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THE WRONG SIDE(S) OF THE TRACKS:
ESTIMATING THE CAUSAL EFFECTS OF RACIAL SEGREGATION ON CITY OUTCOMES

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ABSTRACT

At the metropolitan level there is a striking negative correlation between residential racial segregation and population characteristics -- particularly for black residents -- but it is widely recognized that this correlation may not be causal. This paper provides a novel test of the causal relationship between segregation and population outcomes by exploiting the arrangements of railroad tracks in the 19th century to isolate plausibly exogenous variation in cities' susceptibility to segregation. I show that, conditional on miles of railroad track laid, the extent to which track configurations physically subdivided cities strongly predicts the level of segregation that ensued after the Great Migration of African-Americans to northern and western cities in the 20th century. At the start of the Great Migration, though, track configurations were uncorrelated with racial concentration, ethnic dispersion, income, industry, education, and population, indicating that reverse causality is unlikely. Instrumental variables estimates demonstrate that segregation leads to lower incomes and lower education among blacks. For whites, there is a mix of positive and negative effects: segregation decreases the probability of being a college graduate or a high earner, but also decreases the probability of being poor or unemployed. Segregation could generate these effects either by affecting human capital acquisition of residents of different races and socio-economic groups ('production') or by inducing sorting by race and SES into different cities ('selection'). This paper provides evidence that is most consistent with a combination of both production and selection.

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I. Introduction

Residential segregation by race is one of the most visible characteristics of many American cities. Although African-Americans represent just over one-tenth of the U.S. population, the average urban African-American lives in a neighborhood that is over half black (Glaeser and Vigdor 2003). Cities vary in the extent to which their black populations live in black enclaves, and more segregated cities on average have worse characteristics than less segregated cities on measures ranging from infant mortality to educational achievement (Massey and Denton 1993).[1]

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

In the first case, that of omitted variable bias, some unmeasured economic, political, or other attribute may lead certain cities to both have more segregation and have more negative characteristics than other cities. For example, stereotypical "struggling" U.S. cities, such as Detroit, are highly segregated and their residents have low human capital, 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 city 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 outcomes of cities to be estimated. In this paper I address concerns about omitted variable bias by using a function of 19th-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 20th century. I formalize the widely observed phenomenon of the "wrong side of the tracks" by showing that cities that were subdivided by railroads into a greater number of physically defined neighborhoods—which arguably serves as a technology for creating segregation—became significantly more segregated during the Great Migration than did other cities. This instrumental variable strategy allows me to identify the causal effect of segregation on net city-level outcomes**.**

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

[1] Throughout the paper I use the term "city" to refer to a metropolitan area.

railroad division drives current city outcomes through racial segregation, rather than through some other mechanism.

Using railroad division and its interaction with proximity to the South to instrument for segregation, I examine the effect of segregation on human capital outcomes separately by race. These outcomes include education (both high-school dropout share and college-graduate share), income (both share of residents in poverty and share with income above $150,000), and employment. I find that exogenously increasing segregation causes cities to have significantly fewer white and black residents who are high-income or have completed college. The African-American populations of such cities are also significantly more likely to be poor, and less likely to have finished high school. The share of the white population at risk for especially negative outcomes, however, appears to be diminished in more segregated cities: whites in more segregated cities are significantly less likely to be poor and more likely to be employed than whites in less segregated cities.

These results do not themselves demonstrate that segregation determines individual human capital outcomes, because endogenous migration may produce such differences between cities. That is, people may respond to segregation by systematically sorting between cities in ways that alter average city demographic characteristics. With non-random migration, segregation can affect aggregate city outcomes, such as share of the population with college degrees, without actually altering the outcomes of residents directly. For example, stereotypically "struggling" cities might have low numbers of college graduates because segregation directly leads to inefficient education funding and lowers educational achievement (a direct effect on individuals), or because people dislike segregation and those with college degrees and high wages are willing to pay to go elsewhere (an indirect effect on cities). Direct production and indirect compositional mechanisms are both of economic interest, but only the direct effect can be considered a causal effect of segregation on the *outcomes of individual people*. The combination of direct and indirect effects, which is easier to observe, must be considered the aggregate effect of segregation on the *average outcomes of the populations of places*. Throughout the paper, I refer to these direct effects as "production" effects, these indirect effects as "selection" effects, and the combination of the two as "aggregate" effects.

In order to understand the implications of the differences that I find between human capital outcomes in more versus less segregated cities, it is important to distinguish between production and selection as contributors to the aggregate effects. Evidence of production effects supports models (e.g. Benabou 1996) that propose that segregation directly changes the aggregate production of human capital through neighborhood-level externalities.[2] Evidence of indirect effects on sorting between cities can help us understand what underlies the migration trends that are making some cities experience a population explosion while others are shrinking (Vigdor 2006). In particular, as cities increasingly become concerned with attracting the "creative class" (Moretti 2004), the demand among the highly educated for city characteristics such as low levels of segregation could have significant implications for city government behavior.

In section II.C, I present a model of the processes of production and selection and discuss how the two might be distinguished from each another. To examine whether the effects I observe result from production or selection, in section IV.D I perform several additional analyses using railroad division as an instrument. Looking at rents and population flows, I find evidence that demand for more segregated cities is lower among both blacks and whites; this result implies that some of the negative effects on segregated cities' populations may be due to selection. Looking at outcomes for young adults who have had little time to move, I find similar relationships between segregation and human capital as in the population overall; this result suggests that at least some of the effect of segregation may be a direct production effect on individual outcomes. In other words, I find evidence supporting arguments that segregation has both "production" and "selection" effects on city outcomes.

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

II. Framework and instrument

A. Historical framework

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

Pre-segregation. In the 19th 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

[2] I use the term "neighborhood-level externalities" very broadly here, to denote anything that depends on the population of the neighborhood and affects individuals. Relevant neighborhood characteristics might include group political capital, economic resources, social networks, etc.

in the former slave states.[3] This observation is particularly relevant to this analysis, because the bulk of railroad tracks were laid prior to 1900 (Atack and Passell 1994); this timing makes it implausible that railroads in the North and West were laid with the intent of segregating African-Americans.[4]

The creation of segregation. During the Great Migration (roughly 1915 to 1950), large numbers of African-Americans migrated into Northern and Western cities from the South. Cities that had tolerated small black populations (Massey and Denton 1993, Weaver 1955) became highly segregated as their black populations grew (Cutler et al. 1999). Black segregation generally resulted largely from deliberate government policies and from collective action by white residents, not from market forces (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.

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[3] 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.

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

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

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

Post-civil rights movement. Government policy towards residential segregation changed gradually during the civil rights era, and a clear break in housing policy came in 1968 with the Fair Housing Act and its outright prohibition of discrimination. High levels of segregation nonetheless have persisted in most American cities up to the present day (Cutler, Glaeser, and Vigdor 1999). At the same time, the gap between black and white human capital, which showed signs of narrowing during the civil rights era, has 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 outcomes through production (causal effects of segregation on the attributes of those living in segregated cities), and/or selection (non-random migration of individuals with those attributes into or out of segregated cities). Most previous research has focused, implicitly, on one or the other of these channels.

Wilson (1996) argues that racial segregation increases skill segregation within the black community, thus causing negative outcomes for low-skilled blacks through peer effects. That is, he claims that segregation *produces* a black "underclass." Collins and Margo (2000) find evidence consistent with Wilson's story for the post-civil rights period. Similarly, Card and Rothstein (forthcoming) argue that, controlling for student background, residential segregation during high school *produces* lower test scores for black students relative to whites.[5]

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

A separate branch of the literature has focused on selection. Bayer, Fang, and McMillan (2005) and Baum-Snow (2007) argue that, through neighborhood choice, individuals reveal preferences for areas that are racially and economically homogeneous; Boustan (2006) argues that racial segregation is partly the result of efficient Tiebout sorting based on tastes for public good spending. In contrast, other research finds that many whites and blacks have stated tastes for neighborhood integration (Bobo et al. 1994), as well as revealed preferences for integrated cities (Cutler et al. 1999). Either tastes in regard to integration/segregation or concerns about the production effects of segregation could generate selective migration; the implied direction and magnitude of population flows might 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 the direct production and indirect selection effects together. A partial exception is Cutler and Glaeser (1997), whose model assumes that all whites are high-skilled, so that segregation can have no effect on white human capital. Under this assumption, whites have no production motive for preferring segregation—implicitly, they simply have tastes for segregation. Cutler and Glaeser assume that blacks, by contrast, are concerned about production but do not have tastes regarding segregation. Finally, they address the selection channel by attempting to eliminate it; they do so by limiting their empirical strategy to questions for which they believe migration will not be important.

In this paper, I assume instead that segregation can produce variation in white as well as black outcomes, and that both blacks and whites may have tastes for either segregation or integration. I then assume that migration (selection) among high-skilled and low-skilled whites and blacks can be motivated by concerns about human capital (production) and/or by tastes. Finally, I assume that the aggregate effects of segregation on the characteristics of both the black and the white populations may result from a combination of selection and production effects.

C. Model of causal link

The above literature review motivates the following formalization of how the production and selection effects of segregation may be distinguishable in equilibrium. In particular, the model will illustrate how segregation may have different implications for individual measures of economic success (education, employment, income) and for aggregate measures of demand for places (rent, migration). However, the empirical results presented in the later part of the paper do not depend on the specific assumptions of this model.

1. Static model

Assume two small open-economy cities that exist for two generations. City I has technology such that it will have two perfectly racially integrated tracts, while city S has technology such that it will have two perfectly racially segregated tracts. In all other ways, these two cities are identical (Figure 1a). At time zero, corresponding to the Great Migration, each city is randomly assigned the same population of measure one, 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:

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

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.[6] According to equation (1), the production of offspring human capital depends not only on an individual's own human capital but also the average human capital of neighborhood residents. This implies that average human capital is a neighborhood-level public good. Moreover, it implies that the production of human capital is affected by racial segregation, since racial composition determines the average human capital in the neighborhood. 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 $\mu_{Hw} > \mu_{Hb}$.

Human capital might depend on the neighborhood in which one grows up in because of a literal "peer effect" (Benabou 1996) or because of neighborhood characteristics proxied by peers (Card and Rothstein, forthcoming). Such characteristics would include anything that affects the development of human capital at the individual level and that might depend on the human capital in a neighborhood's elder generation. These characteristics could include: school or health services funding, the political power of the neighborhood, whether a chemical dumping ground is sited in the neighborhood, how connected residents are to job networks, the neighborhood crime rate, etc. In related work (Ananat and Washington 2006; Ananat, La Ferrara, and Mele, in progress), coauthors and I explore which of these mechanisms appear to be most important; the goal of this paper is simply to capture the reduced form effects of segregation on outcomes.

If we assume that residents cannot move between cities, then it is clear from the model that the skill gap between blacks and whites will persistently be weakly larger in S than in I (Figure 1b). Over time, the μ_H of blacks and whites in I will weakly converge, as blacks and whites are exposed the same average human capital generation after generation (Figure 1c). 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 (Figures 1d and 1e). Further, if α is nonzero, these relationships will not be weak: whites in S in each generation will have strictly higher μ_H than whites and blacks in I, who in turn will have strictly higher μ_H than blacks in S. However, it is theoretically ambiguous whether overall average human capital will be higher in I or in S (Figure 1e); that will depend on whether $\alpha > 1$ or $\alpha < 1$.[7]

We can identify empirically which is higher by observing whether there are more residents with high human capital in segregated or in integrated cities. We can define

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

[7] If own human capital and neighborhood average human capital are substitutes in the production of next generation human capital, 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, then segregation—which exposes the offspring of the racial group with the greater human capital to neighborhoods with greater human capital— will result in higher human capital than integration.

Note, however, that the expected value of the human capital of one's offspring is always increasing with the neighborhood average human capital, so that it is always productive, from any individual's perspective, to be in a neighborhood with higher average human capital. In the absence of perfect markets, (e.g. coordinated payments by the black community to the white community to compensate for integrating neighborhoods), city S may not integrate even if integration is more efficient overall (Schelling 1971).

various levels of human capital (e.g. share who are high school graduates, share who are college graduates, or share who earn more than $150,000) to estimate the effects of segregation at different margins of the skill distribution. Taken together, the differences in these observed shares will reflect the effect of segregation on individual economic success and also will answer the question first posed by Cutler and Glaeser (1997), "are ghettoes good or bad?", for society as a whole.

2. Model with city choice

If moving costs are low enough that migration between cities occurs, then observed differences in the skill distribution cannot be interpreted simply as resulting from the production effects of segregation, for two reasons. First, concerns about production effects might cause differential migration between cities by skill and race, so that the actual production effects of segregation are obscured by sorting. 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 neighborhoods; this would raise the average type in I and make integration appear more productive than segregation. Second, when moving is possible, 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 $\alpha = 0$. For example, if high-skilled people tend to have positive tastes ($\sigma > 0$) for integration, then city I actually might end up with more high-skilled whites than S. That process would overturn the ordering in the static model that whites in S will have higher p_{H2} than whites in I (Figure 1f).

It may be possible to differentiate empirically between city differences generated by production and by selection by examining indicators of relative demand for cities I and S. Differential demand by city will cause rents to change in order to clear the housing market. With equal initial population of measure 1 in each city, the initial price of housing in city I relative to city S can be normalized to 0, but as aggregate demand tilts toward one city or another, the rent premium or discount for living in city I will change (Figure 1g).[8]

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.[9] This implies that, 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 in equilibrium 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 type 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) will re-sort between cities. We can therefore observe equilibrium rents and population

[8] If we assume that in the segregated city the share of the city occupied by each race can grow or shrink based on the its proportion of the population, and that the housing market is competitive, then we can make the simplifying assumption that the housing market clears on the city level. In Section IV.A, I find that rent differentials are similar for blacks and whites, suggesting that in fact the housing market does clear on the city level.

[9] 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

flows in order to make inferences about the production and selection processes at work (Figure 1h).

For example, if people pay higher rents in more integrated cities and have positive net migration rates to more integrated cities, then the demand for integration is clearly high. As long as preferred racial composition is a normal good, the high demand for integration will tend to lead to selection of those with higher average human capital into integrated cities. If both high demand for, and high average human capital in, integrated cities are observed among a population group, then it will be difficult to tell whether the group demands integration because it is more productive than segregation for that group's human capital or simply because of tastes for integration. In other words, in this scenario indirect selection will be impossible to distinguish from direct production effects; the strongest possible conclusion is that at least one of the tastes for, or human capital effects of, integration must be positive.

Conversely, however, if a group is observed demonstrating high demand for integrated cities, but has better average characteristics in *segregated* cities, then it is possible to distinguish between production and selection explanations for observed human capital levels. This set of results would imply that segregation is more productive than integration for that group, but that the group on average has tastes for integration. For example, Figure 1f depicts an equilibrium in which blacks have better outcomes in I than in S—consistent with either the static model or with city choice, under the assumption that blacks do not have a strong distaste for integration. Meanwhile, whites are less likely to have very negative outcomes in S than in I, but also less likely to have

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.

very positive outcomes—the distribution of skills is more spread out in I than in S. This result for whites is not consistent with the static model. It is, however, consistent with a city choice model in which high-skilled whites prefer I to S. To test this explanation, we can examine the population flows of whites between S and I—if whites pay higher rents in I and are moving towards I, then the skill distribution in Figure 1f can be accounted for by the assumption that integration is a normal good among whites. If higher rents and positive population flows hold among both the low- and the high-skilled white populations, then we can conclude from the lower rate of very negative outcomes for whites in S that segregation is indeed more productive for whites than is integration—it protects them from negative outcomes—and thus migration must be caused by positive tastes for integration.

D. Instrumental approach

In order to test for these or other patterns of outcomes, empirical variation approaching a randomized experiment is required. In the ideal experiment, one would test for the potential effects of segregation implied by the model described above 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.
3) Then, the relationship between segregation and the economic outcomes (education, employment, income) of the offspring generation would be measured. This is the production 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 for 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.[10] The variation I exploit is created by idiosyncrasies in the layout of railroad tracks that eases the collective definition of neighborhoods.

In many cities it is self-evident that railroads tend to define neighborhood boundaries. Although explicit explanations for why railroads per se do so are not available in the literature, the use of a standardized marker such as railroads is exactly what would be predicted by a "coordinated expectations" model of conflict with limited communication (Schelling 1963). A railroad provides a clear demarcation that facilitates collective agreement on neighborhood boundaries by 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. When a community is interested in remaining separate from a particular group, railroads can facilitate collective action in enforcing segregation by reducing coordination costs. As Schelling (1963) describes the function of an obvious landmark

[10] The quasi-experimental design I exploit does not allow the clean separation of 3) from 4). However, empirically I attempt to distinguish between the effects of production (3) and selection (4).

for an army, railroad tracks become for the white community "one spot to which they can retreat without necessarily being expected to retreat further" (p.71).[11]

As the black population of a city grew during the Great Migration, the city's ghetto areas had to expand if segregation was to be maintained. Once railroad tracks were used as a focal point for coordinating expectations in a specific instance, it was likely that they would be used again throughout the city if available. Cities that were subdivided by railroads into many small insular neighborhoods could use those boundaries to redefine the enclave areas of the city, one neighborhood at a time, and still practice "containment," whereby the black population remained concentrated and isolated. On the other hand, in cities where railroads were not configured in such a way as to define many neighborhoods, expanding an enclave meant breaching a major divide. Once the black population increased enough that spillover was inevitable, segregation no longer could be maintained as easily in the open area on the other side.

Figure 2 illustrates this concept. Binghamton, NY, and York, PA, were similar in the total quantity of railroad tracks laid by 1900 (shown in heavy lines, circumscribed by a four kilometer-radius circle). They also had similar industrial bases and experienced comparable, substantial changes in African-American population (these characteristics are discussed in detail later in the paper). However, York's railroads were configured such that they created many insular neighborhoods, particularly in the center of the city. In the year 2000, its Census tracts show its black population relatively concentrated in this set of small, railroad-defined neighborhoods (tract-percent-black is represented by the heaviness of tract shading). In Binghamton, on the other hand, railroads are tightly clustered, leaving some areas too long and narrow to encompass neighborhoods and

[11]Thanks to Glenn Loury for pointing me towards this reference.

others too wide open to create meaningful population restrictions. In contrast to York, Binghamton's year 2000 Census tracts show its black population relatively evenly dispersed throughout much of the city.

E. Instrumental measurement and validity

1. Defining railroad division

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

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-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 the Census (Figure 2). 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.

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 city land into subunits.

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

The RDI quantifies the extent to which the city's land is divided into smaller units by railroads. If a city were completely undivided by railroads, so that the area of its single neighborhood was 100% of the total city area, then the RDI would equal 0. If a city were infinitely divided by railroads, so that each neighborhood had area near zero, then the RDI would equal 1. The more subdivided a city, the more "sides" there are to its tracks, and the more possible boundaries between groups are available to use as barriers enforcing segregation. In particular, if railroads created many small neighborhoods (high value of RDI), then it would have been possible during the Great Migration to relieve pent-up housing demand by allowing an 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[12] on the y-axis, demonstrates a positive relationship between RDI and segregation.

2. Interaction between RDI and proximity to the South

[12] This index, called the index of dissimilarity, is described in the next section.

In addition to a generally positive relationship with segregation, RDI should interact positively with increasing demand in determining equilibrium segregation. Figure 4a depicts the way in which the relationship between demand for segregation and equilibrium segregation should depend on the cost of segregation. Because segregation is bounded at 0 and 1, there should have been low segregation regardless of cost at very low demand. At extremely high demand, segregation should have been high even if cost was high. But at intermediate levels of demand, the places that faced a lower cost of segregation should have experienced rapidly rising segregation as soon as demand became non-zero, while the places that faced a high cost of segregation should have seen little change in segregation even when demand rose significantly.

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 always 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. Thus, proximity to the nearest former slave state, which varies greatly between cities, even within states such as Michigan and California and regions such as New England, strongly predicts black inflows and therefore demand for segregation.

If RDI truly predicts the cost of segregation, and proximity to the South predicts demand for it, then in places where RDI is high (low cost of segregation), proximity to the South should have a concave relationship with segregation; in places where RDI is low (high cost of segregation), segregation should be lower, and its relationship with proximity to the South convex. As shown in Figure 4b—a quadratic estimate of the relationship between proximity to the South and equilibrium segregation for cities with above- and below-median RDI—the empirical relationship is indeed consistent with the proposed relationship.

I therefore proxy for demand using expected black inflows as predicted by proximity to the nearest former slave state. By exploiting proximity and its interaction with RDI as additional sources of variation, I can increase the strength of my quasi-experiment. Moreover, this additional instrument creates another falsification test for the quasi-experiment, because 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 substantial demand for segregation, such as Seattle, or Lansing, MI.

III. Data and Empirical Measures

A. Segregation measures

In addition to the maps described in Appendix A, the major data sources are U.S. Census Bureau reports on metropolitan demographics (various years), 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

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

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 non-blacks) would have to move to a different census tract in order for the proportion black in each neighborhood to equal the proportion black in the city as a whole?" By construction, the index can range from zero to one. Note that an index of zero is improbable in the absence of central planning.

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 19th and 20th century segregation indices as well as metropolitan characteristics from Cutler and Glaeser (1997) and additional data from the 2000 Census from Glaeser and Vigdor (2003). In addition, the data include four other measures of segregation from the 1990 Census reported in Cutler, Glaeser, and Vigdor (1999), all of which also are based on measures developed in Massey and Denton (1988). These include clustering, concentration, and centralization (which rely on geographical data about the proximity, size, and location of a city's census tracts that only become available in 1990), and isolation, which is another way of aggregating neighborhood composition that is highly correlated with dissimilarity.[13]

B. Additional RHS variables

In every first-stage regression I control for kilometers of railroad track per square kilometer in the historical city (described further in Appendix A). Using this control assures that RDI does not simply capture the amount of railroad track in the city[14], but

[13] Clustering is a measure of the extent to which the neighborhoods that are primarily home to blacks adjoin each other. Concentration is a measure of the extent to which blacks are located in the most physically dense neighborhoods. Centralization is a measure of the extent to which blacks are located in the neighborhoods closest to the center of the city. Isolation is a measure of the extent to which the average black person lives in a neighborhood that has more blacks than the overall city share.

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

rather represents the configuration of track conditional on total track. I propose that configuration drives variation in the supply curve for segregation at the city level.

As motivated by Figure 4, I use proximity to the South as an additional instrument that proxies for each city's expected black inflows. Cities in my sample that are 100 miles closer to the South averaged half a percentage point higher black population by 1940 (the t-statistic for the correlation is 5.26), a gap that had nearly doubled since 1910.[15] I propose that variation in expected black inflows drove demand for segregation.

Finally, I include in my set of instruments the interaction between supply and demand, i.e. the product of proximity and RDI. It might be preferable to use only the interaction as an instrument, leaving the main effects of proximity and RDI out, but doing so would result in a weak instrument problem, given my small sample size. Moreover, the specification and falsification checks detailed below suggest that the proximity and configuration main effects also generally meet the requirements outlined in Angrist and Imbens (1994).

C. Outcome measures

For the main outcomes, I measure aggregate city[16] characteristics by race, using data from published Census reports. These outcome measures include the proportions of

[15] Alternatively, I have used a city's WWII military contracts per capita as a predictor of black inflows (Dresser 1994), and found results consistent with those in the paper. Since subsequent literature has raised concerns about the excludability of that measure (Collins 2001), I do not include them here.

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

a city's blacks and whites who are poor, unemployed, high-school dropouts, college graduates, or who have household incomes above $150,000. I choose these measures because my focus is on the effect of segregation on basic human capital outcomes of both blacks and whites, and on how that effect may differ at the top versus the bottom of the distribution of outcomes. The first three measures should reflect the effects of segregation on the relatively disadvantaged segment of the population, who might be on the margin of poverty, unemployment, and dropping out of high school. The last two measures should reflect the effects of segregation on the population at risk for very positive outcomes, high education and high income.

In order to assess to what extent these aggregate population impacts can be attributed to either (or both) production or selection, 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 metropolitan areas drives human capital differences between cities, I examine measures of aggregate demand for Metropolitan Statistical Areas (MSAs) by race. These measures include percent of the black and white populations that are new to the MSA, median rent by race, and crowding. Second, following Cutler and Glaeser (1997), I examine the human capital of individual young adults, aged 22 to 30, based on their MSA of residence five years prior to the Census, as reported in the 1990 IPUMS (Ruggles et al. 2004). These data should reflect the relationship between exposure to segregation as a child and eventual human capital as an adult. When looking at young adults, I shift my outcome measures, again following Cutler and Glaeser (1997). I substitute idleness (i.e., not

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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.

employed and not in school) for unemployment. I also include never-married mother as an outcome (never-married is a rare characteristic in the overall population of mothers, but occurs with reasonable frequency among young mothers).

IV. Results

To motivate the empirical work, Table 1a reports the 1990 characteristics of cities with 19th-century railroad division indexes above and below the median. The segregation index is 20 percent higher in cities with above-median railroad division than in cities below the median. Moreover, outcomes by race in more versus less segregated cities follow a pattern that is consistent with the disparate resources that might result from segregation: in cities with greater railroad division, the white population appears to have better characteristics for some but not all outcomes, while the black population appears to have worse characteristics in all cases. Many of the differences are statistically significant, even using this crude approximation of the variation in railroad division.

Table 1b further motivates my estimation strategy by showing the relationship between railroad division and outcomes, measured separately by proximity to the South (divided here into above versus below 400 miles from the nearest slave state). Railroad division is much more strongly, consistently, and significantly correlated with outcomes in cities that are close enough to the South to have had significant black inflows during the Great Migration than it is in cities that are far from the South. Together, Tables 1a and 1b lend evidence that the combination of latent supply of segregation (railroad division) and latent demand for segregation (expected black inflows) is necessary to generate exogenous segregation as required for a valid natural experiment. Next, I explore this relationship further using two-stage least squares.

A. First stage

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

$$(4)\ Seg = \alpha_1 Z + \alpha_2 X + \mu$$

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

and then estimated using two-stage least squares analysis. The right-hand side variable of interest in equation (4), Seg, represents a city's current level of segregation. Z is a vector of instrumental variables. In this vector, I proxy for the supply of segregation technology with the RDI. I also proxy for the demand for segregation technology, as determined by black inflows, by using proximity to the South. Finally, I proxy for the interaction of supply and demand by using the product of RDI and proximity. X is a vector of control variables including total railroad length and, in some specifications, region dummies.

The first panel of Table 2 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). It also strongly predicts three of the other four aspects of segregation provided in Cutler, Glaeser, and Vigdor (1999).

In addition, the assumption that RDI should matter more where there are greater expected black inflows also is upheld, as shown in the second panel of Table 2. RDI remains highly significant, and the interaction between RDI and proximity to the South is also positive and highly significant in predicting 1990 dissimilarity. The F-statistic for the joint significance of the two main effects and their interaction in predicting dissimilarity is 17.72, well above the Stock and Yogo (2002) threshold for three instruments and a single endogenous regressor.

B. Robustness of first stage

For my instrumental variable strategy to be valid, it must be the case not only that railroad configuration leads to segregation; it must also be the case that both railroad configuration and people were each assigned to cities quasi-randomly, not in ways that reflect other underlying city characteristics. Two obvious concerns about endogeneity would, if true, undermine this assumption and invalidate the instrumental strategy I employ: 1) railroads developed in ways that reflected existing 19th-century characteristics of cities, and/or 2) people sorted themselves into cities based on railroad configuration. Next I investigate these possibilities.

1. Evidence that railroad configuration was assigned quasi-randomly

What if, rather than having developed their configuration in the 19th century by happenstance, railroads were laid to intentionally segregate other groups, such as white ethnics or the poor, or in ways that reflected existing characteristics of cities? This would mean that the treatment was systematically correlated with other aspects of cities that also might drive current outcomes; thus the effect of RDI-induced segregation would be impossible to isolate and identify.

To check for evidence supporting these concerns, I test the hypothesis that RDI has no relationship to city characteristics prior to the Great Migration. The most obvious way that RDI would relate to city outcomes is if the 19th-century choice of railroad configuration itself were driven partly by local economic or social characteristics. Historical accounts of the reasons for railroad configuration, summarized in Appendix B, provide no support for that possibility. Moreover, Table 3 presents evidence that the instruments do not strongly predict population characteristics in 1910, a full decade after the end of major railroad construction, at the last Census prior to the Great Migration. The left-hand panel of Table 3 shows tests of the predictive power of the RDI, proximity to the South, and the interaction between them, for a variety of characteristics of cities prior to the start of the Great Migration. These include, for 1910, physical size, population, dissimilarity and isolation measures for the foreign-born versus native-born population,[17] and percentage black, and for 1915, streetcars per capita.[18] None of the RDI or track length, and only one of the interaction coefficients, are significant at conventional levels. Proximity to the South is significant for physical size and for 1910 immigrant dissimilarity, but the coefficients on immigrant dissimilarity run in the opposite direction of those for later racial dissimilarity, suggesting that immigrant segregation is not a proxy for later racial segregation.

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

[18] Provided in Cutler, Glaeser, and Vigdor (1999) for 13 cities in this sample.

Even if railroad configuration and proximity to the South do not predict pre-period outcomes, it is still possible that they reflect other local characteristics that drive current outcomes. As shown in Table 1b, 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. Nonetheless, I also test directly for two of the most obvious ways that RDI might reflect present-day underlying differences between places. First, RDI might reflect regional geographic variation, and region may affect current outcomes for other reasons. However, in the main results section I replicate the results with dummies for Census region included; while the standard errors of the estimates increase, the results remain essentially the same. Second, railroad configuration could reflect the value of land in the local area (although the historical record indicates that land prices were of minor concern; see Appendix B). Low or variable property values could lead to residential segregation by income, which, because of the correlation between race and income, could appear as racial segregation. However, the last column of Table 3 demonstrates that, even in the current period, RDI does not predict income segregation.[19] Moreover, the positive relationship between RDI and the characteristics of the at-risk white population that I demonstrate in the main results section suggests that RDI acts through race rather than through income.

2. Evidence that people were assigned quasi-randomly

If people sorted themselves into cities based on railroad configuration, or some pre-characteristic associated with railroad configuration, then the makeup of the treatment and control groups would be systematically correlated with other aspects of the

[19] Income segregation is insignificant in the U.S. relative to racial segregation; the highest of any metropolitan dissimilarity index for income in 1990 is .28, while the *lowest* 1990 dissimilarity for African-

cities. Such a correlation would, again, make the effect of RDI-induced segregation impossible to isolate and identify.

In the ideal experiment, this would not be a concern because residents initially would be randomly assigned to cities. Of course, actual random assignment did not occur, but for my instrumental strategy to be valid residents merely must have been assigned to cities in a way that was uncorrelated with railroad configuration.

The fact that cities did not differ observably in economic or social characteristics based on RDI prior to the Great Migration makes a zero correlation plausible. However, migrants still might have sorted between cities based on RDI itself, or based on something unobservable that was correlated with RDI. The right-hand panel of Table 3 tests for the possibility of initial selection by examining the human capital characteristics of cities in 1920, after the first wave of the Great Migration but before segregation could begin to have any noticeable direct effects on human capital. These characteristics include percentage black, literacy rate, labor force participation, and the share of employment in trade, manufacturing, and railroads. None of the coefficients for RDI, proximity to the South, or their interaction are significantly different from zero. These results provide a stronger test that cities did not vary on pre-characteristics and also provide evidence against initial sorting between cities.

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 and proximity must induce meaningful variation in degree of racial segregation; i.e. there must exist a strong first stage. This

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Americans is .33.

requirement is supported in Tables 1a and 1b and borne out by the first stage estimation, as shown in Table 2. Second, it must be the case that railroad configuration and proximity affected city outcomes through segregation, not through some other channel. The fact that the interaction of RDI and proximity predict segregation and outcomes much more strongly than does either on its own 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 the interaction of RDI and proximity. Table 3 provides evidence that this assumption holds as well.

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

C. OLS and two-stage least squares estimates for MSAs

In this section, I analyze the effect of RDI on the demographic characteristics of city residents. These characteristics reflect the aggregate impacts of RDI-induced segregation on city populations; they incorporate the impacts of both direct production effects on individuals and indirect selection effects on cities' populations through migration. In section IV.D, I analyze migration, housing demand, and the human capital of young adults in order to assess to what extent these aggregate impacts can be attributed to either production or selection effects, or both.

Ordinary least squares (OLS) estimates of the relationship between segregation and outcomes by race are shown in the top panel of Table 4. The OLS results show strong negative relationships between segregation and black outcomes across the board. However, the OLS results do not show strong correlations between segregation and white outcomes; only the findings of an improved white poverty rate and a lower white college graduate share are statistically significant.

The middle panel of Table 4 shows two-stage least squares estimates of the relationship between segregation and outcomes by race. The first three outcomes measured in Table 4 demonstrate that RDI-induced dissimilarity reduces the extent to which very negative outcomes are observed for a city's white population. A 10-percentage point increase in dissimilarity predicts a 1.4-percentage point decrease in the white poverty rate, a 0.2-percentage point decrease in the white high-school dropout rate, and a 0.9-percentage point decrease in the white unemployment rate. The first and last of these effects are statistically significant at conventional levels. In contrast, RDI-induced dissimilarity increases the extent to which very negative outcomes are observed for a city's black population. A 10-percentage point increase in dissimilarity predicts a 2.9-percentage point increase in the black poverty rate, a 2.8-percentage point increase in the black high-school dropout rate, and a 0.2-percentage point increase in the black unemployment rate. The first two effects are statistically significant at conventional levels.

The right-hand columns in Table 4 show that RDI-induced segregation decreases the extent to which very positive outcomes for both blacks and whites are observed in a city. A 10-percentage point increase in dissimilarity lowers the fraction of a city's whites who are college graduates by 2.3 percentage points and the fraction of blacks by 3.2 percentage points. Similarly, it lowers the fraction of whites with household incomes above $150,000 by 0.8 percentage points and the fraction of blacks by 0.5 percentage points. The negative effects on these characteristics are highly significant for blacks and are significant or marginally significant for whites.

The bottom panel of Table 4 tests for the possibility that RDI variation is correlated with region, and thus that the relationship between RDI and outcomes simply reflects regional differences. All of the regressions in the bottom panel include dummies for Census region. Because of the small sample size, these regressions are less precise and are only sometimes statistically significant. Nonetheless the results are qualitatively similar for poverty, high school dropout, unemployment, and college graduation with and without region dummies. The sole exception is the result for the share of the population earning more than $150,000, which remains negative for both blacks and whites but moves close to zero. It appears that the relationship between segregation and the share of residents with very high earnings exists mostly between regions.

Despite the evidence that RDI is uncorrelated with pre-characteristics of people and places, but strongly predicts segregation and current city characteristics, there still remains one threat to the interpretation of RDI as an instrument for segregation. That is the possibility that something about railroad configuration affects city growth directly rather than through racial residential segregation only. For example, railroad configuration might somehow alter the available investment opportunities directly. It might make transportation difficult or it might separate people from each other generally, which could cause income segregation or make the city unpopular. The cumulative evidence, however, weighs against this possibility. As shown in Figure 4b, the RDI predicts segregation much more strongly in cities close to the South than in those that are farther away; thus, if it is true that RDI affects human capital only through segregation, then it also should predict human capital much more strongly in cities within 400 miles of the South than in those that are farther away. Table 1b shows the reduced-form effects of railroad division on outcomes separately by whether cities are within 400 miles of the South. Because the total sample size is reduced in each regression, the standard errors of the estimates increase. Nonetheless, it is clear that, overall, the relationships reported in Table 4 persist in those cities within 400 miles of the South, and are uniformly weaker in cities more than 400 miles away. This result suggests that in the absence of black inflows, RDI does not have its own effect on city outcomes.

In sum, railroad division predicts worse outcomes for blacks and a narrowed range of outcomes (fewer very positive or very negative) for whites, but this is only true in times and places where segregation had been made salient by large black inflows. Railroad division does not predict segregation on other dimensions, including income and ethnicity, and does not predict pre-period characteristics, such as the structure of industry or the characteristics of early in-migrants. These results provide evidence that railroad division does drive differences in city characteristics that we observe today, and that it does so through racial segregation rather than through some other mechanism.

A comparison of the OLS estimates with estimates that use RDI as an instrument reveals that OLS makes segregation appear worse for low-end outcomes and better for high-end outcomes than it is, particularly for the white population. That is, OLS appears to be biased downward for negative outcomes and biased upward for positive outcomes, especially for whites. These biases imply that places with a lot of economic inequality, particularly within the white population, are endogenously segregated. By contrast, two-stage least squares estimates make it clear that segregation does not *cause* inequality—rather, it worsens black outcomes across the board while actually reducing economic inequality among whites. These results suggest that other, unobserved factors that cause inequality have resulted in omitted variable bias in estimates of the effects of segregation on city populations using OLS. For example, one possible cause of bias might be that some cities have lower tastes for economic and social equality than do others, and thus end up both more economically unequal within race and more physically separate between races.

Comparing OLS and IV can help us to understand how cities differ when they select into segregation rather than having segregation exogenously assigned. Such comparisons can't, however, help us to understand how exogenously assigned segregation causes individual *people* to select between cities. For that we need another analysis, which follows.

D. Tests for selection and production mechanisms

1. Selection

Table 5 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 10-percentage point increase in segregation leads to 1.3 percentage points fewer new white residents and 3.0 percentage points fewer new black residents.

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

The fact that migration appears to differ in observable ways between more and less segregated cities means that selection is a plausible explanation for at least part of the variation in human capital. In other words, at least some of the negative effect of segregation on the characteristics of blacks, and on the share of the population with high income and education, may come indirectly through selective migration. Low demand, in general, will reflect the sorting away of better-off blacks, and of the well-off in general (as those who can best afford to leave do), and should lead to negative selection and to worse average characteristics among those who remain behind. As shown in Figure 1h, the combination of worse outcomes and lower city demand implies that a group dislikes segregation, and/or that segregation is unproductive for that group. The lower frequency of negative characteristics among whites, on the other hand, suggests that whites have distastes for segregation but that segregation reduces their risk of negative outcomes.

2. Production

When I follow Cutler and Glaeser's (1997) approach to focus on young adults aged 22 to 30, I find results that are consistent with those shown in Table 4 for aggregate city characteristics among all ages. These estimates, shown in Table 6, use 1990 Census individual data (Ruggles et al. 2004) on 22- to 30-year-olds, include single-year-of-age dummies. The regressors also include: MSA-level RDI, proximity to the South, RDI*proximity, and the interaction of each of these with an indicator for whether the individual is African-American.

The main effects of RDI, and the interaction of RDI and proximity to the South, which can be interpreted as the reduced form effect of segregation on whites, include both reduced probabilities of very negative outcomes for whites (significant for idleness and for never-married mother) and very positive outcomes (significant for college graduate). The net effect of RDI, RDI*proximity, and the interactions of those with the individual indicator for black predict negative effects across the board for African-Americans (significant for poverty, idleness, never-married mother, and college graduate). The results using my instrumental variables strategy, therefore, are robust to an individual-level specification.

Under the assumption used in Cutler and Glaeser (1997) that the characteristics of 22- to 30-year-olds did not significantly drive their migration earlier than five years prior to observation, these results imply that segregation directly causes worse outcomes for blacks and reduced inequality of outcomes for whites. These implications are consistent with the conclusions implied by Tables 1a, 1b, and 4, which suggest that segregation is productive in reducing very negative outcomes for whites, but is unproductive on other margins.

IV. Discussion

In sum, segregation creates places where whites are less likely to be either very badly-off or very well-off, and where blacks are worse-off across the board, compared to places that are less segregated. These equilibrium characteristics may reflect differences in the effect of segregation on *people*—for example, segregated cities may transfer resources to at-risk whites at the expense of blacks and the well-off. Alternatively, they could strictly reflect the effect of segregation on *places* through differential migration—for example, blue-collar whites could prefer segregated cities, while other groups prefer less segregated cities. Moreover, these effects could reinforce each other, so that in equilibrium both are at work. The empirical results are most consistent with the hypothesis that both of these effects are at work.

Ordinary least squares estimates do not differ significantly from two-stage least squares estimates for individual characteristics, such as educational outcomes and black unemployment. However, the OLS estimates appear to significantly understate segregation's role in reducing white poverty and unemployment. In addition, OLS significantly understates the negative effects of segregation on black poverty and on the share of both whites and blacks with high education and income. Overall, the correlations between segregation and outcomes seem to understate the extent to which segregation drives better outcomes for relatively disadvantaged whites at the expense of the average characteristics of other groups.

Previous research on this topic has failed to identify these effects of segregation on places, because researchers have lacked a good instrument for metropolitan segregation. They have therefore been unable to separate the effect of segregation on aggregate city outcomes from reverse causality and omitted variables. My results suggest interpreting findings by Boustan (2006), Bayer et al. (2005), and Baum-Snow (2007) on segregation-reinforcing moves by individuals as coping strategies that are relevant *within* segregated urban areas. As modeled by Schelling (1971), segregated equilibria may be difficult to change through individual market action. Given the limited choices available in segregated MSAs, people may prefer to live in neighborhoods with others like themselves rather than in neighborhoods where they are in the minority. Thus, within MSAs they may make decisions that reinforce segregation, yet still move out of those cities when possible (Vigdor 2006). The implication for cities is that actions to reduce aggregate segregation may help to correct the type of market failure outlined in Schelling (1971) and to increase demand for the MSA.

The implication that segregation may improve outcomes for some whites is consistent with work by Card and Krueger (1992), who argue that racial division increases the ability of local government to transfer resources from the black to the white community. As argued by Vigdor (2006), smaller racial achievement gaps in some cities than in others can be attributed partially to smaller resource disparities in those cities. Collins and Margo (2000) have argued, however, that segregation only became bad for black outcomes after the civil rights movement. Although that question is beyond the scope of this paper, the results of ongoing research are consistent with such a hypothesis: Ananat and Washington (2006), using railroad division to predict segregation, show that the post-civil rights development of black political power has been weaker in more segregated cities. In future research, I will explore in more detail the mechanisms through which segregation appears to affect city characteristics. Besides political power, some plausible channels for these effects include the location of metropolitan amenities and disamenities, economic redistribution, and school finance.

To answer the more general question posed by Cutler and Glaeser (1997), ghettoes appear to be good for the human capital of some groups but are likely bad for others. Moreover, aggregate demand implies that segregation is a metropolitan disamenity. In other words, and in what is perhaps the most important finding of this paper, revealed preferences suggest that the average American, when choosing a city, considers ghettoes to be bad.

Appendix A. Extracting the Railroad Division Index from Maps

Figure A illustrates the process of extracting railroad data through the example of Anaheim, CA. For each city, its map or maps were used to first identify its physical size, shape and location at the time its map was drawn. A Geographic Information Systems program, ArcGIS, was used to create a convex polygon that was the smallest such polygon that could contain the entire densely inhabited urban area. Dense habitation, defined as including any area with houses and frequent, regular cross-streets, was identified by visual examination. ArcGIS then was used to identify the centroid of this polygon, and this point was defined as the historical city center. A four-kilometer radius circle around this point became the level of observation for the measurement of railroads. This approach meant that differences in initial city area would not distort the measurement of initial railroads: cities that were, at the time, very small still would be coded with railroads that affected later development, after the population had expanded; cities that were already large would have only those railroads in their center cities included. It should be noted, however, that about 75% of the cities were smaller than 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.

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

The final sample of 121 cities is derived as follows: Cutler and Glaeser (1997) provide data for all MSAs with at least 1000 black residents. Of these, I include only those MSAs in states that were not slave-owning at the time of the Civil War, because those states had few African-Americans prior to the Great Migration.[20] Further, my sample was limited by the set of historical maps held by the Harvard Map Library.[21] 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

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

[21] The maps that provide railroad placement information were created by the U.S. Geological Survey as part of an effort to document the country's topography, beginning in the 1880s.[21] These maps display elevation, bodies of water, roads, railroads, and (in many cases) individual representations of non-residential buildings and private homes.[21] 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, itself be treated as an outcome of its potential segregation. Because the Census only provides data for large places, though, it is not possible to get information for places that are still small. Note that if segregation reduces economic development, then more towns will fail to achieve MSA status and thus be censored in the treatment group than the control group. This will cause an upward bias in the treatment effect estimate of segregation on growth, attenuating it towards zero. Alternatively, if segregation increases economic development, then the bias will run in the other direction; since the sign of the coefficient is now reversed, it is again an attenuation bias. Thus censoring on eventual MSA status should bias, if at all, towards a finding of no result.

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

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

1. *Slope*. Throughout the main period of railroad construction, land was plentiful while both labor and capital were scarce (Atack and Passell 1994). Therefore, land was the marginal input into American railroads in the U.S. (contrary to the experience in Europe).

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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.

Hence, microvariation in ground slope, which was the primary challenge in railroad construction (Wellington, *Economic Theory of the Placement of Railways*, 1911), drove elaborate surface configurations. "American railroads avoided topographic obstacles rather than level them, bridge them, or tunnel through them" (Atack and Passell 1994, p. 444).

2. *Competition*. The first practical railroads, in the early 19th century, were a product of pre-capitalist mercantilism. Typically, a city and its leading businessmen would fund the building of a line from an agricultural area to its downtown as an incentive to farmers to choose it as a shipping destination (Taylor and Neu 1956).[22] 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.[23]

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 [24] and to cover unsettled areas, was determined by this goal of a national network.

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

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

[24] 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 (one reason may be that most towns were small enough that such barriers or local disamenities didn't have significant meaning). The main objection instead was to connection *per se*. Businesses

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

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

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complained because towns with disconnected trains had developed an economy of middlemen, such as handlers for freight and service establishments for waiting crew and passengers (Taylor and Neu 1956).

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Figure 1. The Ideal Experiment

Figure 1a. City I is perfectly integrated; city S is perfectly segregated.

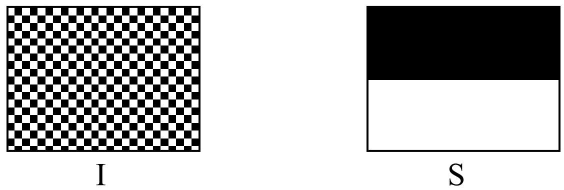


Figure 1b.

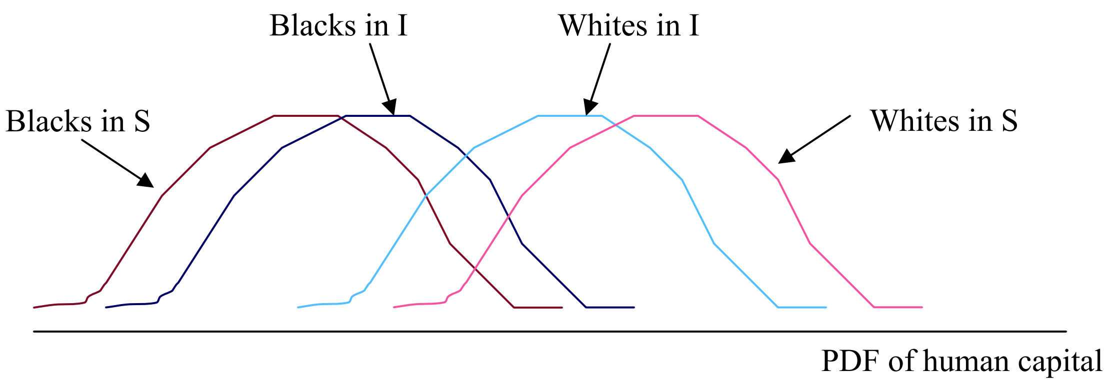


Figure 1c.

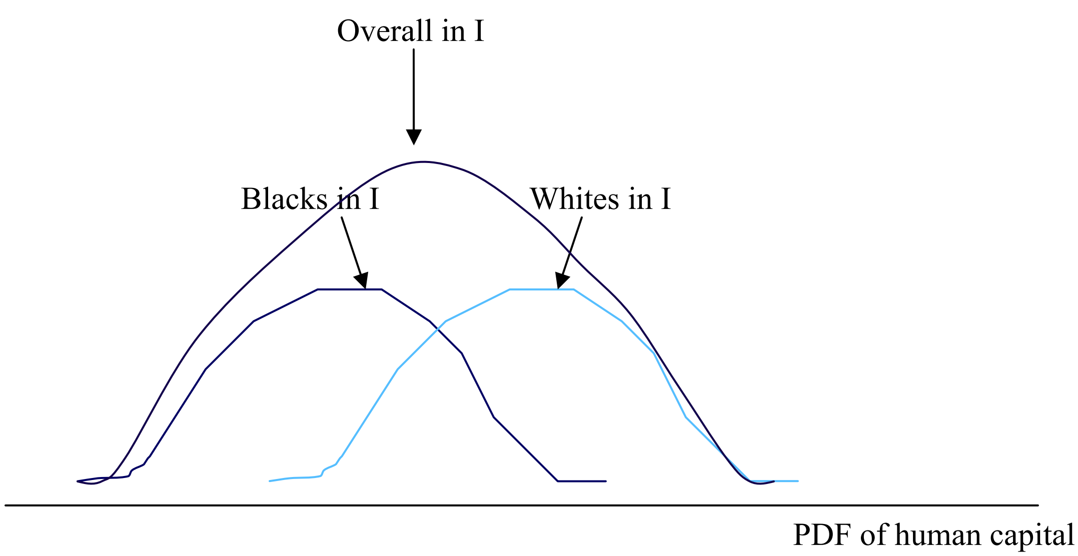


Figure 1d.

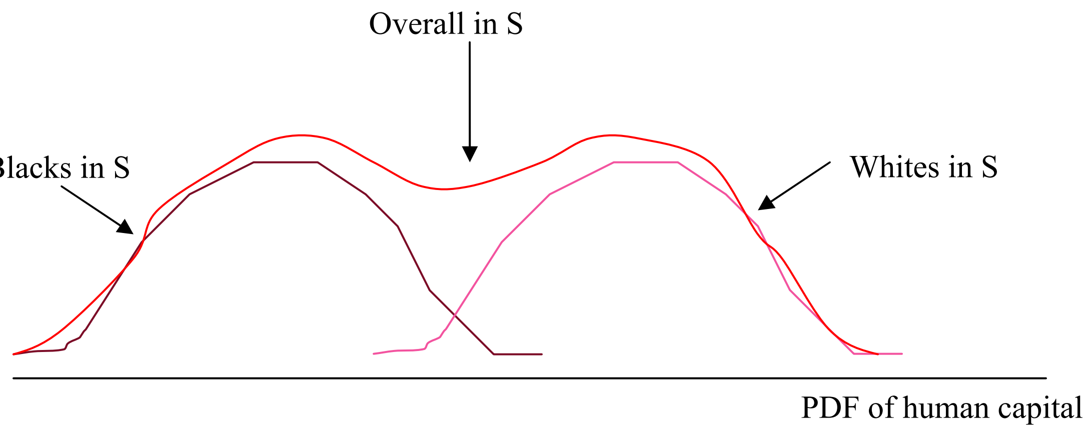


Figure 1e.

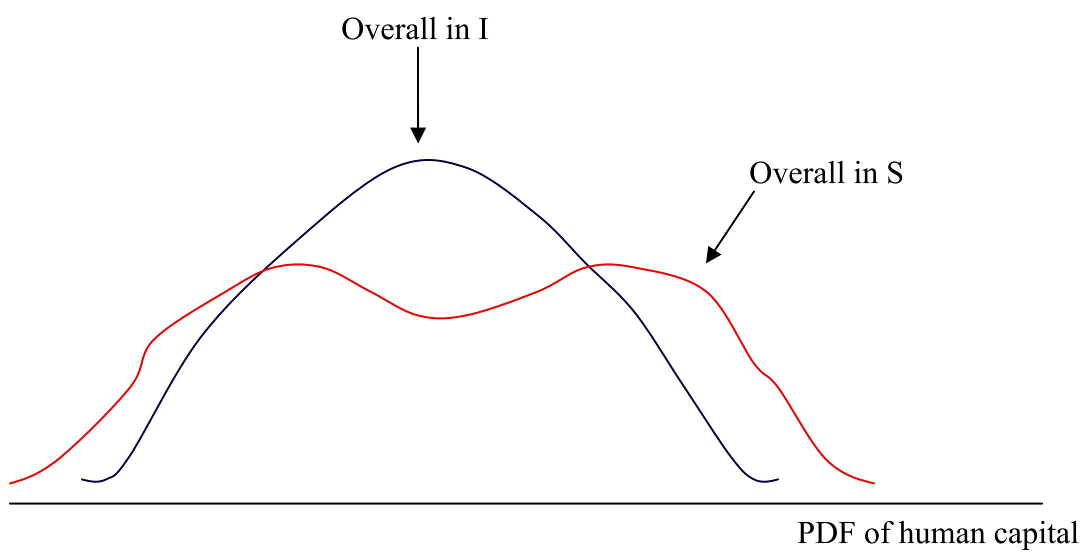


Figure 1f.

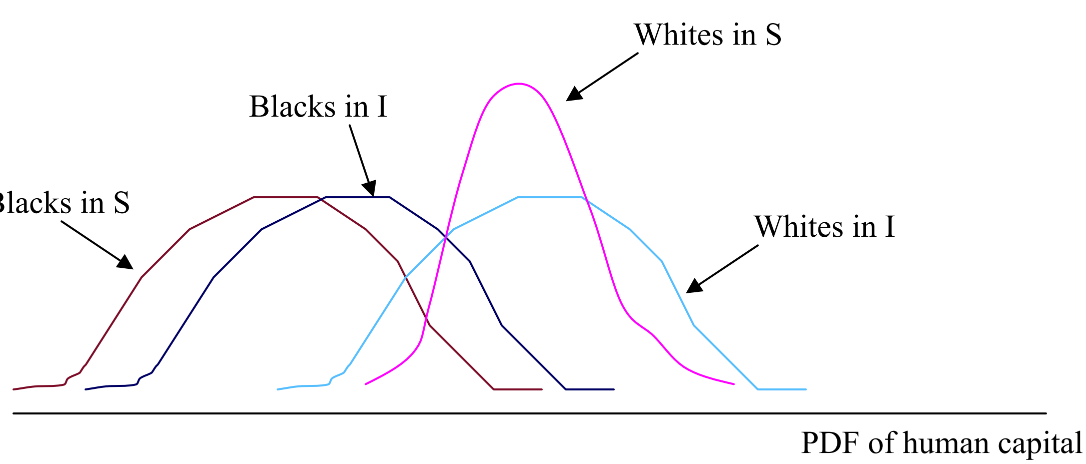
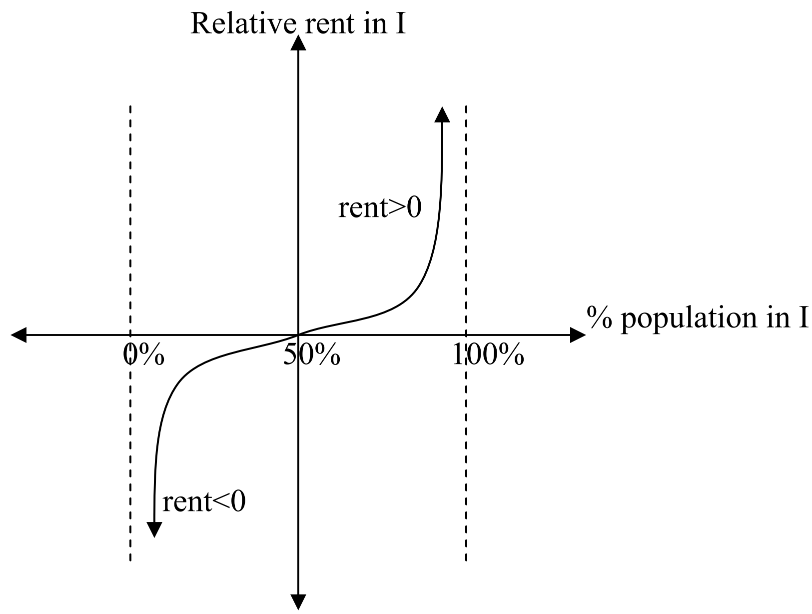


Figure 1g.



A basic rent function, $rent_I = \eta\left(\frac{\frac{1}{2} - pop_I}{pop_I\left(pop_I - 1\right)}\right)$, describes the relative rent in city I.

Note that the parameter η will determine for what range of populations rent will rise less than population (housing supply is elastic) and at what threshold level rent will begin rising faster (housing supply is inelastic). A smaller η implies elastic housing over a broader range of populations.

Figure 1h. Implications of outcome-demand pairs

OBSERVED:		IMPLIED:	
rent/migration in city S vs. I	outcomes in city S vs. I	tastes for segregation	Production effect of segregation on human capital
Lower	Better	-	+
Higher	Worse	+	-
Lower	Worse	at least one -	
Higher	Better	at least one +	

Figure 1h shows the implications of outcome-demand pairs in segregated city S relative to integrated city I. For example, if the group has low demand for segregated cities in spite of the fact that segregation is productive for that group, then that implies that the group must on average have distastes for segregation.

Figure 2.

Binghamton, NY | York, PA

19th century railroads, shown in red within the 4-kilometer 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 manufacturing bases (in fact, Binghamton was somewhat more industrial than York), York became more segregated, as can be seen from the smaller, more concentrated area of African-Americans near the railroad-defined neighborhoods at the city's center. Rivers in blue.

Figure 3. Final sample of MSAs

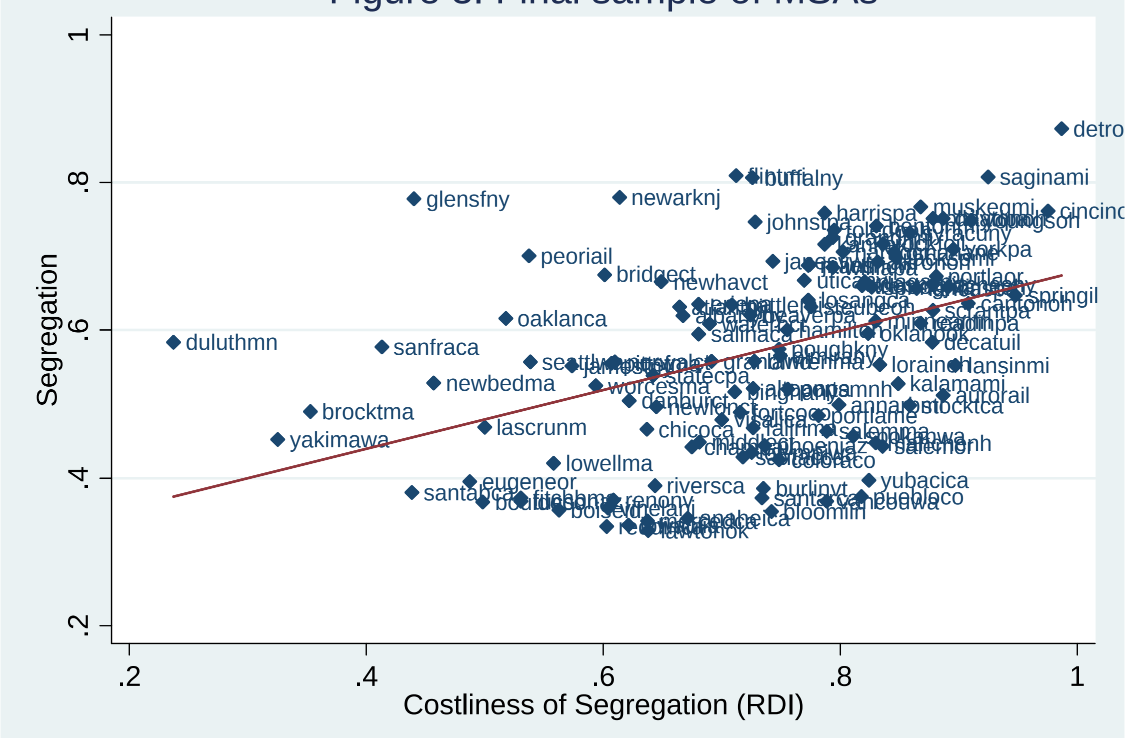


Figure 4. Relationship of cost, demand, and equilibrium segregation

Figure 4a. Proposed relationship between demand and segregation (seg) by costliness of segregation

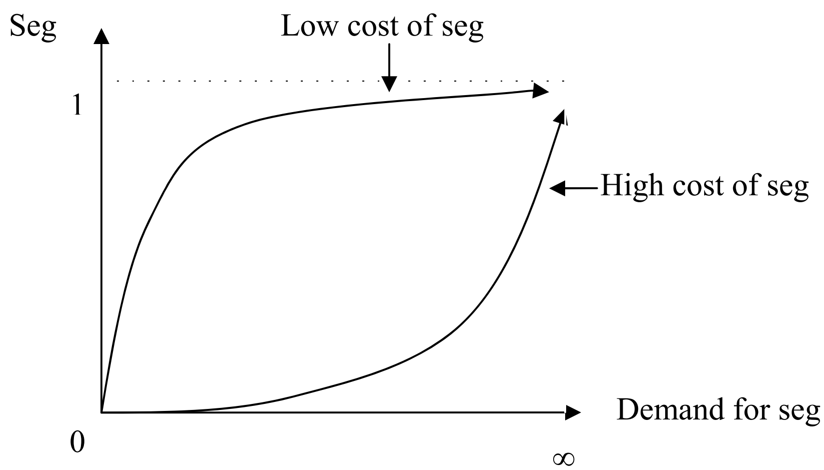


Figure 4b. Empirical relationship between demand and Segregation by costliness of Segregation

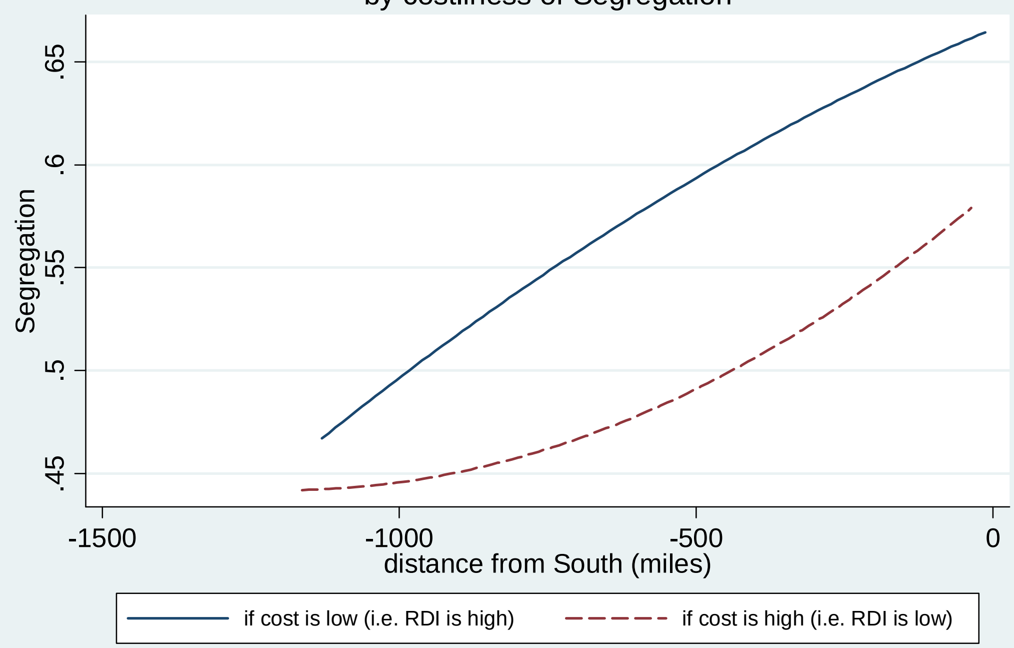


Figure A. Measuring the railroads of Anaheim, CA

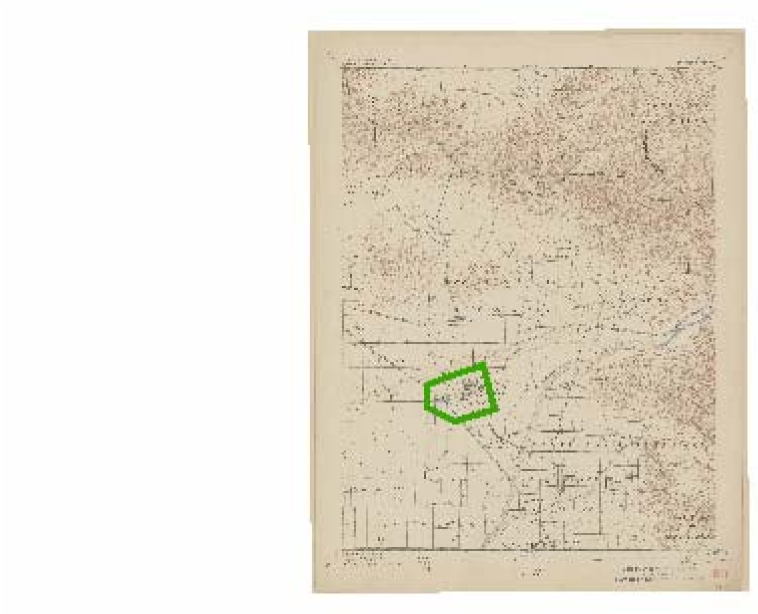


Figure A1. 1894 15’ map showing Anaheim, CA, which is marked in green.

Figure A2. The outline of the densely occupied area of Anaheim, defined as dense housing (each house is represented by a dot) and regular streets. The centroid of the occupied area is marked in blue.

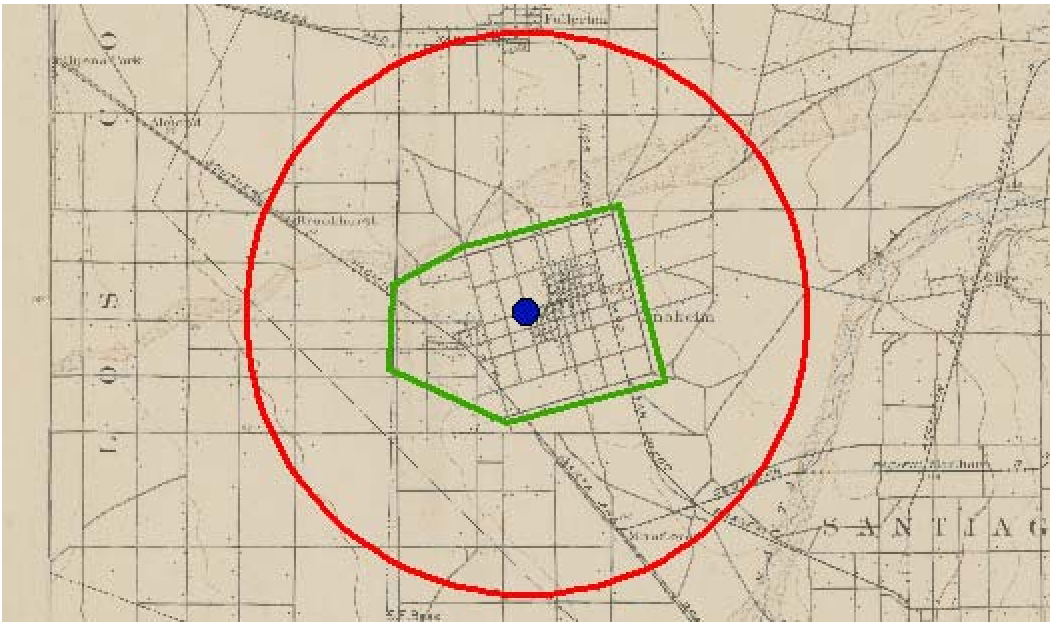


Figure A3. The historical city center is defined as the 4 kilometer-radius circle around the centroid of the historical city, and is shown here in red.

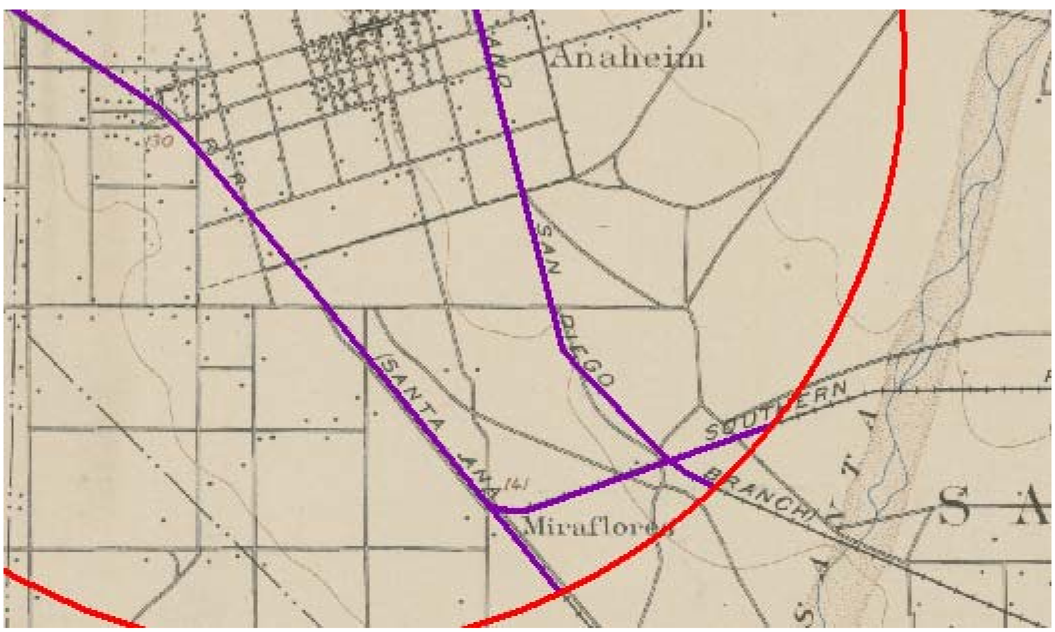


Figure A4. Every railroad within the 4-kilometer circle is marked and measured—detail is shown here in violet.

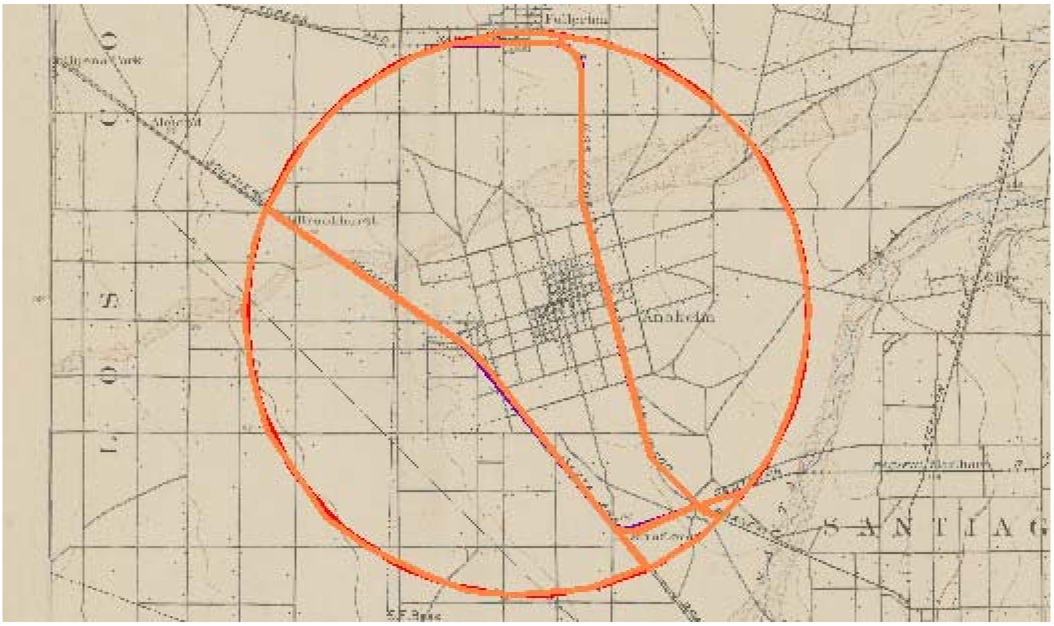


Figure A5. Neighborhoods are defined as polygons created by the intersection of railroads with each other and with the perimeter. Anaheim contains five neighborhoods, shown here in orange. The area of each neighborhood is calculated and used to calculate a RDI measuring the subdivision of the historical city center.

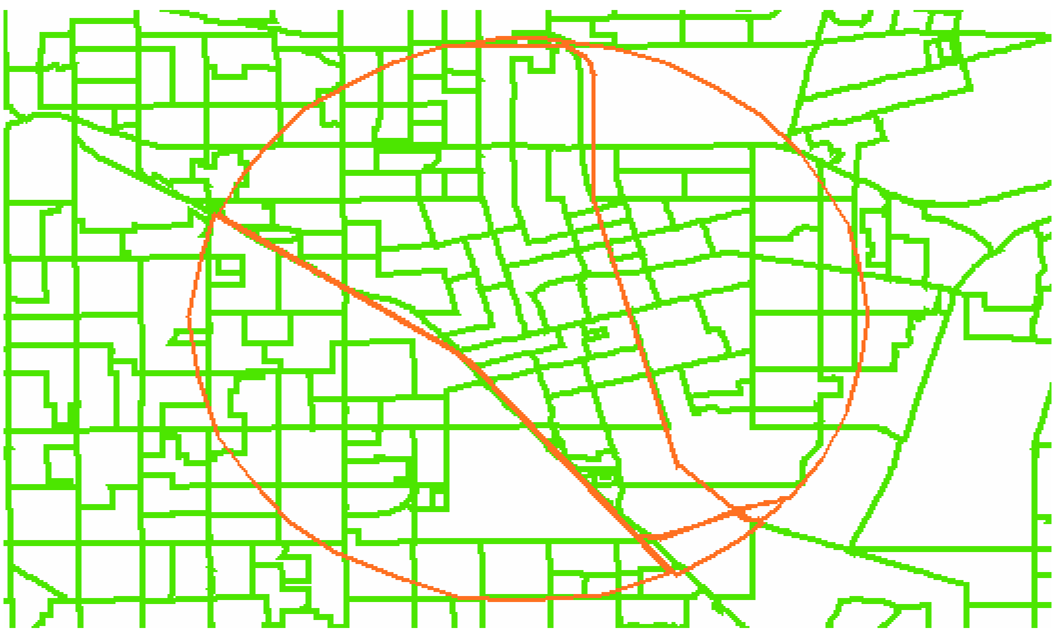


Figure A6. Year 2000 census tracts are shown in green. Note that current neighborhood borders, as defined by the US Census Bureau in 2000, closely follow historical railroad tracks.

Table 1a. Mean Outcomes for Cities with Above- and Below-Median Railroad Division

		Low-RDI cities	*High-RDI cities*	*Difference in Means*	*T-Statistic for Significance of Difference in Means*
Segregation					
Dissimilarity		0.513	0.623	-0.11	-4.89
		(0.017)	(0.015)		
Outcomes:	*among:*				
Poverty rate	*whites*	0.106	0.083	0.023	3.953
		(0.005)	(0.003)		
	blacks	0.253	0.275	-0.023	-1.57
		(0.122)	(0.008)		
Percent of adults who are high school dropouts	*whites*	0.168	0.147	0.021	1.86
		(0.010)	(0.005)		
	blacks	0.24	0.263	-0.023	-1.334
		(0.013)	(0.011)		
Unemployment rate	*whites*	0.055	0.048	0.008	2.735
		(0.023)	(0.002)		
	blacks	0.124	0.126	-0.002	-0.312
		(0.006)	(0.004)		
Percent of adults who are college graduates	*whites*	0.267	0.244	0.023	1.313
		(0.014)	(0.010)		
	blacks	0.166	0.135	0.031	2.209
		(0.011)	(0.008)		
Percent of households with more than $150,000 in income	*whites*	0.045	0.038	0.007	1.245
		(0.005)	(0.003)		
	blacks	0.02	0.015	0.005	2.22
		(0.002)	(0.010)		
N		60	61		

Table 1b. Effect of RDI on Outcomes among Cities with Large vs. Small Expected African-American Inflows

Outcome:	*among:*	*<400 miles from nearest slave state*	*>400 miles from nearest slave state*
Poverty rate	*whites*	-0.127	-0.024
		(0.047)	(0.030)
	blacks	0.175	-0.046
		(0.087)	(0.078)
Percent of adults who are high school dropouts	*whites*	-0.093	0.021
		(0.062)	(0.073)
	blacks	0.031	-0.119
		(0.084)	(0.111)
Unemployment rate	*whites*	-0.054	-0.009
		(0.017)	(0.014)
	blacks	0.003	-0.051
		(0.036)	(0.054)
Percent of adults who are college graduates	*whites*	-0.121	-0.081
		(0.087)	(0.129)
	blacks	-0.127	-0.014
		(0.063)	(0.075)
Percent of households with more than $150,000 in income	*whites*	-0.02	-0.012
		(0.036)	(0.040)
	blacks	-0.009	0.002
		(0.012)	(0.016)
N		90	31

Robust standard errors in parentheses.

Table 2. First Stage Relationship between Railroad Configuration and Segregation

	Dissimilarity	*Isolation*	*Clustering*	*Concentration*	*Centralization*
I. Base specification					
RDI	0.357	0.472	0.511	0.506	0.285
	(0.088)	(0.100)	(0.120)	(0.188)	(0.152)
track length (m/km^2)	18.514	35.514	56.263	-0.668	-7.183
	(10.731)	(13.962)	(14.355)	(10.956)	(4.601)
II. Full specification					
RDI	0.493	0.68	0.78	0.751	0.317
	(0.126)	(0.161)	(0.197)	(0.280)	(0.196)
Proximity to the South (miles/1000)	-0.21	-0.32	-0.428	-0.388	-0.054
	(0.144)	(0.187)	(0.184)	(0.358)	(0.290)
*RDI*proximity (miles/1000)*	0.49	0.68	0.781	0.779	0.04
	(0.200)	(0.260)	(0.266)	(0.480)	(0.372)
track length (m/km^2)	13.319	29.511	51.149	-7.103	-6.355
	(8.512)	(11.208)	(11.870)	(8.009)	(4.654)
*f-stat for joint sig. of RDI, proximity, RDI*proximity*	17.72	15.37	9.86	6.66	1.42
p-value of f-stat	0	0	0	0	0.242

Robust standard errors in parentheses. N=121.

Table 3. Specification checks

	1910 city characteristics						1920 city characteristics						
	Physical area (square miles/1000)	*Population (thousands)*	*Ethnic dissimilarity index*	*Ethnic isolation index*	*% black*	*Street-cars per cap. (thousands) (1915)*	*% black*	*% literate*	*Labor force particip-ation*	*% of employment in Trade*	*Percent of employment in Manufac-turing*	*Percent of employment in Railroads*	*1990 income seg.*
RDI	23.29	510.357	-0.4	-7.967	-0.013	0.257	-0.005	0.119	-0.018	-0.028	0.265	-0.061	0.073
	(17.447)	(398.246)	(0.303)	(13.012)	(0.02)	(0.181)	(0.03)	(0.062)	(0.041)	(0.146)	(0.222)	(0.059)	(0.054)
Proximity to the South (miles/1,000)	-48.406	-744.81	0.742	0.00017	0	-0.445	0	-0.11	0.076	-0.082	-0.12	-0.021	-7E-07
	(18.298)	(392.108)	(0.261)	(0.0001)	(0.02)	(0.238)	(0.03)	(0.063)	(0.056)	(0.13)	(0.342)	(0.036)	(6E-07)
*RDI*proximity (miles/1,000)*	35.197	916.524	-0.894	-0.00018	0.032	0.509	0.05	0.132	-0.103	0.153	0.399	0.035	8E-07
	(31.735)	(631.729)	(0.355)	(0.0002)	(0.03)	(0.425)	(0.04)	(0.078)	(0.076)	(0.178)	(0.463)	(0.052)	(8E-07)
Track length per square kilometer	-38.668	4238.265	36.072	-914.375	13.905	-4.075	13.764	0.61	-3.432	-0.833	13.784	1.396	-2.102
	(502.551)	(9300.751)	(54.585)	(1891.3)	(1.19)	(20.666)	(1.14)	(0.775)	(1.418)	(2.849)	(9.155)	(2.234)	(1.549)
N	58	46	49	49	46	13	42	121	121	121	121	121	69

Table 4. OLS and 2SLS Estimates of Relationship between Segregation and City Outcomes

Poverty rate		*Percent of adults who are high school dropouts*		*Unemployment rate*		*Percent of adults who are college graduates*		*Percent of households with more than $150,000 in income*	
				OLS					
white	*black*	*white*	*black*	*white*	*black*	*white*	*black*	*white*	*black*
-0.073	0.182	0.041	0.348	-0.015	0.091	-0.156	-0.322	-0.022	-0.025
(0.019)	(0.045)	(0.035)	(0.053)	(0.011)	(0.024)	(0.061)	(0.051)	(0.020)	(0.008)
				IV					
white	*black*	*white*	*black*	*white*	*black*	*white*	*black*	*white*	*black*
-0.144	0.294	-0.002	0.281	-0.085	0.021	-0.231	-0.322	-0.083	-0.049
(0.041)	(0.087)	(0.070)	(0.096)	(0.021)	(0.053)	(0.127)	(0.077)	(0.041)	(0.014)
				IV with region dummies					
white	*black*	*white*	*black*	*white*	*black*	*white*	*black*	*white*	*black*
-0.228	0.181	-0.054	0.108	-0.072	0.027	-0.215	-0.36	-0.008	-0.005
(0.116)	(0.186)	(0.182)	(0.255)	(0.046)	(0.111)	(0.216)	(0.167)	(0.070)	(0.030)

Robust standard errors in parentheses. N=121. The endogenous variable, segregation, is measured using the 1990 dissimilarity index. All 2SLS regressions include RDI, proximity to the South, and RDI*proximity as instruments, and also control for total track length per square kilometer.

Table 5. 2SLS Estimates of Relationship between Segregation and City Demand

Percent of residents who are inmigrants		*Median rent*		*Median rent as a percent of income*		*Share of households with more than one person per room*	
				OLS			
white	*black*	*white*	*black*	*white*	*black*	*white*	*black*
0.153	0.294	314	392	8.535	3.49	0.062	0.103
(0.032)	(0.052)	(84)	(76)	(1.337)	(2.676)	(0.014)	(0.022)
				IV			
white	*black*	*white*	*black*	*white*	*black*	*white*	*black*
0.133	0.303	750	767	15.550	6.646	0.177	0.266
(0.052)	(0.092)	(192)	(149)	(2.510)	(4.318)	(0.033)	(0.050)

Robust standard errors in parentheses. N=121. The endogenous variable, segregation, is measured using the 1990 dissimilarity index. All 2SLS regressions include RDI, proximity to the South, and RDI*proximity as instruments, and also control for total track length per square kilometer.

Table 6. Reduced Form Estimates of Effect of Segregation Instruments on Individual Outcomes of 22- to 30-Year-Olds

	High school dropout	*In poverty*	*College graduate*	*Household income >$150,000*	*Idle*	*Never-married mom*
RDI	-0.068	-0.056	-0.093	-0.036	-0.098	-0.038
	(0.097)	(0.112)	(0.144)	(0.037)	(0.072)	(0.017)
*RDI*black*	0.046	0.246	-0.158	-0.01	0.189	0.218
	(0.033)	(0.042)	(0.035)	(0.007)	(0.027)	(0.020)
Proximity to the South (miles/100,000)	0.178	0.076	-0.106	0.014	0.135	0.052
	(0.143)	(0.093)	(0.130)	(0.030)	(0.066)	(0.018)
*Proximity to the South*black (miles/100,000)*	-0.083	-0.192	0.276	0.022	-0.183	-0.118
	(0.123)	(0.095)	(0.062)	(0.008)	(0.069)	(0.031)
*RDI*proximity (miles/100,000)*	-0.38	-0.139	0.199	-0.022	-0.21	-0.082
	(0.238)	(0.120)	(0.174)	(0.037)	(0.094)	(0.028)
*RDI*proximity*black (miles/100,000)*	0.336	0.431	-0.44	-0.03	0.312	0.253
	(0.226)	(0.181)	(0.106)	(0.016)	(0.103)	(0.053)
Track length per square kilometer	-13.529	-11.342	12.017	1.682	4.304	0.784
	(11.923)	(8.601)	(12.646)	(2.500)	(7.268)	(1.363)
*Track length *black*	32.118	18.354	-5.818	0.34	18.041	6.313
	(14.547)	(13.288)	(9.809)	(1.810)	(11.544)	(6.011)
p-value of f-test for joint significance of:						
RDI and proximity	0.272	0.302	0.033	0.514	0.081	0.015
*RDI, RDI*black, RDI*proximity, and RDI*proximity*black*	0.247	0	0	0.128	0	0
*RDI*black & RDI*proximity*black*	0.154	0	0	0.185	0	0

Robust standard errors in parentheses. N=478,619.

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-level)	0.355	-0.053	0.318	-0.043	0.037	0.586
Isolation index--1940 (ward-level)	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-level)	0.736	-0.029	0.742	-0.019	-0.006	0.862
Dissimilarity index--1940 (ward-level)	0.57	-0.032	0.57	-0.022	0	0.99
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	0.48
Population—1890	129,829	-56323.79	66,044	-19199.36	63,785	0.242
Population—1940	390,895	-170643.4	203,676	-40731.77	187,219	0.206
Population—1970	919,239	-261007.3	681,599	-129697.5	237,640	0.375
Population—1990	689,768	-135048.7	590,189	-96574.45	99,580	0.538
# of wards—1890	17.778	-3.724	13.421	-1.731	4.357	0.288
# of wards—1940	15.929	-2.641	14.122	-1.44	1.807	0.519
# of tracts—1940	146.059	-53.033	103.348	-21.92	42.711	0.417
# of tracts—1970	211.118	-61.515	161.811	-29.971	49.307	0.432
# of tracts—1990	203.687	-44.957	137.496	-20.894	66.191	0.131
Total area—1900	19,283	-5711.826	11,764	-1755.343	7,519	0.147
Total area—1940	32,855	-9499.416	27,137	-6214.421	5,718	0.61
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
% of blacks employed as servants-1915	0.21	-0.015	0.207	-0.013	0.002	0.9
Increase in urban mileage in 1950s	0.237	-0.019	0.248	-0.021	-0.011	0.713
# 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	-0.02	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	-0.52	-0.437	-0.363	1.26	0.044
Person-weighted density	1808.075	-338.133	1270.52	-75.12	537.555	0.049
N	246		121			