THE LABOR MARKET EFFECTS OF THE 1960s RIOTS

by

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Working Paper No. 03-W24

December 2003

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Revised: December 2003

Prepared for the Brookings-Wharton Papers on Urban Affairs

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1. Introduction

Although the United States has experienced race-related civil disturbances throughout its history, those that occurred in the 1960s were unprecedented in their frequency and scope. Between 1964 and 1971, hundreds of riots erupted in American cities, resulting in large numbers of injuries, deaths, and arrests, as well as in considerable property damage that was concentrated in predominantly black neighborhoods. Law enforcement authorities took extraordinary measures to end the riots, sometimes including the mobilization of National Guard units. In retrospect, the riots marked a turning point in American racial politics, as the carefully orchestrated demonstrations of the early Civil Rights Movement gave way to violent, chaotic civil disturbances.

At the time of their occurrence, the riots prompted congressional investigations into their proximate and underlying causes and into their immediate consequences in the form of looting, property damage, injuries, and deaths (U.S. Senate 1967). Subsequently, a large sociology literature developed that attempted to identify city-level correlates of the occurrence and severity of riots (see, inter alia, Wanderer 1969, Spilerman 1971, Lieske 1978, Carter 1986, and Myers 1997). But there have been comparatively few studies of a systematic, econometric nature that examine the impact of the riots on the relative economic status of African Americans, or on the cities and neighborhoods in which the riots took place (Aldrich and Reiss 1970, Frey 1979, Kelly and Snyder 1980, King 2001).[1]

In this paper we study the impact of the 1960s riots in the context of long-term racial disparities in labor market outcomes. Among full-time male workers, the racial gap in average earnings narrowed up to 1975, with periods of sharp convergence (for example, the 1940s) alternating with periods of relative stasis (for example, the 1950s and early 1960s). Since 1970, racial convergence in earnings has slowed markedly,

[1] Aldrich and Reiss (1970) examined the impact of riots in Boston, Chicago, and Washington, DC on small businesses, primarily retail establishments. They find a direct negative impact of the riots through property damage and an indirect effect through higher insurance rates, which created incentives to move or close up shop. Frey (1979) examined the causes of "white flight" in the 1960s in a sample of 39 metropolitan areas. He used the number of riots per 100,000 central city population as an explanatory variable for the city-to-suburb net mobility rate; the estimated effects are positive but weak. Kelly and Snyder (1980) is closest in spirit to our paper. Using city level data, they regress nonwhite family income in 1970 on income in 1960, measures of riot frequency and severity, and other controls. The results are small and statistically insignificant.

and a substantial part of the observed convergence has been driven by the selection of low-income black males out of the full-time labor force (Brown 1984, Chandra 2000). Over the same period, the proportion of blacks living in "high poverty" urban neighborhoods increased sharply (Wilson 1987), and black ghettos turned increasingly "bad" in the sense that residential segregation led to increasingly poor socioeconomic outcomes among young blacks (Cutler and Glaeser 1997, Collins and Margo 2000).

The post-1970 rise in concentrated poverty in black central-city neighborhoods has received a great deal of scholarly attention (see, inter alia, Murray 1984, Wilson 1987, Jencks 1992, Massey and Denton 1993, Sugrue 1996). One prominent view, associated with the work of William Julius Wilson (1987), is that the underlying causes of this adverse trend are essentially macroeconomic in nature. Technological change and the relative decline of manufacturing employment may have reduced relatively high-wage job openings for urban, unskilled workers. The geographic concentration of poverty was then reinforced by the movement of relatively well-off blacks from central-city neighborhoods to suburbs and the proliferation of single-parent households. Another prominent view, associated primarily with the work of Massey and Denton (1993), emphasizes that pre-existing residential segregation and ongoing racial discrimination in housing allowed for the potent endogenous magnification of adverse economic shocks. Feedback between macroeconomic forces, residential segregation, and social norms may have pushed entire neighborhoods into a downward socioeconomic spiral. In the context of the literature on the concentration of black poverty, the hypothesis we are pursuing is that the riots may be examples of such negative shocks.

Like any shock, some of a riot's impact will be felt directly – in this case, by individuals who were immediately affected by the event itself. To some extent, these effects may be offset by private sector responses (for example, insurance payments) or changes in government policies (subsidies or loans to riot-afflicted businesses or infrastructure investment in riot areas). But other, and potentially much larger, effects may be indirect. As discussed in more detail below, a riot might alter the course of a city's economy by influencing the economic decisions of individuals who were not directly affected by the event. In essence, the hypothesis under investigation has two parts: that a riot's effect on African Americans' labor market outcomes was, on net, negative; and that the magnitude of the local effect increased with the

severity of the local riot.

We present two complementary empirical analyses. The first uses aggregate, city-level data on income, employment, unemployment, and the area's racial composition from the published volumes of the federal censuses.[2] After constructing an index of riot severity, we estimate the "riot effect" by both ordinary least squares (OLS) and two-stage least squares (2SLS). The second empirical approach uses individual-level census data from the Integrated Public Use Microdata Series for 1950, 1970, and 1980 (IPUMS, Ruggles and Sobek 1997). We adopt a difference-in-difference framework to compare blacks' labor market outcomes in cities that had severe riots with blacks' outcomes in cities that did not have severe riots, after controlling for a variety of relevant individual characteristics.

The findings, which are broadly consistent across different types of data and estimation techniques, suggest that the riots had negative effects on blacks' income and employment that were economically significant and that appear to have been larger in the long run (1960-1980) than in the short run (1960-1970). We view these findings as suggestive rather than definitive for two reasons. First, the data are not detailed enough to identify the precise mechanisms at work. Second, the wave of riots may have had negative spillover effects to cities that did not experience severe riots; if so, we would tend to underestimate the riots' overall effect.

2. A Brief Characterization of Race-Related Riots in the 1960s

1960s Riots in Historical Perspective

The United States has a long and terrible history of race-related riots. Gilje (1996) documents scores of riots including ante-bellum attacks on free blacks, Civil War draft riots in the North that targeted blacks for abuse, Reconstruction and post-Reconstruction collective violence against southern blacks, and inner-city eruptions during World War I, the Great Depression, and World War II when blacks competed with whites for jobs and housing.

[2] To use 1960 as a starting point, we have to proceed at the city-level because the 1960 public use microdata samples do not identify city of residence.

The riots of the summer of 1943 provide the closest parallel to those of the 1960s. There were close to 50 riots in that year, including one in Detroit in which 34 people were killed (25 of them black). Harlem also erupted in violence, and although the riot was not as severe as Detroit's in terms of fatalities, looting and property destruction occurred on a large and possibly unprecedented scale. 1,500 stores were looted or damaged, virtually all in predominantly black neighborhoods (Gilje 1996, p. 158).

The riots during the 1960s were not unprecedented in their individual severity (measured in terms of deaths), but as a group, their high frequency, wide geographic distribution, and level of destructiveness were unique. The 1960s riots were also historically unusual in that they were characterized by what sociologists termed "black aggression" (though the aggression was rarely directed towards physically harming white civilians), in contrast to most previous significant race-related riots which were characterized by whites attacking blacks.[3]

Measuring the incidence and severity of "race-related riots" requires that one define such an event. Spilerman (1970, 1971) posited an operational definition that has stood as the literature's standard for years. To enter his sample, a riot had to involve at least 30 participants, some of whom were African-Americans engaged in "aggressive" behavior (for instance, looting or property damage); had to occur outside a school setting; and had to be "spontaneous" in the sense that it was not the adjunct of a organized civil rights protest. Spilerman drew on the Congressional Quarterly's *Civil Disorder Chronology* (1967), the Kerner Commission report (National Advisory Commission on Civil Disorders, 1968), an index prepared by the *New York Times*, and the "Riot Data Review" prepared by the Lemberg Center for the Study of Violence at Brandeis University.[4] Each of these primary sources used somewhat different definitions of a riot, collected

[3] There were, of course, instances of violence against white bystanders, police, and shop owners. Nonetheless, the Kerner Commission report asserts: "While the civil disorders of 1967 were racial in character, they were not interracial. The 1967 disorders, as well as earlier disorders of the recent period, involved action within Negro neighborhoods against symbols of white American society – authority and property – rather than against white persons" (1968, p. 110).

[4] The Kerner commission report was preceded by hearings before the Senate's Permanent Subcommittee on Investigations headed by John L. McClellan (U.S. Senate 1967). The Subcommittee's staff identified cities that had experienced riots and then surveyed the mayors of those cities seeking information about the proximate causes and severity of the event. The McClellan data cover the years 1965 to 1967; and, therefore, do not include riots occurring in 1968 (such as those following the assassination of Martin Luther King). It is not clear from the testimony of Robert Emmet Dunne and Crichton Jones, who

different dimensions of data, and covered different time frames.[5] However, with some margin of error, the sources can be combined to document the date and location of each significant disturbance and to construct measures of riot severity.

Carter (1986) extended the Spilerman data to 1971, cross-checked the data with other sources, added new information, and in general, refined the database for subsequent studies. For 1964 to 1971, the dataset includes the dates and location of each riot, as well as the number of arrests, injuries, occurrences of arson, and deaths. We use Carter's data to construct an index of riot severity that is central to our measurement of the riots' effects. Each riot (indexed by j) is assigned a value $S_j = \sum_i (X_{ij} / X_{iT})$ where X_{ij} is a component of severity (deaths, injuries, arrests, arsons, and days of rioting) and X_{iT} is the sum of component X_{ij} across all riots. That is, S_j is the proportion of all riot deaths that occurred during riot j, plus the proportion of all riot injuries that occurred during riot j, plus the proportion of all arrests, and so on. Summed over all riots, there are five total index points (a reflection of the five components that enter the index). We add the index values for each riot within a city to form a cumulative city-level riot severity measure.

The potential shortcomings of the index are clear. Counts of destructive events do not necessarily correspond to economic damage, let alone to a riot's impact on economic agents' expectations. One might argue, for example, that potentially important components are missing from the index, or that given the existing components, some should weigh more heavily than others.[6] Nonetheless, we believe that the index is a useful measure of riot severity for several reasons. First, the individual components of the index are highly positively correlated, and so in practice it matters little if, for example, we treat the proportion of

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collected and organized the Subcommittee's statistics, exactly how the cities were identified, or what criteria were used to determine whether the disturbance was "major" and therefore worthy of inclusion in the study. See Part 1 of the hearings for the testimony, the data, and the survey instrument sent to the mayors.

[5] The McClellan report (see the previous footnote) appears to use the most stringent criteria, with an emphasis on high levels of violence (number of deaths), involvement of law enforcement (number of arrests), and destruction of physical property (looting, arson), while the Lemberg Center used the loosest criteria. Unfortunately, the data collected by the Lemberg Center do not overlap (in terms of timing) those collected by McClellan or Kerner, since they start in 1968 and end in 1971. The *Times* index essentially replicates the material found in the other sources.

[6] Unfortunately, consistent value-based measures of property damage do not exist.

deaths as "more important" than the proportion of injuries.[7] Given the rather high correlations among observable measures of severity, one might reasonably expect that they are well-correlated with unobservable components as well. Second, any alternative choice of weights would necessarily be as ad hoc as our choice of equal weights.[8] Third, to conserve degrees of freedom, to facilitate instrumental variable estimation, and given the components' positive correlation, the use of an index is far more practical than entering each component separately in the regressions.

Table 1 summarizes each component of the index by year, and also reports the overall index by year and census region. The most obvious aspect of the data is the strong concentration of riot activity in 1967 and 1968, which together account for 3.3 out of the 5.0 total index points. When the index numbers are arrayed by census region, there appears to be a comparatively even geographic spread of riot activity, with the Midwest (1.57) and South (1.53) outpacing the Northeast (1.11) and West (0.79).[9] This impression is true in the sense that major riots occurred in every region, but it is misleading because the "severity" was heavily concentrated in a relatively small number of events (and cities), not spread evenly over them. For example, no deaths occurred in 91 percent of the 752 riots underlying table 1, and just six riots account for nearly 60 percent of the total number of fatalities (228). By far, the most deadly riots were in Detroit in July 1967 (43 deaths); Los Angeles in August 1965 (34 deaths); and Newark in July 1967 (24 deaths).[10] Using the index as a broader measure of severity, the riot in Washington following Martin Luther King's assassination (S = 0.34) would join Los Angeles in 1965 (0.48), Detroit in 1967 (0.44), and Newark

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[7] Nonetheless, we do note some results from regressions run using deaths as the only measure of severity. The correlations among deaths, arsons, arrests, and injuries across riots are high: at least 0.64 (deaths and injuries) and as high as 0.87 (deaths and arsons). Correlations of these variables with days of riots are somewhat lower, ranging from 0.32 to 0.48. All correlations are statistically significant at the one percent level. Later in the paper, we sum S_j over riots within cities for the city-level measures of riot severity. To test the robustness of this index, we created five alternative indices, each of which omitted one of the observed severity components. The resulting indices were highly correlated with one another and with the base index used here, with a correlation coefficient ranging from 0.96 to 0.99.

[8] Our measure of riot severity is "absolute" in the sense that we do not scale severity by population; however, our city-level regressions control for population directly or indirectly in the IPUMS regressions when we include area fixed effects (see below).

[9] Washington DC and Baltimore, which had sizable riots, are counted in the census South.

[10] The other three were in Washington, DC in April 1968 (11 deaths); Cleveland, July 1968 (10 deaths); and Chicago, April 1968 (9 deaths).

in 1967 (0.23) as the most severe events on record. Fully 90 percent of the riots receive index values of less than 0.01. As discussed in detail below, the inter-city variation in riot severity will play a key role in our empirical strategy for measuring the riots' effects on labor market outcomes.

Causes of the Riots

The occurrence of the riots at a time when, at the national level, blacks' economic prospects were improving belies any simple causal connection running from economic status to riot severity. Although post-riot government reports and journalistic accounts are replete with speculation and anecdotal evidence, the causes of the 1960s riots became a major research topic in sociology in the early 1970s. The point of departure for nearly all subsequent academic work was a series of papers by Spilerman (1970, 1971, 1976) which estimated multivariate models of riot incidence and severity. In this work, the unit of observation was the city, and the independent variables were drawn from the 1950 and 1960 federal censuses and related government documents. Spilerman's principal finding was simple: the absolute size of the black population (positively correlated with riots) and southern location (negatively correlated with riots) were the best predictors (in a statistical sense) of the incidence and severity of the riots. He found little support for a wide range of other seemingly plausible explanatory factors, including a variety of indicators of blacks' absolute and relative (to whites) economic status.[11]

Thus, taken literally, Spilerman found that conditional on black population size and region, severe riots were essentially idiosyncratic events. The chronologies of specific riots suggest this is not as far-fetched an interpretation as it might at first sound. In many (perhaps most) cases, there were identifiable, idiosyncratic "sparks" that, through a series of unforeseen complications, turned a minor altercation into a full-blown riot. The spark might be an encounter gone wrong between a black motorist and the police (as in

[11] Spilerman and subsequent authors relate riots to socioeconomic conditions in 1950 or 1960 census data. It is unknown whether short-term movements in socioeconomic conditions (e.g., between 1960 and 1965) would be more useful predictors of riot activity. Recently, Chandra and Foster (forthcoming) reported state-level evidence suggesting a complex relationship between riot occurrence and the residual wage gap between blacks and whites (after accounting for observable differences in human capital).

Watts), or an impromptu, incendiary speech by activist H. Rap Brown (as noted in a congressional report).[12] The most incendiary event was surely the assassination of Martin Luther King in April 1968, after which more than 100 riots erupted.

Subsequent research has modified Spilerman's work by improving the quality of the riot data, by using event history analysis, by introducing co-variates that were not available to (or not considered by) Spilerman, and by extending the time frame under study (Lieske 1978; Carter 1986, 1990; Olzak et al. 1996; Myers 1997, 2000; DiPasquale and Glaeser 1998; Chandra and Foster forthcoming). But nearly all of the "second generation" studies confirm Spilerman's original finding that black population size and region are the most consistent and quantitatively important explanatory variables for riot incidence and severity in the 1960s.[13]

Most recently, Myers (1997, 2000) has found that contagion played a role in determining the geographic pattern of riots. Riots were given extensive television news coverage, suggesting one mechanism (not the only one) by which an outbreak of violence in one city might spill over to another, especially if they shared the same media outlets. This contagion effect appears to have waned quickly over time. The occurrence of a second (or higher order) riot also appears to have been more likely following an initial disturbance, though within cities, riots declined in severity over time (Spilerman 1976). The sociological studies cannot rule out the possibility of underlying city-specific causes, but it is clear that matching the events to observable city-level correlates, beyond location, black population size, and proximity to other riots, is extremely difficult.

3. How Might Riots Affect Labor Markets?

In this section we sketch a simple framework describing the potential labor-market effects of a riot. Our model supposes that people and businesses choose locations that maximize utility and profits

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[12] The Senate hearings on Riots, Civil, and Criminal Disorders (1967, part 1) includes a table describing "major riots", including their "triggering incident".

[13] As we point out in section 4, the form of local government may have mattered in determining the occurrence and severity of riots, along with weather. Spilerman considered the former, finding some evidence of effects, but did not consider the latter, at least not in his published work.

respectively. Household utility is a function of the benefits and costs associated with inhabiting a particular space. The benefits come in many forms: access to local public goods (schools, churches, entertainment, and so on), proximity to one's place of work, and proximity to one's friends and family. The costs include rent or mortgage interest payments, property upkeep and insurance, and taxes. For businesses, the benefits derive from the flow of revenue associated with the location, which in turn might depend on demand from local residents or on proximity to other businesses. The costs derive from rent or mortgage interest payments, labor costs, property upkeep and insurance, and taxes. We suppose that in the short run, movement is inhibited by fixed costs associated with "starting over" in new locations.

The occurrence of a riot may have direct and indirect effects on the level and location of economic activity. The direct effects are experienced by individuals whose connection to the riot is immediate: an injured rioter, a resident whose home is in the line of fire, a business owner whose establishment is torched or looted, and so on. For some individuals, the direct effects are irreversible (obviously, for anyone who is killed), but for others, the effect may be transitory, depending on subsequent decisions made in light of changed perceptions of the economic environment.

Consider, for example, a business owner whose establishment is damaged or looted and, therefore, is temporarily shut down. Whether or not the business reopens depends on the expected benefits and costs of doing so at that particular location relative to all others (and relative to staying permanently closed and putting the remaining capital to some other use).[14] For some, the costs of rebuilding or restocking may be covered by insurance or public subsidies, but others may be uninsured or ineligible for assistance. Looking forward, the expected costs of operation in that location may increase after the riots. There may be higher insurance costs (see Aldrich and Reiss 1970, Bean 2000), expenses from the installation of additional security features (fire and burglar alarms), higher interest rates on small business loans, and higher taxes to

[14] A business may be viable in the short run – that is, with its capital stock fixed – but only because it can cover variable costs, not because the rate of profit is "normal" in the long run. Even if the costs of rebuilding are covered by insurance or other means and the costs of operation do not rise, it may not pay to re-open if, prior to the riot, the business was not economically viable in the long-run sense. Some such owners may relocate elsewhere in the city, but others may leave entirely, taking whatever capital remains with them.

pay for redistribution programs or an increased police presence.[15] At the same time, the expected benefits of being in that location might fall, especially if the firm's revenue depends heavily on business from nearby firms or residents.

If only direct effects come into play, the labor market implications of a riot might be small and short-lived, especially when viewed at the city level. Even at their worst, the 1960s riots never directly involved vast numbers of people or vast amounts of capital. For example, total property damage during the Detroit riot in July 1967 was approximately $50 million dollars, a small share of total property value in the city. The great majority of Detroit's 500,000 black residents at the time had no direct involvement in the riot (Widick 1989, p. 167). The worst of the direct effects were borne by the residents and businesses located in the general vicinity of 12th Street, where the riot originated after a police raid on a "blind pig" (an after-hours drinking establishment).

However, even if the direct effects are limited, a riot's ultimate economic impact may be magnified through endogenously propagated indirect effects that unfold over a longer period of time. After a riot, firms and residents might revise their expectations of the benefits, costs, and risks of locating in or near a particular central-city neighborhood even if they were not directly affected by the riot. If some residents leave and firms close due to the initial shock (and are not replaced instantaneously), local economic activity and employment may slacken. The web of potential knock-on effects is extensive: personal income and local business revenues may fall; local sources of tax revenue may diminish; the area may experience a rise in crime and a decline in publicly provided services; and declining rents and property values (and perhaps the out-migration of the relatively well-off) may exacerbate the concentration of poverty in inner city neighborhoods. Along the lines of Wilson (1987) and Massey and Denton (1993), the idea here is that a process of negative decline may reinforce itself and may be concentrated in predominantly black

[15] There were reasonable grounds for expectations of higher taxes for redistribution and police. For example, the Governor's Commission Report on the Watts riot made three "high priority" recommendations: 1) "cooperative programs" with businesses for the training and employment of blacks; 2) "a new and costly approach to educating the Negro child"; and 3) increased police efforts regarding crime prevention and community relations (1965, p. 8). Systematic evidence on the extent to which such programs were actually undertaken is scarce. See Hahn (1970) for some discussion of the issue.

neighborhoods.

The downward spiral could continue, in theory, until the location is entirely abandoned, with all workers and capital relocating elsewhere. In practice, due to the large stock of immobile residential capital (Glaeser and Gyourko 2001), nontrivial relocation costs, and perhaps government efforts at revitalization, the spiral may eventually arrest itself. But the new labor market equilibrium may differ significantly from the initial equilibrium. The central city may have fewer employed workers who earn lower wages than before (essentially reflecting a leftward shift of labor demand), and it may have fewer high human-capital residents (reflecting relocation to the suburbs or other cities).[16] Relatively poor central-city blacks may be especially unlikely to relocate from adversely affected areas due to a variety of labor, housing, and credit market imperfections, including, but not limited to, racial discrimination.

In sum, a riot could lead to a decline in economic activity due to the destruction of physical capital, a rise in costs of production, and a decline in perceived security. Although it is difficult to be precise about the functional form, it seems reasonable to hypothesize that the net effects, if any, are increasing in the severity of the riot. Because the riots were concentrated in central-city black neighborhoods, it also seems reasonable to hypothesize that the effects, if any, were felt most strongly by central-city black residents. In predominantly black neighborhoods, those with the most capital at stake and those facing the highest potential tax burdens would have the greatest incentive (and ability) to relocate. White central-city neighborhoods might lose residents and businesses as well, not because of direct physical destruction of property, but because of changes in expected taxes, security costs, and public services. Again, those with the most capital at stake and those with the most taxable resources would have the greatest incentive to depart for the suburbs or, perhaps, other cities.

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[16] With perfectly mobile labor, a leftward demand shift in the central city would be followed immediately by out-migration such that wages would equalize across locations. With imperfectly mobile labor, any such response would take time, and wages would be depressed during the period of adjustment. If the skill mix of the central city changes (due to selective out-migration), observed wages will be lower in the new equilibrium. Blanchard and Katz (1992) explore the dynamics of adjustments to labor demand shocks at the state-level in the 1978-1990 period. In their model, a decline in wages in one location (relative to wages elsewhere) attracts firms seeking to minimize labor costs. If so, wages and employment tend to move back toward their initial levels. Empirically, they find that this job-creation mechanism is not strong and that out-migration plays a key role in adjusting to the shock.

4. Empirical Framework

We use variation across cities in the severity of the riots to estimate their impact on African-Americans' labor market outcomes. The validity of this strategy depends on two assumptions. First, we assume that the riots' effects were concentrated in the cities that actually experienced riots. In theory, however, it is possible that very well integrated labor markets could dissipate adverse shocks quickly, leaving no trace of a wage effect in cross-city comparisons (though possibly leaving evidence of migration). Also, if the riots had strong inter-city spillover effects on perceptions of the benefits and costs associated with central-city locations, then cross-city comparisons would tend to understate the riots' impact. The second major assumption is that the geographic distribution of riots was exogenous to blacks' economic status prior to the riots. We have already discussed the sociology literature's findings in this regard. If the riot variation is essentially random, then estimation by ordinary least squares is straightforward. Nevertheless, we report estimates for specifications that control directly for pre-1960 trends in labor market outcomes. We also relax the riot exogeneity assumption by pursuing two-stage least squares estimates.

Our analysis draws on two sources of information regarding labor market outcomes: city-level data from the published census volumes, and individual-level data for metropolitan areas from the IPUMS (Ruggles and Sobek 1997).[17] The published city-level data are particularly useful in this case because the IPUMS sample for 1960 does not identify cities and because, in the 1970 and 1980 samples, metropolitan area or central-city status is undisclosed for some households. The main advantage of the individual-level data is that one can observe and control for a variety of individual and household characteristics. For now, we restrict our attention to changes in income, employment, unemployment, and the racial composition of city populations.

City-Level Approach

[17] In future work, we intend to examine the CPS samples, which contain data on an annual basis, though for a comparatively small set of cities. We also have begun to match maps of the riot areas to maps of census tracts for a handful of cities, which will allow us to follow specific neighborhoods over time.

We focus on cities with total populations of at least 100,000 and black populations of at least 1,000 in 1960, providing a base sample of 130 cities.[18] We also exclude cities that, according to the relevant issues of the City and County Data Book, had large changes in boundaries during the period under study. For each city, we summed the index values for each riot (as defined above) that occurred between 1964 and 1971. In the regressions below, the index is first entered in quadratic form as an explanatory variable for changes in black labor market outcomes. Then, to simplify the analysis, we split cities into two groups, "severe" and "not severe", on the basis of the index values.[19] A relatively small number of cities fall into the "severe" group, but they account for the overwhelming majority of deaths, injuries, arrests, and arsons in the sample. For example, 77 percent of the deaths in the base sample occurred in the "severe" riot cities. On the basis of the index, the severe riot cities are: Los Angeles, Detroit, Washington, Newark, Baltimore, Chicago, Cleveland, New York, Mobile, and San Francisco.

In the simple "severe dummy variable" specification, let *y* stand for an economic outcome and *S* stand for the severity of the 1960s riots (*S* = 1 if riots were "severe"), and consider the following two regression equations:

$$\{\text{eq. 1}\}: \Delta y_{1970\text{-}1960} = X\beta + \gamma S + e$$

$$\{\text{eq. 2}\}: \Delta y_{1980\text{-}1960} = X\beta + \gamma S + e$$

The *X* vector includes a set of city-level characteristics such as region indicators, the manufacturing proportion of employment in 1960, black population size in 1960, and total population size in 1960. The treatment group consists of blacks living in cities for which S = 1, and the control group consists of blacks living in cities for which S = 0.[20] Essentially, this is a difference-in-difference (DD) estimator in which time-invariant city-specific effects, region-specific trends, and city-invariant period-specific effects are differenced

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[18] In 1960, 83 percent of blacks in cities with at least 25,000 residents lived in cities with at least 100,000 residents.

[19] A city's index is considered to be "severe" (= 1) if the index value falls into approximately 90th percentile (or higher) of the distribution of severity. See the text for a list of the severe riot cities

[20] Data are not reported for black income and unemployment for 1960. Rather, the data pertain to the nonwhite population. For most cities, the black and nonwhite proportions of the population in 1960 are very similar. The results are similar if we exclude cities with substantial fractions of nonblack, nonwhites.

out, and identification comes from differences in changes in *y* across the two groups of cities (conditional on *X*).[21] In equation 1, γ is a rough measure of the "short-run" impact of a severe riot; that is, the effect (if any) in the census closest in time to the period of the riots (1970) relative to the "pre-riot" census (1960). Equation 2 measures the "long-run" impact. Since many of the "not severe" cities did have small riots, γ should be interpreted as the effect of severe riots over and above any effects associated with small riots (as opposed to the effect of a severe riot relative to a no-riot counterfactual).

In general, unobserved trends and shocks that are correlated with the occurrence of "treatment" threaten the credibility of difference-in-difference estimators. Controlling for observable economic characteristics, perhaps including the 1950 to 1960 trend in labor market outcomes, may reduce the scope for bias from unobserved shocks and trends – the idea being that cities similar on observables may be similar on unobservables as well. Alternatively, instrumental variables may help isolate variation in riot severity that is plausibly exogenous to unobserved labor market shocks and trends.

One possible set of instrumental variables derives from differences in city government structure. The sociology literature suggests that differences in governmental form may have implied differences in responsiveness to the political interests of the local black population and therefore differences in the likelihood and severity of riots (see Lieberson and Silverman 1965; Spilerman 1971, 1976). Along these lines, many riots were preceded by a series of racial incidents spread over a period of weeks, and it is possible that some governmental forms responded more effectively to alleviate the building tension. We use a dummy variable indicating the presence of a city manager to help predict the incidence of riots even after

[21] In principle, using white outcomes as an additional level of control, one could pursue a difference-in-difference-in-difference (DDD) estimator. One might hope that this third "difference" would absorb race-invariant city-specific shocks and trends, but because it is highly plausible that whites in riot-cities responded in some way to the "treatment" event, it is difficult to justify using whites as an additional control group. Moreover, the 1960 census volumes do not report white-specific outcomes at the city-level. We can measure black outcomes relative to the overall city outcomes, but this is highly imperfect because blacks were a substantial proportion of many cities' populations. Backing out figures for whites would be possible if (for example) city-wide and nonwhite average incomes were reported, but in fact, the tables report medians. In the next section, we do report some DDD estimators using the IPUMS data.

controlling for black population size and region.[22] We take the position that the government's structure is unlikely to alter black economic outcomes directly, and therefore, is a legitimate instrument. In this regard, there is no evidence of significant correlations between the city-manager variable and pre-existing trends (1950-1960) in nonwhite income, employment, or unemployment.[23]

Our second instrumental variable strategy is to make use of weather data for April of 1968. The idea is that a specific, identifiable event – the assassination of Martin Luther King – greatly increased the likelihood of a riot during the month. However, in places where the weather was unfavorable, and in particular in places where it rained, riots may have been less likely or less severe.[24] Although, as far as we know, rainfall has not been considered in the sociology literature on the 1960s riots, there is anecdotal evidence that rain has dampened or precluded political protests and civil disturbances at various times and places. For example, in his discussion of "the riot that didn't happen," Sidney Fine notes that rainfall played a key role in defusing an emerging riot in Detroit in August 1966 (1989, p. 140). Most recently, after two nights of riots, a CNN.com headline on June 19, 2003 read "Rain, curfew help bring quiet night to Benton

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[22] Additional governmental characteristics such as the use of non-partisan elections and the proportion of the city council that is elected at large made little contribution in the implicit first stage of the instrumental variable procedure. The sociology literature tends to argue that, in theory, mayors may be more responsive to minority needs than city managers, but the evidence does not support that hypothesis. Spilerman (1976) finds a positive correlation between mayors and riots; Eisinger (1973) finds a positive correlation between mayors and black protest activity; and we find a significant negative correlation between riot severity and city managers. The negative correlation is consistent with the view that use of city managers led to greater "professionalism" in local government in general, and police better prepared to deal with civil disturbances in particular, but we admit that this is pure speculation.

[23] Regressing the 1950-1960 change in nonwhite family income, change in nonwhite unemployment rate, and change in nonwhite employment rate on the city manager variable yields the following coefficients: -0.047 (t-stat = 0.58) for income; -0.004 (t-stat = 0.46) for unemployment; 0.003 (t-stat = 0.21) for employment. The samples are similar to those in tables 2A, 3A, and 4A, and control for region, black and total population size, and manufacturing employment.

[24] Riot activity was very high in the two weeks after the assassination, but even later in the month, riot activity was substantially higher than in previous Aprils (1964-1967). So, we used rainfall for the entire month. When we limit the "rain window" to ten days after the assassination, we get broadly similar results. The Kerner Commission did note that several cities with substantial riots in 1967 appeared to have relatively high temperatures around the time of the riots (1968, p. 123). This observation has been supported in statistical analyses by Baron and Ransberger (1978) and Carlsmith and Anderson (1979). There is a more general criminology literature that links temperature and violent crime (e.g., see Field 1992). We may attempt to use temperature variation as a second weather instrument in future work.

Harbor".[25] The instruments hold up well in the implicit first stage of our two-stage least squares estimates: city managers and more rainfall in April of 1968 are consistently associated with a lower values of S.[26]

City-level Results

To estimate the effect on median family income, we use both a "broad" and a "narrow" sample. The broad sample includes all available cities for comparison. The narrow sample uses a smaller set of comparison cities for which the 1950 IPUMS can be used to establish a pre-1960 trend in nonwhite income. Table 2A reports estimates of the riots' effect on the change in median black family income between 1960 and 1970. The first three columns all use the raw index numbers for severity (in quadratic form), whereas the last six columns use a dummy variable for severe riot cities. The last three columns are two-stage least square estimates of the riot impact, relying on the instrumental variables discussed above. Columns 3, 6, and 9 all include controls for the 1950-1960 trend in black family incomes for a reduced set of cities. To help distinguish the influence of changing sample composition from that of changing specification, columns 2, 5, and 8 exclude the trend variable but use the reduced sample of cities.

Column 1's results indicate that riots were associated with slower income growth for blacks through the relevant range of the riot index (0 to 0.5). The coefficients suggest a maximum negative impact around a riot index value of 0.3 (approximately the center of the "severe riot" range). At that point, the estimated negative riot effect on median black family income is over 12 percent. Moving to the smaller sample (column 2) and adding the 1950-1960 trend (column 3) has a small impact on the estimated riot effect – the profile becomes steeper but still reaches a maximum impact around 0.3, at which point the riot effect on income is almost negative 16 percent. The significant positive coefficient on the *South* dummy variable reflects the convergence of southern blacks' incomes on the incomes of blacks elsewhere.

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[25] (http://www.cnn.com/2003/US/Midwest/06/18/michigan.unrest/).

[26] In the implicit first stage of IV estimates below (with 102 cities), the April 1968 rainfall coefficient is -0.033 (t-stat = 2.35), and the city manager coefficient is -0.12 (t-stat = 2.13). The regression also includes region dummies, black population size, total population size, and manufacturing proportion of employment 1960. The F-statistic for the joint significance of rainfall and city manager is 3.7. Key results are similar when estimated using limited-information maximum likelihood.

Qualitatively, the results are similar in columns 4 to 6 which replace the quadratic severity index with a simple dummy variable for cities that had the most severe riot experiences. In each column, the estimated riot effect on black income is about -0.09 and statistically significant. The 2SLS point estimates for the riot effect (columns 7 to 9) are larger in magnitude and somewhat weaker in terms of statistical significance (though still significant at the 10 percent level at least). All of the results in table 2A are consistent with a non-trivial negative effect of riots on black income.

Table 2B reports results for similar regressions, run for the 1960 to 1980 period. The results suggest that the riots' effects were not transitory. Cities that experienced riot-associated relative declines in income during the 1960s did not catch up during the 1970s. The coefficients in columns 1 to 6 are roughly similar in magnitude to those from table 2A, but the estimated riot-effect profile is somewhat steeper in the quadratic specifications of columns 1 to 3 (reaching an impact of -0.22 in column 1 at a riot index value of 0.3). Likewise, column 4 estimates an average "severe riot city" effect of about 12 percent (compared to 9 percent in table 2A), and again, the 2SLS coefficients increase in magnitude, as do the associated standard errors.

We have checked the robustness of the basic income results (columns 1 and 4 of tables 2A and 2B) in several ways: we limited the sample to cities with at least 50,000 black residents in 1960 (essentially, a large city sample); we split the sample into southern and non-southern cities and ran separate regressions; and we replaced the quadratic severity index with a quadratic in the number of persons killed in riots. In each case, the negative and statistically significant (or nearly so) association of riots with declines in median black family income persists.[27]

The true nature of these apparent relative income declines is difficult to discern from the city-level data. Two distinct, but not necessarily exclusive, hypotheses suggest themselves. First, the riots could have negatively affected the labor market outcomes of people residing in the riot cities throughout the 1960s and 1970s. Second, relatively high income blacks could have moved out of riot cities after their occurrence, leading to a decline in average income of those remaining (a compositional change). Though imperfect, we

[27] These results are available from the authors on request.

can use measures of median schooling levels for adult black (or nonwhite) males in 1960 and 1970 to see if a compositional change drives the observed results. Adding this change in average schooling to the regressions of columns 1 or 4 in table 2A (not shown) does not undercut the estimated riot effect, though it does have a positive, independent relation to income changes. Furthermore, regressing the change in education on measures of riots severity, region dummies, and so on, suggests that there was no significant difference in the change in education levels between the severe riot cities and others. Thus, the compositional story does not appear to drive the negative income results, at least for the 1960s. A third potential avenue would be for family units (over which median income is measured) to disintegrate faster in the riot cities than elsewhere; in particular, a relative increase in the proportion of female headed households could drive a relative decline in the census measure of median family income. The published city-level data are not consistently detailed enough to test this hypothesis, but we intend to explore the issue in future research.

Tables 3A and 3B report estimates of the effect of riots on male unemployment rates.[28] Since black (or nonwhite) unemployment figures are available for 1950 for all the cities, there is no change in sample composition across the columns. Columns 1, 4, and 7 control only for region; columns 2, 5, and 8 add variables for total and black population size and the proportion of employment in manufacturing; and columns 3, 6, and 9 include the 1950-1960 unemployment trend as an independent variable. There is no strong evidence that riot severity affected black male unemployment rates, and in fact, most of the point estimates suggest a relative decline in measured unemployment rates (though not statistically significant).

Tables 4A and 4B repeat the exercise for male employment ratios (defined as the ratio of employed males over the male population above a particular age).[29] Here, we find little evidence of a short-run effect

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[28] We would like to estimate the effect on employment rates calculated over the adult population (rather than unemployment rates calculated over the labor force), but the published city-level data do not facilitate that approach.

[29] The 1960 published census data reports the number of men over age 14 (the denominator for the employment rate) whereas the 1970 and 1980 censuses report the number over age 16. Since most 15 and 16 year olds are not employed, the shift in the denominator's definition tends to cause an understatement of the magnitude of the decline in black male employment over time. There is no reason to believe that this biases the estimated riot effect.

(between 1960 and 1970), especially when we include a pre-existing trend. However, the long-run effect is strongly negative even when controlling for the pre-existing trend: measuring between 4 and 8 percentage points in columns 4 to 6. Like the results for median income, the 2SLS coefficients are larger (but less precisely estimated) than the OLS coefficients. Thus, even though variation in the city-level black male unemployment rate is weakly correlated with riot severity (table 3B), it appears that between 1960 and 1980 there were especially large declines in black male employment rates in cities with severe riots (table 4B). Further evidence on the riots' effect on employment is gleaned from the IPUMS data in the paper's next section.

Table 5A reports estimates of the riots's impact on the black share of total city population during the 1960s. Controlling only for region (columns 1, 4, and 7), it is clear that cities that had riots had comparatively large increases in the share of the city population that was black. The differences are large and statistically significant, reaching a maximum impact of about 0.11 in column 1 and an average "severe riot city" effect of 0.07 in column 4. As more variables are added to the regressions, however, the estimated riot effects diminish in magnitude: the maximum impact suggested by column 3 (after accounting for 1950-1960 trend) is only about 0.03, and in column 6 is smaller yet. After accounting for the 1950-1960 trend in black population share, the 2SLS estimate in column 9 yields a very small (and imprecise) point estimate. Qualitatively, the results from table 5B, which cover 1960 to 1980, are quite similar to those in 4A: Severe riots were strongly positively correlated with increases in black population share, but those increases were apparently well underway before the riots occurred.[30]

Micro-Level IPUMS Data

Our second empirical analysis uses individual-level data for men living in metropolitan areas from the 1950, 1970, and 1980 IPUMS. Unfortunately, metropolitan areas are not disclosed in the 1960 sample,

[30] Similar regressions run for the size of city population return somewhat similar results. Cities with severe riots lost population relative to those that did not (by about 8 percent during 1960s, controlling only for region), but as more control variables are added, the negative population effect tends to diminish. Because cities change geographic size in non-trivial ways over time, we view the "total population" results as highly speculative.

and it is not possible to observe both metropolitan area and central-city status simultaneously in the available 1970 samples. Together, these limitations imply that we cannot make micro-level comparisons between 1960 and other years, and we cannot limit the scope to central-city black residents. Despite several differences between this second analysis and that of the previous section, the basic identification strategy is the same: to compare changes in labor market outcomes in areas that had severe riots with areas that did not.

The regressions are estimated by OLS, and take the following basic form, where Y is an individual's labor market outcome (income or employment), and S is a dummy variable equal to one for metro areas that had severe riots.[31]

$$\{\text{eq. 3}\}: Y = \alpha + \beta_1 X + \beta_2 S + \beta_3 \text{Year} + \beta_4 (S \times \text{Year})$$

In this specification, β_4 measures the average change in *Y* for black workers in the severe riot cities relative to blacks in non-severe riot cities, after accounting for differences in personal and regional characteristics (*X*). The X variables include age (quartic), education (quadratic), marital status, migrant status (dummy for those residing in state that is different from birth state, dummy for foreign born), region or residence indicators (four regions), and region interacted with the year dummy (allowing for region-specific trends). We compare 1950 with 1970 in one set of regressions, and we compare 1970 and 1980 in a second set (*Year* = 1 in the later year of the comparison). It is possible to include metro-area fixed effects in these regressions. Doing so makes it impossible to identify coefficients on any time-invariant city characteristics, but β_4 is still identifiable.

We also report difference-in-difference-in-difference (DDD) estimates which use white workers to provide another layer of comparison. We do not rely heavily on the DDD estimates because whites might not form an effective control group to the extent that they too were affected by or responded to the riots, but the perspective is nevertheless still of interest. The DDD estimates indicate whether blacks in severe

[31] The list of "severe" cities in previous section and "severe" metro areas in this section match up in a straightforward way (though obviously metro areas are larger than cities). One exception is that Philadelphia ranked high in the metro-area summation of the riot index even though the city proper did not rank high in the city-level summation. We treat the Philadelphia metro area as a "severe riot" area. Excluding Philadelphia from subsequent regressions has little effect on the results.

riot cities fared worse relative to whites in severe riot cities than blacks in non-severe riot cities fared relative to whites in non-severe riot cities.

Panel A of table 6 reports results for log annual income among male workers who were between 18 and 64 years of age, were not in school, and worked at least 40 weeks in the relevant year. Column 1 suggests that blacks in severe riot cities lost some ground (about two percent) relative to blacks living in other cities between 1950 and 1970, though the decline is not statistically significant. The addition of city fixed effects in column 2 has little impact on the coefficient, indicating that unobserved city-specific fixed factors did not drive the result. The estimates in columns 3 and 4, using a DDD approach, are nearly identical to those in columns 1 and 2, suggesting that the relative decline of blacks in severe riot cities (relative to blacks elsewhere) was not matched by a similar decline among whites (relative to whites elsewhere). The point estimates for the 1970s (columns 5 and 6) are similar in magnitude, but more precisely estimated, suggesting a relative decline of 2.5 to 3.0 percent in blacks' annual income in the riot cities. These losses are smaller, but still negative, when differenced by white income trends in columns 7 and 8.

Conditioning the sample on those who worked at least 40 weeks makes a significant difference to the magnitude of the estimates for income changes in the 1970s.[32] Removing the "working" condition entirely and re-running the regression in column 5 with all men 18 to 64 (and not in school) results in a severe-riot coefficient of -0.085 (t-stat = 5.12). Including only men who were in the labor force (regardless of weeks worked) results in a severe-riot coefficient estimate of -0.065 (t-stat = 4.04). (Regressions are not in table.) These findings suggest that there were relatively poor employment prospects in severe-riots during the 1970s, and we explore this possibility directly in panel B of table 6.

The employment regressions show no decline in the likelihood of employment for black men between 1950 and 1970 in severe riot states; if anything, the likelihood appears to have risen. There is evidence, however, of a 3 to 4 percentage point decline in the employment rate of black men in severe riot

[32] The condition does not appear to make a significant difference for the 1950 to 1970 income results.

cities during the 1970s. This decline is consistent and statistically significant across columns 5 through 8. Moreover, the decline appears to be stronger among younger black workers: the coefficient in columns 5 increases in magnitude to 4.5 percentage points when the sample is limited to men under 40 years of age, and to 6 percentage points when limited to men under 30.

The declining employment rate reflects both a decline in labor force participation and a rise in unemployment relative to blacks elsewhere. Regressing labor force participation (=1 if in labor force) on the same set of variables as in column 5 returns a riot coefficient of -0.021 (t-stat = 2.83). Limiting the sample to those in the labor force, and using unemployed status as the dependent variable yields a coefficient of 0.022 (t-stat = 2.69).

5. Conclusion

In the 1960s, the United States experienced a large number of race-related civil disturbances. Although social scientists have long studied the riots' causes, the riots' consequences have received much less attention. The riots were concentrated in neighborhoods that were predominantly African-American, and in theory, they may have depressed the relative economic status of some African Americans through a downward spiral in neighborhood employment opportunities, property values, and peer quality. Measuring such effects is difficult, in large part because the riots may have been responses to unobserved forces that also simultaneously influenced labor markets. Nonetheless, given Spilerman's contention that the distribution and severity of riots were essentially random (conditional on black population size and region), and alternatively, given the scope for instrumental variable estimation that isolates plausibly exogenous variation in riot severity, we believe that a solid measurement of riot treatment effects is within reach.

Thus far, the empirical evidence on the riots' effects on African Americans' labor market outcomes is mixed, but highly suggestive and worthy of further exploration. Our examination of data from the published census volumes and the IPUMS samples suggests the existence of adverse riot effects on family income and male employment. For example, controlling for region and city characteristics, median family income declined significantly after 1960 in the cities that had severe riots relative to those that did not. A

relative decline in annual income for males is also apparent in the micro-level data spanning the 1970s, especially when the sample is widened to include men who worked relatively few weeks. The micro-level data also reveal a relative decline in the likelihood of male employment and labor force participation during the 1970s in the riot cities. Since all of these effects are measured by comparing blacks' outcomes across cities, and since it is possible that there were negative spillovers from the severe riots to other cities, our estimates of the riots' effects might be understated.

At this stage, our empirical results should be viewed as highly tentative. In addition to considerable refinement to the data and analysis presented in this paper, we plan to extend our analysis to other economic outcomes, especially those related to housing markets, such as housing values and residential segregation (see Collins and Margo 2003). We also intend to explore other sources of data. In particular, the analysis in this paper has looked for effects using city-level measures of riot severity on rather widely spaced (in a temporal sense) census data. It seems clear from our preliminary analysis that riot effects exist, but they are difficult to tease out of the data. In this regard, one extension would be to explore the public use samples of the March Current Population Survey (CPS), which become available annually in the mid-1960s. While the CPS does not identify the city of residence for all individuals, it does identify the major cities, some of which had severe riots. Another strategy is to look for effects at a smaller geographic scale. We are in the process of matching maps of riot activity within cities with census tract maps to facilitate an "up close" look at the neighborhoods in which the riots occurred. Further work with the CPS and census tract data may provide a better sense of where and when the effects of riots took hold.

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Table 1: The Riots of the 1960s, Frequency and Severity

	1964	1965	1966	1967	1968	1969	1970	1971	Total
Riots	11	11	53	158	289	124	68	38	752
Days of Riots	34	20	109	408	739	284	126	82	1,802
Killed	2	35	11	83	66	13	13	5	228
Injured	996	1,132	525	2,801	5,302	861	710	414	12,741
Arrested	2,917	4,219	5,107	17,011	31,680	4,730	2,027	1,408	69,099
Occurrences of Arson	238	3,006	812	4,627	6,041	369	283	459	15,835
Index Value	0.163	0.504	0.275	1.349	1.956	0.374	0.230	0.149	5.000
Northeast	0.145	0.003	0.027	0.419	0.288	0.125	0.078	0.023	1.532
Midwest	0.008	0.011	0.180	0.750	0.501	0.079	0.042	0.004	1.107
South	0.010	0.001	0.019	0.107	1.055	0.115	0.104	0.121	0.786
West	0.000	0.489	0.050	0.073	0.112	0.056	0.006	0.001	1.574

Notes: See text for definition of a riot. Each riot (j) is assigned a value $S_j = \sum_i (X_{ij} / X_{iT})$ where X_{ij} is a component of severity (days of rioting, injuries, arrests, deaths, and arsons) and X_{iT} is the sum of X_{ij} across all riots. Summed over all riots in the dataset, there are five total index points (a reflection of the five components that enter the index).
Source: Carter (1986).

Table 2A: Riots and Change in Log Median Family Income for Blacks, 1960-1970

	1: OLS	2: OLS	3: OLS	4: OLS	5: OLS	6: OLS	7: 2SLS	8: 2SLS	9: 2SLS
Riot Index	-0.8330 (3.17)	-1.179 (3.07)	-1.121 (2.67)	-----	-----	-----	-----	-----	-----
Riot Index^2	1.391 (2.41)	2.101 (2.92)	1.983 (2.63)	-----	-----	-----	-----	-----	-----
Severe Riot	-----	-----	-----	-0.09168 (4.27)	-0.08522 (2.78)	-0.08560 (2.51)	-0.2688 (1.96)	-0.1792 (1.80)	-0.1878 (1.76)
Black 60	4.28 e-07 (2.75)	4.44 e-07 (2.39)	4.29 e-07 (2.22)	3.62 e-07 (2.66)	3.33 e-07 (2.15)	3.28 e-07 (2.07)	7.09 e-07 (2.14)	5.18 e-07 (1.88)	5.29 e-07 (1.83)
Pop 60	-5.33 e-08 (2.43)	-5.22 e-08 (2.13)	-5.08 e-08 (2.02)	-3.99 e-08 (2.04)	-3.30 e-08 (1.61)	-3.26 e-08 (1.61)	-6.33 e-08 (1.87)	-4.49 e-08 (1.56)	-4.54 e-08 (1.55)
Prop. Manu. 60	0.002458 (2.53)	0.001699 (1.00)	0.001516 (0.97)	0.002419 (2.55)	0.001917 (1.18)	0.001650 (1.16)	0.002578 (2.50)	0.002104 (1.23)	0.001851 (1.16)
Trend 1950-60	-----	-----	-0.07050 (1.25)	-----	-----	-0.09621 (1.72)	-----	-----	-0.09741 (1.58)
Midwest	0.05313 (2.47)	0.05390 (1.86)	0.05653 (2.18)	0.05888 (2.71)	0.06525 (2.21)	0.06884 (2.59)	0.06180 (2.51)	0.08305 (2.16)	0.08823 (2.44)
South	0.1089 (4.30)	0.1147 (3.16)	0.1058 (2.93)	0.1193 (4.74)	0.1450 (4.12)	0.1316 (3.86)	0.1192 (4.11)	0.1635 (3.79)	0.1515 (3.49)
West	0.007036 (0.21)	-0.02873 (0.70)	-0.03629 (0.88)	0.01943 (0.59)	0.01124 (0.31)	0.0002322 (0.01)	0.04133 (1.01)	0.07007 (0.93)	0.06403 (0.77)
Constant	0.4048 (11.96)	0.4345 (7.07)	0.4843 (6.79)	0.3889 (11.69)	0.3856 (6.67)	0.4561 (7.14)	0.3804 (9.97)	0.3609 (5.12)	0.4301 (5.52)
N	102	41	41	102	41	41	102	41	41
R^2	0.28	0.53	0.54	0.31	0.48	0.51	0.16	0.40	0.41
Mean Change	0.5103	0.5327	0.5327	0.5103	0.5327	0.5327	0.5103	0.5327	0.5327

Notes: t-statistics are in parentheses. Median family income figures for 1960 and 1970 are from the published census volumes. A family income measure for 1950 is constructed using the IPUMS (excluding households of one person). Regional designations follow census convention.
Sources: Manufacturing and population variables are from issues of the City and County Data Book (tabulated in ICPSR 7735). The city manager instrumental variable is from the Governmental Units Analysis Data (Aiken and Alford 1998; ICPSR 28). Rainfall data are from the National Climatic Data Center website (www.ncdc.noaa.gov). See table 1 and text for discussion of riot severity data.

Table 2B: Riots and Change in Log Median Family Income for Blacks 1960-1980

	1: OLS	2: OLS	3: OLS	4: OLS	5: OLS	6: OLS	7: 2SLS	8: 2SLS	9: 2SLS
Riot Index	-1.415 (3.12)	-1.626 (2.83)	-1.333 (3.15)	-----	-----	-----	-----	-----	-----
Riot Index^2	2.290 (2.42)	2.853 (2.55)	2.160 (2.70)	-----	-----	-----	-----	-----	-----
Severe Riot	-----	-----	-----	-0.1178 (2.42)	-0.07175 (1.19)	-0.08307 (2.07)	-0.5133 (1.91)	-0.3641 (1.22)	-0.3916 (1.49)
Black 60	3.54 e-07 (1.68)	4.23 e-07 (1.95)	5.13 e-07 (2.56)	1.50 e-07 (0.83)	2.00 e-07 (0.98)	3.38 e-07 (2.01)	9.25 e-07 (1.46)	7.65 e-07 (1.07)	9.49 e-07 (1.48)
Pop 60	-4.08 e-08 (1.25)	-5.05 e-08 (1.69)	-6.33 e-08 (2.32)	-1.12 e-08 (0.37)	-2.16 e-08 (0.71)	-3.83 e-08 (1.48)	-6.30 e-08 (1.00)	-5.69 e-08 (0.84)	-7.74 e-08 (1.21)
Prop. Manu. 60	0.001467 (0.85)	-0.0007769 (0.35)	0.0006385 (0.21)	0.001343 (0.78)	-0.0002467 (0.09)	0.001092 (0.34)	0.001749 (0.93)	0.0009615 (0.23)	0.002528 (0.54)
Trend 1950-60	-----	-----	-0.3520 (1.89)	-----	-----	-0.4104 (2.30)	-----	-----	-0.4614 (2.01)
Midwest	0.1460 (4.16)	0.06738 (1.32)	0.0440 (0.63)	0.1544 (4.31)	0.06816 (1.30)	0.04231 (0.58)	0.1622 (3.78)	0.1230 (1.32)	0.09676 (0.92)
South	0.2714 (5.50)	0.2131 (3.07)	0.1931 (2.94)	0.2866 (5.74)	0.2413 (3.24)	0.2126 (3.23)	0.2976 (4.80)	0.3178 (2.61)	0.2894 (2.73)
West	0.1239 (1.95)	0.03368 (0.37)	0.06182 (0.82)	0.1399 (2.20)	0.06199 (0.66)	0.08159 (1.19)	0.1961 (2.48)	0.2903 (1.13)	0.3239 (1.37)
Constant	0.9780 (15.31)	1.085 (11.76)	1.246 (10.19)	0.9538 (14.94)	1.019 (10.44)	1.227 (9.80)	0.9325 (12.79)	0.9217 (5.02)	1.1505 (5.25)
N	85	31	31	85	31	31	85	31	31
R^2	0.39	0.61	0.69	0.37	0.53	0.65	0.07	0.15	0.23
Mean Change	1.119	1.138	1.138	1.119	1.138	1.138	1.119	1.138	1.138

Notes and sources: t-statistics are in parentheses. See table 2A for discussion of variables.

Table 3A: Riots and Change in Black Male Unemployment Rate, 1960-1970

	1:OLS	2: OLS	3: OLS	4:OLS	5: OLS	6: OLS	7: 2SLS	8: 2SLS	9: 2SLS
Riot Index	-0.1274 (1.50)	-0.09022 (0.86)	-0.1459 (1.61)	-----	-----	-----	-----	-----	-----
Riot Index^2	0.1868 (0.87)	0.1476 (0.65)	0.2709 (1.45)	-----	-----	-----	-----	-----	-----
Severe Riot	-----	-----	-----	-0.007919 (0.94)	0.0008385 (0.10)	-0.005686 (0.64)	0.005244 (0.22)	0.04014 (0.88)	0.04477 (0.92)
Black 60	-----	-5.60 e-08 (1.31)	-3.82 e–08 (1.00)	-----	-8.51 e-08 (2.20)	-6.34 e-08 (1.78)	-----	-1.62 e-07 (1.54)	-1.63 e-07 (1.44)
Pop 60	-----	1.01 e-08 (1.66)	7.81 e-09 (1.45)	-----	1.31 e-08 (2.36)	1.08 e-09 (2.19)	-----	1.82 e-08 (1.89)	1.76 e-08 (1.73)
Prop. Manu. 60	-----	-0.0009548 (2.93)	-0.0009579 (3.43)	-----	-0.0009697 (2.99)	-0.0009666 (3.47)	-----	-0.001006 (3.01)	-0.001013 (3.48)
Trend 1950-60	-----	-----	-0.3222 (3.72)	-----	-----	-0.3177 (3.67)	-----	-----	-0.2854 (3.06)
Midwest	0.01398 (1.65)	0.01398 (1.65)	0.02874 (3.04)	0.01409 (1.54)	0.01441 (1.68)	0.02947 (3.05)	0.01349 (1.45)	0.01375 (1.54)	0.02711 (2.65)
South	0.002360 (0.27)	0.002360 (0.27)	0.01338 (1.53)	0.01695 (2.36)	0.003489 (0.40)	0.01503 (1.73)	0.01670 (2.36)	0.003474 (0.38)	0.01384 (1.47)
West	0.01877 (1.80)	0.01877 (1.80)	0.02189 (2.41)	0.03707 (3.91)	0.01871 (1.82)	0.02337 (2.56)	0.03658 (3.91)	0.01384 (1.15)	0.01673 (1.46)
Constant	-0.003094 (0.25)	-0.003094 (0.25)	-0.01405 (1.27)	-0.03742 (5.85)	-0.004244 (0.34)	-0.01646 (1.47)	-0.03840 (5.91)	-0.002314 (0.18)	-0.01277 (1.06)
N	102	102	102	102	102	102	102	102	102
R^2	0.26	0.26	0.42	0.16	0.25	0.41	0.14	0.18	0.29
Mean Change	-0.02288	-0.02288	-0.02288	-0.02288	-0.02288	-0.02288	-0.02288	-0.02288	-0.02288

Notes and sources: t-statistics are in parentheses. See table 2A for discussion of variables.

Table 3B: Riots and Change in Black Male Unemployment Rate, 1960-1980

	1: OLS	2: OLS	3: OLS	4: OLS	5: OLS	6: OLS	7: 2SLS	8: 2SLS	9: 2SLS
Riot Index	-0.0009105 (0.01)	-0.09153 (0.78)	-0.1266 (1.12)	-----	-----	-----	-----	-----	-----
Riot Index^2	0.05026 (0.27)	0.1705 (0.79)	0.2456 (1.17)	-----	-----	-----	-----	-----	-----
Severe Riot	-----	-----	-----	0.002970 (0.40)	-0.006324 (0.64)	-0.01191 (1.15)	0.04237 (1.67)	0.05818 (1.27)	0.06028 (1.23)
Black 60	-----	8.52 e-08 (1.65)	1.01 e-07 (1.86)	-----	7.54 e-08 (1.74)	9.65 e-08 (1.97)	-----	-5.09 e-08 (0.47)	-4.71 e-08 (0.40)
Pop 60	-----	-1.18 e-08 (1.54)	-1.41 e-08 (1.79)	-----	-1.03 e-08 (1.55)	-1.28 e-08 (1.79)	-----	-1.91 e-09 (0.17)	-3.03 e-09 (0.25)
Prop. Manu. 60	-----	0.0004629 (1.01)	0.0004458 (0.93)	-----	0.0004608 (1.02)	0.000449 (0.95)	-----	0.0003947 (0.83)	0.0003789 (0.77)
Trend 1950-60	-----	-----	-0.2531 (1.81)	-----	-----	-0.2555 (1.84)	-----	-----	-0.2000 (1.42)
Midwest	0.02104 (1.86)	0.01948 (1.71)	0.03197 (2.43)	0.02136 (1.92)	0.02009 (1.79)	0.03304 (2.51)	0.01913 (1.61)	0.01883 (1.55)	0.02883 (2.04)
South	-0.02282 (2.45)	-0.01893 (1.51)	-0.01257 (0.90)	-0.02276 (2.48)	-0.01798 (1.47)	-0.01112 (0.80)	-0.02575 (2.65)	-0.01977 (1.47)	-0.01457 (0.99)
West	-0.02509 (2.19)	-0.01608 (1.16)	-0.01120 (0.86)	-0.02454 (2.27)	-0.01462 (1.08)	-0.008409 (0.65)	-0.02688 (2.33)	-0.02376 (1.57)	-0.01981 (1.30)
Constant	0.04876 (5.80)	0.03449 (1.95)	0.03450 (1.95)	0.04872 (6.62)	0.03271 (1.92)	0.02350 (1.34)	0.04580 (6.11)	0.03617 (2.02)	0.02931 (1.60)
N	85	85	85	85	85	85	85	85	85
R^2	0.23	0.25	0.32	0.23	0.25	0.32	0.12	0.10	0.13
Mean Change	0.04516	0.04516	0.04516	0.04516	0.04516	0.04516	0.04516	0.04516	0.04629

Notes and sources: t-statistics are in parentheses. See table 2A for discussion of variables.

Table 4A: Riots and Change in Black Male Employment Rate 1960-1970

	1: OLS	2: OLS	3: OLS	4: OLS	5: OLS	6: OLS	7: 2SLS	8: 2SLS	9: 2SLS
Riot Index	-0.1135 (0.93)	-0.1625 (1.27)	0.05298 (0.46)	-----	-----	------	-----	-----	-----
Riot Index^2	0.2348 (0.74)	0.2923 (1.04)	-0.1482 (0.67)	-----	-----	------	-----	-----	-----
Severe Riot	-----	-----	-----	-0.01618 (1.38)	-0.02288 (1.96)	-0.007996 (0.84)	0.002276 (0.06)	0.006213 (0.10)	-0.02016 (0.35)
Black 60	-----	9.33 e-08 (1.37)	8.14 e-08 (1.52)	-----	9.54 e-08 (1.56)	9.69 e-08 (2.13)	-----	3.84 e-08 (0.27)	1.20 e-07 (0.97)
Pop 60	-----	-1.78 e-08 (1.79)	-1.56 e-08 (1.97)	-----	-1.64 e-08 (1.89)	-1.66 e-08 (2.42)	-----	-1.26 e-08 (0.97)	-1.82 e-08 (1.63)
Prop. Manu. 60	-----	0.0006636 (1.23)	0.0006523 (1.44)	-----	0.0006663 (1.24)	0.0006508 (1.45)	-----	0.0006397 (1.16)	0.0006619 (1.45)
Trend 1950-60	-----	-----	-0.4921 (4.79)	-----	-----	-0.4828 (4.75)	-----	-----	-0.4768 (4.29)
Midwest	-0.000124 (0.01)	-0.001476 (0.14)	-0.03088 (2.83)	0.0009914 (0.10)	-0.0001849 (0.02)	-0.03069 (2.79)	0.0001437 (0.01)	-0.000671 (0.07)	-0.03011 (2.69)
South	-0.00764 (0.61)	-0.001139 (0.08)	-0.03528 (3.16)	-0.006432 (0.52)	0.0008964 (0.07)	-0.03522 (3.14)	-0.006795 (0.54)	0.0008851 (0.06)	-0.03477 (3.02)
West	-0.02334 (1.38)	-0.01097 (0.58)	-0.01739 (1.10)	-0.02075 (1.28)	-0.007430 (0.40)	-0.01773 (1.12)	-0.02143 (1.27)	-0.01103 (0.51)	-0.01612 (0.86)
Constant	-0.004008 (0.43)	-0.02368 (1.19)	-0.01438 (0.88)	-0.005622 (0.69)	-0.02726 (1.40)	-0.01360 (0.84)	-0.006989 (0.78)	-0.02583 (1.29)	-0.01436 (0.86)
N	102	102	102	102	102	102	102	102	102
R^2	0.03	0.06	0.37	0.05	0.07	0.37	0.03	0.05	0.36
Mean Change	-0.01264	-0.01264	-0.01264	-0.01264	-0.01264	-0.01264	-0.01264	-0.01264	-0.01264

Notes: The employment rate is calculated using published census data. It is the ratio of employed males over total males over age 14 (in 1960) or age 16 (in 1970). Since a comparatively high proportion of 15 and 16 year olds are not employed, the change in definition tends to understate the decline in employment over time, but we have no reason to believe that it biases the regression results. t-statistics are in parentheses.
Sources: See table 2A.

Table 4B: Riots and Black Male Employment Rates, 1960-1980

	1: OLS	2: OLS	3: OLS	4: OLS	5: OLS	6: OLS	7: 2SLS	8: 2SLS	9: 2SLS
Riot Index	-0.6087	-0.5621	-0.3566	-----	-----	-----	-----	-----	-----
	(3.25)	(2.23)	(1.50)						
Riot Index^2	0.8864	0.8400	0.4393	-----	-----	-----	-----	-----	-----
	(2.29)	(1.78)	(1.04)						
Severe Riot	-----	-----	-----	-0.07231	-0.06167	-0.04398	-0.1373	-0.1620	-0.1625
				(4.19)	(2.31)	(1.87)	(2.94)	(1.81)	(1.74)
Black 60	-----	-4.54 e-08	-6.63 e-08	-----	-1.16 e-07	-1.26 e-07	-----	8.10 e-08	1.02 e-07
		(0.44)	(0.73)		(1.27)	(1.66)		(0.36)	(0.47)
Pop 60	-----	6.08 e-09	9.89 e-09	-----	1.79 e-08	1.94 e-08	-----	4.76 e-09	4.09 e-09
		(0.43)	(0.80)		(1.46)	(1.89)		(0.22)	(0.19)
Prop. Manu. 60	-----	-0.001448	-0.001383	-----	-0.001504	-0.001444	-----	-0.001401	-0.001337
		(1.69)	(1.68)		(1.73)	(1.74)		(1.53)	(1.52)
Trend 1950-60	-----	-----	-0.4789	-----	-----	-0.4749	-----	-----	-0.3909
			(3.36)			(3.41)			(2.30)
Midwest	0.01599	0.01663	-0.01505	0.01769	0.01996	-0.01297	0.02136	0.02193	-0.004884
	(0.95)	(1.05)	(0.84)	(1.03)	(1.23)	(0.72)	(1.14)	(1.24)	(0.23)
South	0.04709	0.02436	-0.005024	0.05104	0.03084	-0.000489	0.05598	0.03361	0.008239
	(2.83)	(1.17)	(0.25)	(2.95)	(1.45)	(0.02)	(3.06)	(1.46)	(0.36)
West	0.07288	0.04825	0.03651	0.08258	0.05485	0.03895	0.08644	0.06907	0.05812
	(2.91)	(1.52)	(1.33)	(3.44)	(1.78)	(1.43)	(3.55)	(1.93)	(1.70)
Constant	-0.1149	-0.06503	-0.05876	-0.1262	-0.07454	-0.06391	-0.1214	-0.07992	-0.07199
	(10.97)	(2.09)	(1.95)	(13.17)	(2.42)	(2.14)	(12.54)	(2.43)	(2.22)
N	85	85	85	85	85	85	85	85	85
R^2	0.27	0.30	0.42	0.26	0.30	0.41	0.18	0.19	0.28
Mean Change	-0.1033	-0.1033	-0.1033	-0.1033	-0.1033	-0.1033	-0.1033	-0.1033	-0.1033

Notes: The employment rate is calculated using published census data. It is the ratio of employed males over total males over age 14 (in 1960) or age 16 (in 1980). Since a comparatively high proportion of 15 and 16 year olds are not employed, the change in definition tends to understate the decline in employment over time, but we have no reason to believe that it biases the regression results. t-statistics are in parentheses.
Sources: See table 2A.

Table 5A: Change in Black Proportion of City Population, 1960-1970

	1: OLS	2: OLS	3: OLS	4: OLS	5: OLS	6: OLS	7: 2SLS	8: 2SLS	9: 2SLS
Riot Index	0.6673	0.4520	0.1732	-----	-----	-----	-----	-----	-----
	(5.59)	(2.85)	(1.30)						
Riot Index^2	-0.9851	-0.6946	-0.2674	-----	-----	-----	-----	-----	-----
	(3.45)	(2.65)	(1.18)						
Severe Riot	-----	-----	-----	0.06668	0.03457	0.01156	0.09825	0.05277	-0.005251
				(3.86)	(1.69)	(0.75)	(3.92)	(0.91)	(0.09)
Black 60	-----	2.65 e-07	1.16 e-07	-----	3.46	1.38	-----	3.10 e-07	1.59 e-07
		(2.58)	(1.44)		(3.71)	(1.79)		(2.26)	(1.48)
Pop 60	-----	-4.36 e-08	-1.86 e-08	-----	-5.46	-2.13	-----	-5.22 e-08	-2.19 e-08
		(3.16)	(1.66)		(4.51)	(2.03)		(3.90)	(1.95)
Prop. Manu. 60	-----	0.0006529	0.0004573	-----	0.0007046	0.000466	-----	0.0006883	0.0004693
		(1.50)	(1.41)		(1.58)	(1.45)		(1.52)	(1.49)
Trend 1950-60	-----	-----	0.5559	-----	-----	0.5835	-----	-----	0.6100
			(4.24)			(4.61)			(3.68)
Midwest	-0.01795	-0.02271	-0.01622	-0.01968	-0.02529	-0.01681	-0.02113	-0.02560	-0.01616
	(1.72)	(2.21)	(2.37)	(1.72)	(2.31)	(2.45)	(1.81)	(2.37)	(2.20)
South	-0.03787	-0.03939	-0.01464	-0.04360	-0.04509	-0.01543	-0.04422	-0.04509	-0.01409
	(3.46)	(2.69)	(1.08)	(3.62)	(2.95)	(1.13)	(3.66)	(3.02)	(0.95)
West	-0.03436	-0.01947	-0.01791	-0.04406	-0.02288	-0.01899	-0.04523	-0.02513	-0.01686
	(3.17)	(1.68)	(2.27)	(3.77)	(1.86)	(2.37)	(3.55)	(1.74)	(1.62)
Constant	0.04905	0.03448	0.01787	0.06232	0.04165	0.01955	0.05998	0.04253	0.01779
	(5.81)	(2.14)	(1.44)	(6.97)	(2.42)	(1.56)	(6.70)	(2.44)	(1.31)
N	102	102	102	102	102	102	102	102	102
R^2	0.41	0.50	0.64	0.31	0.47	0.63	0.27	0.47	0.63
Mean Change	0.04258	0.04258	0.04258	0.04258	0.04258	0.04258	0.04258	0.04258	0.04258

Notes and sources: t-statistics are in parentheses. See table 2A for discussion of variables.

Table 5B: Change in Black Proportion of City Population, 1960-1980

	1: OLS	2: OLS	3: OLS	4: OLS	5: OLS	6: OLS	7: 2SLS	8: 2SLS	9: 2SLS
Riot Index	0.8514 (2.86)	0.3560 (1.08)	-0.08810 (0.27)	-----	-----	-----	-----	-----	-----
Riot Index^2	-1.212 (1.58)	-0.5559 (0.86)	0.1366 (0.21)	-----	-----	----	-----	-----	-----
Severe Riot	-----	-----	-----	0.08079 (2.36)	0.01222 (0.32)	-0.02119 (0.66)	0.1442 (3.30)	0.04331 (0.48)	0.003883 (0.05)
Black 60	-----	5.46 e-07 (2.60)	2.96 e-07 (1.65)	-----	6.36 e-07 (3.20)	3.05 e-07 (1.62)	-----	5.75 e-07 (2.43)	2.74 e-07 (1.44)
Pop 60	-----	-8.28 e-08 (2.89)	-4.06 e-08 (1.57)	-----	-9.31 e-08 (3.57)	-3.98 e-08 (1.48)	-----	-8.90 e-08 (3.38)	-3.92 e-08 (1.53)
Prop. Manu. 60	-----	0.001985 (2.03)	0.001720 (1.98)	-----	0.002041 (2.13)	0.001721 (2.02)	-----	0.002009 (2.08)	0.001711 (1.98)
Trend 1950-60	-----	-----	0.8758 (3.39)	-----	-----	0.8854 (3.63)	-----	-----	0.8444 (3.08)
Midwest	-0.01195 (0.61)	-0.02289 (1.16)	-0.01255 (0.76)	-0.01291 (0.61)	-0.02457 (1.21)	-0.01164 (0.70)	-0.01649 (0.77)	-0.02518 (1.25)	-0.01271 (0.73)
South	-0.01598 (0.71)	-0.009839 (0.36)	0.03133 (1.18)	-0.01980 (0.84)	-0.01319 (0.49)	0.03316 (1.23)	-0.02462 (1.00)	-0.01405 (0.53)	0.03036 (1.07)
West	-0.06028 (2.64)	-0.01888 (0.78)	-0.01829 (0.91)	-0.07225 (3.22)	-0.01990 (0.84)	-0.01549 (0.82)	-0.07601 (2.97)	-0.02431 (0.86)	-0.01903 (0.78)
Constant	0.09028 (5.98)	0.03519 (1.07)	0.007285 (0.24)	0.1077 (7.69)	0.04009 (1.22)	0.004749 (0.16)	0.1030 (7.28)	0.04176 (1.26)	0.007651 (0.24)
N	85	85	85	85	85	85	85	85	85
R^2	0.27	0.42	0.54	0.18	0.41	0.54	0.12	0.40	0.54
Mean Change	0.09631	0.09631	0.09631	0.09631	0.09631	0.09631	0.09631	0.09631	0.09631

Notes and sources: t-statistics are in parentheses. See table 2A for discussion of variables.

Table 6: Riots, Income, and Employment in the IPUMS Data

	1: 1950-1970 DD Black Only	2: 1950-1970 DD Black Only	3: 1950-1970 DDD Black &White	4: 1950-1970 DDD Black &White	5: 1970-1980 DD Black Only	6: 1970-1980 DD Black Only	7: 1970-1980 DDD Black &White	8: 1970-1980 DDD Black & White
Panel A: Annual Income								
Severe × 1970	-0.02309 (0.97)	-0.02175 (0.96)	-----	-----	-0.02539 (1.97)	-0.02983 (2.23)	-----	-----
Severe × 1970 × Black	-----	-----	-0.02830 (0.81)	-0.02228 (1.05)	-----	-----	-0.01537 (1.15)	-0.01741 (1.41)
N	24,245	24,245	258,131	258,131	48,194	48,194	501,620	501,620
MSA Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
Panel B: Employment								
Severe × 1970	0.02698 (1.56)	0.03352 (2.95)	-----	-----	-0.03514 (3.15)	-0.03327 (4.88)	-----	-----
Severe × 1970 × Black	-----	-----	0.03073 (1.71)	0.03380 (3.65)	-----	-----	-0.03844 (4.15)	-0.03896 (6.91)
N	33,057	33,057	311,477	311,477	71,291	71,291	619,716	619,716
MSA Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Each coefficient is from a separate regression. t-statistics are in parentheses. All regressions include males, ages 18 to 64, who are not in school. The income regressions only include those employed for at least 40 weeks in the preceding year. All regressions control for age (quartic), education level (quadratic), marital status, migrant status (dummies for foreign born and those born in a state different from residence), region of residence, year dummy, interactions of region and year dummies, dummy for severe riot areas, interaction of severe riot area and year dummies. All of these control variables are interacted with a black dummy variable in the DDD regressions.
Sources: Micro-data are from Ruggles and Sobek (1997). Riot data are from Carter (1986) and discussed in table 1.