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

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Abstract

This paper provides new evidence on the causal impacts of citywide 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. Our analysis finds that higher segregation reduces upward mobility for Black children from households across the income distribution and White children from low-income households. Moreover, segregation lowers academic achievement while increasing incarceration and teenage birth rates. An analysis of mechanisms shows that segregation reduces government spending, weakens support for anti-poverty policies, and increases racially conservative attitudes among White residents.

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1 Introduction

A large literature has documented the important role of place in shaping the long-run outcomes of children (Chyn and Katz, 2021). 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 citywide 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 important prior work such as (Cutler and Glaeser, 1997; Ananat, 2011) also examines the effects of segregation, 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 from the Opportunity Atlas (Chetty et al., 2020*a*). 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 to a greater extent by railroads in the 19th century became more segregated in the decades following the Great Migration. The main outcomes of interest are measures of upward mobility by race and parental rank in the national income distribution that are based on IRS records for children in the 1978–1983 birth cohorts.[1]

Our IV estimates reveal that racial segregation reduces the intergenerational mobility of Black

[1]Mobility is measured based on observed later-life ranks in the nationwide income distribution. For both children and parents, the national distribution used to determine percentiles is not race-specific.

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 the average mobility for this group. Since Black children born to parents in the 1st percentile end up in the 27th percentile ($17,500 in annual household income) on average, a drop to the 22nd percentile ($12,666) amounts to $4,834 in lost income each year. 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.

For White children, we find evidence of heterogeneous impacts with segregation worsening outcomes for those from lower-income households and benefiting children from the top of the distribution. 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%). There are also detectable declines in mobility for White children whose parents have income at the 25th and 50th percentiles of the income distribution. At the 100th percentile, we find that segregation has a significant, small positive impact on a child's income rank.

Our analysis also shows that the effects of segregation extend beyond children's long-run income ranks. Using additional data from the Opportunity Atlas and the Stanford Education Data Archive (SEDA), we find that racial segregation leads to large increases in the probability that boys are ultimately incarcerated, raises the likelihood that girls give birth while they are a teenager, and lowers average test scores. The impacts are especially large-in-magnitude for incarceration and teenage childbearing. For example, a 1 SD increase in segregation leads to a 29% increase in incarceration for Black boys from the poorest families and a 22% increase for White boys.

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 the supply and demand for government programs and policies that plausibly affect upward mobility. We find 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. To understand these results on government expenditures, we also study survey-based measures of political attitudes and racial attitudes. Our analysis of the latter is motivated by prior research linking support for redistributive programs to racial resentment (Gilens, 1999; Fox, 2004; Wetts and Willer, 2018; Metzl, 2019). We provide evidence that segregation weakens support for welfare and anti-poverty programs while worsening White individuals' attitudes toward minorities and opposition to integration related policies such as affirmative action and race-based school busing. The survey-based findings are consistent with evidence from Ananat and Washington (2009), which studies alternative measures of racial and political attitudes.

To conclude, we conduct a back-of-the-envelope analysis that uses our estimates to understand the aggregate economic costs of racial segregation for the cohorts in our sample of metro areas. This exercise aims to shed light on the long-run effects of historical segregation and should not be interpreted as estimating the benefits from integration policies implemented today. This is because the institutional and attitudinal shifts produced by decades of segregation may be preserved even with spatial re-sorting. Our simple approach combines our main estimates with information on the segregation experienced by children in our sample. The results suggest that segregation lowers upward mobility by 46% for the poorest Black children and by 30% for the poorest White children. Decreases in mobility for children from richer families are smaller in magnitude but sizable. Moreover, because segregation especially reduces the upward mobility of Black children, our estimates imply that segregation accounts for the majority of the Black-White mobility gap. These decreases in upward mobility translate into decreases in children's long-run income of nearly $80 billion per year.

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 White individuals 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 which finds that segregation worsens average schooling attainment, SAT scores, and poverty rates for Black individuals (Cutler and Glaeser, 1997; Card and Rothstein, 2007; Ananat, 2011; De La Roca, Ellen and Steil, 2018). Prior work finds mixed evidence that segregation affects the economic outcomes of White children (Cutler and Glaeser, 1997; Ananat, 2011; De La Roca, Ellen and Steil, 2018) or racial attitudes expressed by White individuals (Cutler, Glaeser and Vigdor, 1999). 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. Our use of an instrumental variable strategy and more extensive data reveals that the effects of segregation are broader than previously identified. Our work also extends on 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*). In addition to our use of an instrumental variable strategy, we innovate from important prior work by showing that segregation affects public good provision and attitudes in ways that could drive the widespread declines in upward mobility that we document.

In addition, this paper relates to research on the Great Migration of Black individuals out of the South (Boustan, 2010; Collins and Wanamaker, 2015; Boustan, 2016; Shertzer and Walsh, 2019; Calderon, Fouka and Tabellini, 2020; Stuart and Taylor, 2021; Derenoncourt, 2022; Baran, Chyn and Stuart, 2022). Our work is most closely related to important recent work by Derenoncourt

(2022), which provides evidence that Great Migration population flows reduced upward mobility. Relative to her work, we offer two contributions. First, her analysis studies the effects of greater levels of Black migration and finds small and statistically insignificant effects for White children. In contrast, we find heterogeneous impacts of racial segregation for White children with particularly detrimental impacts for those from lower-income households. Second, we perform supplementary analysis that demonstrates that racial segregation has distinct impacts on upward mobility outside of other demographic changes associated with the Great Migration. In a specification that uses instruments based on historical railroad configurations and the shift-share approach used in prior work (e.g., Boustan, 2010; Fouka, Mazumder and Tabellini, 2020; Derenoncourt, 2022), we find that higher levels of racial segregation and increases in the Black population share due to the Great Migration each have distinct negative impacts on upward mobility of children.

Finally, our findings relate to the literature studying how historical events and institutions in the U.S. shape long-run and contemporaneous outcomes (Nunn, 2009, 2014). Within this broad literature, we provide new evidence that the racial segregation facilitated specifically by the 19th placement of railroad lines had far reaching impacts on the economic mobility of Black and White children and racial attitudes. These findings add to a rich existing literature on the historical determinants of economic progress and welfare for minorities in the U.S. (Alsan and Wanamaker, 2017; Albright et al., 2021; Derenoncourt, 2022; Williams, 2022; Feir, Gillezeau and Jones, 2023).

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; Logan and Parman, 2017; 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 non-White 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. More broadly, the correlation between racial segregation (measured using the dissimilarity index) in the years 1970 and 1990 across all metro areas 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*), we assume that 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.[2] Equation (1) is a linear projection, so ϵ_i is an orthogonal residual. We allow equation (1) to differ by children's 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 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.

The framework above 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

[2]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.

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 study this phenomenon as part of our analysis.

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 the 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. For example, 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.

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 White residents (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 an RDI of 0, while a city that is divided into a nearly infinite number of railroad neighborhoods would have an 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.

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 through 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. The appendix contains two results that support the assumption that RDI affects upward mobility through racial segregation. First, estimates are very similar if we include railroad track length in the specification as in the main specification of Ananat (2011). Second, estimates also are very similar if we control for city characteristics as of 1910 and 1920 that Ananat (2011) uses for a balance test exercise.[3] Our focus on intergenerational mobility does not require that segregation has no effect on the income of parents. However, segregation will only affect mobility if it changes children's outcomes conditional on their parents' income, as explained in Section 3.

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.[4]

Our empirical strategy identifies a reduced-form effect of segregation that we interpret as a summary measure of both contemporaneous and historical channels. Specifically, consider the following two possibilities. 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

[3] Appendix A shows that the balance test results conducted by Ananat (2011) are similar when not controlling for historical railroad track length.

[4] 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.

the labor market.[5] Second, the IV estimates of β_p could reflect a range of effects from 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 had effects on local institutions or local government policies that shape outcomes of adults and children. Because our main instrumental variable specification is exactly identified, estimating this reduced form impact of RDI is straightforward.[6]

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. We link this sample to several additional data sources as summarized below. Appendix Table 1 provides a more detailed overview of each variable utilized in our main analysis and the underlying data sources.

For each metropolitan 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.[7] 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 the respective cohorts were aged 31–37). In addition to absolute mobility measures, we study incarceration and teenage pregnancy rates from the Opportunity Atlas. 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. We also study schooling outcomes using average

[5] Our analysis of mechanisms in Section 6 aims to provide suggestive evidence on the relationship between segregation and contemporary government policies.

[6] In principle, one might want to estimate the effects of segregation measured in the early 20th century (rather than in 1990). Unfortunately, it is not possible to construct comparable dissimilarity indices in the early 20th century for most metros in our sample because tract-level data are not available. Conversely, the approach to measuring segregation using complete count Census data from Logan and Parman (2017) cannot be implemented using modern publicly available data.

[7] 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.

test scores for White and Black students from SEDA (Reardon et al., 2021). These data cover mandatory state standardized assessments in math and reading language arts for students in grades 3 through 8 during the 2008–2009 through 2017–2018 school years.

We link the sample to additional data sources to explore the robustness of our results and study mechanisms. We use decennial Census data from 1910 to 1990 to measure the Black population share and number of Black residents in a metro area. To decompose how places influence children's long-run outcomes, we use exposure effect estimates from Chetty and Hendren (2018*b*). As detailed in Section 5.4, exposure effect estimates 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.

We also study mechanisms using several datasets that allow us to examine the supply and demand for redistributive programs and other government policies. We measure government expenditures using the average amounts of spending reported in the 1987 and 1992 Census of Governments. To measure political and social attitudes, we rely on survey responses from various waves of the Cooperative Congressional Election Study (CCES) and the American National Elections Studies (ANES). From the CCES, we use measures of support for decreasing spending on welfare programs, health care, and education; opposition to increases in the minimum wage; two questions designed to proxy for "racial resentment"; opposition to affirmative action; and support for aggressive policing policies (as a complementary measure of racial attitudes and resentment). From the ANES, we use two questions on opposition to school racial integration and busing from historical surveys (waves between 1970 and 1994).[8] We follow Kling, Liebman and Katz (2007) in creating general summary measures of these variables. In particular, we create a redistributive policy in-

[8] Appendix C provides details on these surveys and the specific questions used.

dex based on the four CCES questions on attitudes on spending and the minimum wage, a racial resentment index using the five relevant questions from the CCES and ANES, and an aggressive policing index using five questions from the CCES. Each index equals an equally-weighted average of z-scores of the underlying questions. This approach increases statistical power by pooling related measures.

All underlying data provide information specific to U.S. counties, which we aggregate to 1990-vintage metro area definitions used by Ananat (2011). We construct averages using weights based on the 1990 county population for the Opportunity Atlas and political measures, and the number of students for the school outcomes. We do not weight sums (e.g., government expenditures).

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—similar to the difference between Minneapolis (0.61) and Philadelphia (0.75) or between Philadelphia and Detroit (0.87)—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 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 has heterogeneous effects for White children. For White children from lower-income families, segregation reduces mobility, with the IV estimates showing that a 1 SD increase in racial segregation leads to a 3.4 percentile (10%) 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. In contrast, White children from the richest families (i.e., the 100th percentile) appear to benefit from segregation, although these estimates are relatively small-in-magnitude (a 1.2 percentile increase).[10]

The appendix contains additional results that support the robustness of the findings in Table 1. First, columns 2 and 3 of Appendix Table 3 show that results are similar when controlling for the historical railroad track density as in Ananat (2011) or the 1910–1920 city characteristics that Ananat (2011) uses for a balance test exercise. Second, column 4 shows that results are similar when including fixed effects for the Census Northeast and Midwest regions (the West region is the omitted category). Third, column 5 shows that the results are similar when controlling for the unemployment rate and manufacturing employment share in 1970 and 1990, which suggests that our findings are not driven by differential exposure to deindustrialization or rust belt decline.[11] Fourth, column 6 shows that the results are nearly identical when controlling for income segregation using

[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. For example, the presence of more attractive suburban neighborhoods or jurisdictions within a metro area could independently increase both segregation and upward mobility. 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 6 shows that IV estimates on several potential mechanisms are larger in magnitude than OLS estimates.

[10]These results add to the mixed evidence of how segregation affects White children. Cutler and Glaeser (1997) examine whether segregation affects education and earnings of young adults observed in the 1990 Census and the National Longitudinal Study of Youth. Their estimates for White children vary, showing positive impacts in some specifications and negative impacts in others. Looking at young adults in the 1990 Census, Ananat (2011) finds that segregation increases the probability that White children have exactly a high school degree, with this effect arising from a decrease in the share of White children who are high school drop-outs and a decrease in the share of White children who have some college education or a college degree.

[11]Controlling for the levels of these variables in 1970 and 1990 also implicitly controls for the change in these variables over this time period. The results are also robust to controlling for the additional 1990 city characteristics used in robustness exercises in Ananat (2011).

the dissimilarity index approach of Cutler and Glaeser (1997).[12] These results reduce concerns about omitted variable bias. Fifth, Appendix Table 4 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). Sixth, 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. Seventh, 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 5, 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.

Finally, Appendix Table 6 examines the robustness of our results to using alternative measures of racial segregation. We focus on the dissimilarity index to maintain comparability to Ananat (2011); however, as with her results, ours are also not sensitive to using these other broad measures (isolation, clustering, concentration, and centralization). Most importantly, we find that the effects of racial segregation are, if anything, somewhat larger when we use a dissimilarity index from the 1940 Census.[14] Intuitively, these larger effects could arise because racial segregation is self-reinforcing or because the earlier segregation measure better captures changes to local areas that emerged in the middle of the 20th century. Note that our preferred approach uses the 1990 dissimilarity index because it is most closely tied to the cohorts for whom mobility data are available.

[12] Moreover, Appendix Table 2 shows that the railroad division index does not predict income segregation.

[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 calculate the dissimilarity index for 1940 using enumeration districts, which are geographic areas that were feasible to be covered by a single census surveyor. Using census tracts to calculate the dissimilarity index would limit the sample size to 23 metro areas because tracts were not used in most cities during this period (Cutler, Glaeser and Vigdor, 1999).

5.2 Effects of Segregation Versus Black Population Share

Do the estimates in the previous section 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, in addition to discussing distinct mechanisms such as decreases in public expenditures. Because racial segregation is positively correlated with the Black population share and the number of Black residents, it is possible that our results reflect the impacts of these demographic variables instead of segregation.[15]

Our findings on the impact of segregation for White children is one initial distinction that suggests that our segregation results are not driven by the response to Black migration isolated in Derenoncourt (2022). Her analysis shows that increases in the Black population had statistically insignificant and small-in-magnitude impacts on the mobility of White children from low- and high-income households.[16] This contrasts with the large and significant impacts of segregation that we detect for White children with parents at or below the 50th percentile of the income distribution.

To further explore Black population changes and the interpretation of our results, we conduct a supplementary analysis that separately identifies the effects of citywide segregation and Black population shares using two sources of plausibly exogenous variation. Specifically, we are interested in the following regression model for income mobility:

$$\overline{y}_{c,p} = \alpha_p + \text{Seg}_c\beta_p + \text{BlackSharePctile}_c\gamma_p + \epsilon_{c,p}, \tag{6}$$

where $\text{BlackSharePctile}_c$ is the percentile of the 1990 Black population share. To address the endogeneity of the Black population share, we build on the approaches from previous work (e.g., Boustan, 2010; Fouka, Mazumder and Tabellini, 2020; Derenoncourt, 2022) and rely on a shift-share instrument that is based on pre-existing settlements of African Americans who lived outside

[15] For example, cities in our sample with higher segregation in 1990 also have much higher Black population shares (correlation: 0.54).

[16] Derenoncourt (2022) estimates that a 1-SD increase in the Great Migration shock led to an insignificant and small reduction of mobility for low-income White children equal to 0.75 percentiles.

of the South prior to the Great Migration. As in our main analysis, we also rely on historical railroad track configuration as an instrument.

Intuitively, the shift-share instrument approach that we introduce in this analysis combines two sources of variation. First, it leverages variation over time in total Black emigration from the South for each decade from 1910 to 1990—a period that precedes the beginning of the Great Migration (circa 1915) and extends to the period when we measure racial segregation and the Black population share (1990). Second, it predicts inflows to each Northern metro area in our sample based on the share of Southern-born Black individuals living there in 1910.

Formally, our instrument for $\text{BlackSharePctile}_c$ is based on the predicted number of Black migrants to a metro area from 1910 to 1990 defined as follows:

$$\text{PredictedBlackMigrants}_c^{1910-1990} = \sum_s \sum_{t=1910}^{1980} w_{s,c}^{1910} M_s^{t,t+10}, \tag{7}$$

where $w_{s,c}^{1910}$ is the share of African American migrants born in Southern state s that lived in metropolitan area c in 1910, and $M_s^{t,t+10}$ is the net number of Black migrants that moved away from state s between years t and $t+10$. We construct $M_s^{t,t+10}$ using the forward survival method (e.g., Gregory, 2005; Boustan, 2010; Fouka, Mazumder and Tabellini, 2020), as detailed in Appendix D. We divide the predicted number of Black migrants to a metro area by the total population of the metro area in 1910. Following Derenoncourt (2022), we use percentiles of this ratio as our instrumental variable to ensure that our results are not driven by outliers.[17]

Table 2 reports results from our analysis of the distinct impacts of segregation and Black population shares on outcomes of children. For comparison, column 1 reports estimates from our main specification that only instruments for segregation on the sample for which we can construct the shift-share instrument.[18] Based on equation (6), column 2 reports IV estimates on the impacts on mobility for Black children.[19] For all Black children except those with parents at the 100th

[17]We also specify the Black population share as a percentile to reduce the potential role of outliers.

[18]These results differ only slightly from those in Table 1 because we do not construct a predicted migration instrumental variable for two metro areas in Oklahoma, which is treated as part of the South.

[19]The Kleibergen and Paap (2006) F-statistic for the model with two endogenous variables and two instruments is

percentile, we find consistently negative and statistically significant impacts of segregation. Notably, column 2 also shows that we replicate the qualitative finding from Derenoncourt (2022): we find that a 1 percentile point increase in the Black population share results in a significant 0.056 percentile point reduction in Black mobility for children with parents at the 25th percentile of the income distribution. The estimates in column 4 for White children indicate that segregation lowers mobility, but the Black population share does not have an independent effect for this group.

Overall, the weight of the evidence in this analysis suggests that our estimated impacts of segregation do not simply reflect differences in the relative size of the Black population. Comparing Tables 1 and 2, the main qualitative conclusions on the effects of segregation remain the same. The main distinction across tables is that the point estimates are slightly attenuated relative to a model that omits the Black population share.

5.3 Impacts of Segregation on Incarceration, Teenage Births, and Test Scores

Next, we extend our analysis by studying incarceration (for men), teenage pregnancy (for women) and schooling achievement.[20] Panels A and B of Table 3 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 6.8 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.[21] 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%).

8.5.

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

[21]Appendix Table 7 shows that these results are robust to controlling for different sets of observed variables.

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.

Finally, Panel E examines childhood academic achievement as measured by average scores on statewide 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 0.07 SD decline for White students. This finding suggests that the segregation-induced decline in upward mobility does not arise simply 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.

5.4 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.[22] 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:

$$\overline{y}_{c,p} = \lambda_{c,p} + \theta_{c,p}, \tag{8}$$

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 of $\lambda_{c,p}$ 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

[22] 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.

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 4 reports estimates of the effects of segregation on upward mobility and exposure effects. For comparison, columns 1 and 2 reproduce the 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 from Chetty and Hendren (2018*b*) by assuming a 20-year duration of childhood exposure.[23]

The easiest-to-interpret estimates in Table 4 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 the pooled *mobility* estimates are reasonably informative about both groups at lower incomes. However, if the exposure effects differ by race, then the pooled exposure effects would predominately reflect the patterns for White children. 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% (=0.113/0.288) of the effects of segregation on mobility for children at the 1st percentile are due to the impacts on exposure effects. Similarly, changes in exposure effects account for 31% (=0.062/0.197) 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 pooled upward mobility.

Interestingly, the finding of a substantial role for non-exposure effects differs from Chetty and

[23] See Appendix B for full details on the measure of exposure effects used in our analysis.

Hendren (2018*b*), which examines the correlation between upward mobility, exposure effects, and racial segregation across all commuting zones in the U.S. (including rural areas and 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 other effects diverge.

6 Segregation, Government Spending, and Political Economy

6.1 Government Spending

To further explore how segregation lowers upward mobility, Table 5 studies segregation and a range of categories of government spending. Our analysis is motivated by a large literature showing that various public programs have important impacts on long-run child outcomes (e.g., East et al., 2017; Bailey et al., 2020). We find that a 1 SD increase in racial segregation decreases total expenditures per capita by 39%. The declines are broad-based, with large and significant reductions in education, public safety, welfare and health, and infrastructure. Education is the largest expenditure category in general, and it accounts for the largest share of the decline in expenditures, at 38%. Decreases in public safety expenditures and welfare and health expenditures account for a further 32% of the reduction in total spending. The decrease in public safety expenditures is consistent with Cox et al. (2022), who find that racial segregation also reduces police expenditures per capita.

6.2 Political Economy

The results so far suggest that reduced public spending may play a role in explaining why segregation worsens mobility for both Black and lower-income White children. In this section, we explore why racial segregation weakens government spending. One explanation suggested by previous research is that residential segregation may affect racial attitudes and thereby shape preferences for redistribution. We build on prior work on this topic by providing causal estimates of racial segregation on both policy preferences and racial attitudes.

In Panel A of Table 6, we begin our analysis of the political economy of racial segregation by studying an index based on four CCES questions measuring opposition to state legislature spending and increases in the minimum wage. The estimates in the first row reveal that a 1 SD increase in segregation increases opposition to redistributive spending by 0.46 SD (i.e., the effect of 0.382 divided by the SD of the index of 0.835). The subsequent rows show that a 1 SD increase in segregation leads to a 0.20–0.75 SD decrease in support for specific redistributive policies. The value of analyzing an index of outcomes is underscored by the lack of statistical precision when examining specific outcomes (except for the minimum wage).

Why might segregation reduce support for redistributive policies? A broad literature across the social sciences has suggested a role for racial resentment in eroding support for and implementation of inequality-reducing policies (Gilens, 1995, 1996; Tesler, 2012; Metzl, 2019; Cramer, 2020; McGhee, 2021).[24] To examine the link between segregation and racial attitudes, Panel B of Table 6 presents results for an attitudes index that is based on questions taken from the CCES and the ANES that measure racial resentment and opposition to government policies that support minorities. While opinions on affirmative action and school integration and busing do not directly measure racial attitudes, anti-Black attitudes have been associated with opposition to these policies (Sears, Hensler and Speer, 1979; Kluegel and Smith, 1982; Bobo, 1983). All measures are scaled so that higher values reflect more out-group hostility.

Our main finding in these results is that a 1 SD increase in racial segregation causes a 0.69 SD increase in the racial attitudes index. The disaggregated results show relatively similar, significant effects on each of the underlying measures of the index, ranging from 0.58 to 0.85 SDs. Notably, these 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

[24] Note that the effects of racial segregation on preferences for redistribution are conceptually distinct from the effects of the Black population share (Abascal, Ganter and Baldassarri, 2021). This latter effect is more analogous to the literature on immigration and ethnic diversity (Alesina, Baqir and Easterly, 1999; Alesina, Miano and Stantcheva, 2023; Luttmer, 2001; Halla, Wagner and Zweimüller, 2017; Dustmann, Vasiljeva and Piil Damm, 2019; Bazzi et al., 2019; Giuliano and Tabellini, 2020; Steinmayr, 2021).

and less support for government aid to Black individuals.[25,26]

As a complementary measure of racial attitudes and resentment, we also study support for aggressive policing in Panel C. Our analysis is motivated by a long history of police forces being used to enforce and exacerbate racial disparities in the U.S. (e.g., Alexander, 2010). We explore policing attitudes by constructing an index from CCES measures of whether individuals oppose bans on chokeholds by police, the creation of "bad cop" registries, the use of police-worn body cameras, laws that allow individuals to sue police, and mandatory minimum sentencing laws.[27] We find that racial segregation increases White individuals' support for aggressive policing, with a 1 SD increase in segregation leading to a 0.46 SD increase in the index. These results underscore the breadth and depth of segregation's impacts on policy and racial views.

Finally, motivated by the heterogeneous effects of segregation on upward mobility of White individuals, Table 7 provides additional results that expand our analysis to consider whether policy attitudes are shifted differently across income groups and by race.[28] Panels B and C of Table 7 estimate effects of segregation separately for White survey respondents who are in the bottom and top halves of the income distribution (based on a family income cutoff of $60,000). While lower- and higher-income White respondents move in the same direction, we see larger responses for lower-income White individuals across all three families of attitudes and that these are only significant for lower-income White individuals. A 1 SD increase in segregation has a larger impact for lower-income White individuals on attitudes toward redistributive policy (0.50 SD vs. 0.21

[25]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 uses the same ANES data from which we take questions, we use distinct questions regarding attitudes toward school racial integration and school busing policies. Our analysis also differs because we use 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.

[26]These results differ from existing associations in the literature. For example, using OLS regressions estimated on data from the General Social Survey, Cutler, Glaeser and Vigdor (1999) find a negative relationship between segregation and White individuals' support of a ban on interracial marriage and no significant relationship between segregation and White individuals' willingness to vote for a Black president or beliefs about the inherent intelligence of Black individuals.

[27]While there is limited work on attitudes toward specific policing policies, prior work has shown variation over time and large racial disparities in broader attitudes such as confidence in police (see Owens and Ba 2021 for discussion).

[28]Table 7 displays results for summary indices by sub-group. Appendix Tables 8, 9, and 10 show results for the full sets of index components.

SD), race (0.73 SD vs. 0.38 SD), and aggressive policing (0.50 SD vs. 0.10 SD). In Panel D, we show that Black respondents move in the opposite direction (increased support for redistribution) suggesting that the pattern observed for low-income White individuals is not simply driven by decreases in upward mobility. This analysis reveals that segregation leads to particularly large reductions in lower-income White individuals' desire for redistributive spending, even though this group is harmed more by segregation (relative to higher-income White individuals) and more likely to benefit from increased government spending and minimum wage increases.[29]

Overall, these results are consistent with theoretical predictions based on the contact hypothesis (Allport, 1954). Fewer intergroup interactions in segregated cities may incubate and foment racial resentment among White residents.[30] Relatedly, by concentrating a racial out-group, segregation may create a salient racial threat (Key, 1949; Kinder and Mendelberg, 1995; Oliver and Mendelberg, 2000; Rocha and Espino, 2009) that may be further exploited by politicians and others as a scapegoat for worsened outcomes for lower-income White individuals (Grosjean, Masera and Yousaf, 2020; Bauer et al., 2021).[31]

7 Aggregate Long-Run Impacts of Historical Racial Segregation

In this section, we conduct a back-of-the-envelope exercise that uses our estimates to explore the aggregate impacts of racial segregation for the cohorts in our sample. This analysis complements an emerging literature that has sought to quantify the aggregate impacts of racial stratification (e.g., Fogel and Engerman, 1974; Hsieh et al., 2019; Heblich, Redding and Voth, 2022; Logan, 2022).

[29] Our results complement the broader literature on why individuals may vote against their material interests (Bartels, 2008). Explanations of this in the U.S. context have cited racism (Lee and Roemer, 2006), moral values and social identity (Bonomi, Gennaioli and Tabellini, 2021; Enke, Polborn and Wu, 2022), misinformation (DellaVigna and Kaplan, 2007; Cruces, Perez-Truglia and Tetaz, 2013; Martin and Yurukoglu, 2017), and distrust in government (Kuziemko et al., 2015), among others.

[30] Another related mechanism as discussed by Enos and Celaya (2018) is that "segregation facilitates categorization, making social categories, such as ethnicity, more cognitively salient and, thus, leading to stereotyping and discrimination (Enos, 2017)."

[31] The pattern of results by income could also be interpreted as being consistent with predictions based on "last place aversion." Kuziemko et al. (2014) provide theory and empirical evidence showing that low-income individuals express less support for redistributive policies that aid others who are even lower in the income distribution. While Kuziemko et al. (2014) are careful to argue that last place aversion does not simply reflect the racialization of policies (e.g., Gilens, 1996), it is possible that such last place aversion is heightened by these factors (Darity, 2022).

To be clear, we do *not* interpret this exercise as predicting the effects of a desegregation policy in the present since the long-run effects of segregation have contributed to shifts in both structural and interpersonal racism that may have calcified and remain persistent even if a policy of racial re-sorting were instituted today. Rather, we instead attempt to estimate the counterfactual if cities had *never been* segregated.

We begin by gauging the extent to which racial segregation depresses economic mobility. Our approach is based on combining our main IV estimates with information on the average level of segregation. In particular, we estimate the loss in mobility caused by segregation as $\hat{\beta}_{r,p}\overline{\text{Seg}_{r,c}}$, where $\hat{\beta}_{r,p}$ is the IV estimate reported in Table 1 and $\overline{\text{Seg}_{r,c}}$ is the average level of segregation experienced by children of race r in our sample.

The results of this exercise, shown in Table 8, suggest that segregation has severe aggregate consequences for economic mobility. For example, a Black child whose parents have income at the 1st percentile of the nationwide income distribution reaches the 27th percentile of the income distribution on average. Our estimates imply that, if not for the harmful effects of segregation, average mobility for this group would be the 50th percentile. These numbers imply that segregation lowers the poorest Black children's mobility by 46% relative to the no-segregation counterfactual. We can also express this change in mobility in terms of dollars. Because the 27th percentile of the income distribution corresponds to \$9,869 (in 2015 dollars) and the 50th percentile corresponds to \$29,305, these estimates imply that segregation lowers the annual income of Black individuals from the poorest families by \$19,436. Segregation also leads to substantial decreases in economic mobility for White children whose parents are in the bottom of the income distribution. Perhaps most strikingly, the estimates imply that segregation accounts for the vast majority of differences in economic mobility between Black and White children. These results are consistent with discussion in stratification economics that emphasizes how racism seeks to uphold and further gaps between the dominant group and subaltern groups, even if that means lowering overall welfare (Darity, 2022).

We further explore the aggregate consequences of segregation by converting these changes in mobility into changes in aggregate income. Let $\Delta_{r,p}$ be the average effect of segregation on long-run income for children of race r whose parents have income percentile p. To calculate the total impact of segregation on income for children of a given race, we would ideally add these impacts over the race-specific parental income distribution:

$$\Delta_r^* = \sum_p N_{r,p}\Delta_{r,p}, \tag{9}$$

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 can 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.[32]

To overcome data limitations, we make several simplifying assumptions. 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:

$$\Delta_r = N_{r,<50}\Delta_{r,25} + (N_r - N_{r,<50})\Delta_{r,75}. \tag{10}$$

We estimate $\Delta_{r,p}$ as the change in income associated with the impact of segregation on upward mobility, as expressed in columns 3 and 4 of Table 8.

We estimate that the segregation-induced declines in upward mobility translate into a decrease in children's long-run income of \$80 billion per year across the income groups we consider. As seen in Table 9, the decrease in income is particularly large for children from lower-income fam-

[32]The Opportunity Atlas constructs this estimate by combining publicly available data from the 2000 Census on the number of individuals 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.

ilies. Although segregation has disproportionately negative impacts on Black children on a per capita basis, the larger number of White children means that the largest aggregate decline in income is for lower-income White children. While a greater number of lower-income White children are harmed by segregation, it is worth highlighting that there is greater harm per capita among Black children. One interpretation is that the shifts in outcomes (e.g., public spending reduction) most directly harm Black families; however, perhaps as a byproduct, some of these policies also end up harming low-income White families.

Given the simple nature of these calculations, several caveats should be kept in mind. These calculations rely 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 4), our use of the coefficients from Table 1 could overstate the decrease in children's long-run earnings from segregation.[33] Other considerations suggest that these simple calculations could understate the costs of segregation. First, these calculations apply only to children who were younger than age 18 and living in our sample cities as of the year 2000. Second, these calculations do not account for the harmful impacts of segregation on parents' labor market outcomes or costs associated with the dynamics of wealth accumulation (Darity and Frank, 2003; Aliprantis, Carroll and Young, 2019; Ashman and Neumuller, 2020). Third, our focus on income does not capture how segregation affects other important determinants of utility such as psychological well-being (Clark, Chein and Cook, [1952] 2004) or other non-economic costs in terms of safety (Cook, Logan and Parman, 2018). Given the challenge of quantifying these different channels, we view the simple calculations as only suggestive of the aggregate reductions in children's long-run opportunities due to segregation.

[33]We prefer to use the results from Table 1 because the exposure effect estimates are not available separately by race and do not capture causal effects of places that do not scale with years of exposure.

8 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. For White children, we find that segregation worsens mobility for those from lower-income households, while there are positive impacts for those from the wealthiest families. The pattern of results for the effects of segregation on incarceration for boys and teenage girls is similar to our findings for mobility for both Black and White children.

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 supply and demand for social programs and characteristics of places that plausibly shape upward mobility. Specifically, we find that segregation leads to reductions in government expenditures, increases opposition to redistributive policies, and worsens racial attitudes.

Overall, our analysis implies that the causal impacts of historical racial segregation are important for understanding spatial disparities in economic mobility across U.S. cities. Moreover, our findings are consistent with the hypothesis that public good provision and political economy considerations are important mechanisms that have negative impacts on upward mobility for Black children across the income distribution and White children from lower-income households. The results also provide suggestive evidence that segregation undermines support for redistributive programs by affecting racial attitudes. Although Black-White racial segregation in the U.S. 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.

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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	-0.118***	-0.329***	-0.045	0.270
	(0.025)	(0.092)		
25th percentile	-0.111***	-0.289***	-0.039	0.339
	(0.019)	(0.072)		
50th percentile	-0.105***	-0.255***	-0.035	0.397
	(0.020)	(0.064)		
75th percentile	-0.099***	-0.222***	-0.030	0.455
	(0.026)	(0.067)		
100th percentile	-0.082	-0.131	-0.018	0.611
	(0.050)	(0.114)		
Panel B. White Mobility				
1st percentile	-0.063**	-0.248***	-0.034	0.357
	(0.025)	(0.065)		
25th percentile	-0.023	-0.164***	-0.022	0.450
	(0.020)	(0.049)		
50th percentile	0.009	-0.098**	-0.013	0.524
	(0.017)	(0.039)		
75th percentile	0.043***	-0.029	-0.004	0.601
	(0.015)	(0.033)		
100th percentile	0.098***	0.086**	0.012	0.728
	(0.018)	(0.041)		

Notes: This table reports point estimates and heteroskedasticity robust standard errors (in parentheses) from regressions in which the key independent variable is the racial dissimilarity index in 1990. Each combination of cells reports results from regressions 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 1 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. Statistical significance is denoted by: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.
Source: Authors' calculations using data from Ananat (2011) and Chetty et al. (2020*a*).

Table 2: Effects of Racial Segregation and Black Population Share on Upward Mobility, by Race and Parental Income Rank in Nationwide Distribution

	Black upward mobility (1)	Black upward mobility (2)	White upward mobility (3)	White upward mobility (4)
Panel A. 1st Percentile				
1990 Dissimilarity Index	-0.335***	-0.225**	-0.254***	-0.285***
	(0.095)	(0.109)	(0.067)	(0.102)
1990 Black Share Percentile		-0.062		0.018
		(0.044)		(0.042)
Panel B. 25th Percentile				
1990 Dissimilarity Index	-0.295***	-0.195**	-0.168***	-0.184**
	(0.075)	(0.079)	(0.050)	(0.078)
1990 Black Share Percentile		-0.056*		0.009
		(0.032)		(0.032)
Panel C. 50th Percentile				
1990 Dissimilarity Index	-0.260***	-0.170***	-0.101**	-0.103*
	(0.066)	(0.066)	(0.040)	(0.062)
1990 Black Share Percentile		-0.051*		0.001
		(0.029)		(0.025)
Panel D. 75th Percentile				
1990 Dissimilarity Index	-0.226***	-0.145**	-0.030	-0.019
	(0.069)	(0.072)	(0.034)	(0.050)
1990 Black Share Percentile		-0.046		-0.006
		(0.034)		(0.020)
Panel E. 100th Percentile				
1990 Dissimilarity Index	-0.134	-0.077	0.087**	0.120**
	(0.117)	(0.143)	(0.042)	(0.053)
1990 Black Share Percentile		-0.033		-0.019
		(0.066)		(0.022)
Panel F. Summary Statistics				
SD, Dissimilarity Index	0.135	0.135	0.135	0.135
SD, Black Share Percentile	0.290	0.290	0.290	0.290

Notes: This table reports point estimates and heteroskedasticity robust standard errors (in parentheses) from regressions in which the key independent variables are the racial dissimilarity index in 1990 and the percentile of the Black population share distribution in 1990. In columns 1 and 3, the instrumental variable is the railroad division index (RDI). In columns 2 and 4, the instrumental variables are the RDI and the percentile of the predicted change in Black population from 1910 to 1990 as a share of total population in 1910. See notes to Table 1 for additional details on specification, sample, and sources. Sample contains 119 non-Southern metro areas for which the RDI variable and predicted migration variable are available. Statistical significance is denoted by: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 3: Effects of Racial Segregation on Incarceration, Teenage Births, and Grade 3–8 Test Scores

	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	0.180***	0.503***	0.068	0.233
	(0.067)	(0.165)		
25th percentile	0.108***	0.248***	0.034	0.131
	(0.030)	(0.074)		
50th percentile	0.076***	0.134***	0.018	0.085
	(0.020)	(0.051)		
75th percentile	0.053***	0.055	0.008	0.053
	(0.020)	(0.055)		
100th percentile	0.034	-0.015	-0.002	0.025
	(0.027)	(0.072)		
Panel B. White Male Incarceration				
1st percentile	0.010	0.102**	0.014	0.063
	(0.013)	(0.043)		
25th percentile	0.002	0.043**	0.006	0.029
	(0.006)	(0.018)		
50th percentile	-0.001	0.018**	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**	-0.006	-0.001	0.001
	(0.002)	(0.004)		
Panel C. Black Female Teenage Birth				
1st percentile	0.450***	0.793***	0.107	0.488
	(0.073)	(0.194)		
25th percentile	0.395***	0.703***	0.095	0.396
	(0.057)	(0.142)		
50th percentile	0.332***	0.601***	0.081	0.292
	(0.045)	(0.103)		
75th percentile	0.283***	0.521***	0.071	0.210
	(0.045)	(0.102)		
100th percentile	0.193***	0.375**	0.051	0.061
	(0.063)	(0.165)		
Panel D. White Female Teenage Birth				
1st percentile	0.083	0.474***	0.064	0.278
	(0.052)	(0.152)		
25th percentile	0.050	0.340***	0.046	0.206
	(0.039)	(0.111)		
50th percentile	0.021	0.218***	0.029	0.140
	(0.027)	(0.074)		
75th percentile	-0.006	0.107**	0.014	0.080
	(0.017)	(0.042)		
100th percentile	-0.036***	-0.017	-0.002	0.014
	(0.011)	(0.018)		
Panel E. Test Scores in Grades 3–8				
Black test scores	-0.531***	-0.998***	-0.135	-0.496
	(0.135)	(0.323)		
White test scores	-0.039	-0.513	-0.069	0.250
	(0.136)	(0.318)		

Notes: This table reports point estimates and heteroskedasticity robust standard errors (in parentheses) from regressions in which the key independent variable is the racial dissimilarity index in 1990. The outcome variables are incarceration rates for men (Panels A and B), teenage birth rates for women (Panels C and D), and state standardized test scores for students in grades 3 to 8 (Panel E). See notes to Table 1 for additional details on specification, sample, and sources. Statistical significance is denoted by: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 4: 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	-0.329***	-0.248***	-0.288***	-0.113***	-0.175***
	(0.092)	(0.065)	(0.071)	(0.034)	(0.051)
Effect of 1 SD increase	-0.045	-0.034	-0.039	-0.015	-0.024
Mean of Dep. Var.	0.270	0.357	0.322	-0.003	0.325
Panel B. 25th Percentile					
1990 Dissimilarity Index	-0.289***	-0.164***	-0.197***	-0.062***	-0.135***
	(0.072)	(0.049)	(0.054)	(0.024)	(0.041)
Effect of 1 SD increase	-0.039	-0.022	-0.027	-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	-0.255***	-0.098**	-0.113***	-0.008	-0.105***
	(0.064)	(0.039)	(0.042)	(0.020)	(0.035)
Effect of 1 SD increase	-0.035	-0.013	-0.015	-0.001	-0.014
Mean of Dep. Var.	0.397	0.524	0.503	-0.002	0.505
Panel D. 75th Percentile					
1990 Dissimilarity Index	-0.222***	-0.029	-0.032	0.046*	-0.078**
	(0.067)	(0.033)	(0.037)	(0.025)	(0.031)
Effect of 1 SD increase	-0.030	-0.004	-0.004	0.006	-0.011
Mean of Dep. Var.	0.455	0.601	0.586	-0.001	0.588
Panel E. 100th Percentile					
1990 Dissimilarity Index	-0.131	0.086**	0.098**	0.100***	-0.002
	(0.114)	(0.041)	(0.046)	(0.037)	(0.031)
Effect of 1 SD increase	-0.018	0.012	0.013	0.013	0.000
Mean of Dep. Var.	0.611	0.728	0.720	-0.001	0.721

Notes: This table reports point estimates and heteroskedasticity robust standard errors (in parentheses) from regressions 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 (i.e., an estimate of $\lambda_{c,p}$ from Chetty and Hendren (2018*b*) scaled by assuming 20 years of exposure). 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. Statistical significance is denoted by: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 5: Effects of Racial Segregation on Public Expenditures

	OLS	2SLS		
	1990 Dissimilarity Index	1990 Dissimilarity Index	Effect of 1 SD increase	Mean of Dep. Var.
Dependent variable	(1)	(2)	(3)	(4)
Total expenditures per capita	-3.429***	-4.364***	-0.590	1.494
	(0.844)	(1.302)		
Education expenditures per capita	-1.396***	-1.674***	-0.226	0.681
	(0.344)	(0.608)		
Public safety expenditures per capita	-0.493***	-0.610***	-0.083	0.169
	(0.128)	(0.178)		
Welfare and health expenditures per capita	-0.681***	-0.796**	-0.108	0.228
	(0.246)	(0.333)		
Infrastructure expenditures per capita	-0.340***	-0.492***	-0.067	0.172
	(0.092)	(0.159)		
Other expenditures per capita	-0.520***	-0.791***	-0.107	0.244
	(0.115)	(0.209)		

Notes: This table reports point estimates and heteroskedasticity robust standard errors (in parentheses) from regressions in which the key independent variable is the racial dissimilarity index in 1990. Expenditures per capita are the average from 1987 and 1992, measured in thousands of 1990 dollars per person. Each cell of the table has 121 metro observations. See notes to Table 1 for additional details on specification and sample. Statistical significance is denoted by: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Source: Authors' calculations using data from Ananat (2011) and U.S. Bureau of the Census (2015).

Table 6: Effects of Racial Segregation on White Residents' Attitudes

	OLS	2SLS		
	1990 Dissimilarity Index	1990 Dissimilarity Index	Effect of 1 SD increase	SD of Dep. Var.
Dependent variable	(1)	(2)	(3)	(4)
Panel A. Redistributive Policy Attitudes				
Redistributive Policy Attitudes Index	1.282**	2.822*	0.382	0.835
	(0.519)	(1.477)		
Index Components				
Decrease State Legislature Spending on Welfare	1.686***	2.219	0.300	1.000
	(0.602)	(1.737)		
Decrease State Legislature Spending on Health	0.992	2.085	0.282	1.000
	(0.677)	(1.507)		
Decrease State Legislature Spending on Education	1.160*	1.468	0.198	1.000
	(0.691)	(1.564)		
Oppose Minimum Wage Increase	1.290**	5.516***	0.746	1.000
	(0.617)	(2.126)		
Panel B. Racial Attitudes				
Racial Attitudes Index	2.177***	4.562***	0.617	0.889
	(0.605)	(1.464)		
Index Components				
Racial Resentment A	2.420***	4.269**	0.577	1.000
	(0.676)	(1.663)		
Racial Resentment B	2.425***	5.046***	0.682	1.000
	(0.644)	(1.750)		
Oppose Affirmative Action	2.071***	4.895***	0.662	1.000
	(0.643)	(1.624)		
Oppose School Integration (ANES)	2.715***	4.763***	0.644	1.000
	(0.716)	(1.510)		
Oppose School Busing (ANES)	0.981	6.280***	0.849	1.000
	(0.979)	(2.043)		
Panel C. Aggressive Policing Attitudes				
Aggressive Policing Attitudes Index	0.581	2.334*	0.316	0.692
	(0.488)	(1.226)		
Index Components				
Oppose Ending Mandatory Minimum Laws	1.154	1.263	0.171	1.000
	(0.728)	(1.533)		
Oppose Body Cams	-0.397	1.648	0.223	1.000
	(0.740)	(1.873)		
Oppose Choke Hold Bans	-0.308	1.727	0.234	1.000
	(0.670)	(1.669)		
Oppose Bad Cop Registry	1.196*	3.371**	0.456	1.000
	(0.624)	(1.607)		
Oppose Allowing Individuals to Sue Police	1.260*	3.662***	0.495	1.000
	(0.655)	(1.357)		

Notes: This table reports point estimates and heteroskedasticity robust standard errors (in parentheses) from regressions in which the key independent variable is the racial dissimilarity index in 1990. All measures are constructed using responses to the CCES (except opposition to school busing and integration, which are taken from the ANES), as detailed in Appendix C. Racial Resentment A reflects agreement with the statement "The Irish, Italian, Jews and many other minorities overcame prejudice and worked their way up. Blacks should do the same." Racial Resentment B reflects disagreement with "Generations of slavery and discrimination have created conditions that make it difficult for Blacks to work their way out of the lower class." Index components are z-scores, and the summary indices are equal to the average of their respective components. Statistical significance is denoted by: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 7: Effects of Racial Segregation on Attitudes, by Income for White Respondents and Black Respondents

	OLS	2SLS		
	1990 Dissimilarity Index	1990 Dissimilarity Index	Effect of 1 SD increase	SD of Dep. Var.
Dependent variable	(1)	(2)	(3)	(4)
Panel A. All White Respondents				
Redistributive Policy Attitudes Index	1.282**	2.822*	0.382	0.835
	(0.519)	(1.477)		
(3-Item) Racial Attitudes Index	2.306***	4.737***	0.641	0.954
	(0.627)	(1.628)		
Aggressive Policing Attitudes Index	0.581	2.334*	0.316	0.692
	(0.488)	(1.226)		
Panel B. White, Below Median Income				
Redistributive Policy Attitudes Index	2.209***	3.286**	0.444	0.885
	(0.544)	(1.450)		
(3-Item) Racial Attitudes Index	2.687***	5.239***	0.709	0.968
	(0.638)	(1.702)		
Aggressive Policing Attitudes Index	1.502***	2.926*	0.396	0.799
	(0.536)	(1.519)		
Panel C. White, Above Median Income				
Redistributive Policy Attitudes Index	-0.002	2.002	0.271	1.304
	(0.889)	(2.165)		
(3-Item) Racial Attitudes Index	1.669**	3.173*	0.429	1.140
	(0.786)	(1.731)		
Aggressive Policing Attitudes Index	-0.396	0.991	0.134	1.320
	(0.884)	(1.615)		
Panel D. Black Respondents				
Redistributive Policy Attitudes Index	-1.412	-2.934	-0.397	1.661
	(1.242)	(2.504)		
(3-Item) Racial Attitudes Index	-2.940**	-4.827**	-0.653	1.549
	(1.391)	(2.398)		
Aggressive Policing Attitudes Index	-0.281	-0.211	-0.028	1.979
	(1.044)	(2.017)		

Notes: This table reports point estimates and heteroskedasticity robust standard errors (in parentheses) from regressions in which the key independent variable is the racial dissimilarity index in 1990. The racial attitudes index here differs from Table 6 by excluding the outcomes from the ANES (attitudes toward school integration and school busing), which has a smaller sample size. The family income cutoff for Panels B and C is $60,000. When constructing z-scores, we use the mean and standard deviation for all White respondents to facilitate comparisons of the effects of segregation across different groups. Statistical significance is denoted by: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 8: Simple Calculations of Aggregate Changes in Upward Mobility due to Racial Segregation

	Mean upward mobility		Income at percentile	
	Actual (1)	No segregation (2)	Actual (3)	No segregation (4)
Panel A. Black Mobility				
1st percentile	0.270	0.496	9,869	29,305
25th percentile	0.339	0.538	15,582	32,734
50th percentile	0.397	0.573	20,696	35,337
75th percentile	0.455	0.608	25,878	38,898
100th percentile	0.611	0.701	38,898	47,723
Panel B. White Mobility				
1st percentile	0.357	0.511	17,249	30,159
25th percentile	0.450	0.552	25,018	33,596
50th percentile	0.524	0.585	31,016	37,103
75th percentile	0.601	0.619	37,997	39,808
100th percentile	0.728	0.675	51,177	45,602
Panel C. Black-White Mobility Gap				
1st percentile	0.087	0.015	7,380	854
25th percentile	0.111	0.014	9,436	862
50th percentile	0.127	0.012	10,320	1,766
75th percentile	0.146	0.011	12,119	909
100th percentile	0.117	-0.026	12,279	-2,121

Notes: In Panels A and B, column 1 reports the observed mean upward mobility rate by race and parental income rank. Column 2 calculates the counterfactual level of mobility in a scenario with no racial segregation, 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 (0.688 for Black children and 0.621 for White children). Columns 3 and 4 report the individual income amount (measured in 2015 dollars) associated with the percentiles in columns 1 and 2, respectively. In Panel C, we calculate the difference in upward mobility rates and associated income levels between White and Black children.
Source: Authors' calculations using data from Ananat (2011) and Chetty et al. (2020*a*).

Table 9: Simple Calculations of Aggregate Changes in Children's Earnings due to Segregation

	Change in income per person, $ (1)	Number of children, millions (2)	Total change in income, billions $ (3)
Black Individuals			
Black, 25th percentile	-17,152	1.5	-25.7
Black, 75th percentile	-13,020	0.7	-9.6
White Individuals			
White, 25th percentile	-8,577	3.4	-28.7
White, 75th percentile	-1,811	8.8	-15.9
Black and White Individuals			
Total	–	14.3	-79.8

Notes: Column 1 reports the change in individual income associated with the impacts of segregation on upward mobility, calculated as the difference between columns 3 and 4 of Table 8. Column 2 reports the number of children from households below or above the nationwide median in our sample cities. Column 3 equals the product of columns 1 and 2, expression in billions of year 2015 dollars.

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

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 2 report our replication of Table 1 of Ananat (2011). With minor exceptions, we replicate her results exactly.[34] 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.[35] 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 7 and 7 show that our IV estimates are similar when controlling for historical railroad track density (column 2) and when controlling for the baseline city characteristics that are available for all metros (column 3).

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

[34] 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.

[35] 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, 2016).

(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 the survey-based measures of attitudes toward redistributive policy, race, and aggressive policing that appear in Tables 6 and 7.

Redistributive Policy Attitudes: To proxy broader attitudes toward redistributive policy, we use questions on state policy spending (Welfare, Health Care, Education) and minimum wage policy—questions asked in multiple waves of the CCES (Ansolabehere, 2012; Ansolabehere and Schaffner, 2013; Schaffner and Ansolabehere, 2015; Schaffner, Ansolabehere and Luks, 2019, 2021).[36] For state program spending (asked in 2014, 2016, 2018, and 2020), respondents were asked about five categories, of which we omit Transportation and Law Enforcement since the redistributive implications are more ambiguous. For the minimum wage questions, we use questions in three years (2016, 2018, and 2020) that are similar but about different possible amounts ($12 vs. $15) at different levels (state vs. federal) and by different political bodies (state vs. Congress).

- *State Legislature Spending*: "State legislatures must make choices when making spending decisions on important state programs. How would you like your legislature to spend money on each of the five areas below?"[37] (1: Greatly Increase, 2: Slightly Increase, 3: Maintain, 4: Slightly Decrease, 5: Greatly Decrease). These are in questions CC426 (2014), CC16_426 (2016), CC18_426 (2018), CC20_443 (2020), and the original value coding was maintained.
  - Welfare
  - Health Care
  - Education
- *Minimum Wage Increases*: These questions originally were coded as (1: For, 2: Against) and recoded to binary 0/1 with 1 corresponding to "Against":
  - 2016 (CC16_351K): "Congress considers many issues. If you were in Congress would you vote FOR or AGAINST each of the following?": "Raises the federal minimum wage to $12 an hour by 2020."
  - 2018 (CC18_414A): "If your state put the following questions for a vote on the ballot, would you vote FOR or AGAINST?": "Raise the state minimum wage to $12 an hour."
  - 2020 (CC20_350B): "Over the past two years, Congress voted on many issues. Do you support each of the following proposals?": "Raise the minimum wage to $15 an hour."

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

[37] The second sentence was asked slightly differently only in 2016 as, "Would you like your legislature to increase or decrease spending on the five areas below?"

For all questions in Table 6, we limit the sample to White respondents, giving us roughly 10,000 to 13,000 respondents in each survey wave in the Ananat (2011) sample of metros. Since legislature spending questions were asked across four survey waves, the total sample size is roughly 44,000 respondents, whereas for the minimum wage question asked in three waves, the total sample is roughly 36,000 respondents. For heterogeneity analysis in Table 7, the sample sizes per wave are roughly 1,000 Black respondents and 4,000 to 6,000 respondents for each of the above/below median income groups.

Racial Attitudes: As noted in the text, to gauge racial attitudes we use questions corresponding to the concept of "racial resentment," as well as policy positions that are racially-charged (affirmative action and school integration/busing policies).[38] 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. 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 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)

Again, there are roughly 10,000 to 12,000 White respondents in each survey wave in the Ananat (2011) sample of metros, for a total of roughly 53,000 observations for the racial resentment questions (five waves) and 35,000 for the affirmative action question (three waves). We construct averages using 1990 county population weights.

[38] Racial resentment is a measure of "symbolic racism" (also referred to as "modern racism"), described by Henry and Sears (2002) as capturing the idea that "among whites, new forms of prejudice embody negative feelings toward blacks as a group combined with a sense that blacks violate cherished American values." This line of research argues that this new form of racism has overtaken the older belief system that "incorporated social distance between the races, beliefs in the biological inferiority of blacks, and support for formal discrimination and segregation." As noted by Cramer (2020), "the dominant measure of symbolic racism in political science has been the racial resentment scale, developed for the American National Election Study (ANES) in the mid-1980s by Kinder and Sanders (1996)."

The final two measures of racial attitudes used in Table 6 regard 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:

- *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 enough about (1986,1990 AND LATER: interested enough 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 (1964-1970: 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.[39] Since these ANES measures

[39] The underlying counts of White survey respondents captured in these metro areas are as follows: School Integra-

have much smaller sample sizes, we do not include them in the sub-group analyses presented in Table 7.

Aggressive Policing Attitudes: To measure attitudes toward aggressive policing, we use a subset of questions asked on a module newly-added to the CCES in the 2020 wave. Specifically, we use five of the eight questions in this module (CC20_334), omitting questions about spending (on increasing or decreasing the number of police and on sharing surplus military weapons and equipment from the Department of Defense). The additional questions that we omit are highly correlated with other measures in the module and would strengthen statistical significance; however, their implications for aggressiveness are somewhat ambiguous. For the questions that we use, each has the possible options of "Support" or "Oppose," which we code as binary with 1 corresponding to "Oppose":

- "Do you support or oppose each of the following proposals?"
  - "Eliminate mandatory minimum sentences for non-violent drug offenders." (CC20_334a)
  - "Require police officers to wear body cameras that record all of their activities while on duty." (CC20_334b)
  - "Ban the use of choke holds by police." (CC20_334e)
  - "Create a national registry of police who have been investigated for or disciplined for misconduct." (CC20_334f)
  - "Allow individuals or their families to sue a police officer for damages if the officer is found to have 'recklessly disregarded' the individual's rights." (CC20_334h)

As this module is present only in 2020, the sample size for this set of questions is roughly 12,000 respondents.

Family Income Heterogeneity Finally, in Table 7 we look at heterogeneity by income and race. For income, we use the questions on family income across all survey years. This question was worded as follows: "Thinking back over the last year, what was your family's annual income?" ("faminc" in 2010, 2012, 2014, and 2016; "faminc_new" in 2018 and 2020). Response options were "Less than $10,000," "$10,000-19,999,"...,"$70,000-79,999," "80,000-99,999," and so on (2010 was recoded to match the later years). For these results, we drop the roughly 10% of respondents who "Prefer not to say" for this income question.

D Details on Constructing an Instrumental Variable for Black Population Share

This appendix describes how we construct an instrumental variable for the 1990 Black population share of a metropolitan area, as analyzed in Section 5.2.

tion Policies: 288 (1970), 793 (1978), 408 (1986), 312 (1990), 741 (1992), 579 (1994). School Busing: 498 (1980), 355 (1984).

Formally, our instrument for the 1990 Black population share percentile, $\text{BlackSharePctile}_c$, is based on the predicted number of Black migrants to a metro area from 1910 to 1990, defined as follows:

$$\text{Predicted Black Migrants}_c^{1910-1990} = \sum_s \sum_{t=1910}^{1980} w_{s,c}^{1910} M_s^{t,t+10}, \tag{D1}$$

where $w_{s,c}^{1910}$ is the share of African American migrants born in Southern state s that lived in metropolitan area c in 1910, and $M_s^{t,t+10}$ is the net number of Black migrants that moved away from state s between years t and $t+10$.

We construct $w_{s,c}^{1910}$ using the complete count 1910 Census (Ruggles et al., 2021), which contains information on individuals' county of residence and state of birth. In particular, $w_{s,c}^{1910}$ is equal to the number of Black individuals who were born in Southern state s and resided in non-Southern county c divided by the total number of Black individuals who were born in Southern state s and resided outside the South.[40]

We construct $M_s^{t,t+10}$ using the forward survival method, as in other work (e.g., Gregory, 2005; Boustan, 2010; Fouka, Mazumder and Tabellini, 2020). In particular, we estimate net migration out of a state between years t and $t+10$ as

$$M_s^{t,t+10} = P_s^{t+10} - \sum_a g_a^t P_{s,a}^t - P_s^t b^t, \tag{D2}$$

where P_s^t is the total Black population in state s in year t, $P_{s,a}^t$ is the population in five-year age a, g_a^t is the nationwide survival rate, and b^t is the nationwide birth rate. We construct population from 1910–1940 using complete count Census data (Ruggles et al., 2021). For 1950–1990, we construct population using county-level tabulations from the Census (Manson et al., 2022). We estimate the survival rate g_a^t as the ratio of the weighted number of individuals in a five-year birth cohort observed in the Census in year $t+10$ to the weighted number of individuals in the same five-year cohort in year t. We estimate the birth rate as the ratio of the weighted number of individuals who were born between years t and $t+10$ to the weighted number of individuals observed in year t. We construct these population counts using complete count Census data for 1910–1940 and sample data for 1950–1990 (Ruggles et al., 2021, 2022).[41]

To construct our instrument, we divide $\text{Predicted Black Migrants}_c^{1910-1990}$ by the population of the metro area in 1910. Following Derenoncourt (2022), we use percentiles of this ratio as our instrumental variable to ensure that our results are not driven by outliers.

[40] For the purpose of constructing this instrument, we follow Derenoncourt (2022) in defining the South to consist of Alabama, Arkansas, Florida, Georgia, Kentucky, Louisiana, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Virginia, and West Virginia. We aggregate counties to 1990 metropolitan area definitions, as is done in our main analysis.

[41] We use Black individuals born in the United States for calculating survival and birth rates.

Appendix Table 1: Overview of Key Variables, Samples, and Data

Variables	**Years Measured**	**Sample**	**Source**
Dissimilarity Index	1990	121 non-Southern metropolitan Areas	Cutler, Glaeser, and Vigdor (1999), via Ananat (2011)
Railroad Division Index	Late 19th century	121 non-Southern metropolitan Areas	Ananat (2011)
Income mobility, incarceration, and teenage pregnancy for children with parents at the 1st, 25th, 50th, 75th, and 99th percentiles of the national income distribution	Children's income (2010 and 2014–2015); Parents' income (1994, 1995, 1998–2000)	Children born from 1978-1983	Chetty et al. (2020)
Estimated exposure effect (the causal effect of spending one additional year of childhood in a given CZ)	1996–2012	Children born from 1980-1988 who moved once across commuting zones between 1997 and 2010	Chetty and Hendren (2018b)
Average math and reading test scores on standardized exams	2008–2009 to 2017–2018 school years	Children enrolled in grades 3–8 in these years (likely born in the 1999-2009 cohorts)	Reardon (2021)
Local government expenditures: Total, education, public safety, welfare and health, infrastructure, other	1987 and 1992	Local government units	Census of Governments
Redistributive policy attitudes	Attitudes toward state legislature spending (2014, 2016, 2018, 2020) and minimum wage policies (2016, 2018, 2020)	CCES respondents	Cooperative Congressional Election Study (CCES)
Racial attitudes (except for school integration and busing) and aggressive policing attitudes	Racial resentment (2010, 2012, 2014, 2018, 2020); Affirmative action in 2010, 2012, 2014); Policing policies (2020)	CCES respondents	Cooperative Congressional Election Study (CCES)
Racial attitudes on school integration and school busing	School integration policy (1970, 1978, 1986, 1992, 1994); school busing policy (1980, 1984)	ANES respondents	American National Election Studies (ANES)

Notes: This table provides further details on the key variables used in our analysis, the samples on which each measure is based, and the data sources. Further details on the CCES and ANES measures can be found in Appendix C.

Appendix Table 2: 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	0.014	-1.917	0.010	0.276	121
	(0.033)	(2.292)	(0.033)		

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. We depart from Ananat (2011) by constructing an income segregation measure using the approach of Cutler and Glaeser (1997) from the underlying tract-level data because this variable is missing for 52 metro areas in the Ananat (2011) data. Statistical significance is denoted by: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.
Source: Authors' calculations using data from Ananat (2011), Cutler, Glaeser and Vigdor (1999), and Manson et al. (2022).

Appendix Table 3: Effects of Racial Segregation on Upward Mobility, Robustness to Controlling for Observed Variables

	2SLS Coefficient on 1990 Dissimilarity Index					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Black Mobility						
1st percentile	-0.329***	-0.339***	-0.331***	-0.469**	-0.438**	-0.330***
	(0.092)	(0.106)	(0.088)	(0.192)	(0.174)	(0.093)
25th percentile	-0.289***	-0.298***	-0.316***	-0.391***	-0.399***	-0.290***
	(0.072)	(0.084)	(0.083)	(0.144)	(0.143)	(0.073)
50th percentile	-0.255***	-0.264***	-0.303***	-0.325***	-0.367***	-0.255***
	(0.064)	(0.074)	(0.088)	(0.117)	(0.129)	(0.064)
75th percentile	-0.222***	-0.230***	-0.290***	-0.259**	-0.334***	-0.221***
	(0.067)	(0.076)	(0.100)	(0.114)	(0.127)	(0.066)
100th percentile	-0.131	-0.137	-0.256*	-0.081	-0.246	-0.128
	(0.114)	(0.128)	(0.152)	(0.204)	(0.184)	(0.112)
Panel B. White Mobility						
1st percentile	-0.248***	-0.269***	-0.307***	-0.243**	-0.356***	-0.253***
	(0.065)	(0.077)	(0.079)	(0.111)	(0.130)	(0.065)
25th percentile	-0.164***	-0.181***	-0.213***	-0.186**	-0.266***	-0.168***
	(0.049)	(0.059)	(0.057)	(0.086)	(0.101)	(0.050)
50th percentile	-0.098**	-0.111**	-0.138***	-0.140**	-0.195**	-0.100**
	(0.039)	(0.047)	(0.044)	(0.068)	(0.080)	(0.040)
75th percentile	-0.029	-0.038	-0.060	-0.093*	-0.121*	-0.029
	(0.033)	(0.039)	(0.038)	(0.055)	(0.062)	(0.034)
100th percentile	0.086**	0.083*	0.069	-0.014	0.002	0.088**
	(0.041)	(0.045)	(0.053)	(0.054)	(0.057)	(0.039)
Controls						
Historical railroad track density		✓				
1910–1920 city characteristics			✓			
Region fixed effects				✓		
1970 & 1990 unemp. rate & manufacturing emp. share					✓	
Income segregation						✓

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 historical railroad track length per square kilometer. The results in column 3 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 4 controls for Census region fixed effects, column 5 controls for the unemployment rate and manufacturing employment share in 1970 and 1990, and column 6 controls for income segregation using the dissimilarity index approach in Cutler and Glaeser (1997). See notes to Table 1 for additional details on sample. Statistical significance is denoted by: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Source: Authors' calculations using data from Ananat (2011), Chetty et al. (2020*a*), and Manson et al. (2022).

Appendix Table 4: Effects of Racial Segregation on Upward Mobility, Robustness to Alternative Confidence Interval Estimates

		Confidence interval		
	Point estimate	Asymptotic	Anderson-Rubin	tF
	(1)	(2)	(3)	(4)
Panel A. Black Mobility				
1st percentile	-0.329	[-0.510, -0.148]	[-0.582, -0.172]	[-0.564, -0.095]
25th percentile	-0.289	[-0.431, -0.147]	[-0.493, -0.172]	[-0.473, -0.106]
50th percentile	-0.255	[-0.381, -0.130]	[-0.435, -0.151]	[-0.418, -0.093]
75th percentile	-0.222	[-0.353, -0.091]	[-0.404, -0.108]	[-0.391, -0.052]
100th percentile	-0.131	[-0.353, 0.092]	[-0.396, 0.090]	[-0.419, 0.158]
Panel B. White Mobility				
1st percentile	-0.248	[-0.376, -0.120]	[-0.426, -0.137]	[-0.413, -0.083]
25th percentile	-0.164	[-0.260, -0.068]	[-0.298, -0.081]	[-0.289, -0.040]
50th percentile	-0.098	[-0.174, -0.022]	[-0.201, -0.032]	[-0.197, 0.001]
75th percentile	-0.029	[-0.094, 0.037]	[-0.112, 0.034]	[-0.113, 0.056]
100th percentile	0.086	[0.007, 0.166]	[0.001, 0.174]	[-0.017, 0.189]

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 5: Relationship Between RDI and Upward Mobility by Distance from the South

	All metros		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)	Railroad Division Index (5)	Effect of 1 SD increase (6)	Mean of Dep. Var (7)
Panel A. Black Mobility							
1st percentile	-0.132***	-0.019	-0.150***	-0.021	-0.064	-0.009	0.270
	(0.023)		(0.030)		(0.048)		
25th percentile	-0.116***	-0.016	-0.140***	-0.020	-0.058**	-0.008	0.339
	(0.017)		(0.023)		(0.027)		
50th percentile	-0.102***	-0.014	-0.132***	-0.019	-0.053***	-0.007	0.397
	(0.018)		(0.023)		(0.020)		
75th percentile	-0.089***	-0.013	-0.124***	-0.017	-0.048	-0.007	0.455
	(0.024)		(0.027)		(0.031)		
100th percentile	-0.052	-0.007	-0.101**	-0.014	-0.034	-0.005	0.611
	(0.047)		(0.049)		(0.082)		
Panel B. White Mobility							
1st percentile	-0.099***	-0.014	-0.108***	-0.015	-0.052	-0.007	0.357
	(0.022)		(0.028)		(0.038)		
25th percentile	-0.066***	-0.009	-0.075***	-0.011	-0.047*	-0.007	0.450
	(0.016)		(0.021)		(0.025)		
50th percentile	-0.039***	-0.006	-0.049***	-0.007	-0.043**	-0.006	0.524
	(0.013)		(0.017)		(0.018)		
75th percentile	-0.011	-0.002	-0.021	-0.003	-0.039**	-0.006	0.601
	(0.012)		(0.016)		(0.018)		
100th percentile	0.034*	0.005	0.025	0.003	-0.032	-0.005	0.728
	(0.019)		(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 all 121 metros in our analysis sample. Columns 3–4 report results for 92 metros that are less than 400 miles from the South, and columns 5–6 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. Statistical significance is denoted by: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Appendix Table 6: Effects of Racial Segregation on Upward Mobility, Robustness to Alternative Segregation Measures

Segregation measure:	1990 Dissimilarity		1990 Isolation		1990 Clustering		1990 Concentration		1990 Centralization		1940 Dissimilarity	
	IV estimate (1)	1 SD effect (2)	IV estimate (3)	1 SD effect (4)	IV estimate (5)	1 SD effect (6)	IV estimate (7)	1 SD effect (8)	IV estimate (9)	1 SD effect (10)	IV estimate (11)	1 SD effect (12)
Panel A. Mobility Estimates												
Black, 25th percentile	-0.289***	-0.039	-0.201***	-0.038	-0.181***	-0.041	-0.229***	-0.054	-0.431*	-0.089	-0.568**	-0.049
	(0.072)		(0.041)		(0.041)		(0.079)		(0.243)		(0.268)	
Black, 75th percentile	-0.222***	-0.030	-0.154***	-0.029	-0.139***	-0.031	-0.176***	-0.042	-0.330*	-0.069	-0.418*	-0.036
	(0.067)		(0.041)		(0.038)		(0.059)		(0.187)		(0.233)	
White, 25th percentile	-0.164***	-0.022	-0.114***	-0.022	-0.103***	-0.023	-0.130**	-0.031	-0.245*	-0.051	-0.482**	-0.042
	(0.049)		(0.033)		(0.032)		(0.054)		(0.141)		(0.231)	
White, 75th percentile	-0.029	-0.004	-0.020	-0.004	-0.018	-0.004	-0.023	-0.005	-0.043	-0.009	-0.110	-0.010
	(0.033)		(0.022)		(0.020)		(0.029)		(0.055)		(0.126)	
Panel B. Summary Statistics												
SD of segregation measure	0.135		0.189		0.227		0.238		0.207		0.087	
Correlation with 1990 dissimilarity index	1.000		0.850		0.762		0.501		0.156		0.562	
F-statistic	21.57		32.10		24.72		7.46		3.16		5.44	

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 segregation measure indicated in the top row. 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 25th percentile of the nationwide income distribution). Odd-numbered columns present estimates in which the segregation measure is instrumented by the railroad division index (RDI). Even-numbered columns scale the coefficients reported in the preceding column by one standard deviation of the segregation measure, which is indicated in Panel B. For columns 1–10, sample contains 121 non-Southern metro areas for which the RDI variable is available. For columns 11–12, we limit the sample to the 69 metro areas where the 1940 Black population share is at least 1 percent to ensure that the 1940 segregation measure is meaningful. Statistical significance is denoted by: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Source: Authors' calculations using data from Ananat (2011), Cutler, Glaeser and Vigdor (1999), and Chetty et al. (2020*a*).

Appendix Table 7: Effects of Racial Segregation on Incarceration, Teenage Births, and Grade 3–8 Test Scores, Robustness to Controlling for Observed Variables

	2SLS Coefficient on 1990 Dissimilarity Index					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Black Male Incarceration						
1st percentile	0.503***	0.507***	0.575***	0.690**	0.647**	0.503***
	(0.165)	(0.184)	(0.209)	(0.308)	(0.280)	(0.165)
25th percentile	0.248***	0.243***	0.281***	0.342**	0.306**	0.249***
	(0.074)	(0.081)	(0.083)	(0.138)	(0.119)	(0.074)
50th percentile	0.134***	0.124**	0.149**	0.185**	0.152*	0.135***
	(0.051)	(0.056)	(0.060)	(0.092)	(0.081)	(0.050)
75th percentile	0.055	0.043	0.059	0.077	0.047	0.057
	(0.055)	(0.062)	(0.078)	(0.099)	(0.096)	(0.053)
100th percentile	-0.015	-0.030	-0.022	-0.019	-0.047	-0.012
	(0.072)	(0.082)	(0.107)	(0.130)	(0.130)	(0.069)
Panel B. White Male Incarceration						
1st percentile	0.102**	0.109**	0.105**	0.150**	0.185**	0.102**
	(0.043)	(0.050)	(0.046)	(0.072)	(0.076)	(0.043)
25th percentile	0.043**	0.046**	0.046**	0.068**	0.080**	0.043**
	(0.018)	(0.021)	(0.019)	(0.031)	(0.033)	(0.019)
50th percentile	0.018**	0.019*	0.020**	0.033**	0.035**	0.018**
	(0.008)	(0.010)	(0.009)	(0.014)	(0.014)	(0.008)
75th percentile	0.004	0.005	0.006	0.014**	0.011*	0.004
	(0.004)	(0.004)	(0.005)	(0.007)	(0.006)	(0.004)
100th percentile	-0.006	-0.006	-0.004	-0.001	-0.008	-0.007*
	(0.004)	(0.005)	(0.006)	(0.008)	(0.006)	(0.004)
Panel C. Black Female Teenage Birth						
1st percentile	0.793***	0.805***	0.929***	0.608**	0.931***	0.791***
	(0.194)	(0.218)	(0.256)	(0.284)	(0.333)	(0.193)
25th percentile	0.703***	0.714***	0.810***	0.578***	0.847***	0.702***
	(0.142)	(0.159)	(0.185)	(0.201)	(0.254)	(0.142)
50th percentile	0.601***	0.610***	0.676***	0.544***	0.751***	0.601***
	(0.103)	(0.115)	(0.129)	(0.161)	(0.198)	(0.103)
75th percentile	0.521***	0.529***	0.571***	0.518***	0.675***	0.522***
	(0.102)	(0.116)	(0.130)	(0.193)	(0.202)	(0.102)
100th percentile	0.375**	0.380**	0.378*	0.470	0.538*	0.377**
	(0.165)	(0.188)	(0.219)	(0.331)	(0.301)	(0.164)
Panel D. White Female Teenage Birth						
1st percentile	0.474***	0.539***	0.562***	0.441*	0.772***	0.481***
	(0.152)	(0.179)	(0.169)	(0.239)	(0.298)	(0.153)
25th percentile	0.340***	0.389***	0.408***	0.330*	0.569***	0.345***
	(0.111)	(0.131)	(0.123)	(0.176)	(0.219)	(0.112)
50th percentile	0.218***	0.251***	0.266***	0.229*	0.384***	0.221***
	(0.074)	(0.088)	(0.081)	(0.119)	(0.147)	(0.075)
75th percentile	0.107**	0.127**	0.139***	0.138**	0.217***	0.108**
	(0.042)	(0.050)	(0.046)	(0.070)	(0.084)	(0.043)
100th percentile	-0.017	-0.013	-0.004	0.035	0.029	-0.018
	(0.018)	(0.020)	(0.024)	(0.032)	(0.032)	(0.017)
Panel E. Test Scores in Grades 3–8						
Black test scores	-0.998***	-1.066***	-1.418***	-0.990*	-1.848***	-0.994***
	(0.323)	(0.362)	(0.501)	(0.522)	(0.592)	(0.322)
White test scores	-0.513	-0.599*	-1.116***	-0.918*	-1.533***	-0.550*
	(0.318)	(0.357)	(0.340)	(0.555)	(0.579)	(0.298)
Controls						
Historical railroad track density		✓				
1910–1920 city characteristics			✓			
Region fixed effects				✓		
1970 & 1990 unemp. rate & manufacturing emp. share					✓	
Income segregation						✓

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 3 for additional details on specifications. Statistical significance is denoted by: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Appendix Table 8: Effects of Racial Segregation on White, Below Median Income Residents' Attitudes

	OLS	2SLS		
Dependent variable	1990 Dissimilarity Index (1)	1990 Dissimilarity Index (2)	Effect of 1 SD increase (3)	SD of Dep. Var (4)
Panel A: Redistributive Policy Attitudes				
Redistributive Policy Attitudes Index	2.209***	3.286**	0.444	0.885
	(0.544)	(1.450)		
Index Components				
Decrease State Legislature Spending on Welfare	2.264***	2.122	0.287	1.027
	(0.595)	(1.715)		
Decrease State Legislature Spending on Health	2.621***	4.165**	0.563	1.130
	(0.772)	(1.648)		
Decrease State Legislature Spending on Education	2.239***	1.206	0.163	1.080
	(0.693)	(1.666)		
Oppose Minimum Wage Increase	1.711**	5.652***	0.764	1.115
	(0.706)	(2.101)		
Panel B: Racial Attitudes				
(3-Item) Racial Attitudes Index	2.687***	5.239***	0.709	0.968
	(0.638)	(1.702)		
Index Components				
Racial Resentment A	2.801***	4.174***	0.564	1.009
	(0.687)	(1.591)		
Racial Resentment B	2.948***	5.591***	0.756	1.032
	(0.642)	(1.833)		
Oppose Affirmative Action	2.313***	5.954***	0.805	1.081
	(0.698)	(1.919)		
Panel C: Aggressive Policing Attitudes				
Aggressive Policing Attitudes Index	1.502***	2.926*	0.396	0.799
	(0.536)	(1.519)		
Index Components				
Oppose Ending Mandatory Minimum Laws	2.000***	0.286	0.039	1.179
	(0.763)	(1.590)		
Oppose Body Cams	-0.048	2.969	0.402	1.414
	(0.959)	(2.421)		
Oppose Choke Hold Bans	1.252	4.448*	0.601	1.225
	(0.899)	(2.356)		
Oppose Bad Cop Registry	1.969**	2.642	0.357	1.295
	(0.791)	(2.428)		
Oppose Allowing Individuals to Sue Police	2.338***	4.286**	0.580	1.166
	(0.716)	(1.862)		

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. All measures are constructed using responses to the CCES, as detailed in Appendix C. Racial Resentment A reflects agreement with the statement "The Irish, Italian, Jews and many other minorities overcame prejudice and worked their way up. Blacks should do the same." Racial Resentment B reflects disagreement with "Generations of slavery and discrimination have created conditions that make it difficult for Blacks to work their way out of the lower class." Index components are z-scores, and the summary indices are equal to the average of their respective components. Statistical significance is denoted by: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Appendix Table 9: Effects of Racial Segregation on White, Above Median Income Residents' Attitudes

	OLS	2SLS		
Dependent variable	1990 Dissimilarity Index (1)	1990 Dissimilarity Index (2)	Effect of 1 SD increase (3)	SD of Dep. Var (4)
Panel A: Redistributive Policy Attitudes				
Redistributive Policy Attitudes Index	-0.002	2.002	0.271	1.304
	(0.889)	(2.165)		
Index Components				
Decrease State Legislature Spending on Welfare	0.737	2.565	0.347	1.486
	(1.003)	(2.490)		
Decrease State Legislature Spending on Health	-0.660	0.380	0.051	1.523
	(1.042)	(2.348)		
Decrease State Legislature Spending on Education	-0.297	0.764	0.103	1.571
	(1.107)	(2.316)		
Oppose Minimum Wage Increase	0.211	4.297	0.581	1.395
	(0.926)	(2.693)		
Panel B: Racial Attitudes				
(3-Item) Racial Attitudes Index	1.669**	3.173*	0.429	1.140
	(0.786)	(1.731)		
Index Components				
Racial Resentment A	2.042**	3.233*	0.437	1.157
	(0.804)	(1.807)		
Racial Resentment B	1.479*	3.773*	0.510	1.253
	(0.895)	(1.969)		
Oppose Affirmative Action	1.487*	2.513	0.340	1.271
	(0.851)	(1.806)		
Panel C: Aggressive Policing Attitudes				
Aggressive Policing Attitudes Index	-0.396	0.991	0.134	1.320
	(0.884)	(1.615)		
Index Components				
Oppose Ending Mandatory Minimum Laws	-0.161	0.594	0.080	1.497
	(1.039)	(1.968)		
Oppose Body Cams	-1.223	-1.715	-0.232	1.816
	(1.207)	(3.231)		
Oppose Choke Hold Bans	-1.771	-0.549	-0.074	1.816
	(1.190)	(2.313)		
Oppose Bad Cop Registry	0.485	3.907*	0.528	1.736
	(1.140)	(2.123)		
Oppose Allowing Individuals to Sue Police	0.691	2.719	0.368	1.812
	(1.176)	(2.385)		

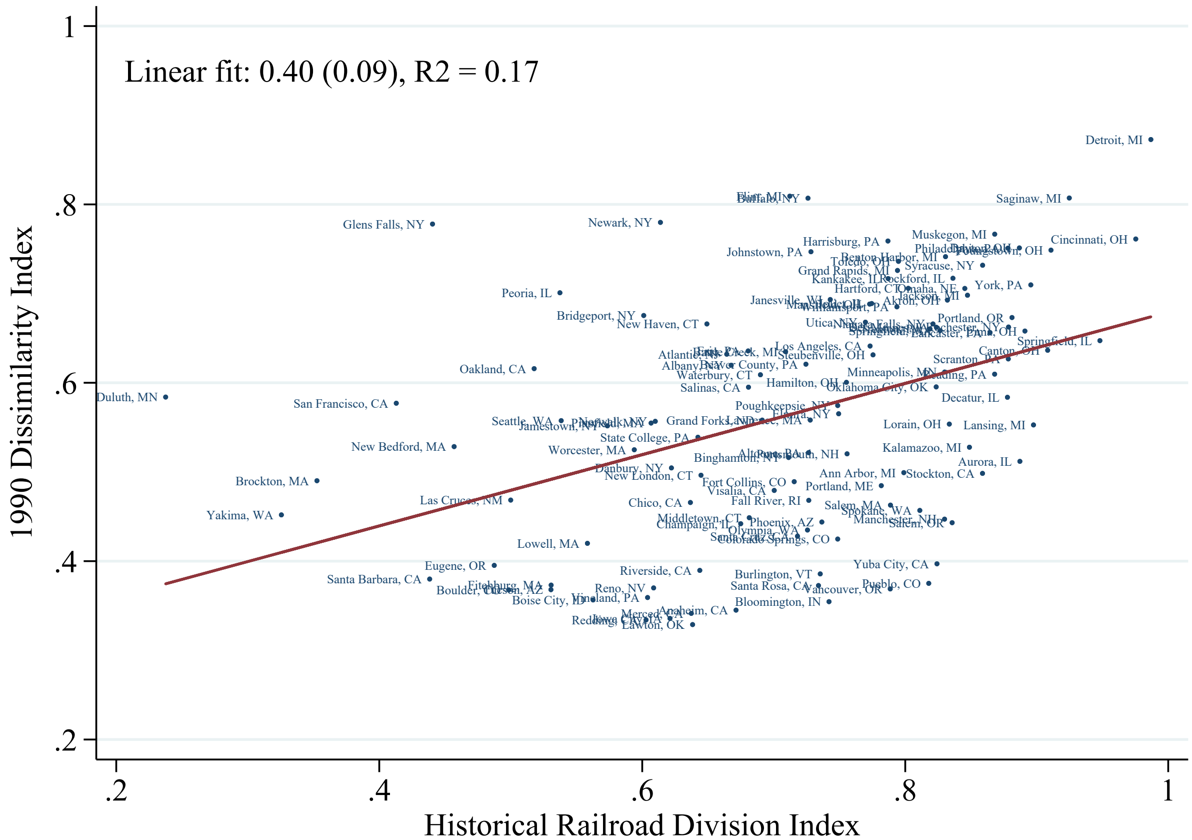
Notes: This table reports point estimates and heteroskedasticity robust standard errors (in parentheses) from models in which the key independent variable is the racial dissimilarity index in 1990. All measures are constructed using responses to the CCES, as detailed in Appendix C. Racial Resentment A reflects agreement with the statement "The Irish, Italian, Jews and many other minorities overcame prejudice and worked their way up. Blacks should do the same." Racial Resentment B reflects disagreement with "Generations of slavery and discrimination have created conditions that make it difficult for Blacks to work their way out of the lower class." Index components are z-scores, and the summary indices are equal to the average of their respective components. Statistical significance is denoted by: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Appendix Table 10: Effects of Racial Segregation on Black Residents' Attitudes

	OLS	2SLS		
Dependent variable	1990 Dissimilarity Index (1)	1990 Dissimilarity Index (2)	Effect of 1 SD increase (3)	SD of Dep. Var (4)
Panel A: Redistributive Policy Attitudes				
Redistributive Policy Attitudes Index	-1.412	-2.934	-0.397	1.661
	(1.242)	(2.504)		
Index Components				
Decrease State Legislature Spending on Welfare	-0.102	0.618	0.084	2.145
	(1.543)	(3.392)		
Decrease State Legislature Spending on Health	0.110	-3.866	-0.523	2.837
	(1.740)	(4.022)		
Decrease State Legislature Spending on Education	-4.116**	-3.723	-0.503	2.547
	(1.981)	(3.488)		
Oppose Minimum Wage Increase	-1.363	-3.787	-0.512	1.778
	(1.350)	(3.928)		
Panel B: Racial Attitudes				
Racial Attitudes Index	-2.940**	-4.827**	-0.653	1.549
	(1.391)	(2.398)		
Index Components				
Racial Resentment A	-2.191	-7.334**	-0.992	1.827
	(1.438)	(3.657)		
Racial Resentment B	-3.722**	-3.020	-0.408	1.813
	(1.572)	(3.092)		
Oppose Affirmative Action	-2.908	-4.127	-0.558	2.184
	(1.857)	(3.064)		
Panel C: Aggressive Policing Attitudes				
Aggressive Policing Attitudes Index	-0.281	-0.211	-0.028	1.979
	(1.044)	(2.017)		
Index Components				
Oppose Ending Mandatory Minimum Laws	1.144	1.878	0.254	2.780
	(1.568)	(3.007)		
Oppose Body Cams	1.389	-0.349	-0.047	3.041
	(1.647)	(3.067)		
Oppose Choke Hold Bans	-2.608	0.013	0.002	3.296
	(1.851)	(2.931)		
Oppose Bad Cop Registry	0.408	-2.232	-0.302	2.734
	(1.676)	(3.301)		
Oppose Allowing Individuals to Sue Police	-1.736	-0.363	-0.049	2.912
	(1.630)	(2.484)		

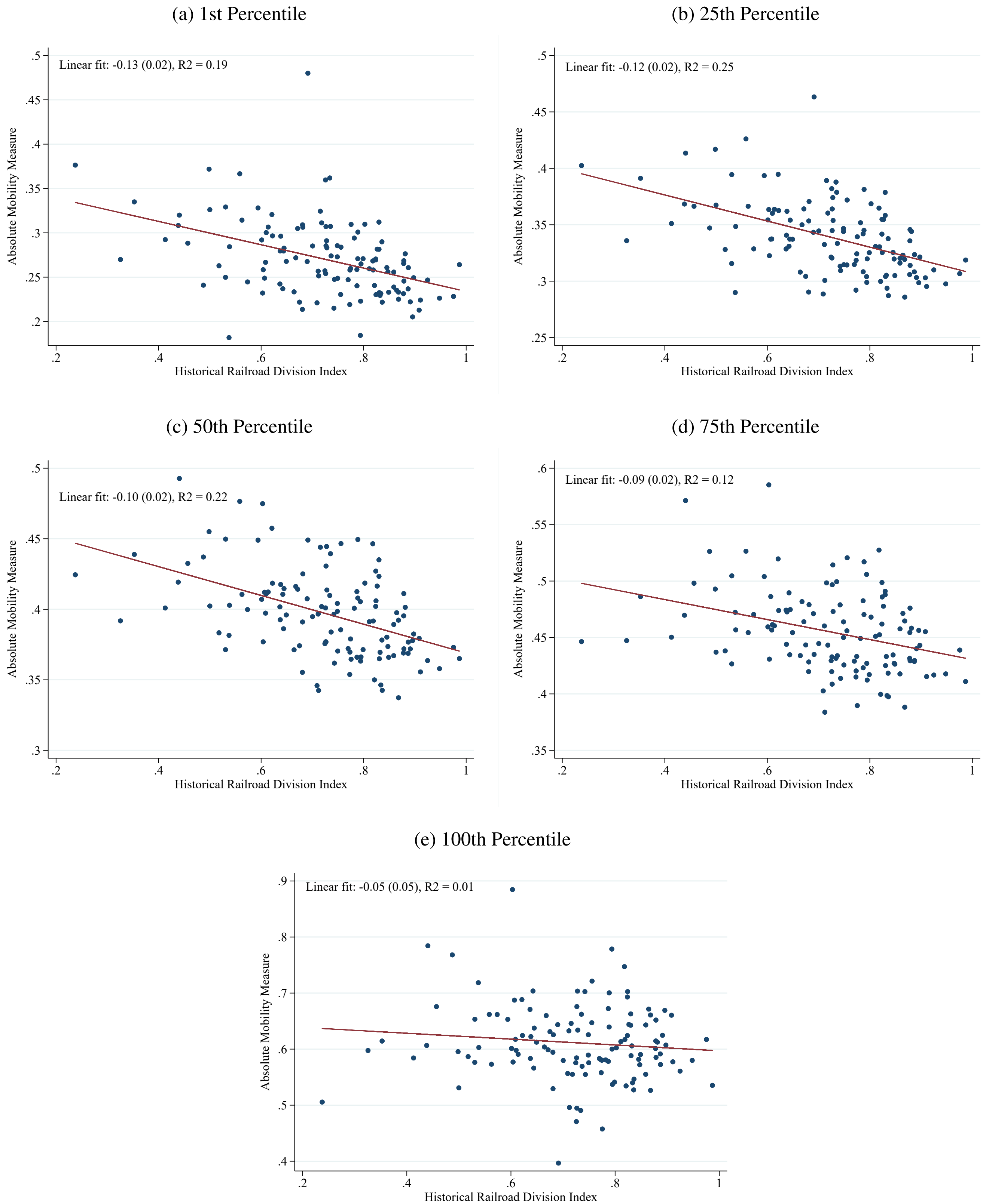
Notes: This table reports point estimates and heteroskedasticity robust standard errors (in parentheses) from models in which the key independent variable is the racial dissimilarity index in 1990. All measures are constructed using responses to the CCES, as detailed in Appendix C. Racial Resentment A reflects agreement with the statement "The Irish, Italian, Jews and many other minorities overcame prejudice and worked their way up. Blacks should do the same." Racial Resentment B reflects disagreement with "Generations of slavery and discrimination have created conditions that make it difficult for Blacks to work their way out of the lower class." Index components are z-scores, and the summary indices are equal to the average of their respective components. Statistical significance is denoted by: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

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



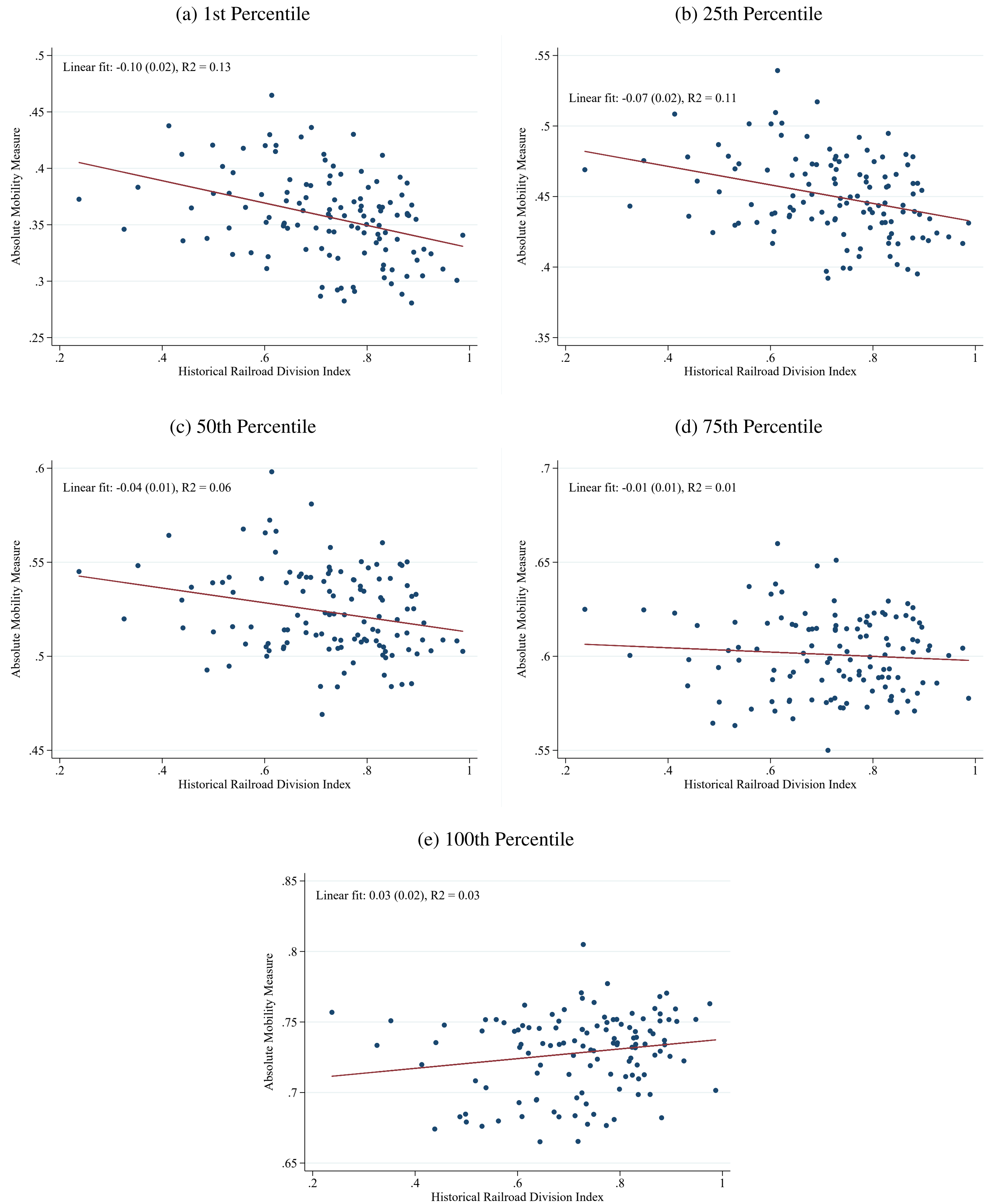
Notes: Figure displays the relationship between the racial dissimilarity index in 1990 and the railroad division index (RDI). Sample contains 121 non-Southern cities.
Source: Authors' calculations using data from Ananat (2011).

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



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



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*).