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The Great Migration and Educational Opportunity†

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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 the 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* H75, I26, J13, J15, J24, N32, N92)

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 six 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 that 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 that received more Black migrants, raise the question of whether the migration *ever* yielded meaningful benefits to children.

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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 post–World War II 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 1992a, 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 2018a, b; Chyn 2018; Nakamura, Sigurdsson, and Steinsson 2022; 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.[1] 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

[1] We follow other studies of the Great Migration by referring to the non-South as the "North" for convenience.

obtain higher schooling than elsewhere (e.g., moving to Pittsburgh, where 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 ninetieth and tenth percentiles 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.[2]

We conduct several robustness tests that demonstrate that 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

[2] 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 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 0.29.

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 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.[3]

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).

[3]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 twenty-fifth 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.

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 2023; Collins and Wanamaker 2014, 2015; Eriksson 2019; Shertzer and Walsh 2019; Stuart and Taylor 2021a, b; Tabellini 2019; Shi et al. 2022). 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 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 United States using data on children born in the 1980s (Chetty et al. 2014; Chetty, Hendren, and Katz 2016; Chetty and Hendren 2018a, 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 1992a; 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.

I. Historical Background

Economic and social opportunities for African Americans varied widely across the United States 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 3 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

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	1,939	218
Avg. years of education, nonmigrants 14–18	5.99	435	8.15	289
Median Black household income	341	435	578	293
Avg. earnings, nonmigrant 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 100,000)	12.74	434	4.95	293
Lynching rate (per 100,000)	40.5	354	92.6	24
Incarceration rate (per 100,000)	816	435	1,193	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: The table reports unweighted averages across counties in our analysis sample with nonmissing values of each variable. Our analysis sample is limited to counties with at least 25 Black migrants age 14–18. See online Appendix G for details on variable construction and sources.

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

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 (US 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 those 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 from 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. 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.[4] 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).[5] Term length, teacher salaries, and other schooling inputs also varied along these lines.[6]

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). In addition, 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. Also, 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 for Black children of intraregional migration during our time period are also an open question. Again, the historical context of our study suggests a plausibly important 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 tenth percentile of the county-level distribution, while it was \$850 at the ninetieth percentile. In the South, the tenth and ninetieth percentiles were \$260 and \$520. These differences are comparable to the average North–South

[4] Alexander et al. (2017) present descriptive evidence on overall differences in outcomes of children of migrants and nonmigrants. 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.

[5] 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 in 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).

[6] 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.

difference.[7] There was also large intraregional variation in educational attainment and schooling inputs, especially in the South.

II. Empirical Strategy and Data

A. *Econometric Model*

Our goal is estimate the causal impact of each county on Black children's educational attainment as of 1940.[8] 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:[9]

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

The parameter of interest in equation (1) is the *place effect* γ_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 θ_i, which we refer to as *schooling capital*. We assume that schooling capital can be decomposed into demographics $\mathbf{X}_i$, household characteristics $\mathbf{H}_i$, unobserved factors that are correlated with parent origin (o) and destination (j) locations, and an orthogonal residual:

$$\theta_i = \mathbf{X}_i\psi + \mathbf{H}_i\lambda + \eta_o^{orig} + \eta_j^{dest} + \eta_j^{nm} + \tilde{\eta}_i. \tag{2}$$

The terms η_o^{orig}, η_j^{dest}, and η_j^{nm} are fixed effects for migrant parents' origin location, migrant parents' destination location, and nonmigrant parents' place of residence, respectively. The fixed effect η_o^{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 η_j^{dest} measures whether the unobserved average schooling capital of children of migrants differs across destination locations. We assume $\eta_j^{nm} = 0$ for migrants and $\eta_o^{orig} = \eta_j^{dest} = 0$ for nonmigrants. 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

[7] For the entire United States, the tenth and ninetieth percentiles were \$260 and \$750, respectively.

[8] 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.

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

assumption implies that there is no interaction between individual attributes and the effects of location on child outcomes.[10]

B. *Estimation and Identification*

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

$$Y_i = \mathbf{X}_i \psi + \mathbf{H}_i \lambda + \tau_o^{orig} + \tau_j^{dest} + \tau_j^{nm} + \tilde{\eta}_i, \quad (3)$$

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

The key challenge in estimating equation (3) is identification of place effects γ_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 $\mathbf{X}_i$ and $\mathbf{H}_i$. In this case, we would have $\eta_o^{orig} = \eta_j^{dest} = \eta_j^{nm} = 0$, and so γ_j could be identified directly from estimates of τ_j^{dest} in equation (3). A more plausible assumption is that differences in schooling capital are captured by the combination of $\mathbf{X}_i$, $\mathbf{H}_i$, and the origin fixed effect τ_o^{orig}. This assumption would follow from a model in which the birthplace 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 $T_{ij} \equiv \mathbf{1}\{j(i) = j\}$ be an indicator for whether person i lives in location j. In addition, define $h_i = \mathbf{H}_i \lambda$ as the index of *observed* schooling capital. This index captures how household characteristics $\mathbf{H}_i$ are related to a child's years of schooling and depends on the parameter vector λ in equation (3). Finally, consider the following auxiliary regression in the sample of migrant children:

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

The explanatory variables are demographics $\mathbf{X}_i$, plus fixed effects for migrant parents' origin location (h_o^{orig}) and destination location (h_j^{dest}). The fixed effect h_j^{dest} describes whether the index of observed schooling capital differs across destinations. Consequently, h_j^{dest} is the counterpart to η_j^{dest}, where the former reflects

[10] 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 online Appendix Figure 1), which suggests that the additive separability assumption does not severely influence our results.

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 $\hat{h}_i = \mathbf{H}_i \hat{\lambda}$, where $\hat{\lambda}$ comes from OLS estimation of equation (3). We provide details on the variables in $\mathbf{H}_i$ 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): $\text{corr}\left(T_{ij}, \eta_j^{dest}\right) = \text{corr}\left(T_{ij}, h_j^{dest}\right)$ *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): $\text{std}\left(\eta_j^{dest}\right)/\text{std}\left(h_j^{dest}\right) = \text{std}\left(\eta_o^{orig}\right)/\text{std}\left(h_o^{orig}\right)$ *in the sample of migrants.*

This assumption allows us to pin down the amount of selection on unobserved schooling capital, $\text{std}\left(\eta_j^{dest}\right)$, 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 η_j^{dest}:

$$\hat{\eta}_j^{dest} = \frac{\hat{\text{std}}\left(\hat{\tau}_o^{orig}\right)}{\hat{\text{std}}\left(\hat{h}_o^{orig}\right)} \hat{h}_j^{dest}, \tag{5}$$

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).[11,12]

The key distinction between $\mathbf{X}_i$ and $\mathbf{H}_i$ in this model is that variables in $\mathbf{H}_i$ help identify selection on unobserved factors. As a result, variables that might be related to children's educational attainment and their location belong in $\mathbf{H}_i$. Our baseline specification of $\mathbf{H}_i$ 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 IIIE, we show that our results are nearly identical when we add several other variables to $\mathbf{H}_i$: indicators for whether only the father is present, whether only the mother is

[11] Online Appendix A follows Finkelstein, Gentzkow, and Williams (2021) and derives equation (5) formally.

[12] 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 $\hat{h}_i \equiv \mathbf{H}_i \hat{\lambda}$ 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 $\hat{\text{std}}\left(\hat{\tau}_o^{orig}\right)$ and $\hat{\text{std}}\left(\hat{h}_o^{orig}\right)$. Finally, we estimate $\hat{\eta}_j^{dest}$ using equation (5).

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.[13] Given these choices, we include a limited set of variables in $\mathbf{X}_i$: indicators for sex and age.

Equation (5) demonstrates how this approach uses selection on observables—in terms of both the ratio of standard deviations of origin effects and the amount of selection on observed schooling capital, h_j^{dest}—to adjust for the *remaining* selection on unobserved schooling capital, η_j^{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 ($h_j^{dest} > 0$) also attracting children with higher amounts of unobserved schooling capital ($\eta_j^{dest} > 0$).[14] 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 η_j^{dest} would have the wrong sign. In online 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 unobservables is plausible. As noted in Section I, 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 IIE), and the children of these adults likely had higher unobserved human capital (because of either "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 IIIF, 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 (2018b) 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. Online 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).

[13] These variables are similar to those included in Card, Domnisoru, and Taylor (2022).

[14] Because the destination fixed effects are normalized to have migrant-weighted averages of zero, a positive value of h_j^{dest} or η_j^{dest} implies that such a destination attracts children with above-average levels of observed or unobserved schooling capital.

C. *Estimating the Effect of Moving North*

The results obtained from the approach in Section IIB 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 following estimate:

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

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

$$Y_i = \mu_0 + \mu_1 M_i + \mathbf{X}_i \mu_2 + u_i, \tag{7}$$

where Y_i measures an outcome in adulthood (such as earnings), M_i is an indicator for residing in the North, and $\mathbf{X}_i$ is a vector of controls to adjust for selection into migration. The most stringent specifications use matched census data to include premigration household fixed effects, ensuring that identification comes from comparisons of siblings who vary in migration decisions. The term μ_1 is the key parameter of interest in this regression, which is identified by comparing migrants and nonmigrants born in the South.

When destinations are exogenous, it is straightforward to show that $\hat{\Delta}^{N-S}$ in equation (6) converges to μ_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 IIIB and online 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.

D. *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 $\mathbf{X}_i$ and $\mathbf{H}_i$.

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.[15] Overall, 80 percent of Black children ages 14–18 lived with at least one parent in the 1940 census.[16] Section IIIG 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 nonmigrants (who reside in their birth state and may live in the South or North).[17] The inclusion of nonmigrants helps identify ψ and λ 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 migrants. These migrant children lived in 728 destination counties in 1940.[18] 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.[19] 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.[20] Moreover, the vast majority

[15] Card, Domnisoru, and Taylor (2022) use a similar sample restriction in their study of intergenerational mobility in education using the 1940 census.

[16] 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.

[17] 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.

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

[19] Prior work on place effects by Chetty and Hendren (2018a) estimates models of exposure effects using Internal Revenue Service 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.

[20] 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. Online Appendix Figure 2 reports the share of each cohort that is living in the North in the 1930 and 1940 censuses.

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.[21] We match individuals based on first and last name, birth state, age, and race, using the algorithm of Abramitzky et al. (2021). We restrict the matched sample to individuals who are uniquely matched from 1920 to 1940 and from 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 nonmigrant children.[22] 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 additional robustness tests.[23]

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.[24] 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.[25]

[21] Online Appendix D provides full details on the construction of the matched sample.

[22] 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).

[23] 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 they are 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.

[24] In particular, a comparison of Black individuals observed in the 1940 census shows that average years of schooling for 18-year-olds are 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, respectively, 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.

[25] 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.

Table 2—Summary Statistics, Analysis Sample

	Nonmigrant			Migrant		
Location in 1940:	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)

E. *Patterns of Education and Migration*

Table 2 reports summary statistics for migrants and nonmigrants 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 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.[26]

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 nonmigrants. We measure the favorability of migrant observables by computing the average index of observed schooling capital, $\hat{h}_i = \mathbf{H}_i \hat{\lambda}$, for migrants that move to each county j, using parents' education in $\mathbf{H}_i$. The figure illustrates a binned scatterplot that shows how the observed index in county j is correlated with the average educational attainment of nonmigrant children in the county. The slope coefficient of 0.21 implies that destinations with an extra year of nonmigrant average educational

[26] Online 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 twelfth grade education or more.

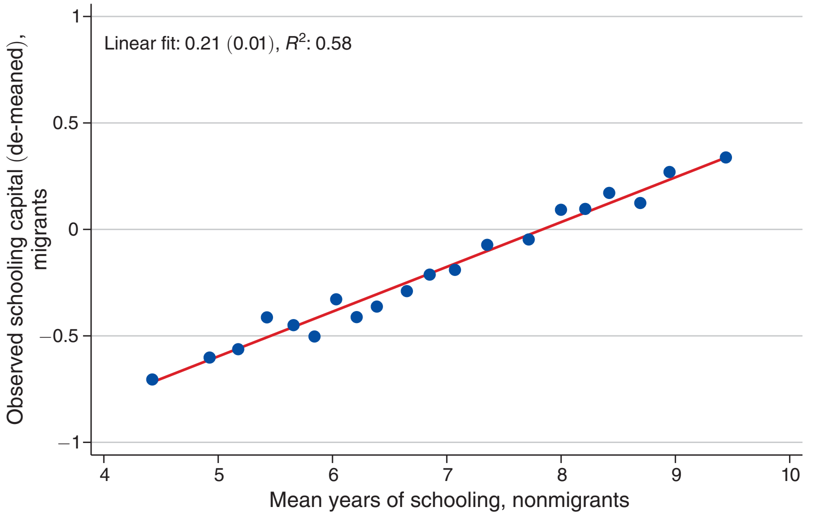

FIGURE 1: OBSERVED SCHOOLING CAPITAL OF MIGRANTS AND EDUCATIONAL ATTAINMENT OF NONMIGRANTS, BLACK CHILDREN AGE 14–18

Notes: Figure displays a binscatter of the de-meaned observed schooling capital, $\hat{h}_i = \mathbf{H}_i\hat{\lambda}$, of migrants against the average educational attainment of nonmigrants. We include indicators for father's and mother's education in $\mathbf{H}_i$ 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)

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.

F. *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, $\hat{h}_j^{dest}$, and unobserved variables, $\hat{\eta}_j^{dest}$.

The bottom panel of Table 3 reports the standard deviation of observed and unobserved schooling capital across destinations. Both observed and unobserved

Table 3—Inputs into Selection Correction

	Standard deviation
Origin components	
Observed schooling capital (h_o^{orig})	0.081
	[0.076, 0.087]
Unobserved schooling capital (η_o^{orig})	0.082
	[0.070, 0.096]
Destination components	
Observed schooling capital (h_j^{dest})	0.392
	[0.382, 0.400]
Unobserved schooling capital (η_j^{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, η_o^{orig}, is identified directly from equation (3) as τ_o^{orig}:

$$Y_i = \mathbf{X}_i \psi + \mathbf{H}_i \lambda + \tau_o^{orig} + \tau_j^{dest} + \tau_j^{nm} + \tilde{\eta}_i,$$

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

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

This equation identifies h_o^{orig} and h_j^{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

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

The key confounding variable for estimation of place effects is η_j^{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)

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 IIIE 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.

III. 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

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 (γ_j)	1.074
	[1.057, 1.093]
Schooling capital ($\bar{\theta}_j$)	0.793
	[0.758, 0.832]
Correlation of γ_j and $\bar{\theta}_j$	0.153
	[0.110, 0.194]
Selection corrected	
Place effects (γ_j)	0.848
	[0.812, 0.888]
Schooling capital ($\bar{\theta}_j$)	1.009
	[0.954, 1.073]
Correlation of γ_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):

$$Y_i = \mathbf{X}_i \psi + \mathbf{H}_i \lambda + \tau_o^{orig} + \tau_j^{dest} + \tau_j^{nm} + \tilde{\eta}_i,$$

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

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

that our estimates and conclusions are robust to alternative ways of adjusting for selection on unobservables.

A. *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 nonmigrants' schooling capital. A positive covariance does not indicate a failure of the selection correction but, instead, is consistent with the same

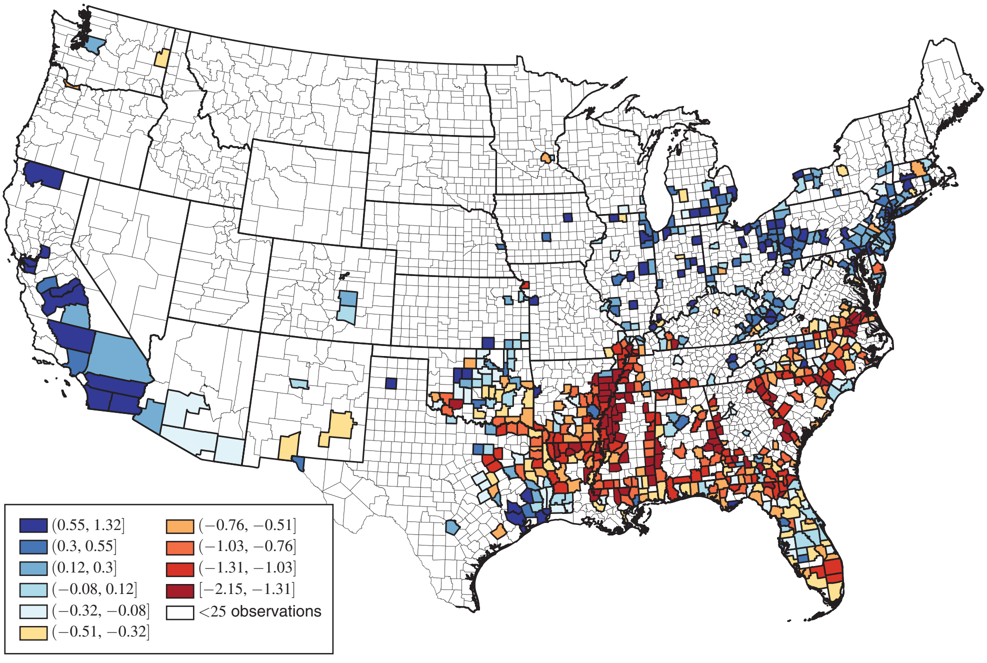

Figure 2. Place Effects on Years of Schooling in 1940, Black Children Age 14–18

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)

factors increasing the schooling of migrant and nonmigrant 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 ninetieth percentile leads to a 0.6-year increase in schooling relative to the average place, while the tenth 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.[27,28]

A natural question is how closely the selection-corrected place effects correspond to the outcomes of nonmigrants. To examine this, Figure 3 plots place effects against average years of schooling for Black children of nonmigrants. The

[27] 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.

[28] We also estimate place effects separately for girls and boys. These results are reported in online 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.

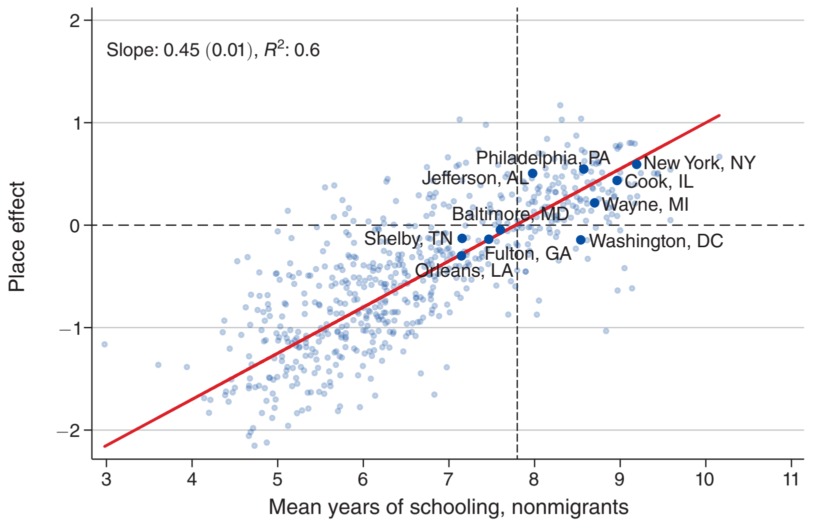

Figure 3. Place Effects versus Average Years of Schooling for Nonmigrants, Black Children Age 14–18

Notes: Figure displays empirical-Bayes-adjusted place effects against average years of schooling for nonmigrants. 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)

slope coefficient of 0.45 implies that when their family moved to a county with one year higher schooling attainment among nonmigrant children, children of migrants gained an additional 0.45 years on average. This indicates substantial but incomplete convergence in outcomes for migrant children.[29] Moreover, simple comparisons of counties on the basis of nonmigrants' 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 nonmigrants' schooling.[30]

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

[29] Place effects for Black children also are higher in counties where White children of nonmigrant parents have higher education, as shown in online Appendix Figure 5.

[30] 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 the 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. Online Appendix Figure 6 provides additional evidence on this issue by plotting place effects against the share of migrant children in each destination.

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 twenty-fifth 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)

place effects (with mean zero and standard deviation one).[31] 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.

[31] 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.

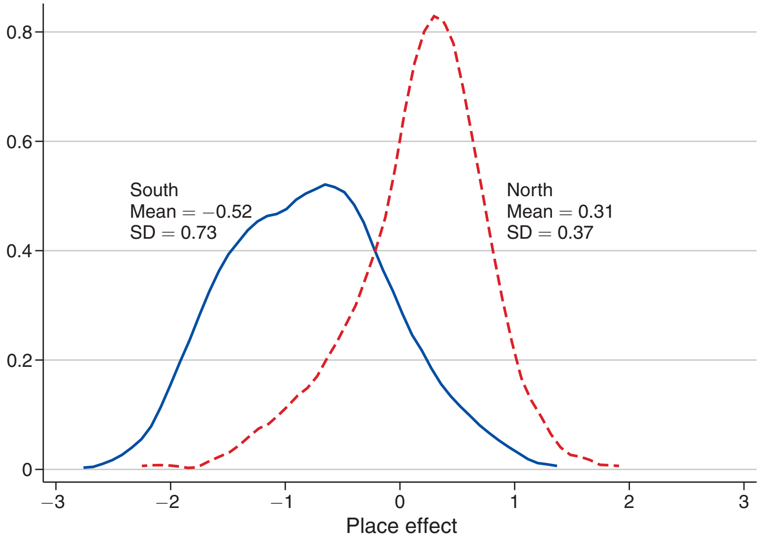

FIGURE 4. DISTRIBUTION OF PLACE EFFECTS ON YEARS OF SCHOOLING IN SOUTH AND NORTH, BLACK CHILDREN AGE 14–18

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)

B. *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.[32] 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.[33]

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).[34] The increase in quality-adjusted education would most

[32] 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.

[33] 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).

[34] We calculate the effect of moving to the North on the educational attainment of all children (i.e., migrants and nonmigrants) by multiplying the 0.8-year effect of moving to the North by the 21 percent of Black children that 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).

likely be higher given prior evidence on regional differences in the quality of schooling (Card and Krueger 1992b; Carruthers and Wanamaker 2017b). 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 IIC, an alternative estimate of the effect of moving North comes from a regression with a North indicator as the treatment variable. Online 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 ten migrants.[35] 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 online 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.[36]

C. *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, 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

[35] As detailed in online 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 ten migrants.

[36] 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).

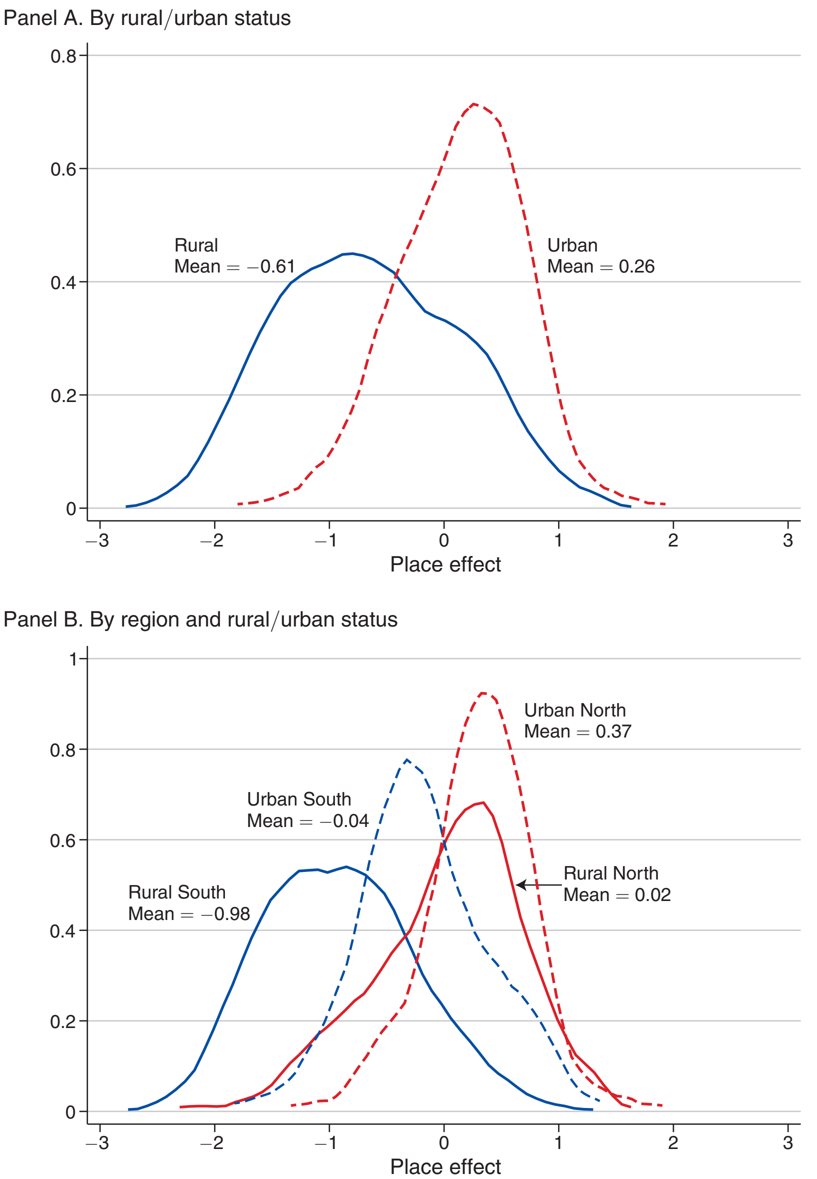

Figure 5. Distribution of Place Effects by Region and Rural/Urban Status, Black Children Age 14–18

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

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

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.[37]

D. *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 (e.g., Myrdal 1944).

We investigate racial heterogeneity by estimating place effects following our approach from Section II for the children of Southern-born White migrants. There were large outmigration flows of White individuals from the South during the Great Migration.[38] Online Appendix Figure 7 summarizes these results by illustrating densities of place effects for White children in Northern and Southern counties.[39]

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 0.83 years for Black children.[40] In line with this result, the North and South distributions for White children display much more overlap than those for Black children. These results complement other recent work studying the importance of place of residence (Tan 2019b, 2023).

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 that increase Black children's educational attainment by 1 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.

[37] 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.

[38] 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).

[39] 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.

[40] Online 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.

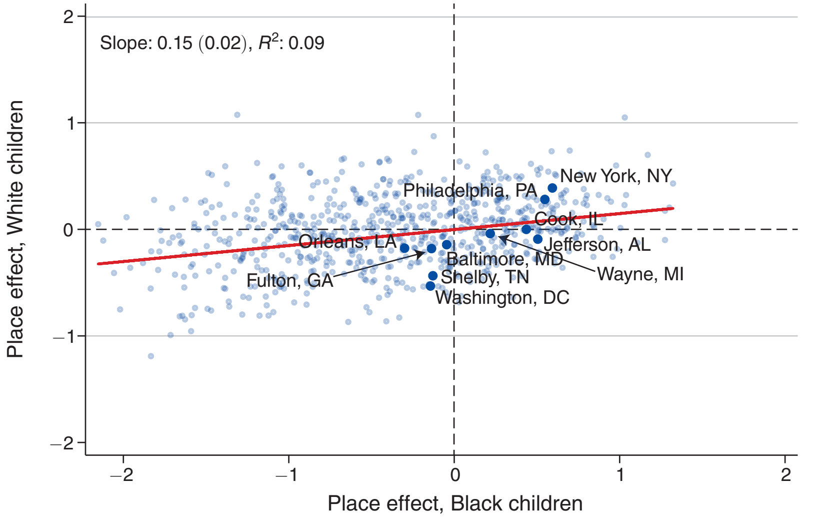

Figure 6. Place Effects for White versus Black Children Age 14–18

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)

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), and segregation and police spending (Derenoncourt 2022). Historical accounts suggest that White backlash would be stronger in places where the Black population share rose by more (Henri 1975). However, online Appendix Figure 8 shows that place effects for Black children are larger in destinations where the Black population share rose by more from 1910 to 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 IV and VA.

E. *Robustness: Additional Variables for Selection Correction*

Our baseline model includes indicators for father's and mother's years of schooling in $\mathbf{H}_i$. 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 $\mathbf{H}_i$. Our second model includes indicators for parental schooling plus indicators for whether

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

	(1)	(2)	(3)	(4)
Panel 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	
Panel 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
Panel 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 $\mathbf{H}_i$. In specification (1), $\mathbf{H}_i$ includes indicators for father's and mother's education. In (2), $\mathbf{H}_i$ 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), $\mathbf{H}_i$ 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, $\mathbf{H}_i$ 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, whether children's grandfather (observed in 1920) is literate, whether he is a farmer, and his Duncan socioeconomic index (a measure of income based on occupation).

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

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.[41] One

[41] The standard deviation of place effects and the average North–South difference in place effects also are very similar across the three specifications of $\mathbf{H}_i$.

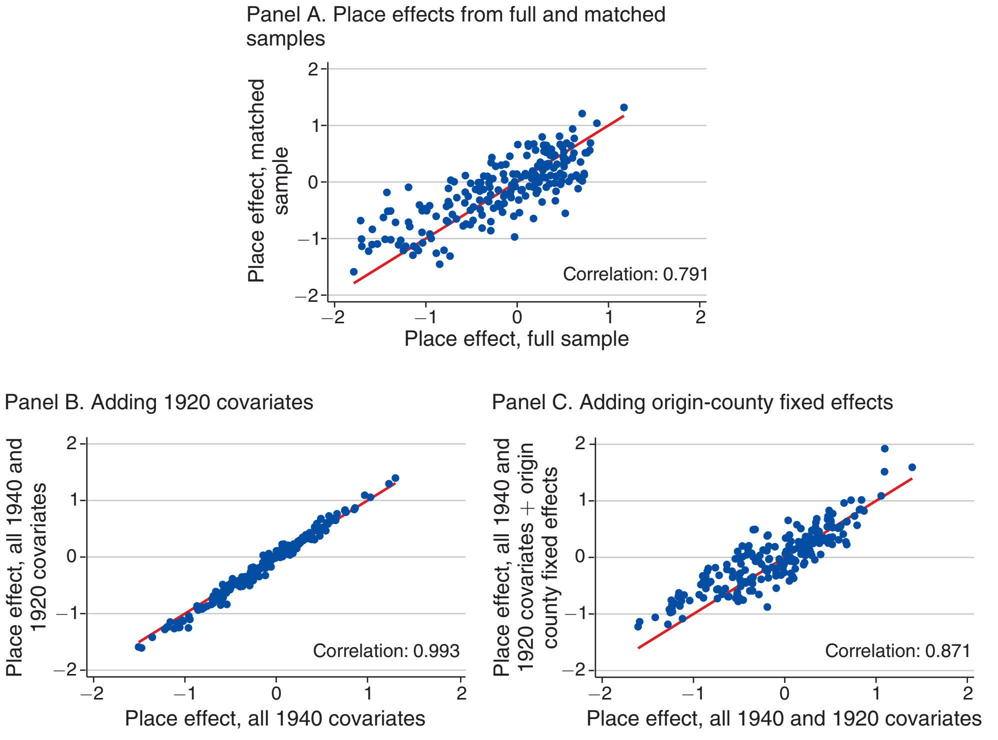

Figure 7. Robustness of Place Effects to Additional Selection Correction Variables from Matched Sample

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 $\mathbf{H}_i$. 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)

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 premigration 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 baseline specification (where $\mathbf{H}_i$ 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.[42] 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 $\mathbf{H}_i$ 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, 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 $\mathbf{H}_i$ 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 $\mathbf{H}_i$ (including covariates from the 1940 and 1920 censuses) 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 fixed effects, the results from different versions of $\mathbf{H}_i$ 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.

F. *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

[42] The matched sample also differs slightly on observable characteristics, as discussed in online Appendix D.

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, $h_i = \mathbf{H}_i \lambda$) than the unobserved component of children's schooling capital:

$$\operatorname{corr}\left(T_{ij}, h_j^{dest}\right) > \operatorname{corr}\left(T_{ij}, \eta_j^{dest}\right). \tag{8}$$

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

$$\operatorname{std}\left(\eta_j^{dest}\right) < \operatorname{std}\left(h_j^{dest}\right) \frac{\operatorname{std}\left(\eta_o^{orig}\right)}{\operatorname{std}\left(h_o^{orig}\right)}. \tag{9}$$

These inequalities lead to violations of Assumptions 1 and 2. However, as discussed in Finkelstein, 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): $\operatorname{corr}\left(T_{ij}, \eta_j^{dest}\right) = C_1 \operatorname{corr}\left(T_{ij}, h_j^{dest}\right)$ *in the sample of migrants for all j.*

ASSUMPTION 4 (Relaxed Relative Importance): $\operatorname{std}\left(\eta_j^{dest}\right) / \operatorname{std}\left(h_j^{dest}\right) = C_2\left[\operatorname{std}\left(\eta_o^{orig}\right) / \operatorname{std}\left(h_o^{orig}\right)\right]$ *in the sample of migrants.*

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

$$\hat{\eta}_j^{dest} = C_1 C_2 \frac{\widehat{\operatorname{std}}\left(\hat{\tau}_o^{orig}\right)}{\widehat{\operatorname{std}}\left(\hat{h}_o^{orig}\right)} \hat{h}_j^{dest}. \tag{10}$$

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.

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 IIIF. 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)

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.

G. *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 1 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 online 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.[43] 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 online 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).

H. *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 twentieth 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 would not be included in our analysis sample). Using infant mortality rate data from Bailey et al. (2018), we

[43] 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 online 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 (i) very few Black youth obtained more than 12 years of schooling and (ii) individuals largely completed schooling by age 18.

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 online Appendix F.

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

IV. 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 2018b; 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).[44]

Column 1 of Table 8 reports 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 of resources in segregated schools in the South (e.g., Margo 1990; Card and Krueger 1992a, b; Carruthers and Wanamaker 2017a, 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 US children born

[44] 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 (2019a) uses a twin-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).

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

	Dependent variable: Place effect, children's education		
	Bivariate regressions	Multivariate regressions	
	(1)	(2)	(3)
Teachers per pupil	0.467	0.175	0.123
	(0.0371)	(0.0321)	(0.0315)
Median Black household income	0.619	0.427	0.361
	(0.0299)	(0.0333)	(0.0329)
Homicide rate	−0.413	−0.174	−0.0948
	(0.0584)	(0.0408)	(0.0385)
Incarceration rate	0.0425	−0.0103	−0.0142
	(0.0519)	(0.0260)	(0.0250)
NAACP chapter	0.430	0.156	0.117
	(0.0330)	(0.0310)	(0.0307)
South indicator			−0.479
			(0.0793)
Observations (counties)	728	728	728
R^2	—	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. Column 1 reports estimates of separate bivariate regressions for each explanatory variable. Columns 2–3 report estimates of multivariate regressions. Heteroskedasticity-robust standard errors in parentheses. See online Appendix G for details on variable construction and sources.

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

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.[45]

[45] Empirical studies yield mixed evidence on the importance of parental income and resources for child education in historical US 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 nonwinners. Studies in contemporary contexts also provide conflicting evidence on the importance of parental income. For example, studies of the Earned Income Tax Credit program suggest that cash transfers have meaningfully large impacts on test scores and college going (Dahl and Lochner 2012; Bastian and Michelmore

To further gauge the degree to which the positive correlation between place effects and median Black family income reflects potential earnings gains of migrants, we use our main approach from Section II to estimate place effects for log earnings of Black men ages 25–64 born in the South.[46] 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.[47,48]

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 (US 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 networks and community engagement—has important impacts on social and economic outcomes (Coleman 1988; Putnam 2000; Sampson, Raudenbush, and Earls 1997; Stuart and Taylor 2021b). 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

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 only to large increases in resources from lottery winnings. Jacob, Kapustin, and Ludwig (2015) 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.

[46] The 1940 census measures wage and salary income but not total earnings (which also include self-employment income). We impute earned income for self-employed individuals based on their race, region, and occupation, as detailed in online Appendix G.

[47] 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).

[48] In online 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.

throughout the twentieth century.[49] Column 1 of Table 8 shows that place effects were significantly stronger in counties with an 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 2 of Table 8 reports results. We continue to see a strong positive relationship between place effects and teachers per pupil: a 1 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 an 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 3 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 an 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 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 nonmigrant 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).[50] 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 intraregional 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.

[49] In our sample, 337 of 728 counties had an NAACP chapter in 1940.

[50] 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).

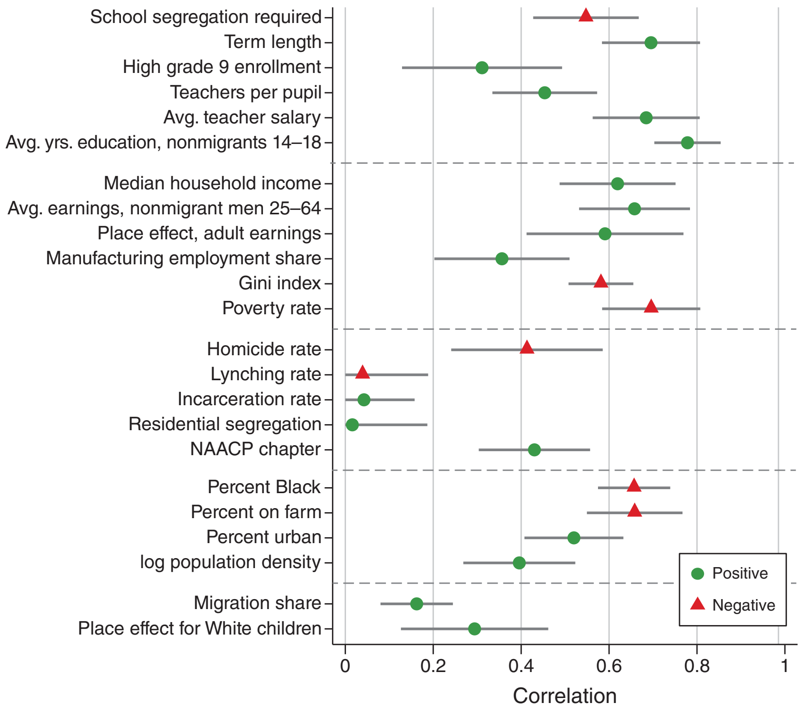

Figure 8. Full List of Correlates of Place Effects, 1940

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

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

V. The Geography of Black Opportunity over Time

As highlighted in the introduction, 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 twenty-fifth 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 positive 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.[51]

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 fifteenth. By the 1990s, the mobility measure was 0.5 standard deviations below average and ranked sixty-seventh. 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.[52] We study the mechanisms underlying these changes in Black children's opportunities next.

A. *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 online 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 IV, we focus on the roles of school quality, parental labor market opportunities, crime, incarceration, and social capital. Column 1 of Table 9 reports estimates of the descriptive relationship between changes in opportunity measures

[51] 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.

[52] Online 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 is 0.30 when using exposure effects for all races from Chetty and Hendren (2018a).

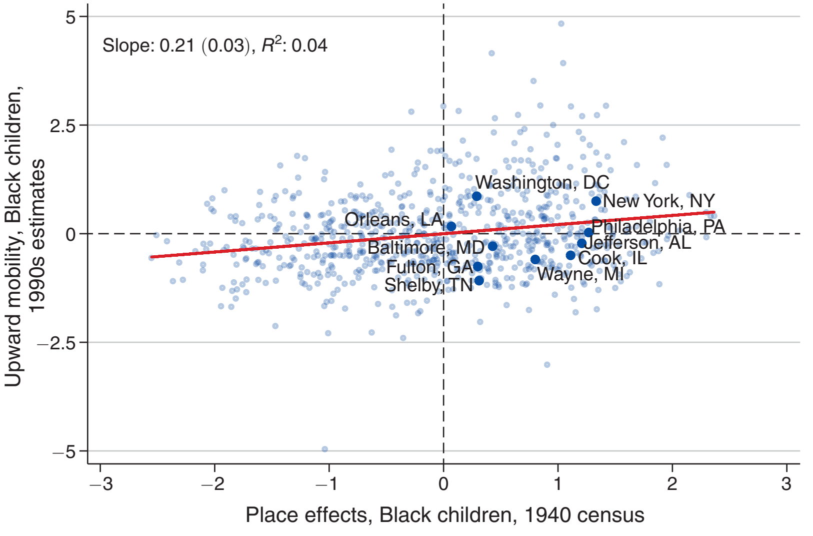

Figure 9. Relationship between 1940 Place Effects and 1990s Upward Mobility

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 twenty-fifth 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)

and place characteristics. Formally, we estimate the following first-difference specification:

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

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, β, 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 column 1 reinforce many of the conclusions from our descriptive analysis in Section IV. 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 an NAACP chapter (measured between 1960 and 1940; coefficient: 0.18).[53] The magnitudes of these correlations are generally similar to those in Table 8. At the same

[53] The data from Gregory and Estrada (2019) only contain information on NAACP chapters up to 1960.

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

	Dependent variable: Δ Opportunity measure (1990s versus 1940)			
	Bivariate regressions	Multivariate regressions		
	(1)	(2)	(3)	(4)
Δ *Teachers per pupil*	0.250	0.243	0.234	0.166
	(0.0314)	(0.0306)	(0.0305)	(0.0362)
Δ *Median Black household income*	0.429	0.405	0.393	0.360
	(0.0429)	(0.0416)	(0.0420)	(0.0429)
Δ *Homicide rate*	−0.161	−0.129	−0.0987	−0.0694
	(0.0381)	(0.0396)	(0.0417)	(0.0400)
Δ *Incarceration rate*	−0.111	−0.123	−0.120	−0.151
	(0.0285)	(0.0275)	(0.0272)	(0.0307)
Δ *NAACP chapter*	0.177	−0.0574	−0.0679	−0.0685
	(0.105)	(0.0969)	(0.0979)	(0.0984)
Δ *Percent Black*	−0.445		−0.204	−0.135
	(0.0825)		(0.0868)	(0.0893)
South indicator				0.407
				(0.121)
Observations (counties)	728	728	728	728
R^2	—	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 an 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. Column 1 reports estimates of separate bivariate regressions for each explanatory variable. Columns 2–4 report estimates of multivariate regressions. Heteroskedasticity robust standard errors in parentheses. See online Appendix G for details on variable construction and sources.

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

time, the results for incarceration in Table 9 contrast with the cross-sectional evidence. Specifically, we find that a 1 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 2 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 3 and 4, 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.[54]

Finally, we undertake two additional exercises to demonstrate that the conclusions from our within-place approach are robust. First, online Appendix Table 3 demonstrates that results are qualitatively similar when we rely on an alternative

[54] Online Appendix Figure 10 supplements these results by displaying binned scatterplots for each of the mechanisms included in Table 9.

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 (2018b) (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.[55] 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, online Appendix 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.

B. *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) that 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

[55] 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.

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.

VI. Conclusion

During the twentieth 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 narrowing US 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 1992a; 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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