

**The Wrong Side(s) of the Tracks:  
The Causal Effects of Racial Segregation on Urban Poverty and Inequality**

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## Abstract

At the metropolitan level there is a striking negative correlation between residential racial segregation and population characteristics—particularly of black residents—but it is widely recognized that this correlation may not be causal. This paper provides a novel test of the causal relationship between segregation and population characteristics by exploiting the arrangements of railroad tracks in the 19th century to isolate plausibly exogenous variation in cities' susceptibility to segregation. I show that, conditional on miles of railroad track laid, the extent to which track configurations physically subdivided cities strongly predicts the level of segregation that ensued after the Great Migration of African-Americans to northern and western cities in the 20th century. At the start of the Great Migration, though, track configurations were uncorrelated with racial concentration, ethnic dispersion, income, industry, education, and population, indicating that reverse causality is unlikely. Even today, track configurations have no correlation with population characteristics in cities that were too far from the South to have received significant black in-migration during the Great Migration, which indicates that track configuration does not affect cities through any channel other than racial segregation. Instrumental variables estimates demonstrate that segregation increases cities' rates of black poverty and overall black-white income disparities, while decreasing their rates of white poverty and inequality within the white population.

## I. Introduction

Residential segregation by race is one of the most visible characteristics of many American cities. Although African-Americans represent just over 10 percent of the U.S. population, the average urban African-American lives in a neighborhood that is more than 50 percent black (Glaeser and Vigdor 2001). Cities vary in the extent to which their black populations live in black enclaves, and more segregated cities on average have worse characteristics than less segregated cities on measures ranging from infant mortality to educational achievement (Massey and Denton 1993).<sup>1</sup>

Segregation holds a longstanding position as one of the prime suspects in explaining the persistent economic inequality between blacks and whites. A number of papers have attempted to measure the effects of segregation on individual labor market and human capital outcomes (e.g. Massey and Denton 1993, Wilson 1996, Cutler and Glaeser 1997, Card and Rothstein 2006). Cutler and Glaeser (1997), among others, have expressed skepticism about this type of measure, however, for two reasons: omitted variable bias and endogenous migration.

In the first case, that of omitted variable bias, some unmeasured economic, political, or other attribute may lead certain cities to have both more segregation and more negative characteristics than other cities. For example, cities such as Detroit are highly segregated and their residents have poor economic outcomes, but other characteristics—such as political corruption or the legacy of a manufacturing economy—may be a cause of both. Failure to entirely capture such attributes will cause omitted variable bias in OLS estimates of the relationship between segregation and population characteristics.

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<sup>1</sup> Throughout the paper I use the term “city” to refer to a metropolitan area.

Instrumenting for a city's level of segregation can solve this problem of omitted variable bias, thereby allowing the net effect of segregation on population characteristics of cities to be estimated. In this paper I address concerns about omitted variable bias by using a function of 19<sup>th</sup>-century railroad configurations, conditional on total length of railroad, to instrument for the extent to which cities became segregated as they received inflows of African-Americans during the 20<sup>th</sup> century. I formalize the widely observed phenomenon of the “wrong side of the tracks” by showing that cities that were subdivided by railroads into a greater number of physically defined neighborhoods—which arguably serves as a technology for creating segregation—became significantly more segregated during the Great Migration than did other cities. This instrumental variable strategy allows me to identify the causal effect of segregation on net city-level outcomes.

I present evidence showing that there is little possibility of contamination of this instrument. Railroad division satisfies the instrumental variable validity requirements outlined in Angrist and Imbens (1994). Unlike variation used in other work on segregation (e.g. Cutler and Glaeser 1997), it strongly and robustly predicts metropolitan segregation and does not separately predict confounding metropolitan outcomes.<sup>2</sup>

Using this instrument, I examine the effect of segregation on cities' income distributions separately by race. I find that exogenously increasing segregation causes cities to have African-American populations with higher poverty rates and white

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<sup>2</sup> It does not predict outcomes in times or in places where there was negligible black presence, and it does not predict segregation on other dimensions, including income and ethnicity. It does not predict pre-period characteristics, including the structure of industry and the segregation of groups that were stigmatized prior to the arrival of blacks. And, after the Great Migration, railroad division does not predict outcomes in places that were too far from the South to receive large black inflows. These results provide evidence that railroad division drives current city outcomes through racial segregation, rather than through some other mechanism.

populations with lower poverty rates. Segregation increases inequality between blacks and whites as well as inequality within the black community, while reducing inequality within the white community.

To better understand how segregation has led to city-level differences in poverty and inequality, I explore other ways in which city populations differ according to segregation—in particular migration patterns and youth educational attainment. Identifying these differences can help clarify whether differences in populations result from causal treatment effects of segregation on individual-level human capital or from the sorting of different human-capital groups between cities as an endogenous response to segregation. For example, Detroit might have high poverty rates because segregation directly leads to inefficient education funding and lowers educational achievement (a direct effect on individual characteristics), or because people dislike segregation and those with high wages are willing to pay to go elsewhere (an effect on city characteristics but not on individuals). My empirical results, while not conclusive on this point, are most consistent with the hypothesis that both of these types of effects are at work.

The paper proceeds as follows. In section II, I discuss the historical and conceptual framework, and provide theoretical motivation for the instrumental approach. In section III, I summarize the data. In section IV, I present the main results and conduct robustness and falsification checks of my results. In section V, I conclude.

## II. Framework and instrument

### A. Historical framework

The history of residential racial segregation in the non-Southern urban United States can be roughly divided into three periods:

*Pre-segregation.* In the 19<sup>th</sup> century, very few African-Americans lived outside of the South. Even as late as 1910, 90 percent of the country's African-Americans still lived in the former slave states.<sup>3</sup> This observation is particularly relevant to this analysis, because the bulk of railroad tracks were laid prior to 1900 (Atack and Passell 1994); this timing makes it implausible that railroads in the North and West were laid with the intent of segregating African-Americans.<sup>4</sup>

*The creation of segregation.* During the Great Migration (roughly 1915 to 1950), large numbers of African-Americans migrated into Northern and Western cities from the South. Cities that had tolerated small black populations (Massey and Denton 1993, Weaver 1955) became highly segregated as their black populations grew (Cutler et al. 1999). Black segregation generally resulted largely from deliberate government policies and from collective action by white residents, i.e. not solely from uncoordinated market choices (Massey and Denton 1993). Put in economic terms, the Great Migration stimulated collective demand for segregation, and that demand was increasing with the level of in-migration. In the context of collective demand generated by black inflows,

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<sup>3</sup> Author's calculation from 1910 IPUMS data. I define "slave states" as those where slaveholding was legal at the onset of the Civil War. These include Delaware, Maryland, the District of Columbia, Virginia, West Virginia, North Carolina, South Carolina, Georgia, Florida, Alabama, Mississippi, Louisiana, Tennessee, Kentucky, Missouri, Texas, and Arkansas. My analysis throughout the paper excludes MSAs in these states.

<sup>4</sup> There were other stigmatized groups in those cities in the 19<sup>th</sup> century; however, there is no evidence that railroads were laid with the intent to segregate those groups. First, it appears that despite popular images of "Little Italy" and the like, ethnic segregation in the U.S. was never very high. Massey and Denton (1993) provide qualitative evidence for this claim:

"[European ethnic] immigrant enclaves in the early twentieth century...differed from black ghettos in three fundamental ways. First, unlike black ghettos, immigrant enclaves were never homogeneous and always contained a wide variety of nationalities, even if they were publicly associated with a particular national origin group...A second crucial distinction is that most European ethnics did not live in immigrant 'ghettos,' as ethnically diluted as they were...The last difference between immigrant enclaves and black ghettos is that whereas ghettos became a permanent feature of black residential life, ethnic enclaves proved to be a fleeting, transitory state in the process of immigrant assimilation." (32-33)

Second, to the extent that there was ethnic segregation, it does not appear related to railroads; in Section IV, I show that there is no relationship between measures of 1910 ethnic dispersion and railroad configuration.

technology to ease the coordination of segregation (such as railroad division, I will argue) should have increased equilibrium segregation.

*Post-civil rights movement.* Government policy towards residential segregation changed gradually during the civil rights era, and a clear break in housing policy came in 1968 with the Fair Housing Act and its outright prohibition of discrimination. High levels of segregation nonetheless have persisted in most American cities up to the present day (Cutler, Glaeser, and Vigdor 1999). At the same time, income gaps between blacks and whites, which showed signs of narrowing during the civil rights era, have persisted, particularly in more segregated cities. Many commentators (e.g. Massey and Denton 1993, Wilson 1996) have hypothesized that persistent segregation is partly responsible for this racial disparity in outcomes.

#### B. Previous research

Persistent segregation could affect city-level average resident attributes through treatment effects on the attributes of individual residents and/or through non-random migration of individuals with those attributes into or out of segregated cities. Most previous research has focused, implicitly, on either one or the other of these channels.

Wilson (1996) argues that racial segregation, by increasing skill segregation within the black community, causes negative outcomes for individual low-skilled blacks through peer effects; these individual treatment effects, he claims, produce a black “underclass” in segregated cities. Collins and Margo (2000) find evidence consistent with Wilson’s story for the post-civil rights period. Similarly, Card and Rothstein (2007) argue that, controlling for student background, residential segregation during high school leads

to lower test scores for black students relative to whites through treatment effects on individual students.<sup>5</sup>

A separate branch of the literature has focused on selection. Bayer, Fang, and McMillan (2005) and Baum-Snow (2007) argue that, through neighborhood choice, individuals reveal preferences for areas that are racially and economically homogeneous; Boustan (2006) argues that racial segregation is partly the result of efficient Tiebout sorting based on tastes for public good spending. In contrast, other research finds that many whites and blacks have stated tastes for neighborhood integration (Bobo et al. 1994), as well as revealed preferences for integrated cities (Cutler et al. 1999). These conflicting results on preferences may be consistent with each other if the direction and magnitude of population flows differ by race and skill. For example, Vigdor (2002) finds that in recent years African-Americans with above-median education were less likely to migrate into segregated cities than were less-educated African-Americans.

Little empirical research has considered both individual-treatment and population-selection effects of segregation together. A partial exception is Cutler and Glaeser (1997), whose model assumes that whites have tastes for segregation and that segregation affects the level of blacks' human capital. They raise the issue of selection but attempt to eliminate it from their analysis by limiting their empirical strategy to questions for which they believe migration will not be important.

In this paper, I move beyond past work by assuming that segregation can affect the human capital of whites as well as blacks, and that both blacks and whites may have

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<sup>5</sup> In theory, direct effects of segregation on individual-level human capital could cause either better or worse outcomes for the segregated group, and in fact Borjas (1995) finds positive relationships between segregation and outcomes for U.S. immigrants; there does not appear to be any such evidence for African-Americans, however.

tastes for either segregation or integration. I then assume that migration (selection effects) can be motivated by concerns about human capital (individual-treatment effects) and/or by tastes. Finally, I assume that the aggregate effects of segregation on the income distributions of cities' black and white populations may result from a combination of selection and individual-treatment effects.

### C. Model of causal link

The above literature review motivates the following illustration of how the individual-treatment and the selection effects of segregation may be distinguishable in equilibrium. Note, however, that the empirical results presented in the later part of the paper do not depend on the specific assumptions of this model.

#### 1. *Static model*

Assume two small open-economy cities that exist for two generations. City I has technology such that it will have two perfectly racially integrated tracts, while city S has technology such that it will have two perfectly racially segregated tracts. In all other ways, these two cities are identical (Figure 1a). At time zero, corresponding to the Great Migration, each city is randomly assigned the same population of measure one,  $\beta$  of which is black and  $1 - \beta$  white. We can infer from the historical record that at the time of the Great Migration the average human capital of the black population,  $\mu_{Hb}$ , is lower than the average among the white population,  $\mu_{Hw}$ .

Consider the following human capital production function for an individual's offspring:

$$(1) \quad E[\lambda_2] = f(\lambda_1) \mu_{H1}^\alpha,$$

where  $\lambda_1$  is the individual's human capital level;  $\mu_{H1}$  is the average human capital in the individual's neighborhood;  $\alpha \geq 0$ ; and  $E[\lambda_2]$  is the expected value of one's offspring's human capital.<sup>6</sup> According to equation (1), the production of offspring human capital depends not only on an individual's own human capital but also, when  $\alpha$  is strictly greater than zero, the average human capital of neighborhood residents.<sup>7</sup> With  $\alpha > 0$ , the production of human capital is affected by racial segregation, since racial composition determines the average human capital in the neighborhood. That is, in city I, blacks and whites experience the same neighborhood average human capital,  $\beta\mu_{Hb} + (1 - \beta)\mu_{Hw}$ , because each of the two neighborhoods is a microcosm of the city. In city S, blacks are exposed to average human capital  $\mu_{Hb}$ , while whites are exposed to  $\mu_{Hw} > \mu_{Hb}$ .

If residents cannot move between cities, then the skill gap between blacks and whites will persistently be larger (weakly larger, if  $\alpha = 0$ ) in S than in I. Over time, the  $\mu_H$  of blacks and whites will weakly converge in I, as blacks and whites are exposed to the same average human capital generation after generation. In S, on the other hand, white offspring will be consistently exposed to neighborhoods with higher average human capital than will blacks, leading to weakly greater aggregate inequality in S than in (Figure 1b). If  $\alpha = 0$ , so human capital is not a function of neighborhood capital, then overall and within-race human capital will be equal in I and S; if  $0 < \alpha < 1$ , then overall

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<sup>6</sup> For a discussion of other possible permutations of the peer-effects equation, see Cook and Ludwig (2006).

<sup>7</sup> Human capital might depend on the neighborhood in which one grows up in because of a literal "peer effect" (Benabou 1996) or because of neighborhood characteristics proxied by peers (Card and Rothstein 2006), e.g. school or health services funding, the political power of the neighborhood, whether a chemical dumping ground is sited in the neighborhood, how connected residents are to job networks, the neighborhood crime rate, etc. In related work (Ananat and Washington 2007; Ananat, in progress), I explore which of these mechanisms appear to be most important; the goal of this paper is simply to capture the global effects of segregation on outcomes.

human capital will be greater in city I than in city S; if  $\alpha > 1$ , then overall human capital will be highest in city S.<sup>8</sup>

We can identify empirically the relative levels of human capital by observing the eventual income distribution in exogenously segregated cities. Again, absent migration, the differences in the aggregate income distributions between S and I will reflect the effect of segregation on individual human capital and also will answer the question first posed by Cutler and Glaeser (1997), “are ghettos good or bad?”, for society as a whole.

## *2. Model with city choice*

If moving costs are low enough that migration between cities occurs, then observed differences in the income distribution cannot be interpreted simply as resulting from the treatment effects of segregation on individual human capital, for two reasons. First, concerns about offspring outcomes might cause people to migrate between cities in ways that differ by skill and race, so that the actual treatment effects of segregation are obscured by sorting.<sup>9</sup> Second, when moving is possible, any tastes for integration  $\sigma$  (positive or negative) may affect city composition, and could cause differences in observed skill distribution by city even if in fact  $\alpha = 0$ .<sup>10</sup>

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<sup>8</sup> If own human capital and neighborhood average human capital are substitutes in the production of next generation human capital (i.e.  $\alpha < 1$ ), then integration—the exposure of all individuals to equal human capital—will result in higher human capital than segregation. If own human capital and neighborhood average human capital are complements (i.e.  $\alpha > 1$ ), then segregation—which exposes the offspring of the racial group with the greater human capital to neighborhoods with greater human capital— will result in higher human capital than integration. Note, however, that the expected value of the human capital of one’s offspring is always increasing with the neighborhood average human capital, so that it is always productive, from any individual’s perspective, to be in a neighborhood with higher average human capital. In the absence of perfect markets, (e.g. coordinated payments by the black community to the white community to compensate for integrating neighborhoods), city S may not integrate even if integration is more efficient overall (Schelling 1971).

<sup>9</sup> For example, high-skilled blacks who live in S may move to I to take advantage of the higher human capital mix available to blacks in integrated cities; this would raise the average type in I and make integration appear more productive than segregation.

<sup>10</sup> For example, if high-skilled people tend to have positive tastes ( $\sigma > 0$ ) for integration, then city I might end up with more high-skilled whites than S (an outcome impossible in the static model).

If both  $\alpha$  and  $\sigma$  are nonzero, then an individual deciding whether to move will have to weigh the importance of his taste in neighborhood composition and the (discounted) expected skill of his offspring against the price of living in his preferred city.<sup>11</sup> Within race and skill, individuals will sort by preference  $\sigma$  for integration, so that the individual of race  $r$  and skill  $\lambda$  with preference  $\sigma_{r\lambda}^*$  is indifferent between the two cities, while those with  $\sigma > \sigma_{r\lambda}^*$  choose city I and those with  $\sigma < \sigma_{r\lambda}^*$  choose city S.

This discussion has two important empirical implications. First, if rents are lower in cities with more exogenous segregation, then either  $\alpha < 1$  (segregation is less productive than integration), or the average  $\sigma$  is large (most people have tastes for integration), or both. Second, as long as taste and skill are not perfectly inherited, then migration will persist even in equilibrium, as offspring who find themselves with different tastes or skills from their parents (and thus with different willingness to pay) resort between cities. We can therefore observe equilibrium rents and population flows in order to make inferences about the treatment and selection processes at work, as illustrated in Figure 1c.

#### D. Instrumental approach

To test for these or other patterns of outcomes requires empirical variation approaching a randomized experiment. Ideally, one would conduct the following test using two initially identical cities with small open economies:

- 1) At time zero, one city would be assigned perfect residential segregation, the other perfect residential integration.

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<sup>11</sup> It is possible—indeed likely—that in city S, high-skilled blacks, with no access to neighborhoods with high average skill, will want to separate from low-skilled blacks in order to “catch up” with white neighborhoods’ average values (Wilson 1996; Cutler and Glaeser 1997). They might more easily do so, however, by moving to integrated cities than by attempting coordinated moves within cities (Vigdor 2002).

- 2) Each city would be randomly assigned black residents from the initial black skill distribution and white residents from the initial white distribution.
- 3) Then, the relationship between segregation and the income distribution of the offspring generation would be measured. This is the individual-treatment effect of segregation.
- 4) Finally, residents would be allowed to move and aggregate demand for cities (rent, migration) by race and skill would be measured to determine tastes for segregation and its consequences. This is the selection effect of segregation.

The instrumental approach that I use approximates part 1) of this ideal experiment by providing plausibly exogenous variation in the ease with which cities could segregate. I argue that it also approximates part 2) because, as I will demonstrate, this variation appears not to be confounded with initial differences in the characteristics of residents and in-migrants, nor do there appear to be significant observable initial differences in the cities, other than railroad configuration, that would be likely to drive some sort of unobserved sorting.<sup>12</sup>

The variation I exploit is created by idiosyncrasies in the layout of railroad tracks that ease the collective definition of neighborhoods. In many cities it is self-evident that railroads tend to define neighborhood boundaries. Although explanations for why railroads per se do so are not available in the literature, the use of a standardized marker such as railroads is exactly what would be predicted by a “coordinated expectations” model of conflict with limited communication (Schelling 1963). A railroad provides a clear demarcation that facilitates collective agreement on neighborhood boundaries by

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<sup>12</sup> The quasi-experimental design I exploit does not allow the clean separation of 3) from 4). However, empirically I attempt to distinguish between individual-treatment effects (3) and selection effects(4).

residents, real estate agents, police, and others. Moreover, unlike roads (of which there are too many) or rivers (of which there are too few), railroads often cover the landscape at the proper intervals for defining neighborhoods.

As the black population of a city grew during the Great Migration, the city's ghetto areas had to expand if segregation was to be maintained. As Schelling (1963) describes the function of an obvious landmark for an army, a set of railroad tracks plausibly became for the white community "one spot to which they [could] retreat without necessarily being expected to retreat further" (p.71).<sup>13</sup> Once railroad tracks were used as a focal point for coordinating expectations in a specific instance, it was likely that they would be used again throughout the city if available. Cities that were subdivided by railroads into many small insular neighborhoods could use those boundaries to redefine the ghetto areas of the city, "falling back" one neighborhood at a time while still practicing "containment," whereby the black population remained concentrated and isolated. In cities where railroads were not configured in such a way as to define many neighborhoods, however, expanding an enclave meant breaching a major divide; once the black population increased enough that spillover was inevitable, segregation no longer could be maintained as easily in the open area on the other side.

Figure 2 illustrates this concept; it shows for two cities the railroad tracks that had been laid by 1900 (represented by heavy lines), as well as the Census 2000 tract-percent-black (represented by the heaviness of tract shading; tracts are outlined in thin lines). Binghamton, NY, at left, and York, PA, at right, are similar in terms of the total quantity of track laid by 1900 and experienced comparable, substantial changes in African-American population during the Great Migration. However, York's railroads were

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<sup>13</sup>Thanks to Glenn Loury for pointing me towards this reference.

configured such that they created many insular neighborhoods, particularly in the center of the city, while in Binghamton, railroads were tightly clustered, leaving some areas too long and narrow to encompass neighborhoods and others too wide open to create meaningful population restrictions. In contrast to York, whose current black population is relatively concentrated within its set of small, railroad-defined neighborhoods, Binghamton's black population is now relatively evenly dispersed throughout much of the city.

#### E. Instrumental measurement and validity

Formally, I approximate the ideal randomized experiment for places by exploiting the configuration of tracks into shapes that define uniform subunits of land (details follow later in this section) in a city's historical center, conditional on total track length. The process of extracting railroad data from a city map is explained in detail in Appendix A; briefly, I collected from 19<sup>th</sup>-century maps of 121 cities information about the railroads that covered a 50-square kilometer circular area centered on the historical city (the circles are visible in Figure 2). The choice of this area size provides the advantage that, while all the cities studied exceed this perimeter today, about 75% of the cities were smaller than that area when mapped. So, for most cities, this measure includes railroads that were laid on unoccupied land without any need to consider human occupants.

Within this four-kilometer-radius circle, every railroad track was identified and its length measured, and the area of the "neighborhoods" created by its intersections with

each other railroad was calculated. Historical railroads do quite well at predicting the borders of current neighborhoods as identified by Census tracts (Figure 2).<sup>14</sup>

From the data generated this way, I create a measure of a city's railroad-induced potential for segregation. I define a "railroad division index," or RDI, which is a variation on a Herfindahl index that measures the dispersion of a city's land into subunits.

$$(2) \quad RDI = 1 - \sum_i \left( \frac{area_{neighborhoodi}}{area_{total}} \right)^2$$

The RDI quantifies the extent to which the city's land is divided into smaller units by railroads. If a city were completely undivided by railroads, so that the area of its single neighborhood was 100% of the total city area, then the RDI would equal 0. If a city were infinitely divided by railroads, so that each neighborhood had area near zero, then the RDI would equal 1. The more subdivided a city, the more possible boundaries between groups are available to use as barriers enforcing segregation. In particular, if railroads created many small neighborhoods (high value of RDI), then it would have been possible during the Great Migration to relieve pent-up housing demand by allowing a black enclave to expand into another neighborhood, while still maintaining a new railroad barrier between the enclave and the rest of the city. This high RDI should have facilitated persistent segregation even as the black population increased. As shown in Figure 3, a scatterplot of these cities, with RDI on the x-axis and segregation index<sup>15</sup> on the y-axis, demonstrates a positive relationship between RDI and segregation.

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<sup>14</sup> The actual land area within the circle also was calculated, so that measurement could be adjusted for available observed land when working with maps that truncated city observations or included substantial bodies of water.

<sup>15</sup> This index, called the index of dissimilarity, is described in the next section.

In addition to a generally positive relationship with segregation, RDI should interact positively with increasing demand in determining equilibrium segregation. The demand for segregation is difficult to measure directly; moreover, it probably varies endogenously. However, one consistent driver of the demand for segregation, regardless of other city characteristics, has been the size of the black population.<sup>16</sup> From the historical evidence it appears that, across cities with varying underlying characteristics, the demand for segregation consistently goes up as the percent black in the city goes up (Massey and Denton 1993, Weaver 1955). The percent black *per se* also may be endogenous, but the fact that the Great Migration originated from the South meant that on average black inflows were higher in cities that were closer to former slave states. Cities 100 miles closer to a former slave state experienced, on average, 28 percent (0.5 percentage point) larger expansions in their black population share by 1940 (t-statistic=3.56) than did other cities in my sample. Proximity to the nearest former slave state, which varies greatly between cities—even within states such as Michigan and regions such as New England<sup>17</sup>—strongly predicts black inflows and therefore may function as a proxy for demand for segregation.

If proximity to the South truly predicts demand for segregation, then segregation should increase with proximity to the South and should increase more in places where RDI is high (so segregation is cheap). As shown in Figure 4—which separately plots the relationship between proximity to the South and equilibrium segregation for cities with above- and below-median RDI—that is the case. This additional source of variation in

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<sup>16</sup> Alternatively, I have attempted to use a city's WWII military contracts per capita as a predictor of black inflows (Dresser 1994). However, this variable suffers from weak instrument problems; in addition, subsequent literature has raised concerns about the excludability of that measure (Collins 2001).

<sup>17</sup> The 11 sample cities in Michigan range in distance from 228 to 451 miles from the nearest former slave state; the 22 sample cities in New England range from 159 to 400 miles from the nearest former slave state.

segregation creates the following falsification test for the quasi-experiment created by RDI: if RDI only affects outcomes through segregation, then it shouldn't have any effect in cities that are too far from the South to have had substantial demand for segregation, such as Seattle, WA, Lansing, MI, or Portland, ME.

### III. Data and Empirical Measures

#### A. Segregation measures

In addition to the maps described in Appendix A, the major data sources are U.S. Census Bureau reports on metropolitan demographics (various years), individual Census microdata from ipums.org, measures of metropolitan segregation from Cutler and Glaeser (1997) and Cutler, Glaeser, and Vigdor (1999), and proximity of the city to the nearest former slave state.

Segregation is captured by a dissimilarity index. Dissimilarity is defined as

$$(3) \text{ Index of dissimilarity} = \frac{1}{2} \sum_{i=1}^N \left| \frac{\text{black}_i}{\text{black}_{total}} - \frac{\text{nonblack}_i}{\text{nonblack}_{total}} \right|,$$

where again  $i = 1 \dots N$  is the array of census tracts in the area. It can be considered the answer to the question, “What percent of blacks (or non-blacks) would have to move to a different census tract in order for the proportion black in each neighborhood to equal the proportion black in the city as a whole?” By construction, the index can range from zero to one. Since tracts contain roughly 4000 individuals apiece, a city with 62 tracts that is 10 percent black would average a dissimilarity index of .029 (standard deviation .0028) if African-American households were randomly assigned across tracts;<sup>18</sup> in reality, the mean index is .57 (standard deviation .135).

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<sup>18</sup> Simulations by author of 1000 cities with size and black population equal to the median sample statistics.

I use the Cutler/Glaeser/Vigdor segregation data provided online by Vigdor (<http://trinity.aas.duke.edu/~jvigdor/segregation/>). These data come from various decennial Censuses, and include 19<sup>th</sup> and 20<sup>th</sup> century segregation indices as well as metropolitan characteristics from Cutler and Glaeser (1997) and from Cutler, Glaeser, and Vigdor (1999).<sup>19</sup>

## B. Additional variables

In every first-stage regression I control for kilometers of railroad track per square kilometer in the historical city (described further in Appendix A). Using this control assures that RDI does not simply capture the amount of railroad track in the city,<sup>20</sup> but rather represents the configuration of track conditional on total track.

For the main outcomes, I measure aggregate city<sup>21</sup> income distributions by race, using poverty rates from published Census reports and measures of inequality generated from public-use microdata (Ruggles et al. 2004). In order to assess to what extent these aggregate population impacts can be attributed to either (or both) individual treatment effects or selective migration effects, I also look at two other sets of outcomes: housing demand and the human capital of young adults.

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<sup>19</sup> Results are similar to those shown here when using other measures of segregation, including isolation, clustering, and concentration indexes.

<sup>20</sup> The density of railroads might be directly correlated with city outcomes for many reasons, such as industrial composition or physical attractiveness.

<sup>21</sup> I collect city outcomes from published Census reports (U.S. Census Bureau 2005). Although at the time that tracks were laid each of these cities was physically separated by open space from other cities, over the last century urban growth has meant that many once-distinct metropolitan areas are now conglomerates. To surmount this problem, I collect data for the reporting area which best centers on the original city center without containing other original city centers. Thus I use MSA-level data for the 64 cities that have remained independent MSAs. For MSAs in which multiple city centers are each in a separate county, I assign to each city the characteristics for the county that holds that city's original urban center. Doing so allows me to differentiate between the effect of an original center on its county level outcomes and the combined effect of several centers on MSA-level outcomes (e.g. outcomes for the New York-Northern New Jersey-Long Island Consolidated MSA). Fifty-three cities are in unique counties but share an MSA with at least one other city. Finally, for the 17 cities that share a single county with another city, I assign the characteristics of the politically-defined city itself to the observation.

First, to test the hypothesis that selective migration between more and less segregated MSAs drives human capital differences between cities, I examine measures of aggregate demand for cities by race. These measures include percent of the black and white populations that are new to the MSA, median rent by race, and crowding, from published Census reports. Second, I examine the human capital of individual young adults, aged 22 to 30 in the 1980 Census microdata, who were born just after the Great Migration, as a function of their MSA of residence five years prior<sup>22</sup> to the Census. Following Cutler and Glaeser (1997), I argue that these data should reflect the relationship between exposure to segregation as a child and eventual human capital as an adult.

#### IV. Results

##### A. First stage

If RDI-induced segregation is virtually randomly assigned, then the relationship between segregation and outcomes can be captured using simple equations. Segregation can be modeled as a classic endogenous regressor affecting outcomes at the city level,

$$(4) \text{Seg} = \alpha_1 \text{RDI} + \alpha_2 X + \mu$$

$$(5) Y = \beta_1 \text{Seg} + \beta_2 X + \varepsilon,$$

and then estimated using two-stage least squares analysis. The right-hand side variable of interest in equation (5),  $\text{Seg}$ , represents a city's current level of segregation.  $X$  is a vector of control variables that always includes total railroad length and, in some specifications, includes region indicators, manufacturing share, and/or population as additional controls.

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<sup>22</sup> Ideally, I would use MSA of birth; however, the Census only provides birth information at the level of the state. Previous MSA residence is available only for five years prior to the Census.

The first column of Table 1 shows that the first assumption required by my strategy (that the railroad division index must induce meaningful variation in the degree of racial segregation, i.e. there must exist a strong first stage) holds. Controlling for track per square kilometer in the historical city center, the neighborhood RDI generated by the configuration of track strongly predicts the metropolitan dissimilarity index in 1990. An increase of one standard deviation in the RDI (0.141) predicts a highly significant increase in dissimilarity of one-third of a standard deviation (0.050, t-statistic=4.07). The relationship also holds when controlling for proximity to the South, region indicators, manufacturing share, population, and black population share.<sup>23</sup>

#### B. Validity of first stage

For my instrumental variable strategy to be valid, it must be the case not only that railroad configuration leads to segregation; it must also be the case that both railroad configuration and people were each assigned to cities quasi-randomly, not in ways that reflect other underlying city characteristics.

To check this, I first test whether RDI had a relationship to city characteristics prior to the Great Migration, which would raise the possibility that 19<sup>th</sup>-century railroad configuration was not randomly assigned but rather was driven partly by local economic or social characteristics. Historical accounts of the reasons for railroad configuration (summarized in Appendix B) provide no support for that possibility; moreover, Table 1 presents evidence that RDI is unrelated to population characteristics even in 1910, a full decade after the end of major railroad construction, at the last Census prior to the Great Migration. Columns 2 through 7 show tests of the predictive power of the RDI for a variety of characteristics of cities prior to the start of the Great Migration. These include,

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<sup>23</sup> Results available upon request.

for 1910, physical size, population, dissimilarity and isolation measures for the foreign-born versus native-born population,<sup>24</sup> and percentage black, and for 1915, streetcars per capita.<sup>25</sup> None of the six coefficients on RDI are significant. Of the twelve total coefficients estimated, only one—the relationship between track length and 1910 percent black—is significant at conventional levels.

I also test whether residents appear to have been assigned to cities quasi-randomly or whether people actually sorted themselves into cities based on either railroad configuration or some pre-characteristic associated with railroad configuration. The fact that cities did not differ observably based on RDI prior to the Great Migration reduces the likelihood of the latter possibility. However, migrants still might have sorted between cities based on RDI itself, or on something I cannot observe that was correlated with RDI. Columns 8 through 13 of Table 1 test for the possibility of initial selection by examining the human capital characteristics of cities in 1920, after the first wave of the Great Migration but before segregation could begin to have any noticeable direct effects on human capital. These characteristics include percentage black, literacy rate, labor force participation, and the share of employment in trade, manufacturing, and railroads. Again, none of the six coefficients on RDI are significant, and of the twelve total coefficients estimated, only two—the relationships of track length with percent black and 1920 labor force participation—are significant at conventional levels. Taken together, the

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<sup>24</sup> European ethnic immigrant segregation was at its historical peak in 1910, according to Massey and Denton (1993). Its historical peak, and even the peak segregation of particularly stigmatized immigrant ethnic groups, was quite low relative to the historical peak of black segregation. The maximum recorded isolation index was 0.39 for Italians in Worcester, MA in 1910 (Cutler, Glaeser, and Vigdor 2005); by contrast, the *median* isolation index for blacks in 1970 was .37.

<sup>25</sup> Provided in Cutler, Glaeser, and Vigdor (1999) for 13 cities in this sample.

null results of all twelve tests in Table 1 provide strong evidence that RDI and populations were assigned to cities orthogonally.

Although railroad configuration does not predict pre-period outcomes, it is still possible that RDI currently affects city outcomes through channels other than racial segregation. Below I will demonstrate that RDI has little impact on outcomes in cities where low black inflows were expected; this suggests that RDI has no direct relationship with current urban characteristics. I also test directly for two of the most obvious ways that RDI might reflect present-day underlying differences between places. First, RDI might reflect regional geographic variation, and region may affect current outcomes for other reasons. However, I have replicated the results with indicators for Census region included; while the standard errors of the estimates increase, the results remain essentially the same.<sup>26</sup> Second, railroad configuration could reflect the value of land in the local area (although the historical record indicates that land prices were of minor concern; see Appendix B). Low or variable property values could lead to residential segregation by income, which, because of the correlation between race and income, could appear as racial segregation. However, the last column of Table 1 demonstrates that, even today, RDI does not predict income segregation.<sup>27</sup> Moreover, the countervailing effects of RDI on black versus white outcomes that I demonstrate in the main results section suggest that RDI acts through race to affect income rather than working directly through income.

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<sup>26</sup> Results available upon request.

<sup>27</sup> Income segregation is insignificant in the U.S. relative to racial segregation; the highest of any metropolitan dissimilarity index for income in 1990 is .28, while the *lowest* 1990 dissimilarity for African-Americans is .33.

In total, three assumptions are necessary in order to postulate that the quasi-experiment generated by railroad configuration and proximity to the South approximates the ideal experiment, and therefore that it is a valid instrumental strategy for measuring the effects of segregation on places. 1) RDI must induce meaningful variation in degree of racial segregation; i.e. there must exist a strong first stage. RDI meets this requirement, as shown by Column 1 of Table 1. 2) It must be the case that railroad configuration affected city outcomes through segregation, not through some other channel. The facts that (as shown in Table 1) RDI did not affect pre-Great Migration city characteristics and that (as I will show) RDI affects outcomes only in places that received significant black inflows, lend credibility to this assumption. 3) People must have been assigned to cities quasi-randomly; i.e., during the Great Migration people must not have self-selected into cities in any way that is correlated with RDI. Table 1 provides evidence that this assumption holds as well.

In sum, railroad division does not predict outcomes in times or places where there were not large black inflows, and it does not predict segregation on other dimensions, including income and ethnicity. It does not predict pre-period characteristics, including the structure of industry and the segregation of groups that were stigmatized prior to the arrival of blacks. It does not predict the initial characteristics of city in-migrants. These results taken together provide evidence that railroad division, while predicting racial residential segregation, is not correlated with other early city or population characteristics that might also affect cities today. It therefore meets the requirements for a valid instrument (Angrist and Imbens 1994) for use in two-stage least squares estimation. The next section details the two-stage estimation.

### C. OLS and 2SLS estimates for MSAs

In this section, I analyze the effect of RDI-induced segregation on the income distribution of city residents; these effects incorporate both direct treatment effects on individual human capital and indirect selection effects on cities' populations through migration.<sup>28</sup> In the next section, I will analyze migration, housing demand, and the human capital of young adults in order to assess to what extent these aggregate impacts can be attributed to either individual-treatment or selection effects, or both.

Ordinary least squares (OLS) and two-stage least squares (2SLS) estimates of the relationship of segregation with poverty and inequality by race are shown in Columns 1 through 4 of Table 2.<sup>29</sup> The within-race results, shown in the top panel, demonstrate that segregation increases poverty and inequality within the black community and decreases poverty and inequality within the white community. A one-standard-deviation (14 point) increase in dissimilarity causes roughly a 2.7 percentage point decrease in white poverty and a 4.7 percent decrease in the white Gini index; by contrast it leads to a 12.3 percentage point increase in black poverty and a 5.7 percent increase in the black Gini index.<sup>30</sup> A comparison of the OLS estimates with estimates that use RDI as an instrument reveals that OLS tends to understate the effects of segregation on poverty and inequality, although the differences in magnitude between the OLS and 2SLS estimates are significant only for whites.

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<sup>28</sup> Indirect effects on city population-level earnings may also operate through agglomeration economies; however, agglomeration economies cannot account for the racial disparities in earnings shown below, and testing for them is beyond the scope of the current paper.

<sup>29</sup> Results are similar when indicators for Census region are included and standard errors are clustered on region.

<sup>30</sup> Results using 90:10, 90:50, and 50:10 ratios are similar.

The bottom panel of Table 2 examines the effect of segregation on inequality between blacks and whites. There appears to be no effect of segregation on income disparities between well-off blacks and well-off whites, as measured by comparing the 90<sup>th</sup> percentile of the two income distributions to one another. By contrast, and consistent with the respective effects of segregation on white and black poverty rates, segregation leads to a dramatic increase in inequality between the worst-off whites and worst-off blacks. On average whites at the 10<sup>th</sup> percentile have incomes 107 percent higher than blacks at the 10<sup>th</sup> percentile; a one-standard-deviation increase in segregation causes that gap to increase to 148 percent. Similarly, the gap between the worst-off blacks and well-off whites increases substantially. By contrast, a one-standard deviation increase in segregation leads to a significant 11 percent narrowing of the income gap between the 90<sup>th</sup> percentile of blacks and the 10<sup>th</sup> percentile of whites. Again, the 2SLS estimates tend to be larger than the OLS estimates, although reduced precision means that the differences are not significant.

Columns 5 and 6 of Table 2 present falsification checks designed to test for the possibility that RDI is affecting poverty and inequality directly rather than only through racial residential segregation. For example, it could be that railroad configuration somehow alters available business investment opportunities directly, or makes transportation difficult, or weakens social ties between neighborhoods. If there is a direct relationship between RDI and income, however, we should be able to observe it even where Great Migration black inflows were low enough that demand for segregation was weak. Recall that, as shown in Figure 4, segregation is lower in cities far from the South than in those that are close, and that differences in segregation by RDI are not significant

in those cities. Thus, if it is true that RDI affects outcomes only through segregation, then it should have little effect on the income distributions of cities more than 400 miles away from the South.

Columns 5 and 6 show the reduced-form effects of railroad division on outcomes for cities that are at least 400 miles from the South. Of the 8 coefficient estimates, only one is significant at conventional levels, and three are in the opposite direction of the main result. These estimates indicate that no meaningful relationship exists between RDI and the income distribution in cities where RDI could have little effect on segregation. This falsification check thus lends credibility to the claim that RDI drives poverty and inequality through segregation and not through other channels.

Taken together, the results in Table 2 imply that segregation causes cities to have white populations with higher and more equal incomes and to have black populations with lower and more unequal incomes. These results do not, however, allow us to distinguish whether segregation causes individuals to earn different incomes than they would in the absence of segregation, whether it causes individuals with different incomes to select into different cities, or whether it causes both phenomena. Below I explore these different possible channels for the effect of segregation on city characteristics.

#### D. Tests for treatment and selection mechanisms

##### *1. Selection*

Table 3 shows migration and housing market characteristics by race reported at the urban level from the 1990 Census. Cities with more RDI-induced segregation have significantly fewer new residents, either black or white. A one-standard-deviation

increase in segregation leads to 2.2 percentage points fewer new white residents and 3.8 percentage points fewer new black residents.

It is possible that there are fewer new residents in more segregated cities because out-migration is lower, leading to few vacancies. However, the evidence on housing values shown in Table 5 suggests that segregated cities are in fact in less demand. First, more segregated places have significantly lower median rents for both blacks and whites (results are similar for mortgage costs and home values, which are not shown). These effects do not appear to be driven merely by a lower cost of living in more segregated cities, since rents are also lower as a fraction of income (significantly lower for whites). Second, lower expenditures on housing do not seem to reflect less consumption of housing in more segregated cities; blacks and whites are significantly less likely to live in crowded homes (that is, homes with more than one person per room) in more segregated cities.

The bottom panel of Table 3 presents falsification tests of the relationship between RDI and city demand in places too far from the South to have significant RDI-induced differences in segregation. In these places, RDI appears to have nearly no relationship with city demand; all of the coefficients are in the opposite direction of the estimates for the overall sample, and all but one (median gross rent among blacks) are small and insignificant. In other words, in places where RDI does not induce segregation, it appears to have either no or a small positive effect on city desirability, in contrast to the negative effect of RDI through segregation on city desirability.

The fact that migration appears to differ in observable ways between more and less segregated cities means that selection is a plausible explanation for at least part of the

variation in human capital. As laid out in Figure 1c, the combination of low demand and worse outcomes (higher poverty and inequality) for blacks in more segregated cities is consistent with either negative effects of segregation on human capital or simply distastes for segregation that induce selective migration, or both. However, the fact that demand appears low among whites as well as blacks, even though whites appear to do better (lower poverty and inequality) in more segregated cities, suggests that segregation must produce higher levels of human capital among whites—and that at least some whites must nonetheless have distastes for segregation or its effects.

## *2. Individual treatment effects*

I further test for evidence of individual-treatment effects of segregation on human capital by following Cutler and Glaeser's (1997) approach, which is to focus on the human capital of young adults as a function of the city in which they resided five years ago. These estimates, shown in Table 4, use individual data (Ruggles et al. 2004) on 22- to 30-year-olds in the 1980 Census in order to isolate effects on those born shortly after the Great Migration; their birth years are 1950 to 1958. Regressions include single-year-of-age dummies. Results are clustered on MSA.

Higher RDI-induced segregation in the city in which a young person born between 1950 and 1958 resided five years ago predicts a significantly higher probability of non-college attendance among blacks. A one-standard-deviation increase in segregation leads to a marginally significant 3.8 percentage point increase in the share of blacks who are high school dropouts, a significant 9 percentage point increase in the share of blacks who have exactly a high school education, and a 10 percentage point decrease in the share of blacks with some college. For whites, a one-standard-deviation

increase in segregation leads to a nonsignificant 0.9 percentage point drop in the share high school dropouts, a significant 5 percentage point increase in the share with exactly a high school education, and a significant 3 percentage point drop in the share with some college. There is no significant effect of segregation on white or black college graduation, consistent with the effects of segregation being concentrated in the lower portion of the income-skill distribution.

A falsification check using only cities far from the South reveals no significant relationships between segregation and any outcomes; one quarter of the estimates are in the opposite direction of the main results. Again, this lends evidence that results in the main sample reflect effects of RDI on human capital through segregation rather than through some other channel.

Cutler and Glaeser (1997) assume that the characteristics of 22- to 30-year-olds did not significantly drive their migration earlier than five years prior to observation. I strengthen support for this assumption by limiting my analysis to young adults born shortly after the Great Migration, who are (relatively) unlikely to have been affected by selective migration by their parents in response to effects of segregation that became visible after the Great Migration. Under Cutler and Glaeser's assumption, the results in Table 4 suggest that segregation causes lower human capital accumulation among blacks and that it concentrates human capital accumulation at a moderate, high-school-graduate level among whites, thereby reducing white skill inequality.

These results are consistent with segregation causing differences in the accumulation of individual human capital. They thereby provide some evidence against

the hypothesis that the effect of segregation on city characteristics is solely a product of selective migration.

#### IV. Discussion

In sum, segregation creates places where black poverty and inequality are higher and white poverty and inequality are lower, compared to places that are less segregated. These equilibrium characteristics could result from selective migration in response to segregation—for example, blue-collar whites could prefer segregated cities, while other groups prefer less segregated cities. Alternatively, these characteristics could reflect direct effects of segregation on individual human capital—for example, segregated cities might generate this equilibrium by transferring education and employment resources to at-risk whites at the expense of blacks and the better-off. Moreover, these effects could reinforce each other, so that in equilibrium both forces are at work. The empirical results are most consistent with the hypothesis that both of these effects are at work.

The evidence that segregation may improve outcomes for some individual whites is consistent with work by Card and Krueger (1994), who argue that racial division increases the ability of local government to transfer schooling resources from the black to the white community. As argued by Vigdor (2006), smaller racial achievement gaps in some cities than in others can be attributed partially to smaller resource disparities in those cities.

To answer the more general question posed by Cutler and Glaeser (1997), “Are ghettos good or bad?”, ghettos appear to be bad for African-Americans and good for whites in terms of the individual outcomes of young adults. However, aggregate demand

implies that segregation is also a metropolitan disamenity. In other words, revealed preferences suggest that the average American, when choosing a city, considers ghettos to be bad.

## Appendix A. Extracting the Railroad Division Index from Maps

Figure A illustrates the process of extracting railroad data through the example of Anaheim, CA. For each city, its map or maps were used to first identify its physical size, shape and location at the time its map was drawn. A Geographic Information Systems program, ArcGIS, was used to create a convex polygon that was the smallest such polygon that could contain the entire densely inhabited urban area. Dense habitation, defined as including any area with buildings and frequent, regular cross-streets, was identified by visual examination. ArcGIS then was used to identify the centroid of this polygon, and this point was defined as the historical city center. A four-kilometer radius circle around this point became the level of observation for the measurement of railroads. This approach meant that differences in initial city area would not distort the measurement of initial railroads: cities that were, at the time, very small still would be coded with railroads that affected later development, after the population had expanded; cities that were already large would have only those railroads in their center cities included. It should be noted, however, that about 75% of the cities were smaller than  $16\pi$  square kilometers when mapped, and many were much smaller, so for most cities this measure includes railroads that were laid on unoccupied land without need to consider habitation.

Visual examination reveals that the historical city center created in this way is typically quite close to what would be identified as the current city center if using a current map. Within this four-kilometer circle, every railroad was identified, its length measured, and the area of the “neighborhoods” created by its intersections with each other railroad calculated. Historical railroads predict quite well the borders of current neighborhoods as identified by the Census. The actual land area within the circle also was calculated, so that measurement could be adjusted for available observed land when working with maps that truncate city observations or include substantial bodies of water.

The final sample of 121 cities is derived as follows: Cutler and Glaeser (1997) provide data for all MSAs with at least 1000 black residents. Of these, I include only those MSAs in states that were not slave-owning at the time of the Civil War, because those states had few African-Americans prior to the Great Migration.<sup>31</sup> Further, my

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<sup>31</sup> Specifically, I exclude Delaware, Maryland, Washington, DC, Virginia, West Virginia, North Carolina, South Carolina, Georgia, Florida, Alabama, Mississippi, Louisiana, Tennessee, Kentucky, Missouri, Texas, and Arkansas. Nearly 90% of African-Americans resided in one of these states in 1910 (author’s calculation from 1910 IPUMS data).

sample was limited by the set of historical maps held by the Harvard Map Library.<sup>32</sup> The library depends on donations and estate purchases, etc., to collect maps, and therefore there are gaps in its collection. I have compared the full Cutler and Glaeser (1997) sample to the sample available from the Harvard Map Library. The cities for which the library could not provide maps appear quite similar in both historical and current characteristics measured by Cutler, Glaeser, and Vigdor (shown in Table A), differing at the 5-percent significance level on only 4 of 46 measures.

Ideally, my sample would include all places outside the South that were incorporated prior to the Great Migration, so that they were potential destinations for African-Americans leaving the South. Then the growth of the place into an MSA could itself be treated as an outcome of its potential segregation. Because the Census only provides data for large places, though, it is not possible to get information for places that are still small. Note that if segregation reduces economic development, then more towns will fail to achieve MSA status and thus be censored in the treatment group than the control group. This will cause an upward bias in the treatment effect estimate of segregation on growth, attenuating it towards zero. Alternatively, if segregation increases economic development, then the bias will run in the other direction; since the sign of the coefficient is now reversed, it is again an attenuation bias. Thus censoring on eventual MSA status should bias, if at all, towards a finding of no result.

## Appendix B. The History of U.S. Railroad Construction

My instrumental variables strategy requires that tracks were not initially laid in order to define neighborhoods, or for any other reason that might eventually affect urban outcomes. In addition to testing for such relationships mathematically, which I do in section IV of the main text, it is useful to refer to the historical record indicating the primary drivers of track configuration. Doing so allows me to identify any possibility that first-order considerations in laying track included some that were likely to have independent effects on current outcomes. The record indicates three main drivers of railroad placement in the United States.

1. *Slope*. Throughout the main period of railroad construction, land was plentiful while both labor and capital were scarce (Atack and Passell 1994). Therefore, land was the marginal input into railroads in the U.S. (contrary to the experience in Europe). Hence, microvariation in ground slope, which was the primary challenge in railroad construction (Wellington, *Economic Theory of the Placement of Railways*, 1911), drove elaborate surface configurations. “American railroads avoided topographic obstacles rather than level them, bridge them, or tunnel through them” (Atack and Passell 1994, p. 444).

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<sup>32</sup> The maps that provide railroad placement information were created by the U.S. Geological Survey as part of an effort to document the country’s topography, beginning in the 1880s.<sup>32</sup> These maps display elevation, bodies of water, roads, railroads, and (in many cases) individual representations of non-residential buildings and private homes. The edges of a 15-minute map are exogenously defined in round 15-minute units, so that, for example, a map will extend from -90°30’00” longitude and 43°45’00’ latitude (in the southeast corner) to -90°45’00’ longitude and 44°00’00’ latitude (in the northwest corner).

Because the Harvard Map Library collection is incomplete, there are 77 cities in non-South states available in the Cutler and Glaeser data for which I do not have the necessary map observations. In addition, in 15 cities I observe only some fraction of the four-kilometer-radius land area I wish to observe, since the cities overlap two or more 15-minute areas and I have maps only for some subset of those areas. Finally, in 40 cases the city overlaps multiple areas and I observe all of the areas.

2. *Competition*. The first practical railroads, in the early 19<sup>th</sup> century, were a product of pre-capitalist mercantilism. Typically, a city and its leading businessmen would fund the building of a line from an agricultural area to its downtown as an incentive to farmers to choose it as a shipping destination (Taylor and Neu 1956).<sup>33</sup> To insure that other cities did not benefit from their railroad investments, cities deliberately constructed railroads in ways that made them incompatible with each other.<sup>34</sup>

3. *National security*. During the Civil War, it became clear that having hundreds of short unconnected roads rather than a national network inhibited military activities. After the Civil War, Congress imposed a standard gauge on all railroads and subsidized private companies to create a single network throughout the country. Much of the placement of railroads, both to connect existing roads in settled areas<sup>35</sup> and to cover unsettled areas, was determined by this goal of a national network.

The most obvious factor missing from this list is the price of land. An obvious objection to the assumption of independence between the initial positioning of railroads and other neighborhood characteristics is that railroads systematically should have been built on land with depressed prices because it was less desirable. At the time, however, land in the U.S. was so plentiful that Congress literally was giving it away under the Homestead Act and other public land liberalizations. In fact, government gave land to railroad companies just to get them to build, and in many cases had to give massive amounts—up to 40 miles on alternating sides of the road—because the land was worth so little when undeveloped (Atack and Passell 1994, chapters 9 and 16). Thus, it made poor business sense to emphasize land cost over the cost of materials, labor, and energy consumption, a point emphasized by Wellington (1911).

From this evidence on the history of railroad placement, I argue that relative railroad subdivision of a city's topography was incidental. It was driven plausibly by the initial placement of unrelated tracks and the later need to connect them via the flattest and most direct route. It was thus plausibly uncorrelated with other relevant city characteristics. This qualitative evidence complements the quantitative evidence that I present in section IV supporting a zero correlation.

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<sup>33</sup> For instance, the Boston and Worcester line was designed by Boston merchants to divert trade between Worcester and Providence (Taylor and Neu 1956, p. 4).

<sup>34</sup> In Portland, Maine, the developers of a through line to Montreal consciously chose a gauge incompatible with existing Portland-Boston lines, for fear that otherwise “Boston would capture their trade and make them merely a satellite city” (Taylor and Neu, 1956, p.18). The Maine legislature forbade existing lines to change their gauges in response.

<sup>35</sup> Many people resisted the movement to standardize and connect railroads in towns that had these discontinuities. A clear threat to the independence between railroad placement and other town characteristics would exist if differential resistance was based on concerns about city topography—that citizens objected that railroads would divide neighborhoods or cause other disamenities. In fact, however, the historical record makes no mention of railroads' use and effects as a social barrier at the time they were being laid and connected (one reason may be that most towns were small enough that such barriers or local disamenities didn't have significant meaning). Objectors' main concern instead was connection *per se*. Businesses complained because towns with disconnected trains had developed an economy of middlemen, such as handlers for freight and service establishments for waiting crew and passengers (Taylor and Neu 1956).

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Table 1. Testing RDI as an Instrument

	<i>First stage</i>	<i>Falsification Checks</i>												
		<i>1910 city characteristics</i>						<i>1920 city characteristics</i>						
	<i>1990 dissim- ilarity index<sup>a</sup></i>	<i>Physical area<sup>a</sup> (sq. miles/1000)</i>	<i>Pop.<sup>b</sup> (1000s)</i>	<i>Ethnic dissim- ilarity index<sup>a</sup></i>	<i>Ethnic isolation index<sup>a</sup></i>	<i>% black<sup>b</sup></i>	<i>Street-cars per cap.<sup>a</sup> (1915)</i>	<i>% black<sup>b</sup></i>	<i>% literate<sup>b</sup></i>	<i>Labor force partici- pation<sup>b</sup></i>	<i>% of empl. in Trade<sup>b</sup></i>	<i>% of empl. in Manuf- acturing<sup>b</sup></i>	<i>% of empl. in Rail- roads<sup>b</sup></i>	<i>1990 income seg.<sup>a</sup></i>
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
<i>RDI</i>	<b>0.357</b> <b>(0.088)</b>	-3.993 (11.986)	0.666 (1.363)	0.076 (0.185)	0.027 (0.070)	-0.001 (0.010)	-0.132 (0.183)	0.013 (0.009)	0.053 (0.030)	0.028 (0.024)	-0.080 (0.094)	0.191 (0.137)	-0.074 (0.068)	0.032 (0.032)
<i>Track length per square kilometer</i>	18.514 (10.731)	-574.401 (553.669)	75.553 (134.814)	15.343 (53.249)	-12.439 (17.288)	<b>9.236</b> <b>(0.649)</b>	3.361 (20.507)	<b>9.119</b> <b>(0.615)</b>	0.180 (0.880)	<b>-3.427</b> <b>(1.500)</b>	-0.152 (2.910)	18.400 (10.911)	1.592 (2.428)	-2.504 (1.626)
<i>N</i>	121	58	121	49	49	46	13	121	121	121	121	121	121	69

<sup>a</sup>From Cutler-Glaeser-Vigdor data. <sup>b</sup>Calculated from ipums.org. Robust standard errors in parentheses.

Table 2. The effects of segregation on poverty and inequality among blacks and whites

	OLS: Effect of 1990 Dissimilarity Index		Main results: 2SLS RDI as instrument for Dissimilarity		Falsification: Reduced form effect of RDI among cities far from the South	
	(1)	(2)	(3)	(4)	(5)	(6)
	<u>whites</u>	<u>blacks</u>	<u>whites</u>	<u>blacks</u>	<u>whites</u>	<u>blacks</u>
<u>Within-race poverty and inequality</u>						
Gini index	<b>-0.079</b> (0.037)	<b>0.459</b> (0.093)	<b>-0.334</b> (0.099)	<b>0.875</b> (0.409)	-0.110 (0.066)	0.167 (0.424)
Poverty rate	<b>-0.073</b> (0.019)	<b>0.182</b> (0.045)	<b>-0.196</b> (0.065)	<b>0.258</b> (0.108)	-0.036 (0.035)	-0.136 (0.094)
	<u>white:black ratios</u>		<u>white:black ratios</u>		<u>white:black ratios</u>	
<u>Between-race inequality</u>						
90 white: 90 black	0.111 (0.086)		-0.131 (0.312)		<b>-0.443</b> (0.217)	
10 white: 10 black	<b>1.295</b> (0.249)		<b>2.727</b> (0.867)		-0.135 (0.532)	
90 white: 10 black	<b>1.172</b> (0.282)		<b>1.789</b> (0.758)		-0.449 (0.558)	
90 black: 10 white	-0.234 (0.131)		<b>-0.807</b> (0.384)		0.130 (0.248)	
N	121		121		29	

2SLS and reduced form estimates control for total track length. All outcomes except poverty rates are logged. Robust standard errors in parentheses.

Table 3. The effects of segregation on city demand

<i>Percent of residents who are in-migrants</i>		<i>Median rent</i>		<i>Median rent as a percent of income</i>		<i>Share of households with more than one person per room</i>	
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>OLS</i>							
<i>white</i>	<i>black</i>	<i>white</i>	<i>black</i>	<i>white</i>	<i>black</i>	<i>white</i>	<i>black</i>
<b>-0.153</b>	<b>-0.294</b>	<b>-314</b>	<b>-392</b>	<b>-8.535</b>	-3.490	<b>-0.062</b>	<b>-0.103</b>
<b>(0.032)</b>	<b>(0.052)</b>	<b>(84)</b>	<b>(76)</b>	<b>(1.337)</b>	(2.676)	<b>(0.014)</b>	<b>(0.022)</b>
<i>IV</i>							
<i>white</i>	<i>black</i>	<i>white</i>	<i>black</i>	<i>white</i>	<i>black</i>	<i>white</i>	<i>black</i>
<b>-0.155</b>	<b>-0.271</b>	<b>-636</b>	<b>-624</b>	<b>-16.666</b>	-3.416	<b>-0.116</b>	<b>-0.165</b>
<b>(0.073)</b>	<b>(0.115)</b>	<b>(276)</b>	<b>(157)</b>	<b>(3.643)</b>	(5.387)	<b>(0.037)</b>	<b>(0.047)</b>
<i>Falsification: Reduced form effect of RDI among cities far from the South</i>							
<i>white</i>	<i>black</i>	<i>white</i>	<i>black</i>	<i>white</i>	<i>black</i>	<i>white</i>	<i>black</i>
0.019	0.058	295	<b>326</b>	0.427	3.660	0.034	0.062
<b>(0.063)</b>	<b>(0.158)</b>	<b>(276)</b>	<b>(158)</b>	<b>(2.061)</b>	<b>(3.572)</b>	<b>(0.038)</b>	<b>(0.048)</b>

Robust standard errors in parentheses. All 2SLS and reduced form regressions control for total track length per square kilometer.

Table 4. The effects of segregation on human capital of 22- to 30-year-olds in 1980

<i>High School Dropout</i>		<i>High School Graduate</i>		<i>Some College</i>		<i>College graduate</i>	
<i>OLS</i>							
<i>white</i>	<i>black</i>	<i>white</i>	<i>black</i>	<i>white</i>	<i>black</i>	<i>white</i>	<i>black</i>
-0.003	<b>0.346</b>	<b>0.222</b>	<b>0.263</b>	-0.175	<b>-0.411</b>	-0.044	<b>-0.199</b>
<b>(0.032)</b>	<b>(0.081)</b>	<b>(0.043)</b>	<b>(0.118)</b>	<b>(0.040)</b>	<b>(0.117)</b>	<b>(0.065)</b>	<b>(0.097)</b>
<i>IV</i>							
<i>white</i>	<i>black</i>	<i>white</i>	<i>black</i>	<i>white</i>	<i>black</i>	<i>white</i>	<i>black</i>
-0.064	0.274	<b>0.373</b>	<b>0.643</b>	<b>-0.217</b>	<b>-0.745</b>	-0.092	-0.172
<b>(0.120)</b>	<b>(0.166)</b>	<b>(0.117)</b>	<b>(0.292)</b>	<b>(0.090)</b>	<b>(0.279)</b>	<b>(0.123)</b>	<b>(0.168)</b>
<i>Falsification: Reduced form effect of RDI among cities far from the South</i>							
<i>white</i>	<i>black</i>	<i>white</i>	<i>black</i>	<i>white</i>	<i>black</i>	<i>white</i>	<i>black</i>
0.089	0.100	-0.003	0.320	-0.021	-0.274	-0.065	-0.146
<b>(0.085)</b>	<b>(0.235)</b>	<b>(0.086)</b>	<b>(0.292)</b>	<b>(0.064)</b>	<b>(0.300)</b>	<b>(0.079)</b>	<b>(0.232)</b>

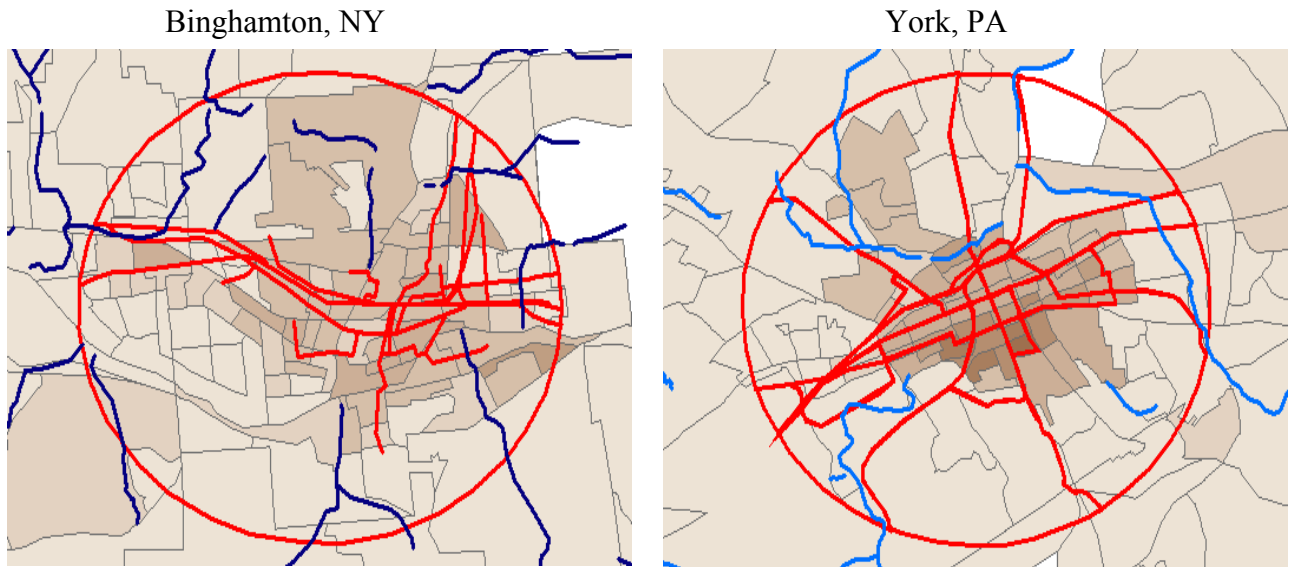
Robust standard errors in parentheses. Standard errors corrected for arbitrary nonindependence between observations within an MSA. All regressions include individual years-of-age indicators; all 2SLS and reduced form regressions control for total track length per square kilometer.

Table A. Mean Characteristics of Cities In and Out of Sample

<i>Cutler-Glaeser-Vigdor Variable</i>	<i>Not in sample</i>	<i>(Standard error)</i>	<i>In sample</i>	<i>(Standard error)</i>	<i>Difference in means</i>	<i>p-value of t-test for the difference in means</i>
<i>Isolation index—1890</i>	0.049	(0.007)	0.053	(0.008)	-0.004	0.698
<i>Isolation index--1940 tract</i>	0.355	(0.053)	0.318	(0.043)	0.037	0.586
<i>Isolation index--1940 ward</i>	0.234	(0.034)	0.198	(0.023)	0.036	0.361
<i>Isolation index—1970</i>	0.343	(0.034)	0.365	(0.023)	-0.022	0.578
<i>Isolation index—1990</i>	0.229	(0.022)	0.214	(0.017)	0.015	0.586
<i>Dissimilarity index—1890</i>	0.385	(0.032)	0.383	(0.024)	0.002	0.956
<i>Dissimilarity index--1940 tract</i>	0.736	(0.029)	0.742	(0.019)	-0.006	0.862
<i>Dissimilarity index--1940 ward</i>	0.57	(0.032)	0.57	(0.022)	0.000	0.990
<i>Dissimilarity index—1970</i>	0.744	(0.015)	0.74	(0.012)	0.004	0.843
<i>Dissimilarity index—1990</i>	0.574	(0.016)	0.569	(0.012)	0.005	0.798
<i>Percent black—1890</i>	0.03	(0.005)	0.027	(0.003)	0.004	0.532
<i>Percent black—1940</i>	0.058	(0.007)	0.041	(0.005)	0.018	0.034
<i>Percent black—1970</i>	0.056	(0.006)	0.062	(0.005)	-0.006	0.477
<i>Percent black—1990</i>	0.067	(0.006)	0.061	(0.005)	0.005	0.480
<i>Population—1890</i>	129,829	(56323.8)	66,044	(19199.4)	63,785	0.242
<i>Population—1940</i>	390,895	(170643.4)	203,676	(40731.8)	187,219	0.206
<i>Population—1970</i>	919,239	(261007.3)	681,599	(129697.5)	237,640	0.375
<i>Population—1990</i>	689,768	(135048.7)	590,189	(96574.5)	99,580	0.538
<i># of wards—1890</i>	17.778	(3.724)	13.421	(1.731)	4.357	0.288
<i># of wards—1940</i>	15.929	(2.641)	14.122	(1.440)	1.807	0.519
<i># of tracts—1940</i>	146.059	(53.033)	103.348	(21.920)	42.711	0.417
<i># of tracts—1970</i>	211.118	(61.515)	161.811	(29.971)	49.307	0.432
<i># of tracts—1990</i>	203.687	(44.957)	137.496	(20.894)	66.191	0.131
<i>Total area—1900</i>	19,283	(5711.826)	11,764	(1755.343)	7,519	0.147
<i>Total area—1940</i>	32,855	(9499.416)	27,137	(6214.421)	5,718	0.610
<i>Total area—1970</i>	2,344	(604.403)	1,615	(201.438)	729	0.184
<i>Total area—1990</i>	2,387	(469.458)	1,826	(262.051)	561	0.262
<i>Per capita street car passengers—1915</i>	204.214	(15.422)	179.002	(19.461)	25.212	0.334
<i>% of blacks employed as servants-1915</i>	0.21	(0.015)	0.207	(0.013)	0.002	0.900
<i>Increase in urban mileage in 1950s</i>	0.237	(0.019)	0.248	(0.021)	-0.011	0.713
<i># of local governments—1962</i>	62.925	(10.281)	55.551	(7.477)	7.374	0.558
<i>Inter-governmental revenue sharing—1962</i>	0.262	(0.011)	0.248	(0.007)	0.014	0.261
<i>Centralization index—1990</i>	0.741	(0.016)	0.77	(0.019)	-0.029	0.264
<i>Clustering index—1990</i>	0.207	(0.015)	0.177	(0.021)	0.03	0.235
<i>Concentration index--1990</i>	0.556	(0.020)	0.656	(0.022)	-0.1	0.001
<i>Income segregation—1990</i>	0.23	(0.006)	0.247	(0.004)	-0.017	0.061
<i>Black income segregation—1990</i>	0.554	(0.012)	0.546	(0.009)	0.008	0.573
<i>Educational exposure index—1990</i>	-0.084	(0.007)	-0.088	(0.005)	0.004	0.602
<i>Manufacturing share--1990</i>	0.172	(0.009)	0.189	(0.006)	-0.017	0.118
<i>Median income</i>	31,484	(716.257)	31,606	(572.361)	-123	0.893
<i>Median education</i>	-0.162	(0.019)	-0.143	(0.013)	-0.019	0.392
<i>Share of moms who are single</i>	0.236	(0.018)	0.26	(0.016)	-0.024	0.339
<i>Average commuting time</i>	0.823	(0.520)	-0.437	(0.363)	1.26	0.044
<i>Person-weighted density</i>	1808.075	(338.133)	1270.52	(75.120)	537.555	0.049
<i>N</i>	246		121			



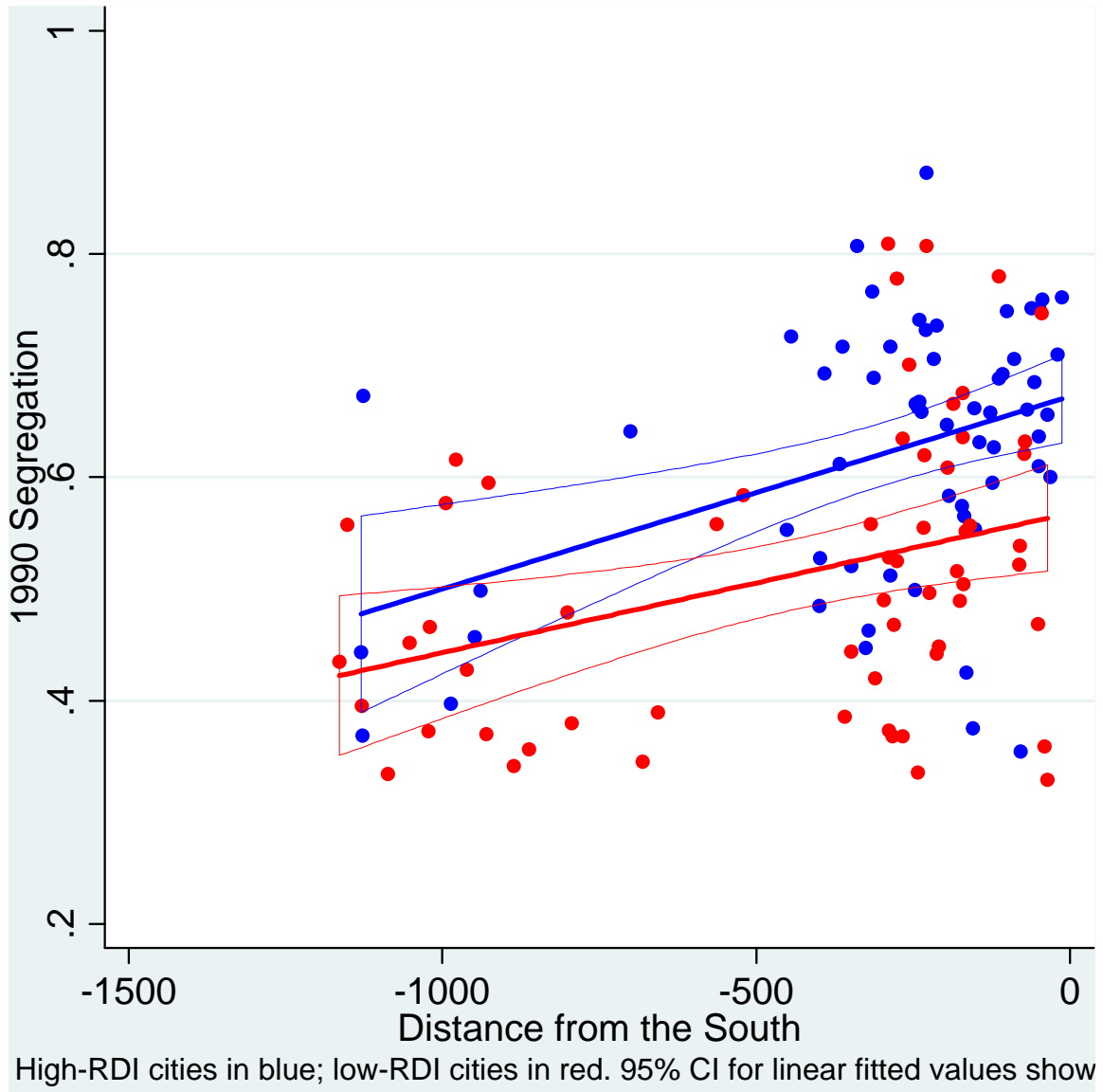
**Figure 2. The Natural Experiment—2 Examples**



19<sup>th</sup> century railroads, shown in red within the 4-km radius historical city center, divide York, PA into a larger number of smaller neighborhoods than do the railroads in Binghamton, NY. Thus, even though the two cities had similar total lengths of track, similar African-American population inflows, and similar economic bases, York became more segregated, as can be seen from the smaller, more concentrated area of African-Americans near the railroad-defined neighborhoods at the city's center. Rivers in blue.



Figure 4. Relationship between Distance to the South and Segregation, by RDI



## Figure A. Measuring the railroads of Anaheim, CA

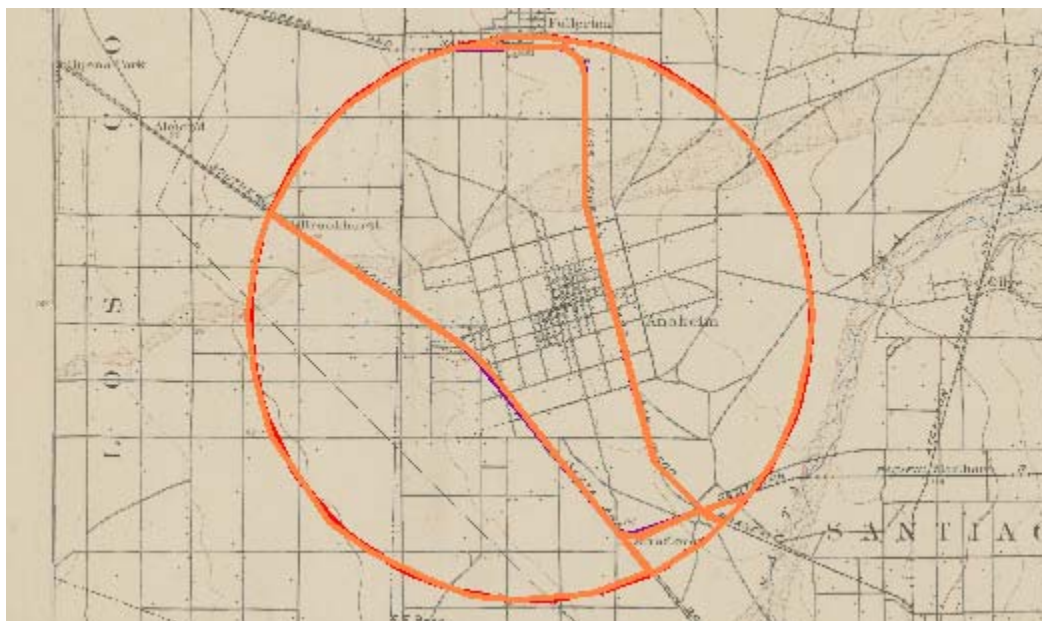


Figure A1. Neighborhoods are defined as polygons created by the intersection of railroads with each other and with the perimeter of a 4-kilometer radius circle that around the historical city center. Anaheim contains five neighborhoods, shown here in orange on the 1894 USGS map. The area of each neighborhood is calculated and used to calculate an RDI measuring the subdivision of the historical city center.

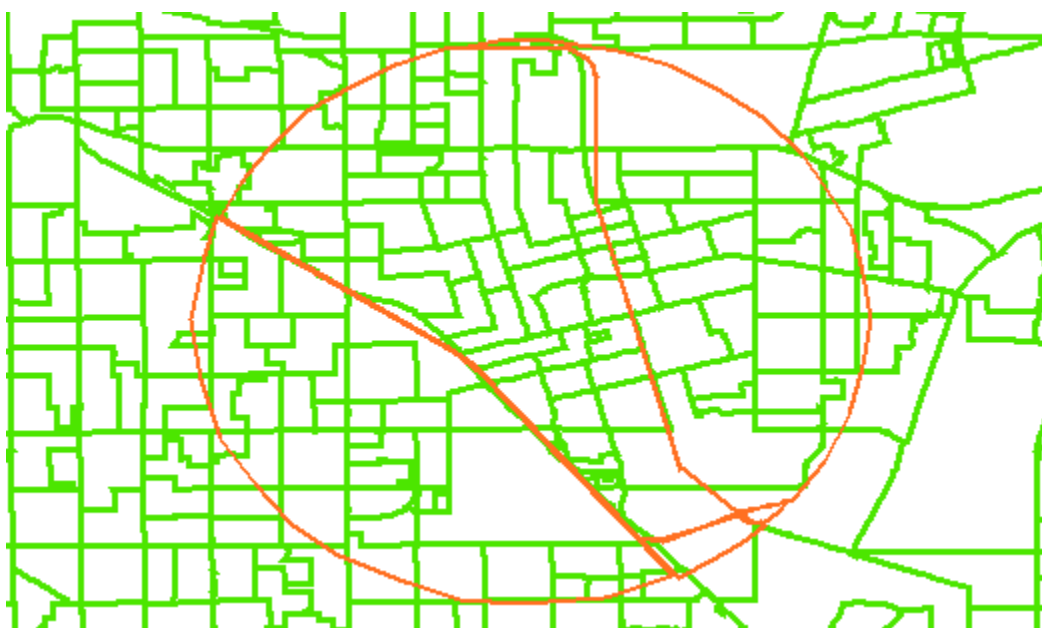


Figure A2. The 1894 neighborhoods are shown projected onto a Year 2000 census map; modern tract borders are shown in green. Note that current neighborhood borders, as defined by the US Census Bureau in 2000, closely follow historical railroad tracks.