reported in parentheses four-year average shock.

Rather than using a categorical representation of shock, I estimate the effect of $\%MassLayoffs$ directly (i.e. $NQ = \%MassLayoffs$). I present these results in Table 5. The first three columns present results using the AFGR as the dependent variable. Column 1 shows results from a model with covariates and county and yearly fixed effects, column 2 includes three years of lags and column 3 shows results presenting the four-year average of $\%MassLayoffs$. The last three columns replaces the AFGR with the dropout rate as the dependent variable. The findings from these set of analyses mimic the results found in Tables 3 and 4. Current and one- and three-year lags have no effects on graduation rates, but a two-year lag has a negative effect, which is driven by counties experiencing large shocks. Current and yearly lags have no effects on dropout rates. The four-year average of $\%MassLayoffs$ has a significant negative effect on graduation rates (a 1% increase in the proportion of the labor force affected by a mass layoff decreases the AFGR by 1%) and a significant positive effect on dropout rates (a 1% increase in the proportion of the labor force affected by a mass layoff increases the dropout rate by 7%).

In summary, the regular OLS models show that shocks to county quality negatively affect the educational health of counties by decreasing graduation rates and increasing dropout rates. However, specifying a highly parameterized model by including time variant covariates and county and year fixed effects yields non statistically significant effects. Modelling the timing of the shocks by including yearly lags and a four-year average variable yields more interesting results. While current shocks do not affect current rates, a large shock two years ago affects the probability of a student graduating by the end of his 4th year in high school. I find no lag effects on dropout rates. Exposure to a sustained shock over a longer period of time appears to negatively affect graduation and increase dropout rates.

Instrumental variable regression

In Tables 3 through 5, the inclusion of fixed effects and several time variant county-specific variables should mitigate concerns that unobserved factors correlated with mass layoff occurrence and severity have an independent influence on graduation and dropout rate fluctuations. However, we can further isolate the exogenous component of mass layoff severity by specifying more narrowly the definition of a mass layoff event.

The BLS conducts employer interviews to collect information on layoff events[8]. In addition to information on the total number of affected workers, the status of the worksite, and recall expectations, the BLS asks for the economic reason for the layoff. Twenty-five reasons for separation are categorized under the following seven broad categories[9]: Business demand, Disaster/safety, financial, organizational, production, seasonal, and other. The key assumption underlying my empirical strategy is that mass layoff occurrence and severity are not tied to local conditions. For example, we want to exclude data from a company that lays

[8]Employer participation is voluntary. The non response rate is typically around 5%

[9]The twenty five reasons by category are listed in the Appendix C

Table 4.4: The impact of neighborhood level shocks on county log dropout rate, non-linear shock, 2003-2008: OLS results

	(1)	(2)	(3)	(4)
$LowShock_{it}$	0.5444**	-0.0311	-0.0353	
	(0.0392)	(0.0187)	(0.0197)	
$HighShock_{it}$	0.4872**	-0.0493	-0.0546	
	(0.0449)	(0.0296)	(0.0308)	
Log Population		-0.7111**	-0.7829**	-0.8278**
		(0.3231)	(0.3733)	(0.3729)
Percentage Non-white		0.4578	0.6061	0.6719
		(1.258)	(1.327)	(1.3144)
Median Income		4.59e-06	-4.66e-06	5.17e-06
		(5.21e-06)	(5.26e-06)	(5.24e-06)
$LowShock_{it-1}$			-0.0151	
			(0.0197)	
$HighShock_{it-1}$			-0.0045	
			(0.0297)	
$LowShock_{it-2}$			0.0072	
			(0.0205)	
$HighShock_{it-2}$			0.0105	
			(0.0308)	
$LowShock_{it-3}$			-0.0290	
			(0.0207)	
$HighShock_{it-3}$			-0.0560	
			(0.0326)	
Four-year Avg. Low Shock				0.1075**
				(0.0691)
Four-year Avg. High Shock				0.0502**
				(0.0769)
County Fixed Effect	-	X	X	X
Year Fixed Effect	-	X	X	X
N	19,286	19,286	19,265	19,265
R^2	0.033	0.095	0.093	0.089

** $p < 0.05$

Clustered standard errors are reported in parentheses

Source: Author's calculations from the Bureau of Labor Statistics (BLS) Mass Layoffs Department, the National Center for Education Statistics, U.S. Census, and the Census Small Area Income and Poverty Estimates

Table 4.5: The impact of neighborhood level shocks on county log graduation and dropout rate, linear shock, 1996-2009: OLS results

	Graduation Rate			Dropout Rate		
$\%MassLayoffs_{it}$	0.0007	0.0006		-0.008	-0.0092	
	(0.0004)	(0.0006)		(0.0041)	(0.0043)	
Log Population	0.2054**	0.2344**	0.2347	-0.7059**	-0.7784**	-0.7993
	(0.0218)	(0.0281)	(0.0280)	(0.3228)	(0.3725)	(0.3742)
Percentage Non-white	-0.0913	-0.2286**	-0.2284**	0.4705	0.6201	0.6055
	(0.0565)	(0.1032)	(0.1035)	(1.2583)	(1.3211)	(1.3208)
Median Income	2.96e-06**	3.97e-06**	3.99e-06**	4.44e-06	4.31e-06	4.72e-06
	(5.46e-07)	(6.86e-07)	(6.83e-07)	(5.20e-06)	(5.25e-06)	(5.26e-06)
$\%MassLayoffs_{it-1}$		0.0006			-0.0037	
		(0.0008)			(0.0039)	
$\%MassLayoffs_{it-2}$		-0.0015**			-0.0004	
		(0.0006)			(0.0041)	
$\%MassLayoffs_{it-3}$		0.0007			-0.0089	
		(0.0005)			(0.0044)	
Four-year Avg			-1.0346**			7.3821**
$\%MassLayoffs_{it}$			(0.5318)			(5.6839)
County Fixed Effect	X	X	X	X	X	X
Year Fixed Effect	X	X	X	X	X	X
N	39,361	33,269	33,269	19,286	19,265	19,265
R^2	0.011	0.005	0.005	0.094	0.092	0.092

** $p < 0.05$

Clustered standard errors are reported in parentheses

Source: Author's calculations from the Bureau of Labor Statistics (BLS) Mass Layoffs Department, the National Center for Education Statistics, U.S. Census, and the Census Small Area Income and Poverty Estimates

off employees from a specific site due to unfavorable county economic conditions or because workers from that site are generally less productive. In these examples, the economic profile of the county or the characteristics of the workers from that specific site are likely correlated with county level graduation and dropout rates. We want to only include data from companies that lay off employees because of macro conditions, such as statewide or nationwide recessions or within company reorganizational decisions.

The analysis presented in the previous section includes all mass layoffs regardless of reason. Of the seven broad reasons for a mass layoff event, two are directly tied to macro-level factors: business demand, which is tied to macro economic and consumer market conditions, and organizational reasons, which are tied to company organizational changes largely independent of local conditions. Business demand reasons are associated with a decreasing or an unfavorable level-of-demand for a company's products/services that can be attributed to conventional economic factors and cycles such as changes in consumer preferences, increased domestic and import competition, and concluded contractual agreements, all of which are typically linked to macro-level factors. Organizational reasons refer to significant changes in the company's organization, its corporate structure or ownership.

The other five categories are more likely to include reasons that are associated with local characteristics. Disaster and safety reasons are potentially connected to local weather conditions. Financial reasons are associated with a company's attempts to cope with a constrained financial situation, which is tied to the cost control and cutting of unproductive plants. Production reasons are associated with factors that affect a company's ability to manufacture a given product, which may be tied to local conditions given local labor governance, local costs of materials, or declining productivity. Seasonal reasons are associated with a decrease in demand for a company's products that can be attributed to natural cycles or to cultural/societal practices that are seasonally determined, all of which can be linked to local natural, economic, and social conditions.

I further capture the exogenous portion in the variation of mass layoff occurrence by taking into account the metropolitan-wide labor market conditions of a county. The metropolitan area unemployment rate is a proxy for macro-level economic conditions that impact mass layoff severity within a county. Given that metropolitan areas contain many counties, the economic downturn of one single county would not explain the metropolitan unemployment rate. Instead, a high unemployment rate is likely driven by the economic decline of several counties within the region, signaling poor conditions beyond local county structure. Everything else being equal, the greater the metropolitan unemployment rate, the more likely the economic conditions within a county have more to do with macro-level factors than with locally driven reasons.

Several studies have employed aggregate variables as instruments to estimate the effects of community conditions on resident outcomes. Evans, Oates, and Schwab (1992) use metropolitan area unemployment, college completion, poverty rate, and median income as instruments to measure the effects of school level disadvantage on dropout rate and teen fertility rates. Foster and McLanahan (1996) employ a similar strategy to determine the effects of census tract level dropout rates on an individual's chance of finishing high school.

Rivkin (2001) uses metropolitan area and county level instruments to estimate the effects of peer group characteristics on student schooling success. These authors argue that aggregation reduces problems introduced by the conflation between school and neighborhood choice, because school and residential location decisions tend to occur within metropolitan areas [10].

Aggregate level instruments do have shortcomings, including the possibility of metropolitan area characteristics influencing schooling outcomes within a county and the modest correlation between aggregate level variables and their corresponding lower level variables, which raises the specter of a weak instrument (Murray, 2006). The extent to which county level dropout rates correlate with metropolitan area economic conditions weakens the use of aggregate level conditions as instruments. Rivkin (2001) argues that aggregate level instruments exacerbate endogeneity. However, there is no reason why this must be so and his results are equally consistent with the interpretation that community effects matter.

Since mass layoff reasons are available only at the state level, I use an instrumental variables approach to further isolate plausibly exogenous variation in mass layoff occurrence to measure the effects of decreased county quality on graduation and dropout rates. The instrument is defined as follows:

$$I_{imst} = (ML_{st}^{bd} + ML_{st}^{org}) * Unemp_{mst}, \tag{4.6}$$

where ML_{st}^{bd} and ML_{st}^{org} are the number of mass layoffs in state s at time t that are due to business demand and organizational reasons, respectively, and $Unemp_{mst}$ is the unemployment rate in the metropolitan area m that county i resides in. The instrument is the number of state level mass layoffs due to business demand and organizational reasons interacted with the metropolitan unemployment rate. The number of mass layoffs due to business and organization reasons were obtained from the BLS and the metropolitan unemployment rates were calculated from the BLS Local Area Unemployment Statistics. The instrument provides a measure of each county's susceptibility to mass layoffs linked to macro-level factors. The mass layoff events occurring in a county residing in a metropolitan area with a high unemployment rate and in a state experiencing a large number of mass layoffs caused by business demand and organizational reasons are more likely tied to macro-level conditions.

I use a two-stage least squares approach to estimate the IV model. Instead of using the trichotomous definition of NQ as defined in equation (3), I collapse the no- and low-shock categories and use a dichotomous version. I do this because the results in the previous section largely show no difference between the two categories and to run a first stage regression specified as a linear model[11] The independent variables in the first stage are the instrument defined in equation (6), county and year fixed effects, and time varying county level characteristics.

[10]See Card and Krueger (1996) for a discussion of the advantages of aggregate data in the estimation of school effects.

[11]IV estimates using a multinomial specification are unstable. In a comparison of various specifications of the first stage, including linear and probit, Rassen and co-authors (2008) find few substantive differences in the results produced by the various models. Using a linear regression for the first stage estimates generates consistent second stage estimates, while using a non-linear first stage does not (Angrist and Krueger 2001).

$$NQ_{imst} = \theta I_{imst} + \sigma_1 log(Pop_{it}) + \Delta\% Nonwhite + \sigma_3 \text{Median Inc} + \psi_1 County_i + \psi_2 Year_t \quad (4.7)$$

The second stage regresses the log graduation or dropout rate on the predicted values of NQ from the first stage regression, time varying covariates, and county and year fixed effects with standard errors on the coefficient estimates adjusted for the two stage procedure[12].

$$log(SP_{it}) = \beta \widehat{NQ}_{it} + \sigma_1 log(Pop_{it}) + \Delta\% Nonwhite + \sigma_3 \text{Median Inc} + \psi_1 County_i + \psi_2 Year_t \quad (4.8)$$

Table 6 presents the results from the instrumental variable analysis using the log graduation rate as the outcome variable. The second panel of the table presents the partial F statistics on the instrument and their p-values calculated by estimating the first-stage regression specified in equation (7). The statistically significant F values reveal that the chosen instrument is a strong predictor of mass layoff occurrence. Column 1 shows reduced form results for a model containing time varying covariates and county and year fixed effects. I find that the shock variable is positive but not statistically significant. Column 2 includes one-, two- and three-year lag shock variables. I instrument each of these lag variables with the one-, two- and three-year lags of the instrument. I find that the one- and three-year lags are not statistically significant, the current year shock remains not statistically significant, but the two-year lag is negative and significant. Counties experiencing a large shock experience a decrease in their graduation rates of 18% compared to counties experiencing no or minor shocks. The effect is slightly larger compared to the OLS estimate (11%), which likely reflects the pooling of the no- and minor-shock conditions into one category. Another possible explanation for the difference is that the IV captures the portion of mass layoffs with a greater effect since it is external to the community. Mass layoffs endemic to the community have a greater likelihood of being absorbed or resolved internally, muting the effect.

Column 3 presents the IV results for the four-year average shock variable. I find that the negative statistically significant effect from the OLS model persists. Graduation rates decrease by 17% in counties experiencing a shock sustained over a four-year period. The IV estimate is larger than the OLS effect (9%), once again likely reflecting the pooling of the no- and minor-shock conditions into one category.

Table 7 presents IV results using the log dropout rate as the dependent variable. As in the graduation rate models, the statistically significant first stage F statistics indicate that the instrument strongly predicts mass layoff occurrence. In column 1, I find a non-statistically significant effect on the shock variable. The models shown in columns 2 and 3 include yearly lags and a measure of persistent shock, respectively. Using an IV approach for these models

[12] That is, calculate the standard errors using the actual data on mass layoffs, not the predicted data

Table 4.6: The impact of neighborhood level shocks on county log average freshman graduation rate, 1996-2009: IV Results

	(1)	(2)	(3)
$Shock_{it}$	0.0901	0.0307	
	(0.0587)	(0.0612)	
Log Population	0.1844**	0.2242**	0.2233
	(0.0238)	(0.0363)	(0.0359)
Percentage Non-white	-0.069	-0.2891	-0.2478**
	(0.049)	(0.0363)	(0.0722)
Median Income	2.87e-06**	3.05e-06	3.03e-06**
	(5.33e-07)	(-6.90e-07)	(6.15e-07)
$Shock_{i(t-1)}$		0.0826	
		(0.0603)	
$Shock_{i(t-2)}$		-0.1842**	
		(0.1067)	
$Shock_{i(t-3)}$		0.2073	
		(0.1525)	
Four-year Avg. Shock			-0.1757**
			(0.1297)
First stage regression (partial F-stat)			
$Instrument_{it}$	33.44	13..8	
	(0.0000)	(0.0000)	
$Instrument_{it-1}$		9.12	
		(0.0000)	
$Instrument_{it-2}$		3.52	
		(0.0000)	
$Instrument_{it-3}$		2.8	
		(0.0246)	
Four-Year Avg. Instrument			6.94
			(0.0085))
County Fixed Effects	X	X	X
Year Fixed Effects	X	X	X
N	22,355	18,870	18,870

** $p < 0.05$
Clustered standard errors are reported in parentheses
Models include county and year fixed effects. Instrument is the number of a state's mass layoffs that are due to business demand and organizational reasons interacted with the metropolitan area unemployment rate. For models with lags, two lags of instrument are used. For first stage regressions, partial F-tests on the first stage instruments are reported with p-values in parentheses.
Source: Author's calculations from the Bureau of Labor Statistics (BLS) Mass Layoffs Department, the National Center for Education Statistics, U.S. Census, and the Census Small Area Income and Poverty Estimates

does not change the overall findings from the OLS models in the previous section, namely that a lagged decrease in county quality from a mass layoff shock does not affect county log dropout rates, but a large average four-year average does.

Tables 8 and 9 show IV results using $\%MassLayoffs$ as the measure of shock and the log graduation rate and dropout rate as the dependent variables, respectively. In both tables, the first stage F statistics are statistically significant, indicating a highly predictive instrument. The second stage regression results mimic the findings from the OLS models. The two-year lag and four-year average $\%MassLayoffs$ negatively impact the graduation rate. The coefficient on the two-year lag doubles from 0.0015 to 0.0032, while the coefficient on the four-year average slightly increased (-1.03 in the OLS compares to -1.32 in the IV). Table 9 shows that the larger the four-year average $\%MassLayoffs$, the greater the dropout rate. The IV coefficient size on the four-year average (7.1980) is similar to the OLS coefficient size (7.3821).

In summary, the analysis improves upon the OLS models by attempting to capture a more exogenous version of the mass layoff variable. Rather than using all mass layoff events, I include only those that are due to macro-level factors, which I define as business demand and organizational related. The BLS does not report mass layoffs by reason at the county level. Therefore, I use state level mass layoffs by reason as the instrumental variable, but interact it with the metropolitan area unemployment rate. The instrument captures the susceptibility of a county to shocks unrelated to local conditions. Using a two-stage least squares approach, the results do not substantively change the findings from the OLS models presented in Tables 3-5.

A more direct way of isolating exogenous mass layoff events would be to use county level mass layoffs due to business demand or organizational reasons in the construction of the shock variable. However, since mass layoffs by reason are not available at the county level, I must employ an IV approach. Given the issues raised by researchers (see Durlauf, 2004; Galster, 2008) on the use of aggregate measures as instruments, I consider the IV method and its results as a robustness test to the OLS findings.

Robustness Checks

The main findings - a negative shock to county quality two years ago impacts current graduation rates, current and one- and three-year lag shocks do not affect graduation or dropout rates and enduring or sustained shocks negatively impact graduation and dropout rates - prove to be robust to the IV specification presented in the previous section. In this section, I conduct several other checks to the OLS findings to ensure the results are robust to other plausible model specifications. The results of these robustness checks can be found in Appendix D.

First, Figure 2 indicates that the relationship between shock and dropout rate may have reversed after 2005. In other words, counties with high shocks experienced lower dropout rates before 2005 and higher dropout rates thereafter. The reversal may be muting the effects

Table 4.7: The impact of neighborhood level shocks on county log dropout rate, non-linear shock, 2003-2008: IV results

	(1)	(2)	(3)
$Shock_{it}$	-0.0276	-0.02990	
	(0.7585)	(1.0067)	
Log Population	-0.7508	-0.6543	-0.665
	(0.3405)	(0.4877)	(0.3634)
Percentage Non-white	0.4228	1.6768	0.6623**
	(1.4504)	(1.7329)	(1.3011)
Median Income	1.59e-05**	-2.34e-05	-5.79e-07**
	(6.49e-06)	(-8.69e-06)	(5.01e-06)
$Shock_{i(t-1)}$		-1.9035	
		(1.6031)	
$Shock_{i(t-2)}$		0.1029	
		(1.2188)	
$Shock_{i(t-3)}$		-1.5096	
		(0.6767)	
Four-year Avg. Shock			0.1562**
			(0.4889)
First stage regression (partial F-stat)			
$Instrument_{it}$	19.9	13.67	
	(0.0000)	(0.0000)	
$Instrument_{it-1}$		12.13	
		(0.0000)	
$Instrument_{it-2}$		10.13	
		(0.0000)	
$Instrument_{it-3}$		9.27	
		(0.0000)	
Four-Year Avg. Instrument			19.63
			(0.0000))
County Fixed Effects	X	X	X
Year Fixed Effects	X	X	X
N	11,738	11,717	11,717

** $p < 0.05$
Clustered standard errors are reported in parentheses
Models include county and year fixed effects. Instrument is the number of a state's mass layoffs that are due to business demand and organizational reasons interacted with the metropolitan area unemployment rate. For models with lags, two lags of instrument are used. For first stage regressions, partial F-tests on the first stage instruments are reported with p-values in parentheses.
Source: Author's calculations from the Bureau of Labor Statistics (BLS) Mass Layoffs Department, the National Center for Education Statistics, U.S. Census, and the Census Small Area Income and Poverty Estimates

Table 4.8: The impact of neighborhood level shocks on county log graduation, linear shock, 1996-2009: IV results

	(1)	(2)	(3)
$\%MassLayoffs_{it}$	0.0125	0.0489	
	(0.0077)	(0.2579)	
Log Population	0.1874**	0.2236**	0.1932**
	(0.0239)	(0.0664)	(0.0278)
Percentage Non-white	-0.0652	-0.3308	-0.2425**
	(0.0488)	(0.6340)	(0.0670)
Median Income	2.75e-06**	2.90e-06	2.95e-06**
	(4.96e-07)	(3.49e-06)	(5.67e-07)
$\%MassLayoffs_{i(t-1)}$		0.0063	
		(0.4325)	
$\%MassLayoffs_{i(t-2)}$		-0.0032**	
		(0.3405)	
$\%MassLayoffs_{i(t-3)}$		0.0024	
		(0.1512)	
Four-year Avg.			-1.3200**
%Mass Layoffs			(0.0082)
First stage regression (partial F-stat)			
$Instrument_{it}$	96.35	27.24	
	(0.0000)	(0.0000)	
$Instrument_{it-1}$		33.88	
		(0.0000)	
$Instrument_{it-2}$		25.45	
		(0.0000)	
$Instrument_{it-3}$		13.13	
		(0.0000)	
Four-year Avg. Instrument			63.54
			(0.0000)
County Fixed Effects	X	X	X
Year Fixed Effects	X	X	X
N	22,355	18,870	18,770

** $p < 0.05$

Clustered standard errors are reported in parentheses

Models include county and year fixed effects. Instrument is the number of a state's mass layoffs that are due to business demand and organizational reasons interacted with the metropolitan area unemployment rate. For models with lags, two lags of instrument are used. For first stage regressions, partial F-tests on the first stage instruments are reported with p-values in parentheses.

Source: Author's calculations from the Bureau of Labor Statistics (BLS) Mass Layoffs Department, the National Center for Education Statistics, U.S. Census, and the Census Small Area Income and Poverty Estimates

Table 4.9: The impact of neighborhood level shocks on county log dropout rate, linear shock, 2003-2008: IV results

	(1)	(2)	(3)
$\%MassLayoffs_{it}$	-0.0079	-0.0188	
	(0.2156)	(0.5204)	
Log Population	-1.5557**	-1.2767**	-0.641**
	(0.5538)	(0.8018)	(0.2900)
Percentage Non-white	1.5184	1.3475	1.0262
	(2.0719)	(0.9578)	(1.0410)
Median Income	-1.91e-05**	-3.61e-04	-8.29e-06**
	(9.16e-06)	(1.03e-02)	(3.98e-06)
$\%MassLayoffs_{i(t-1)}$		-0.0128	
		(0.2223)	
$\%MassLayoffs_{i(t-2)}$		-0.0419	
		(0.1156)	
$\%MassLayoffs_{i(t-3)}$		-0.0401	
		(1098.354)	
Four-year Avg.			7.1980**
%Mass Layoffs			(2.0465)
First stage regression (partial F-stat)			
$Instrument_{it}$	19.87	15.75	
	(0.0000)	(0.0000)	
$Instrument_{it-1}$		21.16	
		(0.0000)	
$Instrument_{it-2}$		35.69	
		(0.0000)	
$Instrument_{it-3}$		31.21	
		(0.0000)	
Four-year Avg. Instrument			91.47
			(0.0000)
County Fixed Effects	X	X	X
Year Fixed Effects	X	X	X
N	11,738	11,717	11,717

** $p < 0.05$
Clustered standard errors are reported in parentheses
Models include county and year fixed effects. Instrument is the number of a state's mass layoffs that are due to business demand and organizational reasons interacted with the metropolitan area unemployment rate. For models with lags, two lags of instrument are used. For first stage regressions, partial F-tests on the first stage instruments are reported with p-values in parentheses.
Source: Author's calculations from the Bureau of Labor Statistics (BLS) Mass Layoffs Department, the National Center for Education Statistics, U.S. Census, and the Census Small Area Income and Poverty Estimates

of a shock and thus separate models, one measured pre-2005 and the other model measured post-2005, were tested but yielded non-statistically significant results.

Second, I include year-by-state interactions in the model. While the county and year fixed effects absorb any persistent relationships between schooling persistence rates and mass layoff occurrence in a particular county or nationwide within a particular year, we may be concerned with relationships in a county that change over time that are not absorbed by the yearly median income, population, and percent non-white variables. Although I cannot include county-by-year effects, year-by-state fixed effects capture any characteristics about a state that change over time. Including these interactions largely do not change the results.

Finally, following Annanat et al. (2011), I conduct a falsification test in which I estimate the models using future mass layoff occurrence instead of lagged occurrences. These regressions test the assumption that mass layoff events, conditional on county and year fixed effects, can be viewed as exongenous shocks. Significant estimates from these regressions would indicate that counties experiencing a yearly or persistent mass layoff event in a given year already had a declining graduation rate or rising dropout rate. The results are generally small and statistically not significant and indicate that any changes in schooling persistence rates do not occur until after a mass layoff event occurs.

4.7 Discussion and Conclusion

Since schools are embedded within neighborhoods and are thus a reflection of them, parsing out the independent effects of neighborhood and school contexts has proven to be methodologically difficult. In their study of school effects on test scores, Downey, Broh, and von Hippel (2004) provide insight into a possible strategy for separating school and neighborhood influences. The authors isolate the school effect by taking the difference between test scores measured when the school is *on*, typically in the Spring when the student has had a full year of schooling, and *off*, typically at the beginning of the school year immediately after summer vacation. I employ a similar approach in this paper by using mass layoff events to study the effects of an economic shock on county level schooling persistence rates. Similar to the calendar switching the school on after the summer, a mass layoff event decreases the quality of a county. We know precisely the level at which the perturbation occurs. The event does not occur at the school or family levels, but at the county level, allowing us to assign estimated effects solely to county conditions.

Using log graduation and dropout rates as measures of schooling success, I find that a negative shock to county quality at time t has no effect on either county level graduation or dropout rates at time t. I also find that previous yearly shocks measured at times $t-1$, $t-2$ and $t-3$ have no effect on dropout rates. However, I find that a large shock measured at $t-2$ has an effect on graduation rates and an average four-year shock, which encompasses the 9th through 12th grade years for an on-time graduating senior, has a negative effect on dropout and graduation rates. The results are robust to various model specifications.

The results point to the importance of accounting for the magnitude and temporality of shocks to community quality. We don't expect minor shocks to impact geographic entities as large as counties, especially those with relatively large populations, a versatile work force, and a diverse set of businesses. In many cases, large counties have very small percentages of their labor force affected by mass layoffs. For example, San Diego county, which has a population of 3 million and covers approximately 4,000 square miles, experienced a mass layoff shock in 1998. But, the shock affected only 0.06 percent of the county's labor force, which likely has a minor impact on a county with such a large population that covers a wide geography and has a variety of businesses supporting its economic health. But, a shock has the capability of significantly impacting a county if it is larger in size and wider in reach. However, we don't necessarily expect the shock to have an immediate impact on a county's overall quality, which encompasses a complex, interwoven set of financial, political and social processes. For example, the shock may lead wealthier residents to leave the county, which would then reduce local funding for schools. The effects of reduced funding may then translate into cut backs on resources and programs, which would then impact the performance of students. We should not expect this process to occur instantaneously, but over a long period of time.

I also find a compelling story about the role persistent disadvantage plays in the relationship between community quality and academic success. A single large shock at time $t-2$ has an effect on graduation rates at time t. But, shocks occurring over a 4 year period, $t-3$ to t, also have an effect on rates, even if these shocks are relatively small. One set of findings tells the story that a single large shock can have an effect on a county, but it takes time for it to surface. Another set of findings tells the story that a county experiencing persistent shocks, minor or large, over time are also affected. In both cases, the timing of the effects matter.

Previous studies estimating the effects of the local community on schooling persistence yield mixed results. Harding (2003) reports propensity score matching estimates from the Panel Study of Income Dynamics (PSID) indicating that living in a high poverty neighborhood reduces the odds of high school graduation by 50 percent. Other studies report a more modest impact. For example, Brooks-Gunn et al (1993) and Aaronson (1998), both using PSID data, estimate the neighborhood effect on graduation rates in the range of 4 to 7 percent. Some studies find no effect at all. For instance, Plotnick and Hoffman (1995), using sibling matching in the PSID, and Ginther, Haveman, and Wolfe (2006) find that family background mediate the effect of neighborhoods on dropout rates. Jacob (2004) using a housing voucher program in Chicago to study the effects of moving into lower poverty neighborhoods finds no difference in dropout rates between movers and stayers.

Many of these studies routinely measure neighborhood characteristics at a single point in time, implicitly viewing these conditions as permanent rather than temporary traits (Timberlake 2007). Unlike in these previous estimates of community effects, the story I uncover is one of persistence and enduring disadvantage. Wodtke, Harding, and Elwert (2011) find that sustained exposure to neighborhood disadvantage has an impact on the chances of graduating high school. They assert that measuring neighborhood context only once will understate the full impact of extended exposure. They find that growing up in the most disadvantaged neighborhoods reduces the probability of graduation by 20 percentage points for black children and 8 percentage points for non-black children. My results are not directly comparable due to different measures of graduation (on-time vs. ever), the stratification of the population (all vs black/non-black) and level of aggregation (county vs. individual). Additionally, they look at average lifetime duration in poor neighborhood conditions whereas I examine average conditions during high school. Nevertheless, their effect sizes are similar to those found in this study. I find that a large, longer term shock reduces county level on-time graduation rates by 10 percentage points in the regular OLS model and 17 percentage points in the IV model.

There are several reasons why such effects might occur. First, although mass layoffs are much larger in size and scope compared to regular unemployment events, most of the county level shocks experienced during the time period of the analysis is not overwhelmingly large. Less than 5% of county-year observations experience a mass layoff shock that affects more than 5% of the labor force population. We cannot extrapolate from the results to determine whether significantly large shocks have similar effect patterns. Second, counties are relatively large and administratively stable enough over time that they may have the appropriate infrastructure to absorb single, relatively minor shocks to its overall quality. Once indicators of community distress reach critical threshold levels, negative outcomes among youth begin to increase (Crane 1991). I cannot test whether shocks to smaller areal units, such as the census tract, may be more susceptible to single year shocks, since mass layoff data are not collected at a lower geographic level.

Lastly, future opportunity sets differ between single- and persistent-shock counties. A single mass layoff shock would signal poor *current* economic conditions. Therefore, high school students would weather the shock by staying in school to wait for better conditions, rather than leaving school prematurely. A shock persisting over an extended time period might signal longer lasting or more permanent poor economic conditions. Youth in these communities will perceive the returns to education to be lower than they actually are, and hence acquire less education than they would in communities signaling optimistic economic conditions. When the future does not appear promising, adolescents are more likely to become disinterested in formal education. Rather than reducing lifetime expected returns by foregoing several years of income, students will not graduate (or enter the job market and not graduate on time) and begin earning a salary immediately, which may include participation in economic crime (Bellair, Roscigno and McNulty 2003). Peer and role model influences likely contribute to this effect, as students will observe recent graduates and adults with high school degrees struggling to gain employment. At some point, a cascading effect may occur over a long period of time such that norms equating dropping out and poor community conditions are accepted.

While performance accountability policies have largely focused on the school or district, the results of this study indicate that educational stakeholders should be more cognizant of the external factors that can negatively influence student performance. Understanding that the school and community have distinct influences on adolescent schooling success is important for policy because policy makers may want to formulate legislation and programs that focus on one context without completely ignoring the other. For example, school centric policies like school choice have greater potential to change the school but not the neighborhood, while community centric programs like housing mobility interventions might generate large changes in the neighborhood but less so in the school (Sanbonmatsu et al 2006).

A persistent effect is more consistent with Wilson's (1987) arguments regarding the consequences of concetrated poverty. The results presented in this paper support recent work demonstrating the importance of duration of exposure in the estimation of neighborhood effects on child development. While the present study does not speak to the specific characteristics of effective policies, it does establish a connection between a longer term, concentrated downturn in community conditions and the aggregate outcomes of residents, reaffirming the long standing belief that a committment to long lasting community improvement is necessary for solving the problems identified by Wilson and other advocates of extensive place-based social reform.

Chapter 5

Conclusion

In the face of the long-standing tradition within the social sciences of studying phenomena exclusively at the individual level, William Julius Wilson (1987) controversially hypothesized in his seminal book *The Truly Disadvantaged* that macro social and economic conditions play a crucial role in independently affecting individual life chances. Since his publication, a growing body of evidence has shown that conditions independent of the individual and outside the family have important consequences for individual health, education, and socioeconomic well-being. Researchers have also found that children are more susceptible to neighborhood conditions. This finding carries significant consequences since children today compared with their predecessors are more likely to be raised in extremely poor neighborhoods.

My primary research goal in this dissertation is directed towards understanding the influence of environmental conditions on youth outcomes, specifically those that are schooling related. Measuring the influence of neighborhoods on schooling outcomes is not a unique research endeavor. However, hanging like a cloud over much of this research is the problem of selection bias, or the possibility that results are driven by the unobservable characteristics of individuals and families selecting into specific types of neighborhoods. In this dissertation, I use a strategy, natural experiments, largely ignored in the neighborhood effects literature to obviate these issues. The natural experiment method makes use of naturally occurring phenomena that can be argued to induce some form of randomization across individuals. If we can find phenomena that exogenously place communities into better or worse conditions while leaving family and school level variables undisturbed, we can estimate the reduced form impact of neighborhood quality on the education outcomes of adolescent residents independent of individual, family and school level factors.

In chapter one of my dissertation, I outline the methodological problems of estimating neighborhood effects within the context of youth educational well-being. I describe the three levels of selection bias, individual, family and school, using a multi level framework. Understanding the sources of bias across these three levels is important since unlike other outcomes, academic success is heavily dependent on not just individual and family level factors, but also school quality, which is typically overlooked in most neighborhood effects studies. I then outline the conditions under which a natural experiment can solve these problems. This chapter sets the stage for the two empirical articles that make up the rest of my dissertation.

In chapters two and three, I apply the natural experiment approach to measure the effects of diminished or shocked community conditions on aggregate level schooling persistence rates. In chapter two, I use the 1960s civil rights riots and the 1992 Los Angeles Rodney King riots. I find that the diminished conditions in U.S. cities caused by large urban riots in the 1960s had short- (1960-1970) and long- (1960-1980) term effects on adolescent persistence rates. However, using a more contemporary shock, I find that diminished conditions caused by the Los Angeles riots in 1992 had only short term effects.

In chapter three, I use yearly county level mass layoff shocks in the United States during 1996 through 2010. Unlike the analysis in chapter two, which was restricted to decennial census data, the results of this analysis provides a more nuanced understanding of how neighborhoods affect adolescent schooling outcomes since yearly data is available. I was able to determine how the size, timing and persistence of shocks to county conditions affect aggregate level schooling rates. Counties are not affected by minor shocks and do not experience the effects of larger shocks immediately, but only after the shock has had time to work its way through the various complex pathways that connect neighborhoods to adolescent residents. I also find shocks, minor and large, that persist over time have an effect. These results, which capture the temporal aspects of neighborhood conditions, coincides with the recent trend in the neighborhood effects literature to turn away from point-in-time measurements of neighborhood quality and towards measures of duration (Wodtke et al. 2011).

In both empirical chapters, I assert that riots and mass layoffs are largely exogenous, sustained events affecting the various amenities that make up the quality of a community. More importantly, these shocks are not only exogenous at the individual, family and community levels, but also independent of school level factors. A riot or mass layoff may affect school quality within a community, but the origins of that shock are exogenous to the school. In both studies, I find evidence of long-term effects of diminished community conditions on schooling persistence rates and employ a variety of modelling specifications to test the robustness of these results. The contribution of the two chapters to the current literature goes beyond just showing that community conditions impact schooling persistence. They also exhibit the usefulness of more novel and sophisticated econometric techniques when lacking ideal data. Researchers should aim for data from randomized experiments, but in most cases they are not available. Instead of claiming defeat, we can still extract interesting results if we apply more powerful tools and test the robustness of the findings across a variety of specifications.

5.1 Limitations of the study

Several caveats concerning the data and methodology used in this dissertation are worth discussing. First, the units of analysis in chapters 2 and 3 are the city and county, respectively. Neighborhood effects studies typically rely on large, nationally representative surveys to utilize individual level data. Such data doesnt exist for the relevant time period (1950-1980) of the civil rights riots analysis. Individual level data specific to Los Angeles during the years before and after the 1992 riot also do not exist. Therefore, the analyses may fall prey to ecological fallacy, which consists in thinking that relationships observed for groups, such as cities and counties, necessarily hold for individuals.

Even if we can minimize ecological fallacy, we are still confronted with its geographic variant: the modifiable areal unit problem (MAUP), in which conclusions made based on data aggregated to a particular spatial unit (e.g. city) may change if one aggregates the same underlying data to a different spatial level (e.g. census tract). Unfortunately, these issues cannot be directly dismissed since the analyses in chapters 2 and 3 are restricted to city and county level data, respectively.

Since the empirical analyses are confined to aggregate units of observations, the significant effects derived from these analyses could be driven by selective mobility. As hypothesized in chapters 2 and 3, the diminished conditons caused by a riot or mass layoff shock may cause families with high achieving children to move. The composition effect of lower performing students remaining in or moving into the neighborhood simply increases aggregate level dropout rates. The diminished conditions negatively affect aggregate level rates because higher achieving youths move out, which is still an interesting result in and of itself, however the individuals who lived in the neighborhood at the time of the shock were not affected. As a way of addressing this issue, I include contemporaneous post-shock demographic variables in the models presented in chapters 2 and 3. I include changes in population size, percent ethnicity and percent living in poverty. We should not attach causal interpretations to these models given their likely endogeneity. Nevertheless, these changes in relevant contemporaneous variables did not alter the results reported and thus are not responsible for the strong correlation between shocks to community conditions and relative changes in schooling persistence rates.

5.2 Suggestions for future research

The dissertation has shown a potential strategy for estimating the effects of community conditions on schooling persistence rates while minimizing the selection problems that have plagued previous studies of neighborhood effects. The results of the analyses show that community conditions matter, but left unanswered is *how* neighborhoods matter. That is, the dissertation does not identify the potential mechanisms that tie community quality with the schooling outcomes of adolescent residents. Although the chapters speculate on the specific characteristics that are activated when community shocks occur, more work can be done to understand the particular pathways that directly connect neighborhood conditions with child well being. Echoing other empirical studies (Sampson 2008, Harding et al. 2011), more work needs to be done on identifying the specific mechanisms that drive the significant effects found in the types of neighborhood research done in this dissertation.

Given the limitations delineated in the previous section, another extension of the dissertation is to apply a natural experiment approach to individual level data. Not only will this align with what is traditionally done in the current neighborhood effects literature, it will also eliminate issues related to MAUP and ecological fallacy. Future research should also examine the presence of heterogenous effects. The effects of a neighborhood may differ by race, socioeconomic status, gender and a host of other demographic characteristics. Neighborhood effects may also differ by other measures of schooling success, such as test scores and grades, and age or grade level.

In order to fully assess the effectiveness and applicability of the natural experiment strategy in estimating neighborhood effects, future research should apply the approach using other natural and social phenomena and to measure the effects of the neighborhood on other outcomes, such as crime, teenage pregnancy rates and employment. A major goal of this dissertation is to provide researchers a theoretical and empircal springboard for using natural experiments to study neighborhood effects. Despite their increasing use in the social sciences, natural experiments still lack a thorough statistical grounding and an empirical foundation, especially within the neighborhood effects literature. Continuing to apply this method in different contexts will help determine its sensitivity to varying specifications, clarify its statistical properties and provide a complete range of its applicability.

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Appendix A

Natural Experiment Conditions

In this section, I set forth an analytic framework that permits us to determine the conditions that must be met in order to use an event such as an urban riot or natural disaster to estimate the reduced form effect of the variable of interest D (e.g. neighborhood quality) on the dependent variable (e.g. test scores). The framework established here builds on the work of Angrist (1998) and White (2006) analyzing the economic effects of natural experiments both in cross sectional and time series settings.

I consider a data generating process with explicit causal structure in order to define the effects of interest and to specify the formal conditions permitting the identification of these effects. Suppose that the variable of interest Y_t is determined by the following relationship

$$Y_t = v(\Delta_t, \mathbf{X}_t) \tag{A.1}$$

where Δ_t is an indicator variable for an event we want to use in a natural experiment, such as the occurence of a large scale riot, at time t i.e. $\Delta_t = 0$ in a neighborhood not experiencing the event and $\Delta_t = 1$ in a neighborhood experiencing the event; and $\mathbf{X}_t$ is a vector of determining variables. The variable Δ_t is the lever that turns down quality D_t within a neighborhood. Note that we do not explicitly account for the variable of interest D_t[1] since underyling this framework we assume Δ_t creates exogenous variation in its distribution. The response function v is unknown, but is typically assumed linear in empirical work.

At a specific time t there are units experiencing and not experiencing an event. Let T_0 denote regime 0 ($\Delta_t = 0$) observation indexes, $T_0 = \{t \in N : \Delta_t = 0\}$ and let T_1 denote regime 1 ($\Delta_t = 1$) observation indexes $T_1 = \{t \in N : \Delta_t = 1\}$. We can conceive of T_0 as neighborhoods not affected by a riot and thus did not experience a downgrade in conditions and T_1 as neighborhoods affected by a riot and thus did experience a decrease in overall quality. We can define Y_t separately for each regime

$$\begin{aligned} Y_t &= v_0(X_t) = v(0, X_t), \qquad t \in T_0 \\ &= v_1(X_t) = v(1, X_t), \qquad t \in T_1 \end{aligned}$$

[1]By not using the reduced form estimate, we can adopt the framework from an instrumental variables regression

Following White (2005), we can separate the vector $\mathbf{X}_t$ into two separate components: $\tilde{X}_t$ and $\ddot{X}_t$ are a set of potential causes or determining variables, of which $\tilde{X}_t$ is observed and $\ddot{X}_t$ is not. We now have the concepts to specify the data generating process

Data Generating Process: The observed data are generated from a realization of the sequence of random variables $(Y_t, \Delta_t, \tilde{X}_t, \ddot{X}_t)$, $t = 0, 1, 2, ...$, where $(\tilde{X}_t, \ddot{X}_t)$ stably isolates Δ_t for Y_t according to

$$Y_t = v(\Delta_t, \tilde{X}_t, \ddot{X}_t), \qquad \text{t = 0,1,2,...,}$$

for some unknown measurable scalar-valued function v, where Δ_t is $\{0, 1\}$-valued. For $j = 0, 1$, define $T_j = \{t \in N : \Delta_t = j\}$ and assume that for all $\ddot{X}_t$, $(\tilde{X}_t, \ddot{X}_t)$ has joint distribution F_j, $\ddot{X}_t$ has distribution G_j, $\tilde{X}_t$ has joint distribution H_j, and the conditional distribution of $\ddot{X}_t$ given $\tilde{X}_t = \ddot{x}_t$ is $\tilde{G}_j(\cdot|\tilde{x}_t)$. The realizations of Y_t, Δ_t and $\tilde{X}_t$ are observed, whereas those of $\ddot{X}_t$ are not observed.

Given the data generating process, we can define the effect of the event given ($\tilde{x}_t$, $\ddot{x}_t$) as

$$\theta(\tilde{x}_t, \ddot{x}_t) = v_1(\tilde{x}_t, \ddot{x}_t) - v_0(\tilde{x}_t, \ddot{x}_t), \tag{A.2}$$

This effect is unobservable, however, because $v_1(\tilde{x}_t, \ddot{x}_t)$ and $v_0(\tilde{x}_t, \ddot{x}_t)$ are not simultaneously observable: we can usually observe only one of these quantities. Instead, we can estimate the *treatment effect on the treated*

$$\begin{aligned} \theta^{tot} &= \int \theta(\tilde{x}_t, \ddot{x}_t) d\tilde{G}_1(\ddot{x}|\tilde{x}) d\tilde{H}_1(\tilde{z}) \\ &= \int v_1(\tilde{x}_t, \ddot{x}_t) d\tilde{G}_1(\ddot{x}|\tilde{x}) d\tilde{H}_1(\tilde{z}) - \int v_0(\tilde{x}_t, \ddot{x}_t) d\tilde{G}_1(\ddot{x}|\tilde{x}) d\tilde{H}_1(\tilde{z}) \\ &= \mu_1 - \mu_{10} \end{aligned}$$

The estimator θ^{tot} represents the average effect of the shock for those treated in a given time t, averaging across time and over underlying conditions in that unit.

The above framework now allows us to identify the conditions in which we can estimate the effect of interest θ. I explicity permit the joint distribution of $(\tilde{X}_t, \ddot{X}_t)$ to depend on Δ_t

$$F_0(\tilde{x}_t, \ddot{x}_t) = F(\tilde{x}_t, \ddot{x}_t|\Delta_t = 0)$$
$$F_1(\tilde{x}_t, \ddot{x}_t) = F(\tilde{x}_t, \ddot{x}_t|\Delta_t = 1)$$

where $F(\cdot|\Delta_t)$ is the conditional distribution of $(\tilde{X}_t, \ddot{X}_t)$ given Δ_t. The equivalence of F_0 and F_1 means that Δ_t is independent of $(\tilde{X}_t, \ddot{X}_t)$. This is the first condition the event underlying Δ_t must meet

Condition 1: Exogeneity Across Space. The joint distributions of F_0 and F_1 of the included observables $\tilde{X}_t$ and the unobservable determinants $\ddot{X}_t$ are identical across units.

For this to hold, $H_0 = H_1$ and $\tilde{G}_0 = \tilde{G}_1$.

These conditions are precisely what a random experiment achieves: the treatment Δ_t is randomly assigned. In certain circumstance it may be possible to achieve $H_0 = H_1$ and $\tilde{G}_0 = \tilde{G}_1$, however this assumption is generally too strong to be met in most empirical contexts. Therefore, most researchers employ the more realistic condition that treatment is independent *upon conditioning on* $\tilde{X}_t$

$$Y_t \perp \Delta_t | \tilde{X}_t$$

We can take advantage of the time series setting and specify an estimator that eliminates secular trends. We specify post and pre event periods, with the event lasting from time $\tau + 1$ to n. Let T_{00} denote regime 0 ($\Delta_t = 0$) observation indexes before the event, $T_{00} = \{t \in [0, \tau] : \Delta_t = 0\}$, T_{10} denote regime 0 ($\Delta_t = 0$) observation indexes after the event $T_{10} = \{t \in [\tau + 1, n] : \Delta_t = 0\}$, and let T_{01} denote regime 1 ($\Delta_t = 1$) observation indexes before the event $T_{01} = \{t \in [0, \tau] : \Delta_t = 1\}$, T_{11} denote regime 1 ($\Delta_t = 1$) observation indexes after the event $T_{11} = \{t \in [\tau + 1, n] : \Delta_t = 1\}$. Given T_{it}, we can establish separate regime by period distributions F_{jt}, G_{jt} and H_{jt}. We now can define the differences-in-differences estimator, which subtracts the post-pre estimator in the untreated group from the treated group

$$\begin{aligned}
\theta^{did} &= \int \theta(\tilde{x}_t, \ddot{x}_t) d\tilde{G}_{1t}(\ddot{x}|\tilde{x}) d\tilde{H}_{1t}(\tilde{z}) - \int \theta(\tilde{x}_t, \ddot{x}_t) d\tilde{G}_{0t}(\ddot{x}|\tilde{x}) d\tilde{H}_{0t}(\tilde{z}) \\
&= (\int v_{11}(\tilde{x}_t, \ddot{x}_t) d\tilde{G}_{1t}(\ddot{x}|\tilde{x}) d\tilde{H}_{1t}(\tilde{z}) - \int v_{10}(\tilde{x}_t, \ddot{x}_t) d\tilde{G}_{1t}(\ddot{x}|\tilde{x}) d\tilde{H}_{1t}(\tilde{z})) - \\
&\quad (\int v_{01}(\tilde{x}_t, \ddot{x}_t) d\tilde{G}_{0t}(\ddot{x}|\tilde{x}) d\tilde{H}_{1t}(\tilde{z}) - \int v_{00}(\tilde{x}_t, \ddot{x}_t) d\tilde{G}_{0t}(\ddot{x}|\tilde{x}) d\tilde{H}_{1t}(\tilde{z})) \\
&= (\mu_{11} - \mu_{10}) - (\mu_{01} - \mu_{00})
\end{aligned}$$

Defining μ in expectation form, we get

$$\begin{aligned}
\mu_{11} &= E(Y_t | \Delta_t = 1, T_1) \\
\mu_{10} &= E(Y_t | \Delta_t = 1, T_0) \\
\mu_{01} &= E(Y_t | \Delta_t = 0, T_1) \\
\mu_{00} &= E(Y_t | \Delta_t = 0, T_0)
\end{aligned}$$

The DID estimator (assuming the conditional assumption) can be written out as follows

$$(\mu_{11} - \mu_{10}) - (\mu_{01} - \mu_{00}) + E(\ddot{X}_{11} - \ddot{X}_{10}) - E(\ddot{X}_{01} - \ddot{X}_{00})$$

The key assumption will therefore be $E(\ddot{X}_{11} - \ddot{X}_{10}) = E(\ddot{X}_{01} - \ddot{X}_{00})$, which means that the outcomes for the two regimes must have similar trajectories over the two time periods absent any treatment effect.

The DID estimator makes the panel nature of the framework more explicit. The fact that T_0 and T_1 may themselves contain sequences of observations in a time series is an important aspect of the present setup that separates it from a cross sectional setting. With time series data, a Δ_1 unit at time t is being compared to a previous period, presumably but not restricted to the previous contiguous period, when it was not under treatment. Assumption 1 can be broken for two reasons. First, treated and non treated units have different observed and unobserved characteristics that may be driving differences (i.e. $H_0 \neq H_1$ and $\tilde{G}_0 \neq \tilde{G}_1$). This is true before and after treatment. However, observables and unobservables may be equal absent of treatment, but the treatment itself alters the trajectory of $\tilde{X}$ or $\ddot{X}$ in either regime. In other words, if the unit treated at time t adjusts $\tilde{X}$ or $\ddot{X}$ in time $t-1$ *in expectation of being treated*, then assumption 1 breaks. Stated formally we have assumpton 2

Condition 2: Exogeneity Across Time. If t is $\{0,1\}$-valued, where $t=0$ denotes pre event $t \in [0,\tau]$ and $t=1$ denotes post event $t \in [\tau+1,n]$, we define $T_{it} = \{t \in [0,1] : \Delta_t = j\}$ and establish separate treatment regime and period distributions F_{jt}, H_{jt} and G_{it}. To estimate θ, $H_{10} = H_{11}$ and $\tilde{G}_{10} = \tilde{G}_{11}$.

If a neighborhood expects the event to occur during time t, it may alter variables $\tilde{X}$ and $\ddot{X}$ to soften or completely eliminate the impact. For example, shifting general funds to accomodate expected loses in the public sector or attract new businesses or invest in current businesses to offset negative labor market and economic effects. Many researchers use natural disasters as examples of natural experiments (Kirk 2009; Torche 2011). However, many communities have experienced natural disasters in the past and thus are prepared to a certain extent for future disasters. Although it is arguable that earthquakes in California and severe disasters like Hurricane Katrina are largely random, disasters that are seasonal in certain regions, such as tropical storms and milder hurricanes in the gulf coast, have less validity as natural experiments because community members are prepared for such events to occur. Once again, we can control for observed variables $\tilde{X}_t$ pre treatment, but any unobserved factors jointly driving changes in the response variable and correlated with the expectation of treatment will bias and likely understate the estimated effect θ.

In the natural experiments literature, focus has largely been on meeting condition 1. The importance of condition 2 cannot be understated, as many natural experiments, such as natural disasters or programs inacted by policies, are not necessarily "natural" if the participants can prepare in advance for the event. This is an important point, as not only do observables and unobservables pre treatment change in expectation of the event, but who gets treatment may also change conditioned on the population remaining the same. In other words, the preparation may alter the population receiving the treatment. In a clinical trials setting, this phenomenom is called selection into treatment.

Another condition which must be met that is particular to a neighborhood effects estimation framework is that the event Δ_t must be enacted at the neighborhood level and be widespread enough to affect neighborhood level overall quality NQ. We can decompose $\ddot{X}_t$ by context c: $\ddot{X}_t = \ddot{X}_t^{cn} + \ddot{X}_t^{c1} + \ddot{X}_t^{c2} + ... + \ddot{X}_t^{ck}$, where ck represents context k, for example the school, and n represents the neighborhood context. We can formally state the third condition as:

Condition 3: The event Δ_t has a non-zero average effect on neighborhood variable N_t and has no independent effect on any non neighborhood context variables

a.) $Cov(N_t, \Delta_t) \neq 0$
b.) $Cov(\ddot{X}^{cn}, \Delta_t) = 0$
c.) $Cov(\ddot{X}^{ck}, \Delta_t | N_t, \tilde{X}^n_t, \ddot{X}^n_t) = 0 \qquad \forall k \neq n$

The first property ensures that Δ_t actually captures some variation in N_t. This means that the shock is wide and strong enough to procure some non neglible effect on the neighborhood variable of interest. The second property ensures that unobserved neighborhood level variables are not correlated with the event. Finally, the third property ensures that the event does not have an independent (of neighborhood) effect on any other context k, such as the school, family or individual. Put another way, the last two properties require the shock to impact the outcome only indirectly through N_t and not via any other route. Condition 3 follows closely the exclusion and inclusion restriction properties of an instrumental variables framework (Angrist 1998).

Another condition that must be met is known in the causal analysis literature as the stable unit treatment value assumption, or SUTVA (Rubin 1980). The assumption states that unit $i's$ potential outcomes are unaffected by whether unit k ($k \neq i$) is treated or untreated. A violation of this assumption occurs when an event has spillover effects on neighboring communities. Formally, let $\mathbf{\Delta}_t$ denote a treatment-allocation *vector*, which indicates event status for all units, and Δ_{it} as the i-th element of this vector. Further, let $v_{ijt}(\Delta_{it}, \tilde{x}_t, \ddot{x}_t)$ denote the outcome for unit i in regime j at time t.

Condition 4: Stable Unit Treatment Value Assumption. For any two allocations $\mathbf{\Delta}_t$ and $\mathbf{\Delta}'_t$, if $\Delta_{it} = \Delta'_{it}$, then $v_{ijt}(\Delta_{it}, \tilde{x}_t, \ddot{x}_t) = v_{ijt}(\Delta'_{it}, \tilde{x}_t, \ddot{x}_t)$

SUTVA amounts to assuming that the event is well-defined and that there is no interference between units. Without this assumption, the number of possible causal effects for each individual in the population increases exponentially with the number of other people in the population.

Finally, we impose a *monotonicity assumption* stating that the event Δ_t does not change the status of the underlying variable of interest D_{it} in opposite directions for different units

Condition 5: Monotonicity. Define $D_{it}(\Delta_t)$ as the treatment value for unit i given event indicator $\Delta_t = j$ where $j = 0, 1$. $D_{it}(\Delta_t = 1) \geq D_{it}(\Delta_t = 0)$ for all $i = 1, ...N$

The monotonicity assumption rules out the possibility of defiers. Assume $D_{it} = d'$ as treated (the value we would expect when $\Delta_t = 1$) and $D_{it} = d$ as untreated (the value we would expect when $\Delta_t = 0$). Condition 5 prohibits units changing from $D_{it} = d'$ to $D_{it} = d$ when the event changes from $\Delta_t = 0$ to $\Delta_t = 1$. Condition 5 assumes no heterogenous effects (see Angrist and Imbens (1994) for more details).

If an event satisfies conditions 1-5, namely that it is randomly assigned across space and time, its average effect on D is nonzero, it satisfies the exclusion restriction and the monotonicity assumptions, and SUTVA holds, then we can use it to estimate the causal effect of D on Y.

Appendix B

Riots Analysis: Robustness Checks

Table B.1: Ordinary Least Squares Regression: Log non-enrollment Rates and Riot Severity, 1960-1970 and 1960-1980

	1960-70		1960-80	
Shock Severity	0.0015**	0.0133**	0.0255	0.0061
	(0.0008)	(0.0042)	(0.0379)	(0.0045)
Shock Severity^2	-0.0007		0.0009	
	(0.0015)		(0.0018)	
Log Population	3.43e-08**	-6.51e-08	-3.74e-09	3.03e-08
	(1.43e-08)	(1.31e-07)	(1.24e-08)	(1.46e-07)
Log Population^2		5.64e-14		-1.75e-14
		(5.15e-14)		(6.00e-14)
Log Population^3		-5.74e-21		1.81e-21
		(4.57e-21)		(5.40e-21)
Percent non-white	-0.0834	-0.097	-1.0314**	-1.0124**
	(0.2533)	(0.2584)	(0.2832)	(0.2741)
Midwest	-0.2317**	-0.2355**	-0.0763	-0.0724
	(0.0736)	(0.0745)	(0.0644)	(0.0638)
Northeast	-0.1492	-0.1518	-0.1656**	-0.1628**
	(0.0824)	(0.0832)	(0.0778)	(0.0774)
West	-0.1352	-0.1363	0.2986**	0.3005**
	(0.0903)	(0.0909)	(0.0771)	(0.0773)
1960 Log non-enrollment Rate	-0.3299**	-0.3273**	-0.2276**	-0.2263**
	(0.0848)	(0.0867)	(0.0823)	(0.0837)
Median Housing Value	-0.1994	-0.2048	0.0797	0.0849
	(0.1106)	(0.1108)	(0.1163)	(0.1150)
Unemployment Rate	-0.7226	-0.7803	-1.3314	-1.2394
	(1.5276)	(1.5025)	(1.4698)	(1.4323)
Percentage 25+ w/ HS Degree	-0.5892	-0.5755	-1.0372**	-1.0439**
	(0.3536)	(0.3559)	(0.3623)	(0.3649)
Observations	302	302	300	300
R^2	0.143	0.144	0.268	0.268

Includes polynomials for Riot Severity and Log Population

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

Source: See Table 3.3 for sources

Table B.2: Ordinary Least Squares Regression: Log non-enrollment Rates and Riot Severity, 1960-1970 and 1960-1980

	1960-70		1960-80	
Shock Severity	0.0127**	0.0083**	0.0063	0.0078
	(0.0037)	(0.0040)	(0.0041)	(0.0047)
Log Population	1.83e-08	2.92e-08	4.93e-09	1.08e-08
	(1.41e-08)	(3.84e-08)	(1.36e-08)	(3.75e-08)
Percent non-white	-0.1322	0.0158	-1.0103**	-1.0916**
	(0.2669)	(0.2838)	(0.2878)	(0.3039)
Midwest	-0.2474**	-0.1829**	-0.0717	-0.1118
	(0.0744)	(0.0797)	(0.0642)	(0.0711)
Northeast	-0.1576	-0.1285	-0.1651**	-0.1532
	(0.0839)	(0.0901)	(0.0786)	(0.0827)
West	-0.1362	-0.0649	0.2982**	0.1862**
	(0.0906)	(0.0949)	(0.0773)	(0.0825)
1960 Log non-enrollment Rate	-0.3342**	-0.3103**	-0.2269**	-0.2620**
	(0.0849)	(0.0774)	(0.0820)	(0.0945)
Median Housing Value	-0.2154	-0.1412	0.0837	0.1108
	(0.1115)	(0.1225)	(0.1166)	(0.1267)
Unemployment Rate	-1.2159	-1.101	-1.1559	-0.4861
	(1.4986)	(1.4221)	(1.4663)	(1.4828)
Percentage 25+ w/ HS Degree	-0.6175	-0.557	-1.0369**	-0.8702**
	(0.3575)	(0.3717)	(0.3674)	(0.3906)
Observations	296	257	294	255
R^2	0.144	0.12	0.260	0.264

Columns 1 & 3: Eliminates Large Riot Severity Cities

Columns 2 & 4: Eliminates Non-Riot Cities Sharing Border

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

Source: See Table 3.3 for sources

Table B.3: Difference-in-Differences: Log non-enrollment Rates and Riot occurrence, 1960-1970 and 1960-1980

	1960-70		1960-80	
Shock Year	-0.4126**	-0.4158**	0.4108**	0.4027**
	(0.0427)	(0.0428)	(0.096)	(0.0959)
Shock City	0.0417	0.0294	0.0362	0.0226
	(0.0355)	(0.0355)	(0.0361)	(0.0360)
Shock Year x Shock City	0.1221**	0.1230**	0.1193**	0.1178**
	(0.052)	(0.0519)	(0.0541)	(0.0539)
Log Population	2.10e-07**	3.93e-07**	2.06e-07**	4.20e-07**
	(4.75e-08)	(1.04e-07)	(5.11e-08)	(9.87e-08)
Log Population^2	-2.17e-14**	-1.34e-13**	-2.39e-14**	-1.58e-13**
	(6.35e-15)	(4.91e-14)	(7.17e-15)	(4.35e-14)
Log Population^3		1.15e-20**		1.42e-20**
		(4.70e-21)		(4.18e-21)
Percent non-white	-0.1724	-0.1795	-0.1401	-0.1431
	(0.1362)	(0.1346)	(0.118)	(0.1183)
Midwest	-0.3330**	-0.3282**	-0.2323**	-0.2256**
	(0.0401)	(0.0401)	(0.0435)	(0.0435)
Northeast	-0.1676**	-0.1645**	-0.1678**	-0.1619**
	(0.0464)	(0.0463)	(0.0443)	(0.0445)
West	-0.2535**	-0.2522**	0.0664	0.0671
	(0.0537)	(0.0535)	(0.0564)	(0.0563)
Median Housing Value	-0.3321**	-0.3252**	-0.2078**	-0.2004**
	(0.0652)	(0.065)	(0.0633)	(0.0633)
Unemployment Rate	-0.2544	-0.2415	-3.4010**	-3.3339**
	(0.9503)	(0.9501)	(0.8228)	(0.8208)
Percentage 25+ w/ HS Degree	-1.5009**	-1.5121**	-2.4039**	-2.4131**
	(0.1847)	(0.1843)	(0.1996)	(0.1994)
Observations	604	604	603	603
R^2	0.645	0.647	0.51	0.514

Includes polynomials for Log Population

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

Source: See Table 3.3 sources

Table B.4: Difference-in-Differences: Log non-enrollment Rates and Riot occurrence, 1960-1970 and 1960-1980

	1960-70		1960-80	
Shock Year	-0.4057**	-0.3900**	0.4216**	0.3577**
	(0.0429)	(0.0504)	(0.0978)	(0.1137)
Shock City	0.064	0.0512	0.0556	0.0292
	(0.0359)	(0.0388)	(0.0366)	(0.0398)
Shock Year x Shock City	0.1208**	0.0828**	0.1207**	0.1059**
	(0.0529)	(0.0472)	(0.0552)	(0.0483)
Log Population	6.82e-08**	1.43e-07**	6.48e-08**	1.32e-07**
	(1.72e-08)	(2.89e-08)	(2.11e-08)	(3.34e-08)
Percent non-white	-0.212	-0.2795	-0.1398	-0.2485
	(0.1453)	(0.1611)	(0.1219)	(0.1400)
Midwest	-0.3374**	-0.3165**	-0.2344**	-0.2136**
	(0.041)	(0.0445)	(0.044)	(0.0479)
Northeast	-0.1816**	-0.1784**	-0.1807**	-0.1610**
	(0.0466)	(0.052)	(0.0445)	(0.0482)
West	-0.2502**	-0.2845**	0.0721	0.0022
	(0.0544)	(0.0572)	(0.0571)	(0.0617)
Median Housing Value	-0.3421**	-0.2561**	-0.2088**	-0.1529**
	(0.0656)	(0.077)	(0.0643)	(0.0757)
Unemployment Rate	-0.2899	-0.8667	-3.4939**	-3.2697**
	(0.9718)	(0.9949)	(0.8478)	(0.9016)
Percentage 25+ w/ HS Degree	-1.5203**	-1.5325**	-2.4299**	-2.3486**
	(0.1859)	(0.219)	(0.2022)	(0.2196)
Observations	592	514	591	513
R^2	0.639	0.618	0.501	0.482

Columns 1 & 3: Eliminates Large Riot Severity Cities

Columns 2 & 4: Eliminates Non-Riot Cities Sharing Border

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

Source: See Table 3.3 for sources

Appendix C

Extended Layoff Reasons and Higher-Level Reason Categories

- Business Demand Reasons
  - Contract cancellation
  - Contract completion
  - Domestic competition
  - Excess inventory/saturated market
  - Import competition
  - Slack work/insufficient demand/non-seasonal business slowdown
- Disaster/Safety Reasons
  - Hazardous work environment
  - Natural disaster (not weather related)
  - Non-natural disaster
  - Extreme weather-related event
- Financial Reasons
  - Bankruptcy
  - Cost control/cost cutting/increase profitability
  - Financial difficulty
- Organizational Reasons
  - Business-ownership change
  - Reorganization or restructuring of company

- Production Reasons
  - Automation/technological advances
  - Energy related
  - Government regulations/intervention
  - Labor dispute/contract negotations/strike
  - Material or supply shortage
  - Model changeover
  - Plant or machine repair/maintenance
  - Product line discontinued
- Seasonal Reasons
  - Seasonal
  - Vacation period - school related or otherwise
- Other
  - Federal government cutbacks - Not Defense-related
  - Federal government cutbacks - Defense-related
  - Data not provided

Appendix D

Mass Layoffs Analysis: Robustness Checks

Table D.1: The impact of neighborhood level shocks on county log average freshman graduation rate, non-linear shock, 1996-2004: OLS Results

	(1)	(2)	(3)	(4)
$LowShock_{it}$	-0.0607**	-0.0023	-0.006	
	(0.0045)	(0.0031)	(0.0049)	
$HighShock_{it}$	-0.0923**	0.0001	-0.0032	
	(0.0058)	(0.0036)	(0.0051)	
Log Population		0.1617**	0.2974**	0.2980**
		(0.0346)	(0.0514)	(0.0514)
Percentage Non-white		-0.0068	-0.1544	-0.1529
		(0.0679)	(0.1464)	(0.1463)
Median Income		9.15e-07	-1.83e-06	-1.86e-06
		(9.62e-07)	(1.88e-06)	(1.85e-06)
$LowShock_{it-1}$			-0.0003	
			(0.0038)	
$HighShock_{it-1}$			0.002	
			(0.0065)	
$LowShock_{it-2}$			-0.0002	
			(0.0046)	
$HighShock_{it-2}$			0.0035	
			(0.0046)	
$LowShock_{it-3}$			0.0079	
			(0.007)	
$HighShock_{it-3}$			0.0059	
			(0.0047)	
4-year Avg. Low Shock				-0.058**
				(0.006)
4-year Avg. High Shock				-0.084**
				(0.007)
County Fixed Effect	-	X	X	X
Year Fixed Effect	-	X	X	X
Observations	27,631	24,514	18,442	18,442
R^2	0.023	0.009	0.010	0.010

** $p < 0.05$
Clustered standard errors are reported in parentheses
Source: See Table 4.4

Table D.2: The impact of neighborhood level shocks on county log average freshman graduation rate, non-linear shock, 2005-2009: OLS Results

	(1)	(2)	(3)	(4)
$LowShock_{it}$	-0.0397**	-0.0012	0.0015	
	(0.0053)	(0.0037)	(0.0038)	
$HighShock_{it}$	-0.0781**	0.009	0.0119	
	(0.0077)	(0.008)	(0.0077)	
Log Population		0.2965**	0.2472	0.2523**
		(0.1316)	(0.129)	(0.1285)
Percentage Non-white		-0.1564	-0.1467	-0.1251
		(0.3558)	(0.3519)	(0.3510)
Median Income		-2.13e-06 **	-2.03e-06**	-2.11e-06**
		8.19e-07	8.66e-07	(8.38e-07)
$LowShock_{it-1}$			0.0027	
			(0.0052)	
$HighShock_{it-1}$			0.0078	
			(0.0054)	
$LowShock_{it-2}$			0.0047	
			(0.0036)	
$HighShock_{it-2}$			-0.0036	
			(0.017)	
$LowShock_{it-3}$			0.0148	
			(0.058)	
$HighShock_{it-3}$			0.0049	
			(0.0058)	
4-year Avg. Low Shock				-0.0398**
				(0.020)
4-year Avg. High Shock				-0.091**
				(0.0098)
County Fixed Effect	-	X	X	X
Observations	14,856	14,847	14,827	14,827
R^2	0.012	0.003	0.004	0.003

** $p < 0.05$

Clustered standard errors are reported in parentheses

Source: See Table 4.4

Table D.3: The impact of neighborhood level shocks on county log dropout rate, 2003-2004: OLS Results

	(1)	(2)	(3)	(4)
$LowShock_{it}$	0.5144**	0.0015	-0.0177	
	(0.0357)	(0.0417)	(0.0493)	
$HighShock_{it}$	0.4173**	-0.0166	0.0201	
	(0.0472)	(0.0622)	(0.0868)	
Log Population		-0.5612	-0.5809	-0.6116
		(2.2819)	(2.2645)	(2.2727)
Percentage Non-white		2.4522	2.4209	2.2276
		(4.8828)	(4.8496)	(4.8811)
Median Income		-5.15e-06	-6.11e-06	-5.14e-06
		(3.74e-05)	(3.70e-05)	(3.75e-05)
$LowShock_{it-1}$			-0.0392	
			(0.0937)	
$HighShock_{it-1}$			0.0263	
			(0.0972)	
$LowShock_{it-2}$			-0.0572	
			(0.0588)	
$HighShock_{it-2}$			0.0499	
			(0.1057)	
$LowShock_{it-3}$			0.1084	
			(0.1026)	
$HighShock_{it-3}$			-0.0464	
			(0.0684)	
4-year Avg. Low Shock				0.2244**
				(0.0551)
4-year Avg. High Shock				0.2641**
				(0.0814)
County Fixed Effect	-	X	X	X
Year Fixed Effect	-	X	X	X
Observations	4,762	4,762	4,762	4,762
R^2	0	0.002	0.007	0.002

** $p < 0.05$

Clustered standard errors are reported in parentheses

Source: See Table 4.4

Table D.4: The impact of neighborhood level shocks on county log dropout rate, non-linear shock, 2005-2008: OLS Results

	(1)	(2)	(3)	(4)
$LowShock_{it}$	0.6395**	-0.03	-0.0374	
	(0.0444)	(0.0236)	(0.0264)	
$HighShock_{it}$	0.5505**	-0.0286	-0.0367	
	(0.0527)	(0.0411)	(0.0437)	
Log Population		-1.355	-1.3531	-1.3945
		(0.7967)	(0.8077)	(0.8080)
Percentage Non-white		-0.6756	-0.5655	-0.4906
		(2.8435)	(2.8589)	(2.8414)
Median Income		-9.33e-07	-9.52e-07	-8.27e-07
		7.59e-06	7.59e-06	(7.56e-06)
$LowShock_{it-1}$			-0.0189	
			(0.0245)	
$HighShock_{it-1}$			-0.0053	
			(0.0251)	
$LowShock_{it-2}$			-0.0245	
			(0.0288)	
$HighShock_{it-2}$			0.0129	
			(0.0441)	
$LowShock_{it-3}$			-0.0153	
			(0.0441)	
$HighShock_{it-3}$			-0.0731	
			(0.0445)	
4-year Avg. Low Shock				0.0865**
				(0.0088)
4-year Avg. High Shock				0.0113**
				(0.0089)
County Fixed Effect	-	X	X	X
Year Fixed Effect	-	X	X	X
Observations	14,524	14,524	14,503	14,503
R^2	0.039	0.021	0.022	0.022

** $p < 0.05$

Clustered standard errors are reported in parentheses

Source: See Table 4.4

Table D.5: The impact of neighborhood level shocks on county log average freshman graduation rate, non-linear shock, 1996-2009: OLS Results with State x Year Fixed Effects

	(1)	(2)	(3)
$LowShock_{it}$	-0.0011	-0.0016	
	-0.0022	-0.0028	
$HighShock_{it}$	0.001	0.0002	
	-0.0034	-0.0041	
Log Population	0.1178**	0.1207**	0.1192**
	-0.0239	-0.0303	(0.0302)
Percentage Non-white	-0.0309	-0.0853	-0.0847
	-0.0613	-0.1155	(0.1154)
Median Income	-3.60e-07	-5.09e-07	-5.07e-07
	(5.65e-07)	(6.90e-07)	(6.86e-07)
$LowShock_{it-1}$		-0.0005	
		-0.0031	
$HighShock_{it-1}$		0.0003	
		-0.0036	
$LowShock_{it-2}$		-0.0023	
		-0.0037	
$HighShock_{it-2}$		-0.0055	
		-0.0061	
$LowShock_{it-3}$		0.0021	
		-0.0041	
$HighShock_{it-3}$		-0.0022	
		-0.004	
4-year Avg. Low Shock			-0.0351**
			(0.0043)
4-year Avg. High Shock			-0.0824**
			(0.0052)
County Fixed Effect	X	X	X
State x Year Fixed Effect	X	X	X
Observations	39,361	33,269	33,269
R^2	0.064	0.057	0.057

** $p < 0.05$
Clustered standard errors are reported in parentheses
Source: See Table 4.4

Table D.6: The impact of neighborhood level shocks on county log dropout rate, non-linear shock, 2003-2008: OLS Results with State x Year Fixed Effects

	(1)	(2)	(3)
$LowShock_{it}$	-0.0316	-0.0393	
	(0.0195)	(0.0206)	
$HighShock_{it}$	-0.0449	-0.05	
	(0.0298)	(0.0313)	
Log Population	0.0231	0.082	0.0525
	(0.326)	(0.3917)	(0.3908)
Percentage Non-white	0.63	0.6323	0.6046
	(1.2588)	(1.3433)	(1.3318)
Median Income	7.49e-07	5.90e-07	6.48e-07
	(5.83e-06)	(5.89e-06)	(5.88e-06)
$LowShock_{it-1}$		-0.014	
		(0.0208)	
$HighShock_{it-1}$		-0.0098	
		(0.0224)	
$LowShock_{it-2}$		-0.04	
		(0.0219)	
$HighShock_{it-2}$		0.0182	
		(0.0307)	
$LowShock_{it-3}$		0.0026	
		(0.0322)	
$HighShock_{it-3}$		-0.0543	
		(0.0344)	
4-year Avg. Low Shock			0.1667**
			(0.0702)
4-year Avg. High Shock			0.0272**
			(0.0076)
County Fixed Effect	X	X	X
State x Year Fixed Effect	X	X	X
Observations	19,282	19,261	19,261
R^2	0.09	0.091	0.090

** $p < 0.05$
Clustered standard errors are reported in parentheses
Source: See Table 4.4

Table D.7: The impact of neighborhood level shocks on county log graduation and dropout rate, linear shock, 1996-2004: OLS results

	Graduation Rate			Dropout Rate		
$\%MassLayoffs_{it}$	0.3158	0.2636		-0.8581	3.553	
	(0.1985)	(0.2348)		(5.8183)	(8.7395)	
Log Population	0.1618**	0.2976**	0.2972**	-0.5648	-0.6913	-0.5646
	(0.0346)	(0.0515)	(0.0514)	(2.279)	(2.294)	(2.2725)
Percentage Non-white	-0.0075	-0.1538	-0.1537	2.3944	2.4171	2.3398
	(0.068)	(0.1464)	(0.1464)	(4.8856)	(4.8769)	(4.8685)
Median Income	9.13e-07	-1.83e-06	-1.83e-06	-5.43e-06	-4.81e-06	-5.60e-06
	(9.62e-07)	(1.84e-06)	(1.83e-06)	(3.75e-05)	(3.76e-05)	(3.76e-05)
$\%MassLayoffs_{it-1}$		0.3448			6.1384	
		(0.2429)			(7.1701)	
$\%MassLayoffs_{it-2}$		0.4056			3.6803	
		(0.328)			(7.059)	
$\%MassLayoffs_{it-3}$		0.4399			-2.1304	
		(0.2765)			(5.3927)	
4-year Avg			1.4468			1.9147
$\%MassLayoffs_{it}$			(0.7594)			(23.2027)
County Fixed Effect	X	X	X	X	X	X
Year Fixed Effect	X	X	X	X	X	X
N	24,514	18,442	18,442	4,762	4,762	4,762
R^2	0.009	0.01	0.01	0.002	0.003	0.002

** $p < 0.05$

Clustered standard errors are reported in parentheses

Source: Author's calculations from the Bureau of Labor Statistics (BLS) Mass Layoffs Department, the National Center for Education Statistics, U.S. Census, and the Census Small Area Income and Poverty Estimates

Table D.8: The impact of neighborhood level shocks on county log graduation and dropout rate, linear shock, 2005-2009: OLS results

	Graduation Rate			Dropout Rate		
$\%MassLayoffs_{it}$	0.2093	0.1262		-1.497	-1.8316	
	(0.1836)	(0.1983)		(2.2530)	(2.2736)	
Log Population	0.1176**	0.1202**	0.1202**	0.0237	0.0647	0.0674
	(0.0239)	(0.0303)	(0.0303)	(0.3261)	(0.3916)	(0.3916)
Percentage Non-white	-0.031	-0.085	-0.0848	0.6255	0.5792	0.5801
	(0.0612)	(0.1155)	(0.1155)	(1.2597)	(1.3354)	(1.3350)
Median Income	-2.15e-06	-2.10e-06	-2.09e-06	-1.24e-06	-1.26e-06	-1.44e-06
	(8.24e-07)	(8.53e-07)	(8.33e-07)	(7.58e-06)	(7.59e-06)	(7.59e-06)
$\%MassLayoffs_{it-1}$		-0.4799			1.2939	
		(0.3353)			(2.0692)	
$\%MassLayoffs_{it-2}$		-0.0908			0.3661	
		(0.2229)			(1.7741)	
$\%MassLayoffs_{it-3}$		0.0343			-4.193	
		(0.2287)			(2.2908)	
4-year Avg			-0.3861			-4.6648
$\%MassLayoffs_{it}$			(0.5322)			(5.4538)
County Fixed Effect	X	X	X	X	X	X
Year Fixed Effect	X	X	X	X	X	X
Observations	39,361	33,269	33,269	19,282	19,261	19,261
R^2	0.064	0.057	0.057	0.09	0.09	0.090

** $p < 0.05$

Clustered standard errors are reported in parentheses

Source: Author's calculations from the Bureau of Labor Statistics (BLS) Mass Layoffs Department, the National Center for Education Statistics, U.S. Census, and the Census Small Area Income and Poverty Estimates

Table D.9: The impact of neighborhood level shocks on county log graduation and dropout rate, linear shock, 1996-2009: OLS results with State x Year Fixed Effects

	Graduation Rate			Dropout Rate		
$\%MassLayoffs_{it}$	0.2093	0.1262		-1.497	-1.8316	
	(0.1836)	(0.1983)		(2.253)	(2.2736)	
Log Population	0.1176**	0.1202**	0.1202**	0.0237	0.0647	0.0674
	(0.0239)	(0.0303)	(0.0303)	(0.3261)	(0.3916)	(0.3916)
Percentage Non-white	-0.031	-0.085	-0.0848	0.6255	0.5792	0.5801
	(0.0612)	(0.1155)	(0.1155)	(1.2597)	(1.3354)	(1.3350)
Median Income	-3.61e-07	-5.18e-07	-5.14e-07	6.68e-07	6.75e-07	6.21e-07
	(5.65e-07)	(6.87e-07)	(6.86e-07)	(5.83e-06)	(5.89e-06)	(5.89e-06)
$\%MassLayoffs_{it-1}$		-0.4799			1.2939	
		(0.3353)			(2.0692)	
$\%MassLayoffs_{it-2}$		-0.0908			0.3661	
		(0.2229)			(1.7741)	
$\%MassLayoffs_{it-3}$		0.0343			-4.193	
		(0.2287)			(2.2908)	
4-year Avg			-0.3861			-4.6648
$\%MassLayoffs_{it}$			(0.5322)			(5.4538)
County Fixed Effect	X	X	X	X	X	X
State x Year Fixed Effect	X	X	X	X	X	X
Observations	39,361	33,269	33,269	19,282	19,261	19,261
R^2	0.064	0.057	0.057	0.09	0.09	0.090

** $p < 0.05$

Clustered standard errors are reported in parentheses

Source: Author's calculations from the Bureau of Labor Statistics (BLS) Mass Layoffs Department, the National Center for Education Statistics, U.S. Census, and the Census Small Area Income and Poverty Estimates

Table D.10: The impact of neighborhood level shocks on county log graduation and dropout rate, 1996-2009: OLS results with future mass layoffs

	Log AFGR	Log Dropout	Log AFGR	Log Dropout
$LowShock_{it}$	0.0012	-0.032		
	(0.0024)	(0.0188)		
$HighShock_{it}$	0.0067	-0.0544		
	(0.0035)	(0.0298)		
Log Population	0.2038**	-0.7249**	0.2043**	-0.7376**
	(0.0219)	(0.3381)	(0.0219)	(0.3389)
Percentage Non-white	-0.0906	0.5042	-0.0915	0.5059
	(0.0567)	(1.2752)	(0.0566)	(1.2755)
Median Income	-2.93e-06**	4.44e-06	-2.93e-06**	4.43e-06
	(5.48e-07)	(5.24e-06)	(5.48e-07)	(5.23e-06)
$\%MassLayoffs_{it}$			0.5804**	-2.6337
			(0.1902)	(2.3225)
$LowShock_{it+1}$	0.0025	-0.0211		
	(0.0028)	(0.0196)		
$HighShock_{it+1}$	0.0009	-0.0810		
	(0.0051)	(0.0429)		
$\%MassLayoffs_{it+1}$			-0.0348	-3.0694
			(0.3243)	(2.0398)
County Fixed Effect	X	X	X	X
Year Fixed Effect	X	X	X	X
Observations	39,354	19,279	39,354	19,279
R^2	0.014	0.019	0.014	0.019

** $p < 0.05$

Clustered standard errors are reported in parentheses

Source: Author's calculations from the Bureau of Labor Statistics (BLS) Mass Layoffs Department, the National Center for Education Statistics, U.S. Census, and the Census Small Area Income and Poverty Estimates