

DISCUSSION PAPER SERIES

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Opportunity**

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## ABSTRACT

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# The Great Migration and Educational Opportunity\*

This paper studies the impact of the First Great Migration on children. We use the complete count 1940 Census to estimate selection-corrected place effects on education for children of Black migrants. On average, Black children gained 0.8 years of schooling (12 percent) by moving from the South to North. Many counties that had the strongest positive impacts on children during the 1940s offer relatively poor opportunities for Black youth today. Opportunities for Black children were greater in places with more schooling investment, stronger labor market opportunities for Black adults, more social capital, and less crime.

**JEL Classification:** N32, J15, J24, H75

**Keywords:** Great Migration, human capital, education, place effect

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# 1 Introduction

The twentieth century migration of Southern-born African Americans—the Great Migration—was a landmark event in American history. Seeking better economic and social opportunities for themselves and their children, over seven million African Americans left the South between 1915 and 1970. While Black migrants earned substantially more than their counterparts who remained in the South (Collins and Wanamaker, 2014; Boustan, 2017), they also died earlier (Black et al., 2015) and faced higher incarceration rates (Eriksson, 2019).

In contrast to the increasing evidence on the impacts of the Migration on adults, there is less research on the consequences for children. Important work by Derenoncourt (2022) finds that Northern cities which received more Black migrants between 1940 and 1970 had lower rates of upward mobility for African American children born in the 1980s. This reduction in mobility appears to stem from changes in local public goods and neighborhood quality. Tabellini (2019) finds that the arrival of Black migrants between 1915 and 1930 led to reductions in public expenditures. These results, along with evidence from Boustan (2010) and Shertzer and Walsh (2019) showing that White individuals left cities and neighborhoods which received more Black migrants, raise the question of whether the Migration *ever* yielded meaningful benefits to children.

This paper provides new evidence on how moving North affected the children of African Americans who migrated during the first wave of the Great Migration (between 1915 and 1940). This focus complements work by Boustan (2010) and Derenoncourt (2022), who study postwar migration. The historical context provides several reasons why a Black child might have benefited from moving during this period. In the South, school quality was generally low, and there were fewer economic and social opportunities (e.g., Myrdal, 1944; Margo, 1990; Card and Krueger, 1992*a,b*; Card, Domnisoru and Taylor, 2022). Moreover, an emerging literature demonstrates that childhood residence exerts a powerful influence on long-run outcomes (Kling, Liebman and Katz, 2007; Gould, Lavy and Paserman, 2011; Chetty et al., 2014; Damm and Dustmann, 2014; Chetty, Hendren and Katz, 2016; Chetty and Hendren, 2018*a,b*; Chyn, 2018; Nakamura, Sigurdsson and

Steinsson, 2019; Chyn and Katz, 2021; Chyn, Collinson and Sandler, 2022). Nonetheless, the mixed impacts of migration for adults and the countervailing forces identified by previous work highlight the challenges that Black migrants faced when searching for better opportunity.

Our approach centers on estimating place-specific effects on child outcomes using full population records from the 1940 Census. We estimate place effects at the county level for all destinations chosen by Southern-born migrants. This allows us to compare the effects of moving North relative to staying in the South, which is key to assessing the impacts of the Great Migration on children.<sup>1</sup> Moreover, we use the county-level estimates to conduct a novel descriptive analysis of the mechanisms that drive place effects. Our analysis can distinguish mechanisms more clearly than prior work, most of which focuses on broad North-South comparisons.

The 1940 Census records are ideal for our analysis for three reasons. First, our key outcome of interest is educational attainment, which was first recorded by the Census in 1940. Second, these records provide a sufficiently large sample to study migration to over 720 destinations. Third, since most children completed their education before leaving home in 1940 (Card, Domnisoru and Taylor, 2022), we are able to observe children’s educational attainment and the characteristics of migrant parents.

To estimate impacts, we follow recent studies of place effects by comparing outcomes for movers. Specifically, we obtain selection-corrected estimates by using the two-step methodology introduced in Finkelstein, Gentzkow and Williams (2021). In the first step, we examine differences in education for children between ages 14 and 18 whose migrant parents moved to different destinations, controlling for the household head’s origin state and observable characteristics of children and families. The second step addresses remaining selection on unobserved household characteristics by implementing an adjustment to our comparisons based on the correlation between migrant destination choices and observables. Intuitively, the idea is to compare children in migrant households from the same origin state that moved to different destinations. To the extent that children in certain destinations obtain higher schooling than elsewhere (e.g., moving to Pittsburgh, where

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<sup>1</sup>We follow other studies of the Great Migration by referring to the non-South as the “North” for convenience.

children had relatively high levels of achievement, rather than Baltimore, where children had lower achievement), this suggests the presence of causal place effects. However, the methodology also asks whether parents who moved to better areas were more educated (or otherwise advantaged) than parents who moved to worse areas, and this information is used to adjust for selection on unobserved components. This approach builds on the influential methodologies from Altonji, Elder and Taber (2005) and Oster (2019).

We find that moving to the North during the first wave of the Great Migration had substantial positive impacts on educational outcomes of children. Moving to the average Northern destination rather than the average Southern destination increased educational attainment by 0.8 years, which is 12 percent of average educational attainment in our sample (6.8 years). This effect is 24 percent of the nationwide Black-White educational gap in 1940 (3.4 years), and 43 percent of the total Black-White convergence in educational attainment between the 1922 and 1926 birth cohorts (0.4 years). In terms of place effects, 84 of the best 100 counties are in the North, while 96 of the worst 100 counties are in the South. Notably, we also provide evidence that the selection correction reduces omitted variable bias that standard approaches fail to capture. Adjusting for selection on unobservables reduces the estimated effect of moving North by 39 percent.

Our results also reveal large variation in place effects *within* the North and South. While moving outside of the South is strongly associated with improvements in education, there are several Southern destinations that were beneficial to children. For example, Jefferson County, Alabama (home of Birmingham) led to 0.5 additional years of schooling on average, compared to the average destination chosen by Black migrants. In contrast, the county containing New Orleans led to 0.3 fewer years of schooling on average. Consequently, the Birmingham-New Orleans difference is about the same as the average North-South difference. As a summary statistic, we focus on areas at the 90th and 10th percentile in the North and South. We find that the 90-10 gap is 1.2 years in the North and 1.6 years in the South. These gaps respectively equal 18 and 24 percent of average schooling in our sample.<sup>2</sup>

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<sup>2</sup>We also find substantial heterogeneity in place effects in two additional dimensions. First, we find that urban areas had more beneficial place effects relative to rural locations. Second, our analysis shows that place effects vary

We conduct several robustness tests that demonstrate our main results are not sensitive to changes in model specification, identifying assumptions, or the definition of the sample. First, we show that our results are nearly identical when using different sets of characteristics observed in 1940 to adjust for selection on unobservables. This evidence indicates that our results are not compromised by selection on dimensions that are correlated with variables measured in the 1940 Census. However, one concern is that the 1940 Census has a limited set of household covariates, so we address this limitation by matching fathers from 1940 to the complete count 1920 Census. Again, our place effect estimates are very similar when we add a battery of covariates for fathers and grandfathers in 1920 or include fixed effects for fathers' county of origin. Second, we also show that our conclusions do not change when we use relaxed versions of the identifying assumptions imposed in the selection correction. One natural hypothesis is that there is more selection on parents' human capital than on children's schooling capital. When we allow for this possibility, we find that the North-South difference and the cross-area variance of place effects is slightly larger than in our main approach. More generally, we show that our main findings are very similar across a range of potential violations of our identifying assumptions. Third, we show that our results are robust when modifying our main sample, which contains 14–18 year old children living with a parent. Our results are very similar when including children living with any relative (which covers 91 percent of children), when focusing on 14–16 year olds, and when measuring eighth grade attainment as the main outcome. This evidence indicates that our results are not driven by sample selection or censored outcomes.

To shed light on mechanisms, we study correlates of 1940 place effects at the county-level by compiling data on a range of historical measures of local area characteristics. We find that place effects were considerably larger in areas where school quality was higher, Black adults had better labor market opportunities, and homicide rates were lower. Migrant children also had better educational outcomes in areas with National Association for the Advancement of Colored People (NAACP) chapters, which we interpret as a proxy for stronger social capital. The importance of

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by race. We estimate place effects for children of Southern-born White migrants and compare these to the estimates for Black children. The correlation between Black and White place effects is 0.26.

these factors is also apparent in multivariate regressions.

In the final component of our analysis, we compare our historical measures of place effects with more recent estimates for children born in the 1980s. Many of the places with the largest positive place effects in 1940 offer relatively limited opportunities for Black children today. For example, we estimate substantial benefits in 1940 for children who move to the counties that contain Chicago, Detroit, Cleveland, and St. Louis. Chetty et al. (2020) use contemporary data and show that children in several of these locations tend to have relatively poor outcomes. Overall, the correlation between our 1940 place effect estimates and contemporary measures of county-level opportunity is 0.20.<sup>3</sup>

To understand these changes in Black opportunity over time, we conclude with a descriptive analysis that focuses on the changes in local area characteristics. Echoing the results of our cross-sectional exploration of mechanisms, we find that place effects grew in the latter half of the twentieth century in counties with greater investment in school quality and stronger growth in Black family income. Increases in homicide and incarceration rates are associated with reductions in place effects. Notably, these factors play an important role even when holding the other factors constant (e.g., there is an independent role of incarceration, conditional on the homicide rate).

Overall, this paper has three main contributions. First, we provide new evidence on how the Great Migration affected children's opportunities—one of the driving forces behind the Migration that has received relatively little attention. Our work complements papers studying impacts of the Great Migration on adults and cities (e.g., Black et al., 2015; Boustan, 2010, 2017; Calderon, Fouka and Tabellini, 2019; Collins and Wanamaker, 2014, 2015; Eriksson, 2019; Shertzer and Walsh, 2019; Stuart and Taylor, 2021*a,b*; Tabellini, 2019; Shi et al., 2021). Our analysis is also closely related to Derenoncourt (2022), which finds that the second wave of the Great Migration had negative long-run impacts on economic opportunity for Black children born in Northern cities during the 1980s. We show that the children of Black migrants who moved North during the first

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<sup>3</sup>For contemporary measures of county-level opportunity, we primarily rely on estimates of Black upward mobility from Chetty et al. (2020). Upward mobility is defined as the mean household income rank for children whose parents were at the 25th percentile of the national income distribution. Chetty et al. (2020) construct this measure for children born between 1978 and 1983 who grew up during the 1980s and 1990s.



wave of the Great Migration benefited substantially, despite the challenges that African American migrants faced.

Second, we contribute to the emerging literature on place effects. Recent work examines how child outcomes vary across areas in the U.S. using data on children born in the 1980s (Chetty et al., 2014; Chetty, Hendren and Katz, 2016; Chetty and Hendren, 2018*a,b*; Chetty et al., 2020). Our work is a historical counterpart to this literature. We provide evidence that place effects changed notably during the the twentieth century and document the changes in economic, social, and demographic characteristics that accompanied these changes in opportunity. Our results underscore the possibility of improving opportunities for African American children via economic growth, additional investments in schools, and improvements in public safety.

Third, our work is broadly related to research on the educational progress of African Americans. Prior studies have highlighted the importance of improvements in school quality in shaping Black economic opportunity (Smith and Welch, 1989; Margo, 1990; Card and Krueger, 1992*a*; Aaronson and Mazumder, 2011; Bayer and Charles, 2018; Card, Domnisoru and Taylor, 2022). Within this literature, our work is most closely related to Card, Domnisoru and Taylor (2022), which studies the intergenerational transmission of education in 1940 for Black children and uses a state border research design to estimate the impact of school quality in the South. Relative to this work, our contributions are new evidence that the Great Migration substantially increased educational attainment of African American children and new estimates of the effects of local area schools based on an analysis of migrants.

## **2 Historical Background**

Economic and social opportunities for African Americans varied widely across the U.S. in the early twentieth century. Comparisons of the South and non-South (for simplicity, we refer to this as the North) reveal the most salient differences. For example, Table 1 shows that median Black household income in 1940 was \$341 in the South (equal to \$6,329 in 2019 dollars) and 70 percent higher in the North (\$578, or \$10,728 in 2019 dollars). Other indicators also showed striking

differences. The poverty rate was 50 percent higher in the South, and the homicide rate was almost three times as large.

These differences in economic and social opportunities provided incentives for millions of African Americans to migrate from the South to the North. About 1.5 million Black migrants moved between 1910 and 1940 during the first wave of the Great Migration. An additional 4.5 million moved during the second wave, from 1940 to 1970 (U.S. Bureau of the Census, 1979, Table 8). A key motivation for these migrants was better labor market opportunities (Scott, 1920; Henri, 1975; Gottlieb, 1987; Grossman, 1989; Marks, 1989; Gregory, 2005; Wilkerson, 2010). Manufacturing employment, which opened to Black workers with the onset of World War I, was an especially attractive pull factor, while declining opportunities in agriculture pushed migrants out of the South (Boustan, 2010). Many migrants left the South by train, especially during the first wave of the Great Migration (Black et al., 2015).

Nearly all accounts of this period suggest that Black individuals perceived that opportunities in the North were better than in the South (Scott, 1920; Rubin, 1960; Gottlieb, 1987; Grossman, 1989). In some cases, Black migrants learned about specific job opportunities from friends or family that had already moved to the North (Scott, 1920; Rubin, 1960; Gottlieb, 1987; Stuart and Taylor, 2021a). Migrants' information also came from labor agents—who offered paid transportation, employment, and housing—or newspapers from the largest cities, like Chicago and Pittsburgh (Gottlieb, 1987; Grossman, 1989). However, many Black individuals wrote to Northern newspapers with basic questions about the availability of jobs and the climate, which suggests that this type of specific information was somewhat rare (Gottlieb, 1987; Grossman, 1989).

What were the consequences of this migration? Previous research points to both positive and negative impacts on African Americans. Previous evidence suggests adults who moved North experienced an 80 to 130 percent increase in their earnings (Collins and Wanamaker, 2014; Boustan, 2017). However, they also faced a higher probability of incarceration (Eriksson, 2019) and a reduction in life expectancy (Black et al., 2015), with the latter driven partly by increased smoking and drinking.

While several papers examine adult outcomes and the Great Migration, the effects for children are relatively understudied.<sup>4</sup> That said, theory and several stylized facts provide suggestive evidence. In addition to higher parental income, access to better schools provides reason to expect that migration may have enhanced child development. The school quality channel is particularly salient given the large variation in educational opportunities between the South and North. All Southern schools were segregated in 1940, and Black schools received much less funding (Margo, 1990). A comparison of Black schools in the South to all schools in the North reveals that the average teacher-pupil ratio was 28 percent higher in the North (see Table 1).<sup>5</sup> Term length, teacher salaries, and other schooling inputs also varied along these lines.<sup>6</sup>

Yet, any positive effects of migrating North on family income and school quality may have been offset by other factors. Residential segregation in Northern cities reduced the quality of neighborhoods and homes available to African Americans, and additional migrants tended to exacerbate the negative consequences of segregation through crowding (Scott, 1920; Myrdal, 1944; Henri, 1975). For example, segregation led to unhealthy conditions in overcrowded housing (Scott, 1920; Myrdal, 1944; Henri, 1975), long distance moves could have been particularly disruptive, and better labor market opportunities would have increased the opportunity cost of investing in children's human capital. In addition, White residents in the North responded to the arrival of African Americans with violence and hostility, leading to adverse impacts on children (Boustan, 2010; Shertzer and Walsh, 2019; Tabellini, 2019; Derenoncourt, 2022).

The consequences of intraregional migration during our time period for Black children are also an open question. Again, the historical context of our study suggests a plausibly important

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<sup>4</sup>Alexander et al. (2017) present descriptive evidence on overall differences in outcomes of children of migrants and non-migrants. They caution against a causal interpretation of their results due to concern over potential omitted variable bias. A main contribution of the current paper stems from the use of an empirical strategy that addresses selection on observed and unobserved factors. In addition, we differ from their work by studying county-specific place effects.

<sup>5</sup>The comparisons in Table 1 focus on the counties in which migrant parents in our sample (described below) resided in 1940. The lynching rate in Table 1 is higher in the North than the South because the only Northern state for which lynching data from Bailey et al. (2008) are available is Kentucky. Our finding that incarceration rates for Black individuals were higher in the North is consistent with Eriksson (2019).

<sup>6</sup>The differences in Table 1 likely overstate the improvement in school resources available to Black migrants, because residential segregation led Black students to attend worse schools than their White peers (Myrdal, 1944). Data on the specific schools attended by Black children in the North are not available.

role for place effects within regions, as there were sizable differences *within* the South and North. In the North, median Black household income in 1940 was \$260 at the 10th percentile of the county-level distribution, while it was \$850 at the 90th percentile. In the South, the 10th and 90th percentiles were \$260 and \$520. These differences are comparable to the average North-South difference.<sup>7</sup> There was also large intraregional variation in educational attainment and schooling inputs, especially in the South.

### 3 Empirical Strategy and Data

#### 3.1 Econometric Model

Our goal is estimate the causal impact of each county on Black children’s educational attainment as of 1940.<sup>8</sup> To achieve this objective, we estimate a flexible model of place effects, based on the approach of Finkelstein, Gentzkow and Williams (2021). We assume the following model for years of education ( $Y_i$ ) of individual  $i$  if they live in location  $j$ :<sup>9</sup>

$$Y_i = \gamma_j + \theta_i. \tag{1}$$

The parameter of interest in equation (1) is the *place effect*  $\gamma_j$ . This term captures all channels by which location affects schooling. For example, a given place effect might be positive due to the availability of better employment opportunities for parents or higher funding for public schools. For estimation, we normalize place effects so that the migrant-weighted average equals zero.

The remaining determinants of schooling of an individual are captured in  $\theta_i$ , which we refer to as *schooling capital*. We assume that schooling capital can be decomposed into demographics  $X_i$ , household characteristics  $H_i$ , unobserved factors that are correlated with parent origin ( $o$ ) and

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<sup>7</sup>For the entire U.S., the 10th and 90th percentiles were \$260 and \$750.

<sup>8</sup>We focus on counties as the unit of geography because some potential mechanisms are particularly local—such as schools and neighborhoods—while others are somewhat broader—such as labor market opportunities. By examining county of residence, our place effects will reflect the labor market opportunities available via commuting.

<sup>9</sup>While our main analysis focuses on years of education, Section 4.7 shows that we obtain similar results when we use binary measures of seventh grade, eighth grade, ninth grade, and tenth grade attainment as outcomes.

destination ( $j$ ) locations, and an orthogonal residual:

$$\theta_i = X_i\psi + H_i\lambda + \eta_o^{\text{orig}} + \eta_j^{\text{dest}} + \eta_j^{\text{nm}} + \tilde{\eta}_i. \quad (2)$$

The terms  $\eta_o^{\text{orig}}$ ,  $\eta_j^{\text{dest}}$ , and  $\eta_j^{\text{nm}}$  are fixed effects for migrant parents' origin location, migrant parents' destination location, and non-migrant parents' place of residence. The fixed effect  $\eta_o^{\text{orig}}$  measures whether the unobserved average schooling capital of children of migrants differs across origin locations (net of the other variables in the model), while the fixed effect  $\eta_j^{\text{dest}}$  measures whether the unobserved average schooling capital of children of migrants differs across destination locations. We assume  $\eta_j^{\text{nm}} = 0$  for migrants and  $\eta_o^{\text{orig}} = \eta_j^{\text{dest}} = 0$  for non-migrants. The residual  $\tilde{\eta}_i$  is orthogonal to the other variables in equation (2) by construction.

A key assumption in this model is the additive separability of place effects and schooling capital in equation (1). This assumption is standard in the literature that estimates place effects using individuals who move to different destinations (Chetty and Hendren, 2018a,b; Finkelstein, Gentzkow and Williams, 2021). The assumption implies that there is no interaction between individual attributes and the effects of location on child outcomes.<sup>10</sup>

### 3.2 Estimation and Identification

We seek to estimate the place effects  $\gamma_j$  from equation (1). Combining equations (1) and (2) yields the main specification that we estimate:

$$Y_i = X_i\psi + H_i\lambda + \tau_o^{\text{orig}} + \tau_j^{\text{dest}} + \tau_j^{\text{nm}} + \tilde{\eta}_i, \quad (3)$$

where  $\tau_o^{\text{orig}}$ ,  $\tau_j^{\text{dest}}$ , and  $\tau_j^{\text{nm}}$  are fixed effects for migrant parents' origin location, migrant parents' destination location, and non-migrant parents' place of residence, respectively. Note that our framework implies  $\tau_o^{\text{orig}} = \eta_o^{\text{orig}}$ ,  $\tau_j^{\text{dest}} = \gamma_j + \eta_j^{\text{dest}}$ , and  $\tau_j^{\text{nm}} = \gamma_j + \eta_j^{\text{nm}}$ .

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<sup>10</sup>We have also estimated models where the dependent variable is the log of years of schooling. These models allow place effects to be proportional to individuals' schooling capital. The results are very similar (see Appendix Figure 1), which suggests that the additive separability assumption does not severely influence our results.

The key challenge in estimating equation (3) is identification of place effects  $\gamma_j$ . Simple comparisons of child outcomes across destinations will not recover place effects if the average schooling capital of children also varies across places. One assumption that would be sufficient for identification is that all differences across locations are due to  $X_i$  and  $H_i$ . In this case, we would have  $\eta_o^{\text{orig}} = \eta_j^{\text{dest}} = \eta_j^{\text{nm}} = 0$ , and so  $\gamma_j$  could be identified directly from estimates of  $\tau_j^{\text{dest}}$  in equation (3). A more plausible assumption is that differences in schooling capital are captured by the combination of  $X_i$ ,  $H_i$ , and the origin fixed effect  $\tau_o^{\text{orig}}$ . This assumption would follow from a model in which the birth place of migrant parents may be related to both child schooling outcomes and destination choice but destination choice is otherwise independent of the unobserved components of schooling capital. That said, this assumption of conditional independence is still relatively strong.

To address the possibility that migrant parent destinations are correlated with unobserved components of child schooling capital, we use selection on observed variables to adjust for selection on unobserved variables. We introduce additional notation to describe this approach. Let  $T_{ij} \equiv 1\{j(i) = j\}$  be an indicator for whether person  $i$  lives in location  $j$ . In addition, define  $h_i = H_i\lambda$  as the index of *observed* schooling capital. This index captures how household characteristics  $H_i$  are related to a child's years of schooling and depends on the parameter vector  $\lambda$  in equation (3). Finally, consider the following auxiliary regression in the sample of migrant children:

$$h_i = X_i\psi^h + h_o^{\text{orig}} + h_j^{\text{dest}} + \tilde{h}_i. \quad (4)$$

The explanatory variables are demographics  $X_i$ , plus fixed effects for migrant parents' origin location ( $h_o^{\text{orig}}$ ) and destination location ( $h_j^{\text{dest}}$ ). The fixed effect  $h_j^{\text{dest}}$  describes whether the index of observed schooling capital differs across destinations. Consequently,  $h_j^{\text{dest}}$  is the counterpart to  $\eta_j^{\text{dest}}$ , where the former reflects differences across destinations in observed schooling capital, while the latter reflects differences in unobserved schooling capital. Equation (4) can be estimated using OLS after constructing an estimate of the index of observed schooling capital. We construct an

estimate of this index as  $\hat{h}_i = H_i \hat{\lambda}$ , where  $\hat{\lambda}$  comes from OLS estimation of equation (3). We provide details on the variables in  $H_i$  below.

With this notation, we can now introduce the two key assumptions required for our selection correction approach. The first assumption says that there is equal selection on unobserved and observed components of schooling capital. The extent of selection is measured by the correlation between individuals' location and the components of schooling capital. Formally, the assumption is:

**Assumption 1** (*Equal selection*)  $Corr(T_{ij}, \eta_j^{dest}) = Corr(T_{ij}, h_j^{dest})$  in the sample of migrants for all  $j$ .

The second assumption says that the importance of unobserved schooling capital relative to observed schooling capital is the same in destinations and origins:

**Assumption 2** (*Relative importance*)  $\frac{SD(\eta_j^{dest})}{SD(h_j^{dest})} = \frac{SD(\eta_o^{orig})}{SD(h_o^{orig})}$  in the sample of migrants.

This assumption allows us to pin down the amount of selection on unobserved schooling capital,  $SD(\eta_j^{dest})$ , using the relative standard deviation of origin fixed effects and the standard deviation of observed schooling capital destination fixed effects, which can be estimated from equations (3) and (4).

Finkelstein, Gentzkow and Williams (2021) show that Assumptions 1 and 2 yield a consistent estimate of the confounding variable  $\eta_j^{dest}$ :

$$\hat{\eta}_j^{dest} = \frac{\widehat{SD}(\hat{\tau}_o^{orig})}{\widehat{SD}(\hat{h}_o^{orig})} \hat{h}_j^{dest}, \quad (5)$$

where we use the fact that  $\hat{\tau}_o^{orig} = \hat{\eta}_o^{orig}$ . We can then construct the place effect as  $\hat{\gamma}_j = \hat{\tau}_j^{dest} - \hat{\eta}_j^{dest}$ , since  $\hat{\tau}_j^{dest}$  is estimated consistently from equation (3).<sup>11,12</sup>

<sup>11</sup>Appendix A follows Finkelstein, Gentzkow and Williams (2021) and derives equation (5) formally.

<sup>12</sup>To summarize, the estimation procedure is as follows. We first estimate equation (3), which yields estimates of the fixed effects  $\hat{\tau}_o^{orig}$  and  $\hat{\tau}_j^{dest}$ , along with the vector  $\hat{\lambda}$ . We then construct  $\hat{h}_i \equiv H_i \hat{\lambda}$  and estimate equation (4), which yields estimates of the fixed effects  $\hat{h}_o^{orig}$  and  $\hat{h}_j^{dest}$ . Given the estimates of the fixed effects, we can estimate the standard deviations  $\widehat{SD}(\hat{\tau}_o^{orig})$  and  $\widehat{SD}(\hat{h}_o^{orig})$ . Finally, we estimate  $\hat{\eta}_j^{dest}$  using equation (5).

The key distinction between  $X_i$  and  $H_i$  in this model is that variables in  $H_i$  help identify selection on unobserved factors. As a result, variables that might be related to children’s educational attainment and their location belong in  $H_i$ . Our baseline specification of  $H_i$  contains separate indicators for father’s and mother’s years of schooling. Parental education is likely to be the most important observed factor related to children’s attainment (e.g., Black, Devereux and Salvanes, 2005; Card, Domnisoru and Taylor, 2022) and migrants’ location choice. In Section 4.5, we show that our results are nearly identical when we add several other variables to  $H_i$ : indicators for whether only the father is present, whether only the mother is present, whether both parents are born in a different state, whether one parent is born in a different state, indicators for parents’ age in five-year intervals, and the number of children in the household.<sup>13</sup> Given these choices, we include a limited set of variables in  $X_i$ : indicators for sex and age.

Equation (5) demonstrates how this approach uses selection on observables—both in terms of the ratio of standard deviations of origin effects and the amount of selection on observed schooling capital,  $h_j^{\text{dest}}$ —to adjust for the *remaining* selection on unobserved schooling capital,  $\eta_j^{\text{dest}}$ . To understand the intuition of this approach, consider a model in which parents choose a destination while considering the payoffs to themselves and their children, with locations differing in the earnings received by parents and the educational benefits received by children. The selection correction in equation (5) relies on locations that attract more educated parents ( $h_j^{\text{dest}} > 0$ ) also attracting children with higher amounts of unobserved schooling capital ( $\eta_j^{\text{dest}} > 0$ ).<sup>14</sup> If, contrary to Assumption 1, locations that attract more educated parents attract children with lower unobserved schooling capital, then the estimate of the confounding variable  $\eta_j^{\text{dest}}$  would have the wrong sign. In Appendix B, we describe a stylized model that generates selective migration and discuss how the selection correction approach adjusts basic patterns in the data to estimate place effects.

How strong are the selection correction assumptions in our setting? The historical context suggests that the assumption that selection on observables takes the same direction as selection on

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<sup>13</sup>These variables are similar to those included in Card, Domnisoru and Taylor (2022).

<sup>14</sup>Because the destination fixed effects are normalized to have migrant-weighted averages of zero, a positive value of  $h_j^{\text{dest}}$  or  $\eta_j^{\text{dest}}$  implies that such a destination attracts children with above-average levels of observed or unobserved schooling capital.



unobservables is plausible. As noted in Section 2, previous research indicates that migrants understood that labor market and educational opportunities were better in the North (e.g., Grossman, 1989; Gregory, 2005). Moreover, more educated adults were more likely to move to the North (as we discuss in Section 3.5 below), and the children of these adults likely had higher unobserved human capital (either because of “nature” or “nurture” channels).

Although our setting provides some guidance on the nature of selection on unobservables, it is worth discussing how we address two remaining concerns surrounding the assumptions in our approach. A first issue is whether equation (5) pins down the correct magnitude of selection. In Section 4.6, we address this concern by showing that our results are robust when varying the degree of selection assumed in the selection correction model. Second, any given place effect might be biased in finite samples. When reporting individual place effects in figures or tables, we follow Chetty and Hendren (2018*b*) and Finkelstein, Gentzkow and Williams (2021) in using an empirical Bayes procedure to shrink estimates to the mean (which is zero), with greater shrinkage for less precise estimates. Appendix C provides details. We construct standard errors of place effects and cross-county variances of place effects using a Bayesian bootstrap (Rubin, 1981), as in Finkelstein, Gentzkow and Williams (2021).

### **3.3 Estimating the Effect of Moving North**

The results obtained from the approach in Section 3.2 allow us to undertake two exercises. First, we use the county-level estimates to examine the distribution of place effects and assess potential mechanisms. Second, we use the estimates to study the overall effect of the Great Migration on children’s educational achievement.

For this second exercise, we estimate the effect of moving North by computing the migrant-weighted difference between Northern and Southern county-level place effects. That is, we use estimates of place effects ( $\hat{\gamma}_j$ ) and information on observed location choices to construct the fol-

lowing estimate:

$$\hat{\Delta}^{N-S} = \sum_{j \in N} \frac{\hat{p}_j}{\hat{p}^N} \hat{\gamma}_j - \sum_{j \in S} \frac{\hat{p}_j}{\hat{p}^S} \hat{\gamma}_j, \quad (6)$$

where  $\hat{p}_j$  is the share of migrants that live in location  $j$ ,  $\hat{p}^N \equiv \sum_{j \in N} \hat{p}_j$  is the share of migrants that live in the North ( $N$ ), and  $\hat{p}^S$  is the share of migrants that live in the South ( $S$ ). The estimate  $\hat{\Delta}^{N-S}$  can be interpreted as comparing the place effect in migrants' average location chosen in the North to the average location chosen in the South.

How does this estimate relate to previous approaches used in the literature on the Great Migration? Prior studies have focused on adult migrants and used design-based approaches to estimate the overall effect of moving North on earnings, health, and incarceration (Collins and Wanamaker, 2014; Black et al., 2015; Boustan, 2017; Eriksson, 2019). These studies estimate impacts using regressions of the form:

$$Y_i = \mu_0 + \mu_1 M_i + X_i \mu_2 + u_i, \quad (7)$$

where  $Y_i$  measures an outcome in adulthood (such as earnings),  $M_i$  is an indicator for residing in the North, and  $X_i$  is a vector of controls to adjust for selection into migration. The most stringent specifications use matched Census data to include pre-migration household fixed effects, ensuring that identification comes from comparisons of siblings who vary in migration decisions. The term  $\mu_1$  is the key parameter of interest in this regression, which is identified by comparing migrants and non-migrants born in the South.

When destinations are exogenous, it is straightforward to show that  $\hat{\Delta}^{N-S}$  in equation (6) converges to  $\mu_1$  in equation (7). If migration decisions are endogenous, these two approaches might recover different estimates of the impact of moving North. Our analysis relies on equation (6), where the estimates of place effects are generated from a model that controls for observables and adjusts for selection on unobservables. In comparison, equation (7) controls for observables.

In Section 4.2 and Appendix E, we provide a detailed comparison of the estimated impact of

moving North obtained from equations (6) and (7). To preview our results for children, we find that adjusting for unobservables notably lowers the magnitude of the estimated benefits of migration.

### 3.4 Data, Samples, and Main Outcome

Our main analysis uses the complete count file from the 1940 Census (Ruggles et al., 2020). The 1940 Census was the first to measure educational attainment, which is our key outcome of interest. The 1940 Census also contains information on demographics and household structure, which we use to construct the variables in  $X_i$  and  $H_i$ .

We use two main sample restrictions to construct a sample of African Americans ages 14 to 18 in 1940. First, we require that children in our sample live with at least one of their parents. Focusing on children living with a parent allows us to determine parents' birthplace and control for other parent and household characteristics. This restriction does not seriously affect the sample composition since most children in 1940 lived with their parents and completed their schooling while living with their parents.<sup>15</sup> Overall, 80 percent of Black children ages 14–18 lived with at least one parent in the 1940 Census.<sup>16</sup> Section 4.7 provides additional tests to assess how the coresidency requirement affects our results.

Second, we also require that parents were between ages 25 and 70 in 1940 and born in the United States. Our sample contains children whose household head is a migrant—someone born in one of the former Confederate States, which we refer to as the South, and living outside their birth state in 1940—and non-migrants—who reside in their birth state and may live in the South or North.<sup>17</sup> The inclusion of non-migrants helps identify  $\psi$  and  $\lambda$  in equation (3). We estimate place effects at the county level and use the head of household's birth state for origin effects.

Overall, the sample contains 650,040 children, and 33 percent (213,751) are children of mi-

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<sup>15</sup>Card, Domnisoru and Taylor (2022) use a similar sample restriction in their study of intergenerational mobility in education using the 1940 Census.

<sup>16</sup>Patterns of coresidency were similar in the North and South. For example, the fractions of children in the North and South that lived with a parent were 0.81 and 0.79, respectively.

<sup>17</sup>We drop the 4 percent of children whose household head was born in the North and lived outside their state of birth in 1940, as these individuals made quite different moves from our sample of interest.

grants. These migrant children lived in 728 destination counties in 1940.<sup>18</sup> While the 1940 Census does not contain detailed information on the timing of migration, we construct a back-of-the-envelope calculation on the duration of residence in Northern destinations by studying the share of migrant children that are living in the North in the 1930 and 1940 Censuses.<sup>19</sup> We estimate that children of migrants to the North who were age 14–18 in 1940 had been living there for a substantial period of time—at least 9.4 years on average.<sup>20</sup> Moreover, the vast majority of individuals in our sample lived in the same county in 1935 and 1940: 89.7 percent of the entire sample and 88.4 percent of children of migrants.

In addition, we construct a supplemental sample by matching Black men in the complete count 1920 and 1940 Censuses.<sup>21</sup> We match individuals based on first and last name, birth state, age, and race, using the algorithm of Abramitzky et al. (2021a). We restrict the matched sample to individuals who are uniquely matched from 1920 to 1940 and 1940 to 1920. For matched Black men, we identify children in their 1940 household. We focus on 27,258 children of matched fathers who are residing in counties with at least 10 children of matched-sample migrants, originating from counties with at least 10 migrant children and 5 non-migrant children.<sup>22</sup> This sample contains 13,896 children of migrants residing in 211 destination counties. The disadvantage of the matched sample is the smaller number of observations. However, the matched sample provides characteristics of fathers and grandfathers in 1920, along with their county of residence in that year, which facilitates

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<sup>18</sup>To increase the reliability of our place effect estimates, we limit the sample to individuals residing in counties with at least 25 migrant children.

<sup>19</sup>Prior work on place effects by Chetty and Hendren (2018a) estimates models of exposure effects using IRS administrative records that provide detailed panel data on household location in every year. Our historical analysis is based on the 1940 Census, which does not provide such detailed information on locations over time.

<sup>20</sup>We compute this lower bound as follows, focusing on Black children who were born between 1922 and 1926 to a household head from the South. In the 1930 Census, 15 percent are living in the North. In the 1940 Census, the corresponding statistic is 16 percent. Setting aside return migration (which was low in this period), this implies that 94 percent of the 1922–1926 cohort who were in the North in 1940 had arrived by 1930. A first conservative assumption is that all individuals who arrived by 1930 arrived in 1930, implying that 94 percent had 10 years of exposure to the North by 1940. A second conservative assumption is that all individuals who arrived between 1930 and 1940 arrived in 1940, which yields an estimate of the average exposure of 9.4 years. Appendix Figure 2 reports the share of each cohort that is living in the North in the 1930 and 1940 Censuses.

<sup>21</sup>Appendix D provides full details on the construction of the matched sample.

<sup>22</sup>Relative to the full sample, we relax the migrant restriction to include more destination counties, and we impose the origin county restrictions to reliably estimate origin county fixed effects, which are used in equation (5).

additional robustness tests.<sup>23</sup>

The main outcome for our analysis is years of schooling, which we construct using information on the reported highest grade of school completed. Tabulations from the 1940 Census suggest that the vast majority of individuals in these cohorts completed their schooling by age 18—and that this pattern was similar in the North and South—which ameliorates concerns about whether our data measure completed years of education.<sup>24</sup> A complication is that some individuals attended ungraded schools during this time; in these cases, enumerators inferred grade attainment based on the number of years of school attended. Ungraded schools were far less common by the 1930s, so this type of measurement error is less of a concern for the children in our sample. However, this measurement error affects the measured education of parents in our sample (Margo, 1986). Our analysis likely avoids the most severe sources of measurement error because all migrant parents are African Americans from the South—implying that we avoid cross-race and cross-regional biases—and our matched sample robustness tests use origin county fixed effects—which adjust for the presence of ungraded schools. We also examine seventh, eighth, ninth, and tenth grade attainment as separate outcome variables.<sup>25</sup>

### **3.5 Patterns of Education and Migration**

Table 2 reports summary statistics for migrants and non-migrants in our sample. On average, children of parents who lived in their Southern birth state have 6.1 years of schooling. Children of migrant parents who moved to another state in the South have 6.5 years of schooling, while children of parents who moved to the North have 8.4 years of schooling. This pattern is consistent

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<sup>23</sup>In particular, we measure fathers' literacy in 1920, school attendance, urban residence, farm residence, and number of siblings. For grandfathers, we measure literacy, Duncan socioeconomic index (based on occupation), and whether working as a farmer. If a grandfather is not present, we set the grandfather variables equal to zero and include an indicator for this outcome.

<sup>24</sup>In particular, a comparison of Black individuals observed in the 1940 Census shows that average years of schooling for 18-year-olds is 97 percent of the average years of schooling for 19-year-olds, who have the highest average level. In the North and the South, completed years of schooling by age 18 are 97 and 98 percent of the maximum. While these comparisons do not hold the cohort constant, we expect that cohort effects are similar across adjacent years.

<sup>25</sup>Following Card, Domnisoru and Taylor (2022), we treat an individual as having attained an eighth grade education if they have at least eight years of schooling or if they have at least seven years of schooling and are currently enrolled in school. Measures of attainment for other grade levels are analogous.

with a causal effect of the North on children’s education, but these patterns also appear for parents’ education, which raises the possibility of selection on unobservables.<sup>26</sup>

Figure 1 provides additional evidence on the scope of selection. Specifically, this analysis sheds light on whether migrant children with more favorable observed characteristics tend to live in destinations with better-educated non-migrants. We measure the favorability of migrant observables by computing the average index of observed schooling capital,  $\hat{h}_i = H_i \hat{\lambda}$ , for migrants that move to each county  $j$ , using parents’ education in  $H_i$ . The figure illustrates a binned scatterplot that shows how the observed index in county  $j$  is correlated with the average educational attainment of non-migrant children in the county. The slope coefficient of 0.21 implies that destinations with an extra year of non-migrant average educational attainment attracted migrants whose children are predicted to have an additional 0.21 years of schooling based on parental education.

The evidence of selection in Table 2 and Figure 1 motivates two features of our econometric approach. First, equation (3) controls directly for selection on observables. Second, we use estimates based on equation (5) to adjust for selection on unobservables. Before presenting our main results, we next discuss the inputs into our selection correction.

### 3.6 Inputs into Selection Correction

The adjustment for selection on unobserved variables depends on the standard deviation of origin fixed effects from equations (3) and (4). The top panel of Table 3 reports these statistics. The cross-origin standard deviation of observed schooling capital, 0.08, is essentially equal to the cross-origin standard deviation of unobserved schooling capital. The ratio of these two numbers, which is 1.01, is a key input into the selection correction in equation (5). Because observed and unobserved schooling capital display similar amounts of variation across origin locations, this implies a one-to-one relationship across destination locations between selection on observed variables,  $\hat{h}_j^{\text{dest}}$ , and unobserved variables,  $\hat{\eta}_j^{\text{dest}}$ .

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<sup>26</sup>Appendix Figure 3 displays educational attainment for children of migrants by their 1940 place of residence. The entire distribution of completed schooling is shifted to the right for those in the North, with the most notable differences between grades 8 and 11. Few individuals in the North or South have a 12th grade education or more.

The bottom panel of Table 3 reports the standard deviation of observed and unobserved schooling capital across destinations. Both observed and unobserved schooling capital vary much more across destinations than origins. This is partly mechanical, as we use destination counties but origin states in our main analysis. The selection correction procedure does not require using the same level of geography for origins and destinations, and we show in Section 4.5 that our estimates of place effects for the matched sample are very similar when using origin county instead of origin state fixed effects. The sizable variation across destinations in unobserved schooling capital underscores the potential for selection.

## **4 Estimates of Place Effects**

This section first reports our county-level estimates of place effects. Next, we report estimates of the overall effect of moving North. After presenting additional evidence on how place effects vary by urban-rural status and race, we demonstrate that our estimates and conclusions are robust to alternative ways of adjusting for selection on unobservables.

### **4.1 County-Level Place Effects**

To summarize the overall importance of place effects, Table 4 presents a variance decomposition of children's educational attainment into the component due to place effects and schooling capital. The equally-weighted standard deviation across counties is 1.4 years of schooling. The top panel of the table shows that, when not adjusting for selection on unobservables, the standard deviation of place effects is 1.1 years, which implies that place effects explain 56 percent ( $=1.074^2/1.429^2$ ) of the cross-county variation in Black children's schooling. The bottom panel presents our preferred, selection-corrected estimates. After adjusting for selection, place effects explain 35 percent of the cross-county variation. Schooling capital explains 50 percent, with the remaining 15 percent explained by the positive covariance between place effects and non-migrants' schooling capital. A positive covariance does not indicate a failure of the selection correction, but instead is consistent with the same factors increasing schooling of migrant and non-migrant children. Table 4 also

highlights the importance of adjusting for selection on unobservables: not doing so overstates the importance of place effects by 60 percent ( $= 1.074^2 / 0.848^2 - 1$ ).

Figure 2 shows the geographic distribution of empirical-Bayes-adjusted place effects, which are normalized so that the migrant-weighted average equals zero. There is large variation: the county at the 90th percentile leads to a 0.6-year increase in schooling relative to the average place, while the 10th percentile county leads to a 1.3-year decrease in schooling. As a result, the 90-10 gap is 1.9 years of schooling, equal to 28 percent of average schooling in our sample (6.8 years). The figure also shows that many of the best places for Black children are outside of the South.<sup>27,28</sup>

A natural question is how closely the selection-corrected place effects correspond to the outcomes of non-migrants. To examine this, Figure 3 plots place effects against average years of schooling for Black children of non-migrants. The slope coefficient of 0.45 implies that, when their family moved to a county with one year higher schooling attainment among non-migrant children, children of migrants gained an additional 0.45 years on average. This indicates substantial, but incomplete, convergence in outcomes for migrant children.<sup>29</sup> Moreover, simple comparisons of counties on the basis of non-migrants' educational attainment would overstate the benefits available to children from moving across counties. While the correlation is strong, there are notable discrepancies. For example, the place effect in Washington, DC is about 0.5 years below its predicted value, while the place effect in Jefferson, Alabama (largest city: Birmingham) is about 0.4 years above its predicted value. These cases point to meaningful differences in children's outcomes that are not driven by the range of factors that influence non-migrants' schooling.<sup>30</sup>

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<sup>27</sup>The 90-10 gap is 1.2 years in the North and 1.6 years in the South. These gaps equal 18 and 24 percent of average schooling in our sample.

<sup>28</sup>We also estimate place effects separately for girls and boys. These results are reported in Appendix Figure 4. Panel A shows that, overall, the two sets of place effects are highly correlated (correlation: 0.82), with a nearly one-to-one relationship (slope coefficient: 1.08). As seen in Panel B, place effects vary somewhat more for boys than girls, both across and within regions.

<sup>29</sup>Place effects for Black children also are higher in counties where White children of non-migrant parents have higher education, as shown in Appendix Figure 5.

<sup>30</sup>An additional question is whether place effects are correlated with Black migration flows. We find that there is a positive but relatively low correlation of 0.16 between place effects and the share of migrant children in each destination. A natural explanation for the relatively small correlation is that migrants considered a variety of factors in deciding where to live, including transportation costs and previous location decisions of family and friends. At the same time, migrants faced considerable barriers (including discrimination in labor and housing markets) and had limited information (especially within regions) about which places were better for their children. Appendix Figure 6



Table 5 summarizes place effects for the 20 largest counties in terms of 1940 Black population. Column 3 displays the place effects, and column 4 reports standardized place effects (with mean zero and standard deviation one).<sup>31</sup> In this set of counties, the largest place effects are for Kings, New York (largest city: Brooklyn), Harris, Texas (Houston), and Allegheny, Pennsylvania (Pittsburgh). These counties increased schooling by 0.6–0.8 years relative to the average county chosen by migrants (1.4–1.6 standard deviations). The worst place effects are for Caddo, Louisiana (Shreveport), Orleans, Louisiana (New Orleans), and Washington, DC, which reduced schooling by 0.1–0.4 years.

## 4.2 The Overall Effect of Moving North

A key motivation for our study is to estimate the overall effect of moving to the North based on the destinations chosen by Black migrants.<sup>32</sup> To explore this further, Figure 4 plots the equally-weighted density of estimated place effects for counties in the South and North. There is substantial overlap, but the Northern distribution has a higher mean and lower variance. Sixty-eight percent of destinations in the North have a positive place effect, compared to 13 percent of counties in the South.<sup>33</sup>

As shown Figure 4, the migrant-weighted average place effect is  $-0.52$  in the South and  $0.31$  in the North. This implies that the overall effect of moving to the North is a 0.83-year increase in schooling, which is equal to 12 percent of the mean in our sample of Black children ages 14–18 (6.8 years) and 24 percent of the nationwide Black-White educational gap in 1940 (3.4 years). The estimate also implies that moving to the North can account for 43 percent of the total Black-White convergence in educational attainment between the 1922 and 1926 birth cohorts (0.4 years).<sup>34</sup> The

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provides additional evidence on the limited correlation between place effects and by plotting place effects against the share of migrant children in each destination.

<sup>31</sup>We do not use migrant weights in standardizing variables, because we also standardize contemporary measures. As a result, standardization can change the sign of the 1940 place effects.

<sup>32</sup>Note that our estimate is specific to the Northern locations chosen by Black migrants in our sample. Any place effects for counties that did not receive Black migrants are not identified by our empirical approach.

<sup>33</sup>Overall, 35 percent of destinations have positive place effects. This reflects the fact that migrants tended to move to destinations with better place effects (since the migrant-weighted average is zero).

<sup>34</sup>We calculate the effect of moving to the North on the educational attainment of all children (i.e., migrants and non-migrants) by multiplying the 0.8-year effect of moving to the North by the 21 percent of Black children that

increase in quality-adjusted education would most likely be higher given prior evidence on regional differences in the quality of schooling (Card and Krueger, 1992*b*; Carruthers and Wanamaker, 2017*b*). As an additional comparison of the North and South, 84 of the best 100 counties (in terms of place effects) are in the North, while 96 of the worst 100 counties are in the South.

Finally, as discussed in Section 3.3, an alternative estimate of the effect of moving North comes from a regression with a North indicator as the treatment variable. Appendix E provides a detailed discussion of estimates from this approach. For children of parents born in the South, we find that the coefficient on the indicator for moving North is equal to 1.01 years when we use basic demographic controls and restrict the sample to children located in counties with at least 10 migrants.<sup>35</sup> When we use a specification that identifies the North effect only among cross-state migrants, the estimate rises slightly to 1.2 years. This last estimate is comparable to the results from our selection-correction approach given its focus on cross-state migration as a source of identifying variation. The estimated 1.2-year schooling effect of migrating North that we find in Appendix E is considerably larger than the estimated 0.8-year effect reported in Figure 4. The key explanation for this difference is that our selection-correction approach has a sizable impact, reducing the North migration effect by 39 percent.<sup>36</sup>

### 4.3 Effects of Moving to Urban and Rural Areas

An additional question of interest is whether place effects differ between urban and rural counties. Educational opportunities for Black children likely were better in urban areas, because of both higher parental income (Smith and Welch, 1989) and higher school quality (Margo, 1990; Card,

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moved to the North. This leads to a 0.17-year effect, which is 43 percent of the 0.4-year convergence in the Black-White educational attainment gap (which we measure for the relevant cohorts using the 1960 Census).

<sup>35</sup>As detailed in Appendix E, we focus on children of parents in the matched sample so that we can examine the sensitivity of results to the inclusion of a range of controls. When examining the matched sample, we focus on children in counties with at least 10 migrants.

<sup>36</sup>In contrast, focusing on the matched sample accounts for much less of the discrepancy between the estimated effects of moving North obtained from a standard multivariate regression and our preferred selection-correction approach. As seen in equation (6), the North-South difference depends on place effects and the share of migrants in each destination, and estimating these quantities from the matched or full samples yields a nearly identical result of 1.2 years. When using the selection-correction on the matched sample, the estimated North-South difference is 0.7 years, which is similar to the 0.8-year difference from the full sample (which includes more counties).

Domnisoru and Taylor, 2022). However, characteristics of parents in urban and rural areas also differed, which makes identifying the urban-rural difference challenging using standard approaches. Our empirical strategy can address this type of selection.

Panel A of Figure 5 displays the density of place effects for urban and rural areas. We define urban and rural counties based on whether more or less than 50 percent of the 1940 population was in an urban area. The results show that the average place effect was 0.26 in urban areas and  $-0.61$  in rural areas. This implies that the overall urban-rural gap was a 0.87-year increase in schooling, which is almost equal to the overall North-South difference.

To what degree does the urban-rural gap simply reflect differences between the North and South? Panel B of Figure 5 displays urban and rural place effect distributions in each region. Notably, the results show that there were substantial benefits to moving from rural counties to urban counties in both the North and South. Within the North, place effects were 0.35 years larger on average in urban counties. In the South, the urban-rural difference is even larger, at 0.94 years. Interestingly, the figure also shows that rural counties in the North were better than urban counties in the South on average, though there is substantial overlap between the two distributions.<sup>37</sup>

#### 4.4 Comparing Place Effects by Race

Finally, an additional comparison of interest is whether place effects vary by race. One motivation for this analysis stems from the idea that Black children may have *differentially* benefited from moving due to racial gaps in schooling quality within the South. For example, Card and Krueger (1992a) show that, as of the 1920s, the pupil-teacher ratio in Southern Black schools was 50 percent higher than in White schools, and the average school term was 20 percent shorter. Another motivation is that White individuals in the North demonstrated violence and hostility in response to the arrival of Black migrants, while White migrants did not face the same degree of backlash

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<sup>37</sup>Note that these results also imply that the overall schooling gap between the North and South is largely due to the difference between place effects in Northern urban and Southern rural counties. As illustrated in Figure 5, the average migrant-weighted place effects in Northern urban and Southern rural counties were 0.37 and  $-0.98$  years, respectively. Urban counties contain 82 percent of migrant children living in the North, while 51 percent of migrant children in the South live in rural counties. Given these shares, we estimate that 0.80 years of the overall 0.83-year moving North effect is due to the difference in place effects in Northern urban and Southern rural areas.

(e.g., Myrdal, 1944).

We investigate racial heterogeneity by estimating place effects following our approach from Section 3 for the children of Southern-born White migrants. There were large out-migration flows of White individuals from the South during the Great Migration.<sup>38</sup> Appendix Figure 7 summarizes these results by illustrating densities of place effects for White children in Northern and Southern counties.<sup>39</sup>

Our main finding is that the North-South difference in place effects for White children is 0.01 years of schooling, which is notably smaller than the corresponding estimate of 0.80 years for Black children.<sup>40</sup> In line with this result, the North and South distributions for White children display much more overlap than for Black children. These results complement other recent work studying the importance of place of residence (Tan, 2019*b*, Forthcoming).

Figure 6 compares place effects for Black and White children directly. The overall correlation is modest, at 0.29. The reported slope coefficient indicates that counties which increase Black children's educational attainment by one year tend to increase White children's schooling by 0.15 years. The correlations in place effects for Black and White children within regions are also relatively low, at 0.27 for the South and 0.18 for the North. Overall, while the correlation in race-specific place effects is positive, the magnitudes are sufficiently low that we conclude that place effects vary by race to a large degree.

One possible explanation for the modest correlation between place effects for Black and White children is that the place effects for Black children reflect specific feedback channels, such as White flight (Boustan, 2010; Shertzer and Walsh, 2019), reductions in government expenditures (Tabellini, 2019), or segregation and police spending (Derenoncourt, 2022). Historical accounts suggest that White backlash would be stronger in places where the Black population share rose

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<sup>38</sup>There were differences between the two migration episodes. Notably, White individuals were considerably more likely to return to the South after migrating North (Gregory, 2005).

<sup>39</sup>To maintain comparability, we focus on destination counties for which we estimate place effects for Black children. The sample contains 2,897,674 White children, of whom 386,258 are children of migrants.

<sup>40</sup>Appendix Table 1 reports correlates of place effects for White children and county-level characteristics to better understand mechanisms. These descriptive results indicate that White children gained more years of schooling in locations with higher school resources and lower homicide rates.

by more (Henri, 1975). However, Appendix Figure 8 shows that place effects for Black children are larger in destinations where the Black population share rose by more from 1910–1940. This relationship is not causal and certainly does not rule out harmful consequences of White backlash. However, destinations where the Black population share rose most—such as New York, Philadelphia, Detroit, and Chicago—apparently offered superior opportunities for Black children net of White backlash. We defer further discussion of these mechanisms to Sections 5 and 6.1 below.

#### **4.5 Robustness: Additional Variables for Selection Correction**

Our baseline model includes indicators for father’s and mother’s years of schooling in  $H_i$ . This is a parsimonious specification, and one concern is that our estimates might be contaminated by dimensions of selection not correlated with parental education. To examine this, we add more variables from the 1940 Census to  $H_i$ . Our second model includes indicators for parental schooling plus indicators for whether only the father is present, whether only the mother is present, whether both parents are born in a different state, and whether one parent is born in a different state. Our third model adds indicators for father’s and mother’s age in five-year intervals and the number of children in the household. Panel A of Table 6 reports correlations of place effects from these different models. The three specifications yield extremely similar results, with place effect correlations all exceeding 0.98.<sup>41</sup> One key takeaway from this exercise is that parents’ education spans essentially all of the selection that can be controlled for with the 1940 Census. This is not surprising, as parents’ education is an especially strong predictor of children’s schooling and location. A second key takeaway is that any remaining selection must be outside the span of these variables.

The main disadvantage of the 1940 Census is that it only includes a limited set of household covariates. To overcome this limitation, we match men across the 1920 and 1940 Censuses to observe their pre-migration characteristics. We are able to match 14 percent of children’s fathers, so an immediate question is whether the place effects differ substantially in the matched sample. To examine this, Panel A of Figure 7 plots the relationship between place effects for our base-

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<sup>41</sup>The standard deviation of place effects and the average North-South difference in place effects also are very similar across the three specifications of  $H_i$ .

line specification (where  $H_i$  contains indicators for parents' schooling) estimated on the full and matched samples. The two sets of results are strongly related (correlation: 0.79). The lack of perfect correlation is not surprising, as the matched sample contains far fewer observations, which lowers the correlation through increased sampling variability.<sup>42</sup> Nonetheless, the high correlation indicates that conclusions drawn from the matched sample are informative about the full sample.

To see whether our results are robust to controlling for additional variables available in the matched sample, we take the most exhaustive version of  $H_i$  from the 1940 Census and add the following variables measured in 1920: whether a father was literate, whether he attended school, whether he lived in an urban area, whether he lived on a farm, how many siblings he had, and whether a grandfather (observed in 1920) was literate, whether the grandfather was a farmer, and the grandfather's Duncan socioeconomic index (a measure of income based on occupation). Panel B of Table 6 presents results for the matched sample, where we continue to use origin state fixed effects (as with the 1940 complete count data). The different versions of  $H_i$  yield estimates that are very strongly correlated (0.95 or higher). Most importantly, the estimates that use covariates from the 1920 Census are nearly identical to those that do not (correlation: 0.99), as also shown in Panel B of Figure 7. This suggests that the limited covariates in the 1940 Census do not meaningfully hinder our ability to adjust for selection.

Another key advantage of the matched sample is that we observe fathers' *county* of residence in 1920, instead of their birth state, which is all that is available in the 1940 Census. Individuals' origin county is correlated with family resources, early life human capital investments, and destination choice (e.g., Black et al., 2015; Stuart and Taylor, 2021a), so this finer level of geographic detail could be important. In Panel C of Figure 7, we plot place effects when using the most-saturated version of  $H_i$  (including covariates from the 1940 and 1920 Census) and either origin state or origin county fixed effects. The two sets of estimates are highly correlated (correlation: 0.87), which provides reassurance that the limited geographic detail in the 1940 Census does not compromise our estimates. Panel C of Table 6 further shows that, when using origin county

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<sup>42</sup>The matched sample also differs slightly on observable characteristics, as discussed in Appendix D.

fixed effects, the results from different versions of  $H_i$  are extremely similar.

We conclude that our estimates from the 1940 Census do not suffer from omitted variable bias that could be mitigated with matched Census data. The robustness of our results to the inclusion of many additional controls supports a causal interpretation of our place effect estimates.

#### 4.6 Robustness: Relaxing Identifying Assumptions

While we have shown that our estimates are not sensitive to the variables used to adjust for selection on unobservables, all of the estimates presented so far rely on Assumptions 1 and 2. In this section, we address remaining concerns by relaxing the identifying assumptions used in our selection-correction approach.

One potential scenario is that parental migration decisions are based *more* on parent human capital (including observed and unobserved components) than on children’s schooling capital. For example, this could occur because there was better information about labor market opportunities for parents than schooling opportunities for children. Historical accounts suggest that this scenario was plausible (e.g., Grossman, 1989; Gregory, 2005).

Greater relative selection of parent human capital has two potential implications for our econometric model. First, location choices could be more strongly correlated with parents’ education (which is the key input into children’s observed schooling capital,  $h_i = H_i\lambda$ ) than the unobserved component of children’s schooling capital:

$$\text{Corr}(T_{ij}, h_j^{\text{dest}}) > \text{Corr}(T_{ij}, \eta_j^{\text{dest}}). \quad (8)$$

Second, there might be less cross-destination variation in the unobserved component of children’s schooling capital than is posited by Assumption 2:

$$SD(\eta_j^{\text{dest}}) < SD(h_j^{\text{dest}}) \frac{SD(\eta_o^{\text{orig}})}{SD(h_o^{\text{orig}})}. \quad (9)$$

These inequalities lead to violations of Assumptions 1 and 2. However, as discussed in Finkel-

stein, Gentzkow and Williams (2021), it is possible to generate selection-corrected results using relaxed assumptions. Specifically, more general assumptions are:

**Assumption 3** (*Relaxed equal selection*)  $\text{Corr}(T_{ij}, \eta_j^{\text{dest}}) = C_1 \text{Corr}(T_{ij}, h_j^{\text{dest}})$  in the sample of migrants for all  $j$ .

**Assumption 4** (*Relaxed relative importance*)  $\frac{SD(\eta_j^{\text{dest}})}{SD(h_j^{\text{dest}})} = C_2 \frac{SD(\eta_o^{\text{orig}})}{SD(h_o^{\text{orig}})}$  in the sample of migrants.

Assumptions 3 and 4 lead to a modified estimate of the confounding variable  $\hat{\eta}_j^{\text{dest}}$ :

$$\hat{\eta}_j^{\text{dest}} = C_1 C_2 \frac{\widehat{SD}(\hat{\tau}_o^{\text{orig}})}{\widehat{SD}(\hat{h}_o^{\text{orig}})} \hat{h}_j^{\text{dest}}. \quad (10)$$

There are two key observations about equation (10). First, Assumptions 1 and 2 impose  $C_1 = C_2 = 1$ . Second, these relaxed assumptions can accommodate the scenario in which there is relatively greater selection on parent human capital. That is, the conditions from equations (8) and (9) imply that  $C_1 < 1$  and  $C_2 < 1$ . If there were relatively greater selection on children's schooling capital, then we could have  $C_1 > 1$  and  $C_2 > 1$ .

Table 7 describes the sensitivity of our results to different assumptions about  $C_1$  and  $C_2$ . For clarity, we focus on the quantity  $C \equiv C_1 C_2$  and generate new selection-corrected estimates of place effects in 1940 using different values of  $C$ . The table reports summary statistics of our place effect estimates as we reduce  $C$  by 50 percent (to 0.5), which is most relevant for considering the scenario where there is positive selection in terms of parent human capital alongside selection in terms of children's schooling capital that is also positive but smaller in magnitude. Specifically, we report the correlation of the relaxed and baseline versions of our estimates, the cross-county standard deviation of place effects, and the average North-South difference. Our conclusions are quite similar when  $C < 1$ . All of the correlations between estimates are close to one (column 1), and the standard deviation of place effects remains substantial (consistently at nearly 0.9 years). The North-South difference grows slightly, from 0.8 to 1.1. We also explore the sensitivity of our results when we increase  $C$  by 50 percent (to 1.5). This situation would arise if migration decisions



were based relatively more on child schooling capital. As demonstrated in Table 7, we find that there is still a substantial North-South gap in cases where  $C > 1$  as well. In sum, these results show that our estimates are robust to potential violations of the key identifying assumptions.

#### **4.7 Robustness: Alternative Sample Definitions and Other Schooling Measures**

This section presents place effect estimates based on alternative sample definitions and measures of schooling. Two concerns motivate these additional results. First, our main sample may suffer from selection because of the requirement that children live with at least one parent. Second, our main analysis may be affected by censoring because some children in our sample are still enrolled in school in 1940.

We begin by assessing whether our analysis is sensitive to the requirement that children live with at least one parent. We do so by comparing our main estimates to those obtained from two alternative samples. The first alternative is an expanded sample that includes children living with any relative. This further reduces the scope for selection since the fraction of 14–18 year old Black children that live with any relative was 91 percent in 1940 (compared to 80 percent living with at least one parent). The second alternative is a sample restricted to children ages 14–16 who live with a parent. This also reduces the scope for selection since a greater share of 14–16 year old Black children live with a parent (84 percent, compared to 80 percent of 14–18 year olds).

The results in the top panels of Appendix Figure 9 show that we obtain similar results using these two alternative samples. Panel A illustrates the relationship between our main place effect estimates (specified as the  $x$ -axis) and the alternative estimates based on the broader sample of children who live with any relative. Panel B has the same format for the results where the alternative sample is children ages 14–16. Our main place effects are very highly correlated with these alternatives, with correlations of 0.99 and 0.97.

Next, we use two approaches to assess whether censoring affects our conclusions. Our main analysis focuses on the years of schooling attained by children ages 14–18. While the vast majority

of schooling is attained by age 18, censoring remains a potential concern.<sup>43</sup> To address this issue, we estimate place effects only using the sample of children who are ages 16–18. In addition, we also estimate place effects on eighth grade completion since this is an outcome subject to less concern over censoring.

The results presented in the bottom panels of Appendix Figure 9 suggest that censoring does not strongly affect our results. Panel C shows that place effects based on the sample of children ages 16–18 are highly correlated with our main estimates (correlation: 0.97). Panel D also shows that there is a high correlation between place effects on eighth grade attainment and those based on years of schooling (correlation: 0.94). In unreported results, we find that place effects on years of schooling are also strongly related to seventh grade attainment (correlation: 0.94), ninth grade attainment (correlation: 0.92), and tenth grade attainment (correlation: 0.84).

#### **4.8 Robustness: Bounding Exercise to Account for Potential Mortality Effects**

As a final robustness exercise, this section summarizes results from a bounding analysis that accounts for selective survival of children. The motivation for this exercise is based on prior research that highlights the potential for migration from the rural South to the urban North during the early 20th century to increase Black infant mortality (Eriksson and Niemesh, 2016). We compute upper and lower bounds for county-level place effects to account for the fact that children may have died early in life (and therefore not be included in our analysis sample). Using infant mortality rate data from Bailey et al. (2018), we compute bounds by assuming that the place effect for children who did not survive is either the minimum or maximum estimated place effect. A detailed discussion of our approach is provided in Appendix F.

The general conclusions from the bounding exercise are similar to our main results. For coun-

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<sup>43</sup>In our sample, 26 percent of 18-year-olds are still enrolled in school at the time of the 1940 Census. However, this number is consistent with the vast majority of schooling being completed by age 18. In particular, the 1940 Census shows that years of schooling for 18-year-olds is 97 percent of schooling for 19-year-olds, who have the highest level of education. If the 18-year-olds that are enrolled in school complete one additional year of education—consistent with the distribution in Appendix Figure 3—then their education would rise by 9.1 percent ( $=1/11$ ). Since only 26 percent of individuals are enrolled in school at age 18, the total increase in schooling is 2.4 percent ( $= 0.091 \times 0.26$ ). In sum, censoring is limited by the facts that (a) very few Black youth obtained more than 12 years of schooling and (b) individuals largely completed schooling by age 18.

ties in the South, the migrant-weighted average upper and lower bounds are  $-0.36$  and  $-0.66$ . In the North, the migrant-weighted average upper and lower bounds are  $0.39$  and  $0.15$ . These estimates suggest that the effect of moving North is at least a 0.51-year increase in schooling, and no more than a 1.05-year increase. Given the conservative nature of these bounds, we view the similarity of our main estimate—a 0.83-year increase in schooling—as reassuring.

## 5 Mechanisms: Correlates of Place Effects

Why did Black children obtain much larger gains in educational attainment in certain destinations than in others? To study this question, we follow prior studies (e.g., Chetty and Hendren, 2018*b*; Finkelstein, Gentzkow and Williams, 2021) and examine cross-sectional correlations between place effect estimates for 1940 and historical measures of local area characteristics. The results in this section should be interpreted cautiously given a natural concern over unobserved factors that vary across locations.

We begin by estimating cross-sectional correlations between 1940 place effects and proxies for county-level school quality, parental labor market opportunities, crime, criminal justice policies, and social capital. These factors have been discussed widely in economics. Our contribution is examining the correlation between these factors and selection-corrected place effects, which have not been estimated in our historical setting before. We construct proxies using the 1940 Census and other historical records (e.g., Biennial Surveys of Education with measures of teachers).<sup>44</sup>

Columns 1–5 of Table 8 report correlations between county-level place effects on child educational attainment and local area characteristics. Place effects are considerably higher in counties with more teachers per pupil (correlation: 0.47). This finding is consistent with previous research showing wide variation in educational opportunity for Black children, especially due to a lack

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<sup>44</sup>Another potential mechanism is that parents which moved to the North might have had fewer children and invested greater resources in the children they had (Becker and Lewis, 1973). Table 2 shows that children of migrants in the North lived in a household with 0.11 ( $=3.92 - 4.03$ ) fewer children than those in the South on average. Tan (2019*a*) uses a twins birth research design to estimate that one additional sibling reduces educational attainment by 0.2 years (for a sample of White children in historical data). An estimate of this magnitude suggests that the smaller family size in the North might account for 0.02 years of the total 0.83-year North effect (i.e., just 2.4 percent).

of resources in segregated schools in the South (e.g., Margo, 1990; Card and Krueger, 1992*a,b*; Carruthers and Wanamaker, 2017*a,b*). This finding is also consistent with other recent research studying how child outcomes vary across locations. Card, Domnisoru and Taylor (2022) find that state-level measures of upward mobility in education are tied to school quality measures for White and Black children born in the 1920s, and they verify this finding at the county-level within the South using a state border research design. Chetty et al. (2014) use comprehensive tax records for U.S. children born in the 1980s and show that intergenerational mobility for all children is strongly correlated with proxies for quality of the K-12 school system. In our setting, a key takeaway is that children benefited when their parents moved to places with better schools, though we cannot isolate the contribution of school quality.

Table 8 also reports a strong relationship between place effects and median Black family income (correlation: 0.62). Black migrants experienced large income gains from moving to the North during the Great Migration. For example, Collins and Wanamaker (2014) study a matched sample of Southern-born men in the 1930 Census, and find that migration increased earnings by 80 to 100 percent. Boustan (2017) finds slightly larger estimates using a matched sample based on the 1940 Census. Higher earnings could have benefited children through the income effect (for example, through better nutrition or a more stable environment), although economic theory does not provide an unambiguous prediction because of the offsetting substitution effect.<sup>45</sup>

To further gauge the degree to which the positive correlation between place effects and median

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<sup>45</sup>Empirical studies yield mixed evidence on the importance of parental income and resources for child education in historical U.S. contexts. On the one hand, Aizer et al. (2016) find that receipt of cash transfers through a pension program for poor mothers increased child educational attainment by one-third of a year, and Aizer et al. (2020) find that improvements in the labor market opportunities available to African Americans after 1940 led to higher educational attainment for Black children. On the other, Bleakley and Ferrie (2016) study large wealth transfers provided through a land lottery in Georgia, finding that sons of winners did not acquire more schooling compared to non-winners. Studies in contemporary contexts also provide conflicting evidence on the importance of parental income. For example, studies of the EITC program suggest that cash transfers have meaningfully large impacts on test scores and college-going (Dahl and Lochner, 2012; Bastian and Michels, 2018). Similarly, Akee et al. (2010) find that transfer payments from casino profits increase educational attainment for Native American children. Bulman et al. (2021) find that college attendance is sensitive to only large increases in resources from lottery winnings. Jacob, Kapustin and Ludwig (2014) find precisely estimated zero impacts on schooling for households that receive a large transfer due to receipt of a housing voucher. Studying a question more similar to our focus on local labor market conditions, Stuart (2022) finds that declines in local economic activity due to the 1980–1982 recession led to lower educational attainment for children.

Black family income reflects potential earnings gains of migrants, we use our main approach from Section 3 to estimate place effects for log earnings of Black men ages 25–64 born in the South.<sup>46</sup> These results show that there is a significant 42 percent increase in earnings for men who moved North and suggest that effects on parental income may drive increases in human capital for children. Although our estimate is considerably smaller than the effect detected in prior studies, our findings still suggest that much of the relationship between place effects for children’s schooling and median Black family income is driven by earnings gains available to adult migrants.<sup>47,48</sup>

In addition to opportunities available at school and home, children’s education may have been shaped by the prevalence of crime. Place effects are considerably lower in counties with higher homicide rates (correlation:  $-0.41$ ). This correlation is consistent with recent causal evidence that increases in the rate of violent crime experienced during late adolescence decrease upward mobility (Sharkey and Torrats-Espinosa, 2017).

While crime displays a substantively large association with place effects, we do not see a strong correlation for the incarceration rate in 1940. One consideration for interpreting this evidence is that the incarceration rate increased notably during subsequent decades. For example, the rate of incarceration per 100,000 people was 131 in 1940 and 293 in 1990 (U.S. Department of Justice, 1982, 1991). Consequently, correlations in 1940 may provide only a limited test of the importance of incarceration as a mechanism for place effects. We return to this issue in the next section where we use an alternative approach to study mechanisms.

Social capital is a final type of mechanism that could explain our place effect estimates. Previous research theorizes and provides evidence that local area social capital—the strength of social

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<sup>46</sup>The 1940 Census measures wage and salary income, but not total earnings (which also includes self-employment income). We impute earned income for self-employed individuals based on their race, region, and occupation, as detailed in Appendix G.

<sup>47</sup>Our analysis also allows us to look at the simple correlation between child schooling and adult earnings place effects. We find that place effects on children’s education are strongly related to the estimated impacts on adult earnings (correlation: 0.59).

<sup>48</sup>In Appendix H, we provide a detailed comparison of the estimated impact of moving North on adult earnings. Our main finding is that a substantial amount of the difference between our bottom-line estimate of a 42 percent earnings gain from moving North and the 80–130 percent estimate from prior work appears to be explained by controlling for observed variables (in particular, education) and focusing on a subset of counties for which there is a sufficiently large sample of migrants that we can feasibly estimate place effects. A smaller, but still significant, share of the difference is explained by adjusting for selection on unobserved migrant characteristics.

networks and community engagement—has important impacts on social and economic outcomes (Coleman, 1988; Putnam, 2000; Sampson, Raudenbush and Earls, 1997; Stuart and Taylor, 2021*b*). We proxy for social capital in our setting by measuring the presence of a local National Association for the Advancement of Colored People (NAACP) chapter in 1940 (Gregory and Estrada, 2019). Founded in 1909, the NAACP played a key role in the civil rights movement throughout the 20th century.<sup>49</sup> Column 5 of Table 8 shows that place effects were significantly stronger in counties with a NAACP chapter (correlation: 0.43).

A natural concern is that these correlations potentially reflect the influence of other variables. To explore this possibility, we estimate a range of multivariate regression models. We standardize both dependent and independent variables to ensure that the coefficients are comparable to the unconditional correlations. Column 6 of Table 8 reports results. We continue to see a strong positive relationship between place effects and teachers per pupil: a one standard deviation increase in teachers per pupil is associated with a 0.18 standard deviation increase in place effects. There is also a strong positive relationship with Median Black income (coefficient: 0.43) and the presence of a NAACP chapter (coefficient: 0.16) and a negative relationship with homicide (coefficient:  $-0.17$ ). Point estimates from the multivariate specification are smaller than the unconditional correlations, but remain statistically significant. The simple regression, with five explanatory variables, explains a sizable 47-percent of the cross-county variation in place effects. To explore how much of the relationship is driven by differences between the North and South, column 7 includes a South indicator. We continue to see a strong relationship between place effects and teachers per pupil, parental income, the homicide rate, and the presence of a NAACP chapter.

While the variables included in Table 8 are motivated by economic theory and prior empirical studies, they represent a limited set of place characteristics. We use this selected set of variables to minimize the issue of multicollinearity that arises when examining highly correlated variables. For a more comprehensive descriptive exploration of mechanisms, Figure 8 reports correlations for additional place characteristics. The results are consistent with those in Table 8: children obtain

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<sup>49</sup>In our sample, 337 of 728 counties had a NAACP chapter in 1940.

more schooling when their parents moved to counties with higher quality schools (as measured by average teacher salary, term length, the absence of required segregation, and non-migrant children's educational attainment), greater access to secondary schools (as proxied by grade 9 enrollment of Black children being high relative to grade 8 enrollment), and better labor market opportunities for parents (as measured by higher average earnings of Black men and a higher manufacturing employment share, along with lower inequality and poverty).<sup>50</sup> Place effects are smaller in counties with a larger Black population share and a larger share of the population living on farms, but interpreting these latter correlations is particularly difficult. For example, the inter- and intra-regional location patterns of African Americans were influenced by slavery and sharecropping, which are associated with different economic and political factors. In addition, African American schools were systematically underfunded (e.g., Margo, 1990), which makes it difficult to separate out any effect due to demographics from public goods.

## **6 The Geography of Black Opportunity Over Time**

As highlighted in Section 1, several recent studies have examined the geography of opportunity using contemporary data. Particularly relevant to this paper, Chetty et al. (2020) estimate county-level measures of upward mobility for Black children. Upward mobility is defined as the mean household income rank for children whose parents were at the 25th percentile of the national income distribution. Chetty et al. (2020) construct this measure for children born between 1978 and 1983.

How do the education-based place effects estimates for 1940 compare to measures of opportunity for more recent cohorts? Table 5 shows that there are notable changes in county-level measures of opportunity during the twentieth century. For example, place effects in 1940 are large and posi-

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<sup>50</sup>To the best of our knowledge, county-level data on the availability of secondary schools for Black children are not available. We construct a proxy measure based on the ratio of ninth to eighth grade enrollment for Black children ages 12 to 17 in the 1940 Census. We define an indicator for high grade 9 enrollment that is equal to 1 when the ratio is at least 0.5 (i.e., when ninth grade enrollment is at least 50 percent of eighth grade enrollment). The correlation between our place effect estimates and this measure is 0.32. Results are similar when we define high secondary school access based on whether the ratio is at least 0.25 (correlation: 0.23).

tive in Cook (Chicago), Allegheny (Pittsburgh), Cuyahoga (Cleveland), and Los Angeles counties. These areas offer relatively poor opportunities for Black youth today, as seen in column 5, which reports standardized values of Black upward mobility for children who grew up in these areas during the 1980s and 1990s. More generally, standardized opportunity measures fell in relative terms for 17 of the 20 largest counties in terms of 1940 Black population.<sup>51</sup>

Table 5 provides additional context on these changes in columns 7 and 8, where we rank opportunity measures among the 100 largest counties in terms of 1940 Black population. One striking example is Cook County (Chicago), Illinois where the place effect in 1940 was 1.1 standard deviations above-average and ranked 15th. By the 1990s, the mobility measure was 0.5 standard deviations below average and ranked 67th. We see similarly large declines in opportunity in other Northern counties, such as Wayne, Michigan (Detroit); St. Louis, Missouri; Allegheny, Pennsylvania (Pittsburgh); and Cuyahoga, Ohio (Cleveland). We also see declines in several Southern counties, including Jefferson, Alabama (Birmingham) and Shelby, Tennessee (Memphis). Counties in the New York City metro area stand out as places where opportunity remained high in relative terms.

Broadening our focus to all 728 counties in our sample, we find only a modest positive correlation in county-level opportunity measures over time. Figure 9 plots standardized upward mobility estimates from the 1990s and standardized place effects from 1940. The correlation between historical and contemporary measures is equal to 0.21. This highlights the extent of change in opportunity over the 50-year period that we study.<sup>52</sup> We study the mechanisms underlying these changes in Black children's opportunities next.

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<sup>51</sup>The education place effects that we estimate in 1940 differ conceptually from the upward mobility measure from Chetty et al. (2020), but both variables broadly reflect the opportunities that are available to Black children living in a county.

<sup>52</sup>Appendix Table 2 shows that the correlation remains modest when using other upward mobility measures. The correlation is 0.43 when using pooled upward mobility estimates for White and Black youth from Chetty et al. (2020) and 0.30 when using exposure effects for all races from Chetty and Hendren (2018a).



## 6.1 Understanding Changes in Opportunity

In this section, we provide a descriptive analysis of the factors that changed place effects for Black children during the latter half of the twentieth century. Specifically, we combine estimates of 1940 place effects and contemporary measures of Black upward mobility from Chetty et al. (2020) to create a two-period panel that has county-level measures of Black child outcomes. As detailed in Appendix G, we complete the panel by drawing on several sources to measure place characteristics in 1940 and circa 1990 (i.e., the period that aligns with the childhood years for the contemporary mobility measure). To facilitate comparisons, we normalize the measures of child outcomes and place characteristics so that each has a mean of zero and a standard deviation of one within each time period.

Pooling historical and contemporary measures of child outcomes allows us to provide suggestive evidence on the mechanisms driving place effects while controlling for time-invariant differences across counties. In line with the analysis in Section 5, we focus on the role of school quality, parental labor market opportunities, crime, incarceration, and social capital. Columns 1–6 of Table 9 report estimates of the descriptive relationship between changes in opportunity measures and place characteristics. Formally, we estimate the following first-difference specification:

$$\Delta ChildOutcomes_j = \alpha + \beta \Delta PlaceCharacteristic_j + \Delta \epsilon_j, \quad (11)$$

where  $\Delta ChildOutcomes_j$  is the difference between the normalized values of upward mobility and 1940 place effects in county  $j$ ,  $\Delta PlaceCharacteristic_j$  is the difference between the normalized values of the place characteristics, and  $\Delta \epsilon_j$  is the first-difference error term. The coefficient of interest,  $\beta$ , describes how a one standard deviation change in the place characteristic correlates with a change in the child outcome measure in standard deviation units.

The estimates in columns 1–6 reinforce many of the conclusions from our descriptive analysis in Section 5. We find large and statistically significant correlations with changes in teachers per pupil (coefficient: 0.25), median Black household income (coefficient: 0.43), the homicide rate

(coefficient:  $-0.16$ ), and the addition of a NAACP chapter (measured between 1960 and 1940; coefficient:  $0.18$ ).<sup>53</sup> The magnitudes of these correlations are generally similar to those in Table 8. At the same time, the results for incarceration in Table 9 contrast with the cross-sectional evidence. Specifically, we find that a one-standard-deviation increase in the incarceration rate from 1940 to 1990 is associated with a 0.11-standard-deviation decrease in child outcomes.

The remaining columns of Table 9 show that the estimates from first-difference multivariate specifications are similar to the unconditional estimates. Column 7 shows that the point estimates generally change by less than a standard error when we estimate a specification that includes all explanatory variables at the same time. In columns 8 and 9, we find there is little difference in the results when we include the change in the Black population share or a South indicator in the multivariate specification. These results suggest that the estimated relationships are not driven by the demographic changes that accompanied the Great Migration or broad regional differences between the South and the rest of the country.<sup>54</sup>

Finally, we undertake two additional exercises to demonstrate that the conclusions from our within-place approach are robust. First, Appendix Table 3 demonstrates that results are qualitatively similar when we rely on an alternative measure of upward mobility for children who grew up during the 1980s and 1990s. Specifically, this analysis uses county-level estimates of childhood exposure effects from Chetty and Hendren (2018*b*) (instead of upward mobility of Black children) to construct the dependent variable in equation (11). Exposure effects represent the gain in earnings associated with spending one additional year in a given area. The strength of these estimates is that the exposure effects better reflect causal impacts of places during the contemporary period.<sup>55</sup> Yet, a key limitation—and the reason that we rely on upward mobility measures in our main specification—is that the exposure effect estimates are *not* race-specific. Second, Appendix

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<sup>53</sup>The data from Gregory and Estrada (2019) only contain information on NAACP chapters up to 1960.

<sup>54</sup>Appendix Figure 10 supplements these results by displaying binned scatterplots for each of the mechanisms included in Table 9.

<sup>55</sup>These estimates are based on a research design that compares children who spend more or less time during childhood in a given area. The variation in exposure arises from differences in children's age at the time that their families moved. These estimates are based on tax return data for all children born between 1980 and 1986. The outcome of interest is adult income rank at age 26.

Figure 11 reports estimates of the relationship between changes in place effects and a more comprehensive set of place characteristics that we can measure in 1940 and the 1990s. These results show that alternative measures of local area characteristics have qualitatively similar associations as the main measures that we examine in Table 9. For example, the change in manufacturing employment shares is positively correlated with the change in opportunity measures, and the magnitude of this relationship is similar for median family income.

## **6.2 Discussion of Mechanisms Driving Changes in Place Effects**

Overall, the results in Tables 8 and 9 indicate that areas with stronger schools, economic prospects, and social capital generate better outcomes for Black children. At the same time, the results also suggest that increases in violent crime and incarceration lead to worse outcomes for children.

One notable comparison for these results is Derenoncourt (2022). She uses a shift-share instrumental variable strategy to identify the impact of the second wave of the Great Migration (lasting from 1940 to 1970) on upward mobility and several place-based mediators. In contrast, we explore the independent roles of several potential mechanisms, without attempting to isolate the component catalyzed by the arrival of Black migrants. She finds that Northern cities (commuting zones) which experienced greater Black migration between 1940 and 1970 have lower rates of upward mobility for Black children born during the 1980s. In an analysis of mechanisms, she shows that both crime rates and incarceration rates causally responded to the intensity of migration. In addition, she finds no evidence that migration impacted local area schooling investment levels.

Our findings complement and extend the results from Derenoncourt (2022) in two main ways. First, we find important roles for school quality and local economic conditions in explaining upward mobility. While Derenoncourt (2022) shows that the arrival of Black migrants might not have had a first-order impact on these variables, we find that school quality and labor market opportunities are positively associated with Black children's educational attainment. Second, we find evidence that crime rates and incarceration relate to differences in child outcomes across areas, and these relationships are robust to controlling for local area racial composition. These results

for crime and incarceration underscore the importance of these mechanisms, which also are highlighted by Derenoncourt (2022) in explaining the effect of Black migration during 1940–1970 on Northern cities.

## 7 Conclusion

During the 20th century, African Americans born in the South sought better opportunity for themselves and their children by migrating. Prior research shows that the Great Migration yielded mixed benefits for adults, as their income rose while their life expectancy declined and the likelihood of incarceration increased (Collins and Wanamaker, 2014; Black et al., 2015; Boustan, 2017; Eriksson, 2019). The consequences of moving to the North for Black children has received less attention.

This paper provides a comprehensive assessment of how moving affected the educational outcomes of migrants' children. Based on selection-corrected county-level estimates of place effects, we find that the average effect of moving from the South to the North was a 0.8 year (12 percent) increase in schooling as of 1940. While the North offered better opportunities on average, there was wide variation in the benefits of migrating. Some places in the South—such as Birmingham, Alabama—were comparable to the best places in the North, while others—such as New Orleans, Louisiana—offered poor prospects to children.

Overall, this paper suggests that the Great Migration played a role in the narrowing U.S. educational disparities by race. The education gap between White and Black individuals shrank between the 1900 and 1970 birth cohorts from 4.0 to 0.9 years—a 78 percent reduction. Existing research finds that improvements in Southern schools played an important role in the relative rise in Black educational attainment (Card and Krueger, 1992*a*; Aaronson and Mazumder, 2011). This paper demonstrates how the Great Migration promoted schooling achievement, thereby enriching our understanding of the relative rise in African Americans' education during the twentieth century.

Most importantly, our findings provide evidence that the opportunities available to Black children depended strongly on place-specific policies and characteristics in our setting. Opportunities

were greater in destinations that offered higher earnings to adults, invested more in their schools, developed social capital, lowered crime, and placed fewer individuals in prison. These results, combined with our finding that place effects changed meaningfully over the second half of the twentieth century, highlight the potential for local factors in driving further progress in closing the Black-White opportunity gap.

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## Main Tables and Figures

Table 1: Place Characteristics in South and North Circa 1940

	South		North	
	Mean (1)	N (counties) (2)	Mean (3)	N (counties) (4)
School segregation required	1.000	435	0.362	293
Term length (days)	153.4	245	179.5	218
Teachers per pupil	0.025	345	0.032	218
Avg. teacher salary	469	344	1939	218
Avg. years of education, non-migrants 14–18	5.99	435	8.15	289
Median Black household income	341	435	578	293
Avg. earnings, non-migrant men 25–64	337	435	582	292
Manufacturing employment share	0.150	435	0.205	293
Income inequality (Gini index)	0.479	435	0.407	293
Poverty rate	0.527	435	0.349	293
Homicide rate (per 100k)	12.74	434	4.95	293
Lynching rate (per 100k)	40.5	354	92.6	24
Incarceration rate (per 100k)	816	435	1193	293
Residential segregation (Theil index)	0.646	428	0.555	285
Percent Black	0.355	435	0.062	293
Percent on farm	0.459	435	0.203	293
Percent urban	0.266	435	0.511	293

Notes: Table reports unweighted averages across counties in our analysis sample with non-missing values of each variable. Our analysis sample is limited to counties with at least 25 Black migrants age 14–18. See Appendix G for details on variable construction and sources.

Source: Authors' calculations using 1940 Census (Ruggles et al., 2020)

Table 2: Summary Statistics, Analysis Sample

Location in 1940:	Non-Migrant			Migrant		
	All (1)	South (2)	North (3)	All (4)	South (5)	North (6)
Years of schooling	6.30	6.10	8.11	7.68	6.52	8.36
Completed grade 8	0.408	0.372	0.732	0.668	0.444	0.800
Female	0.493	0.492	0.498	0.500	0.495	0.503
Age	15.91	15.91	15.93	15.91	15.90	15.91
Father's years of schooling	4.36	4.14	6.56	5.34	4.26	6.01
Mother's years of schooling	5.31	5.11	7.16	6.29	5.27	6.91
Only father present	0.056	0.056	0.060	0.059	0.053	0.063
Only mother present	0.216	0.210	0.270	0.239	0.213	0.254
Both parents from different state	0.006	0.005	0.011	0.851	0.707	0.936
One parent from different state	0.315	0.294	0.506	0.146	0.287	0.063
Father's age	46.76	46.88	45.63	46.12	47.17	45.46
Mother's age	41.49	41.50	41.42	40.77	41.06	40.59
Number of children in household	4.47	4.51	4.14	3.96	4.03	3.92
Number of individuals	436,289	392,995	43,294	213,751	79,378	134,373
Number of counties	719	435	284	728	435	293

Notes: Sample contains Black youth age 14–18. A migrant is defined as someone whose household head was born in the South and lives outside the head's birth state in 1940.

Source: Authors' calculations using 1940 Census (Ruggles et al., 2020)

Table 3: Inputs into Selection Correction

	Standard Deviation
<b>Origin components</b>	
Observed schooling capital ( $h_o^{\text{orig}}$ )	0.081 [0.076, 0.087]
Unobserved schooling capital ( $\eta_o^{\text{orig}}$ )	0.082 [0.070, 0.096]
<b>Destination components</b>	
Observed schooling capital ( $h_j^{\text{dest}}$ )	0.392 [0.382, 0.400]
Unobserved schooling capital ( $\eta_j^{\text{dest}}$ )	0.397 [0.328, 0.466]

Notes: Table reports equally-weighted standard deviations of origin state and destination county fixed effects from equations (3), (4), and (5). In particular, the unobserved schooling capital origin fixed effect,  $\eta_o^{\text{orig}}$ , is identified directly from equation (3) as  $\tau_o^{\text{orig}}$ :

$$Y_i = X_i\psi + H_i\lambda + \tau_o^{\text{orig}} + \tau_j^{\text{dest}} + \tau_j^{\text{nm}} + \tilde{\eta}_i,$$

where  $Y_i$  is the schooling of child  $i$ ,  $X_i$  is a vector of demographic variables,  $H_i$  is a vector of variables that gauge the extent of selection on observables, and the  $\tau$  terms are fixed effects for migrant origin location, migrant destination location, and non-migrant location. The observed schooling index is defined in the above equation as  $h_i = H_i\lambda$ . We use this to estimate equation (4) on the sample of migrant children:

$$h_i = X_i\psi^h + h_o^{\text{orig}} + h_j^{\text{dest}} + \tilde{h}_i.$$

This equation identifies  $h_o^{\text{orig}}$  and  $h_j^{\text{dest}}$  as fixed effects for the origin and destination locations of migrant children. Finally, we use equation (5) to estimate the destination fixed effect for unobserved schooling capital as

$$\eta_j^{\text{dest}} = [SD(\tau_o^{\text{orig}}) / SD(h_o^{\text{orig}})] h_j^{\text{dest}}.$$

The key confounding variable for estimation of place effects is  $\eta_j^{\text{dest}}$ . To construct an unbiased estimate of the standard deviation across origin states, we divide standard deviation estimates by the small sample size correction factor  $c(N) = \sqrt{2/(N-1)}\Gamma(N/2)/\Gamma((N-1)/2)$ , which equals 0.98 for  $N = 11$ . Ninety-five percent confidence intervals are calculated using 200 Bayesian bootstrap replications.

Source: Authors' calculations using 1940 Census (Ruggles et al., 2020)

Table 4: Variance Decomposition of the Determinants of Black Children’s Education

	Standard Deviation
Education index ( $\gamma_j + \bar{\theta}_j$ )	1.429
Unadjusted	
Place effects ( $\gamma_j$ )	1.074 [1.057, 1.093]
Schooling capital ( $\bar{\theta}_j$ )	0.793 [0.758, 0.832]
Correlation of $\gamma_j$ and $\bar{\theta}_j$	0.153 [0.110, 0.194]
Selection corrected	
Place effects ( $\gamma_j$ )	0.848 [0.812, 0.888]
Schooling capital ( $\bar{\theta}_j$ )	1.009 [0.954, 1.073]
Correlation of $\gamma_j$ and $\bar{\theta}_j$	0.179 [0.123, 0.227]

Notes: Table reports equally-weighted standard deviations across counties. This table is based on estimates of equation (3):

$$Y_i = X_i\psi + H_i\lambda + \tau_o^{\text{orig}} + \tau_j^{\text{dest}} + \tau_j^{\text{nm}} + \tilde{\eta}_i,$$

where  $Y_i$  is the schooling of child  $i$ ,  $X_i$  is a vector of demographic variables,  $H_i$  is a vector of variables that gauge the extent of selection on observables, and the  $\tau$  terms are fixed effects for migrant origin location, migrant destination location, and non-migrant location. In the top panel, the unadjusted place effect  $\gamma_j$  is the estimate of the fixed effect for migrants’ destination location,  $\tau_j^{\text{dest}}$ , and schooling capital is the mean of the remaining terms in this equation for non-migrant children. In the bottom panel, the selection-corrected place effect  $\gamma_j$  is the estimate of  $\tau_j^{\text{dest}} - \eta_j^{\text{dest}}$ , and schooling capital again is the mean of the remaining terms in equation (3) for non-migrant children. Ninety-five percent confidence intervals are calculated using 200 Bayesian bootstrap replications.

Source: Authors’ calculations using 1940 Census (Ruggles et al., 2020)

Table 5: Opportunity Measures in 1940 and 1990s for Black Children, Counties with Largest Black Population in 1940

Black population rank, 1940 (1)	County (2)	Place effect, 1940 (3)	Standardized place effect, 1940 (4)	Standardized mobility measure, 1990s (5)	Change in standardized opportunity measures (6)	Place effect rank, 1940 (7)	Mobility measure rank, 1990s (8)
1	New York, NY	0.59	1.33	0.75	-0.58	8	10
2	Cook, IL	0.44	1.11	-0.50	-1.61	15	67
3	Philadelphia, PA	0.55	1.27	0.03	-1.24	10	30
4	Washington, DC	-0.14	0.29	0.86	0.58	44	8
5	Jefferson, AL	0.51	1.21	-0.22	-1.43	13	42
6	Baltimore City, MD	-0.05	0.43	-0.29	-0.71	39	47
7	Wayne, MI	0.22	0.80	-0.59	-1.39	26	73
8	Shelby, TN	-0.13	0.31	-1.08	-1.39	42	93
9	Orleans, LA	-0.30	0.07	0.17	0.10	50	27
10	Fulton, GA	-0.14	0.30	-0.75	-1.05	43	84
11	St Louis City, MO	0.19	0.76	-1.00	-1.76	28	91
12	Kings, NY	0.77	1.57	2.11	0.53	4	2
13	Harris, TX	0.65	1.41	0.54	-0.86	5	14
14	Allegheny, PA	0.62	1.37	-0.24	-1.61	6	43
15	Cuyahoga, OH	0.35	0.98	-0.81	-1.80	20	87
16	Los Angeles, CA	0.40	1.06	-0.13	-1.18	17	35
17	Essex, NJ	0.61	1.35	1.12	-0.23	7	6
18	Duval, FL	0.02	0.51	-0.56	-1.07	35	72
19	Hamilton, OH	-0.07	0.39	-0.87	-1.26	41	89
20	Caddo, LA	-0.37	-0.03	-0.13	-0.10	55	37
Migrant-weighted average, large counties		0.16	0.72	-0.12	-0.84	–	–

Notes: Column 3 displays empirical-Bayes-adjusted place effects for the 20 counties with the largest Black population in 1940. Column 4 reports the standardized version of this variable. Column 5 reports the standardized measure of mean household income rank for Black children whose parents were at the 25th percentile of the national income distribution from Chetty et al. (2020). The migrant-weighted average in the bottom row and ranks in columns 7 and 8 are calculated among the 100 largest counties in terms of 1940 Black population. We do not use weights when standardizing variables.

Source: Authors' calculations using 1940 Census (Ruggles et al., 2020) and Chetty et al. (2020)

Table 6: Correlation of Place Effects from Different Selection Correction Specifications

	(1)	(2)	(3)	(4)
A: Full sample, origin state fixed effects (728 place effects)				
(1)	1.000			
(2)	0.999	1.000		
(3)	0.986	0.985	1.000	
B: Matched sample, origin state fixed effects (211 place effects)				
(1)	1.000			
(2)	0.998	1.000		
(3)	0.952	0.954	1.000	
(4)	0.959	0.959	0.993	1.000
C: Matched sample, origin county fixed effects (211 place effects)				
(1)	1.000			
(2)	0.994	1.000		
(3)	0.912	0.913	1.000	
(4)	0.913	0.913	0.989	1.000
Covariates included in column specification				
Father's education	X	X	X	X
Mother's education	X	X	X	X
Only father present		X	X	X
Only mother present		X	X	X
One parent born in different state		X	X	X
Both parents born in different state		X	X	X
Father's age			X	X
Mother's age			X	X
Number of children in household			X	X
1920 Census covariates				X

Notes: Table reports equally-weighted correlations of place effects based on different sets of variables in  $H_i$ . In specification (1),  $H_i$  includes indicators for father's and mother's education. In (2),  $H_i$  also includes indicators for whether only the mother is present, whether only the father is present, whether both parents are born in a different state, and whether one parent is born in a different state. In (3),  $H_i$  also includes indicators for parents' age in five-year intervals and number of children in the household. In (4), which is only possible with the matched sample,  $H_i$  also includes covariates from the 1920 Census: whether children's father was literate, whether he attended school, whether he lived in an urban area, whether he lived on a farm, how many siblings he had, and whether children's grandfather (observed in 1920) is literate, a farmer, and his Duncan socioeconomic index (a measure of income based on occupation).

Source: Authors' calculations using 1920 and 1940 Census (Ruggles et al., 2020)



Table 7: Robustness to Different Proportionality Constants

$C \equiv C_1 C_2$	Correlation with baseline place effects (1)	Standard deviation of place effects (2)	North-South difference (3)
0.5	0.983	0.947	1.071
0.6	0.988	0.925	1.021
0.7	0.993	0.903	0.972
0.8	0.997	0.883	0.922
0.9	0.999	0.865	0.872
1.0 (Baseline)	1.000	0.848	0.823
1.1	0.999	0.832	0.773
1.2	0.996	0.818	0.724
1.3	0.991	0.806	0.674
1.4	0.983	0.796	0.624
1.5	0.973	0.787	0.575

Notes: Table reports results from relaxing the key identifying assumptions, as described in Section 4.6. Column 1 reports the correlation of place effects with the baseline place effects, in which  $C = 1$ . Column 2 reports the equally-weighted standard deviation of place effects across counties. Column 3 reports the average North-South difference in place effects.

Source: Authors' calculations using 1940 Census (Ruggles et al., 2020)

Table 8: Correlates of 1940 Place Effects on Black Children's Education

	Dependent variable: Place effect, children's education						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Teachers per pupil	0.467*** (0.0371)					0.175*** (0.0321)	0.123*** (0.0315)
Median Black household income		0.619*** (0.0299)				0.427*** (0.0333)	0.361*** (0.0329)
Homicide rate			-0.413*** (0.0584)			-0.174*** (0.0408)	-0.0948** (0.0385)
Incarceration rate				0.0425 (0.0519)		-0.0103 (0.0260)	-0.0142 (0.0250)
NAACP chapter					0.430*** (0.0330)	0.156*** (0.0310)	0.117*** (0.0307)
South indicator							-0.479*** (0.0793)
Observations (counties)	728	728	728	728	728	728	728
R-squared	0.171	0.384	0.172	0.002	0.185	0.471	0.498

Notes: We normalize all variables to have mean zero and standard deviation one. All regressions include a series of indicators for whether variables are missing. Heteroskedasticity-robust standard errors in parentheses. See Appendix G for details on variable construction and sources. Statistical significance is denoted by: \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

Source: Authors' calculations using 1940 Census (Ruggles et al., 2020)

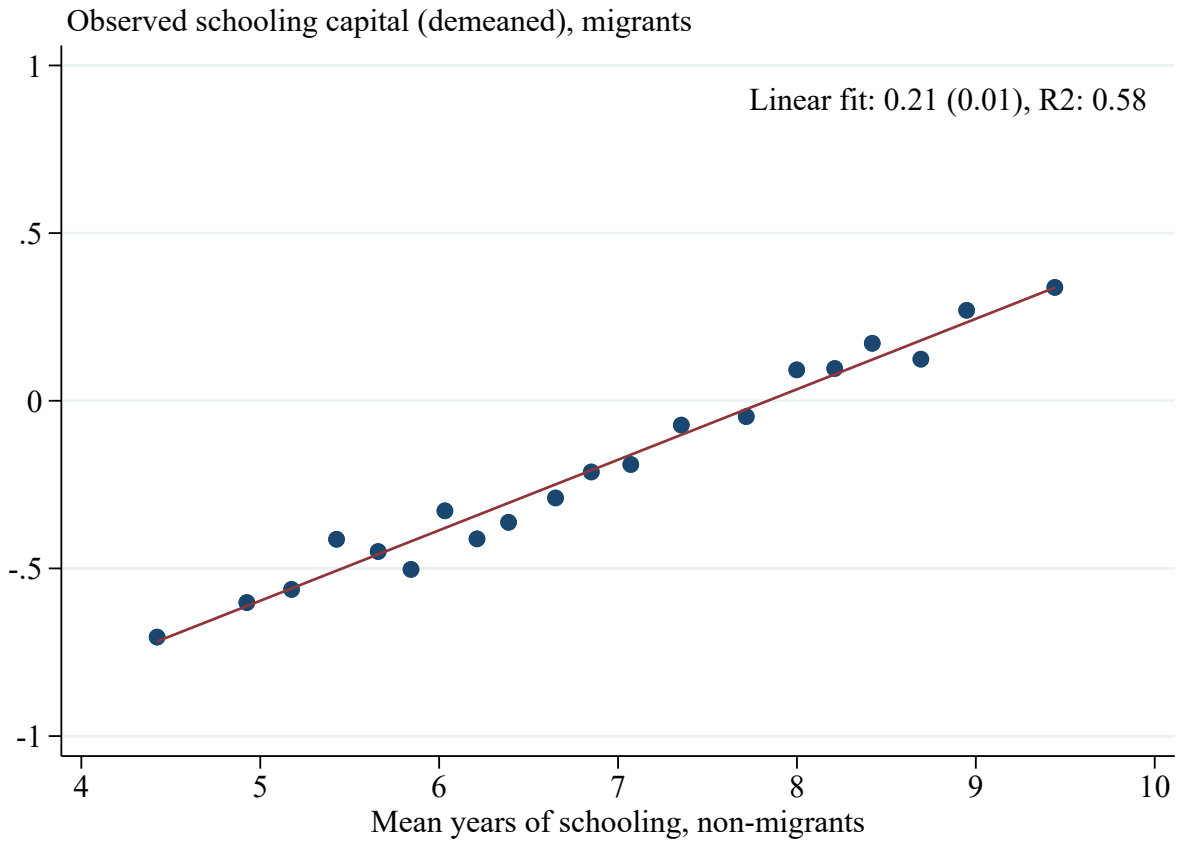
Table 9: Place Effects and Mechanisms, Within-Place Estimates

	Dependent Variable: $\Delta$ Opportunity Measure (1990s vs 1940)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\Delta$ Teachers per pupil	0.250*** (0.0314)						0.243*** (0.0306)	0.234*** (0.0305)	0.166*** (0.0362)
$\Delta$ Median Black household income		0.429*** (0.0429)					0.405*** (0.0416)	0.393*** (0.0420)	0.360*** (0.0429)
$\Delta$ Homicide rate			-0.161*** (0.0381)				-0.129*** (0.0396)	-0.0987** (0.0417)	-0.0694* (0.0400)
$\Delta$ Incarceration rate				-0.111*** (0.0285)			-0.123*** (0.0275)	-0.120*** (0.0272)	-0.151*** (0.0307)
$\Delta$ NAACP chapter					0.177* (0.105)		-0.0574 (0.0969)	-0.0679 (0.0979)	-0.0685 (0.0984)
$\Delta$ Percent Black						-0.445*** (0.0825)		-0.204** (0.0868)	-0.135 (0.0893)
South indicator									0.407*** (0.121)
Observations (counties)	728	728	728	728	728	728	728	728	728
R-squared	0.065	0.121	0.032	0.017	0.004	0.037	0.221	0.226	0.241

Notes: Separately for each year, we normalize all variables to have mean zero and standard deviation one. We then construct the change from 1940 to the 1990s, except for the change in the presence of a NAACP chapter, which is from 1940 to 1960. The dependent variable is the difference between Black upward mobility from Chetty et al. (2020) and place effects in 1940. All regressions include a series of indicators for whether variables are missing. Heteroskedasticity robust standard errors in parentheses. See Appendix G for details on variable construction and sources. Statistical significance is denoted by: \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

Source: Authors' calculations using 1940 Census (Ruggles et al., 2020) and Chetty et al. (2020)

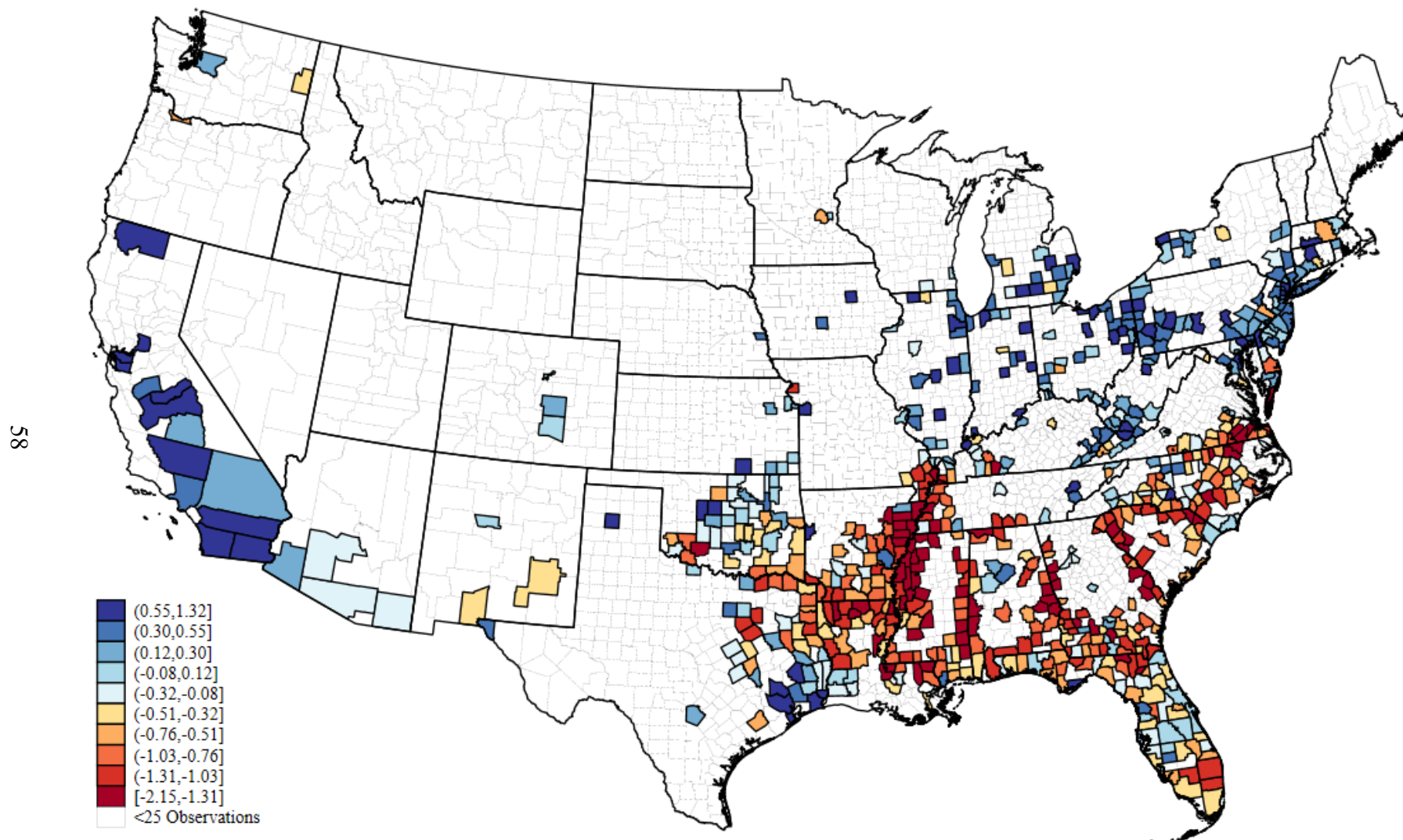
Figure 1: Observed Schooling Capital of Migrants and Educational Attainment of Non-Migrants, Black Children Age 14–18



Notes: Figure displays a binscatter of the demeaned observed schooling capital,  $\hat{h}_i = H_i \hat{\lambda}$ , of migrants against the average educational attainment of non-migrants. We include indicators for father's and mother's education in  $H_i$  in constructing  $\hat{h}_i$ . The positive slope indicates selection on observables, which motivates our use of a selection adjustment.

Source: Authors' calculations using 1940 Census (Ruggles et al., 2020)

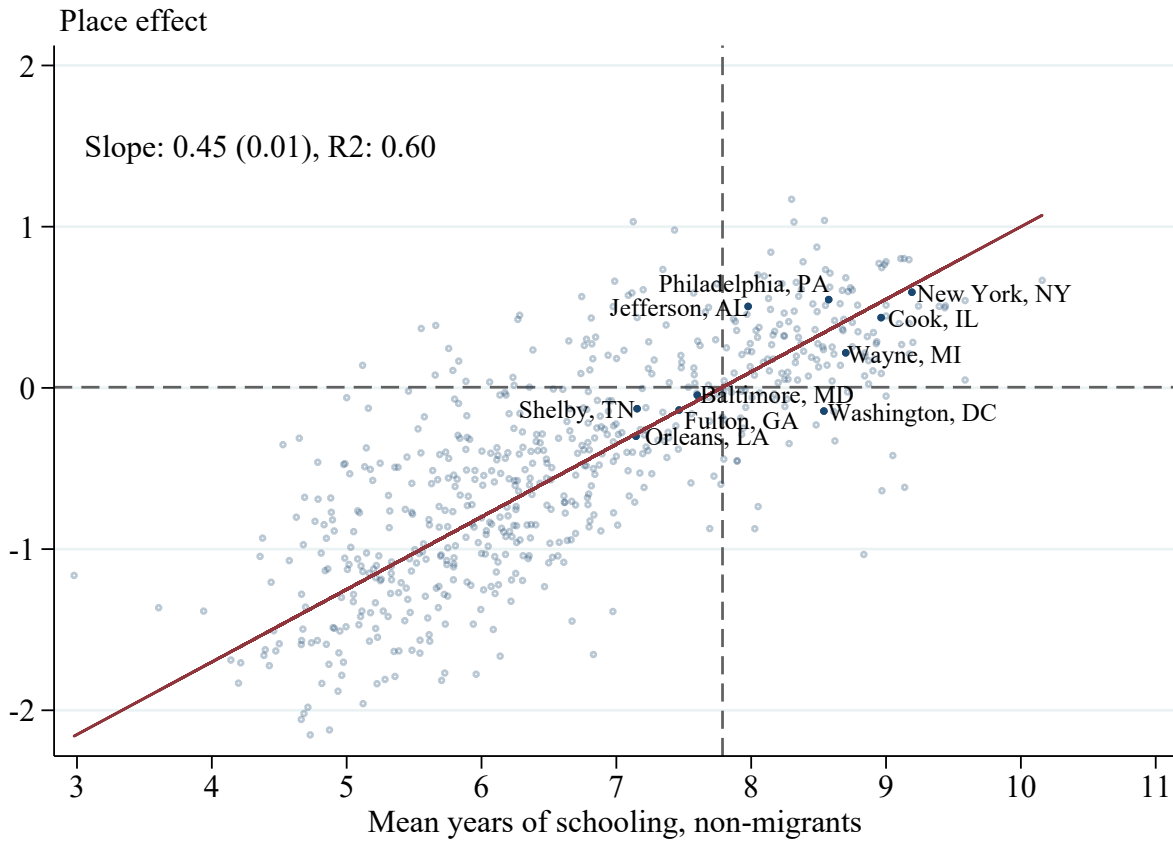
Figure 2: Place Effects on Years of Schooling in 1940, Black Children Age 14–18



Notes: Figure shows empirical-Bayes-adjusted place effects from our baseline specification. Counties with fewer than 25 Black migrant children are shaded in white.

Source: Authors' calculations using 1940 Census (Ruggles et al., 2020)

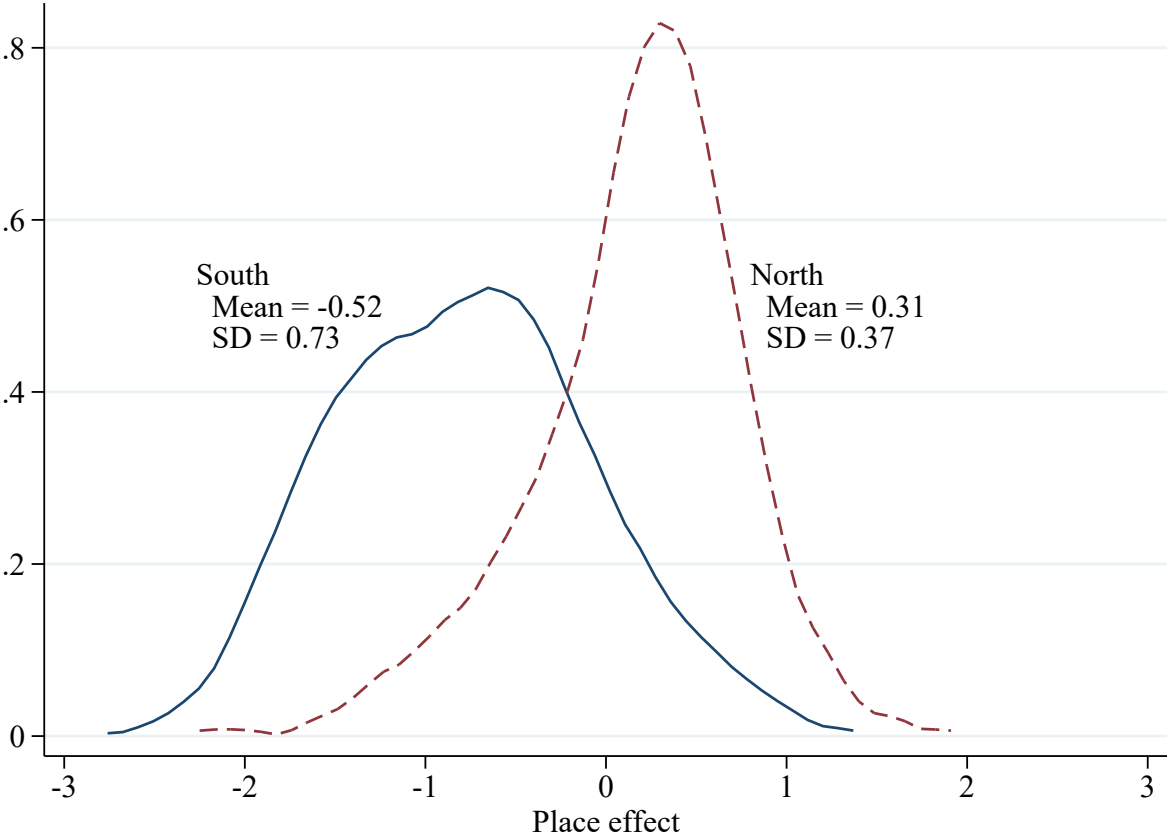
Figure 3: Place Effects versus Average Years of Schooling for Non-Migrants, Black Children Age 14–18



Notes: Figure displays empirical-Bayes-adjusted place effects against average years of schooling for non-migrants. Dashed lines are migrant-weighted averages (0.00 and 7.79). The ten largest counties in terms of 1940 Black population are labeled. To estimate the line of best fit, we use non-empirical-Bayes-adjusted place effects as the dependent variable.

Source: Authors' calculations using 1940 Census (Ruggles et al., 2020)

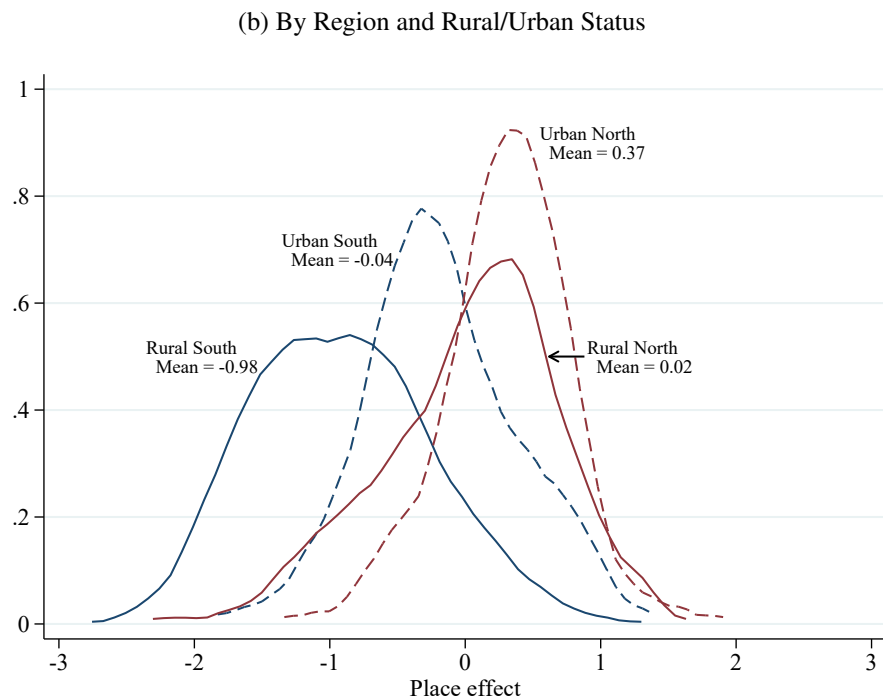
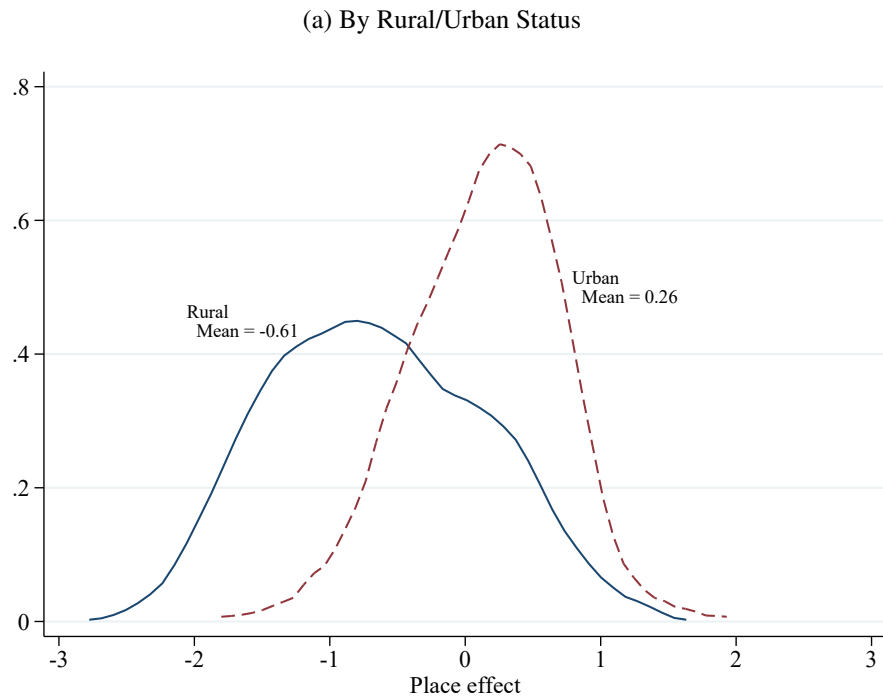
Figure 4: Distribution of Place Effects on Years of Schooling in South and North, Black Children Age 14–18



Notes: Figure shows density of place effect estimates in South and North. Migrant-weighted averages and standard deviations are reported.

Source: Authors' calculations using 1940 Census (Ruggles et al., 2020)

Figure 5: Distribution of Place Effects by Region and Rural/Urban Status, Black Children Age 14–18

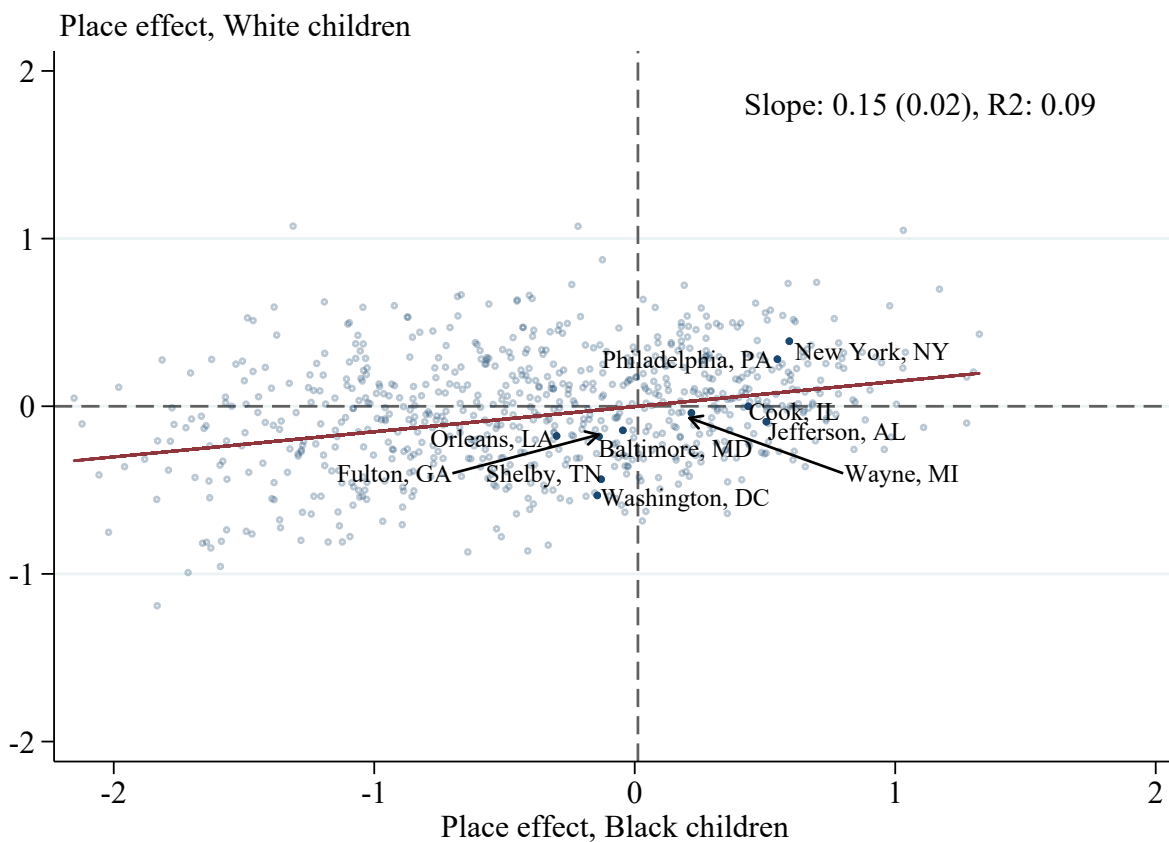


Notes: Figure shows density of place effect estimates in South and North for areas that are mostly urban (1940 percent urban above 50 percent) and mostly rural (1940 percent urban no more than 50 percent). Migrant-weighted averages are reported.

Source: Authors' calculations using 1940 Census (Ruggles et al., 2020)

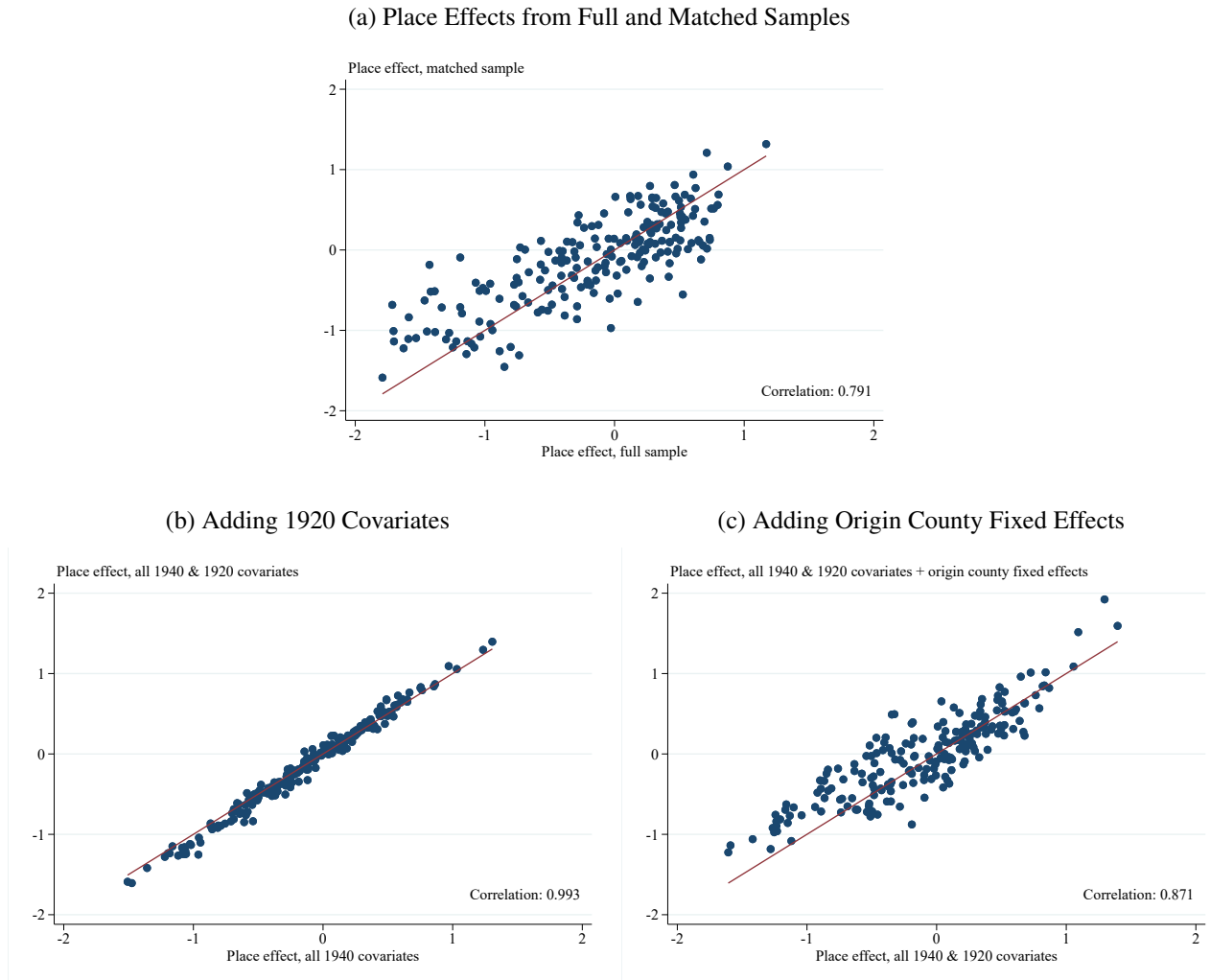


Figure 6: Place Effects for White versus Black Children Age 14–18



Notes: Figure displays empirical-Bayes-adjusted place effects for White and Black children. Dashed lines are migrant-weighted averages (0.00 and 0.00). The ten largest counties in terms of 1940 Black population are labeled. To estimate the line of best fit, we use non-empirical-Bayes-adjusted place effects.  
Source: Authors' calculations using 1940 Census (Ruggles et al., 2020)

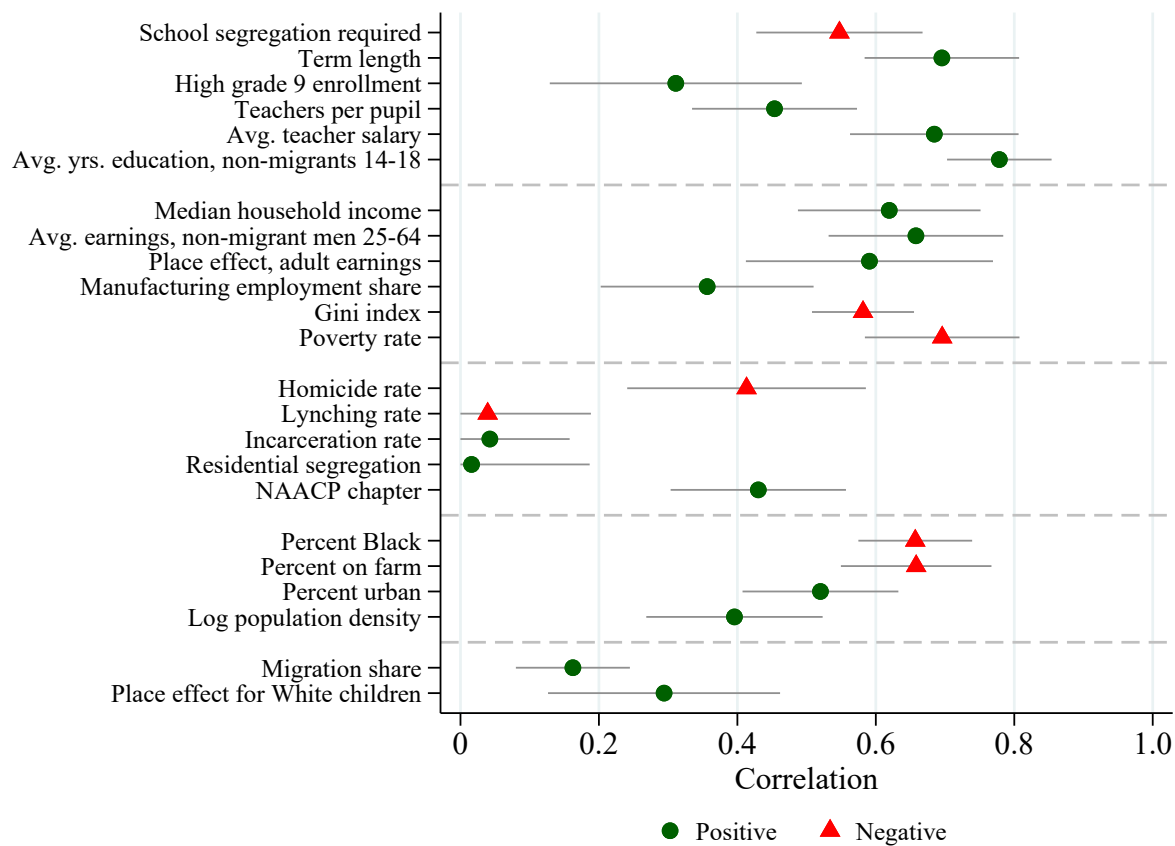
Figure 7: Robustness of Place Effects to Additional Selection Correction Variables from Matched Sample



Notes: Figure displays empirical-Bayes-adjusted place effects from different samples and specifications. Panel A plots place effects from our baseline specification using the full sample (x-axis) and matched sample (y-axis). Panel B plots place effects from the matched sample for the selection correction model that uses all 1940 covariates (x-axis) and the model that additionally uses 1920 covariates in  $H_i$ . Panel C plots place effects from the matched sample for the model that uses all 1940 and 1920 covariates with origin state fixed effects (x-axis) and the model that uses origin county fixed effects. We calculate correlations using non-empirical-Bayes-adjusted place effects.

Source: Authors' calculations using 1920 and 1940 Census (Ruggles et al., 2020)

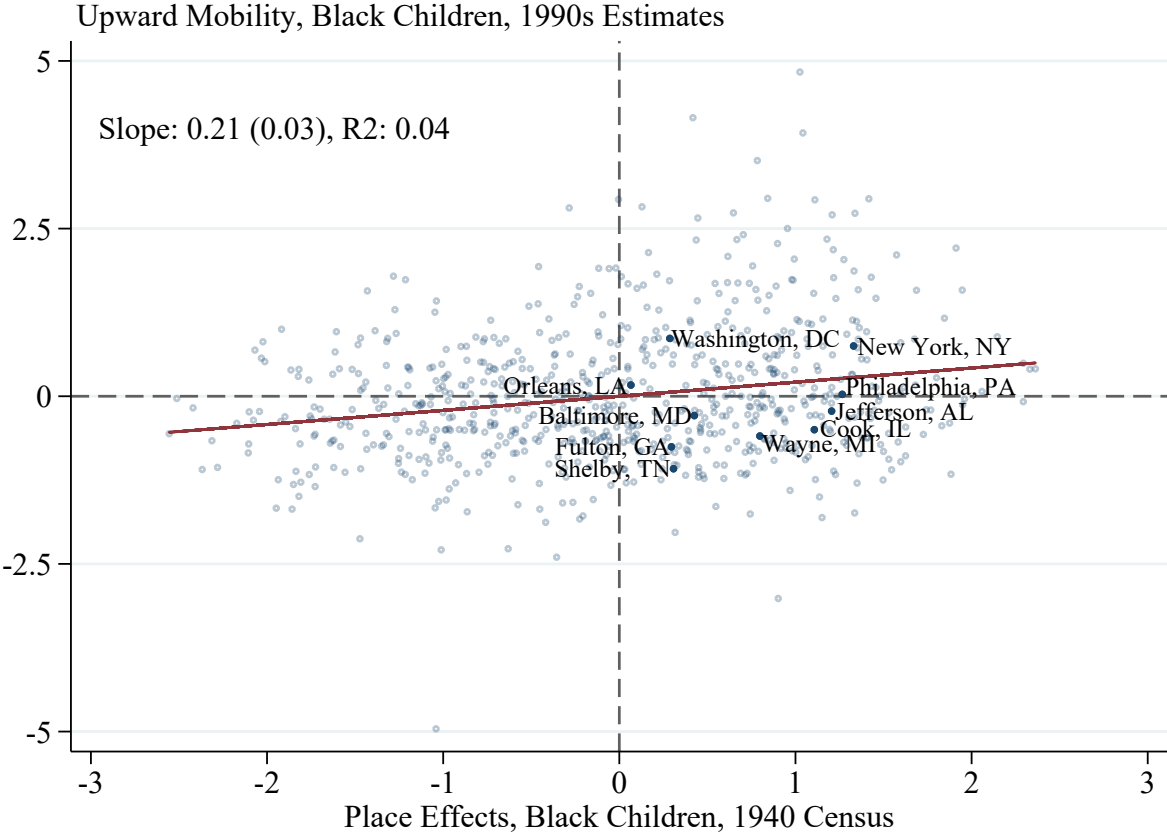
Figure 8: Full List of Correlates of Place Effects, 1940



Notes: Figure displays equally-weighted correlations between place effects on grade attainment and county characteristics. See Appendix G for details on variable construction.

Source: Authors' calculations using 1940 Census (Ruggles et al., 2020)

Figure 9: Relationship between 1940 Place Effects and 1990s Upward Mobility



Notes: Figure displays the scatterplot of normalized 1940 place effect estimates and normalized measures of upward mobility at the county-level. Upward mobility is the mean household income rank for Black children whose parents were at the 25th percentile of the national income distribution. Chetty et al. (2020) construct the upward mobility measure for children born between 1978 and 1983 who grew up during the 1980s and 1990s. We standardize place effect estimates and upward mobility measures so that normalized measures have a mean of zero and a standard deviation of one. Because the estimates from Chetty et al. (2020) are empirical-Bayes-adjusted, we use empirical-Bayes-adjusted place effects in 1940 for the figure and line of best fit.  
 Sources: Authors' calculations using 1940 Census (Ruggles et al., 2020) and Chetty et al. (2020).

# Online Appendix

## A Derivation of Selection Correction

Section 3 follows Finkelstein, Gentzkow and Williams (2021) and introduces the place effect selection correction as equation (5). This appendix reviews how this selection correction equation is derived from Assumptions 1 and 2 in Section 3.

Finkelstein, Gentzkow and Williams (2021) provide the following proposition:

**Proposition 1** *Assumption 1 is equivalent to*

$$\eta_j^{dest} = \frac{SD(\eta_j^{dest})}{SD(h_j^{dest})} h_j^{dest}.$$

*Proof:* Note that  $h_j^{dest}$  and  $\eta_j^{dest}$  are normalized to have mean zero, which implies that  $Cov(T_{ij}, h_j^{dest}) = \frac{N}{N'} h_j^{dest} p(1-p)$  and  $Cov(T_{ij}, \eta_j^{dest}) = \frac{N}{N'} \eta_j^{dest} p(1-p)$ , where  $N$  is the total number of migrants,  $N'$  is the number of migrants with  $T_{ij} = 0$ , and  $p = Pr(T_{ij} = 1)$ . Assumption 1 is then equivalent to:

$$\frac{\frac{N}{N'} h_j^{dest} p(1-p)}{SD(T_{ij})SD(h_j^{dest})} = \frac{\frac{N}{N'} \eta_j^{dest} p(1-p)}{SD(T_{ij})SD(\eta_j^{dest})}.$$

Proposition 1 follows immediately after canceling terms in the above expression.  $\square$

This proposition is helpful since it can be combined with Assumption 2 to obtain equation (5) in Section 3, which is the desired selection-correction expression.

## B Model of Selective Migration

This appendix describes a simple model of selective migration and describes how the selection correction procedure used in the paper adjusts observed patterns in the data to identify causal place effects.

In broad terms, the model features parents who care about their own income and the later-life income of their children. Places differ in the returns to parent human capital and the translation of child schooling capital into educational attainment. When the returns to parent human capital or child schooling capital are increasing in the underlying levels of human capital, there is selective migration in the sense that parents and children with higher levels of human capital move to locations with higher returns.

### B.1 Formal Model

Let  $i$  index a family, which consists of a parent and a child. The parent chooses the location that maximizes his or her utility. The parent's indirect utility from moving to location  $j$  is

$$u_{i,j} = W_{i,j}^p + \delta W_{i,j}^c - \kappa_j - e_{i,j}, \quad (\text{B1})$$

where  $W_{i,j}^p$  is the present discounted value of the parent's earnings if they live in location  $j$ ,  $W_{i,j}^c$  is the present discounted value of the child's (later-life) earnings if their parent moves to location  $j$ ,  $\delta \in [0, 1]$  is a parameter describing how much weight the parent places on the earnings of their child,  $\kappa_j \geq 0$  is a moving cost, and  $e_{i,j}$  is an idiosyncratic preference term. Parents can choose to remain in their origin location,  $o(i)$ , in which case the moving cost equals 0.

The parent has human capital  $\theta_i^p \geq 0$ , which is assumed to be fixed. The earnings of a parent that lives in location  $j$  are:

$$W_{i,j}^p = f(\theta_i^p; \phi_j), \quad (\text{B2})$$

where  $\phi_j$  is a vector that parametrizes earnings in location  $j$ , normalized so that  $\partial f / \partial \phi_j > 0$ . Parent human capital consists of years of schooling,  $S_i^p \geq 0$ , and an independent component,  $\eta_i^p \geq 0$ :

$$\theta_i^p = S_i^p + \eta_i^p. \quad (\text{B3})$$

The child's years of schooling if the parent moves to location  $j$  is  $S_{i,j}^c \geq 0$ . We assume that the parent considers the lifetime earnings of the child to be:

$$W_{i,j}^c = g(S_{i,j}^c; \psi), \quad (\text{B4})$$

where  $\psi$  is a vector that parametrizes earnings, normalized so that  $\partial g / \partial \psi > 0$ . Equation (B4) makes the simplifying assumption that parents focus on the first-order effect of how their location choice affects their child's schooling but not the second-order effect of how their location choice affects the return to schooling that their child will earn in the future. The years of schooling attained by a child are:

$$S_{i,j}^c = \theta_i^c + \gamma_j, \quad (\text{B5})$$

where  $\theta_i^c \geq 0$  summarizes the determinants of a child's educational attainment that do not depend on where the child lives, and  $\gamma_j$  is a place effect on years of schooling. We refer to  $\theta_i^c$  as the child's level of schooling capital. Equation (B5) is a direct analog of equation (1).

Child schooling capital can be correlated with parent human capital. We summarize this relationship as:

$$\theta_i^c = \rho \theta_i^p + \nu_i, \quad (\text{B6})$$

where  $\rho$  describes the degree of intergenerational persistence in human capital and  $\nu_i \geq 0$  is an independent component of child schooling capital. Equations (B3) and (B6) imply that child schooling capital can be correlated with parent schooling, which is a key feature for our empirical approach.

## B.2 Selective Migration in the Model

At this point, we can use the model to discuss selective migration. Plugging equations (B2), (B4), and (B5) into the indirect utility function in equation (B1) yields:

$$u_{i,j} = f(\theta_i^p; \phi_j) + \delta g(\theta_i^c + \gamma_j; \psi) - \kappa_j - e_{i,j}. \quad (\text{B7})$$

Equation (B7) summarizes the determinants of the parent's location decision. The presence of selective migration depends on the shape of the functions  $f(\cdot)$  and  $g(\cdot)$ . If the cross-partial derivative  $\partial^2 f / (\partial \theta_i^p \partial \phi_j) > 0$ , then a parent with a higher level of human capital gains more by moving to a destination with a better labor market (i.e., one with a higher  $\phi_j$ ) than a parent with a lower level of human capital. As a result, there will tend to be selective migration in terms of parent human capital. Selective migration in terms of child schooling capital—so that children with higher levels of schooling capital,  $\theta_i^c$ , tend to live in places with higher place effects on their schooling,  $\gamma_j$ —similarly requires that  $\partial^2 g / (\partial \theta_i^c \partial \gamma_j) > 0$ . Given the functional form in equation (B5), this cross-partial derivative will be positive if  $g(\cdot)$  is a convex function. The value of  $\delta$  is likely to be less than 1, both because parents are not perfectly altruistic and because children's earnings are discounted in present value terms.

To more concretely demonstrate selection into migration, let us briefly consider a version of the model that has three simplifying assumptions. First, we specify functional forms for the earnings of parents and children. Motivated by the large literature which models log earnings as a linear function of years of schooling following Mincer (1958), we assume that the earnings of the parent and child are:

$$W_{i,j}^p = \exp(\phi_j^0 + \phi_j^1 \theta_i^p), \quad (\text{B8})$$

$$W_{i,j}^c = \exp(\psi^0 + \psi^1 S_i^c). \quad (\text{B9})$$

Equation (B8) allows for the possibility that locations differ both in the earnings received by all individuals, as captured by the intercept term  $\phi_j^0 \geq 0$ , and the possibility that locations differ in the return to parent human capital, as captured by the slope term  $\phi_j^1 \geq 0$ . These functional forms generate selective migration in terms of both parent human capital and child schooling capital. Second, we assume there are just two locations: a single destination  $j$  and a single origin  $o$ . Third, we assume that parents do not care about their child's educational attainment (i.e.,  $\delta = 0$ ).

In this simplified version of our model, a parent prefers migrating to location  $j$  over staying in the origin if

$$\exp(\phi_j^0 + \phi_j^1 \theta_i^p) - \kappa_j + e_{i,j} > \exp(\phi_o^0 + \phi_o^1 \theta_i^p) + e_{i,o}, \quad (\text{B10})$$

which says that the return to migration in terms of higher earnings outweighs the moving cost and idiosyncratic preference difference. If the labor market returns in location  $j$  are higher than those in the origin ( $\phi_j^0 > \phi_o^0$  and  $\phi_j^1 > \phi_o^1$ ), then all else equal there will be a threshold level of parent human capital such that parents with  $\theta_i^p$  above this threshold will prefer location  $j$ .<sup>56</sup> Intuitively,

<sup>56</sup>This can be seen by rearranging equation (B10) so that the left-hand side is an increasing function of  $\theta_i^p$  and the right-hand side does not depend on  $\theta_i^p$ :  $\exp(\phi_j^0 + \phi_j^1 \theta_i^p) - \exp(\phi_o^0 + \phi_o^1 \theta_i^p) > \kappa_j - (e_{i,j} - e_{i,o})$ . The threshold level of parent human capital is a function of location-specific earnings returns ( $\phi_j^0, \phi_j^1, \phi_o^0, \phi_o^1$ ), moving costs ( $\kappa_j$ ), and idiosyncratic preferences ( $e_{i,j}, e_{i,o}$ ).

parents with a higher level of human capital are willing to pay the moving cost because they benefit by more from the higher return in labor market  $j$ . In a more general scenario where parents also care about their child's educational attainment, there could be selective migration in terms of both parent human capital and child schooling capital. We discuss the implications of selection in terms of child schooling capital in detail below.

### B.3 Adjusting for Selective Migration to Estimate Place Effects

We use this model to describe how the selection correction procedure used in the text adjusts observed patterns in the data to estimate causal place effects.

For simplicity, we assume that there are two destinations,  $j \in \{L, H\}$ , with location  $H$  having a higher place effect on children's schooling:  $\gamma_H > \gamma_L$ . Using equation (B5), the difference in average education levels between migrant children that live in the two destinations depends on both selection and place effects:

$$\underbrace{E[S_{i,j}^c | j(i) = H] - E[S_{i,j}^c | j(i) = L]}_{\text{Observed difference in child schooling}} = \underbrace{E[\theta_i^c | j(i) = H] - E[\theta_i^c | j(i) = L]}_{\text{Selection on unobserved child schooling capital}} + \underbrace{\gamma_H - \gamma_L}_{\text{Place effects}}. \quad (\text{B11})$$

The most natural concern in our setting is that the average level of children's unobserved schooling capital in destination  $H$  is higher than in destination  $L$ :  $E[\theta_i^c | j(i) = H] > E[\theta_i^c | j(i) = L]$ . This implies that a simple comparison of education levels would overstate the difference in place effects.

There are two distinct reasons why children's unobserved schooling capital might be higher in the destination with a higher place effect in this model. The first reason is that parents care about their child's long-run outcomes when deciding where to move (i.e.,  $\delta > 0$ ) and the long-run earnings return for a child of moving to a destination with a higher schooling place effect is increasing in the level of child schooling capital (i.e.,  $\partial^2 g / (\partial \theta_i^c \partial \gamma_j) > 0$ ). The second reason is that parents care about their own earnings when deciding where to move, a parent with a higher level of human capital will gain more earnings by moving to a destination with a better labor market (i.e.,  $\partial^2 f / (\partial \theta_i^p \partial \phi_j) > 0$ ), destinations that have higher place effects on child schooling also have higher wage returns to parents (i.e.,  $\phi_H^0 > \phi_L^0$  and  $\phi_H^1 > \phi_L^1$ ), and there is a positive correlation between parent human capital and child schooling capital (i.e.,  $\rho > 0$ ). The functional forms in equations (B8) and (B9) generate both forms of selective migration.

These same reasons suggest that parent human capital will also be higher in destination  $H$ :  $E[\theta_i^p | j(i) = H] > E[\theta_i^p | j(i) = L]$ . Although parent human capital is not observed, we can observe parents' education level. As long as parents' education displays the same pattern of selection as parents' human capital, we have a situation where destination  $H$  features both higher levels of child schooling capital (which is not observed) and higher levels of parent education (which is observed). In this simple example, this implies that

$$\underbrace{E[\theta_i^c | j(i) = H] - E[\theta_i^c | j(i) = L]}_{\text{Selection on unobserved child schooling capital}} = \alpha \times \underbrace{E[S_i^p | j(i) = H] - E[S_i^p | j(i) = L]}_{\text{Selection on observed parent schooling}}, \quad (\text{B12})$$

for some positive constant  $\alpha$ . Given a value for  $\alpha$ , we could use equations (B11) and (B12) to identify place effects in this simple example. This example describes the basic intuition behind approaches that use selection on observed variables to address selection on unobserved variables



(Altonji, Elder and Taber, 2005; Oster, 2019; Finkelstein, Gentzkow and Williams, 2021). A key benefit of the approach of Finkelstein, Gentzkow and Williams (2021) is that it uses additional moments of the data to quantify how selection on observables translates into selection on unobservables. Because we study a setting where there is potential selection on both parent and child human capital, it is not straightforward to derive further analytical expressions for the estimator developed by Finkelstein, Gentzkow and Williams (2021). In Section 4.6, we probe the robustness of our results to different assumptions about this scaling factor.

## C Empirical Bayes Adjustment

When reporting county-specific place effects, we use an empirical Bayes adjustment to account for finite sample bias. This section provides details on the adjustment, following Chetty and Hendren (2018b) and Finkelstein, Gentzkow and Williams (2021).

Let  $\gamma_j$  be the true education place effect, which is normalized to have mean 0. Let  $M$  be the average causal place effect (which is 0 by construction). We assume that  $\gamma_j$  is a normally distributed random variable:

$$\gamma_j = M + \eta_j, \tag{C1}$$

with  $\eta_j \sim N(0, \chi^2)$ .

We assume that estimates of  $\gamma_j$  are measured with idiosyncratic error:

$$\hat{\gamma}_j = \gamma_j + \nu_j, \tag{C2}$$

where the estimation error is  $\nu_j \sim N(0, s_j^2)$  and  $s_j$  is the standard error of  $\hat{\gamma}_j$ .

Based on equations (C1) and (C2), we have:

$$\hat{\gamma}_j = M + \eta_j + \nu_j, \tag{C3}$$

where  $\eta_j$  is assumed to be independent of  $\nu_j$ . This implies that

$$\chi^2 = \text{var}(\eta_j) = \text{var}(\hat{\gamma}_j) - \text{var}(\nu_j) \tag{C4}$$

$$= \text{var}(\hat{\gamma}_j) - E(s_j^2). \tag{C5}$$

Equation (C5) yields an estimate of  $\chi^2$  using the variance of estimated place effects and the average standard error of place effects. We calculate standard errors for each place effect using the bootstrap.

In this framework, we can compute optimal predictions  $\gamma_j^{EB}$  for each county  $j$  by minimizing the mean squared prediction error:

$$\sum_{j=1}^J (\gamma_j^{EB} - \gamma_j)^2. \tag{C6}$$

We write the true causal effect of moving to  $j$  as:

$$\gamma_j = \beta_{1j}M + \beta_{2j}\hat{\gamma}_j. \quad (\text{C7})$$

We cannot estimate this regression (since  $\gamma_j$  is unobserved), but the hypothetical regression would allow us to recover  $J$ -many  $\beta_{1j}$  and  $\beta_{2j}$  coefficients that would allow us to compute optimal predictions  $\gamma_j^{EB}$  that minimize the mean squared prediction error:

$$\gamma_j^{EB} = \hat{\beta}_{1j}M + \hat{\beta}_{2j}\hat{\gamma}_j. \quad (\text{C8})$$

While  $\beta_{1j}$  and  $\beta_{2j}$  cannot be estimated directly, we can construct them using the assumptions in equations (C1) and (C2). Specifically, we have

$$\gamma_j^{EB} = \left( \frac{\chi^2}{\chi^2 + s_j^2} \right) \hat{\gamma}_j + \left( \frac{s_j^2}{\chi^2 + s_j^2} \right) M. \quad (\text{C9})$$

Since we assume  $M = 0$ , this simplifies to:

$$\gamma_j^{EB} = \left( \frac{\chi^2}{\chi^2 + s_j^2} \right) \hat{\gamma}_j. \quad (\text{C10})$$

We use equation (C10) to construct empirical-Bayes-adjusted place effects. We estimate  $\chi^2$  using equation (C5),  $s_j^2$  using the bootstrap, and  $\hat{\gamma}_j$  using the selection correction described in Section 3. The empirical Bayes adjustment shrinks place effects with larger standard errors towards the grand mean, which is zero.

## D Matched Sample Details

We create our matched sample by linking men from the 1920 and 1940 full count Censuses, provided by IPUMS and accessed through the NBER server. We start with U.S.-born Black men age 3–52 in the 1920 full count Census. Following Abramitzky et al. (2021a), we link these men to themselves in 1940 using the following procedure<sup>57</sup>:

1. In each dataset, we clean first and last names to remove any non-alphabetic characters and standardize nicknames.
2. We link individuals from 1920 to 1940 in the following way:
  - (a) For each 1920 record, we look for records in the 1940 data that match on cleaned first and last name, race, birth state, and exact birth year. If the match is unique, then we call this pair a match. If there is more than one observation, then we drop the 1920 record from the search and call it unmatched.
  - (b) For the remaining records for which we did not find matches in the previous step, we search for a unique match within  $+/-1$  year of the birth year in 1920. We only accept unique matches.

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<sup>57</sup>In our linking procedure, we also download and use command files from Abramitzky et al. (2021b).

- (c) We repeat the previous step by looking for a unique match within  $\pm 2$  years. If the record still has no unique match, then we call it unmatched.
3. We repeat the same procedure in (2), but this time we link individuals from 1940 to 1920. In the 1940 Census, we restrict the sample to U.S.-born Black men age 25–70.
4. We take the intersection of the two linked samples.

For our analysis, we focus on U.S.-born Black men who were age 3–52 and living in their birth state in 1920.<sup>58</sup> Our match rate for this group is 14.1 percent. This number is similar to match rates for African Americans in the literature. Eriksson (2019), who links Black men in 1940 to the 1900, 1910, or 1920 Censuses, obtains a match rate of 18.6 percent. This is slightly higher than our match rate because some men have two chances to be matched.

Appendix Table 4 shows that, relative to all U.S.-born Black men who were age 10–50 and living in their birth state in 1920 (Column 1), the subset of men in the matched sample (Column 2) are more likely to be literate and have higher likelihood of living in urban areas in the 1920 Census. The fathers of men in our matched sample also have higher socioeconomic status. These differences are statistically significant at the one-percent level, which is not surprising given the very large samples. However, the differences are quite small in magnitude (e.g., the literacy gap is 4.3 percentage points, which is about 6 percent of the average literacy rate).

Our analysis of place effects focuses on children age 14–18 observed in the 1940 Census. To study selection for this sample, we first examine Southern-born Black men who are age 25–70 in the 1940 Census. These are potential fathers of children in our sample. Column 1 of Appendix Table 5 reports statistics for this group, and column 2 reports statistics for the matched sample subset age 25–70. Relative to all Black household heads, men in the matched sample have 0.3 more years of schooling (6 percent) and \$27 more earnings (6 percent). Columns 4 and 5 report statistics for children of the samples in columns 1 and 2. The matched subset has 0.2 more years of schooling (3 percent). Although these differences are statistically significant, the magnitude of the differences are substantively small.

## **E Alternative Approaches to Estimating the Effect of Moving North**

Section 3.3 compares our approach to estimating the effect of moving North—averaging county-specific place effects—and a common approach in prior work—estimating the coefficient on a North indicator. This section describes results from these approaches.

Appendix Table 6 reports results from regressions where the dependent variable is years of schooling for Black children age 14–18. We use the matched sample for this analysis and begin by focusing on children whose parents are born in the South to be consistent with past work. Column 1 shows that children whose parents moved to the North have 2.2 additional years of schooling than children whose parents remained in the South. In column 2, we control for the child’s age and sex, plus indicators for the father’s and mother’s years of schooling and the head of household’s 1920 state of residence. Including these variables reduces the difference considerably, to 1.4 years of schooling, indicating the presence of selection. In column 3, we limit the sample to individuals residing in a county with at least 10 migrants, to be consistent with the restriction used in our

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<sup>58</sup>Allowing for the  $\pm 2$  year difference, this produces a matched sample ages 25–70 in 1940.

preferred approach. This restriction mainly eliminates counties in the South with few migrants. The estimated North-South difference falls to 1.0 years, consistent with these counties having less-educated children.

The regression in column 3 controls for a limited number of covariates, which raises concern about selection driving these results. In column 4, we add fixed effects for the head of household's 1920 county of residence. The estimated difference remains large at 1.0 years. In column 5, we focus on a sample of children for whom a cousin is observed (by virtue of their fathers being in the same 1920 household). This sample restriction does not change the estimated difference. Column 6 adds fixed effects for the father's 1920 household. These fixed effects absorb all differences in children's education that are common across 1920 family lines. In this case, the point estimate falls to 0.37, and the standard error nearly triples. The sensitivity of the results for children contrasts to estimates for adults, where household fixed effects generally have little impact (Collins and Wana-maker, 2014; Boustan, 2017). The specification with household fixed effects is quite demanding of the data, and the results in column 6 do not provide conclusive evidence on the effects of moving North on children's education.

To focus on specifications that are more comparable to our selection-correction approach, the model used for column 7 allows state of origin fixed effects to differ by migrant status and includes county of residence fixed effects for non-migrants (which follows equation (3)). This leads to an increase in the coefficient, to 1.2 years. In column 8, we use the same sample and control variables, but we calculate the implied North-South difference by estimating a regression that replaces the North indicator with 1940 county of residence fixed effects for migrants and constructing averages, as in equation (6). The implied North-South difference is identical from this approach. This highlights the fact that it is possible to aggregate fixed effects to recover the previously-estimated moving North parameter.<sup>59</sup> Finally, columns 9 and 10 do not restrict the sample to children whose parents are born in the South, yielding extremely similar results when estimating a North indicator or aggregating fixed effects. The results in column 10 are the main point of interest because the associated model relies on cross-state migration as a source of identifying variation. Recall that the selection-correction approach from Section 3 also relies on these types of comparisons.

Notably, the implied North-South difference in column 10 is 1.2 years, which is considerably larger than the 0.8 year difference reported in Figure 4. To explain this gap, we turn to Appendix Table 7, where column 1 repeats the estimate from column 10 of Appendix Table 6. The implied North-South difference depends on estimated place effects and weights that reflect the number of observations in each county, as shown in equation (6). The table shows that results are extremely similar when using the full sample to construct observation weights and place effects for counties in the matched sample (columns 2 and 3). In contrast, adjusting for selection on unobservables matters considerably: the North-South difference in column 4 falls by 39 percent, from 1.2 years to 0.7 years. The similarity of the column 4 estimate to the full sample estimate in Figure 4 implies that our focus on the matched sample does not explain the discrepancy in the North-South difference.<sup>60</sup> The empirical Bayes adjustment, shown in column 5, leads to only a slight decrease in this difference.

In sum, Appendix Tables 6 and 7 highlight the importance of adjusting for selection on unob-

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<sup>59</sup>In a regression without covariates, the North coefficient is equal to the difference in average place effects. With covariates, the equality need not be exact.

<sup>60</sup>The estimate in column 4 of 0.7 years is slightly smaller than the estimate of 0.8 years in Figure 4. This is because the full sample contains more counties than the matched sample.

servables. This is possible with our approach, which also allows us to examine heterogeneity in place effects across counties.

## F Bounding Exercise to Account for Potential Mortality Effects

This section conducts a bounding analysis to account for selective survival of children. The motivation for this exercise is based on prior research that highlights the potential for migration from the rural South to the urban North during the early 20th century to increase Black infant mortality (Eriksson and Niemesh, 2016). Our approach computes bounds on county-level place effects that account for the fact that we can only estimate place effects on children who survive.

We begin by writing the true place effect as the weighted average between children who do and do not survive:

$$\gamma_j^* = p_j^{Die} \gamma_j^{Die} + (1 - p_j^{Die}) \gamma_j, \quad (F1)$$

where  $\gamma_j^*$  is the true, unobserved place effect, and  $p_j^{Die}$  is the share of children whose parents moved to county  $j$  but died before aging into our sample, which contains individuals ages 14–18. The place effect among this group is  $\gamma_j^{Die}$ , while  $\gamma_j$  is the place effect in our sample of children observed in the 1940 Census, defined in Section 3.

The key challenge to evaluating equation (F1) is that we cannot estimate place effects for individuals who die before reaching our sample age criteria. However, we can construct an upper and lower bound for  $\gamma_j^*$  by making extreme assumptions about  $\gamma_j^{Die}$ . In particular, we assume that  $\gamma_j^{Die}$  is bounded from above by the maximum estimate of  $\gamma_j$  in our sample. We also assume that  $\gamma_j^{Die}$  is bounded from below by the minimum estimate of  $\gamma_j$  in our sample. This leads to upper and lower bounds:

$$\gamma_j^{UB} = p_j^{Die} \gamma^{UB} + (1 - p_j^{Die}) \gamma_j \quad (F2)$$

$$\gamma_j^{LB} = p_j^{Die} \gamma^{LB} + (1 - p_j^{Die}) \gamma_j. \quad (F3)$$

The ideal estimate of  $p_j^{Die}$  is the share of children whose parents move to county  $j$  and do not survive to age 14. Unfortunately, data to construct this estimate do not exist. Instead, we use infant mortality data from Bailey et al. (2018).<sup>61</sup> Infant mortality rates were considerably higher than child mortality rates, which could lead this approach to overstate the potential importance of mortality.

As an initial examination of the nature of selective mortality, Panel A of Appendix Figure 12 plots the infant mortality rate and our main place effect estimates. The infant mortality rate is lower in counties with higher place effects. However, this relationship is modest in size, as a one-year increase in place effects is associated with a 0.4 percentage point decrease in the infant mortality rate (whose average is 6.2 percent in our sample of counties). This correlation provides little reason to worry that our estimates of positive place effects stem mainly from higher mortality rates.

We summarize our results by calculating the average upper and lower bounds of place effects for counties in the South and North. Panel B of Appendix Figure 12 shows that the average

<sup>61</sup>We use county-level infant mortality rates calculated from 1933–1937. Note that 1933 is the first year where we can observe infant mortality rates for all counties in our sample.

bounds are relatively narrow. In the South, the migrant-weighted average upper and lower bounds are  $-0.36$  and  $-0.66$ , respectively. In the North, the migrant-weighted average upper and lower bounds are  $0.39$  and  $0.15$ , respectively. These estimates suggest that the effect of moving North is at least a 0.51-year increase in schooling, and no more than a 1.05-year increase. Given the conservative nature of these bounds, we view the similarity of our main estimate—a 0.83-year increase in schooling—as reassuring.

## **G Sources and Details for County-Level Measures**

This appendix provides definitions and sources for the county-level measures used in our analysis.

### **G.1 Schooling**

We create measures of historical schooling and school quality using a variety of sources. For 1940, we compute average years of schooling for non-migrant Black individuals (i.e., individuals in a household where the head still lives in his/her state of birth in 1940) ages 14–18 using the complete count Census.

We measure school quality for African Americans in 1940 using two different school resource data sets. For ten Southern states with segregated schools, we construct race-specific school quality variables using county-level data for the year 1939–1940 from Carruthers and Wanamaker (2019). For other states, we construct county-level school quality variables (for all races) for the year 1939–1940 using city-level data from Biennial Surveys of Education (U.S. Office of Education, 1947). These surveys contain data for cities with at least 10,000 residents, and we aggregate cities within a county.<sup>62</sup> As a result, we do not have data on counties where there is no city with at least 10,000 residents in 1940, and the data do not represent school quality in rural areas. We believe that this is a minor limitation, as 88 percent of African Americans in the North lived in urban areas in 1940. More Black students attended rural schools in the South, but the Carruthers and Wanamaker (2019) data cover these schools. We compute county-level averages for teacher salary, number of teachers per pupil, and term length. We impute variables using nearby years when necessary. Unfortunately, school resource data are not available for Florida for the relevant years. In addition to the states covered in Carruthers and Wanamaker (2019), schools were segregated in Delaware, the District of Columbia, Maryland, Missouri, Oklahoma, Virginia, and West Virginia. Since race-specific data are not available, we use Biennial Survey data for these states. We also construct an indicator variable for school segregation being required by law in 1940 by using Jim Crow laws by state from Sutherland (1955).

We also measure teachers per pupil in modern times. County-level data on teachers per pupil are not available in 1990. We use the NCES Common Core Data on teachers per pupil for 2000, the earliest academic year that is available and features nearly complete coverage for our sample of counties.<sup>63</sup>

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<sup>62</sup>We use geographic crosswalks from U.S. Cities Database (n.d.) to match cities with counties by city and state name.

<sup>63</sup>For Massachusetts, Tennessee, and New York City boroughs, we use teacher pupil ratios from 2003, 2004 and 2005, respectively. Teacher pupil ratios were not reported for Buffalo, SD; Issaquena, MS; and Winkler, TX. For each of these counties, we use information available on the adjacent counties that were served by the same school district. Additionally, we use teacher pupil ratios from different years for several Virginia counties and independent

Finally, we also use the 1940 Census to create measures of high grade 9 enrollment at the county-level. This is defined based on the ratio of ninth to eighth grade enrollment for Black children ages 12 to 17. We create an indicator for high grade 9 enrollment based on whether the ratio of ninth to eighth grade enrollment is at least 0.5; our results are similar if we also define the threshold to be 0.25. This measure proxies for access to secondary schools. To the best of our knowledge, county-level data on the availability of secondary schools for Black children are not available.

## G.2 Local Economic Conditions

We use the complete count 1940 Census from Ruggles et al. (2020) and summary files from the 1990 Census and 2005–2009 American Community Survey from Manson et al. (2019) to create measures of median household income, average earnings, manufacturing employment, the Gini index of income inequality, and the poverty rate.

We calculate median household income in 1940 as follows. We begin with the complete count Census data and remove all individuals in group quarters. We impute earnings for individuals who are self-employed.<sup>64</sup> We sum up all earned income at the household level and construct the county-level median. For 1990, we use Census summary files.

We construct average wage and salary income for non-migrant Black men in 1940 (i.e., individuals in a household where the head still lives in his/her state of birth in 1940). We restrict our sample to all non-migrant Black men ages 25–64 and drop individuals with missing income.

To calculate the manufacturing employment share in 1940, we first remove anyone who reports an industry that is “N/A,” “Housework at home,” “School response (students, etc.),” “Retired,” or “Non-industrial response.” We then classify as employed in manufacturing anyone with an industry code (1950 basis) that takes a value between 300 and 500. For 1990, we create the same measure using the Census summary files.

We construct poverty rates in the 1940 Census following Barrington (1997). We measure poverty at the family level instead of the household level.<sup>65</sup> We calculate family income using wage and salary income for wage earners and imputed income for the self-employed (as described above). We assign a 1939 poverty threshold to each family based on gender of household head, farm status, family size and number of children (Barrington, 1997, Table 1). We remove families with more than nine members as no poverty line was defined for larger families. We compute county-level poverty rates as the share of families whose income is below the corresponding

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cities: Staunton City (1998); Charles City, King William, Lancaster, Williamsburg City (2001); Alleghany, Bedford City, Emporia City, Fairfax City (2002); and Clifton Forge City (2006).

<sup>64</sup>The 1940 Census contains only wage and salary earnings. To impute income for the self-employed, we use 1960 Census data from Ruggles et al. (2021) on individuals age 18–64 who are not currently enrolled in school, not in group quarters or on active military duty, and for whom occupation is not missing. We measure median earned income for each race (Black or White), region (of which there are four), and occupation (1950 basis) cell. If there are fewer than 10 observations in a region-race-occupation cell, we use median earned income by region and occupation. Then, we calculate where in the distribution of 1960 wage and salary income each median earnings value falls. Our earned income imputation equals the appropriate percentile of the 1940 wage and salary income distribution.

<sup>65</sup>We use the “famunit” variable for this purpose. For instance, we count each hired hand or servant and his/her family as a separate unit if they are not related to the head. We ensure that every member is related to each other in a family unit by using the “relate” and “sfrrelate” variables. One exception is that we assign any individual in a single-member family unit to the primary family in the household if that individual is 14 or younger.

poverty line.<sup>66</sup>

To measure income inequality, we compute Gini coefficients. We use the complete count Census data for 1940. We begin with all family units that we defined to calculate poverty rates above. We restrict our sample to family units with children (defined as having at least one member age 14 or younger). Following Chetty et al. (2014), we compute the Gini coefficient in county  $j$  as:

$$\text{Gini}_j = \frac{2\text{Cov}(X_{ij}, P_{ij})}{\bar{X}_j}$$

where  $\bar{X}_j$  is the mean family income in county  $j$  and  $\text{Cov}(X_{ij}, P_{ij})$  is the covariance between family income ( $X_{ij}$ ) and the percentile rank ( $P_{ij}$ ) of family  $i$  in county  $j$ . We estimate  $\text{Cov}(X_{ij}, P_{ij})$  by regressing percentile rank ( $P_{ij}$ ) on family income ( $X_{ij}$ ) for each county and multiplying the estimated coefficient by the variance of family income in county  $j$ . To measure income inequality for the later period, we use Census-produced Gini coefficients from the 2005–2009 American Community Survey.

### G.3 Crime and Social Capital Measures

Ideally, we would measure homicides in 1940 to align with our other variables, but these data are not available.<sup>67</sup> Instead, we use annual homicide counts from the Vital Statistics of the United States for the years 1947–1950 (National Office of Vital Statistics, 1949, 1950, 1951, 1952).<sup>68</sup> Because homicides are rare in some counties, we construct the average homicide count over all available years for each county. In the denominator of homicides per capita, we use 1950 county population from Haines and Inter-university Consortium for Political and Social Research (2010). To measure homicide rates in 1990, we use the 1990 FBI Uniform Crime Reports (Federal Bureau of Investigation, 2016), which contain murders reported to police.<sup>69</sup>

We measure the number of lynchings per capita using data from Bailey et al. (2008), which contains information on all known lynchings for several Southern states (AL, AR, FL, GA, KY, LA, MS, NC and TN). We compute the county-level sum of lynchings during the period 1882–1929. We construct the final measure using total population as recorded in the 1940 Census.

We construct the incarceration rate in 1940 following Eriksson (2019). We start by classifying as incarcerated anyone reporting correctional institutions as their group quarter type (i.e., when the group quarters variable “gqtype” is equal to 2). We require any inmate to report a relationship to the household head that is either “institutional inmate” or “boarder/lodger.” To account for inconsistent reporting of group quarter type, we also keep any “institutional inmate” with a group quarter that is not a correctional institution. For 1990, we use the Incarceration Trends dataset from the Vera Institute of Justice (2015). We add up the jail and prison admissions that originate in each county, dividing by 1990 population.

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<sup>66</sup>Ross, Danziger and Smolensky (1987) describe an alternative approach to measuring poverty in 1939 (see also Barrington (1997)). Our results are extremely similar when we use this approach.

<sup>67</sup>Homicide counts from FBI Uniform Crime Reports are available in 1940, but these data cover only large cities during this period.

<sup>68</sup>Here we also use U.S. County and City Data Book Consolidated File (U.S. Bureau of the Census, 2012) as a crosswalk between county names and county fips codes.

<sup>69</sup>Since the crime data is available at the agency level, we also use data from National Archive of Criminal Justice Data (2007), which provides a crosswalk between agencies and counties.



To measure residential segregation in 1940, we use the segregation index developed by Logan and Parman (2017*b*). They use information on the race of next-door neighbors to assess the amount of residential segregation relative to scenarios with complete segregation and no segregation. A key advantage of their approach is that it can be used in rural areas. For segregation in 1990, we construct a Theil index using tract-level data on the share of population that is White, Black, Hispanic, and another race.

We also use county-level data on the presence of National Association for the Advancement of Colored People (NAACP) chapters that come from Gregory and Estrada (2019). Compiled from NAACP annual reports and the branch bulletins, this database shows the spread of NAACP branches between 1912 and 1964. We use the year a local branch was first mentioned in the database to create a measure of whether a county had a local NAACP chapter by 1940, 1950, and 1960.

#### **G.4 Demographic Measures**

We use the complete count 1940 Census from Ruggles et al. (2020) and summary files from the 1990 Census (Manson et al., 2019) to create measures of population density, percent in an urban area, percent on a farm, and percent of the population that is Black.

## **H Place Effects on Earnings for Black Adult Migrants**

In this appendix, we explore place effects for adults using our selection correction approach from Section 3. Our analysis is motivated by the fact that place effects for parental income could be a key channel that drives place effects on schooling for children. We create a sample of Black men ages 25–64 from the 1940 Census and estimate impacts on log earnings.<sup>70</sup> For the selection correction approach, we include fixed effects for age in the vector of demographic variables  $X_i$ . The key vector that measures selection on observables,  $H_i$ , contains fixed effects for an individual's years of schooling, marital status, and number of own children in the household.

Our main finding is that our selection-corrected estimates imply that there were notable labor market benefits for Black men who moved to locations in the North. Similar to our main analysis for children, Appendix Figure 13 displays separate densities for county-level place effect estimates on men's log earnings in the North and South. We estimate that there was a 42 percent increase in earnings from moving North. As noted in Section 5, the place effects on adult earnings are strongly related to place effects on children's education (correlation: 0.59), which suggests that much of the relationship with median Black family income is driven by earnings gains available to adult migrants.

While we find that migration North led to substantial increases in adults' earnings, our estimate of a 42 percent increase in earnings is smaller than the evidence presented in Collins and Wana-maker (2014) and Boustan (2017), who find gains of 80–130 percent. We differ from this prior work by estimating selection-corrected county-level place effects and by examining a broader age range. By comparison, these previous papers estimate regressions of log earnings on an indicator

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<sup>70</sup>The 1940 Census measures wage and salary income, but not total earnings (which also includes self-employment income). We impute earned income for self-employed individuals based on their race, region, and occupation, as detailed in Appendix G.

for living in the North and various controls. Collins and Wanamaker (2014) examine a sample of men ages 21–40 in the 1930 Census, which does not contain direct measures of earnings and so requires the use of earnings imputed by occupation. Boustan (2017) uses a sample of men ages 18–38 in the 1940 Census. We focus on a broader group of 25–64-year-old men than these papers to obtain a larger sample size (which is helpful for our estimation of county-level place effects) and to better represent the fathers of children in our main analysis sample.

We examine potential explanations for why we find a smaller effect of moving North in Appendix Tables 8 and 9. Column 1 of Appendix Table 8 shows that earnings of Black men in our adult sample were 98 percent larger in the North, consistent with the results in Collins and Wanamaker (2014) and Boustan (2017). The North-South earnings gap narrows to 75 percent when controlling for age, education, marital status, number of children, and origin state, which provides initial evidence of the potential for selective migration. When additionally limiting the sample to destination counties with at least 10 migrants (which we use to increase the reliability of our place effect estimates), the North-South gap falls to 61 percent. The remaining columns of Appendix Table 8 show that estimates are similar when controlling for origin county fixed effects (column 4), limiting the sample to brothers and including 1920 household fixed effects (columns 5–6), and using the same controls for observed variables as in the selection correction approach (columns 7–10).

Appendix Table 9 examines the importance of adjusting for selection on unobserved characteristics. Column 3 shows that the average effect of moving to the North based on county-level place effects when not adjusting for selection on unobservables is 56 percent. Adjusting for selection lowers this earnings gain to 42 percent. Thus, adjusting for selection on unobservables leads to a 25 percent decrease in the effect of moving North ( $= (0.42 - 0.56)/0.56$ ).

In sum, a substantial amount of the difference between our bottom-line estimate of a 42 percent earnings gain from moving North and the 80–130 percent estimate from prior work appears to be explained by controlling for observed variables (in particular, education) and focusing on a subset of counties for which there is a sufficiently large sample of migrants that we can feasibly estimate place effects. A smaller, but still significant, share of the difference is explained by adjusting for selection on unobserved variables.

# **I. Appendix Tables and Figures**

Appendix Table 1: Correlates of 1940 Place Effects on White Children’s Education

	Dependent variable: Place effect, children’s education						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Teachers per pupil	0.253*** (0.0475)					0.219*** (0.0476)	0.210*** (0.0497)
Median White household income		-0.0615* (0.0338)				-0.210*** (0.0400)	-0.213*** (0.0403)
Homicide rate			-0.218*** (0.0434)			-0.191*** (0.0434)	-0.179*** (0.0488)
Incarceration rate				-0.0505 (0.0447)		-0.0550 (0.0444)	-0.0551 (0.0446)
NAACP chapter					0.0807** (0.0368)	0.0902** (0.0405)	0.0827* (0.0423)
South indicator							-0.0655 (0.0997)
Observations (counties)	715	715	715	715	715	715	715
R-squared	0.050	0.004	0.047	0.003	0.007	0.108	0.108

Notes: This table reports correlates of 1940 place effects for White children’s education. Sample is limited to the counties for which we estimate place effects for both Black and White children. We normalize all variables to have mean zero and standard deviation one. All regressions include a series of indicators for whether variables are missing. Heteroskedasticity-robust standard errors in parentheses. See Appendix G for details on variable construction and sources. Statistical significance is denoted by: \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

Source: Authors’ calculations using 1940 Census (Ruggles et al., 2020)

Appendix Table 2: Correlation of Upward Mobility in 1990s and Place Effects in 1940

	DV: Upward mobility, 1990s		
	Black upward mobility (1)	Pooled upward mobility (2)	Exposure effects (3)
Place effect, 1940	0.210*** (0.0340)	0.432*** (0.0330)	0.304*** (0.0348)
Observations (counties)	728	728	728
R-squared	0.045	0.186	0.093

Notes: Table reports correlations between measures of upward mobility from the 1990s and place effects from the 1940 Census. Columns 1 and 2 use the mean household income rank for children whose parents were at the 25th percentile of the national income distribution from Chetty et al. (2020). Column 1 uses upward mobility for Black children, and column 2 uses upward mobility for children of all races. Column 3 uses exposure effects from Chetty and Hendren (2018b). We standardize place effect estimates and the upward mobility measure so that normalized measures have a mean of 0 and a standard deviation of 1. As a result, point estimates in this table are correlation coefficients. Heteroskedasticity-robust standard errors in parentheses: \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

Source: Authors' calculation using the 1940 Census (Ruggles et al., 2020), Chetty and Hendren (2018b), and Chetty et al. (2020)

Appendix Table 3: Place Effects and Mechanisms, Within-Place Estimates, Robustness Using Exposure Effect Measure

	Dependent Variable: $\Delta$ Opportunity Measure (1990s vs 1940)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\Delta$ Teachers per pupil	0.156*** (0.0322)						0.142*** (0.0321)	0.111*** (0.0295)	0.107*** (0.0338)
$\Delta$ Median Black household income		0.229*** (0.0456)					0.201*** (0.0410)	0.158*** (0.0388)	0.156*** (0.0412)
$\Delta$ Homicide rate			-0.208*** (0.0353)				-0.179*** (0.0345)	-0.0717** (0.0346)	-0.0701** (0.0354)
$\Delta$ Incarceration rate				-0.106*** (0.0278)			-0.112*** (0.0276)	-0.0991*** (0.0263)	-0.101*** (0.0285)
$\Delta$ NAACP chapter					0.153 (0.0943)		0.00857 (0.0919)	-0.0287 (0.0904)	-0.0288 (0.0906)
$\Delta$ Percent Black						-0.915*** (0.0758)		-0.725*** (0.0805)	-0.721*** (0.0832)
South indicator									0.0216 (0.111)
Observations (counties)	728	728	728	728	728	728	728	728	728
R-squared	0.055	0.039	0.055	0.018	0.003	0.175	0.147	0.230	0.230

Notes: Separately for each year, we normalize all variables to have mean zero and standard deviation one. We then construct the change from 1940 to the 1990s, except for the change in the presence of a NAACP chapter, which is from 1940 to 1960. The dependent variable is the difference between exposure effects from Chetty and Hendren (2018b) and place effects in 1940. All regressions include a series of indicators for whether variables are missing. Heteroskedasticity robust standard errors in parentheses. See Appendix G for details on variable construction and sources. Statistical significance is denoted by: \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Source: Authors' calculations using 1940 Census (Ruggles et al., 2020) and Chetty et al. (2020)

Appendix Table 4: Comparing the Full Population and the Matched Sample of Adults in the 1920 Census

	1920 U.S.-born Black men age 3–52 (1)	Matched sample subset (2)	Difference (3)
Age	21.366	19.537	1.830***
Urban status	0.250	0.267	-0.016***
Farm status	0.554	0.548	0.006***
Number of siblings	1.980	2.215	-0.235***
Literate	0.765	0.808	-0.043***
School attendance	0.447	0.460	-0.013***
Father’s Duncan Index	13.782	14.036	-0.254***
Father’s literacy	0.686	0.724	-0.038***
Father’s farmer status	0.689	0.651	0.038***
North	0.130	0.178	-0.048***
South	0.870	0.822	0.048***
Observations	3,425,187	501,284	–

Notes: Column 1 reports summary statistics for all U.S.-born Black men who were age 3–52 in 1920 and living in their birth state. Column 2 contains the subset of these men in the matched sample. Column 3 reports the difference between these columns, with stars indicating statistical significance based on heteroskedasticity-robust standard errors. \*  $p < 0.10$  \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Source: Authors’ calculation using the 1920 and 1940 Census (Ruggles et al., 2020)

Appendix Table 5: Comparing the Full Population and Matched Sample of Children in the 1940 Census

	1940 Southern-born Black men age 25–70 who have children age 14–18 (1)	Matched sample subset, age 25–70 (2)	Difference (3)	1940 children age 14–18 of Southern-born Black men (4)	Matched sample subset of children (5)	Difference (6)
Years of schooling	4.509	4.782	-0.273***	6.671	6.888	-0.216***
Earnings	476.186	503.369	-27.184***	97.605	93.372	4.233**
Age	46.475	46.822	-0.347***	15.888	15.895	-0.007
Urban status	0.444	0.449	-0.005	0.419	0.424	-0.004
Farm status	0.369	0.366	0.003	0.396	0.394	0.002
Married	0.912	0.921	-0.009***	0.007	0.007	0.000
North	0.286	0.314	-0.028***	0.276	0.303	-0.027***
South	0.714	0.686	0.028***	0.724	0.697	0.027***
Observations	116,366	24,148	–	179,335	37,623	–

Notes: Column 1 reports summary statistics for all Southern-born Black men age 25–70 in the 1940 Census who have children between age 14 and 18. Column 2 contains the subset of these men in the matched sample. Column 4 contains children age 14–18 of Southern-born Black men in column 1, and column 5 contains the matched sample subset. Columns 3 and 6 report the difference between these columns, with stars indicating statistical significance based on heteroskedasticity-robust standard errors.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Source: Authors' calculation using the 1920 and 1940 Census (Ruggles et al., 2020)



Appendix Table 6: Comparison of North Indicator to Place Effects, Black Children's Educational Attainment

	DV: Years of schooling									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
North indicator	2.180*** (0.098)	1.411*** (0.087)	1.008*** (0.120)	1.007*** (0.109)	1.064*** (0.123)	0.369 (0.335)	1.198*** (0.124)		1.210*** (0.124)	
Implied North-South difference from place effects								1.198		1.207
Observations	105,347	105,347	41,092	41,092	26,082	26,082	41,092	41,092	46,867	46,867
Sample										
Household (HH) head born in South	X	X	X	X	X	X	X	X		
In destination county with at least 10 migrants			X	X	X	X	X	X	X	X
At least 2 children with same 1920 family					X	X				
Controls										
Age, sex, parents' education		X	X	X	X	X	X	X	X	X
HH head 1920 state FE		X	X	X	X	X	X	X	X	X
HH head 1920 county FE				X						
HH head 1920 family FE						X				
HH head 1920 state FE × mover indicator							X	X	X	X
1940 county FE × non-mover indicator							X	X	X	X

Notes: The first row reports results from regressing years of schooling on an indicator for living in the North and controls. Standard errors are clustered by 1940 county of residence. The implied North-South difference from place effects comes from a regression that replaces the North indicator with county fixed effects for migrants. We calculate the difference between the average fixed effects in the North and the average fixed effects in the South, where each average is constructed using weights equal to the number of migrants in each county, as in equation (6). Sample contains African Americans age 14–18. Statistical significance is denoted by: \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

Source: Authors' calculation using the 1920 and 1940 Census (Ruggles et al., 2020)

Appendix Table 7: Comparison of Estimated North-South Differences Across Samples and Adjustments, Black Children’s Educational Attainment

	DV: Years of schooling				
	(1)	(2)	(3)	(4)	(5)
Implied North-South difference from place effects	1.207	1.207	1.205	0.736	0.718
Place effects sample	Matched	Matched	Full	Full	Full
Observation weight sample	Matched	Full	Full	Full	Full
Selection correction				X	X
Empirical Bayes adjustment					X

Notes: Table reports the difference between the average fixed effects in the North and the average fixed effects in the South, where each average is constructed using weights equal to the number of migrants in each county as in equation (6). We estimate place effects and measure the number of migrants using the matched sample and full sample (for the latter, focusing on counties for which fixed effects are estimated in the matched sample). Column 4 uses selection-corrected place effects, and column 5 further uses empirical-Bayes-adjusted effects. For all columns, we use the same specification as in column 10 of Appendix Table 6.

Source: Authors’ calculation using the 1920 and 1940 Census (Ruggles et al., 2020)

Appendix Table 8: Comparison of North Indicator to Place Effects, Black Adult Log Earnings

	DV: Log wage and salary earnings									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
North indicator	0.683*** (0.030)	0.559*** (0.029)	0.477*** (0.030)	0.473*** (0.028)	0.484*** (0.031)	0.506*** (0.028)	0.425*** (0.032)		0.424*** (0.032)	
Implied North-South difference from place effects								0.427		0.425
Observations	292,978	288,993	204,122	204,122	49,939	49,939	204,122	204,122	239,704	239,704
Sample										
Born in South	X	X	X	X	X	X	X	X		
In destination county with at least 10 migrants			X	X	X	X	X	X	X	X
At least 2 adult men with same 1920 family					X	X				
Controls										
Age, education, marital status, children		X	X	X	X	X	X	X	X	X
1920 state FE		X	X	X	X	X	X	X	X	X
1920 county FE				X						
1920 family FE						X				
1920 state FE × mover indicator							X	X	X	X
1940 county FE × non-mover indicator							X	X	X	X

Notes: The first row reports results from regressing log earnings on an indicator for living in the North and controls. Standard errors are clustered by 1940 county of residence. The implied North-South difference from place effects comes from a regression that replaces the North indicator with county fixed effects for migrants. We calculate the difference between the average fixed effects in the North and the average fixed effects in the South, where each average is constructed using weights equal to the number of migrants in each county, as in equation (6). Sample contains African American men age 25–64.

Source: Authors' calculation using the 1920 and 1940 Census (Ruggles et al., 2020)

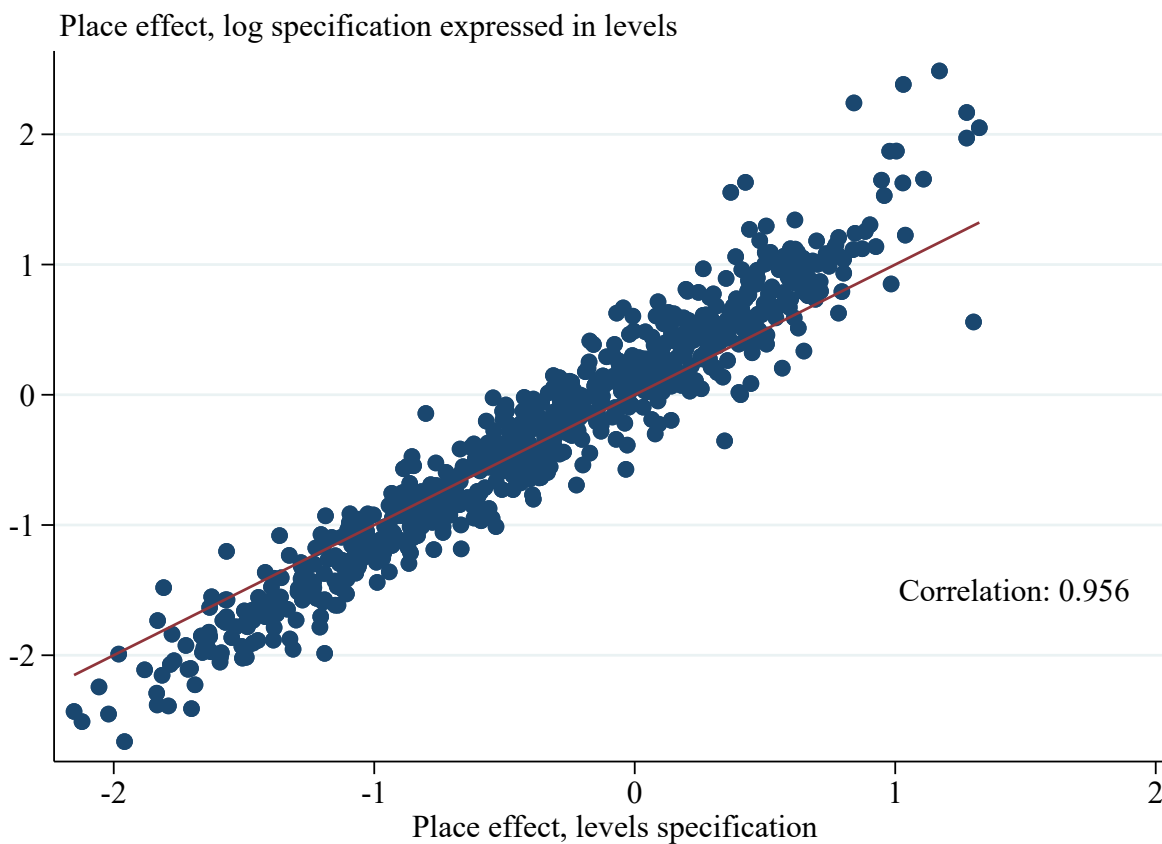
Appendix Table 9: Comparison of Estimated North-South Differences Across Samples and Adjustments, Black Adult Log Earnings

	DV: Log wage and salary earnings				
	(1)	(2)	(3)	(4)	(5)
Implied North-South difference from place effects	0.425	0.425	0.442	0.348	0.346
Place effects sample	Matched	Matched	Full	Full	Full
Observation weight sample	Matched	Full	Full	Full	Full
Selection correction				X	X
Empirical Bayes adjustment					X

Notes: Table reports the difference between the average fixed effects in the North and the average fixed effects in the South, where each average is constructed using weights equal to the number of migrants in each county as in equation (6). We estimate place effects and measure the number of migrants using the matched sample and full sample (for the latter, focusing on counties for which fixed effects are estimated in the matched sample). Column 4 uses selection-corrected place effects, and column 5 further uses empirical-Bayes-adjusted effects. For all columns, we use the same specification as in column 10 of Appendix Table 8.

Source: Authors' calculation using the 1920 and 1940 Census (Ruggles et al., 2020)

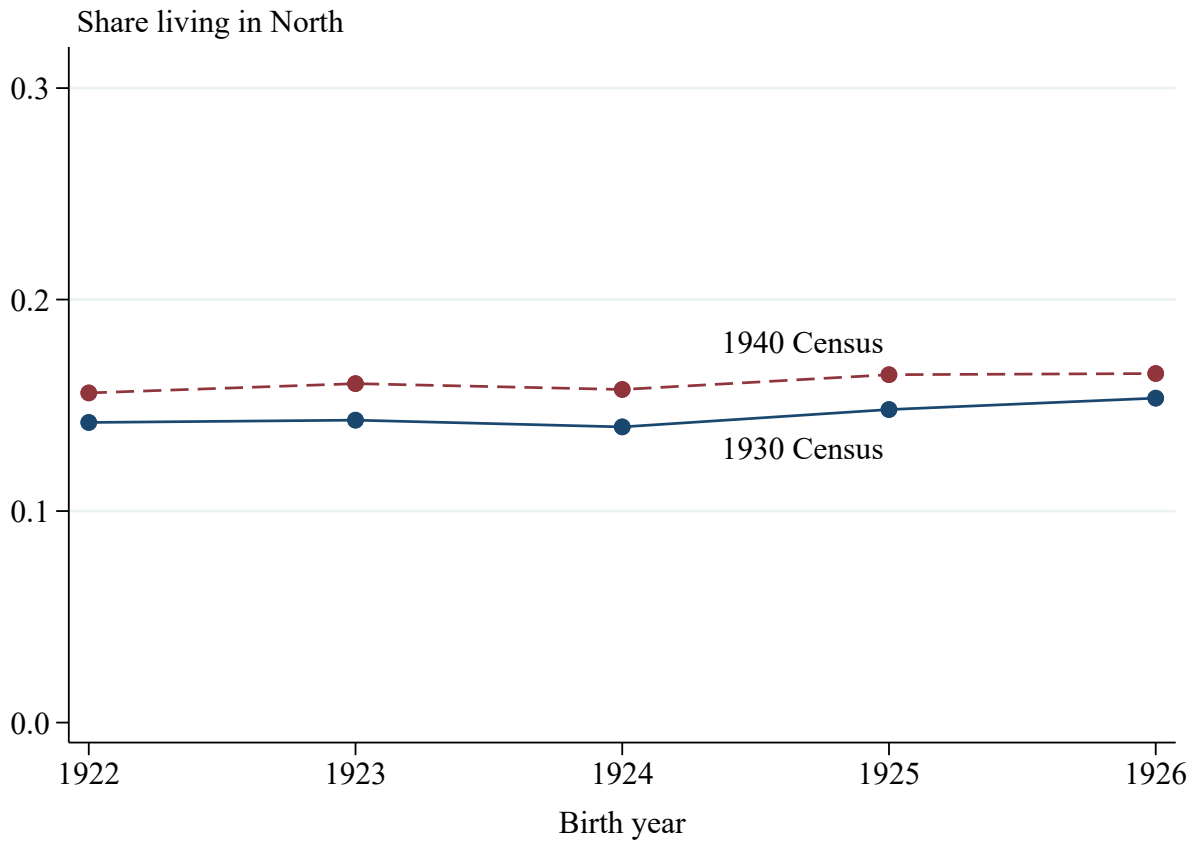
Appendix Figure 1: Comparison of Place Effects from Levels vs. Log Specification



Notes: Figure displays empirical-Bayes-adjusted place effects from our baseline specification ( $x$ -axis) against an alternative specification ( $y$ -axis) that estimates place effects on log years of schooling and converts to levels using the formula:  $\text{place effect in levels} = [\exp(\text{place effect in logs}) - 1] \times \text{mean}$ . For the alternative specification, we estimate the empirical-Bayes-adjusted place effects using the log place effects, and then convert both the adjusted and unadjusted log place effects separately into level place effects. We calculate correlations using non-empirical-Bayes-adjusted place effects.

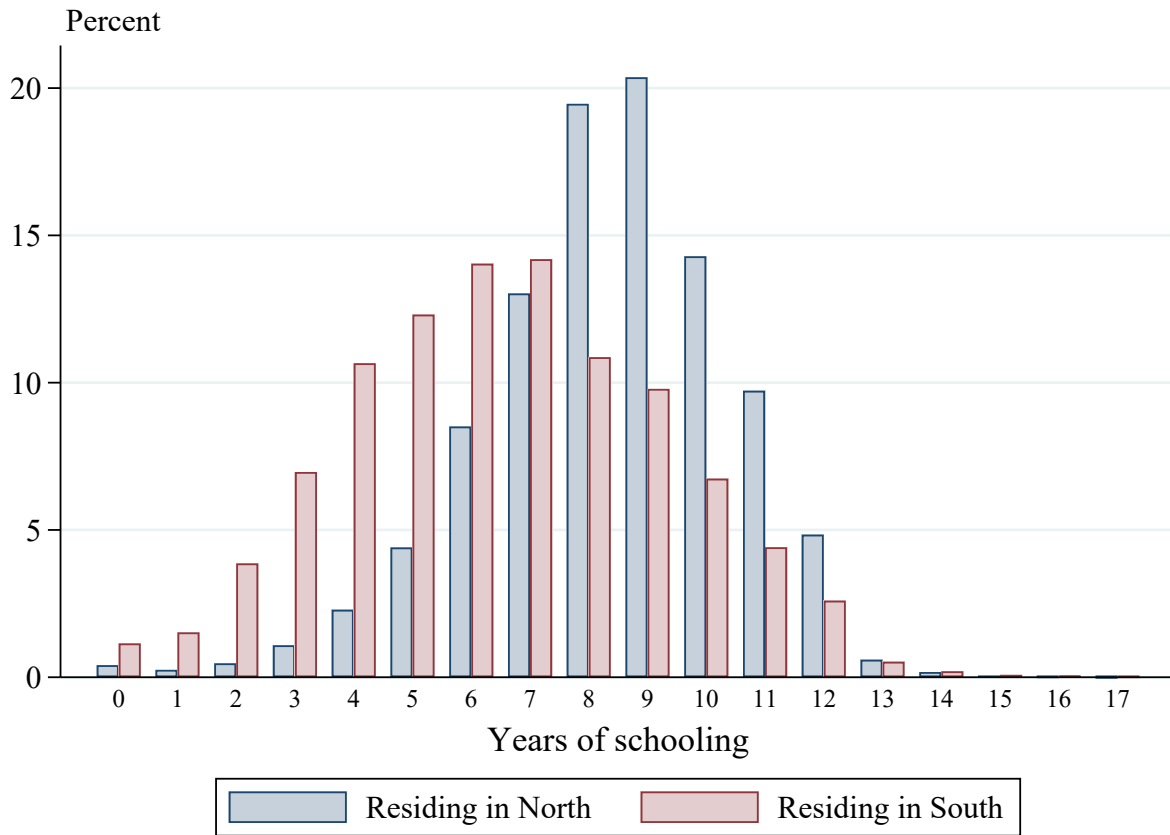
Source: Authors' calculations using 1940 Census (Minnesota Population Center and Ancestry.com, 2013).

Appendix Figure 2: Summary Statistics on Northern Migration in the 1930 and 1940 Censuses



Notes: Figure displays the share of each birth cohort ( $x$ -axis) that is living in the North in the 1930 (navy, solid) and 1940 (maroon, dashed) Censuses. In the 1930 Census, the overall average share living in the North is about 15 percent. In the 1940 Census, the overall average share living in the North is 16 percent. Samples contain Black children born from 1922 to 1926 who are living in a household where the head was born in the South. Source: Authors' calculations using the 1930 and 1940 Censuses (Ruggles et al., 2020)

Appendix Figure 3: Educational Attainment by 1940 Place of Residence, Black Children Age 14–18 with Migrant Parents

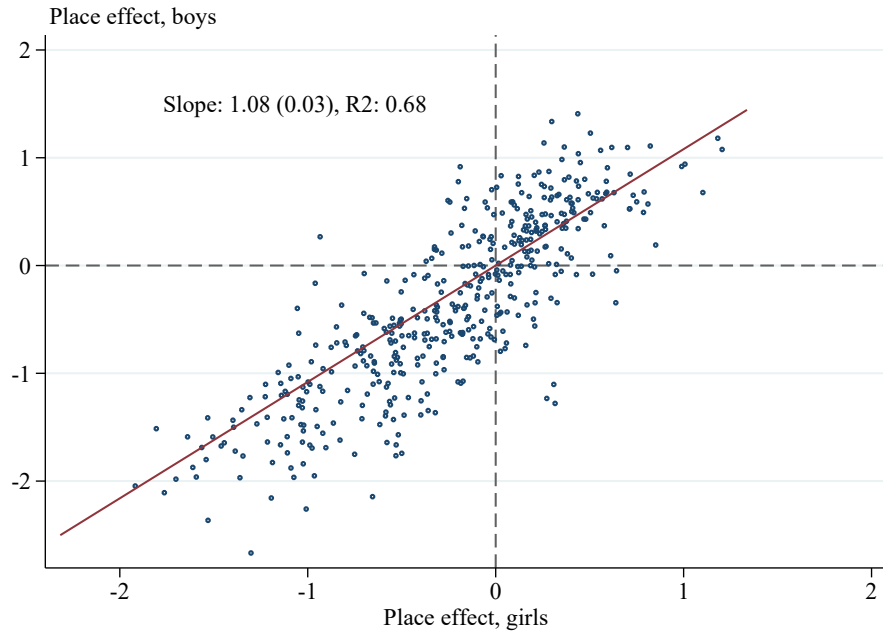


Notes: Sample contains Black children age 14–18 whose household head was born in the South and lives outside the head’s birth state in 1940.

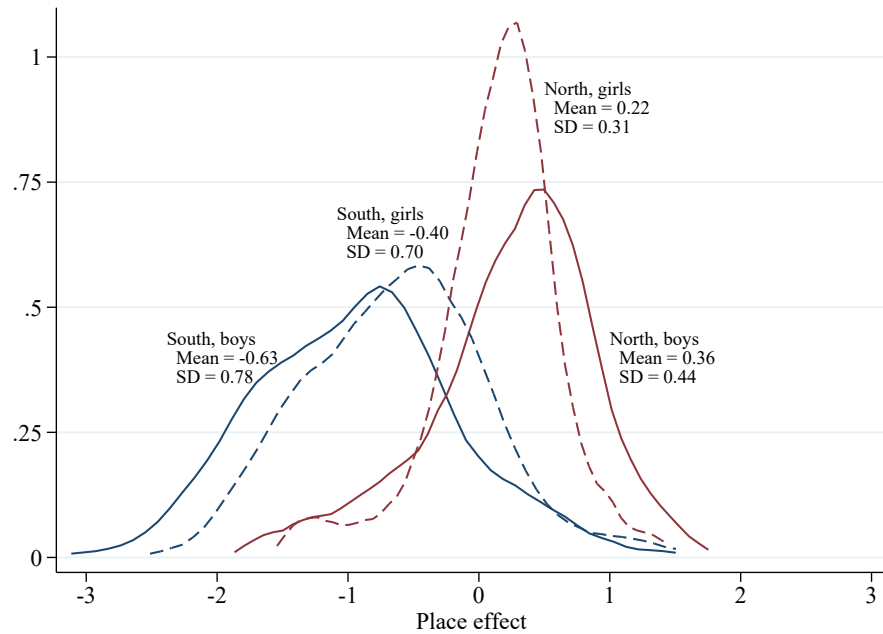
Source: Authors’ calculations using 1940 Census (Ruggles et al., 2020)

Appendix Figure 4: Comparison of Place Effects by Sex, Black Children Age 14–18

(a) Bivariate Relationship



(b) Density of Place Effects, by Region

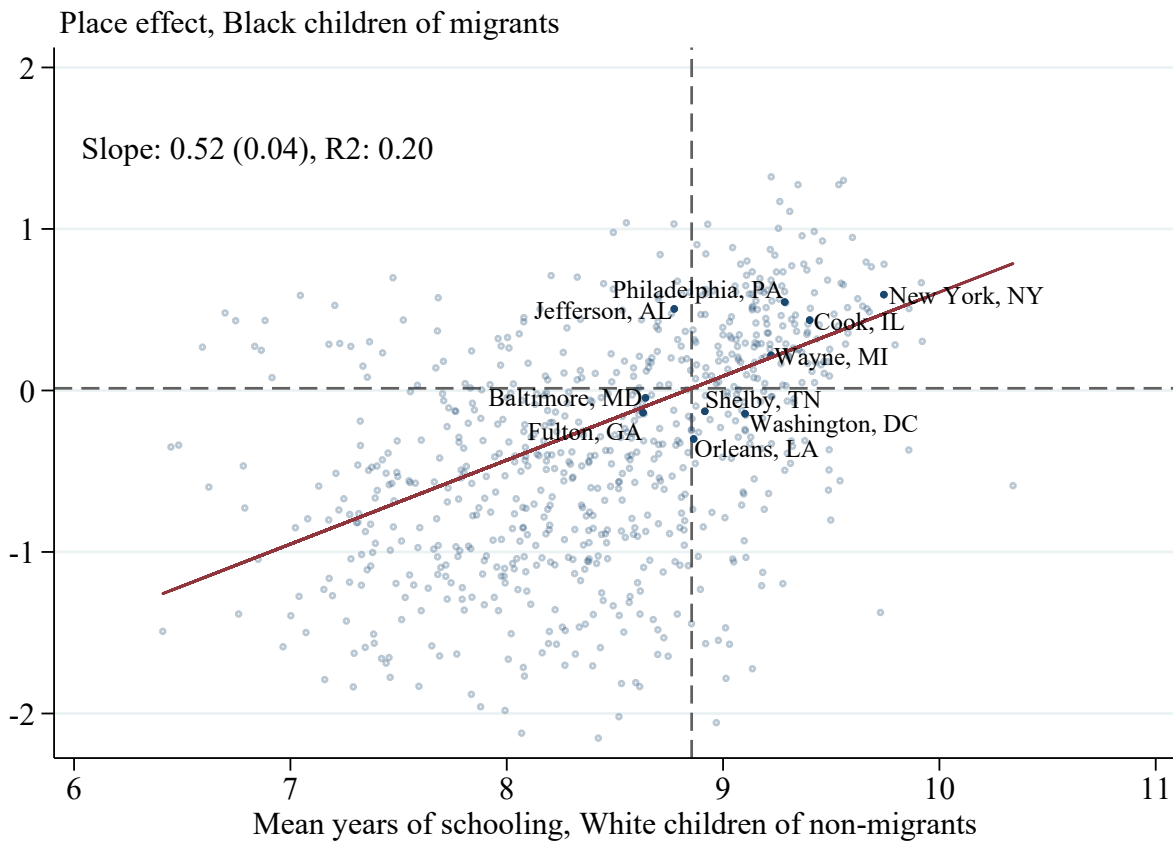


Notes: Panel A displays empirical-Bayes-adjusted place effects for boys and girls age 14–18 in 1940. Dashed lines are migrant-weighted averages. To estimate the line of best fit, we use non-empirical-Bayes-adjusted place effects. Panel B shows the density of place effects in the South and North, alongside migrant-weighted averages and standard deviations.

Source: Authors' calculations using 1940 Census (Ruggles et al., 2020)



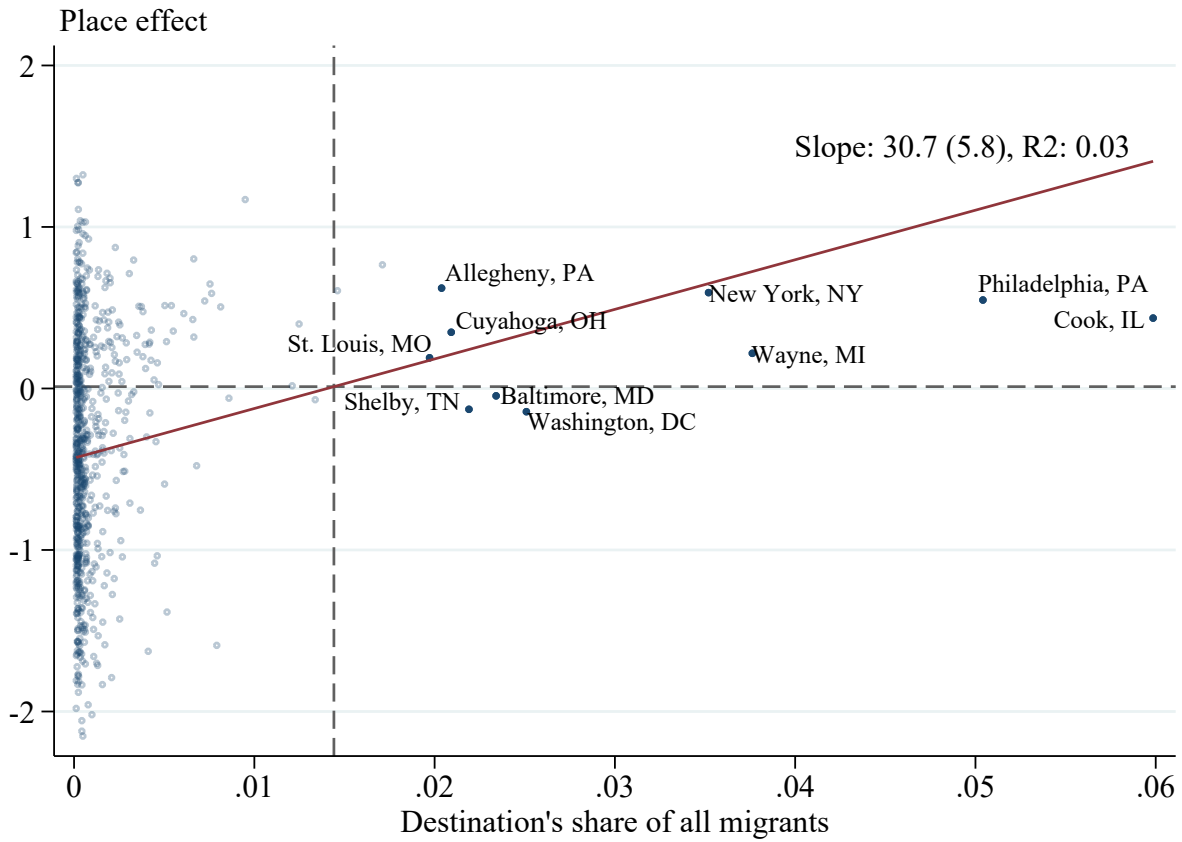
Appendix Figure 5: Place Effects for Black Children versus Average Years of Schooling for White Non-Migrants, Ages 14–18



Notes: Figure displays empirical-Bayes-adjusted place effects for Black children against average years of schooling for White non-migrants. Dashed lines are migrant-weighted averages (0.00 and 8.86). The ten largest counties in terms of 1940 Black population are labeled. To estimate the line of best fit, we use non-empirical-Bayes-adjusted place effects as the dependent variable.

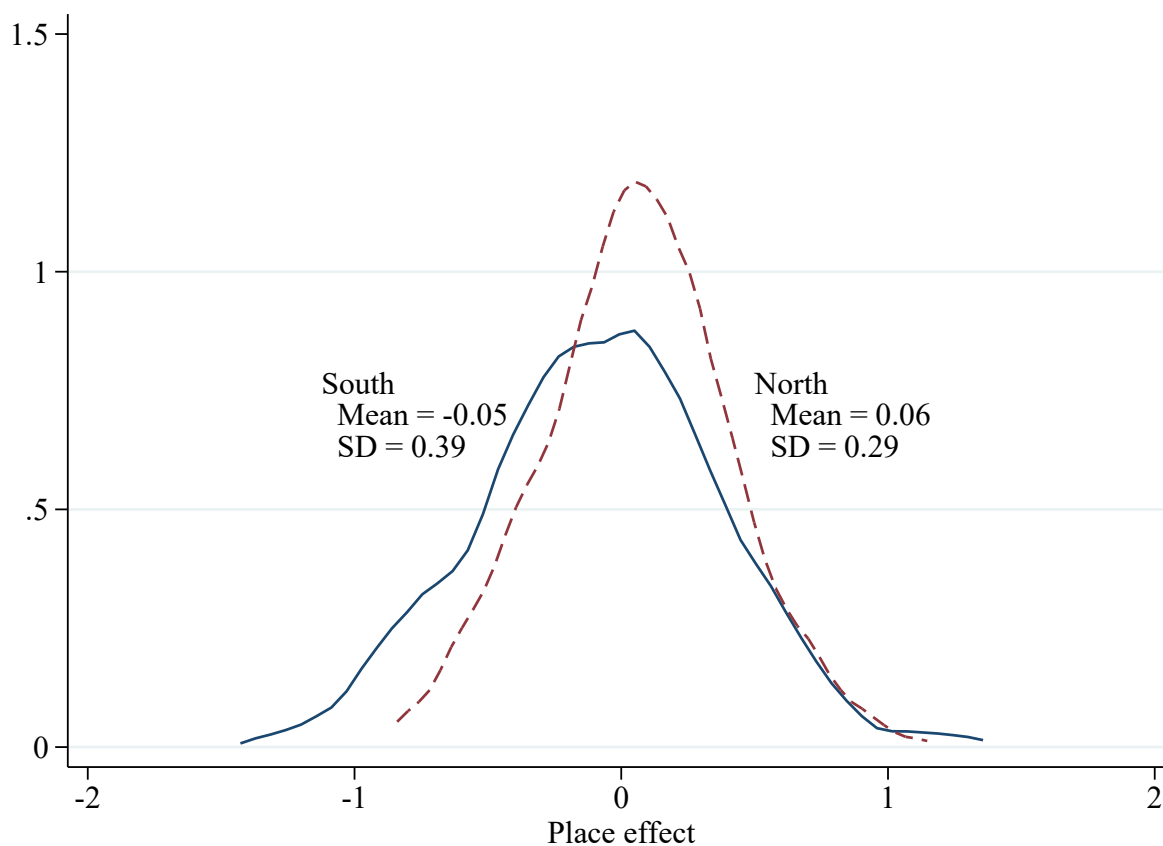
Source: Authors' calculations using the 1940 Census (Minnesota Population Center and Ancestry.com, 2013).

Appendix Figure 6: Place Effects versus Share of Migrants in Destination, Black Children Age 14–18



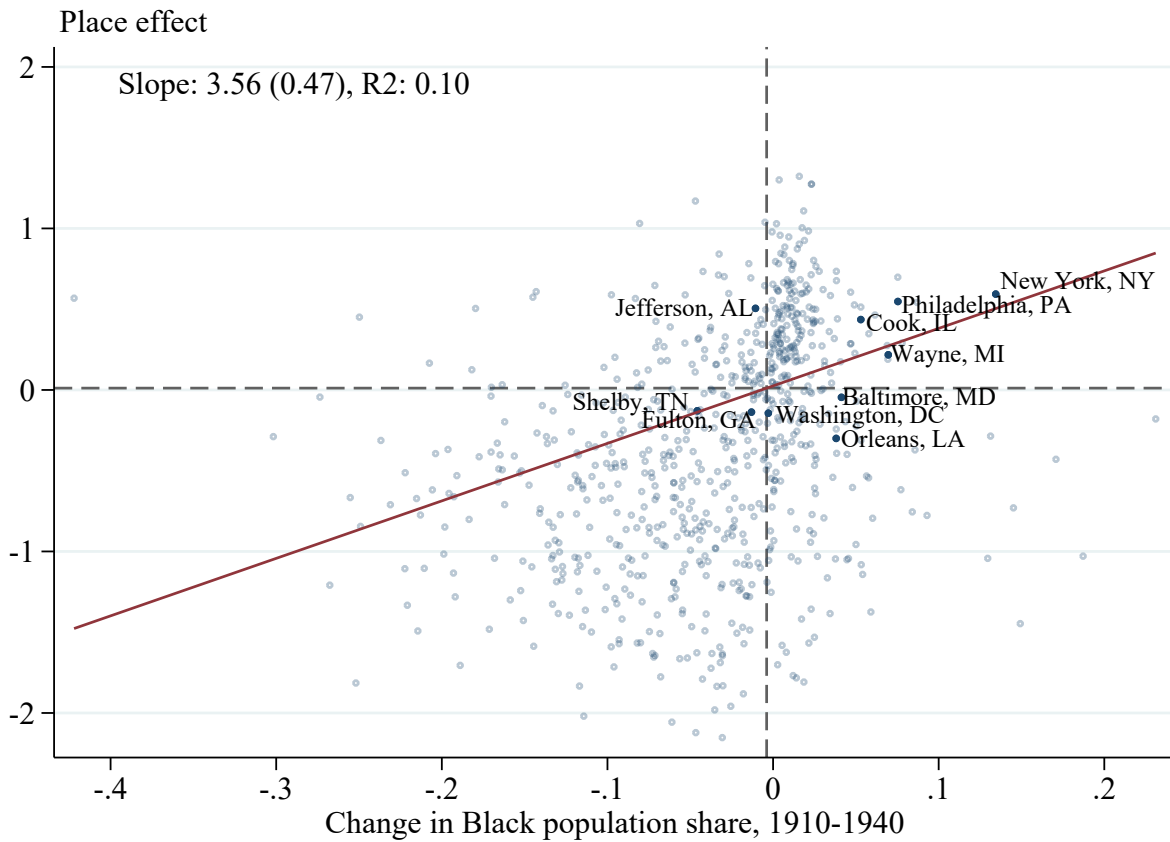
Notes: Figure displays empirical-Bayes-adjusted place effects against the share of children of migrants in each destination. Dashed lines are migrant-weighted averages (0.00 and 0.01). The ten largest counties in terms of migrant child share are labeled; these counties contain 31.5 percent of all migrant children. Source: Authors' calculations using 1940 Census (Ruggles et al., 2020)

Appendix Figure 7: Distribution of Place Effects on Years of Schooling in South and North, White Children Age 14–18



Notes: Figure shows density of place effect estimates in the South and North for White children age 14–18 whose household head was born in the South. Migrant-weighted averages and standard deviations are reported.  
Source: Authors' calculations using 1940 Census (Ruggles et al., 2020)

Appendix Figure 8: Place Effects versus Change in Black Population Share from 1910–1940

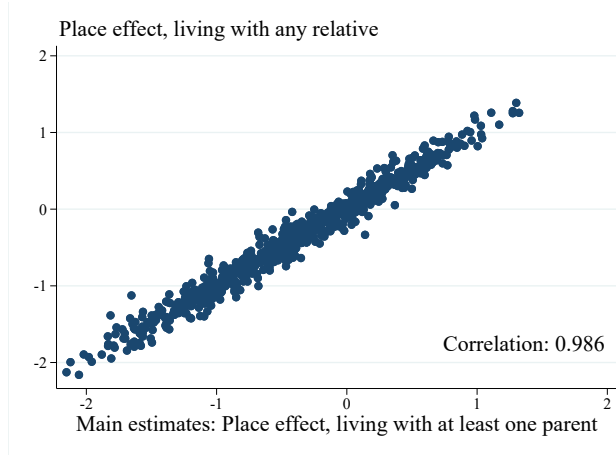


Notes: Figure displays empirical-Bayes-adjusted place effects against the change in the Black population share from 1910–1940. Dashed lines are migrant-weighted averages (0.00 and -0.004). The ten largest counties in terms of 1940 Black population are labeled.

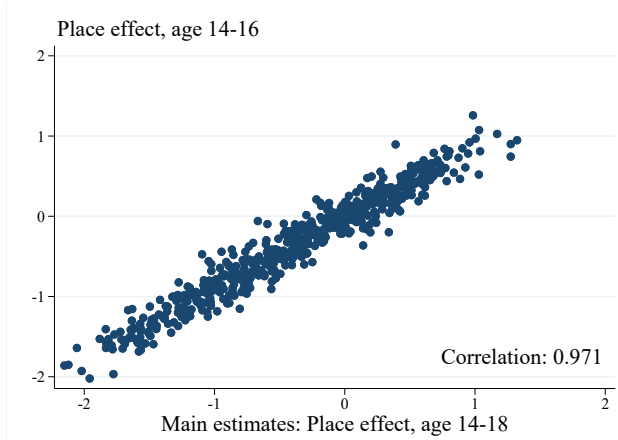
Source: Authors' calculations using 1940 Census (Ruggles et al., 2020)

## Appendix Figure 9: Robustness to Sample Selection and Censoring

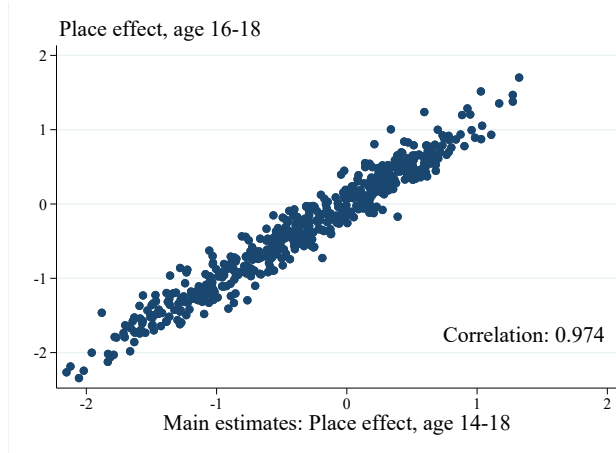
(a) Robustness to Sample Selection: Parental Co-Residence



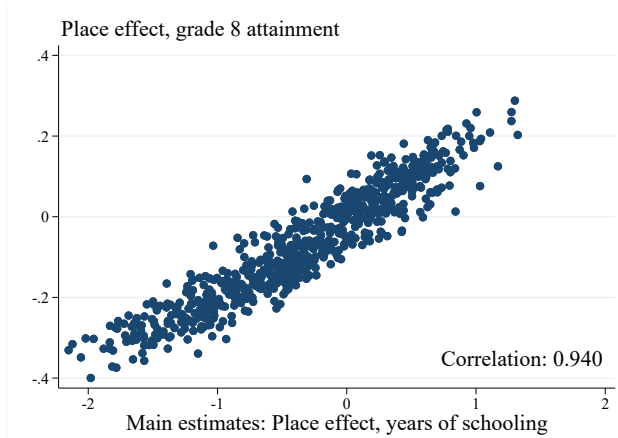
(b) Robustness to Sample Selection: Age



(c) Robustness to Censoring: Age



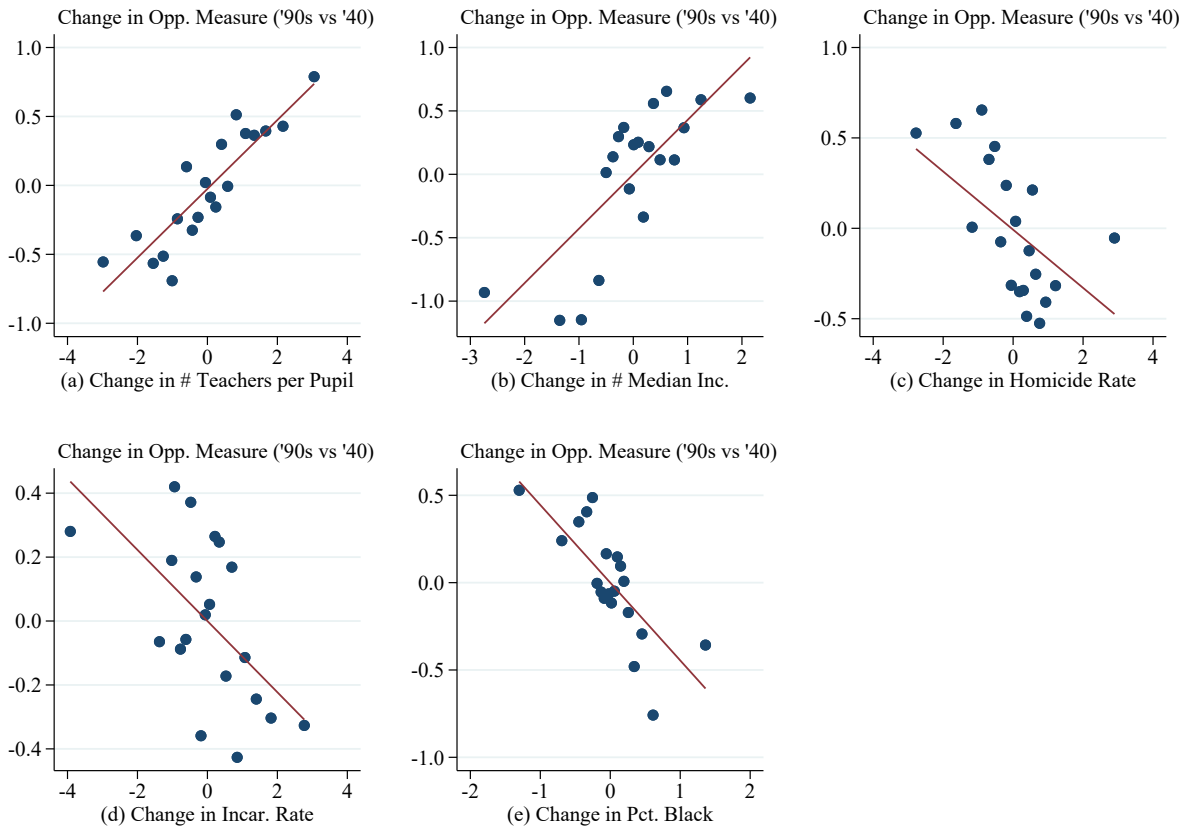
(d) Robustness to Censoring: Dependent Variable



Notes: Figure displays empirical-Bayes-adjusted place effects for Black children. Across all panels, our main estimates (for years of education of children ages 14–18 that live with at least one parent) are shown on the  $x$ -axis. The  $y$ -axis in Panel A displays place effects for children ages 14–18 that live with any relative. Panel B shows results for children ages 14–16 that live with at least one parent. Panel C shows results for children ages 16–18 that live with at least one parent. Panel D plots place effects for grade 8 attainment among our main sample.

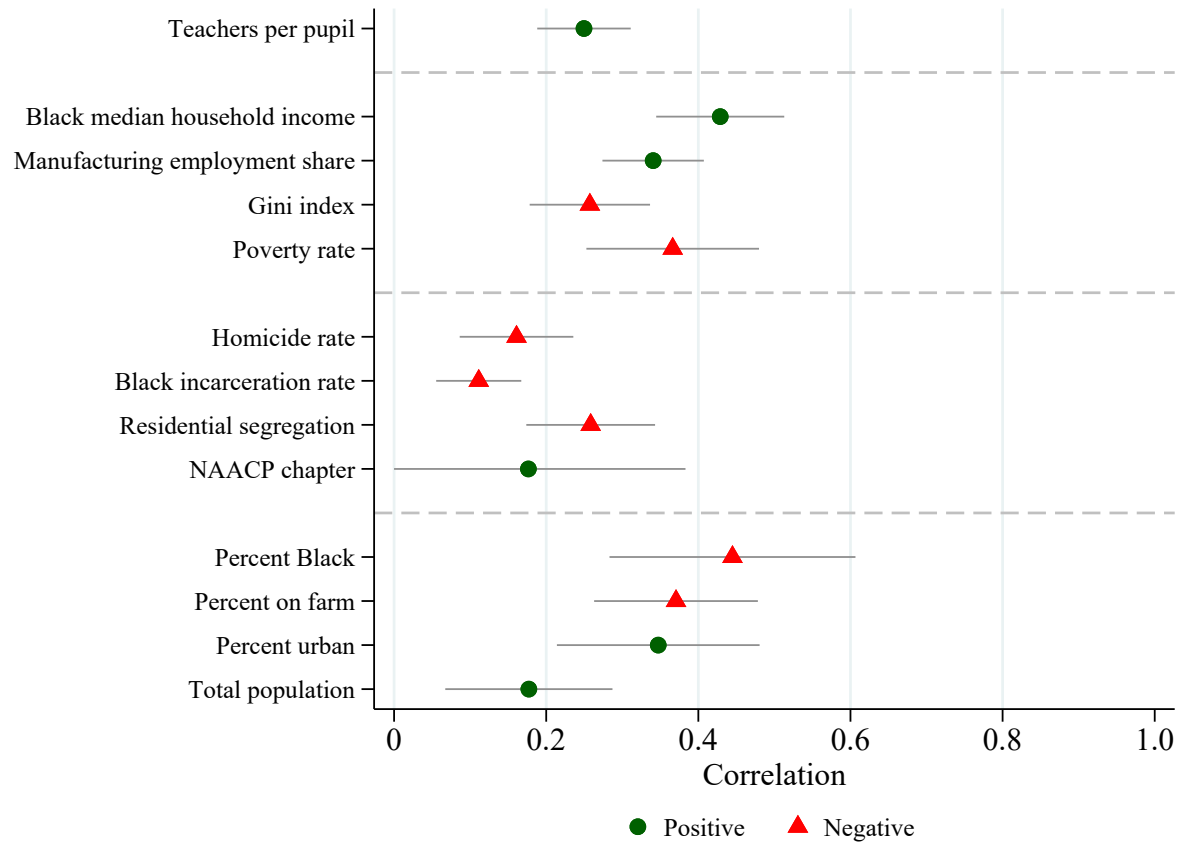
Source: Authors' calculations using 1940 Census (Ruggles et al., 2020)

## Appendix Figure 10: Place Effect Mechanisms, Within-Place Estimates, Binned Scatterplot



Notes: Figure displays the relationship between the change in opportunity measures and the change in place characteristics. Each observation in the plot represents the average change within binned values of the  $x$  and  $y$  axis. We group the data into 20 equally-sized bins. The 1940 measure of place effects is based on our analysis of the 1940 Census. For a contemporary opportunity measure, we use the upward mobility measure from Chetty et al. (2020). Upward mobility is the mean household income rank for children whose parents were at the 25th percentile of the national income distribution. This statistic is calculated for children born between 1978 and 1983, who grew up during the 1990s. Both measures of opportunity are empirical-Bayes-adjusted. We normalize all variables to have a standard deviation of one and a mean of zero. We compute the change for each standardized variable between the contemporary and historical periods. The construction of measures of place characteristics is described in Appendix G.  
 Source: Authors' calculations using 1940 Census (Ruggles et al., 2020) and Chetty et al. (2020)

Appendix Figure 11: Place Effect Mechanisms, Within-Place Estimates, Bivariate Results

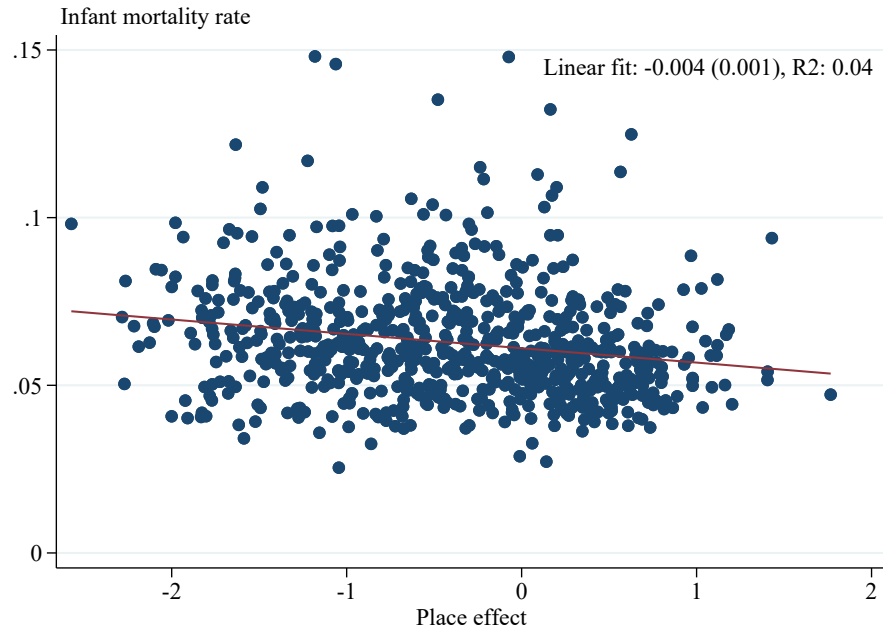


Notes: Figure displays correlations based on an analysis of the change in opportunity measures and the change in place characteristics. The 1940 measure of place effects is based on our analysis of the 1940 Census. For a contemporary opportunity measure, we use the upward mobility measure from Chetty et al. (2020). Upward mobility is the mean household income rank for children whose parents were at the 25th percentile of the national income distribution. This statistic is calculated for children born between 1978 and 1983. The construction of measures of place characteristics is described in Appendix G. We normalize all variables to have a standard deviation of one and a mean of zero. We compute the change for each standardized variable between the contemporary and historical periods. Correlations are based on the change in normalized measures.

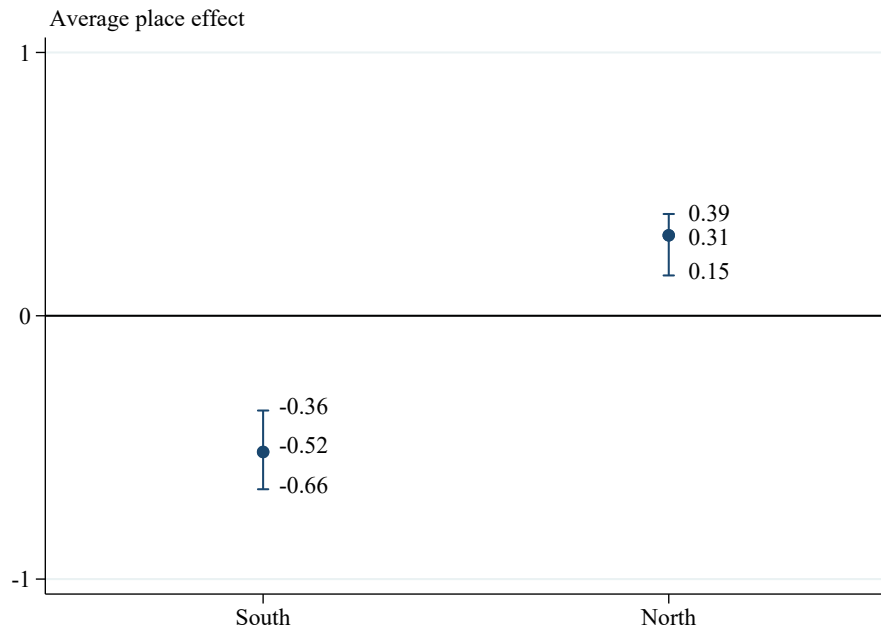
Source: Authors' calculations using 1940 Census (Ruggles et al., 2020) and Chetty et al. (2020)

## Appendix Figure 12: Sensitivity of Results to Child Mortality Differences

(a) Relationship between Infant Mortality Rates and Place Effect Estimates



(b) Upper and Lower Bounds of Place Effects in the South and North

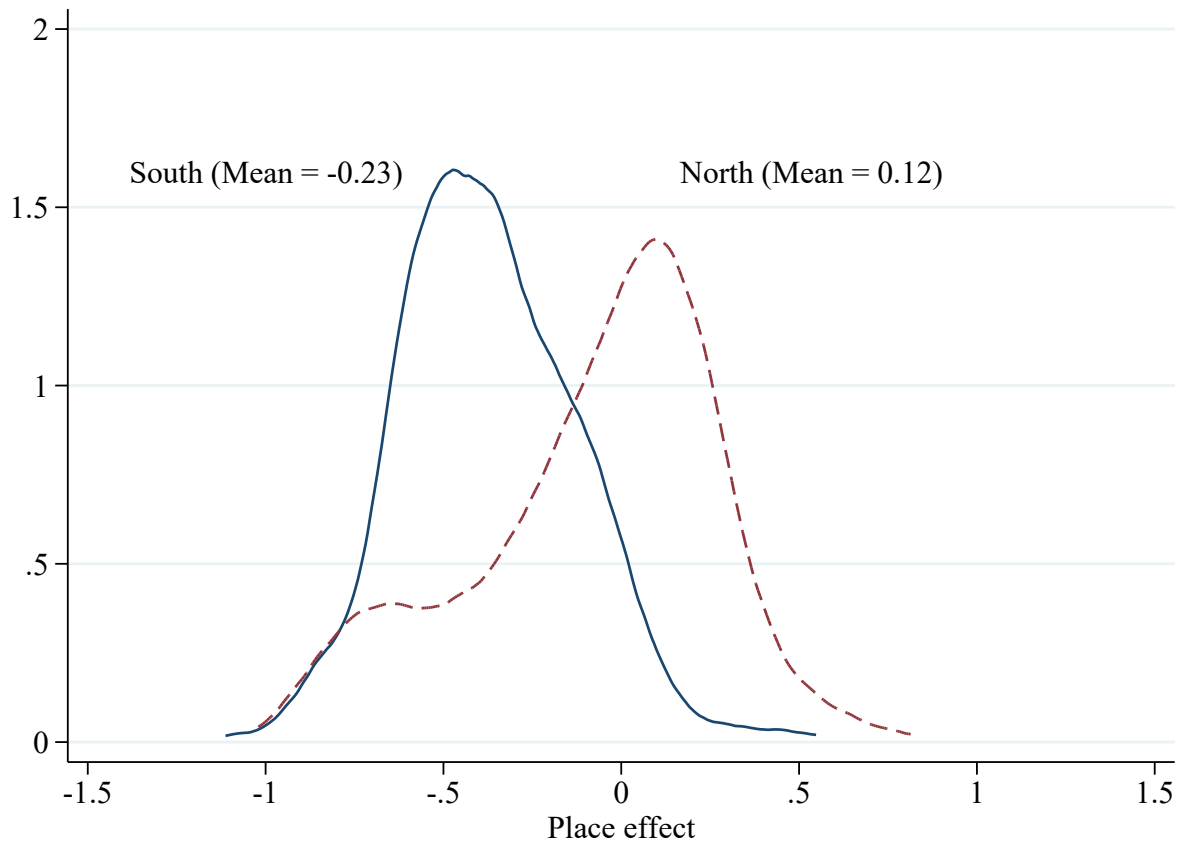


Notes: Panel A displays the relationship between infant mortality rates from 1933–1937 and our baseline place effect estimates. Panel B displays the average upper and lower bound for county place effects in the South and North, respectively. Section F provides details on the constructions of the bounds.

Source: Authors' calculations using 1940 Census (Ruggles et al., 2020) and infant mortality records (Bailey et al., 2008)



Appendix Figure 13: Density of Place Effects on Adult Earnings in South versus North, Black Men Age 25–64



Notes: Figure shows density of place effect estimates in the South and North. Migrant-weighted averages are reported.  
Source: 1940 Complete Count Census