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

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A striking negative correlation exists between an area's residential racial segregation and its population characteristics, but it is recognized that this relationship may not be causal. I present a novel test of causality from segregation to population characteristics by exploiting the arrangements of railroad tracks in the nineteenth century to isolate plausibly exogenous variation in areas' susceptibility to segregation. I show that this variation satisfies the requirements for a valid instrument. Instrumental variables estimates demonstrate that segregation increases metropolitan rates of black poverty and overall black-white income disparities, while decreasing rates of white poverty and inequality within the white population. (*JEL* I32, J15, N31, N32, N91, N92, R23)

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 US population, the average urban African American lives in a neighborhood that is more than 50 percent black (Edward L. Glaeser and Jacob L. 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 (Douglas S. Massey and Nancy A. Denton 1993).[1]

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

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[1] Throughout the paper I use the term "city" to refer to a metropolitan area. My unit of measurement in the empirical analysis is the metropolitan statistical area (MSA).

market and human capital outcomes (e.g., Massey and Denton 1993; William Julius Wilson 1996; David M. Cutler and Edward L. Glaeser 1997; David Card and Jesse Rothstein 2007). However, Cutler and Glaeser (1997), among others, have expressed skepticism about this type of measure 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.

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 nineteenth-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 twentieth 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. The historical record (Arthur Mellen Wellington 1911; Jeremy Atack and Peter Passell 1994) on the placement of railroad tracks suggests that local social or economic concerns did not drive configuration choices, but rather, businessmen and engineers optimized placement with respect to the orientation of nearby destinations and variation in ground slope (see Appendix A for further discussion of these determinants). I test for the orthogonality of early social and economic characteristics of towns to the configuration of railroads, and for the other requirements for a valid instrument (Guido W. Imbens and Joshua D. Angrist 1994), below. This instrumental variable strategy allows me to identify the causal effect of segregation on net city-level outcomes.

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 populations with lower poverty rates. Segregation increases inequality between a city's blacks and whites, and lowers average outcomes within the city's black community, while reducing inequality within the city's white community.

A comparison of two cities, Binghamton, NY, and York, PA, illustrates the relationship I identify between plausibly exogenous railroad configuration, segregation, and black-white population disparities. Both places developed in the mid-nineteenth century by serving key "middleman" purposes between the Great Lakes, farms, and coal country to the west and north and major ports to the east. Both were small flat areas surrounded by hills, with access to water for steam engines (both lie on the Susquehanna River) but no large bodies to navigate, that were therefore the most

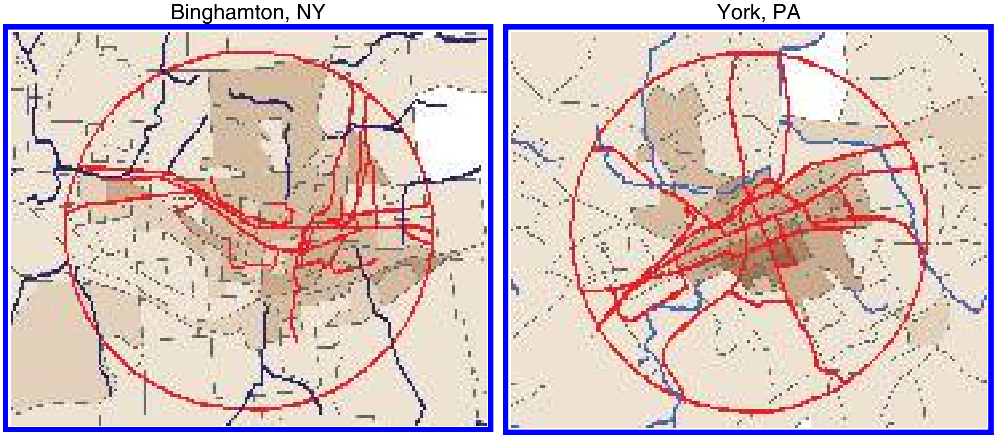


Figure 1. The Natural Experiment—2 Examples

Notes: Nineteenth century railroads, shown in thick gray lines (red online) 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 black (blue online).

advantageous places in their respective geographies to serve as a nexus for railroads. Because of their advantageous geography, several railroad companies built routes through each town during the highly competitive mid-1800s railroad booms, and by 1900 each town had roughly equal total linear kilometers of railroad track running through them. However, because of the configurations of outlying hills, the routes for the tracks chosen in each city differed (this point is developed in Appendix A). These different layouts of human-made railroad barriers would prove important when both places experienced comparable, substantial changes in African American population during the Great Migration.

In many cities it is self-evident that railroads tend to define neighborhood boundaries. For York and Binghamton, the relationship is illustrated in Figure 1. It shows, for each city, 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). Although explanations for why railroads tend to delineate neighborhoods 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 (Thomas C. Schelling 1963). A railroad provides a clear demarcation that facilitates collective agreement on neighborhood boundaries by 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 each city grew during the Great Migration, the cities' ghetto areas had to expand if segregation was to be maintained. As Schelling (1963) describes the function of a visible 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" (Schelling 1963, 71).[2] 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 such as York 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, such as Binghamton (where tracks were tightly clustered, leaving some areas too long and narrow to encompass neighborhoods and others too wide open to create meaningful population restrictions), 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. Eventually, all cities grew enough to exceed the boundaries of the railroad tracks laid through their center. In cities like York, where segregation had become a strong pattern, local expectations about neighborhood tipping could cause segregation to permeate even outlying areas and new suburbs (Schelling 1971). 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. Measures of segregation that cover neighborhoods throughout the respective metropolitan areas show that their respective patterns of racial isolation, once developed, persist even in more newly built areas. York's overall MSA-level segregation index[3] is 1.5 standard deviations above Binghamton's.

The typical correlation between urban problems and segregation is borne out in this pair of cities as well. In York, race relations have been substantially worse, and resistance by whites to integration led to a race riot in 1969. By contrast, the gradual granting of equality to black residents of Binghamton proceeded without notable incident. By 1990, there was no gap in college degrees between black and white residents of Binghamton, while whites in York were twice as likely as local blacks to have a college education.

Of course, the historical paths of any two cities are overdetermined, limiting the ability to draw inferences about exogeneity or about the importance of specific causal factors from a particular case study. The goal of the rest of this paper is to demonstrate the exogeneity of railroad configuration and the causality of relationships between railroad configuration, segregation, and city characteristics quantitatively. I will demonstrate that the divergent patterns of York and Binghamton are no historical fluke, but rather are illustrative of the broader relationship between railroad division, African American in-migration, and urban outcomes. In particular, Northern and Western cities with high railroad division indexes (RDI) that are close enough to the South to have experienced significant black inflows during the Great Migration are today more segregated than otherwise comparable cities, have higher

[2] Thanks to Glenn Loury for pointing me to this reference.
[3] This index, called the index of dissimilarity, is described in Section II.

rates of black poverty and overall black-white income disparities, and have lower rates of white poverty and inequality within the white population.

I present evidence showing that there is little possibility of contamination of the railroad division instrument. This measure satisfies the instrumental variable validity requirements outlined in Imbens and Angrist (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.[4]

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, York 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 I, I discuss the historical and conceptual framework, and provide theoretical motivation for the instrumental approach. In Section II, I summarize the data. In Section III, I present the main results and conduct robustness and falsification checks of my results. In Section IV, I conclude.

I. 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 nineteenth 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.[5] This observation is particularly relevant to this analysis, because the bulk of railroad tracks were laid prior to 1900

[4] 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.

[5] 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.

(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.[6]

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; Robert C. Weaver 1955) became highly segregated as their black populations grew (Cutler, Glaeser, and Vigdor 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, 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 toward 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 nonrandom 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

[6] There were other stigmatized groups in those cities in the nineteenth 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 United States 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" (Massey and Denton 1993, 32–33).

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

blacks through peer effects. These individual treatment effects, he claims, produce a black "underclass" in segregated cities. William J. Collins and Robert A. 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.[7]

A separate branch of the literature has focused on selection. Patrick Bayer, Hanming Fang, and Robert McMillan (2009) and Nathaniel Baum-Snow (2007) argue that, through neighborhood choice, individuals reveal preferences for areas that are racially and economically homogeneous. Leah Platt Boustan (2010) 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 (Lawrence D. Bobo et al. 1994), as well as revealed preferences for integrated cities (Cutler, Glaeser, and Vigdor 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 (but not blacks) have tastes for segregation and that segregation affects the level of blacks' (but not whites') 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 allowing segregation to affect the human capital of whites as well as blacks, and allowing both blacks and whites to have 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 allow the aggregate effects of segregation on the income distributions of cities' black and white populations to 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.

[7] 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, George J. Borjas (1995) finds positive relationships between segregation and outcomes for US immigrants. There does not appear to be any such evidence for African Americans, however.

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 2A). At time zero, corresponding to the Great Migration, each city is randomly assigned the same population of measure one, β 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, μ_{Hb}, is lower than the average among the white population, μ_{Hw}.

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

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

where λ_1 is the individual's human capital level; μ_{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.[8] According to equation (1), the production of offspring human capital depends not only on an individual's own human capital but also, when α is strictly greater than zero, on the average human capital of neighborhood residents.[9] 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 μ_{Hb}, while whites are exposed to $\mu_{Hw} > \mu_{Hb}$.

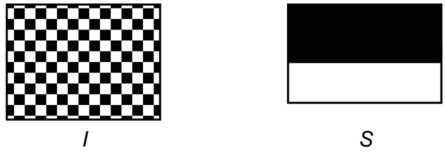
If residents cannot move between cities, then the skill gap between blacks and whites will persistently be larger (weakly larger, if $\alpha = 0$) in *S* than in *I*. Over time, the μ_H of blacks and whites will weakly converge in *I*, as blacks and whites are exposed to the same average human capital 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 *I* (Figure 2b). 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 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*.[10]

[8] For a discussion of other possible permutations of the peer-effects equation, see Philip J. Cook and Jens Ludwig (2006).

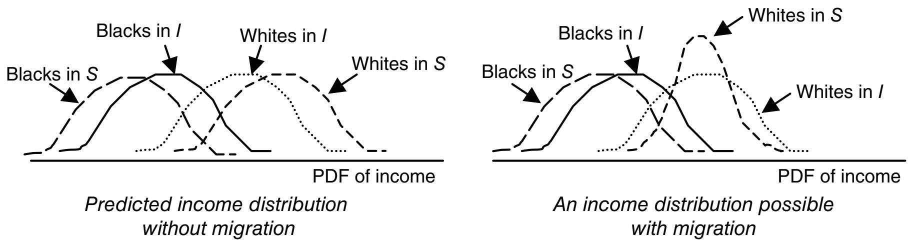
[9] Human capital might depend on the neighborhood in which one grows up because of a literal "peer effect" (Roland J. M. Benabou 1996) or because of neighborhood characteristics proxied by peers (Card and Rothstein 2006), e.g., school or health services funding, the political power of the neighborhood, whether a chemical dumping ground is sited in the neighborhood, how connected residents are to job networks, the neighborhood crime rate, etc. In ongoing work, 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.

[10] 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.

Panel 2A. City *I* is perfectly integrated; city *S* is perfectly segregated.



Panel 2B. Income distribution after multiple generations, assuming peer effects in human capital accumulation



Panel 2C. Implications of outcome-demand pairs with migration

OBSERVED:		IMPLIED:	
Group's average rent/migration in city *S* vs. *I*	Group's average outcomes in city *S* vs. *I*	Group's average tastes for segregation	Treatment effect of segregation on group's human capital
Lower	Better	−	+
Higher	Worse	+	−
Lower	Worse	at least one −	
Higher	Better	at least one +	

Figure 2. The Ideal Experiment

Notes: Panel C shows the implications of outcome-demand pairs in segregated city *S* relative to integrated city *I*. For example (row 1), if a group has low demand for segregated cities in spite of the fact that segregation is associated with better outcomes for that group, then that implies that the group must on average have distastes for segregation.

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 ghettoes good or bad?," for society as a whole.

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).

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.[11] Second, when moving is possible, any tastes for integration σ (positive or negative) may affect city composition, and could cause differences in observed skill distribution by city even if, in fact, $\alpha = 0$.[12]

If both α and σ 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.[13] Within race and skill, individuals will sort by preference σ for integration, so that the individual of race r and skill λ 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 σ 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) re-sort 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 2, panel C.

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.

2. Each city would be randomly assigned black residents from the initial black skill distribution and white residents from the initial white distribution.

[11] 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.

[12] 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 that is impossible in the static model).

[13] 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).

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.[14] The variation I exploit is created by idiosyncrasies in the layout of railroad tracks that ease the collective definition of neighborhoods.

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 B. Briefly, I collected from nineteenth-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 1).[15] The choice of this area size provides the advantage that, while all the cities studied exceed this perimeter today, about 75 percent 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 1).[16]

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.

[14] 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 (part 3) and selection effects (part 4).

[15] 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.

[16] While it is easy to check this relationship visually, I am unable to create a digital correspondence between nineteenth-century railroads and current census tract boundaries, because the two sets of information come from different maps and railroads are projected onto current census maps with error.

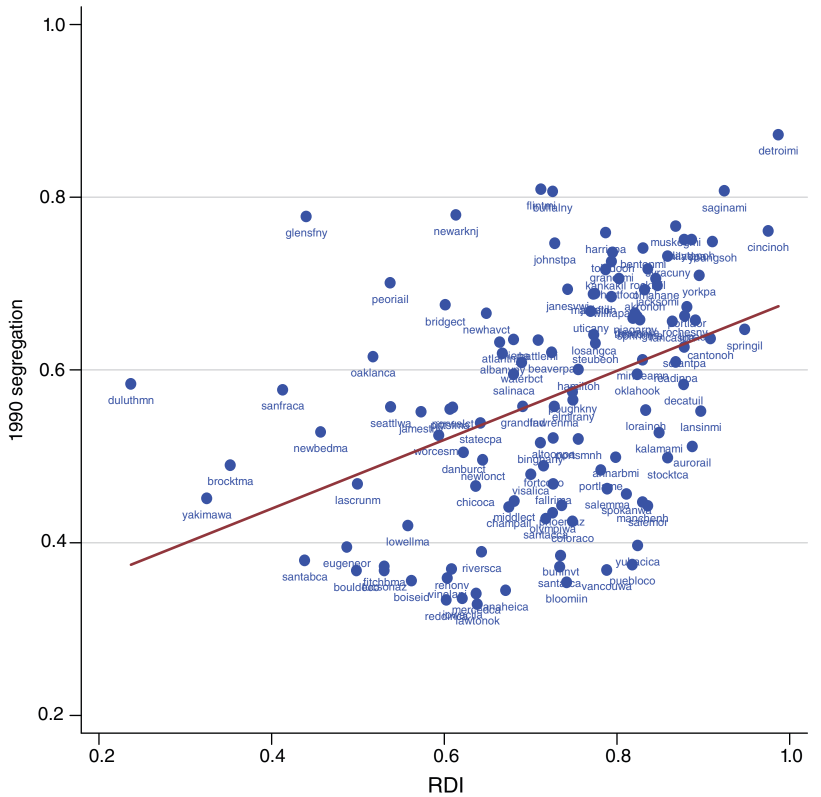


Figure 3. Full Sample Relationship between RDI and Segregation

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

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 percent 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 0, then the RDI would equal 1. The more subdivided a city, the more possible boundaries between groups were 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 on the *y*-axis, demonstrates a positive relationship between RDI and segregation.

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. 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[17]—strongly predicts black inflows and therefore may function as a proxy for demand for segregation.[18]

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 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.

II. Data and Empirical Measures

A. *Segregation Measures*

In addition to the maps described in Appendix B, the major data sources are US Census Bureau reports on metropolitan demographics (various years), individual Census microdata from ipums.org (Steven Ruggles et al. 2004), 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

$$\text{Index of dissimilarity} = \frac{1}{2}\sum_{i=1}^{N}\left|\frac{black_i}{black_{total}} - \frac{nonblack_i}{nonblack_{total}}\right|, \tag{3}$$

where again $i = 1 \ldots N$ is the array of census tracts in the area. It can be considered the answer to the question, "What percent of blacks (or nonblacks) 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

[17] 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.

[18] 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).

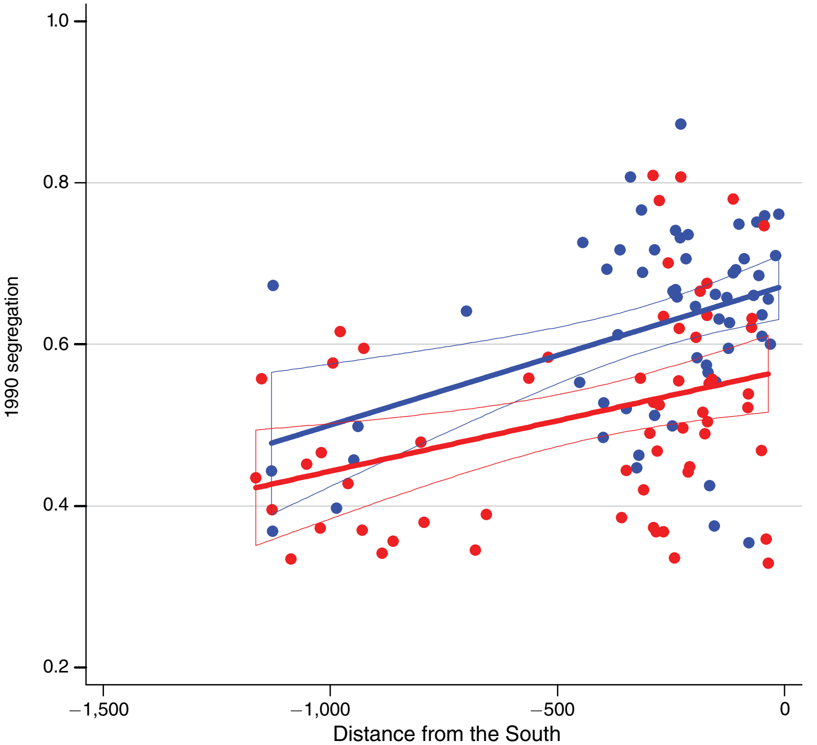


Figure 4. Relationship between Distance to the South and Segregation, by RDI

Notes: High-RDI cities and best-fit line in dark gray (blue online); low-RDI cities and best-fit line in light gray (red online). Ninety-five percent CI for linear fitted values shown.

index can range from zero to one. Since tracts contain roughly 4,000 individuals apiece, a city with 62 tracts that is 10 percent black would average a dissimilarity index of 0.029 (standard deviation 0.0028) if African American households were randomly assigned across tracts.[19] In reality, the mean index is 0.57 (standard deviation 0.135).

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 nineteenth and twentieth century segregation indices as well as metropolitan characteristics from Cutler and Glaeser (1997) and from Cutler, Glaeser, and Vigdor (1999).[20]

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 B). Using this control

[19] Simulations by author of 1,000 cities with size and black population equal to the median sample statistics.

[20] Results are similar to those shown here when using other measures of segregation, including isolation, clustering, and concentration indexes.

assures that RDI does not simply capture the amount of railroad track in the city,[21] but rather represents the configuration of track conditional on total track.

For the main outcomes, I measure aggregate city[22] 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.

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[23] 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.

III. 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 MSA level,

$$Seg = \alpha_1 RDI + \alpha_2 \mathbf{X} + \mu \tag{4}$$

$$Y = \beta_1 Seg + \beta_2 \mathbf{X} + \varepsilon, \tag{5}$$

and then estimated using two-stage least squares analysis. The right-hand side variable of interest in equation (5), *Seg*, represents an MSA's current level of segregation.

[21] The density of railroads might be directly correlated with city outcomes for many reasons, such as industrial composition or physical attractiveness.

[22] I collect city outcomes from published census reports (US 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.

[23] 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.

Table 1—Testing RDI as an Instrument

	First stage	Falsification checks					
		1910 city characteristics					
Outcome:	1990 dissimilarity index[a] (1)	Physical area (square miles/ 1,000)[a] (2)	Pop. (1,000s)[b] (3)	Ethnic dissimilarity index[a] (4)	Ethnic isolation index[a] (5)	Percent black[b] (6)	Street-cars per cap. (1,000s) (1915)[a] (7)
RDI	**0.357**	−3.993	0.666	0.076	0.027	−0.0006	−0.132
	(0.088)	(11.986)	(1.36)	(0.185)	(0.070)	(0.0100)	(0.183)
Track length per square kilometer	18.514	−574.401	75.553	15.343	−12.439	**9.236**	3.361
	(10.731)	(553.669)	(135)	(53.249)	(17.288)	**(0.650)**	(20.507)
Mean of dependent variable	0.568	14.626	1,527	0.311	0.055	1.442 percent	179
N	121	58	121	49	49	121	13

	Falsification checks						
	1920 city characteristics						
Outcome:	Percent black[b] (8)	Percent literate[b] (9)	Labor force participation[b] (10)	Percent of empl. in trade[b] (11)	Percent of empl. in manufacturing[b] (12)	Percent of empl. in railroads[b] (13)	1990 income seg.[a] (14)
RDI	0.0132	0.052	0.029	−0.080	0.185	−0.074	0.032
	(0.0091)	(0.030)	(0.025)	(0.097)	(0.138)	(0.068)	(0.032)
Track length per square kilometer	**9.119**	0.178	**−3.432**	0.230	19.740	1.583	−2.504
	(0.615)	(0.883)	**(1.560)**	(3.122)	(11.023)	(2.417)	(1.626)
Mean of dependent variable	1.558 percent	95.9 percent	41.9 percent	5.77 percent	46.2 percent	0.32 percent	0.217
N	121	121	121	121	121	121	69

Notes: Robust standard errors in parentheses.
[a] From Cutler-Glaeser-Vigdor data; sample limited to what that dataset provides.
[b] Calculated from ipums.org; full sample represented.

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.

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.[24]

[24] Results available upon request.

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, but also 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 nineteenth-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 A) provide no support for that possibility. Recent analysis by Atack and Passel (1994), as well as contemporaneous analysis by Wellington (1911), emphasize orientation of nearby destinations and ground slope over social and economic concerns (which were considered negligible at the time).

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 2–7 show tests of the predictive power of the RDI for a small sampling of characteristics of cities prior to the start of the Great Migration (tests of additional characteristics are available upon request[25]). These include, for 1910, physical size, population, dissimilarity, and isolation measures for the foreign-born versus native-born population,[26] and percentage black, and for 1915, streetcars per capita. None of the six coefficients on RDI are significant. Of the 12 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 8–13 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 and 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 6 coefficients on RDI are significant, and of the 12 total coefficients estimated, only two—the relationships of track length with percent black and 1920 labor force participation—are significant at

[25] I have tested the relationship between RDI, track length, and city characteristics in each decade from 1890–1940 including: total population, black population, foreign-born population, number of political wards, and physical size. Of the 60 coefficients in these 30 regressions, only 4 are significant at conventional levels.

[26] 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 ethnic 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 0.37.

conventional levels. Taken together, the null results of all 12 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 some 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, and also have replicated the results within both the Midwest ($N = 40$) and the Northeast ($N = 49$) separately[27]. While the standard errors of the estimates increase in these replications, the results remain essentially the same.[28] 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 A for a thorough discussion of the historical causes of railroad configuration, orientation of nearby destinations and ground slope). 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.[29] 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. Third, RDI could, today, affect urban outcomes through some channel other than segregation, such as governance structure or economic structure. In Table 3, I control for other current city characteristics, and the results are robust, however.

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. First, 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. Second, it must be the case that railroad configuration affected city outcomes through segregation, not through some other channel. The fact 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, lends credibility to this assumption. Third, 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.

[27] Because the Western sample consists mostly (25 of 32 cities) of cities that are far from the South (for which, as I argue and demonstrate below, RDI should and does have little predictive power), the results cannot be replicated using a subsample that consists only of Western cities.

[28] Results available upon request.

[29] Income segregation is insignificant in the United States relative to racial segregation. The highest of any metropolitan dissimilarity index for income in 1990 is 0.28, while the *lowest* 1990 dissimilarity for African Americans is 0.33.

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 (Imbens and Angrist 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 measure the causal effects of RDI-induced segregation on the income distribution of city residents. These causal effects incorporate both direct treatment effects on individual human capital and indirect selection effects on cities' populations through migration.[30] In the next section, I will analyze migration, housing demand, and the human capital of young adults in order to gather suggestive evidence on to what extent the causal aggregate impacts I identify 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 1–4 of Table 2.[31] 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 3.6 percentage point increase in black poverty and a 12.3 percent increase in the black Gini index.[32] 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.

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 ninetieth 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 tenth percentile have incomes 107 percent higher than blacks at the tenth percentile; a one-standard-deviation increase in segregation causes that gap to increase to

[30] 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.

[31] Results are similar when indicators for census region are included.

[32] Results using 90:10, 90:50, and 50:10 ratios are similar.

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 1990 dissimilarity		Falsification: Reduced form effect of RDI among cities far from the south	
	Whites	Blacks	Whites	Blacks	Whites	Blacks
Outcome:	(1)	(2)	(3)	(4)	(5)	(6)
Within-race poverty and inequality						
Gini index	**−0.079**	**0.459**	**−0.334**	**0.875**	−0.110	0.167
	(0.037)	**(0.093)**	**(0.099)**	**(0.409)**	(0.066)	(0.424)
Poverty rate	**−0.073**	**0.182**	**−0.196**	**0.258**	−0.036	−0.136
	(0.019)	**(0.045)**	**(0.065)**	**(0.108)**	(0.035)	(0.094)
	White:black ratios		White:black ratios		White:black ratios	
Between-race inequality						
90 white: 90 black	0.111		−0.131		**−0.443**	
	(0.086)		(0.312)		**(0.217)**	
10 white: 10 black	**1.295**		**2.727**		−0.135	
	(0.249)		**(0.867)**		(0.532)	
90 white: 10 black	**1.172**		**1.789**		−0.449	
	(0.282)		**(0.758)**		(0.558)	
90 black: 10 white	−0.234		**−0.807**		0.130	
	(0.131)		**(0.384)**		(0.248)	
N	121		121		29	

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

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 ninetieth percentile of blacks and the tenth 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 cities that are close to the South, 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 that are far 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. [33] Of the eight

[33] Four hundred miles is selected as the cutoff for the falsification subsample because it represents the seventy-fifth percentile value of distance from the South. All falsification test estimates for Tables 2, 3, and 4 are extremely

coefficient estimates, only one is significant at conventional levels, and four 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. Given these estimates, any candidate explanation for an effect of RDI on urban outcomes through a channel other than segregation would have to account not only for opposite-sign effects on blacks and on whites but also for zero effects far from the South. This falsification check thus lends credibility to the claim that RDI drives poverty and inequality through segregation and not through other channels.

Table 3 presents robustness checks that replicate the main 2SLS estimates of the effect of segregation on black and white Gini indexes and poverty rates while also controlling for a variety of other city characteristics. The top panel of Table 3 presents estimates while controlling for 1990 city characteristics, including: population; percent black, labor force participation by both whites and blacks; education of both whites and blacks; and manufacturing share. In addition, it presents an estimate, for a limited number of cities for which Cutler, Glaeser, and Vigdor provide data, controlling for the number of local governments (breakup of the metropolitan area into suburbs) formed by 1962. Most of these city characteristics are correlated with racial segregation and with poverty and inequality. If controlling for these characteristics were to alter the instrumented relationship between segregation and poverty and inequality, it would suggest that perhaps the instrument affects those other characteristics directly, and affects poverty and inequality only indirectly. In fact, the estimates of segregation, instrumented with RDI, on city-level poverty and inequality are robust to these controls; none of the point estimates are significantly different from the main estimates in Table 2. The point estimates actually tend to increase slightly (although the standard errors do as well). These results lend confidence that RDI is impacting poverty and inequality through segregation, and not through some other channel.

The second panel of Table 3 presents estimates while controlling for 1920 characteristics, at the time when the Great Migration had just begun. Again, the estimates are highly stable and all remain statistically significant. In addition, I have used these pre-characteristics of cities to generate a propensity score for the probability of having an above-median RDI. The resulting set of scores satisfies the balancing property, and, in the bottom row of Table 3, I present estimates when controlling for this propensity score. Again, the results are highly similar, although the estimates for African Americans become only marginally significant.[34]

Taken together, the results in Tables 2 and 3 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.

similar using distances as low as 300 miles from the South or as high as 500 miles from the South as cutoffs.

[34] Radius, kernel, and nearest-neighbor matching methods using the propensity score all give results comparable to those in Tables 2 and 3.

TABLE 3—ROBUSTNESS CHECKS: 2SLS EFFECTS OF 1990 SEGREGATION, CONTROLLING FOR CITY-LEVEL CHARACTERISTICS

	Outcome: Gini index		Outcome: Poverty rate	
	Whites (1)	Blacks (2)	Whites (3)	Blacks (4)
With controls for 1990 city characteristics				
Population	**−0.371**	**0.898**	**−0.212**	**0.291**
	(0.107)	**(0.434)**	**(0.068)**	**(0.109)**
Percent black	**−0.473**	0.886	**−0.241**	**0.360**
	(0.171)	(0.547)	**(0.097)**	**(0.141)**
Education	**−0.361**	0.887	**−0.162**	0.222
	(0.148)	(0.664)	**(0.080)**	(0.174)
Share employed in manufacturing	**−0.359**	1.106	**−0.272**	0.219
	(0.175)	(0.777)	**(0.124)**	(0.195)
Labor force participation	**−0.295**	**0.907**	**−0.142**	**0.321**
	(0.092)	**(0.393)**	**(0.040)**	**(0.105)**
Number of local governments ($N = 69$)	−0.386	**0.792**	−0.118	**0.519**
	(0.203)	**(0.277)**	(0.077)	**(0.169)**
With controls for 1920 city characteristics				
Population	**−0.374**	**0.900**	**−0.214**	**0.281**
	(0.106)	**(0.442)**	**(0.071)**	**(0.115)**
Percent black	**−0.364**	**0.896**	**−0.199**	**0.296**
	(0.114)	**(0.434)**	**(0.069)**	**(0.109)**
Literacy	**−0.312**	**1.028**	**−0.164**	**0.270**
	(0.107)	**(0.469)**	**(0.061)**	**(0.124)**
Share employed in manufacturing	**−0.398**	**0.900**	**−0.212**	**0.304**
	(0.129)	**(0.478)**	**−(0.080)**	**(0.121)**
Labor force participation	**−0.304**	**0.848**	**−0.187**	**0.243**
	(0.084)	**(0.369)**	**(0.061)**	**(0.104)**
Control for propensity score	**−0.412**	1.038	**−0.189**	0.304
	(0.181)	(0.639)	**(0.094)**	(0.177)

Notes: $N = 121$ except where noted. All estimates control for total track length. Gini indexes are logged. Robust standard errors in parentheses. 1990 education controls include percent of blacks and of whites who have less than a high school diploma, exactly a high school diploma, some college, and college diploma. Propensity score to have an above-median RDI generated based on: 1920 population, 1920 percent black, 1920 share of employment in manufacturing, 1920 literacy, 1920 labor force participation, and distance from the South.

Below I conduct a suggestive exploration of these different possible channels for the effect of segregation on city characteristics.

D. *Suggestive Tests for Treatment and Selection Mechanisms*

Selection.—Table 4 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 4 suggests that segregated cities are, in fact, in less demand.

Table 4—The Effects of 1990 Segregation on 1990 City Demand

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

Notes: Robust standard errors in parentheses. All 2SLS and reduced form regressions control for total track length per square kilometer. $N = 121$ for top two panels; $N = 29$ for falsification check on subset of cities at least 400 miles from the South.

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.[35]

The bottom panel of Table 4 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, railroad division 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, although at most suggestive, means that selection is a plausible explanation for at least part of the variation in human capital. As laid out in Figure 2C, 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

[35] In long-run equilibrium, the housing stock should adjust to demand so that rents are equalized across cities. However, the housing stock evolves slowly. If tastes for segregation have changed since the civil rights era (i.e., post-1970), it is likely that 1990 rents reflect partial equilibrium in the housing market.

Table 5—The Effects of 1980 Dissimilarity on Human Capital of 22- to 30-Year-Olds in 1980

	Outcome: Share who are high school dropouts		Outcome: Share who are high school graduates		Outcome: Share who have some college		Outcome: Share who are college graduates	
	White	Black	White	Black	White	Black	White	Black
OLS								
	0.025	**0.354**	**0.249**	**0.262**	**−0.153**	**−0.355**	−0.121	**−0.262**
	(0.039)	**(0.087)**	**(0.060)**	**(0.137)**	**(0.042)**	**(0.139)**	(0.089)	**(0.126)**
IV								
	−0.144	**0.431**	**0.458**	**0.652**	−0.174	**−0.786**	−0.140	−0.297
	(0.149)	**(0.201)**	**(0.174)**	**(0.333)**	(0.110)	**(0.321)**	(0.152)	(0.219)
Falsification: Reduced form effect of RDI among cities far from the South								
	0.089	0.100	−0.003	0.320	−0.021	−0.274	−0.065	−0.146
	(0.085)	(0.235)	(0.086)	(0.292)	(0.064)	(0.300)	(0.079)	(0.232)

Notes: Robust standard errors, corrected for arbitrary nonindependence of observations within a city, are in parentheses. All 2SLS and reduced form regressions control for total track length per square kilometer. Instrumented variable is 1980 dissimilarity; instrument is RDI. Observations are shares in each educational category within city × single year of age × race cells.

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.

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 5, rely on 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 1980 segregation[36] in the city in which a young person born between 1950 and 1958 resided five years ago predicts significantly lower educational attainment among blacks and a concentration of attainment among whites at the exactly-high school graduate level. A one standard deviation increase in segregation is associated with a significant 6 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 significant 11 percentage point decrease in the share of blacks with some college.

[36] Cutler, Glaeser, and Vigdor provide segregation measures for only 100 of the 121 cities in the sample for 1980, reducing the power of these estimates. More severe reductions occur when using 1970 ($N = 75$), 1960 ($N = 61$), or earlier measures, limiting the ability to conduct a similar analysis in earlier years.

For whites, a one standard deviation increase in segregation is associated with a significant 6 percentage point increase in the share with exactly a high school education, and nonsignificant drops in the share with all other attainment levels. There is no significant effect of segregation on college graduation among whites or blacks, 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 full 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 5 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, while only suggestive, are consistent with segregation causing differences in the accumulation of individual human capital, specifically reducing skill among blacks and reducing skill inequality among whites. The results thereby provide some suggestive 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 quasi-random 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. Results of tests exploring these potential channels for the causal effect of segregation on cities, while only suggestive, 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 Alan B. Krueger (1996), who argue that racial division increases the ability of local government to transfer schooling resources from the black community to the white community. As argued by Clotfelter et al.

(2007), smaller 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 ghettoes good or bad?," ghettoes appear to be bad for African Americans and moderately good for whites in terms of the individual outcomes of young adults. However, aggregate demand implies that segregation is also a metropolitan disamenity for both white and black migrants selecting a residence. In other words, revealed preferences suggest that the average American, when choosing a city, considers ghettoes to be bad.

Appendix A: The Causes of US Railroad Placement

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 III 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 US (contrary to the experience in Europe). Hence, microvariation in ground slope, which was the primary challenge in railroad construction (Wellington 1911), drove elaborate surface configurations. "American railroads avoided topographic obstacles rather than level them, bridge them, or tunnel through them" (Atack and Passell 1994, 444).

2. *Competitive Strategy*. The first practical railroads, in the early nineteenth 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 (George Rogers Taylor and Irene D. Neu 1956).[37] 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.[38]

[37] For instance, the Boston and Worcester line was designed by Boston merchants to divert trade between Worcester and Providence (Taylor and Neu 1956, 4).

[38] In Portland, ME, 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, 18). The Maine legislature forbade existing lines to change their gauges in response.

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[39] 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 United States 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).

More evidence on this point can be drawn from the case studies of Binghamton, NY, and York, PA, where placement was driven by factors 1) and 2) above. Both developed in the mid-nineteenth century as cities serving key "middleman" purposes between the Great Lakes, farms, and coal country to the west and north and major eastern ports (NYC and Baltimore). Both were small flat areas surrounded by hills, with access to water (both lie on the Sasquahanna River) but no large bodies to navigate, that were therefore the most advantageous places in their respective geographies to serve as a nexus for railroads. Because of their advantageous geography, several railroad companies (Delaware, Lackawanna, and Western Railroad and Erie Railroad, for Binghamton; North Central Railroad and Maryland and Pennsylvania Railroad, among others, for York) built routes through each town during the highly competitive mid-1800s railroad booms.

In both cities, railroad companies needed access to nearby destination cities—other small, flat areas that served as additional hubs on the paths from the Great Lakes, coal, and farm country. These destination towns included cities in all directions: to the northeast (Albany, for Binghamton; Lancaster, for York); the northwest (Syracuse, for Binghamton; Harrisburg, for York); the west (Buffalo,

[39] 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).

Binghamton, NY

York, PA

Figure A1. Topographical maps (*Google Maps 2010*)

for Binghamton; Pittsburgh, for York), and the south (Scranton, for Binghamton; Baltimore, for York).

A glance at a topographical map (Figure A1) explains the constraints facing engineers in each town as they charted northeast, northwest, west, and southward paths. Surrounding historical York, the flat area is circular and runs five kilometers in every direction, a larger space than small cities occupied at the time. Therefore tracks ran straight out of town in every direction. Surrounding historical Binghamton, the flat area is J-shaped, running five kilometers east-west, with a northern offshoot at the eastern end. Therefore roads did not run northwest or south out of the city, despite destinations in each of those directions and despite the fact that, within the city itself, each of these areas was flat.

Therefore, despite having roughly the same quantity of tracks headed (eventually) toward destinations in all directions, in Binghamton these roads headed out of town along one of just three routes (east, west, and northeast), while in York they cut up the city into many more slices. These different configurations of human-made barriers would prove important as these cities evolved.[40]

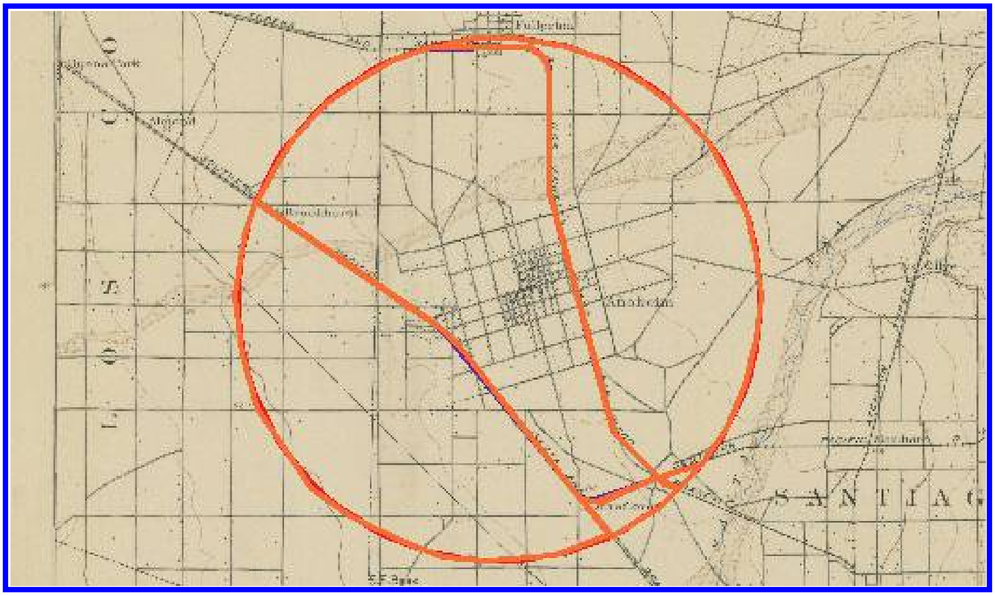
From this evidence, both from case studies and from economic history analysis, on the causes of railroad placement, I argue that relative railroad subdivision of a city's topography was incidental. It was driven plausibly by the initial placement of unrelated tracks and the later need to connect them 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 III supporting a zero correlation.

Appendix B: Extracting the Railroad Division Index from Maps

Figure A2 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 percent of the cities were smaller than 16π 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.

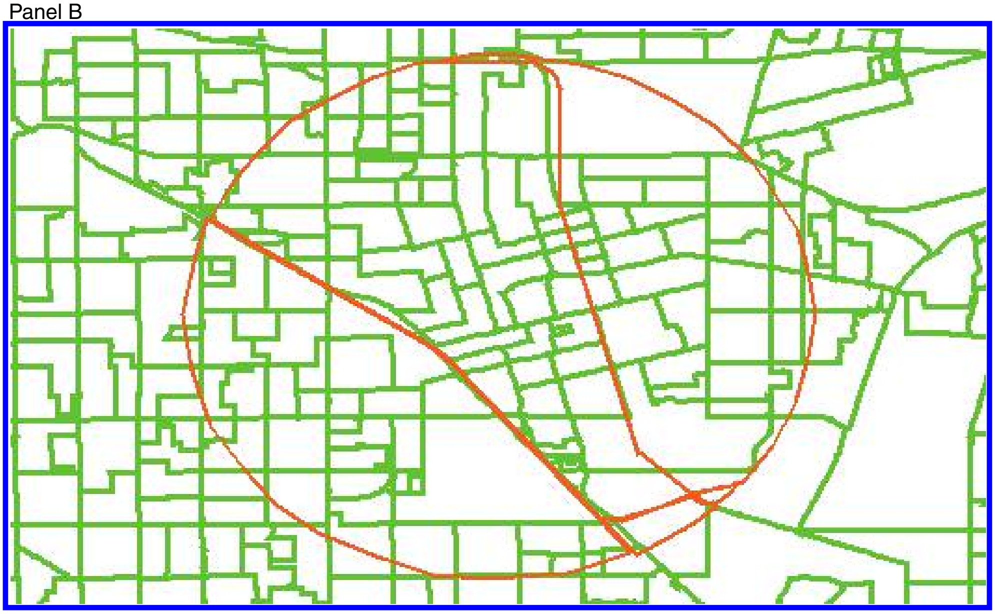
[40] Note that if, instead, elevation determined segregation, Binghamton, with more nearby variation in elevation than York, would likely have more, rather than less, segregation than York.

Panel A



Notes: Neighborhoods are defined as polygons created by the intersection of railroads with each other and with the perimeter of a 4 kilometer radius circle around the historical city center. Anaheim contains five neighborhoods, shown here in thick lines (orange online) 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.

Panel B



Notes: The 1894 neighborhoods are shown projected onto a year 2000 census map; modern tract borders are shown in jagged lines (green online). Note that current neighborhood borders, as defined by the US Census Bureau in 2000, closely follow historical railroad tracks.

Figure A2. Measuring the railroads of Anaheim, CA

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 1,000 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.[41] Further, my sample was limited by the set of historical maps held by the Harvard Map Library.[42] 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, however, 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, toward a finding of no result.

[41] 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 percent of African Americans resided in one of these states in 1910 (author's calculation from 1910 IPUMS data).

[42] The maps that provide railroad placement information were created by the US Geological Survey as part of an effort to document the country's topography, beginning in the 1880s. These maps display elevation, bodies of water, roads, railroads, and (in many cases) individual representations of nonresidential 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.

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

Cutler-Glaeser-Vigdor variable	Not in sample	(Standard error)	In sample	(Standard error)	Difference in means	p-value of t-test for the difference in means
Isolation index–1890	0.049	(0.007)	0.053	(0.008)	−0.004	0.698
Isolation index–1940 tract	0.355	(0.053)	0.318	(0.043)	0.037	0.586
Isolation index–1940 ward	0.234	(0.034)	0.198	(0.023)	0.036	0.361
Isolation index–1970	0.343	(0.034)	0.365	(0.023)	−0.022	0.578
Isolation index–1990	0.229	(0.022)	0.214	(0.017)	0.015	0.586
Dissimilarity index–1890	0.385	(0.032)	0.383	(0.024)	0.002	0.956
Dissimilarity index–1940 tract	0.736	(0.029)	0.742	(0.019)	−0.006	0.862
Dissimilarity index–1940 ward	0.57	(0.032)	0.57	(0.022)	0.000	0.990
Dissimilarity index–1970	0.744	(0.015)	0.74	(0.012)	0.004	0.843
Dissimilarity index–1990	0.574	(0.016)	0.569	(0.012)	0.005	0.798
Percent black–1890	0.03	(0.005)	0.027	(0.003)	0.004	0.532
Percent black–1940	0.058	(0.007)	0.041	(0.005)	0.018	0.034
Percent black–1970	0.056	(0.006)	0.062	(0.005)	−0.006	0.477
Percent black–1990	0.067	(0.006)	0.061	(0.005)	0.005	0.480
Population–1890	129,829	(56,323.8)	66,044	(19,199.4)	63,785	0.242
Population–1940	390,895	(170,643.4)	203,676	(40,731.8)	187,219	0.206
Population–1970	919,239	(261,007.3)	681,599	(129,697.5)	237,640	0.375
Population–1990	689,768	(135,048.7)	590,189	(96,574.5)	99,580	0.538
Number of wards–1890	17.778	(3.724)	13.421	(1.731)	4.357	0.288
Number of wards–1940	15.929	(2.641)	14.122	(1.440)	1.807	0.519
Number of tracts–1940	146.059	(53.033)	103.348	(21.920)	42.711	0.417
Number of tracts–1970	211.118	(61.515)	161.811	(29.971)	49.307	0.432
Number of tracts–1990	203.687	(44.957)	137.496	(20.894)	66.191	0.131
Total area–1900	19,283	(5,711.826)	11,764	(1,755.343)	7,519	0.147
Total area–1940	32,855	(9,499.416)	27,137	(6,214.421)	5,718	0.610
Total area–1970	2,344	(604.403)	1,615	(201.438)	729	0.184
Total area–1990	2,387	(469.458)	1,826	(262.051)	561	0.262
Per capita street car passengers–1915	204.214	(15.422)	179.002	(19.461)	25.212	0.334
Percent of blacks employed as servants–1915	0.21	(0.015)	0.207	(0.013)	0.002	0.900
Increase in urban mileage in 1950s	0.237	(0.019)	0.248	(0.021)	−0.011	0.713
Number of local governments–1962	62.925	(10.281)	55.551	(7.477)	7.374	0.558
Inter-governmental revenue sharing–1962	0.262	(0.011)	0.248	(0.007)	0.014	0.261
Centralization index–1990	0.741	(0.016)	0.77	(0.019)	−0.029	0.264
Clustering index–1990	0.207	(0.015)	0.177	(0.021)	0.03	0.235
Concentration index–1990	0.556	(0.020)	0.656	(0.022)	−0.1	0.001
Income segregation–1990	0.23	(0.006)	0.247	(0.004)	−0.017	0.061
Black income segregation–1990	0.554	(0.012)	0.546	(0.009)	0.008	0.573
Educational exposure index–1990	−0.084	(0.007)	−0.088	(0.005)	0.004	0.602
Manufacturing share–1990	0.172	(0.009)	0.189	(0.006)	−0.017	0.118
Median income	31,484	(716.257)	31,606	(572.361)	−123	0.893
Median education	−0.162	(0.019)	−0.143	(0.013)	−0.019	0.392
Share of moms who are single	0.236	(0.018)	0.26	(0.016)	−0.024	0.339
Average commuting time	0.823	(0.520)	−0.437	(0.363)	1.26	0.044
Person-weighted density	1,808.075	(338.133)	1,270.52	(75.120)	537.555	0.049
N	246		121			

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