The Effects of Racial Segregation on Intergenerational Mobility: Evidence from Historical Railroad Placement*

Eric Chyn
Dartmouth College and NBER

Kareem Haggag
UCLA and NBER

Bryan A. Stuart
Federal Reserve Bank of Philadelphia

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Abstract

This paper provides new evidence on the causal impacts of city-wide racial segregation on intergenerational mobility. We use an instrumental variable approach that relies on plausibly exogenous variation in segregation due to the arrangement of railroad tracks in the nineteenth century (Ananat, 2011). Our analysis finds that higher levels of segregation reduce upward mobility for Black children from households across the income distribution and white children from lower-income households. The decline in upward mobility arises from both causal place and sorting channels. Moreover, segregation lowers primary school test scores and increases incarceration rates, teenage birth rates, and racially conservative attitudes.

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*Chyn: Department of Economics, Dartmouth College, 6106 Rockefeller Center, Hanover, NH 03755, and the National Bureau of Economic Research, eric.t.chyn@dartmouth.edu. **Haggag:** Anderson School of Management, UCLA, 110 Westwood Plaza, Los Angeles, CA 90025, and the National Bureau of Economic Research. **Stuart:** Research Department, Federal Reserve Bank of Philadelphia, 10 Independence Mall, Philadelphia, PA 19106, and IZA, bryan.stuart@phil.frb.org. For helpful comments and suggestions, we thank Patrick Bayer, Fernando Ferreira, and seminar participants at Dalhousie University. The views expressed in this paper are solely those of the authors and do not necessarily reflect the views of the Federal Reserve Bank of Philadelphia or the Federal Reserve System.

1 Introduction

A large literature has documented the important role of place in shaping the long-run outcomes of children. Recent studies have found that upward mobility rates vary considerably across areas in the U.S. and are generally lower for Black children (Chetty et al., 2014; Davis and Mazumder, 2018; Chetty et al., 2020*b*). However, understanding the causal mechanisms underlying disparities in upward mobility remains a key challenge. Existing studies have typically relied on descriptive analyses that measure correlations between upward mobility and characteristics of places.

This paper provides new evidence on the causal impacts of city-wide racial segregation on intergenerational mobility. Our analysis is motivated by prominent work positing that racial sorting affects the life chances of children by reducing access to employment opportunities and important public goods (Wilson, 1987; Massey and Denton, 1993; Durlauf, 1996; Fernandez and Rogerson, 1996). While some papers have estimated causal effects of segregation on education levels and poverty rates (Cutler and Glaeser, 1997; Ananat, 2011), these studies have been unable to estimate impacts on income mobility due to a lack of data on these long-run outcomes.

We make new progress on understanding the effects of segregation by combining a quasi-experimental research design with newly-available data on intergenerational mobility. For our analysis, we rely on the pioneering approach from Ananat (2011) that uses historical railroad configurations in local areas as an instrumental variable (IV) for contemporaneous segregation. This strategy takes advantage of the fact that cities subdivided by railroads in the 19th century became more segregated in the decades after the Great Migration. The main outcomes of interest are contemporaneous measures of upward mobility by race and parental income rank from the Opportunity Atlas (Chetty et al., 2020*a*).

Our IV estimates reveal that racial segregation reduces the intergenerational mobility of Black children, with especially large effects for those from the poorest families. For a child whose parents are at the 1st percentile of the nationwide income distribution, a 1 standard deviation (SD) increase in racial segregation leads to a 4.5 percentile decline in the child's long-run income rank, which amounts to 17% of average mobility for this group. Since Black children born to parents in the 1st percentile end up in the 27th percentile ($17,500 in household income) on average, a drop to the 22nd percentile ($12,666) amounts to $4,834 in lost income. At the 25th percentile, the analogous impact is a 4.0 percentile decline. The negative effects of segregation on mobility are also sizable and statistically significant for Black children whose parents have income at the 50th and 75th percentiles of the distribution.

The negative consequences of segregation are not limited to Black children. Segregation also reduces the upward mobility of white children from lower income families. For a white child whose parents are at the 1st percentile of the nationwide income distribution, a 1 SD increase in racial segregation lowers upward mobility by 3.3 percentiles (9%). We also find evidence of declines in mobility for white children whose parents have income at the 25th and 50th percentiles of the income distribution.

Our analysis also shows that the effects of segregation extend beyond children's long-run income ranks. Using additional data from the Opportunity Atlas, we find that racial segregation leads to large increases in the probability that boys are ultimately incarcerated and in the probability that girls give birth while a teenager. These impacts are especially large, with a 1 SD increase in segregation leading to a 29% increase in incarceration for Black boys from the poorest families and a 22% increase for white boys. Impacts on teenage childbearing are similar in magnitude.

How does segregation shape upward mobility? To assess this question, we undertake two distinct exercises to understand mechanisms. First, we decompose place-specific measures of upward mobility into causal exposure effects (Chetty and Hendren, 2018*a*) and other factors. Exposure effects measure the impact of spending one additional year of childhood living in an area on later-life income for migrant families, while other factors include causal effects of local areas that do not scale with years of exposure to an area and the sorting of parents on unobserved dimensions. We provide evidence that suggests segregation significantly reduces exposure effects for children from lower income families. However, the magnitude of the estimates suggests that about two-thirds of the segregation-induced change in upward mobility arises from other factors.

Our second approach to studying mechanisms involves estimating causal effects of segregation on observable place characteristics that plausibly shape upward mobility. We begin by studying early life human capital, as prior work has shown that differences in school financing are linked to intergenerational mobility (Biasi, 2019). Using test scores from the Stanford Education Data Archive (SEDA), we find that a 1 SD increase in segregation translates into reductions of 0.14-SD for Black students and 0.06-SD for white students. Next, we analyze government expenditures given prior work showing that various public programs have important impacts on long-run child outcomes (East et al., 2017; Bailey et al., 2020). Our analysis shows that racial segregation leads to widespread reductions in government expenditures per capita—a finding that echoes work by Cox et al. (2022) which documents that segregation lowers police expenditures per capita and increases non-white homicide victimization. Finally, motivated by research on the effects of racial discrimination (Bertrand and Duflo, 2017), we study racial attitudes and find that segregation increases white residents' racial resentment and opposition to integration related policies such as affirmative action and race-based school busing. These findings are consistent with Ananat and Washington (2009), which studies alternative measures of racial and political attitudes.

Overall, the main contribution of this paper is to provide new evidence on the link between racial segregation and intergenerational mobility. Segregation has long been a leading candidate to explain persistent economic inequalities between whites and minority groups in the U.S. (Wilson, 1987; Massey and Denton, 1993; Bayer, Charles and Park, 2021). Most directly, our analysis builds on earlier work by Cutler and Glaeser (1997) and Ananat (2011) which finds that segregation worsened average schooling attainment and poverty rates for Black individuals. Relative to these papers, we provide the first analysis of long-run child outcomes by race *and* parent income level and find that segregation harms Black children from nearly all family income levels and white children from lower-income families. In sum, the negative effects of segregation are broader than previously suspected. In addition, our work complements recent research documenting strong negative correlations between racial segregation and rates of upward mobility (Chetty et al., 2014; Andrews et al., 2017; Chetty et al., 2020*b*). We extend on these prior findings in three ways. First, we document that descriptive associations may understate negative impacts of segregation, as ordinary least squares estimates are substantially smaller than our IV results. Second, we show that exposure effects do not account for most of the impact of segregation on upward mobility in our setting. Third, we show segregation affects public good provision in ways that can rationalize the widespread declines in upward mobility we document.

Finally, this paper relates to research on the Great Migration (Boustan, 2010; Collins and Wanamaker, 2015; Boustan, 2016; Shertzer and Walsh, 2019; Calderon, Fouka and Tabellini, 2020; Stuart and Taylor, 2021; Derenoncourt, 2022). Our work is most closely related to recent work by Derenoncourt (2022), which provides evidence that Great Migration population flows reduced upward mobility. While her analysis focuses on the effects of greater levels of Black migration, we focus on the impacts of city-wide racial segregation. Supplementary analyses indicate that racial segregation also affects upward mobility through other channels besides changes in a city's total Black population or the share of residents who are Black.

2 Background on Racial Segregation in the U.S.

Our analysis focuses on U.S. cities outside of the South, where racial segregation has long been a prominent feature (Cutler, Glaeser and Vigdor, 1999; Bayer, Charles and Park, 2021). This phenomenon can be traced back to the Great Migration as nearly 6 million African Americans moved out of the South between 1915 and 1970 in search of better economic and social opportunities. After arriving in Northern cities, these migrants moved to specific neighborhoods due to their relatively disadvantaged economic position and discrimination.

Racial neighborhood sorting historically arose from both centralized and decentralized actions. Racial covenants in many communities prevented the sale of homes to nonwhite individuals in the early 20th century (Rothstein, 2017; Sood, Speagle and Ehrman-Solberg, 2021). These covenants became unenforceable after 1948, but voluntary efforts to limit Black individuals' housing options remained in place. Moreover, realtors refused to serve Black homebuyers in specific neighborhoods, and white mobs threatened Black families with violence and intimidation (Sugrue, 1996; Li, 2021). The arrival of Black migrants was often followed by white households leaving neighborhoods and central cities for less racially diverse areas (Card, Mas and Rothstein, 2008; Boustan, 2010; Shertzer and Walsh, 2019).

Although levels of racial segregation have declined in recent decades, cities that were more segregated during and after the Great Migration continue to be relatively more segregated. For example, metro areas like Cleveland, Chicago, and Detroit were among the ten most-segregated cities in 1970 and 1990. Across all metro areas, the correlation between racial segregation (measured using the dissimilarity index) in 1970 and 1990 is 0.7 (Cutler, Glaeser and Vigdor, 1999).

3 Framework for Understanding Intergenerational Mobility and Segregation

Before turning to our empirical analysis, we discuss how segregation could affect intergenerational mobility in principle. As in Chetty et al. (2014) and Chetty and Hendren (2018*a*), child i's later-life income rank in the nationwide distribution can be summarized with the following equation:

$$y_i = \mu_{c(i)} + \psi_{c(i)} p_i + \epsilon_i, \tag{1}$$

where $c(i)$ is their location during childhood and p_i is their parent's income rank in the nationwide distribution.[1] Equation (1) is a linear projection, so ϵ_i is an orthogonal residual. We allow equation (1) to differ by child race, but suppress that notation for simplicity. Based on this linear relationship, absolute mobility of children is defined as the average nationwide income rank for those who grew up in location c with parents who have nationwide income rank p:

$$\overline{y}_{c,p} = \mu_c + \psi_c p. \tag{2}$$

Equation (2) makes clear that absolute mobility depends on both where children grow up and their parents' income rank.

Prior research suggests that racial segregation may shape a city's absolute mobility rates in several ways. For example, opportunities for minority children may be particularly low if segregation increases exposure to discrimination or reduces access to social networks that facilitate economic success (Wilson, 1987; Massey and Denton, 1993; Cutler and Glaeser, 1997). In addition, children of all races may be negatively impacted if segregation reduces support and funding for local

[1] Prior research shows that the linear specification in equation (1) adequately describes empirical patterns of mobility (Chetty et al., 2014; Chetty and Hendren, 2018*a*,*b*). We assume that all children grow up in a single location to simplify the exposition here, although the measures used in our empirical analysis do not rely on this assumption.

public goods such as schools (Alesina, Baqir and Easterly, 1999). Finally, households may sort systematically across cities with different levels of racial segregation. For example, Vigdor (2002) shows that Black individuals with more education are less likely to migrate into segregated cities than those with less education. To the extent that parents' education affects long-run outcomes of children even after conditioning on parent income, this type of sorting could influence absolute mobility rates.

This framework also clarifies one way in which our study of absolute mobility differs from prior analysis of the effects of segregation on average outcomes (e.g., Cutler and Glaeser, 1997; Ananat, 2011). Formally, the average outcome for children that grow up in location c is $\overline{y}_c = \mu_c + \psi_c \overline{p}_c$. This expression highlights that segregation could affect average child outcomes simply by shifting average parental income in a location ($\overline{p}_c$). This composition effect is not present in our analysis since we study average child outcomes conditional on parental income rank ($\overline{y}_{c,p}$). Nonetheless, absolute mobility could depend on sorting along non-income dimensions, and we examine this issue below.

4 Estimating The Effects of Segregation on Upward Mobility

4.1 Empirical Strategy

To understand how segregation affects income mobility, we estimate regressions of the form:

$$\overline{y}_{c,p} = \alpha_p + \text{Seg}_c \beta_p + \epsilon_{c,p}, \tag{3}$$

where $\overline{y}_{c,p}$ is the absolute mobility measure considered in Section 3 for children that grow up in city c and have parents with income rank p, Seg_c is a measure of racial segregation in 1990, and $\epsilon_{c,p}$ is an error term. Following prior studies (e.g., Cutler and Glaeser, 1997; Ananat, 2011), we measure segregation using the index of dissimilarity:

$$\text{Seg}_c = \frac{1}{2} \sum_{n \in c} \left| \frac{\text{Black}_n}{\text{Black}_c} - \frac{\text{White}_n}{\text{White}_c} \right|, \tag{4}$$

where Black_n is the Black population in census tract n, Black_c is the Black population in the city, and White_n and White_c are defined analogously for white population. This index can be interpreted as the share of the Black population that would have to change neighborhoods to achieve complete integration. The lower bound is 0, indicating complete integration, and the upper bound is 1, indicating complete segregation.

Interpreting OLS estimates of equation (3) as the causal effect of racial segregation on upward mobility is difficult. Segregation arises from many factors—such as local government policies, housing market conditions, the geographic distribution of jobs, and racial animus. These factors could have independent effects on children's long-run outcomes, leading to endogeneity in equation (3). Moreover, the effects of racial segregation could vary based on the factors driving its formation. In this way, OLS estimates may reflect a particular weighted average of heterogeneous effects.[2]

To address the limitations associated with OLS estimates, we rely on prior work by Ananat (2011) which uses a measure of historical railroad placement to construct an IV for contemporaneous segregation in Northern cities. When Black migrants arrived in a city, previously-built railroads served as visible markers that coordinated behaviors among whites (e.g., landlords might not rent to Black families on one side of the tracks). Even as racial boundaries changed during the 20th century, the initial coordination established by railroads facilitated subsequent segregation.

The amount of subdivision generated by railroad track placement influenced the resulting amount of segregation. Intuitively, cities where railroads created a larger number of small, physically separated areas had more potential for racial segregation. To capture this idea, Ananat (2011) uses a railroad division index (RDI):

$$\mathrm{RDI}_c = 1 - \sum_{r \in c} \left(\frac{\mathrm{area}_r}{\mathrm{area}_c} \right)^2, \tag{5}$$

where r indexes "railroad neighborhoods" (polygons constructed by the intersection of historical railroad lines), area_r is the land area in a railroad neighborhood, and area_c is the total land area in city c. The RDI equals one minus a Herfindahl-Hirschman Index in terms of land shares. A city with a single railroad neighborhood would have a RDI of 0, while a city that is divided into a nearly infinite number of railroad neighborhoods would have a RDI of 1.

While we follow Ananat (2011) in using RDI_c as an IV for racial segregation, our main specification differs from her work by not controlling for historical railroad track per square kilometer, a correlate of RDI that could independently affect migration flows and subsequent city outcomes. We make this modeling choice for two reasons. First, recent work by Blandhol et al. (2022) shows that interpreting linear IV estimates as local average treatment effects is not necessarily warranted when covariates are included in the regression. Second, a single outlier in terms of railroad track density leads to sensitivity across models that control for this variable in different ways. The source of this sensitivity is that RDI and railroad track density are strongly correlated when excluding this outlier, which leads to weak instrument problems when attempting to control for railroad track density more flexibly.

[2] One possibility is that long-standing segregation leads to larger reductions in mobility because of its effects on a wide range of local institutions. By comparison, segregation that emerged more recently might have less harmful effects. OLS estimates could reflect both types of segregation.

The validity of this approach rests on the plausibility of an exclusion restriction. We assume that historical railroad placement, RDI_c, is only related to upward mobility via its effects on segregation. Our identification arises in part from geological features, like the slope of land, that affected where historical railroads were built in a city *and* the extent of historical railroad development. Robustness tests discussed in Section 5.1 and balance tests reported in Appendix A provide evidence that supports the assumption that RDI affects upward mobility through racial segregation and not other city characteristics.

In addition to the exclusion restriction, this IV approach requires a relevant first stage. Appendix Figure 1 confirms the finding in Ananat (2011) that higher values of the RDI are associated with increased racial segregation in 1990. The RDI explains 17% of the variation in the 1990 dissimilarity index, and the associated first-stage $F-$statistic is 22.[3]

Our empirical strategy identifies reduced-form effects of segregation that could arise in at least two distinct ways. First, the IV estimates of β_p could stem from contemporaneous changes in the characteristics of local areas that occur due to segregation. For example, segregation in 1990 could influence mobility for children by shaping their access to public goods and opportunities in the labor market. Second, the IV estimates of β_p also could reflect the effects of historical forces. Such a scenario may occur because RDI increased segregation throughout the 20th century and thereby shaped city conditions for past generations. This could matter for the upward mobility of recent cohorts of children if segregation has cumulative effects on local institutions that shape child outcomes.

4.2 Sample and Data Sources

Our main analysis sample consists of the 121 non-Southern metropolitan areas for which Ananat (2011) located 19th century maps needed to construct the RDI. For each area, we use the Opportunity Atlas (Chetty et al., 2020*a*) to construct contemporary measures of race-specific absolute mobility for children whose parents have average income at percentiles 1, 25, 50, 75, and 100 of the nationwide distribution.[4] Mobility is measured by calculating later-life ranks in the nationwide income distribution for children born from 1978–1983 using IRS administrative records on income from 2014–2015 (when these cohorts were aged 31–37). In addition to absolute mobility measures, we study incarceration and teenage pregnancy rates from the Opportunity Atlas.[5]

[3] Robustness tests discussed in Section 5.1 show that our main conclusions do not change when we use approaches that are appropriate regardless of the strength of the instrument.

[4] Chetty et al. (2020*a*) account for the fact that children live in different locations during their childhood by using exposure weights. They construct average income over a 5-year period. The nationwide income distribution used to determine percentiles is not race-specific, which means that a Black and white family at the same percentile have the same income level.

[5] Incarceration is based on the 2010 Census short form, while teenage fertility is based on whether IRS records indicate that a woman claimed a dependent when they were between the ages of 13 and 19.

To examine mechanisms, we link the sample to additional data sources. We use decennial Census data from 1910 to 1990 to measure the Black population share and number of Black residents in a metro area. As an alternative measure of how places influence children's long-run outcomes, we use exposure effect estimates from Chetty and Hendren (2018*b*), which represent the causal effect of spending one additional year of childhood in an area. These estimates are based on the income rank at age 26 for a sample of children whose parents moved once during their childhood using the universe of federal income tax records from 1996–2012. The publicly available data from Chetty and Hendren (2018*b*) allow us to construct exposure effect estimates at income percentiles 1, 25, 50, 75, and 100, as detailed in Appendix B. Unlike measures of upward mobility, exposure effects are pooled across children of all races. To measure schooling outcomes, we use average test scores for white and Black students from SEDA (Reardon et al., 2021).[6] Government expenditure measures come from averaging the amounts reported in the 1987 and 1992 Census of Governments. Finally, we measure political attitudes using survey responses on opposition to school racial integration programs from American National Elections Survey (ANES) waves between 1970 and 1994 and on racial resentment and opposition to affirmative action from Cooperative Congressional Election Study (CCES) waves between 2010 and 2020.[7] All underlying data provide information specific to U.S. counties, which we aggregate to 1990-vintage metro area definitions used by Ananat (2011).[8]

5 Results

5.1 Impacts of Segregation on Intergenerational Mobility

Table 1 presents our main analysis of the effects of racial segregation on upward mobility for Black (Panel A) and white (Panel B) children. Column 1 reports OLS estimates of equation (3) for comparison. Next, column 2 reports our preferred IV estimates based on historical railroad placement. Each row reports the effects on mobility for children whose parents have income at a given percentile.

Our first main finding is that the IV estimates indicate that segregation reduces upward mobility of Black children, especially those from poorer families. For a child whose parents have pre-tax income at the 1st percentile of the nationwide distribution ($2,192), a 1 SD increase in racial segregation leads to a 4.5 percentile decline in the child's long-run income rank. Since the average Black child with parental income at the 1st percentile has income at the 27th percentile of the nationwide

[6]These data cover mandatory state standardized assessments in math and reading language arts for students in grades 3 through 8 during the 2008–09 through 2017–18 school years.

[7]Appendix B provides details on these surveys and the specific questions used.

[8]We construct averages using weights based on 1990 county population for the upward mobility measures and political attitudes and the number of students for the school outcomes. We do not weight sums (e.g., expenditures).

distribution as an adult, the 4.5 percentile decline is equal to 17% of the average mobility for this group. The estimates for children from percentiles 25, 50, and 75 are also significant but smaller in magnitude. For a child with parental income at the 75th percentile, a 1 SD increase in racial segregation leads to a 3.0 percentile (7%) decline in upward mobility. Notably, the OLS estimates understate the negative impacts of segregation.[9]

Our second main finding in Table 1 is that segregation reduces the mobility of white children from lower-income families. The IV estimates show that a 1 SD increase in racial segregation leads to a 3.3 percentile (9%) decrease in upward mobility for white children with parental income at the 1st percentile. The impacts are also negative and statistically significant for white children from percentiles 25 and 50.

Do the estimates in column 2 reflect causal effects of segregation per se on mobility? Previous research by Derenoncourt (2022) shows that the arrival of Black migrants during the Great Migration changed cities in ways that lowered upward mobility. She highlights segregation as one mechanism for the effects of Black population flows, but also discusses distinct mechanisms such as decreases in public expenditures. Because racial segregation is positively correlated with the Black population share and log number of Black residents, it is possible that our results reflect the impacts of these variables instead of segregation.[10] To explore this issue, column 2 of Appendix Table 2 reports results from an augmented specification that controls for both the Black population share and log number of Black residents in each decade between 1910 and 1990. This specification is not our preferred one, because the Black population share and log number of Black residents could be affected by RDI and other factors that led to segregation (i.e., these could be "bad controls"). The effects of segregation on upward mobility are even larger in magnitude in these results.[11] This suggests that our results do not simply reflect differences in the size of the Black population.

The appendix contains additional results that support the robustness of the findings in Table 1. First, columns 3 and 4 of Appendix Table 2 show that results are similar when controlling for historical railroad track density as in Ananat (2011) or 1910–1920 city characteristics that Ananat

[9]As discussed in Section 4.1, OLS and IV estimates could differ for two reasons. First, OLS estimates could suffer from omitted variable bias. A second possibility is that segregation catalyzed by historical railroad placement had more negative impacts on poor Black children, possibly because long-standing segregation led to deeper interpersonal or institutional racism. Consistent with this interpretation, Section 5.4 shows that IV estimates on several potential mechanisms are larger in magnitude than OLS estimates.

[10]In our sample, cities with higher segregation in 1990 also have a higher Black population share (correlation: 0.54) and log number of Black residents (correlation: 0.49).

[11]The explanation for this is that we find a positive relationship between upward mobility and the 1910–1990 Black population measures, *conditional on racial segregation*. For example, this could be explained by Black families moving to metro areas that offer better opportunities for their children. These results do not contradict Derenoncourt (2022), who does not control for racial segregation in her regressions but instead proposes segregation as one mechanism underlying the effects of Great Migration population flows.

(2011) uses for a balance test exercise. Second, column 5 shows that the results are similar when controlling for the 1990 manufacturing employment share, which suggests that our findings are not driven by differential exposure to deindustrialization.[12] These results reduce concerns about omitted variable bias. Third, Appendix Table 3 shows that confidence intervals for our main estimates are similar when using approaches that are appropriate for addressing weak instrument concerns (Anderson and Rubin, 1949; Lee et al., 2021). Fourth, Appendix Figures 2 and 3 show the bivariate relationship between absolute mobility measures for Black and white children and the RDI. The patterns in Table 1 are evident in these scatter plots. These results imply that outliers are not driving our estimates. Finally, we implement the specification check used by Ananat (2011), which relies on the idea that the RDI should only affect outcomes in cities that received a substantial number of Black migrants. Ananat (2011) implements this test by dividing the sample based on whether a city is at least 400 miles away from the South, as cities that were further from the South received fewer migrants.[13] In Appendix Table 4, we show that the relationships between upward mobility and RDI in cities that are within 400 miles of the South mirror the results in Table 1, while coefficients are generally smaller for cities more than 400 miles from the South.

5.2 Impacts of Segregation on Incarceration and Teenage Births

Next, we extend our analysis by studying incarceration (for men) and teenage pregnancy (for women).[14] Panels A and B of Table 2 indicate that racial segregation increases incarceration rates for Black and white children with parental income at the 50th percentile and below. However, the magnitudes are larger for Black individuals. A 1 SD increase in racial segregation leads to a 6.7 percentage point (29%) increase in the probability of incarceration for Black boys from a 1st percentile income family, and a 1.4 percentage point (22%) increase for white boys.[15] There is little effect on incarceration for children from families at the 75th percentile of the income distribution or above, where incarceration rates are much lower.

Panels C and D show that segregation also leads to higher teenage fertility for girls of both races. Similar to our findings for incarceration, the impacts tend to be larger in magnitude for Black children. As seen in column 2, a 1 SD increase in racial segregation raises the probability of a teenage birth for a Black girl from a 1st percentile income family by 11 percentage points (22%). The effect on a white girl from a 1st percentile income family is 6 percentage points (22%). Only for white girls from the richest families do we find no effect of segregation on teenage fertility.

[12] The results are also robust to controlling for the additional 1990 city characteristics used in robustness exercises in Ananat (2011).

[13] Cities further than 400 miles from the South still saw significant increases in the size of the Black population, so we do not view this as a pure placebo test.

[14] We focus on incarceration for men because incarceration rates for women are considerably lower.

[15] Appendix Table 5 shows that these results are robust to controlling for different sets of observed variables.

5.3 Impacts of Segregation on Childhood Exposure Effects

So far, we have shown that racial segregation decreases upward mobility. The change in upward mobility could arise from impacts of segregation on place-specific exposure effects (Chetty and Hendren, 2018*a*) or other factors such as household sorting to cities based on non-income characteristics or causal effects that do not scale with exposure.[16] To make this point formally, consider the following decomposition of mobility for children who grow up in city c and have parents with income rank p:

$$\overline{y}_{c,p} = \lambda_{c,p} + \theta_{c,p}, \tag{6}$$

where $\lambda_{c,p}$ is a causal exposure effect that does not depend on family characteristics besides income and $\theta_{c,p}$ is the city-level average of all other factors that influence mobility for children of parents with income rank p.

To study the degree to which segregation operates due to changes in exposure effects, we use estimates from Chetty and Hendren (2018*b*) of the causal impact of spending a year of childhood living in an area. These estimates are obtained using a research design that relies on variation in children's age at the time of migration. As such, impacts of racial segregation on exposure effects should not reflect sorting (i.e., changes in $\theta_{c,p}$). A key caveat is that exposure effects are only available for pooled samples of children of all races.

Table 3 reports estimates of the effects of segregation on upward mobility and exposure effects. For comparison, columns 1 and 2 reproduce race-specific results on upward mobility from Table 1. In column 3, we report the estimated effects of segregation on pooled upward mobility, which is directly comparable to the pooled measure of exposure effects. Column 4 displays effects of segregation where the dependent variable is an estimate of each city's *full* exposure effect, i.e., we scale the one-year estimated exposure effect by assuming a 20-year duration of childhood exposure.

The easiest-to-interpret estimates in Table 3 are the results for children with parents at lower income percentiles. The effects of racial segregation on upward mobility of Black and white children are most similar in the bottom of the income distribution, so pooled estimates are reasonably informative about both groups at lower incomes. At higher income percentiles, the pooled estimates largely reflect impacts on white children, who constitute a majority of the sample.

We find that racial segregation lowers a city's exposure effects for children from low-income families. Overall, our estimates suggest that 39% (=0.109/0.282) of the effects of segregation on mobility for children at the 1st percentile are due to the impacts on exposure effects. Simi-

[16] Examples of effects that do not scale with years of exposure include the quality of teachers in a particular grade, peer influences in secondary school, and training and employment opportunities for 18-year-olds.

larly, changes in exposure effects account for 31% (=0.060/0.192) of the effects of segregation on mobility for children at the 25th percentile. At higher percentiles of the parent income distribution, we find no evidence of negative impacts on exposure effects. These results suggest that factors besides exposure effects—such as sorting or place effects that do not scale with years of exposure—account for a substantial amount of the effects of segregation on upward mobility.

Interestingly, the finding of a substantial role for sorting effects differs from Chetty and Hendren (2018*b*), which examine the correlation between upward mobility, exposure effects, and racial segregation across all commuting zones in the US (including rural areas and areas in the South). These findings also differ from Derenoncourt (2022), which finds that increases in a city's Black population due to the Great Migration reduced upward mobility for children primarily by changing exposure effects. Future work with more granular data may help explain the conditions under which exposure and selection effects diverge.

5.4 Effects of Segregation on Place Characteristics

To further explore how segregation lowers upward mobility, Table 4 studies segregation and three categories of place characteristics that could serve as mechanisms. First, we examine childhood academic achievement as measured by average scores on standardized tests for primary school students. Segregation reduces test scores of both Black and white students, with a 1 SD increase in segregation leading to a 0.14 SD decline for Black students and a 0.06 SD decline for white students. This finding suggests that the segregation-induced decline in upwards mobility does not simply arise because of worse labor market discrimination or access to jobs (e.g., Bertrand and Mullainathan, 2004; Charles and Guryan, 2011; Kline, Rose and Walters, 2021), but also because of a decrease in children's human capital.

Second, we study government expenditures and find statistically significant and economically meaningful decreases on spending overall. A 1 SD increase in racial segregation decreases total expenditures per capita by 18%. The declines are broad-based, with large and significant reductions in public safety and welfare and health. The decline in education expenditures per capita is smaller in proportional terms (though still sizable at an 11% reduction) and only significant at the 13% level. The decrease in public safety expenditures is consistent with Cox et al. (2022), who find that racial segregation also reduces police expenditures per capita. The decrease in public spending and associated deterioration in neighborhood and school conditions likely contribute to the decline in upward mobility.

Finally, we examine racial attitudes that could affect labor market discrimination and the provision of public goods. Consistent with the contact hypothesis, we find that a 1 SD increase in racial segregation raises white racial resentment by 5%. In line with this, segregation significantly increases stated opposition to race-based affirmative action, government involvement in school racial integration, and school busing policies. Our results build on evidence from Ananat and Washington (2009) which reveals that segregation causes non-Black survey respondents to express more negative feelings toward Black individuals, less support for government aid to Black individuals, and higher likelihoods of identifying as political conservatives and Republicans.[17]

6 Conclusion

Using exogenous variation in racial segregation due to historical railroad placements, this paper shows that segregation leads to widespread reductions in economic mobility. Racial segregation constrains the upward mobility of Black children across the parental income distribution, as well as the mobility of white children from lower-income households. Segregation also increases the chances that boys will be incarcerated and girls will have a child as a teenager.

We conduct two exercises to explore the mechanisms that drive our main results. First, segregation lowers mobility due to both reductions in causal exposure effects and other factors such as sorting along non-income dimensions. Second, segregation has adverse impacts on the characteristics of places that could play an important role in shaping upward mobility. Specifically, we find that segregation leads to reductions in primary school test scores and government expenditures, in addition to worsening racial attitudes and support for integration policies.

We conclude with back-of-the-envelope calculations that use our estimates to understand the aggregate economic costs of racial segregation. As explained in Appendix D, we combine our main IV estimates with information on the segregation experienced by individuals in our sample and estimates of the total number of children by race. The results suggest that eliminating the harmful effects of segregation would increase upward mobility by 78% for the poorest Black children and by 43% for the poorest white children. Increases in mobility for children from richer families are smaller but sizable. Moreover, because segregation especially reduces the upward mobility of Black children, eliminating the effects of segregation would reduce the Black-white mobility gap by over 64%. These increases in upward mobility would translate into an increase in children's long-run income of nearly $1 billion per year (Appendix Table 7). The benefits likely would extend to subsequent generations as well. Although Black-white segregation in the US has declined since 1970, it remains a defining feature of most cities, which suggests policy efforts to reduce its harmful impacts have significant potential for enhancing economic growth and equality.

[17]The main finding of Ananat and Washington (2009) is that segregation reduces the ability to elect Representatives who vote in favor of legislation favored by Black citizens. While their mechanisms analysis also uses the ANES, we use distinct questions regarding attitudes toward school racial integration and school busing policies. Our analysis also differs by using CCES data. An advantage of the CCES is that it has complete coverage of the metro areas in our sample, whereas the ANES contains respondents in less than half of the areas. CCES data contain measures of racial resentment and opposition to affirmative action that may overlap conceptually with the feelings thermometer and the aid attitudes studied in Ananat and Washington (2009).

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Table 1: Effects of Racial Segregation on Upward Mobility, by Race and Parental Income Rank in Nationwide Distribution

	OLS	2SLS		
	1990 Dissimilarity Index (1)	1990 Dissimilarity Index (2)	Effect of 1 SD increase (3)	Mean of Dep. Var (4)
Panel A. Black Mobility				
1st percentile	-0.105***	-0.333***	-0.045	0.270
	(0.026)	(0.093)		
25th percentile	-0.100***	-0.292***	-0.040	0.339
	(0.021)	(0.073)		
50th percentile	-0.096***	-0.258***	-0.035	0.397
	(0.022)	(0.064)		
75th percentile	-0.092***	-0.224***	-0.030	0.455
	(0.028)	(0.067)		
100th percentile	-0.081	-0.131	-0.018	0.612
	(0.052)	(0.114)		
Panel B. White Mobility				
1st percentile	-0.058**	-0.243***	-0.033	0.357
	(0.027)	(0.063)		
25th percentile	-0.019	-0.160***	-0.022	0.450
	(0.021)	(0.047)		
50th percentile	0.012	-0.094**	-0.013	0.523
	(0.018)	(0.037)		
75th percentile	0.044***	-0.025	-0.003	0.601
	(0.016)	(0.032)		
100th percentile	0.097***	0.090**	0.012	0.728
	(0.019)	(0.041)		

Notes: This table reports point estimates and heteroskedasticity robust standard errors (in parentheses) from regression models in which the key independent variable is the racial dissimilarity index in 1990. Each combination of cells reports results from models where the dependent variable is upward mobility for different groups of children (e.g., the first row reports effects on upward mobility for Black children whose parents' income is in the 1st percentile of the nationwide income distribution). Column 1 presents ordinary least squares estimates, while column 2 presents estimates in which the dissimilarity index is instrumented by the railroad division index (RDI). Column 3 scales the coefficients reported in column 2 by one standard deviation of the dissimilarity index (0.135), and column 4 reports the mean of the dependent variable. Sample contains 121 non-Southern metro areas for which the RDI variable is available.
Source: Authors' calculations using data from Ananat (2011) and Chetty et al. (2020*a*).

Table 2: Effects of Racial Segregation on Incarceration and Teenage Births, by Race and Parental Income Rank in Nationwide Distribution

	OLS	2SLS		
	1990 Dissimilarity Index (1)	1990 Dissimilarity Index (2)	Effect of 1 SD increase (3)	Mean of Dep. Var (4)
Panel A. Black Male Incarceration				
1st percentile	0.157**	0.499***	0.067	0.233
	(0.069)	(0.164)		
25th percentile	0.095***	0.247***	0.033	0.131
	(0.030)	(0.073)		
50th percentile	0.067***	0.134***	0.018	0.085
	(0.020)	(0.050)		
75th percentile	0.048**	0.056	0.008	0.053
	(0.022)	(0.055)		
100th percentile	0.031	-0.013	-0.002	0.025
	(0.029)	(0.072)		
Panel B. White Male Incarceration				
1st percentile	0.006	0.100**	0.014	0.063
	(0.014)	(0.042)		
25th percentile	0.001	0.042**	0.006	0.029
	(0.006)	(0.018)		
50th percentile	-0.002	0.017**	0.002	0.015
	(0.003)	(0.008)		
75th percentile	-0.003	0.004	0.001	0.007
	(0.002)	(0.004)		
100th percentile	-0.004**	-0.007*	-0.001	0.001
	(0.002)	(0.004)		
	(0.002)	(0.005)		
Panel C. Black Female Teenage Birth				
1st percentile	0.428***	0.802***	0.108	0.488
	(0.077)	(0.196)		
25th percentile	0.375***	0.709***	0.096	0.396
	(0.060)	(0.144)		
50th percentile	0.315***	0.604***	0.082	0.292
	(0.048)	(0.104)		
75th percentile	0.268***	0.522***	0.071	0.210
	(0.047)	(0.102)		
100th percentile	0.182***	0.371**	0.050	0.061
	(0.067)	(0.165)		
Panel D. White Female Teenage Birth				
1st percentile	0.079	0.457***	0.062	0.278
	(0.055)	(0.145)		
25th percentile	0.048	0.327***	0.044	0.206
	(0.041)	(0.106)		
50th percentile	0.019	0.207***	0.028	0.140
	(0.028)	(0.070)		
75th percentile	-0.006	0.100**	0.014	0.081
	(0.018)	(0.039)		
100th percentile	-0.036***	-0.021	-0.003	0.014
	(0.011)	(0.018)		

Notes: This table reports point estimates and heteroskedasticity robust standard errors (in parentheses) from models in which the key independent variable is the racial dissimilarity index in 1990. The outcome variables are incarceration rates for men and teenage birth rates for women. See notes to Table 1 for additional details on specification, sample, and sources.

Table 3: Decomposing the Effects of Racial Segregation on Upward Mobility into Exposure Effects and Other Factors

	Dependent Variable:				
	Black upward mobility (1)	White upward mobility (2)	Pooled upward mobility (3)	Pooled exposure effect (4)	Pooled non-exposure effect (5)
Panel A. 1st Percentile					
1990 Dissimilarity Index	-0.333***	-0.243***	-0.282***	-0.109***	-0.172***
	(0.093)	(0.063)	(0.067)	(0.033)	(0.050)
Effect of 1 SD increase	-0.045	-0.033	-0.038	-0.015	-0.023
Mean of Dep. Var	0.270	0.357	0.322	-0.003	0.325
Panel B. 25th Percentile					
1990 Dissimilarity Index	-0.292***	-0.160***	-0.192***	-0.060**	-0.132***
	(0.073)	(0.047)	(0.052)	(0.023)	(0.039)
Effect of 1 SD increase	-0.040	-0.022	-0.026	-0.008	-0.018
Mean of Dep. Var	0.339	0.450	0.416	-0.002	0.418
Panel C. 50th Percentile					
1990 Dissimilarity Index	-0.258***	-0.094**	-0.108***	-0.008	-0.100***
	(0.064)	(0.037)	(0.040)	(0.019)	(0.032)
Effect of 1 SD increase	-0.035	-0.013	-0.015	-0.001	-0.013
Mean of Dep. Var	0.397	0.523	0.503	-0.002	0.504
Panel D. 75th Percentile					
1990 Dissimilarity Index	-0.224***	-0.025	-0.028	0.043*	-0.071**
	(0.067)	(0.032)	(0.036)	(0.024)	(0.028)
Effect of 1 SD increase	-0.030	-0.003	-0.004	0.006	-0.010
Mean of Dep. Var	0.455	0.601	0.586	-0.001	0.587
Panel E. 100th Percentile					
1990 Dissimilarity Index	-0.131	0.090**	0.101**	0.095***	0.006
	(0.114)	(0.041)	(0.046)	(0.035)	(0.030)
Effect of 1 SD increase	-0.018	0.012	0.014	0.013	0.001
Mean of Dep. Var	0.612	0.728	0.720	-0.001	0.721

Notes: This table reports point estimates and heteroskedasticity robust standard errors (in parentheses) from models in which the key independent variable is the racial dissimilarity index in 1990. In all regressions the dissimilarity index is instrumented by the railroad division index (RDI). Columns 1 and 2 repeat the estimates from column 2 of Table 1. Column 3 reports comparable estimates for a pooled sample consisting of children of all races. In column 4 the dependent variable is the full-childhood exposure effect from Chetty and Hendren (2018*b*). In column 5 the dependent variable is the component of upward mobility not explained by exposure effects (equal to the outcome in column 3 minus the outcome in column 4). See notes to Table 1 for additional details on specification, sample, and sources.

Table 4: Effects of Racial Segregation on Test Scores, Public Expenditures, and Racial Conservatism

	OLS	2SLS		
Dependent variable	1990 Dissimilarity Index (1)	1990 Dissimilarity Index (2)	Effect of 1 SD increase (3)	Mean of Dep. Var (4)
Black test scores	-0.518***	-1.033***	-0.140	-0.494
	(0.141)	(0.320)		
White test scores	-0.040	-0.475	-0.064	0.248
	(0.140)	(0.306)		
Total expenditures per capita	-1.709**	-2.790**	-0.377	2.052
	(0.756)	(1.251)		
Education expenditures per capita	-0.565*	-0.745	-0.101	0.942
	(0.297)	(0.487)		
Public safety expenditures per capita	-0.322***	-0.465**	-0.063	0.230
	(0.124)	(0.188)		
Welfare and health expenditures per capita	-0.448*	-0.717*	-0.097	0.298
	(0.241)	(0.414)		
Racial resentment	0.687***	1.285***	0.174	3.390
	(0.194)	(0.450)		
Opposition to affirmative action	0.325***	0.855***	0.116	3.047
	(0.122)	(0.276)		
Opposition to school integration	0.672***	1.077***	0.146	1.079
	(0.162)	(0.336)		
Opposition to school busing	0.706	2.796***	0.378	5.954
	(0.484)	(0.839)		

Notes: This table reports point estimates and heteroskedasticity robust standard errors (in parentheses) from models in which the key independent variable is the racial dissimilarity index in 1990. The dependent variables in rows 1 and 2 are state standardized test scores for students in grades 3 to 8 across the 2008–2009 to 2017–2018 academic years. Expenditures per capita (rows 3–6) are the average from 1987 and 1992, measured in thousands of 1990 dollars per person. Racial resentment and measures of opposition to policies (rows 7–10) are constructed using survey responses, as detailed in Appendix B. Each cell of the table has 121 metro observations, except for the cells in the final two rows, which have 53 and 47 metros respectively. See notes to Table 1 for additional details on specification and sample.
Source: Authors' calculations using data from Ananat (2011), Reardon et al. (2021), U.S. Bureau of the Census (2015), Ansolabehere (2012); Ansolabehere and Schaffner (2013); Schaffner and Ansolabehere (2015); Schaffner, Ansolabehere and Luks (2019, 2021), American National Election Studies (2021)

Online Appendix

A Balance Table Results

Ananat (2011) shows that the railroad division index (RDI) is not correlated with a number of 1910–1920 city characteristics when controlling for historical railroad track density. This appendix shows that results are similar when not including this control variable, as is done in the main specifications for this paper.

Columns 1–2 of Appendix Table 1 report our replication of Table 1 of Ananat (2011). With minor exceptions, we replicate her results exactly.[18] Only one of the coefficients on RDI is statistically significant at the 10% level. As discussed by Ananat (2011), these results support the assumption that RDI only affects contemporaneous outcomes via impacts on racial segregation. There are significant correlations with historical track density for four variables.

Column 3 shows that results are similar when excluding historical track density as a control variable. One difference is that column 3 displays a significant positive relationship between RDI and the Black population share in 1910 and 1920. A natural explanation is that places with a higher RDI were more connected to the South via railroads, which facilitated migration in the early twentieth century.[19] The coefficient for 1920 percent literate is significant at the 10% level and identical to the estimate from column 1. The coefficient for 1920 percent of employment in manufacturing is also significant at the 10% level, but very similar in magnitude to the estimate in column 1. Given the SD of the RDI (0.14) and the dependent variable means, the correlations for percent literate and percent of employment in manufacturing are relatively small in magnitude.

In sum, these results suggest that RDI is a useful IV for 1990 segregation even when excluding historical railroad track density as a control. Moreover, Appendix Tables 2 and 5 show that our IV estimates are similar when controlling for historical railroad track density (column 3) and when controlling for the baseline city characteristics that are available for all metros (column 4).

B Details on Constructing Exposure Effect Estimates by Income Percentiles

This appendix describes how we construct exposure effect estimates at income percentiles 1, 25, 50, 75, and 100 using the publicly available data from Chetty and Hendren (2018*b*).

The publicly available data accompanying Chetty and Hendren (2018*b*) do not report impacts on income rank, but instead report the percentage gain in income from spending another year in each location for children with parents at income percentiles 25 and 75. Chetty and Hendren (2018*b*) describe the steps used to scale impacts on rank into the percentage gain in income for the 25th percentile (see pages 1183–1184), but do not report the same scaling factors for the 75th percentile. However, their Table 3 reports location-specific impacts on rank for the 75th percentile, which means the scaling factor can be inferred. After the 75th percentile impact on rank is identified for each place, the linear structure assumed by Chetty and Hendren (2018*b*) in their equation

[18]The exceptions are for 1920 percent literate, labor force participation, and percent of employment in trade, manufacturing, and railroads. The differences between the results from our regressions and those reported by Ananat (2011) do not change any substantive conclusions.

[19]Even though migration flows of Black individuals out of the South were especially large between 1915 and 1970, there was migration before this period (e.g., Boustan, 2017).

(4) allows us to construct impacts on rank for other percentiles. In particular, they specify that the impact on rank for location c and parental income rank p is $\nu_{p,c} = \nu_c^0 + \nu_c^1 p$. This implies that the slope can be computed as $\nu_c^1 = (\nu_{75,c} - \nu_{25,c})/0.5$, and the intercept can be computed as $\nu_c^0 = \nu_{25,c} - \nu_c^1 \times 0.25$. Given values for ν_c^0 and ν_c^1, we can construct $\nu_{p,c}$ for any value of p.

C Details on Racial and Political Attitudes Survey Questions

This appendix provides details on measures of racial resentment, opposition to affirmative action, and opposition to government involvement in school racial integration and school busing that appear in Table 4.

Racial resentment comes from a pair of questions asked in all of the primary (election year) waves of the CCES from 2010 to 2020 except for 2016, a year in which racial resentment was not included in the CCES common content (Ansolabehere, 2012; Ansolabehere and Schaffner, 2013; Schaffner and Ansolabehere, 2015; Schaffner, Ansolabehere and Luks, 2019, 2021).[20] Specifically, we average responses to Questions A and B (after first reverse-scaling Question A so that higher values correspond to higher levels of resentment):

- *Racial Resentment A*: "The Irish, Italians, Jews and many other minorities overcame prejudice and worked their way up. Blacks should do the same without any special favors."
  (1: Strongly agree – 5: Strongly disagree.)

- *Racial Resentment B*: "Generations of slavery and discrimination have created conditions that make it difficult for Blacks to work their way out of the lower class."
  (1: Strongly agree – 5: Strongly disagree.)

The CCES includes other questions relating to racial resentment in 2018 and 2020, but we limit the measure to the two questions that are consistent across the 5 years.

We also use opposition to affirmative action (asked in 2010, 2012, and 2014) as a relevant policy attitude across the CCES sample. The survey question is:

- *Affirmative Action*: "Affirmative action programs give preference to racial minorities in employment and college admissions in order to correct for past discrimination. Do you support or oppose affirmative action?"
  (1: Strongly Support – 4: Strongly Oppose)

For all CCES questions we limit the sample to white respondents, giving us roughly 10 to 13 thousand respondents in each survey wave in the Ananat (2011) sample of metros. We construct averages using 1990 county population weights.

To capture policy attitudes that may be particularly relevant to racial segregation and the diminished school test scores documented in Table 4, we add attitudes toward government involvement in school racial integration and school busing. To do so, we use the ANES cumulative time series which includes questions that have been asked in at least three waves of the biennial survey (American National Election Studies, 2021). Specifically, we use the following questions:

[20] YouGov conducts the CCES surveys over the Internet, drawing samples using a matched random sampling methodology that aims to create nationally representative samples.

- *School Integration Policies*: "Some people say that the government in Washington should see to it that white and black (1962-1966: colored; 1968,1970: Negro) children go (1964-1970: are allowed to go) to the same schools. Others claim this is not the government's business. Have you been concerned (1986,1990 AND LATER: interested) enough about [in] this question to favor one side over the other?"
  (IF YES) "Do you think the government in Washington should —"
  VALUES:
  1. Yes, R has an opinion: see to it that white and black children go (1962-1970: are allowed to go) to the same schools
  2. Yes, R has an opinion: stay out of this area (except 1962: as it is none of government's business)
  9. No, no opinion; DK; depends; no interest/concern; other; both; pro-con

- *School Busing*: "There is much discussion about the best way to deal with racial problems. Some people think achieving racial integration of schools is so important that it justifies busing children to schools out of their own neighborhoods. Others think letting children go to their neighborhood schools is so important that they oppose busing. Where would you place yourself on this scale, or haven't you thought much about this? " (7-POINT SCALE SHOWN TO R)
  VALUES:
  1. Bus to achieve integration
  2 - 6
  7. Keep children in neighborhood schools
  9. DK; haven't thought much about it

We construct a 3-point "opposition to school integration policies" scale with the highest value (2) corresponding to survey response 2 ("stay out of this area"), an intermediate value (1) corresponding to response 9, and the lowest value (0) corresponding to survey response 1 ("see to it that white and black children go to the same schools"). For the school busing measure, we preserve the same 7-point scale for "opposition to school busing", but set survey response 9 to the midpoint of the scale (4). The school integration policies question is asked in 1962, 1964, 1966, 1968, 1970, 1972, 1976, 1978, 1986, 1990, 1992, 1994, and 2000. The school busing question is asked in 1972, 1974, 1976, 1980, and 1984. However, the geographic identifiers are not consistent across all waves. We therefore limit the sample to years in which the FIPS county code is recorded and provided to researchers (1970, 1978, 1986, 1992, and 1994 for school integration; 1980 and 1984 for school busing). Similar to our procedure with the CCES, we limit the sample to white respondents and construct metro averages using 1990 county population weights. Because the ANES sample is much smaller than the CCES, we are left with just 53 metros that have responses for school integration policies and 47 metros with responses on school busing.[21]

[21] The underlying counts of white survey respondents captured in these metro areas are as follows. School Integration Policies: 251 (1970), 758 (1978), 369 (1986), 312 (1990), 741 (1992), 579 (1994). School Busing: 454 (1980), 331 (1984).

D Details on Calculating the Aggregate Impacts of Segregation

This appendix describes how we attempt to gauge the aggregate impact of eliminating the harmful effects of racial segregation on children's income.

Let $\Delta_{r,p}$ be the increase in long-run income after eliminating segregation for children of race r whose parents have income percentile p. To calculate the total impact of eliminating segregation on children of a given race, we would ideally add these impacts over the race-specific parental income distribution:

$$\Delta_r^* = \sum_p N_{r,p}\Delta_{r,p}, \tag{D1}$$

where $N_{r,p}$ is the number of children of race r whose parents have income at the nationwide, race-invariant income percentile p.

Calculating Δ_r^* is not possible because we only estimate impacts of segregation on a limited number of percentiles and the Opportunity Atlas does not provide data on $N_{r,p}$. Instead, the Opportunity Atlas reports an estimate of the number of children of a given race that lived in a household with parental income below the nationwide median as of year 2000.[22]

We make several simplifying assumptions to gauge the aggregate impacts of eliminating segregation on children's upward mobility. First, we use the impacts of segregation on children whose parents are at the 25th and 75th percentiles of the income distribution as summary measures of the impacts for children from households that are below or above the nationwide median. Letting $N_{r,<50}$ be the number of children of race r whose parents have income below the nationwide median and N_r be the total number of children of race r, we can construct a measure of the aggregate race-specific impact as:

$$\Delta_r = N_{r,<50}\Delta_{r,25} + (N_r - N_{r,<50})\Delta_{r,75}. \tag{D2}$$

To estimate $\Delta_{r,p}$, we convert the coefficient from Table 1 (where the dependent variable is percentiles) into dollars of individual income.[23] We do this by calculating the average dollar amount associated with a 1 percentile increase around the 25th and 75th percentiles of the nationwide income distribution for children. We then scale this dollar impact by the average level of segregation (weighted by the number of children, $N_{\text{black}} + N_{\text{white}}$) to identify the impact of removing segregation's harmful effects.

Given the simple nature of this calculation, several caveats should be kept in mind. This calculation relies on estimates of the impact of segregation on upward mobility. Because we find that impacts on exposure effects are smaller than impacts on upward mobility at the 25th percentile (see Table 3), the coefficients from Table 1 could overstate the gain in children's long-run earnings from eliminating the harmful impacts of segregation. Other considerations suggest that this simple calculation could understate the gains from neutralizing segregation. First, this calculation only applies to children who were younger than age 18 and living in our sample cities as of the

[22]The Opportunity Atlas constructs this estimate by combining publicly available data from the 2000 Census on the number of individuals that are below age 18 and of a given race with estimates from confidential data of the share of children whose parents have income below the nationwide median.

[23]Even though the dependent variable in Table 1 is the percentile of children's long-run household income, we use individual income at this step to avoid double-counting impacts.

year 2000. Second, this calculation does not account for improvements in parents' labor market outcomes due to eliminating the harmful effects of segregation. Given the challenge of quantifying these different channels, we view the simple calculations as only suggestive of the improvements that would be possible if segregation did not harm children's long-run opportunities.

Appendix Table 1: Robustness of Balance Table Results to Excluding Historical Track Density Control

	Model with track density		Model without		
	RDI	Track length per square km	RDI	Dep var mean	*N*
Dependent variable	(1)	(2)	(3)	(4)	(5)
Land area (1000s of sq. miles)	-3.993	-574.401	-5.036	14.626	58
	(11.986)	(553.669)	(11.830)		
1910 population (1000s)	0.666	75.553	0.838	1.527	121
	(1.363)	(134.815)	(1.349)		
1910 ethnic dissimilarity index	0.076	15.343	0.119	0.311	49
	(0.185)	(53.249)	(0.162)		
1910 ethnic isolation index	0.027	-12.439	-0.008	0.055	49
	(0.070)	(17.288)	(0.066)		
1910 percent Black	-0.001	9.236***	0.020*	0.014	121
	(0.010)	(0.650)	(0.011)		
1915 street cars per capita (1000s)	-0.132	3.361	-0.121	0.179	13
	(0.183)	(20.507)	(0.150)		
1920 percent Black	0.013	9.119***	0.034***	0.016	121
	(0.009)	(0.615)	(0.011)		
1920 percent literate	0.053*	0.180	0.053*	0.959	121
	(0.030)	(0.880)	(0.030)		
1920 labor force participation	0.028	-3.427**	0.021	0.419	121
	(0.024)	(1.500)	(0.024)		
1920 percent of empl. in trade	-0.080	-0.152	-0.081	0.058	121
	(0.094)	(2.910)	(0.092)		
1920 percent of empl. in manufacturing	0.191	18.400*	0.233*	0.462	121
	(0.137)	(10.911)	(0.137)		
1920 percent of empl. in railroads	-0.074	1.592	-0.070	0.003	121
	(0.068)	(2.428)	(0.065)		
1990 income segregation	0.032	-2.504	0.027	0.217	69
	(0.032)	(1.626)	(0.032)		

Notes: This table reports results from models in which the dependent variable is a city characteristic and the key independent variable is the railroad division index (RDI). Columns 1–2 report point estimates and heteroskedasticity robust standard errors (in parentheses) from a single model that regresses the indicated dependent variable on the railroad division index (RDI) and historical track density (i.e., railroad track length per square kilometer). Column 3 reports results from models that only include the RDI. Columns 1 and 2 are analogous to Table 1 of Ananat (2011). There are minor unexplained differences between these results and those in her table for 1920 percent literate, labor force participation, and percent of employment variables.
Source: Authors' calculations using data from Ananat (2011) and Cutler, Glaeser and Vigdor (1999).

Appendix Table 2: Effects of Racial Segregation on Upward Mobility, Robustness to Controlling for Observed Variables

	2SLS Coefficient on 1990 Dissimilarity Index				
	(1)	(2)	(3)	(4)	(5)
Panel A. Black Mobility					
1st percentile	-0.333***	-0.482***	-0.343***	-0.336***	-0.336**
	(0.093)	(0.148)	(0.107)	(0.088)	(0.144)
25th percentile	-0.292***	-0.389***	-0.301***	-0.320***	-0.308***
	(0.073)	(0.113)	(0.084)	(0.083)	(0.118)
50th percentile	-0.258***	-0.310***	-0.266***	-0.307***	-0.285***
	(0.064)	(0.097)	(0.074)	(0.088)	(0.108)
75th percentile	-0.224***	-0.231**	-0.232***	-0.293***	-0.263**
	(0.067)	(0.101)	(0.077)	(0.099)	(0.112)
100th percentile	-0.131	-0.019	-0.138	-0.257*	-0.201
	(0.114)	(0.180)	(0.128)	(0.152)	(0.176)
Panel B. White Mobility					
1st percentile	-0.243***	-0.382***	-0.264***	-0.299***	-0.239**
	(0.063)	(0.105)	(0.074)	(0.077)	(0.095)
25th percentile	-0.160***	-0.262***	-0.176***	-0.205***	-0.155**
	(0.047)	(0.078)	(0.056)	(0.056)	(0.070)
50th percentile	-0.094**	-0.167***	-0.107**	-0.131***	-0.090
	(0.037)	(0.058)	(0.045)	(0.043)	(0.055)
75th percentile	-0.025	-0.068*	-0.034	-0.053	-0.021
	(0.032)	(0.041)	(0.038)	(0.038)	(0.047)
100th percentile	0.090**	0.097**	0.087*	0.075	0.093
	(0.041)	(0.040)	(0.045)	(0.055)	(0.061)
Controls					
1910–1990 Black population measures		✓			
Historical railroad track density			✓		
1910–1920 city characteristics				✓	
1990 manufacturing emp. share					✓

Notes: This table reports point estimates and heteroskedasticity robust standard errors (in parentheses) from models in which the key independent variable is the racial dissimilarity index in 1990. In all regressions the dissimilarity index is instrumented by the railroad division index (RDI). Column 1 repeats the baseline results from column 2 of Table 1. The results in column 2 come from specifications that control for log Black population and the Black population share in each decade between 1910 and 1990. The results in column 3 come from specifications that control for historical railroad track length per square kilometer. The results in column 4 come from specifications that control for population and the Black population share in 1910, as well as the following characteristics in 1920: Black population share, literacy rate, labor force participation rate, share of employment in trade, share of employment in manufacturing, and share of employment in railroads. Column 5 controls for the share of individuals employed in manufacturing in 1990. See notes to Table 1 for additional details on sample.

Source: Authors' calculations using data from Ananat (2011), Chetty et al. (2020*a*), and Manson et al. (2021).

Appendix Table 3: Effects of Racial Segregation on Upward Mobility, Robustness to Alternative Confidence Interval Estimates

		Confidence interval		
	Point estimate (1)	Asymptotic (2)	Anderson-Rubin (3)	tF (4)
Panel A. Black Mobility				
1st percentile	-0.333	[-0.515, -0.150]	[-0.587, -0.182]	[-0.569, -0.097]
25th percentile	-0.292	[-0.435, -0.149]	[-0.497, -0.174]	[-0.477, -0.107]
50th percentile	-0.258	[-0.384, -0.131]	[-0.439, -0.153]	[-0.421, -0.094]
75th percentile	-0.224	[-0.355, -0.092]	[-0.407, -0.110]	[-0.394, -0.054]
100th percentile	-0.131	[-0.354, 0.091]	[-0.397, 0.089]	[-0.419, 0.157]
Panel B. White Mobility				
1st percentile	-0.243	[-0.366, -0.120]	[-0.415, -0.137]	[-0.403, -0.084]
25th percentile	-0.160	[-0.252, -0.068]	[-0.288, -0.080]	[-0.279, -0.041]
50th percentile	-0.094	[-0.167, -0.021]	[-0.193, -0.030]	[-0.188, 0.001]
75th percentile	-0.025	[-0.088, 0.039]	[-0.106, 0.036]	[-0.107, 0.058]
100th percentile	0.090	[0.009, 0.170]	[0.004, 0.179]	[-0.014, 0.194]

Notes: This table reports point estimates and confidence intervals from models in which the key independent variable is the racial dissimilarity index in 1990. In all regressions the dissimilarity index is instrumented by the railroad division index (RDI). Column 1 repeats the point estimate ($\hat{\beta}$) from column 2 of Table 1. Column 2 reports the 95-percent confidence interval based on the conventional asymptotic approximation, which is $\hat{\beta} \pm 1.965\hat{se}$, where $\hat{se}$ is the heteroskedasticity robust standard error reported in Table 1. Column 3 reports the Anderson and Rubin (1949) confidence interval, and column 4 reports the Lee et al. (2021) tF confidence interval. See notes to Table 1 for additional details on sample, specification, and data.

Appendix Table 4: Relationship between RDI and Upward Mobility by Distance from the South

	Within 400 miles from South		At least 400 miles from South		
	Railroad Division Index (1)	Effect of 1 SD increase (2)	Railroad Division Index (3)	Effect of 1 SD increase (4)	Mean of Dep. Var (5)
Panel A. Black Mobility					
1st percentile	-0.151***	-0.021	-0.067	-0.009	0.270
	(0.030)		(0.048)		
25th percentile	-0.141***	-0.020	-0.060**	-0.009	0.339
	(0.023)		(0.027)		
50th percentile	-0.132***	-0.019	-0.055***	-0.008	0.397
	(0.023)		(0.020)		
75th percentile	-0.124***	-0.017	-0.049	-0.007	0.455
	(0.027)		(0.030)		
100th percentile	-0.101**	-0.014	-0.035	-0.005	0.612
	(0.049)		(0.082)		
Panel B. White Mobility					
1st percentile	-0.109***	-0.015	-0.047	-0.007	0.357
	(0.028)		(0.035)		
25th percentile	-0.075***	-0.011	-0.042*	-0.006	0.450
	(0.021)		(0.022)		
50th percentile	-0.049***	-0.007	-0.039**	-0.005	0.523
	(0.017)		(0.016)		
75th percentile	-0.021	-0.003	-0.035**	-0.005	0.601
	(0.016)		(0.017)		
100th percentile	0.024	0.003	-0.028	-0.004	0.728
	(0.020)		(0.033)		

Notes: This table reports point estimates and heteroskedasticity robust standard errors (in parentheses) from models in which the key independent variable is the railroad division index (RDI). Columns 1–2 report results for 92 metros that are less than 400 miles from the South, and columns 3–4 report results for 29 metros that are at least 400 miles away from the South. Summary statistics (mean and standard deviation) are calculated for the pooled sample of 121 metros. See notes to Table 1 for additional details on sources.

Appendix Table 5: Effects of Racial Segregation on Incarceration and Teenage Births, Robustness to Controlling for Observed Variables

	2SLS Coefficient on 1990 Dissimilarity Index				
	(1)	(2)	(3)	(4)	(5)
Panel A. Black Male Incarceration					
1st percentile	0.499***	0.800***	0.503***	0.568***	0.651**
	(0.164)	(0.262)	(0.183)	(0.208)	(0.267)
25th percentile	0.247***	0.378***	0.241***	0.279***	0.295***
	(0.073)	(0.112)	(0.081)	(0.083)	(0.111)
50th percentile	0.134***	0.188***	0.124**	0.149**	0.135*
	(0.050)	(0.071)	(0.056)	(0.060)	(0.076)
75th percentile	0.056	0.058	0.043	0.060	0.025
	(0.055)	(0.079)	(0.062)	(0.078)	(0.091)
100th percentile	-0.013	-0.058	-0.029	-0.020	-0.073
	(0.072)	(0.109)	(0.082)	(0.107)	(0.125)
Panel B. White Male Incarceration					
1st percentile	0.100**	0.151**	0.107**	0.101**	0.146**
	(0.042)	(0.064)	(0.049)	(0.046)	(0.072)
25th percentile	0.042**	0.064**	0.045**	0.044**	0.061**
	(0.018)	(0.027)	(0.021)	(0.019)	(0.031)
50th percentile	0.017**	0.027**	0.019**	0.019**	0.025*
	(0.008)	(0.012)	(0.009)	(0.009)	(0.014)
75th percentile	0.004	0.007	0.005	0.006	0.005
	(0.004)	(0.005)	(0.004)	(0.005)	(0.006)
100th percentile	-0.007*	-0.009	-0.007	-0.005	-0.010*
	(0.004)	(0.007)	(0.005)	(0.006)	(0.006)
Panel C. Black Female Teenage Birth					
1st percentile	0.802***	0.781**	0.815***	0.942***	0.804***
	(0.196)	(0.326)	(0.220)	(0.257)	(0.300)
25th percentile	0.709***	0.701***	0.720***	0.820***	0.730***
	(0.144)	(0.235)	(0.161)	(0.185)	(0.218)
50th percentile	0.604***	0.611***	0.613***	0.680***	0.647***
	(0.104)	(0.159)	(0.116)	(0.129)	(0.159)
75th percentile	0.522***	0.540***	0.529***	0.571***	0.582***
	(0.102)	(0.150)	(0.116)	(0.129)	(0.164)
100th percentile	0.371**	0.411	0.375**	0.372*	0.462*
	(0.165)	(0.255)	(0.188)	(0.220)	(0.271)
Panel D. White Female Teenage Birth					
1st percentile	0.457***	0.664***	0.520***	0.533***	0.529**
	(0.145)	(0.207)	(0.170)	(0.163)	(0.228)
25th percentile	0.327***	0.473***	0.373***	0.385***	0.379**
	(0.106)	(0.151)	(0.124)	(0.119)	(0.165)
50th percentile	0.207***	0.299***	0.239***	0.249***	0.243**
	(0.070)	(0.100)	(0.083)	(0.079)	(0.109)
75th percentile	0.100**	0.142**	0.119**	0.127***	0.120**
	(0.039)	(0.056)	(0.047)	(0.045)	(0.060)
100th percentile	-0.021	-0.035	-0.018	-0.011	-0.019
	(0.018)	(0.026)	(0.020)	(0.025)	(0.029)
Controls					
1910–1990 Black population measures		✓			
Historical railroad track density			✓		
1910–1920 city characteristics				✓	
1990 manufacturing emp. share					✓

Notes: This table reports point estimates and heteroskedasticity robust standard errors (in parentheses) from models in which the key independent variable is the racial dissimilarity index in 1990. In all regressions the dissimilarity index is instrumented by the railroad division index (RDI). See notes to Table 2 and Appendix Table 2 for additional details on specifications.

Appendix Table 6: Simple Calculations of Aggregate Changes in Upward Mobility When Eliminating Harmful Effects of Segregation

		Counterfactual Level of Segregation			
		Zero		Sample Minimum	
	Mean upward mobility (1)	CF upward mobility (2)	Percent change (3)	CF upward mobility (4)	Percent change (5)
Panel A. Black Mobility					
1st percentile	0.270	0.480	77.8	0.371	37.2
25th percentile	0.339	0.523	54.4	0.427	26.0
50th percentile	0.397	0.560	41.0	0.475	19.6
75th percentile	0.455	0.596	31.0	0.523	14.9
100th percentile	0.612	0.695	13.5	0.652	6.5
Panel B. White Mobility					
1st percentile	0.357	0.511	43.0	0.431	20.6
25th percentile	0.450	0.551	22.4	0.498	10.7
50th percentile	0.523	0.582	11.3	0.551	5.4
75th percentile	0.601	0.616	2.6	0.608	1.2
100th percentile	0.728	0.671	-7.8	0.701	-3.7
Panel C. Black-White Mobility Gap					
1st percentile	0.087	0.031	-64.4	0.060	-31.0
25th percentile	0.111	0.028	-74.8	0.071	-36.0
50th percentile	0.126	0.022	-82.5	0.076	-39.7
75th percentile	0.146	0.020	-86.3	0.085	-41.8
100th percentile	0.116	-0.024	-120.7	0.049	-57.8

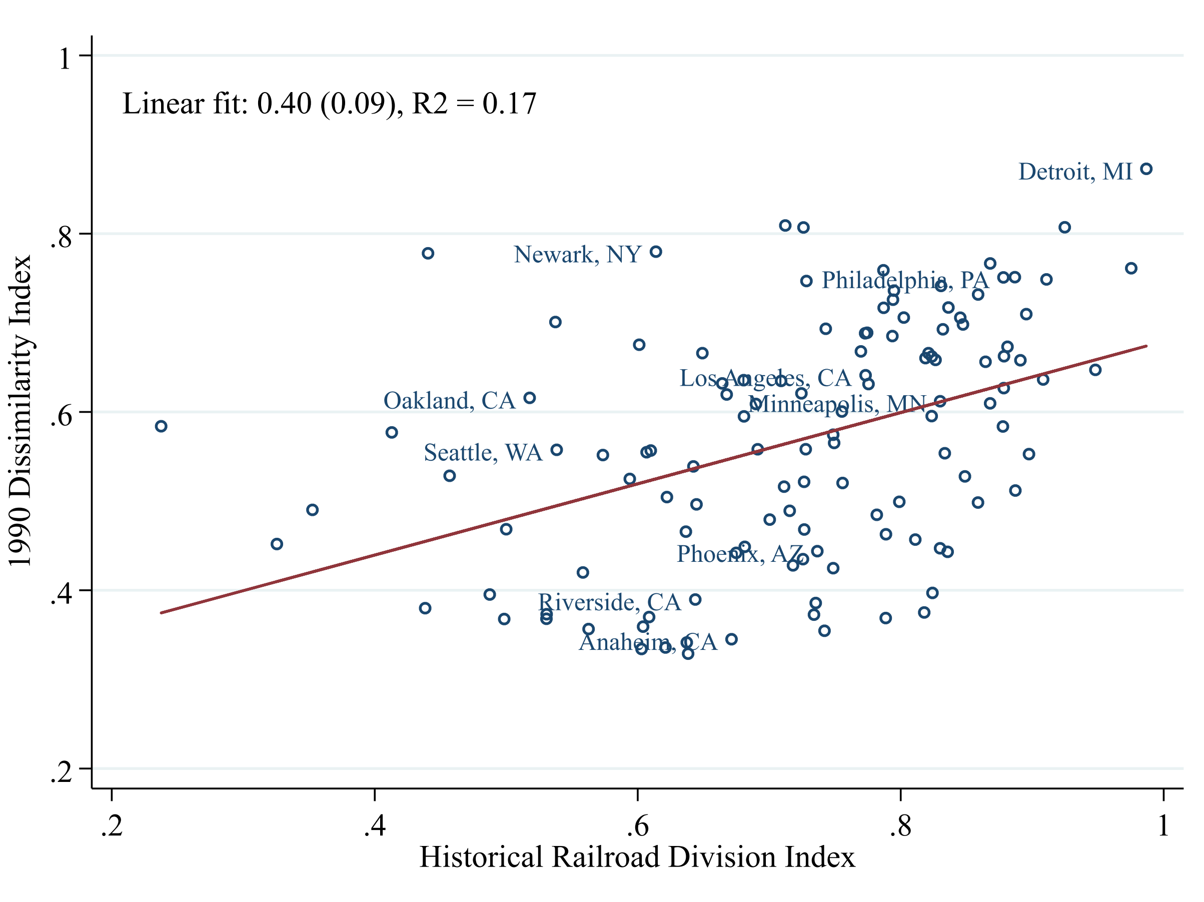
Notes: In Panels A and B, column 1 reports the observed mean upward mobility rate by race and parental income rank. Columns 2–3 consider a counterfactual (CF) scenario in which all cities in our sample have no racial segregation. Column 2 calculates the counterfactual level of mobility, which equals the observed upward mobility rate plus the 2SLS coefficient in Table 1 multiplied by -1 times the population-weighted average level of racial segregation in the sample (0.63). Column 3 calculates the percent increase in mobility in this counterfactual. Columns 4–5 consider a counterfactual scenario in which racial segregation is lowered to its lowest observed value in the sample (Lawton, OK dissimilarity index: 0.33). In Panel C, we calculate the difference in upward mobility rates between white and Black children, and the percent changes in columns 3 and 5 are calculated relative to the observed gaps in column 1.
Source: Authors' calculations using data from Ananat (2011) and Chetty et al. (2020*a*).

Appendix Table 7: Simple Calculations of Aggregate Changes in Children's Earnings When Eliminating Harmful Effects of Segregation

	Coefficient, upward mobility (percentiles) (1)	Dollar equivalent of 1 percentile (2)	Mean impact on individual income (3)	Number of children, millions (4)	Total impact on income, millions $ (5)
Black, 25th percentile	-0.292	850	157	1.55	242
Black, 75th percentile	-0.224	1,321	187	0.78	145
White, 25th percentile	-0.160	850	86	3.49	299
White, 75th percentile	-0.025	1,321	21	9.22	192
Total	–	–	–	15.03	879

Notes: Column 1 reports the 2SLS coefficient on upward mobility from Table 1. Column 2 reports the dollar value of a 1 percentile increase in individual income, calculated by regressing individual income levels on percentiles separately for percentiles 24–26 and 74–76. Column 3 reports the average impact of eliminating segregation on individual income, calculated as the product of columns 1 and 2 and the population-weighted average segregation level (0.63). Column 4 reports the number of children from households below or above the nationwide median in our sample cities. Column 5 equals the product of columns 3 and 4.
Source: Authors' calculations using data from Ananat (2011) and Chetty et al. (2020*a*).

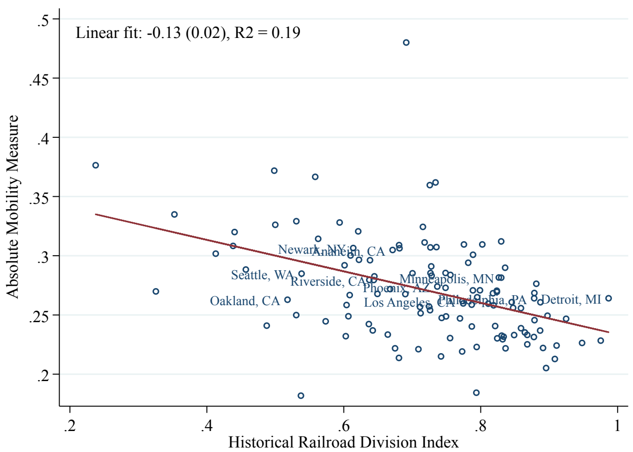
Appendix Figure 1: First Stage Relationship between 1990 Dissimilarity Index and Historical Railroad Division Index



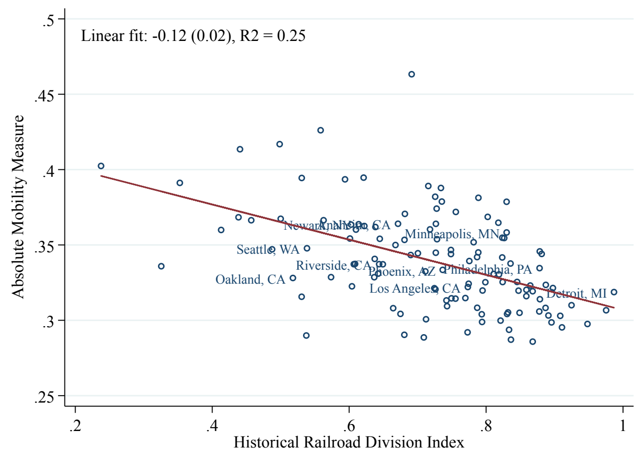
Notes: Figure displays the relationship between the racial dissimilarity index in 1990 and the railroad division index (RDI). Sample contains 121 non-Southern cities. Names are displayed for the 10 largest cities in terms of 1990 population.
Source: Authors' calculations using data from Ananat (2011).

Appendix Figure 2: Bivariate Relationship between Upward Mobility Measures of Black Children and Historical Railroad Division Index

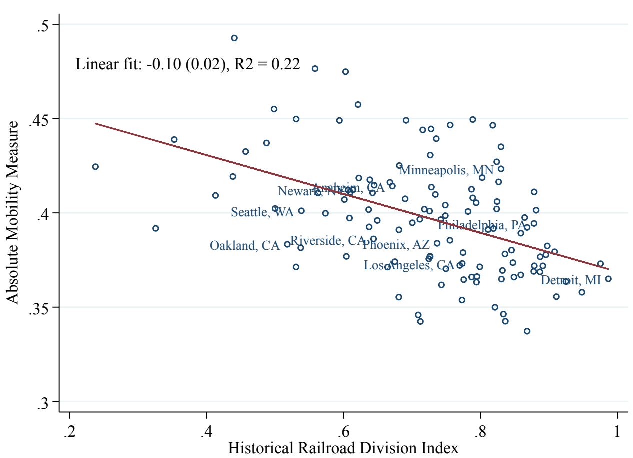
(a) 1st Percentile



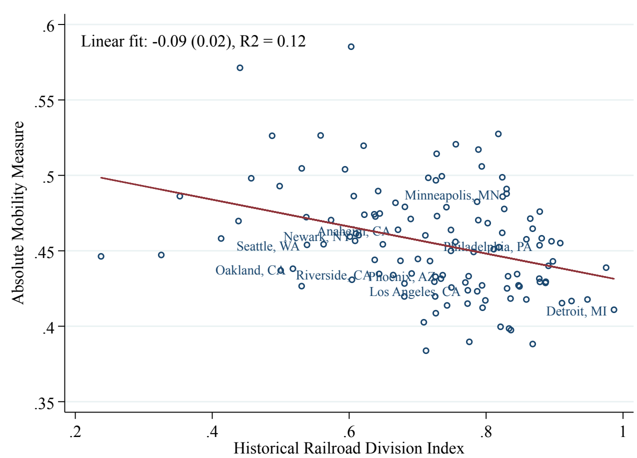
(b) 25th Percentile



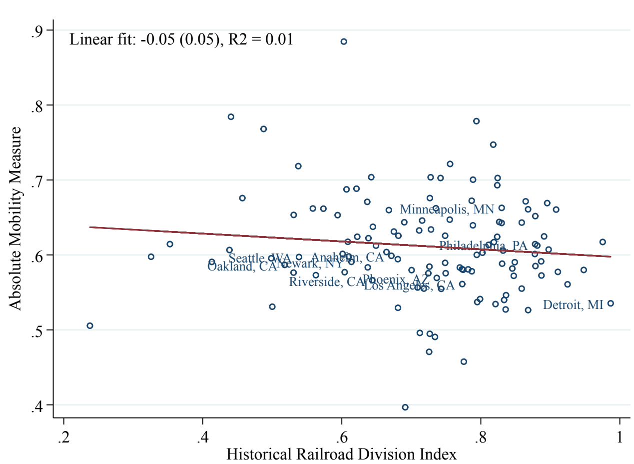
(c) 50th Percentile



(d) 75th Percentile



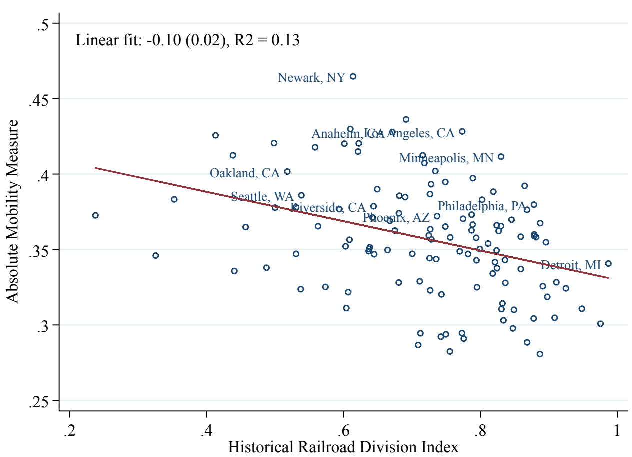
(e) 100th Percentile



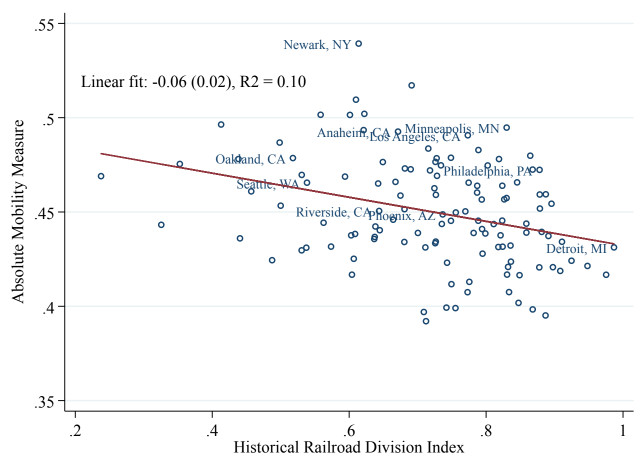
Notes: Figure displays the relationship between absolute mobility of Black children whose parents have income at the percentile indicated in the panel title and the railroad division index (RDI). Sample contains 121 non-Southern cities.
Source: Authors' calculations using data from Ananat (2011) and Chetty et al. (2020*a*).

Appendix Figure 3: Bivariate Relationship between Upward Mobility Measures of White Children and Historical Railroad Division Index

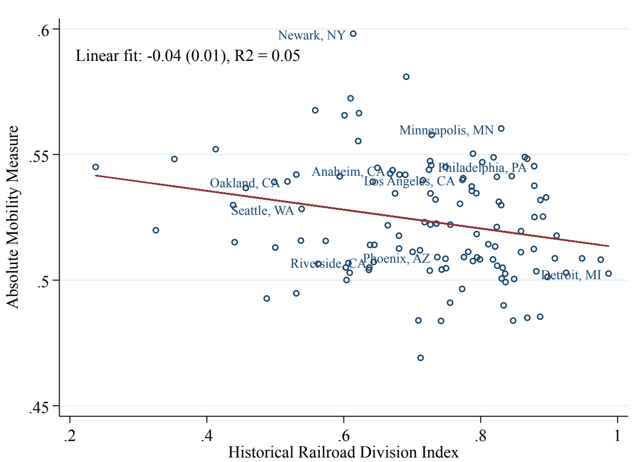
(a) 1st Percentile



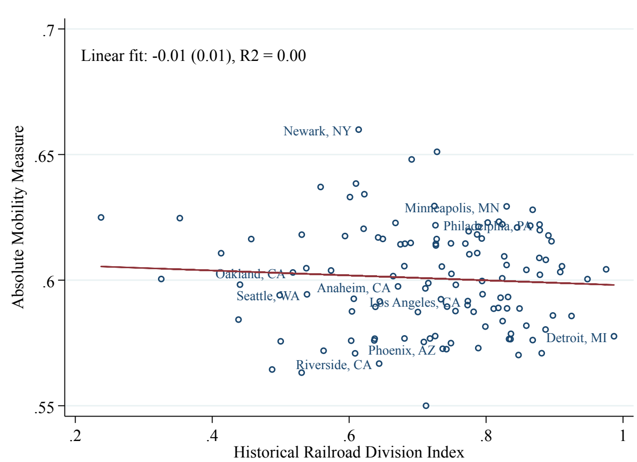
(b) 25th Percentile



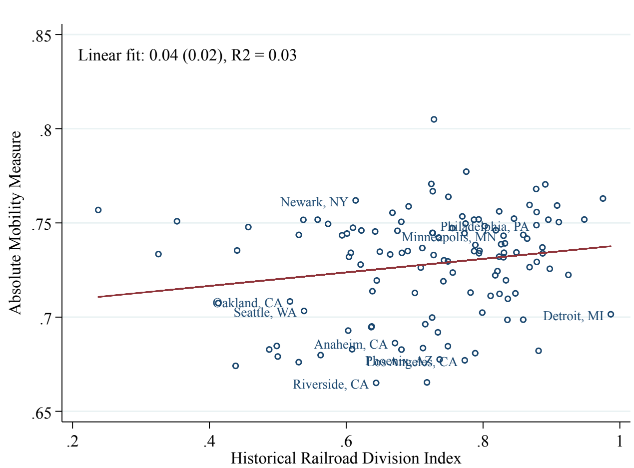
(c) 50th Percentile



(d) 75th Percentile



(e) 100th Percentile



Notes: Figure displays the relationship between absolute mobility of white children whose parents have income at the percentile indicated in the panel title and the railroad division index (RDI). Sample contains 121 non-Southern cities.
Source: Authors' calculations using data from Ananat (2011) and Chetty et al. (2020*a*).