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School of Public Policy and Management
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The Labor Economics of the Great Migration

A dissertation submitted to the graduate school in
partial fulfillment of the requirements for the degree of
Doctor of Philosophy in Economics and Public Policy

by

John Gardner

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May 21, 2014

ACKNOWLEDGEMENTS

I owe a debt of gratitude to my dissertation committee, Lowell Taylor, George-Levi Gayle, Brian Kovak and Robert Miller, for their comments, suggestions and valuable guidance. This work also benefited from discussions with Leah Platt Boustan and seminar participants from the Institute for the Study of Labor (IZA), the Southern Economics Association and the University of Mississippi.

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INTRODUCTION

The Great Migration refers to a period spanning roughly 1915 to 1970 during which millions of black men and women left their homes in the Southern United States in search of greater opportunity in the North. This exodus has been the subject of much scholarship by historians, economists and other social scientists, among whom there is a consensus that, because it dramatically altered the geographic distribution of the black population in the US, the Great Migration had profound implications for blacks' socioeconomic outcomes, both absolutely and relative to whites. In the essays of this dissertation, I study several labor-economic dimensions of the Great Migration that have received comparatively little attention in previous work: the impact of the Great Migration on the wages prevailing in Northern labor markets, the degree to which altruism towards future generations led the Great Migrants to leave the South, and the causal effect of Northward migration on migrants' wages. While the Great Migration is thought to have been instrumental in reducing black-white inequality, accounts of the episode often neglect the fact that many whites migrated North as well, although at much lower rates than blacks. In the first and third essays, I emphasize black-white differences in the effects of the Great Migration on the wages of, respectively, those already in the North and migrants. Throughout, I argue that the Great Migration can be viewed as a case study for internal migration and foreign immigration in general, as well as a historically-accurate counterfactual for contemporary flows of foreign immigrants to the US.

The econometric identification of causal effects and structural parameters is an explicit sub-theme of the dissertation. The first essay addresses the possibilities that migrants may have relocated to high-wage labor markets in the North, and that natives may have moved away from areas receiving large inflows of immigrants, obscuring the relationship between immigration and wages. The second and third take into consideration concerns that migrants may have differed from non-migrants in ways not observed in the data, confounding estimates of the influence of intergenerational altruism on the decision to migrate and the impact of migration on the wages of migrants. In particular, the third essay develops a general framework for interpreting group differences in mean treated-untreated comparisons as bounds on group differences in treatment effects when enrollment is selective.

In the first essay, I estimate the effects of the Great Migration on wages in the North. Using both the local labor markets approach, in which local immigrant shares are related to wages, and the structural, national labor markets approach, in which the estimated parameters of an aggregate production function are used to simulate the wage effects of migration, I find that Southern immigration during this period decreased wages appreciably for blacks, leaving those for whites unchanged. I argue that the source of this disparity is effective imperfect substitution between black and white labor. While studies of contemporary foreign immigration to the US have reached conflicting conclusions, generating controversy over whether native outmigration diffuses local labor-supply shocks, I find no evidence of outmigration among native blacks, permitting a unique cross validation of the local and national approaches. This research enriches our understanding of the consequences of the Great Migration and the evolution of the black white wage gap. In addition, the size and racial composition of the labor-supply shock due to the Great Migration make it a useful counterfactual to observed flows of contemporary foreign immigrants to the US and provide unusually high power for detecting wage effects.

In the second essay, I analyze the relationship between intergenerational altruism and migration by estimating a dynastic residential location choice model using intergenerational panel data that combines information about three generations of black families spanning much of the Great Migration. While many studies have considered intergenerational aspects of migration, there has been little work to quantify the impact of intergenerational altruism on migration behavior; my results provide direct evidence on this impact in the context of the Great Migration. I find that intergenerational altruism increases the probability of migrating by 4-6% on average. My estimates, which allow for correlated unobserved heterogeneity in location preferences, suggest that a large proportion of the population would be unlikely to migrate regardless of altruistic concerns; among the remainder of the population, the effect of altruism is considerably higher, about 12-16%. By comparing the effects of two counterfactual subsidies to the cost of migration, I further show that altruism affects migration primarily by inducing parents who would otherwise remain in the South to migrate North in order increase future generations' welfare.

In the third essay, I study the conditions under which information about the causal effect of a treatment can be identified by applying difference-in-differences regression to two groups that both self-select into treatment. I establish that, in many cases when selection and counterfactual outcomes can be described by a Roy model, differences-in-differences provide a lower bound on group differences in the average effect of the treatment on the treated. This group difference in causal effects is particularly informative in cases where treatment effect heterogeneity is of direct interest or when it is reasonable to assume that the average treatment effect is nonnegative for both groups. Furthermore, because the requirements for identification are relatively weak, this group difference provides a framework for understanding treated-untreated comparisons in causal terms in the absence of a credibly-exogenous source of variation in the propensity to be treated. I use the identification results to interpret North-South wage differentials during the Great Migration in terms of black-white differences in the causal effect of Northward migration on wages, finding that migration increased wages for black migrants by at least 24% as much as for white migrants.

1

THE GREAT MIGRATION AND WAGES IN THE NORTHERN UNITED STATES

1.1 INTRODUCTION

In the early twentieth century, the Southern United States witnessed a tremendous exodus as millions of Southern-born black men and women left their birthplaces in search of greater opportunity in the North. So numerous were these migrants that historians have named their egress the Great Migration. It is difficult to overstate the scale—or social and economic significance—of this migration, which, after its start around 1915, lasted into the 1970s (Carrington, Detragiache, and Vishwanath, 1996; Tolnay, 2003). By 1950, 2.5 million Southern-born blacks lived outside of the South; by 1980, this number was 4 million (Tolnay, 2003). Between 1915 and 1960, nearly 40% of the black population left the South (Carrington et al., 1996). This widespread immigration had profound implications for the demographic composition of the North: as I show below, fully 60% of the black labor force in the North was comprised of Southern immigrants by 1940. Although the Great Migration is rightfully viewed as a distinctively black episode, it was accompanied by substantial migration among whites; white immigrants actually outnumbered black immigrants, comprising just over 5% of the total labor force in the North, compared to about 2.7% for black immigrants.

The Great Migration was thus a large, long-lasting, and racially disparate supply shock to Northern labor markets, suggesting the potential for commensurately large wage effects, and making it a natural and historically meaningful laboratory for understanding how immigration affects absolute and relative wages. In this paper, I estimate the impact of this migration on wages in the North. I do so using two completely different methodologies. The local labor markets approach (see Altonji and Card, 1991; Card, 2007, 2009, 1990, e.g.) uses variation in the amount of immigration across labor markets in order to identify the wage effects of immigration. The structural, national labor market approach (see Borjas et al., 2008; Borjas, 2003, 1994, 1999; Ottaviano and Peri, 2012, e.g.) proceeds by specifying the technology through which aggregate output is produced, estimating the parameters governing this technology, and using these estimates to simulate the effects of immigration on equilibrium wages. Unlike studies of the effects of foreign immigration to the US, I obtain similar results under either methodology, adding credibility to my estimates and suggesting that these empirical approaches are reconcilable in principle.

I find that the Great Migration decreased the wages of blacks working in the North appreciably, but had little effect on those of whites. My flagship local labor markets regression implies that, by 1950, Southern immigration had decreased the annual wages earned by blacks working in the North by as much as 23%, a result which is robust to a number of non-causal interpretations. Under my preferred structural simulation of the impact of the Great Migration, I find that Southern immigration between 1940 and 1960 caused the average black wage to decrease by 22%. Using both approaches, I find that, although white migrants outnumbered black ones, Southern immigration caused little change in the wages of Northern whites.

I argue that the racial disparity in the estimated effect of Southern immigration can be explained by a combination of effective imperfect substitution between blacks and whites (arising either because of productivity differences or labor market segmentation due to racial discrimination) during the study period and outmigration among native whites. I present evidence that some native whites left labor markets that received large influxes of Southerners, diffusing labor-supply shocks across labor markets and, consequently, attenuating estimates of the effect of Southern immigration on white wages based on wage comparisons between markets with different amounts of immigration. This equilibrating outmigration was evidently driven by white Southern immigration, and I find no evidence of an outmigration response among blacks. When the North is treated as one large labor market, however, the only form of outmigration that can attenuate estimates of the impact of immigration on wages is South-North migration, which was substantially less common. My national labor market simulations, which are therefore much less contaminated by geographical arbitrage effects, suggest that segmentation between the markets for black and white labor in the North plays a greater role in explaining the estimated racial disparity in the effects of Southern immigration on wages in the North. I also provide summary statistics and structural elasticity estimates that adduce effective imperfect substitution between black and white labor during the period under study.

This research advances two literatures. The first concerns the economic impacts of the Great Migration and the evolution of the black-white wage gap. It is well known (see, for example, Smith and Welch, 1978, 1989; Donohue, III and Heckman, 1991) that the Great Migration was an important instrument for black relative economic progress. It was not, though, without detriment to its namesake migrants. Blacks in the North faced higher rates of unemployment (Sundstrom, 1998) and earlier mortality (Black, Sanders, Taylor, and Taylor, 2011) than their Southern counterparts. The Great Migration may have also had negative consequences for Northern cities, potentially contributing to discrimination, white flight, residential segregation, and ultimately, exacerbating racial inequality (Frey, 1979; Massey and Eggers, 1990; Massey and Denton, 1993; Tolnay, 2003), though there has been little rigorous empirical work connecting the Great Migration to these phenomena.

In an important contribution, Boustan (2009) finds that the wages of blacks working in the North would have been 7% higher in 1970 absent the Southern immigration that took place between 1940 and 1970, while white wages would have been unchanged. My paper extends Boustan (2009) along two critical dimensions. First, my local labor markets estimates provide quasiexperimental evidence on the impact of the Great Migration on wages that is not sensitive to assumptions about the structure of the labor market. While the national labor markets approach has the virtue of robustness to outmigration-induced attenuation of wage effect estimates, it also makes strong assumptions about the process that determines output, and thus wages. If the assumed structure is a poor representation of the the underlying aggregate production function, the resulting simulations of the impact of immigration on wages will be incorrect. That I obtain similar results using either methodological tack lends credibility to both papers. Second, I assess the sensitivity of estimates of the elasticity of substitution between blacks and whites—a key determinant of the impact of immigration on the wage distribution—to different identification strategies. I present evidence that previous estimates of this elasticity may have been inflated by endogenous labor supply responses to within-skill-group wage fluctuations, particularly among blacks. In addition, my empirical analysis focuses on earlier waves of the Great Migration, during which blacks and whites may have been less effectively substitutable. Furthermore, my wage simulations are weighted to account for the contemporaneous evolution of the national skill distribution and the size of the immigrant labor force in the North. This correction scheme helps offset a mechanical bias that arises because, as a consequence of increases in mean educational attainment over time, skill groups that were initially small tended to experience the largest proportional changes in immigration. Although there is broad agreement between our findings, the differences between my implementation and Boustan's combine to produce large differences in the simulated effects of the Great Migration; I estimate the long-run impact on the mean black wage to be twice that reported in Boustan.

The second literature concerns the effects of international immigration and internal migration on wages and racial wage differentials. Studies of the wage impacts of contemporary foreign immigration to the US have reached conflicting conclusions (cf. Card, 1990; Altonji and Card, 1991; Card and DiNardo, 2000; Card, 2001, 2009; Borjas, 2003, 2006; Ottaviano and Peri, 2012), generating controversy over whether natives exit areas receiving large immigrant inflows and, consequently, whether local labor markets estimates based on geographical correlations or national labor markets estimates based on aggregate production functions are best-equipped to uncover the causal effects of immigration. Additionally, Boustan, Fishback, and Kantor (2010), studying the Great Depression, find no evidence of wage effects, but a strong outmigration response to internal migration. Two aspects of the Great Migration make it a uniquely-clarifying case study. During the periods that I analyze, blacks are clustered into a small number of Northern metropolitan areas, limiting their scope to migrate in response to inflows of Southern immigrants. This eliminates concerns about equilibrating outmigration, allowing me to validate the local and national approaches against each other. I find that they agree strongly, demonstrating that results obtained using these different approaches are inherently reconcilable, and lending credence to the hypothesis that outmigration can, perhaps only partially, explain why the results of studies of foreign immigration depend on the methodology used. The Great Migration's size also makes it particularly useful for analyzing the relationship between migration and wages. As noted above and evidenced below, well over half of the blacks working in the North in 1950 were born in the South. In contrast, the immigrant fraction of the contemporary labor force in the US is considerably smaller.[1] Studying such a large implied labor-supply shock provides me with unprecedented statistical power to detect wage impacts, even when individual wages are highly heterogeneous and samples are relatively small.

Previous work has shown that the impact of immigration on wages depends on the substitutability between the immigrant and native populations (Card, 2009; Ottaviano and Peri, 2012) and that immigrant inflows can exacerbate racial wage differentials (Borjas, 1987; Borjas, Grogger, and Hanson, 2010). By demonstrating that imperfect substitution between blacks and whites concentrated the wage impacts of Southern immigration on blacks, ultimately limiting the ability of the Great Migration to narrow the national black-white wage gap, my results redouble the evidence on these points. That my findings echo those of studies of contemporary foreign immigration suggests that the Great Migration, though it was the product of distinct historical circumstance, is a case study with high external validity. At the same time, because of its differences with contemporary immigrant flows, the Great Migration provides an empirically rich counterfactual to observed foreign immigration. For example, Card (2009) argues that foreign immigration to the US has had little impact on relative wages because the immigrant skill distribution is similar to that for natives. This is in stark contrast to the Great Migration, which disproportionately increased the supply of labor among blacks in the North, decreasing black relative wages commensurately.

1.2 THE GREAT MIGRATION AND LOCAL LABOR MARKETS IN THE NORTH

1.2.1 *Data, sample restrictions, and variable construction*

Although the Great Migration, which began around 1915, lasted through the 1970s, South-North flows among blacks had already started to slow by the 1960s (Eichenlaub et al., 2010; Tolnay, 2003). From the perspective of the effect of Southern immigration on the

1 Card (2001), for example, documents that foreign immigrants comprised less than 15% of the population of the 175 largest cities in 1990.

wages of those living in the North, it is thus earlier waves of the Great Migration that are of primary interest. Detailed wage data from these periods are somewhat scarce; the Census did not collect wage information prior to 1940, while the datasets most widely-used to study wage dynamics (the PSID, NLS, CPS, etc.) did not begin until the 1960s. For this reason, I use the Integrated Public Use Microdata Series (IPUMS) 1% samples of the 1940 and 1950 decennial U.S. Census, provided by the Minnesota Population Center at the University of Minnesota (Ruggles, Alexander, Genadek, Goeken, Schroeder, and Sobek, 2010).[2] These data include information on respondents' earnings and labor supply decisions in the year before enumeration, as well as demographic characteristics such as race, age, education, birthplace and residential location measures. One limitation of these data is that only a subset of households were asked wage and education questions in the 1950 Census, so the resulting samples for that year are relatively small.

I use the Eichenlaub, Tolnay, and Alexander (2010) definition of the "South": Oklahoma, Texas, Arkansas, Louisiana, Kentucky, Tennessee, Mississippi, Alabama, North Carolina, South Carolina, Georgia, Florida and Virginia are Southern states. I refer to all other states as the "North." I restrict the sample to U.S.-born, black and white men, aged 16-64, who earned nonzero income in the year prior to enumeration. Throughout this paper, "native" means born in the North, while "immigrant" means born in the South.

I define local labor markets as metropolitan areas.[3] A potential complication with this choice is that, during the period under study, blacks living in the North are clustered into a small number of metropolitan areas. In the regression estimates below, I drop metropolitan areas with fewer than ten members of the racial group being considered in order to eliminate influential outliers. Appendix Table 1.17 lists, for each Census year, the metropolitan areas with at least ten native black respondents, as well as the sample sizes and amount of race-specific and overall Southern immigration in those areas; although Northern blacks live in a small number of metropolitan areas, there is substantial variation in the size of the immigrant labor force across these areas. In the empirical results that follow, I undertake a number of exercises in order to assess the sensitivity of the estimated effects of immigration to the number of Northern metros included in the black regression samples.

Two of the key dependent variables in the models estimated below are the logarithms of annual and weekly wages. Annual wages are simply self-reported income from wages and salaries in the year preceding the Census. Weekly wages are annual wages divided by the number of weeks worked. I inflate wages to 1999 dollars using the IPUMS-supplied CPI weights.

2 The IPUMS Census sample for 1960 does not include geographical identifiers finer than state of residence, precluding the use of this data for local labor markets analysis of immigration.

3 In the 1950 Census, metropolitan areas are referred to as Standard Metropolitan Areas (SMAs). The 1940 Census did not use a metropolitan area concept, but the IPUMS 1940 Census sample identifies areas falling within the 1950 SMSA boundaries that contain enough respondents to meet confidentiality requirements (Ruggles et al., 2010).

Some of the independent variables used below are constructed metropolitan area averages. Of note, I calculate the fraction of the Northern labor force (men aged 18-64 reporting nonzero income) comprised of Southern-born men, Southern-born black men, and Southern-born white men. When calculating metro-level averages, I weight each observation by the IPUMS-supplied person weights in order to increase the degree to which the averages represent their underlying population moments.

Table 1.1 presents several summary measures of the amount of immigrant labor in the North. Southern immigrants comprised about 8% of the labor force in 1940 and 10% in 1950. However, the distribution of immigrant labor among racial groups is highly skewed, with between 63 and 66% of the black labor force in the North consisting of Southern immigrants, and only 5-7% of the white labor force Southern born. At the same time, black Southerners comprised only 3-4% of the overall labor force in the North, while white Southerners comprised 5-7% of it. These summary statistics show that, while a large fraction of blacks living in the North are Southern-born, the black population is sufficiently small that Southern-born black labor is a comparatively small fraction of the overall labor force in the North. The table also contains corresponding statistics for recent immigration. The 1940 Census identifies the metropolitan areas in which respondents lived five years before enumeration, while the 1950 Census identifies the metropolitan area in the year just before enumeration. Only about two percent of the North's labor force as of 1940 resided in the South in 1935, while only a slightly smaller fraction of the labor force as of 1950 resided in the South in 1949. These statistics suggest that the estimates below are more likely to represent longer-run effects of immigration on wages.

Table 1.2 summarizes the distribution of education among natives and immigrants living in the North. Overall, the native and immigrant distributions are similar, though immigrants are overrepresented at lower educational levels for both races and years, with Southern immigrants less likely to have a high-school degree or better. That the educational distribution among immigrants is not too dissimilar than that for natives suggests that the wage effects of immigration will not be concentrated on any particular native subgroup. For this reason, in the analysis below, I examine the effects of immigration on overall average wages.

1.2.2 *Empirical strategy*

The "local labor markets" approach, which relates variation across geographic areas in the wages of natives to that in immigration in order to deduce the impact of the latter on the former, has a long history in the immigration literature (see, for example, Card, 1990, 2001, 2007; Altonji and Card, 1991; Card and DiNardo, 2000; Card and Lewis, 2007; Boustan et al., 2010; Hunt, 2012). A complication to this approach is that migrants are not randomly assigned to receiving locations. It seems reasonable, for example, to suppose that Southern immigrants selected into areas in the North where, owing to productivity or demand shocks, wages were higher than average.[4] The converse is also possible, e.g. if locations closer to the South simultaneously offered below-average wages and attracted immigrants by virtue of their proximity. In either case, estimates of the effect of immigration on wages based on the spatial correlation between these variables may be biased.

Altonji and Card (1991), drawing on research by Bartel (1989), argue that enclaving among migrants—the tendency of successive generations of immigrants to live in the same areas—provides a source of variation in immigration that is uncorrelated with the contemporaneous economic conditions of local labor markets.[5] To exploit this variation, I predict contemporary immigration shares using 1920 settlement patterns. In particular, I predict the fraction p_{jt} of the labor force comprised of immigrants in area j during decade t using $\hat{p}_{jt} = \mu_{j20} M_t / N_{jt}$, where μ_{j20} is the fraction of all Southern immigrants who resided in area j in 1920, M_t is the total number of Southerners living in the North in decade t, and N_{jt} is the total population of area j in decade t.[6] I then use $\hat{p}_{jt}$ to instrument for the actual fraction of the labor force consisting of immigrants from the South. Of course, instrumental variables estimates based on predicted immigration patterns may still be biased if there are serially-correlated metro-level unobservables that affect both wages and immigration (e.g. if high-average-wage metros in 1920 attracted a disproportionate share of the immigrant population in 1920 and also tended to have above-average wages in 1940 and 1950). I take several steps, detailed below, to account for this possibility.[7]

Figure 1.1 plots, for 1940 and 1950, the fraction of the labor force born in the South against predictions of that fraction based on 1920 settlement patterns. As the figure demonstrates, predicted immigration is a strong instrument for observed immigration, with most of the plotted points lying near the regression line. I present formal first-stage regression results in Appendix Table 1.18, which also shows that predicted immigration

4 This hypothesis is not without empirical support. Tolnay (2003) presents some anecdotal evidence of locational selection, while Collins (1997) argues that demand shocks caused by restrictions on foreign immigration played an important role in catalyzing the Great Migration.

5 Cf. Card and DiNardo (2000); Card and Lewis (2007); Card (2001, 2007, 2009).

6 I use 1920 settlement patterns for several reasons. First, the Great Migration did not begin until roughly 1915, so prior to 1920 the number of Southern immigrants residing in the North is small. Second, metropolitan areas are only identified in the IPUMS Census samples if they are sufficiently populous to meet confidentiality requirements, so using earlier settlement patterns reduces the number of metros that can be matched to lagged migration. Third, as Table 1.1 shows, many of the Southern immigrants living in the North in 1940 were already present by 1935; immigrants living in the North in 1940 may therefore have selected into locations on the basis of local economic conditions during the 1930s, so predicted migration based on 1930 settlement patterns may be more contaminated with selection effects.

7 It is also possible that the local average treatment effect estimated using lagged immigration to instrument for contemporaneous immigration differs from the average treatment effect. This may happen if less-skilled immigrants are more likely to be induced to locate in area j by the amount of existing immigration there. Boustan et al. (2010), who use variation in weather and New Deal spending to instrument for internal migration during the same time period, note this as well.

is strongly correlated with actual immigration in both the white and black regression samples for both years.[8]

1.2.3 *Native wages and Southern immigration*

Table 1.3 presents OLS and IV estimates of the impact of Southern immigration on the annual wages of native Northerners. These specifications model the log annual wage of individual i, living in metropolitan area j, as a linear function of the fraction of the labor force in area j comprised of immigrants, a full set of indicators for age and educational attainment, and in some specifications, additional covariates. I estimate separate models for each racial group and year. In order to account for the fact that the metro-level average variables included as regressors are generated in a first stage, I weight each observation by the number of observations used to construct these averages. The reported standard errors are clustered by metropolitan area.[9]

Specification (1) includes only the proportion of the local labor force comprised of Southern-born workers and indicators for age and education as explanatory variables. Specification (2), following the literature, adds a number of metro-level covariates, including the fractions of the black and white populations employed in manufacturing, the fractions of these populations living on farms, and the average educational attainment (measured in years of completed schooling) among these populations. The purpose of these covariates is to absorb any potential correlation between lagged (or contemporaneous, in the OLS estimates) migration and unobservable metro-level determinants of wages. For the 1950 samples, it is also possible to include lagged averages of the dependent variable. Specifications (3) and (4) mirror (1) and (2) with the exception that they include metro-average log annual wages in 1940.[10] These lagged average dependent variables serve several purposes. First, if the process that determines metro-level average wages over time exhibits true state dependence, excluding these variables may induce a correlation between lagged immigration and contemporaneous wages, even if lagged immigration is otherwise exogenous. Second, lagged immigration may have exerted a first-order effect on wages (e.g. if local labor markets responded to previous immigrant flows by changing their industrial composition); conditioning on lagged average wages can account for this possibility. Third, in the presence of permanent metro-level heterogeneity that affects both immigration and wages (e.g., permanent productivity shifters) and is not absorbed by the other covariates, even IV estimates based on lagged immi-

8 In some of the results below, I also predict race-specific immigrant shares p^r_{jt} using $\hat{p}^r_{jt} = \mu^r_{j20} M_t / N_{jt}$, where μ^r_{j20} is the fraction of the labor force in area j in 1920 consisting of Southern immigrants of race r. Table 1.18 shows that these group-specific predictors are also strong instruments for actual group-specific immigration.

9 Note that the asymptotic distribution of the instrumental variables estimates is independent of the sampling error arising from the estimation of the instruments (see Wooldridge, 2002, Appendix 6A).

10 These specifications are similar to those used in Altonji and Card (1991), Card (2007, 2009) and Boustan et al. (2010).

gration will be biased. I establish in Appendix B that, in my data, the inclusion of a lagged dependent variable is likely to be bias-reducing in this case. Finally, there may be time-varying, serially correlated, metro-level factors (e.g., serially correlated demand or productivity shocks) that affect migration and wages in each time period. Lagged average wages may provide a useful proxy for this type of heterogeneity as well.

The OLS estimates are presented in the left panel of Table 1.3. In the 1940 sample, none of the estimated coefficients on the proportion Southern are statistically significant, although the point estimate is negative for blacks in specification (2). In the 1950 sample, this coefficient is negative and statistically significant in each specification for blacks, and becomes larger in absolute value as more covariates are added to the model, a pattern consistent with selection into high-wage metropolitan areas. The coefficient on the proportion Southern is positive in all of the models estimated for whites, though it is not always statistically significant. The instrumental variables estimates are displayed in the right panel of the table. For blacks, each IV estimate of the coefficient on the proportion Southern is larger in absolute value than its corresponding OLS estimate. With the exception of specification (1) in 1940, the estimated effect of migration is negative for blacks in every specification, exhibiting the same pattern of a larger absolute effect as additional covariates are added to the model. In the richest specification (specification (4), estimated using the 1950 sample), the estimate implies that a 10% increase in the immigrant share of the labor force decreases annual wages among native blacks by about 23%. In contrast, none of the estimates of the effect of immigration on the wages of native whites are statistically significant, though most of the point estimates are positive.

The estimates in Table 1.3 should be viewed as general equilibrium effects that combine the (potentially different) effects of own- and cross-race immigration. In order to decompose these combined effects, I also estimate models that allow for different own- and cross-race effects. Specifically, I replicate specifications (1)-(4), replacing the fraction of the local labor force born in the South with the fraction of the labor force consisting of Southern blacks and that consisting of Southern whites. Table 1.4 presents the estimated coefficients for these models. The relationship between the OLS and IV estimates of these specifications mirrors that in Table 1.3, with the IV coefficients generally more negative than their OLS counterparts. Both the OLS and IV estimates suggest that the negative effect of immigration evident for blacks is driven primarily by competition between Northern-born blacks and black immigrants from the South. In the OLS estimates for blacks, the coefficients on the proportion Southern black are all negative, while those on the proportion Southern white are either positive or smaller in absolute value (none are statistically significant). In the IV estimates, the coefficients on the proportion Southern black are uniformly negative and statistically significant, while the coefficients on the proportion Southern white are small and, in most cases, statistically insignificant.

Among native whites, the evidence is somewhat mixed. Both the OLS and IV estimates suggest a weak positive effect of black immigration on the wages of Northern-born whites, although this effect disappears in the IV estimate of specification (4). At the same time, there is some evidence of a negative own-race effect among whites, with the coefficients on the white immigrant share of the labor force uniformly negative. However, these coefficients are never statistically significant when metro-level averages are included as covariates. The estimates for whites also suggest that the apparent negative effect of immigration on the wages of native blacks cannot be explained by a residually-negative correlation between immigration and unobservable metro-level determinants of wages. Were this the case, one would expect the coefficients on the black immigrant share variables to be negative in the regressions estimated on samples of native whites. These coefficients are uniformly positive, a finding consistent with either positive cross-race effects of immigration or residual bias arising from positive selection into metropolitan areas, but not selection among Southern-born blacks into low-wage areas.

Tables 1.5 and 1.6 present estimates of the effects of overall and, respectively, group-specific Southern immigration on the log weekly wages of natives. The results from both sets of models are similar to those for log annual wages. The IV estimates of the coefficient on the fraction Southern are all negative for blacks (and statistically significant in three of four specifications) while the estimated coefficients for whites are positive or close to zero and statistically insignificant in every specification. Similarly, the estimated models of the effects of group-specific immigration evince a negative effect of black immigration on the wages of native blacks, with little evidence of a substantial own-race effect for whites or cross-race effects for either group. For both the overall and group-specific models, the estimated effects of immigration on weekly wages are generally smaller in absolute value than in the corresponding models of annual wages. For example, the coefficient on the fraction Southern in specification (4) is -2.27 when the dependent variable is the log annual wage and -1.26 when that variable is the log weekly wage. This difference implies that adjustments to employment are one of the channels through which markets for black labor responded to immigration-induced supply shocks. If these adjustments caused some natives to exit the labor force, this finding may also suggest that the effects estimated using native black labor force participants understate the underlying impact of migration on the earnings of the overall population. It is worth noting that even the weekly wage estimates imply a large wage elasticity of labor supply. Sundstrom (1998) notes that black-white relative unemployment rates were higher in the North, a finding that he attributes to racial discrimination. Discrimination may have contributed to such large elasticities among blacks employed in the North by decreasing their labor supply along its intensive margin and possibly reducing their power to bargain for wages.

The justification that underlies my identification strategy is that, other things equal, immigrants prefer areas where others before them have moved. The instrumental variables estimates presented so far have been based on the total fraction of each metropolitan area's labor force comprised of Southern immigrants in 1920. That is, they are based on the propensity of Southerners to migrate to areas where other Southerners have gone. Although, as Figure 1.1 illustrates, predicted immigrant shares obtained this way are strongly related to actual shares, a more compelling behavioral story might be that immigrants originating from a particular locale prefer to migrate to areas where others from that locale have moved. To this end, I also estimate models that instrument for immigration using shares predicted from state-specific historical settlement patterns. I predict the decade-t fraction of the labor force in area j comprised of Southern immigrants using $\hat{p}_{jt} = \sum_k \mu_{kj20} M_{kt} / N_{jt}$, where μ_{kj20} is the fraction of immigrants from state k that resided in metropolitan area j in 1920 and M_{kt} is the number of immigrants originating from k in t. As Appendix Table 1.19 shows, using state-specific predictions to instrument for immigrant shares produces coefficients for both groups that are higher on the real line, regardless of how wages are measured. At the same time, even these IV estimates are lower than the OLS estimates presented previously, a pattern once again consistent with selective immigration into high-wage areas. The race-specific estimates, presented in Appendix Table 1.20, are similar. One explanation for this difference is that, since immigrating to closer areas is less financially and psychologically costly, state-specific settlement patterns may be partially identifying areas to which residents of a particular state are likely to move in response to transient shocks. Predicted immigration based on these settlement patterns may therefore be more contaminated with selection effects, in which case treating the entire South as the location of origin can reduce this bias by better-identifying migrations that are motivated by enclaving behavior.

1.2.4 *Native outmigration and Southern immigration*

Some previous research (see Borjas, 2006; Boustan et al., 2010) has found evidence that internal migration among natives is an important part of the response of local labor markets to immigration. In the presence of this type of equilibrating migration, immigration shocks do not necessarily translate to labor supply shocks, and estimates of the impact of immigration on wages may be attenuated. Both the 1940 and 1950 Census microdata samples provide information on the metropolitan areas in which respondents lived prior to the Census (in the 1940 Census, this is the area of residence five years prior to enumeration; in the 1950 Census it is the area one year prior). I construct an outmigration indicator equal to one if an individual no longer resides in his pre-Census metropolitan area. I then estimate a series of linear probability models that relate the probability of leaving this area to the recent immigrant share of the local population (the number of individuals living in a metro at the time of the Census but residing in the South in the pre-Census period divided by the total population of the metro in this period).[11] I focus on recent migration because outmigration in response to historical flows of immigrants most likely occurred before the pre-Census periods.

Table 1.7 shows the results of this exercise. Specification (1) contains only the recent immigrant share and indicators for age and education; specification (2) adds metro-level covariates.[12] The top panel of the table contains estimates of the overall effect of recent immigration on native outmigration. For 1940, the OLS estimates suggest an outmigration response to recent immigration among native whites (though this effect disappears when covariates are added to the model) and a negative relationship among blacks; for 1950, these estimates do not suggest responsive outmigration among natives of either race. The IV estimates are more positive for both years and both races. When covariates are included in the model, the estimated outmigration response among whites is about 0.7 in in 1940 and 1.4 in 1950, though the latter is not statistically significant, while the coefficients for native blacks are smaller and statistically insignificant. The bottom panel of the table presents the estimated effects of group-specific recent immigration. These estimates show that native white outmigration is primarily a response to flows of white immigrants. The IV estimates of the specification (2) coefficients on the white recent immigrant share are about 0.8 and 2.1 (for 1940 and 1950, respectively; both are significant), while those on the black recent share are very imprecise; for black natives, the IV estimates do not suggest black outmigration in response to any form of recent Southern immigration.

The results in Table 1.7 show that some native whites left metropolitan areas receiving flows of Southern immigrants. Their precise interpretation, however, requires some care. Some of the 1935-1940 and 1949-1950 native outmigration probably represents a response to immigrants who arrived before these periods began, overstating the actual relationship between Southern immigration and native outmigration.[13] A better specification would

11 I follow Boustan et al. (2010) in using recent immigration as a fraction of the pre-Census population in order to avoid a potential bias arising because, other things equal, native outmigration will mechanically increase the contemporaneous immigrant share. In addition, since the total number of recent immigrants is somewhat small in 1950, I use shares of the entire working-age population (rather than only those with nonzero wages) in order to increase the precision of the estimates, although this has little effect on the estimated magnitudes.

12 Estimates from specifications that include lagged average wages (not shown) are similar.

13 A back-of-the-envelope calculation confirms this intuition. Replacing the recent immigrant share with the total immigrant share and estimating specification (2) using the 1950 data produces a coefficient of about 0.06 for native whites. Since recent immigrants comprise roughly 15% of the total immigrant population in 1950, if this coefficient only reflected recent immigration, the implied coefficient on the recent immigrant share would be 0.4 (=.06/.15). The actual coefficient is about 1.4, implying that only roughly 30% of the 1949-1950 native outmigration occurred as a response to immigrants who arrived during this period. This effect is probably less pronounced in the 1940 sample since a five-year interval is more likely to capture outmigration occurring in response to current flows of immigrants. A simple example clarifies this logic. Suppose that half of the natives that will outmigrate as a response to immigration in a given year do so at a one year lag and that recent immigrant shares are roughly constant over time. Within a one-year period, half of the native outmigration attributed to recent immigration will actually be a response to immigrants who arrived in the previous year. Within a five-year period, only 10% of the observed outmigration attributed to immigration within the period will actually be a response to previous immigration, since half of the outmigration that takes place in the first year will be a response to immigrants who arrived before the beginning of the period.

include both the recent immigrant share and the total immigrant share at the beginning of the pre-Census period. Since recent and total immigrant shares are highly correlated and both are potentially endogenous, the estimation of such a model, which requires separate instruments for initial and recent migration, is challenging.[14] Although they likely overstate the actual outmigration response to recent immigration, these estimates do evidence an appreciable response among native whites, but no such response among blacks. These results may help explain the racial difference in the estimated effects of immigration on natives' wages.

1.2.5 *Robustness tests*

A clear pattern emerges from the evidence presented so far: Southern immigration appears to put downward pressure on the wages of native blacks, but exerts no influence on those of native whites. The effect among native blacks is driven by inflows of Southern-born blacks, with only tenuous evidence of cross-group impacts. Finally, immigration among Southern whites appears to induce native whites to move away from receiving areas, possibly diffusing would-be labor supply shocks. In this section, I present a series of tests to determine whether these findings are robust to alternative, non-causal interpretations.

My identification strategy requires that 1920 migration patterns are correlated with contemporary immigrant shares of the labor force but, conditional on covariates, otherwise uncorrelated with unobservable determinants of wages. As discussed above, if there is serially-correlated unobserved heterogeneity in metro-average wages for which the included covariates are poor proxies (and if this heterogeneity is correlated with immigration), IV estimates of the effects of immigration on wages based on lagged migration patterns may still be inconsistent. Recall, however, that I have already presented evidence on the direction of this bias: in models estimated using samples of native whites, the coefficients on the black immigrant share of the labor force are positive, a result which is prima facie inconsistent with the selection of black immigrants into low-average-wage metros. Any residual unobserved heterogeneity is therefore likely to impart an upward bias on my estimates, in which case they can viewed as bounding the actual effects from above.

Note also that my regressions identify the impact of recent immigration on the probability of outmigrating within the same period, not the impact on the total probability of outmigrating.

14 I also estimate models that relate natives' wages to recent immigration (not shown). The estimated effects in these models are generally much larger than those obtained using immigrant stocks, presumably because recent immigration is strongly correlated with existing immigration (in fact, when the total fraction Southern is included in these models, the coefficients on recent migration become small and statistically insignificant). While outmigration is more likely to be related to recent immigration, natives compete in labor markets with both new and existing immigrants, so the total immigrant share is the more appropriate measure of immigration's contribution to the labor supply. The wage effects presented above should therefore be interpreted as combining the effects of recent and existing immigration, to the extent that these differ.

To provide further evidence on this point, I present in Table 1.8 instrumental variables estimates that pool the 1940 and 1950 samples in order to allow for permanent unobserved metro-level heterogeneity. As the results show, demeaning the data substantially reduces the variation in the covariates, increasing the standard errors commensurately. Regardless, the instrumental variables estimates give no indication that the negative effects for blacks shown above owe to a residually-negative noncausal relationship between wages and migration. When no covariates are included, the fixed-effects coefficient for annual wages is statistically significant for blacks and the weekly wages coefficient is marginally so (in fact, three of the four models reject the one-sided hypothesis test that the black coefficient is positive). For whites, none of the estimated coefficients are significantly different from zero, even though these coefficients are estimated more precisely.[15] For both groups, adding metro covariates to the equation increases the estimated standard errors on the immigrant share coefficients, presumably because, after conditioning on individual-level variables, little variation remains in the metro-average variables. I also estimate models of the changes in metro-level average residual wages as a function of changes in immigration, where the residuals are taken from regressions that include a full set of indicators for age and education. The main difference between these estimators is that using mean residual wages does not remove the variation explained by the individual covariates from the area-level covariates, preserving more variation in the latter. The estimates, presented in the bottom panel of Table 1.8, are similar to the microdata fixed-effects results. Among blacks, the instrumental variables coefficients are significant and negative for both annual and weekly wages, while the white estimates are imprecise.[16]

If anything, the results in Table 1.8 suggest that models estimated in levels understate the effect of Southern immigration on wages, in which case they can be viewed as upper bounds. Since the within estimates identify the effects of immigration using innovations over time to the proportion Southern, another possibility is that these estimates capture the effect of more recent immigration, which may have a stronger effect on wages if local labor markets adjust to immigrant flows over time, e.g. through adjustments to the capital stock or changes in industrial composition. Although their confidence intervals are wide, the magnitudes of the fixed-effects estimates for blacks suggest that permanent unobserved heterogeneity may not accurately describe how metro-average wages evolve over time. If, for example, wages are mean-reverting but areas with historically high immigration tend to receive greater immigrant inflows, the coefficient estimates of a model

15 I also estimate fixed-effects specifications that use group-specific migration shares (not shown). Although the estimated coefficients are similar to the those from models of the overall impact of immigration, they are even less precise.

16 The first-difference results shown in the table are from unweighted regressions, though weighting according to the scheme used in Altonji and Card (1991) produces similar results.

with fixed effects will be biased down.[17] For this reason, I prefer the lagged-dependent-variable specifications discussed previously.

As noted in Section 1.2.1, the Northern black population is clustered into a relatively small number of metropolitan areas in my data. For example, because the sample of the 1950 Census, which only asked wage and education questions of a subset of households, is somewhat small, I only identify 22 metropolitan areas with at least ten native blacks. Although this clustering of the Northern black population is a matter of historical fact, it may call into doubt the causal interpretation of my estimates if they are driven by a few metropolitan areas acting as outliers. Figure 1.2 presents graphical evidence that this is not the case. The figure plots, separately by race, metro-average residual log annual wages against immigrant shares in 1950. I generate these figures by regressing both log annual wages and immigrant shares on the specification (4) covariates and averaging them at the metropolitan area level (in the figures labelled "IV," the x-axis contains averages of the projection of actual onto predicted immigration). While they are somewhat noisy, both the OLS and IV plots show that the negative relationship between native black wages and immigration is not an artifact of a small number of influential outliers. In contrast, among whites, both regression lines have small, positive slopes.

Another concern is that the racial difference in the estimated effects of immigration may be a consequence of the dispersion of whites across a larger number of metropolitan areas. The evidence presented previously shows that the negative effect among blacks is not driven by unobserved heterogeneity. However, since native whites live in a more diverse set of locations, if there is heterogeneity in the effect of immigration across different metros, it is possible that a negative effect for whites living in areas also inhabited by blacks is obscured by positive effects in predominantly white areas. To test for this, I replicate the log annual wage regressions, restricting the white samples to the same areas used to estimate the black regressions. Table 1.9 presents the results (in the interest of brevity, I include only the IV estimates). For native whites, the estimated coefficients are very similar to those obtained using the entire white sample, offering no indication that the apparent black-white difference owes to the greater dispersion of whites (the estimates for blacks are identical to those shown above). I also estimate models of native outmigration on the sample of whites living in the metropolitan areas from the black regression sample. These estimates, displayed in Table 1.10, show that native whites living in this geographic subsample are also more likely to leave their metropolitan areas in response to incoming Southern whites.

All of the models estimated so far treat the wages (and migration behavior) of natives as a function of Southern immigration. In principle, this is the correct sample with which to estimate the impacts of immigration on natives. Even if interest centers on the impact

17 This argument is formalized in Appendix B. Of course, it is also possible that there are permanent and mean-reverting components of unobserved heterogeneity. The estimation of such models generally requires many periods of data even when the other regressors are exogenous.

of immigration on the entire Northern labor force, there is a strong case to be made for excluding immigrants from the sample. Although immigrants compete with each other in the labor market—suggesting a potentially negative impact of immigration on the wages of immigrants—there is also reason to believe that the immigrant share of the labor force may affect the wages of immigrants themselves through additional channels. For example, existing immigrants may help match new ones with employers and housing or provide other forms of support. In fact, these network effects are one hypothesized reason for the enclaving behavior that motivates the use of historical settlement patterns to instrument for contemporaneous immigration (Carrington et al., 1996; White and Lindstrom, 2005). Additionally, to the degree that there are metro-level factors that may differentially impact the wages of migrants, selective immigration may introduce more bias into estimates of the effect of immigration on the wages of other migrants. With these caveats in mind, I also estimate models of the effects of immigration on wages (and outmigration) using samples that include Southern immigrants. The primary advantage of extending the sample in this way is that, since over half of the black labor force in the North is Southern-born during this period, doing so increases the number of metropolitan areas that can be used to estimate the effects of immigration on blacks.

The IV estimates for log annual wages are shown in Table 1.11. Including Southern workers in the sample increases the number of metropolitan areas to 47 (from 26) in 1940 and 33 (from 22) in 1950. The estimates for the overall share immigrant are shown in the left panel. For blacks, the point estimates are smaller in absolute value than those obtained using only native workers. Still, the coefficient is statistically significant for the 1940 estimate of specification (2), and for each 1950 specification. The inclusion of Southern workers causes little change in the estimated coefficients for whites. The coefficients on race-specific immigration shares are also similar to those estimated using the native sample. For blacks, the coefficients on the proportion Southern black are more negative than those on the overall proportion Southern, and, when covariates are included, these estimates are statistically significant. The estimates for whites are also similar to those obtained using only natives, with weak evidence of negative own-race and positive cross-race effects, though these coefficients are sensitive to different specifications of the model. Table 1.12 displays IV estimates of the impact of immigration on outmigration obtained using this extended sample. The coefficients on overall and group-specific immigrant shares are similar to those estimated using only natives, suggesting measurable outmigration among whites in response to recent Southern white immigration and no relationship between black outmigration and Southern immigration.

1.2.6 *Discussion*

My key findings—that (i) Southern immigration reduces wages for native blacks, but not whites, and (ii) immigration results in considerable outmigration among native whites, but not blacks—are thus quite resilient to alternative interpretations. The reciprocal relationship between these findings is probably not coincidental: the greater the native outmigration response to immigration, the less immigrant shares represent labor supply shocks, and the more attenuated estimates of the effect of immigration on wages become. Still, since they likely overstate the underlying outmigration response, these results do not imply that white immigrants displace white natives one-for-one. Immigrant shares do measure labor supply shifts, therefore, and cannot completely explain the lack of any apparent effect of immigration on the wages of native whites.

The evidence on native migration in response to foreign immigrants arriving in the US in the second half of the 20th century is ambiguous (contrast Card, 2001 with Borjas, 2006). In comparison, my estimates for whites are quite similar to those obtained by Boustan et al. (2010), despite the analysis of different forms of immigration and use of different identification strategies. Historical context may explain why a consensus emerges regarding earlier internal migration but not later foreign immigration: 1940 marks the end of the Great Depression and 1950 is the height of the white flight phenomenon (Frey, 1979); both may have been periods of unusually high mobility, at least among whites. That blacks reside in a relatively small number of metropolitan areas in the North is a good candidate explanation for the racial difference in my estimates of the native outmigration response to Southern immigration. It is well-known (see Massey and Denton, 1993) that blacks are often segregated into predominantly black neighborhoods. If the same social forces (e.g., discrimination and tastes for like-race neighbors) that drive this segregation also operated on larger geographic scales during the 40s and 50s, native blacks would have had limited scope to migrate away from Northern metropolitan areas receiving large inflows of Southern immigrants.

Insofar as they arise from labor supply effects, the differences between my estimates of the impact of Southern immigration on the wages of black and white natives imply that the degree of competition between blacks and whites in Northern labor markets is limited during the sample period. I find no evidence of an effect of Southern white immigration on native black wages and only tenuous evidence of a small positive effect of black immigration on white wages. While white native outmigration can partially explain the absence of an effect of Southern white immigration on black wages, it cannot fully reconcile the black-white difference: among neither group do I find an outmigration response to black immigration, implying that black immigrants arriving from the South shift local relative labor supply curves. If white natives substitute perfectly for black immigrants, we should expect to see an effect of black immigration on white wages; we do not.

The question of the substitutability of black for white labor thus becomes central. Effective imperfect substitution between racial groups may arise either because members of different groups possess different skills, or because of discrimination-based segregation into occupations, industries, firms, etc. A simple measure of black-white occupational segregation is the correlation between the black and white fractions of the labor force working in different Census occupations. I estimate this correlation to be 0.83 in 1940 and 0.75 in 1950. When I replace occupations with industries, the correlations are 0.87 and 0.77 for these years. Finally, when I classify workers into industry-occupation cells, the correlations are 0.65 and 0.41. These correlations, which do not account for segregation at the employer level, point to substantial imperfect substitution between blacks and whites.

In addition to explaining the absence of negative cross-group effects of immigration on wages, imperfect substitution between racial groups can account for part of the apparent black-white difference in the impact of own-race immigration. In 1940, for example, Southern blacks comprise less than 3% of the Northern labor force, compared to 5% for whites. In comparison, Southern immigrants comprise 63% of the black labor force. The greater the degree of isolation between the markets for black and white labor, therefore, the larger the relative shock to the black labor supply implied by the black immigrant share. If, to give an extreme example, the markets for black and white labor were completely independent, Southern immigration would have caused a modest proportional increase in the supply of white labor while more-than-doubling the supply of black labor. Imperfect substitution would therefore make wage effects among whites more difficult to detect in the data, especially combined with positive cross-group effects due to black immigration and outmigration among natives, both of which would potentially offset the variation in wages across labor markets.

Lastly, Southern immigration may have impacted wages through non-labor-supply channels. Following Becker (1971), one possibility is that Southern immigration, as a consequence of heterogeneous tastes for discrimination, increased the prejudice of marginal employers of blacks in the North, decreasing equilibrium black wages (but having no effect on whites' wages). I present some evidence in the next section that this is not the case. In addition, Southern black immigration surely contributed to the formation of Northern ghettos. The residential segregation literature indicates that the concentration of Northern blacks into crowded urban centers exacerbates poverty in these areas (Massey and Eggers, 1990; Massey and Denton, 1993; Cutler and Glaeser, 1997), though the causal mechanism underlying this effect is unclear (an interesting possibility is that immigration-induced labor-supply shocks depressed wages in these areas, catalyzing ghetto formation). Differentiating between labor-supply and other explanations of the impacts of Southern immigration is beyond my scope, although I note that if these impacts are not driven by supply shocks, they may be less informative about contemporary immigration.

1.3 THE GREAT MIGRATION AND THE AGGREGATE NORTHERN LABOR MARKET

In this section I use the structural, "national labor market" approach to estimating the effect of Southern immigration on wages. Closely following Borjas (2003) and Ottaviano and Peri (2006, 2008, 2012), I first specify the technology through which aggregate output is produced in the Northern US. I then use labor supply and wage data to estimate the parameters of this production function. Finally, I use these parameter estimates to simulate the wage impacts of observed and counterfactual flows of Southern immigrants.

This also the approach used by Boustan (2009), although our implementations differ in several ways, discussed in detail below. While Boustan studies the effects of Southern immigration between 1940 and 1970, I focus on the shorter period spanning 1940 to 1960 in order to emphasize earlier waves of the Great Migration, during which there was more Southern immigration, and make my structural estimates more comparable to the quasiexperimental results presented in Section 1.2.[18] To compare the sensitivity of our findings to implementation details, however, I also generate estimates using data from the entire 1940-1970 period, and I am able to approximately replicate Boustan's results.

A major advantage of the national labor market approach is that, since interregional immigration is less common than intraregional migration, the resulting estimates will be less susceptible to potential spatial arbitrage.[19] Moreover, the ability to simulate the effects of counter-historical changes in Southern immigration allows me to analyze the causes of racial differences in the impact of immigration on wages, though this analysis necessarily takes place through the lens of labor supply dynamics.

I assume that capital, K, and aggregate labor, L, are combined in the North in order to produce output according to

$$Y = AL^{\alpha}K^{1-\alpha}.$$

I further assume that L can be decomposed into subgroups that differ according to their education, experience, race, and regional origin using the following nested CES structure.[20] In the highest nest,

$$L = \left(\sum_e \theta_e L_e^{\frac{\sigma_e - 1}{\sigma_e}} \right)^{\frac{\sigma_e}{\sigma_e - 1}},$$

18 The Census did not collect wage information prior to 1940.

19 Only 1.6% of the Northern labor force in 1940 resided in the South as of 1935, while 7% exited their 1935 metropolitan areas during this period. Between 1949 and 1950, these flows were 1 and 4%, respectively.

20 This production function has become a workhorse in labor supply research. It was introduced by Card and Lemieux (2001), building on Katz and Murphy (1992).

where σ_e is the elasticity of substitution between labor aggregates, L_e, with different educational attainment. These aggregates are composed of workers with the same education but different amounts of labor market experience, so that

$$L_e = \left(\sum_x \theta_{ex} L_{ex}^{\frac{\sigma_x - 1}{\sigma_x}} \right)^{\frac{\sigma_x}{\sigma_x - 1}},$$

where σ_x is the elasticity of substitution between education-experience groups L_{ex}. These groups can be further disaggregated as

$$L_{ex} = \left(\theta_{exb} L_{exb}^{\frac{\sigma_r - 1}{\sigma_r}} + \theta_{exw} L_{exw}^{\frac{\sigma_r - 1}{\sigma_r}} \right)^{\frac{\sigma_r}{\sigma_r - 1}},$$

where σ_r is the elasticity of substitution between blacks and whites with the same education and experience. Finally, the education-experience-race labor aggregates consist of Southern- and Northern-born labor:

$$L_{exr} = \left(\theta_{exrn} L_{exrn}^{\frac{\sigma_i - 1}{\sigma_i}} + \theta_{exrs} L_{exrs}^{\frac{\sigma_i - 1}{\sigma_i}} \right)^{\frac{\sigma_i}{\sigma_i - 1}},$$

where σ_i is the elasticity of substitution between native and immigrant workers within education-experience-race cells.

Under competitive pricing and this production function, the wage of a worker belonging to group (e, x, r, i) satisfies

$$\begin{aligned} \log w_{exri} = \log \left[A\alpha \left(\frac{K}{L} \right)^{1-\alpha} \right] + \frac{1}{\sigma_e} \log L + \log \theta_e + \left(\frac{1}{\sigma_x} - \frac{1}{\sigma_e} \right) \log L_e \\ + \log \theta_{ex} + \left(\frac{1}{\sigma_r} - \frac{1}{\sigma_x} \right) \log L_{ex} + \log \theta_{exr} + \left(\frac{1}{\sigma_i} - \frac{1}{\sigma_r} \right) \log L_{exr} \\ + \log \theta_{exri} - \frac{1}{\sigma_i} \log L_{exri}. \end{aligned} \tag{1.1}$$

Average wages for higher labor aggregates can be obtained from this expression by eliminating terms containing efficiency weights and elasticities belonging to cells nested within them (e.g., eliminating the $\sigma_i^{-1} \log L_{exr}$, $\log \theta_{exri}$ and $\sigma_i^{-1} \log L_{exri}$ terms yields the log of the average wages paid to workers within education-experience-race cells).[21] The empirical implementation proceeds by estimating various levels of the wage equation in order to recover estimates of the elasticities.[22]

21 This can be seen by taking labor-supply-weighted averages of the first-order conditions with respect to each (e, x, r, i) cell.

22 In principle, all of the elasticity and efficiency-weight parameters may be time-varying. However, the assumption that the elasticities are constant over time is crucial to their identification.

1.3.1 *Data, sample restrictions, and variable construction*

In order to estimate the parameters of this production function, I add the IPUMS 1960 and 1970 Census samples to the data described in Section 1.2.1, imposing the same sample restrictions detailed previously (i.e., US-born black and white men, aged 16-64, earning nonzero wages in the year before enumeration). The nesting structure requires the assignment of observations to groups according to their education, experience, race and region of birth. While studies of contemporary foreign immigration typically use four educational groups (high school dropouts, high school graduates, those with some college, college graduates), this classification scheme is inappropriate for the time period that I study. As Table 1.2 shows, nontrivial fractions of the black labor force had completed fewer than five years of schooling in 1940 and 1950, and, even as late as 1960, the fractions with less than an eighth-grade education exceeded those with greater than twelve years of schooling for blacks and whites of either region of origin. Accordingly, I assign individuals to one of five educational categories: less than fifth grade, fifth-to-eighth grade, some high school (between 9 and 11 years of schooling), high school (12 years of schooling), and greater than high school (greater than 12 years).

In assigning experience, I assume that schooling begins at age six and all subsequent years are spent in the labor market. I also assume that the typical worker with less than a fifth-grade education spends 2.5 years in school, one with a fifth-to-eighth grade education spends 6.5 years in school, one with some high school spends 10, one with a high-school diploma (or equivalent) spends 12, and one with post-high-school education spends 13.5. I then compute experience as age, less six, less years spent in school. I drop groups with fewer than one or more than forty years of experience.[23] I then assign workers to one of eight five-year experience groups. Because of the age restrictions that I impose, the remaining sample contains no workers in the lowest education category with fewer than six years of imputed experience. I therefore have 39 education-experience categories for each year of data, and either 234 or 312 education-experience-race-year cells, depending on the sample period (these cells include native and immigrant laborers).

My labor supply measures are based on within-cell observations counts (i.e., I implicitly assume that each worker inelastically supplies one unit of labor). To allow for differences between workers along the intensive margin of labor supply, and to capture responses to immigration along this margin, my wage measures are based on annual earnings. At the lowest nest, my labor supply measure is the number of workers in an education-experience-race-birthplace cell, and my wage measure is the average annual wage in such

23 Boustan (2009) includes other nonwhite men, excludes those who are self employed, enrolled in school, or living in group quarters, and uses a slightly different education-experience imputation scheme. In order to make my structural estimates comparable to the local labor markets estimates above, I use the same sample restrictions for each approach.

a cell. In order to make these measures nationally representative, I weight each observation used in their construction by its IPUMS-supplied sampling weight.

To prelude my structural estimates of the impact of immigration on wages, I present in Figure 1.3 a simple descriptive summary of this impact. Panel (a) plots log average native wages and immigrant shares within education-experience groups from 1940 to 1960 for blacks. This plot demonstrates a clear negative relationship between immigrant-induced labor supply shocks and wages. Panel (b), which repeats this exercise for whites, suggests only a small positive correlation. Two features of this figure warrant further comment. First, if high wages attract workers from the South or the non-working North, an endogenous response that would attenuate the apparent impact of immigration on wages, these wage fluctuations must take place entirely within education-experience groups, since I use only this variation to generate the figures. Second, since the basic units of analysis are education-experience groups dispersed throughout the entire North, the only form of equilibrating outmigration (itself an endogenous labor supply response) that can contaminate these relationships is interregional migration, which is substantially less common than intraregional migration.

1.3.2 *Elasticity estimates*

Equation (1.1) implies that

$$\log\left(\frac{w_{exrnt}}{w_{exrst}}\right) = \log\left(\frac{\theta_{exrnt}}{\theta_{exrst}}\right) - \frac{1}{\sigma_i}\log\left(\frac{L_{exrnt}}{L_{exrst}}\right),$$

where w_{exrit} is the wage paid to labor belonging to group (e, x, r, i) and σ_i is the elasticity of substitution between Northern- and Southern-born labor. In order to recover this elasticity, I estimate several variations of the model

$$\log\left(\frac{\bar{w}_{exrnt}}{\bar{w}_{exrst}}\right) = \lambda_{ex} + \lambda_{et} + \lambda_{xt} - \frac{1}{\sigma_i}\log\left(\frac{\hat{L}_{exrnt}}{\hat{L}_{exrst}}\right) + u_{exrt}, \qquad (1.2)$$

where the $\bar{w}_{exrit}$ represent mean wages within (e, x, r, i) cells at time t, the $\hat{L}_{exrit}$ are estimates of group-specific labor supplies, λ_{ex} is an education-experience group fixed effect, λ_{et} is an education-decade fixed effect, λ_{xt} is an experience-decade fixed effect, and u_{exrt} allows for measurement error and relative wage determinants not incorporated into the model. The purpose of the fixed effects is to absorb the relative productivity term $\log(\theta_{exrnt}/\theta_{exrst})$. The identifying assumption embodied in this estimating equation is that, conditional on these effects, the remaining variation in relative wages is orthogonal to relative labor supplies.[24]

24 The failure of this condition is likely to result in estimates of σ_i^{-1} that are biased towards zero, understating the amount of imperfect substitution. Such a bias, as will be evident below, will lead me to understate the

Panel (a) of Table 1.13 displays the estimates of σ_i^{-1} for both the 1940-60 and 1940-70 sample periods. All regressions presented in this section are weighted by the total number of observations used to construct the dependent and independent variables; reported standard errors are clustered by education-experience group or, as appropriate, education-experience-race group, unless otherwise noted.[25] In order to allow for the possibility that native-immigrant substitutability varies by race, I first estimate (1.2) separately by race. The coefficients from the black regressions are positive and, although the white coefficients have the correct sign, none of the estimates are statistically significant. I also estimate a version of this equation that pools observations from black and white education-experience groups, constraining the elasticity to be the same for both races. The resulting coefficients are small and statistically insignificant. These estimates imply that, among blacks and whites alike, native Northern and Southern immigrant labor are perfectly substitutable.

Based on this evidence, I compute aggregate education-experience-race labor supplies using $\hat{L}_{exrt} = \hat{L}_{exrnt} + \hat{L}_{exrst}$, the sum of the Northern- and Southern-born supplies. The log wage equation (1.1) implies that

$$\log\left(\frac{\bar{w}_{exwt}}{\bar{w}_{exbt}}\right) = \log\left(\frac{\theta_{exwt}}{\theta_{exbt}}\right) - \frac{1}{\sigma_r}\log\left(\frac{L_{exwt}}{L_{exbt}}\right), \tag{1.3}$$

where σ_r is the elasticity of substitution between black and white labor and $\bar{w}_{exrt} = w_{exrnt} \cdot (L_{exrnt}/L_{exrt}) + w_{exrst} \cdot (L_{exrst}/L_{exrt})$ for $r \in \{b, w\}$ is the average wage paid to workers belonging to group (e, x, r) in decade t. I construct analogs to these averages by replacing the w_{exrit} $(i \in \{n, s\})$ with the group means used in the previous set of regressions. I then estimate a series of models of the form

$$\log\left(\frac{\bar{w}_{exwt}}{\bar{w}_{exbt}}\right) = \lambda_{ex} + \lambda_{et} + \lambda_{xt} - \frac{1}{\sigma_r}\log\left(\frac{\hat{L}_{exwt}}{\hat{L}_{exbt}}\right) + u_{ext}, \tag{1.4}$$

where, as before, the fixed effects are included in order to absorb the relative productivity term. I use two instrumental variables strategies to account for the potential endogeneity of the relative labor supply variable. The first follows Borjas (2003) and Ottaviano and Peri (2012) in using this ratio among immigrant workers to instrument for the ratio itself. The theory behind this instrument is that, conditional on the fixed effects, immigration acts as a pure supply shifter. Since immigration itself may respond to wage fluctuations, I also follow Boustan (2009) in using the same ratio among the national stock of Southerners. This instrument operates under the premise that Southerners' labor supply and

degree to which the impacts of immigration are concentrated on immigrants themselves (see Ottaviano and Peri, 2012, for a discussion of this issue). However, if interest centers on racial differences in the impacts of immigration, this bias will be inconsequential.

25 The estimates from unweighted regressions are similar.

educational decisions are less responsive to (within education-experience-race cell) wage shocks in the North.[26]

Although the nested-CES structure technically dictates that (1.4) should be estimated using the sample of all Northern labor (i.e., immigrants and natives), following Boustan (2009), I also estimate versions of (1.4) that use only natives' relative wages, as they are uncontaminated by composition effects from including migrants, as well as versions that use only immigrants' relative labor supplies, as these supplies may be less susceptible to endogenous education effects.[27] The elasticity estimates are presented in panel (b) of Table 1.13. The results from the 1940-1960 sample point to a considerable degree of imperfect substitution between blacks and whites. When wages and labor supply are measured using both natives and immigrants, the OLS estimate of σ_r^{-1} is about .2. Using immigration-induced changes in relative labor supplies to instrument for the overall changes produces an imprecise point estimate of about .1; using the ratio of black to white labor supply among all Southern-born workers as the instrument produces a statistically significant estimate of .2, close to the OLS estimate. When wages are calculated using all Northern labor but only immigrant workers are used to calculate relative labor supplies, the resulting OLS and IV estimates are closer to zero and statistically insignificant.[28] Calculating wages using only natives and labor supplies using all Northern labor, the OLS and IV estimates are similar to those obtained using the full sample, with estimates of the inverse elasticity close to .2. Finally, when this equation is estimated using only immigration-induced changes in relative labor supplies, the estimated elasticities are once again smaller and statistically insignificant. With the exception of models estimated using these changes, the inverse elasticity of substitution between blacks and whites is about .2 across samples and specifications.[29]

When 1970 is added to the sample, the estimated inverse elasticities are smaller. Measuring wages and labor supply using all labor, the OLS estimate is .13; using immigrant labor as an instrument, the estimate is .05 and insignificant; using the national stock of

26 Neither instrument is perfect. Local wage shocks may induce immigration and labor supply among natives (in which case the national stock instrument may be preferable), while national wage shocks may increase labor supply across all nativities (making the immigrant component instrument preferable). Attenuated estimates of σ_r^{-1} will generate wage impacts that understate the effect of immigration on blacks.

27 It also makes little sense to use immigrants' wages in IV regressions that use labor supply among immigrants as an instrument for labor supply itself. Note also that OLS regressions that only use immigrant labor supply are equivalently reduced-form regressions for models that use immigrant labor supply as an instrument.

28 Note that when labor supplies are calculated using immigrant labor alone, only the ratio among the national stock of Southerners can be used as an instrument for the labor supply ratio.

29 A potential explanation for why using immigrant-induced variation in labor supply results in estimated inverse elasticities that are attenuated towards zero is that there are immigrant-specific productivity shocks with which immigrant labor supply is correlated (note that this is not inconsistent with the finding that immigrants and natives are perfect substitutes). Since most of the sample consists of native whites, this correlation may be negligible when using the full sample to measure wages and labor supply but not when only immigrants are used to measure labor supply. This would also explain why, when native wages are related to immigrant labor supply, the resulting estimates are closer to the OLS estimate when the full sample is used to measure wages and labor supply.

Southerners as an instrument, the estimate is .16 and significant. As in the shorter sample, using only immigration-induced changes in relative labor supplies (regardless of how relative wages are measured) as a regressor produces smaller and less precise estimates. When native wages are regressed on all labor, the OLS and IV (using either instrument) estimates are between .13 and .16, similar to the OLS estimate obtained using natives and immigrants to measure wages and labor supplies. The estimates obtained using the 1940-1970 sample are much closer to those presented in Boustan (2009) than those obtained using the shorter time period.[30]

A key identifying assumption of the nested-CES approach is that the elasticities are time-invariant, which is clearly unrealistic. Ultimately, what these regressions identify is an average elasticity over the sample period. It accords with intuition that adding data from a decade during which much social progress took place increases the estimated degree of substitutability between black and white labor. However, it seems unlikely that racial progress alone can explain why the estimated inverse elasticity changes so much when 1970 is included in the estimation sample. The OLS estimate obtained using both natives and immigrants to measure wages and labor supply decreases from .22 to .13 when 1970 is added, a drop of over 40%.

Under the nested-CES technology, an equation like (1.4) also holds in race-specific levels, rather than black-white ratios, providing a specification test for whether estimates of (1.4) can be interpreted as racial elasticities of substitution. Estimating race-specific versions of this equation can also provide insight into whether discrimination, rather than pure technical substitution, can best-explain why blacks and whites appear to be imperfect substitutes. As discussed above, if firms exhibit heterogenous discrimination against blacks, the marginal employers of blacks belonging to skill groups with greater labor supply will be more prejudiced. If heterogeneous discrimination better explains the observed relationship between relative wages and labor supplies, we should expect estimates of σ_r^{-1} obtained using the black sample to be larger in absolute value than those from the white sample.[31]

Estimates of these group-specific equations are given in Table 1.14 (the instruments are now the group-specific labor supplies among Southern immigrants and all Southerners). In the 1940-1960 sample, the point estimates for whites are similar to those estimated in ratios. However, the coefficients on Black$\times \log L_{ext}$ are positive and, in some cases, as large as the coefficients for whites. This result immediately suggests that heterogeneous

30 They remain larger in absolute value, however. Although I can better-replicate the estimates in Boustan (2009) by using her sample restrictions, the resulting simulated effects of immigration are similar in either case. The most important sample restrictions appear to be the inclusion of all racial groups and foreign immigrants to the US.

31 To give a simple theoretical justification for this intuition, if employers exhibit a constant elasticity of discrimination, so that holding relative supplies constant, within-cell black wages are $w_{exbt} = L_{exbt}^{-\delta}$, $\delta > 0$, then the estimating equation becomes $\log w_{exbt} = \lambda_{ex} + \lambda_{et} + \lambda_{xt} - (\sigma_r^{-1} + \delta) \log L_{exbt}$, and the racial difference in the coefficient on the log labor supply represents a test of the hypothesis that $\delta = 0$.

discrimination among employers does not explain the apparent imperfect substitution between black and white labor.[32]

What the estimates do suggest is that labor supply is more responsive to within-cell wage shocks for blacks than whites, particularly during the period spanning 1940-1960. When the equation is estimated by OLS or using the national stock of Southerners as an instrument, the implied coefficient for blacks is zero. If black labor supply is more endogenous, imposing the constraint that the coefficients on black and white log labor supply are the same by estimating the equation in ratios rather than levels will result in attenuated estimates of the inverse elasticity. Indeed, comparing Tables 1.13 and 1.14 reveals that the point estimates for white log labor supply are slightly larger in absolute value than those for log relative labor supply. The race-specific regressions call into question the hypothesis that greater black-white substitution during the 60s can explain why using the longer panel shrinks the estimated inverse elasticities. Looking at the relationship between labor supply and wages among only whites, the estimated inverse elasticities are smaller in absolute value when 1970 is added to the panel, but the changes are less-pronounced (e.g. from -.25 to -.23 when the full sample is used to estimate the elasticity by OLS) and more consistent with the expected effect of racial progress during the 60s on the average elasticity over the full forty-year period. The implied coefficients on black labor supply are also similar across both panels (the OLS estimate using the full sample, e.g., is .08 regardless of whether 1970 is included); if anything, black labor supply appears less endogenous during the 1960s. Evidently, only when the elasticity is estimated using relative wages and labor supply (that is, the equation is estimated in ratios) does adding 1970 to the data have an appreciable effect on the estimates.

One reason why relating black-white differences in log wages to those in log labor supply might attenuate estimates of the inverse elasticity is if white labor supply is negatively correlated with productivity shocks for blacks, which may occur if firms with historically-high black labor supply adjust to use black labor more efficiently (or vice versa).[33] If such technological change occurs slowly, adding later data will introduce greater endogeneity into the relationship between relative wages and labor supplies. Though elucidating the precise reasons why the inverse elasticity is so much smaller in absolute value when it is estimated in ratios on the 1940-1970 panel exceeds my scope, the sensitivity of the estimates to the regression specification used to obtain them warrants interpreting them with

32 This is not to say that imperfect substitution does not arise from other forms of discrimination. For example, blacks may substitute imperfectly for whites—even conditional on education—because they receive schooling of a lower quality (see Card and Krueger, 1992) and therefore have different effective amounts of skill or because of discriminatory racial segregation into industries, occupations, firms, etc. Boustan (2009) provides additional evidence on this point.

33 That is, if the log wage equations are $\log w_{exrt} = \lambda_{ert} + \lambda_{xrt} + \lambda_{exr} - \sigma^{-1} \log L_{exrt} + u_{exrt}$ where u_{exrt} represents within education-experience-race productivity fluctuations, the error term for the equation in ratios will be of the form $u_{exwt} - u_{exbt}$, which will be positively correlated with $\log L_{exwt} / \log L_{exbt}$ if u_{exrt} is positively correlated with L_{exrt} and negatively correlated with $L_{exr't}$.

some care, and may suggest that the elasticity of substitution between blacks and whites remained small at least into the 1970s.[34]

Since the OLS and IV estimates of equation (1.4) are similar, I use the OLS estimates of σ_r^{-1} obtained using all wages and labor, and imposing the normalization that they sum to one, estimate the efficiency weights θ_{exwt} and θ_{exbt} using

$$\hat{\theta}_{wext} = \frac{\exp\left(\hat{\lambda}_{ex}\right)\exp\left(\hat{\lambda}_{et}\right)\exp\left(\hat{\lambda}_{xt}\right)}{1+\exp\left(\hat{\lambda}_{ex}\right)\exp\left(\hat{\lambda}_{et}\right)\exp\left(\hat{\lambda}_{xt}\right)}$$

and

$$\hat{\theta}_{bext} = \frac{1}{1+\exp\left(\hat{\lambda}_{ex}\right)\exp\left(\hat{\lambda}_{et}\right)\exp\left(\hat{\lambda}_{xt}\right)},$$

where the fixed effect terms are taken from the same regressions. I then construct the education-experience labor supply aggregates as

$$\hat{L}_{ext} = \left(\hat{\theta}_{exwt}\hat{L}_{exwt}^{\frac{\hat{\sigma}_r}{\hat{\sigma}_r-1}} + \hat{\theta}_{exbt}\hat{L}_{exbt}^{\frac{\hat{\sigma}_r}{\hat{\sigma}_r-1}}\right)^{\frac{\hat{\sigma}_r-1}{\hat{\sigma}_r}}. \tag{1.5}$$

The theoretical model implies that

$$\log \bar{w}_{ext} = \log\left[A_t\alpha\left(\frac{K_t}{L_t}\right)^{1-\alpha}\right] + \frac{1}{\sigma_e}\log L_t + \log\theta_{et} + \left(\frac{1}{\sigma_x}-\frac{1}{\sigma_e}\right)\log L_{et}$$
$$+\log\theta_{ex} - \frac{1}{\sigma_x}\log L_{ext},$$

where $\bar{w}_{ext} = \bar{w}_{exwt}\cdot(L_{exwt}/L_{ext}) + \bar{w}_{exbt}\cdot(L_{exbt}/L_{ext})$.[35] I construct the sample analogs of these mean wages and estimate σ_x^{-1} (the elasticity of substitution between experience groups with the same educational attainment) using

$$\log \bar{w}_{ext} = \lambda_{et} + \lambda_{ex} - \frac{1}{\sigma_x}\log\hat{L}_{ext} + u_{ext}. \tag{1.6}$$

The λ_{ex} effects are included to control for the $\log\theta_{ex}$ terms and the λ_{et} are included to capture the other components of the log wage equation. In the IV estimate of (1.6), I instrument for the supply of labor by education-experience aggregates using their immigrant analogs, again under the assumption that, conditional on the fixed effects, immigrant labor supply acts as an exogenous supply shifter. For the remaining elasticities, I only measure wages and labor supplies using all Northern labor. The estimates are presented

34 Another possible explanation is that using ratios amplifies classical measurement error and that labor supply is measured with greater error during the 1970s (although we might expect the IV estimates to be less attenuated in this case, which they are not).

35 Here, I follow Borjas (2003) and Ottaviano and Peri (2006, 2008, 2012) in assuming that the efficiency terms θ_{ex} are time-invariant. Ottaviano and Peri (2012) offer an alternative interpretation of this assumption: while these terms may vary over time, they have systematic components that can be recovered from regressions that include education-experience group fixed effects.

in panel (c) of Table 1.13. Using the shorter panel, the OLS estimate of 0.26 is close to the IV estimate of 0.2, with both statistically significant; the estimates from the longer panel are similar (both sets of estimates are close to those reported by Boustan, 2009).

Using the IV estimates of (1.6), I calculate

$$\hat{\theta}_{ex} = \frac{\exp(\hat{\lambda}_{ex})}{\sum_x \exp(\hat{\lambda}_{ex})},$$

again imposing the normalization that the (time-invariant) productivity weights θ_{ex} sum to one. I then use the estimated weights and elasticities to construct the education labor supply aggregates according to

$$\hat{L}_e = \left(\sum_x \hat{\theta}_{ex} \hat{L}_{ext}^{\frac{\hat{\sigma}_x - 1}{\hat{\sigma}_x}} \right)^{\frac{\hat{\sigma}_x}{\hat{\sigma}_x - 1}}. \tag{1.7}$$

Since the mean log wage within education groups is

$$\log \bar{w}_{et} = \log \left[\alpha A_t \left(\frac{K_t}{L_t} \right)^{1-\alpha} \right] + \frac{1}{\sigma_e} \log L_t + \log \theta_{et} - \frac{1}{\sigma_e} \log L_{et},$$

where $\bar{w}_{et} = \sum_x \bar{w}_{ext}(L_{ext}/L_{et})$, the elasticity of substitution, σ_e, between these groups can be estimated using the equation

$$\log \bar{w}_{et} = \lambda_t + \sum_e \kappa_e \cdot t - \frac{1}{\sigma_e} \log \hat{L}_{et} + u_{et}, \tag{1.8}$$

where the κ_e are education-group-specific linear time trends. The purpose of the λ_t is to absorb the $\log \left[\alpha A_t \left(K_t/L_t \right)^{1-\alpha} \right]$ and $\sigma_e^{-1} \log L_t$ terms, and the purpose of the time trends is to control for the $\log \theta_{et}$ term.

I estimate this equation by OLS and IV, again using immigrant labor to instrument for labor supply. The standard errors, reported in panel (c), are heteroskedasticity-robust. As before, the OLS and IV estimates obtained from the shorter panel are quite similar (0.24 and 0.28), and both are statistically significant; the estimates for the longer panel are similar.

As Ottaviano and Peri (2012) note, many of the elasticities estimated in this section have also been estimated by other scholars. Although I use earlier data than most previous research, a comparison may still be instructive. To my knowledge, Boustan (2009) is the only other study to examine substitution between Southern- and Northern-born US workers. Borjas et al. (2010), using decennial Census data from 1960-2000, estimate σ_r^{-1} to be small (their most negative estimate is a statistically insignificant -0.05), suggesting that blacks and whites are perfect substitutes. Given the time period differences between their study and the foregoing one, it is unsurprising that I find a greater degree of imperfect substitution between racial groups. The (inverse) elasticity of substitution between experience groups has been estimated by a number of studies. The estimates of σ_x^{-1} in Ottaviano and Peri (2012) lie in the range (0, -0.20); Boustan (2009) reports an elasticity of 0.19; Card and Lemieux (2001) find estimates in the (-0.10, -0.23) range; Welch (1979) estimates this range to be about (-0.08, -0.22). Even though I use earlier data, my estimates put this inverse elasticity in the (-0.17, -0.22) range, in agreement with previous work. I estimate σ_e^{-1}, the inverse elasticity of substitution between education groups, to lie in the interval (-0.25, -0.31). Ottaviano and Peri's estimates of this parameter are in the range of (-0.22, -0.43), Boustan's is 0.27, Borjas (2003) finds a range of (-0.74, -0.76), while Borjas and Katz (2007) estimate it to be -0.41. While there seems to be more variation in the literature for this parameter, my estimates are similar to those of Ottaviano and Peri; my use of earlier data and a different educational taxonomy may explain some of the differences between previous estimates and mine.

1.3.3 *The effects of Southern immigration on wages*

I use the elasticities estimated in the previous section to simulate the impact of Southern immigration on wages. Taking the total differential of group (e, x, r)'s log wage with respect to the log labor supply from all groups (e', x', r') gives

$$\frac{\Delta w_{exr}}{w_{exr}} = \frac{1}{\sigma_e}\sum_{e'}\sum_{x'}\sum_{r'} s_{e'x'r's}\frac{\Delta L_{e'x'r's}}{L_{e'x'r's}} + \left(\frac{1}{\sigma_x} - \frac{1}{\sigma_e}\right)\sum_{x'}\sum_{r'}\frac{s_{ex'r's}}{s_e}\frac{\Delta L_{ex'r's}}{L_{ex'r's}}$$
$$+ \left(\frac{1}{\sigma_r} - \frac{1}{\sigma_x}\right)\sum_{r'}\frac{s_{exr's}}{s_{ex}}\frac{\Delta L_{exr's}}{L_{exr's}} - \frac{1}{\sigma_r}\frac{s_{exrs}}{s_{exr}}\frac{\Delta L_{exrs}}{L_{exrs}}, \tag{1.9}$$

where, for instance, $s_{e'x'r's}$ is the share of the wage bill accruing to Southern-born labor with education e', experience x', and race r' (and $s_{e'x'r'}$ is the wage bill share accruing to all labor with these characteristics, and so on).[36] This expression shows that the general equilibrium impact of immigration on wages operates through its influence on each labor aggregate. Immigration into each aggregate increases the marginal product of all groups nested within that aggregate, exerting a positive effect on wages. At the same time, immigration also decreases the marginal product of the aggregate itself, depressing wages.

36 Ottaviano and Peri (2012, Appendix A1) provide a derivation. Here I sketch another for the simplified case where $L = \left(\sum_i \theta_i L_i^{\frac{\sigma-1}{\sigma}}\right)^{\frac{\sigma}{\sigma-1}}$. Normalizing the price of output, L, to one and differentiating the marginal product condition gives $\partial \log w_i / \partial \log L_j = (1/\sigma)(\partial \log L / \partial L_j)$. Since production is constant-returns-to-scale, the cost function will take the form $c(L, w) = \alpha L$ where α is a constant that depends the wage vector. Thus, treating L as a function of L_j and holding wages all other inputs constant, we have $d \log c[L(L_j)]/d \log L_j = d \log L(L_j)/d \log L$. Furthermore, $d \log c[L(L_j)]/d \log L_j = (dc[L(L_j)]/dL_j)(L_j/c[L(L_j)])$. Constant returns to scale and the marginal product condition imply that we also have $dc[L(L_j)]/dL_j = (dc(L)/dL)(dL(L_j)/dL_j) = \alpha w_j$. Hence, $d \log c[L(L_j)]/d \log L_j = \alpha w_j L_j / c[L(L_j)] = \alpha s_j$. Since, under perfect competition, profit maximization implies that price equals marginal cost, we must have that $\alpha = 1$, so $\partial \log w_i / \partial \log L_j = s_j/\sigma$.

Hence, immigration to the least-nested (most-nested) aggregate has an unambiguously positive (negative) effect on wages, while immigration to all other aggregates causes opposing effects.[37]

Equation (1.9) incorporates two important simplifications. First, since my estimates suggest perfect substitution between Northern- and Southern-born labor, immigration will have the same impact on the wages of otherwise-similar workers from either region, obviating the need to separately analyze the wages of natives and immigrants. Second, I assume that capital has fully responded to immigration-induced labor supply shocks, so that (1.9) represents the long-run impact of immigration on wages within education-experience-race groups. Since I study the consequences of immigration taking place over twenty and thirty year periods, this seems like the relevant impact. Another implication of this assumption is that immigration induces no long-run change in the average wage (weighting labor aggregates by their wage-bill shares).[38] When production is constant returns to scale, the long-run impacts of immigration on wages are necessarily distributional, with changes in relative wages depending on cell-specific immigration rates, wage-bill shares and elasticities. In reporting immigration-induced changes in wages, I adopt the wage-bill-share weighting scheme of Ottaviano and Peri (2012), calculating the percent change in the average wage to those of race r as

$$\frac{\Delta \bar{w}_r}{\bar{w}_r} = \frac{\sum_e \sum_x \frac{\Delta w_{exr}}{w_{exr}} s_{exr}}{\sum_e \sum_x s_{exr}},$$

and the percent change in the average wage among those of race r with education e as

$$\frac{\Delta \bar{w}_{er}}{\bar{w}_{er}} = \frac{\sum_x \frac{\Delta w_{exr}}{w_{exr}} s_{exr}}{\sum_x s_{exr}}.$$

Appendix Table 1.21 reports labor-cell-specific changes in Southern immigration between 1940 and 1960. Two trends from the table stand out. First, for both races, the changes are greater in higher educational groups; in fact, immigration among blacks with less than a fifth-grade education, or less-experienced workers with a fifth- to eighth-grade education, actually decreased over this period. This trend, which agrees with the changing educational distribution shown in Table 1.2, reflects increasing educational attainment over time, particularly among blacks. Second, with the exception of groups with very low educational attainment, the percent changes in the size of the immigrant labor force are greater for blacks than for whites belonging to almost every education-experience group.

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37 As long as the elements of successive nests are more easily substitutable (i.e., $\sigma_e^{-1} > \sigma_x^{-1} > \sigma_r^{-1}$), the composite impacts will be negative in these intermediate nests.

38 The short-run impact on group (e, x, r) includes an additional term, common to all labor cells, that reflects the immigration-induced change in the ratio of capital to aggregate labor. Since the average compensation to overall aggregate labor is $\log \bar{w} = \log \left[\alpha A (K/L)^{1-\alpha}\right]$, perfect capital adjustment implies no long-run average effect of immigration.

Of course, as equation (1.9) shows, immigration-induced proportional changes in wages depend on existing immigrant shares as well as immigrant flows, a point to which I return below.

In Table 1.15, I report estimates of the change in average wages due to the observed changes in immigration over the periods spanning 1940-60 and 1940-70. Instead of thirty-nine cell-specific changes, I report changes in average wages by race and race-education group. For these simulations, I use OLS estimates of σ_r obtained using all Northern workers to measure wages and labor supplies and IV estimates obtained using the same sample and labor supply among all Southerners as the instrument. From equation (1.9), the average change in wages depends on the initial wage-bill shares of immigrants in each group. Since education increased over time for blacks and whites, many of the immigrants between 1940 and 1970 belonged to education-experience groups that were small in 1940. These groups receive little weight when 1940 wage-bill shares are used to calculate the effects of immigration. For this reason, I also calculate average effects using 1950 wage-bill shares, which better-approximate the mean shares over time.

Between 1940 and 1960, immigration had no effect on the wages of whites, while blacks experienced substantial wage decreases. Using 1940 shares, I find that immigration decreased black wages by about 13-14%. When I use 1950 shares, which better approximate the average educational distribution over the period, this decrease was on the order of 20%. Less-educated whites experienced small wage increases which were offset by small losses among better-educated whites. Less-educated blacks also experienced small increases, but these changes were dominated by very large losses among blacks in higher education groups, among whom the changes in immigration were greater. Using the changes in immigration between 1940 and 1970, as well as the structural parameters estimated using this longer time period, results in a somewhat smaller estimate of the impact of immigration on wages. Weighting skill cells by their 1940 wage-bill shares, the average wage decline for blacks is between between 9 and 11% (close to the effects reported by Boustan); weighting with 1950 shares, the average effect is between 19 and 24%. The effects for whites are negligible for all implementations.

These simulated effects agree with the local labor markets results: the Great Migration caused substantial declines in the wages earned by Northern blacks and little change in those earned by whites. They also agree broadly with Boustan's (2009) findings, although my analysis does suggest that hers may understate the magnitude of the effect of Southern immigration on black wages. Because the elasticity of substitution between black and white labor increased over time (although part of this increase may be due to changes in the endogeneity of labor supply), simulating the effects of Southern immigration using the average elasticity over the entire period masks larger effects from the earlier Great Migration. Furthermore, using 1940 wage bill shares under-emphasizes skill cells that experienced large immigration-induced supply shocks. My estimates probably suffer from similar biases—the nested-CES approach uses a static model to approximate a dynamic process, making it sensitive to implementation details.[39]

To further examine the underlying causes of the racial difference in the effect of immigration on wages, I simulate the impacts of immigration under several counterfactual assumptions, focusing on the 1940-1960 period and using 1950 wage-bill shares. The results of these simulations are presented in Table 1.16. In the first three simulations, I attempt to effect a negative change in white average wages, maintaining imperfect substitution between blacks and whites. The first simulation assumes that, while white immigration changed as observed between 1940 and 1960, there was no black immigration during this period. Under these circumstances, complementarities would have increased the average black wage by 2%, leaving the average white wage unchanged. The second simulation assumes that proportional changes in immigration among whites were the same as those observed for blacks. In this case, the average wage among whites would have increased by about 1% (slightly more than under actual immigration) while that for blacks would have decreased by about 18% (slightly less than under actual immigration). For the third simulation, I assume again that white immigration changed as observed over the period, but that there was no black immigration. To account for the fact that initial immigrant shares are small for whites, I set white wage-bill shares equal to the observed black shares. Here, white wages decline by about 6% (and black wages increase by the same amount since both groups contribute equally to the wage bill). These simulations show the role that greater flows, and initial stocks, of black immigrants play in generating their large estimated wage declines. To underscore the importance of black-white substitution in generating these relative declines, my fourth simulation uses observed shares and immigration, but sets $\sigma_r^{-1} = 0$. Under this condition, the white average wage is essentially unaffected by immigration, and the black average wage increases by less than 1% (for both groups, there are small declines among those with some high school or a high school degree and similar increases among all other education groups). As discussed above, immigrants are a modest fraction of the overall and white labor forces, but a substantial fraction of the black labor force. Since imperfect substitution concentrates the negative impact of black immigration on blacks, it is the last ratio that matters; under perfect substitution, every education-experience group would face small immigration-induced supply shocks and commensurately small wage impacts.

39 Another approach would be to estimate the parameters for each pair of decades in order to obtain the effects over the full period. Even this technique would suffer from approximation bias, as the elasticities and wage-bill shares are likely not constant over decades. With more frequent data, however, this chained version of the nested-CES technique might be preferable.

1.4 CONCLUSION

Using either the local or national approach, I find that Southern immigration decreased the wages of natively-Northern blacks and had essentially no impact on those of native whites. The estimated magnitudes of this impact are also similar across methodologies. The richest specification that I estimate in Section 1.2 implies that a 10% increase in the immigrant share of the labor force decreases the annual wages of native blacks by about 23%; in 1950, immigrants comprised about 10% of the Northern labor force. After estimating the aggregate production function in the North, I simulate a 20% decrease in black wages.[40] The similarity between these results is also consistent with the estimated effects being driven primarily by labor supply dynamics, although in principle even the structural estimates could reflect non-labor-supply causes; elucidating the causal channels remains a topic for further work.

Equilibrating outmigration among native whites may be able to partially explain the racial disparity in the estimated local labor market effects of immigration. It cannot explain the racial disparity estimated using the national approach, since only interregional migration, which is less common, can bias this estimate. Because it concentrates the labor supply effects of black immigration on blacks, effective imperfect substitution between racial groups—which may arise because of skill differences, discrimination, or other reasons—offers a better explanation. Since immigrants constitute a significantly larger share of the black labor force than the white labor force, even a moderate degree of imperfect substitution translates to tighter labor markets for blacks than whites. The industry-occupation segregation measures in Section 1.2 and the elasticity estimates in Section 1.3 both evince imperfect substitution between black and white labor.

These findings have a number of implications. Most directly, they show that the Great Migration had a detrimental effect on the wages of blacks already in the North; deteriorating wages, coupled with residential segregation, may have even helped catalyze the urban poverty observed in the North during the 1970s (see Massey and Eggers, 1990). They also suggest that, though migrating North provided a means for Southern-born blacks to improve their economic status, market forces imposed limits on this opportunity. In their survey of the effects of civil rights legislation, Donohue, III and Heckman (1991) show that migration ceased to be an important factor in black relative wage gains after 1965. While relative wage gains in the South surely played a role, declining relative wages in the North may have contributed to the deceleration of the Great Migration as well.

My findings also provide empirical evidence on the relationship between immigration and inequality, particularly when immigrants belong disproportionately to groups that

40 Since the local labor markets approach estimates the impact of the stock of immigrants in 1950, while the national approach estimates the impact of immigrant flows between 1940 and 1960, these findings are not directly comparable. However, given the timing of the Great Migration, the 1950 stock of immigrants is also the flow of immigrants between 1915 and 1950, providing a justification for comparison.

face labor-market discrimination. Even when immigrants and natives are perfect substitutes, discrimination concentrates the negative impacts of discrimination on the disadvantaged group, exacerbating inequality.[41] That flows of Southern immigrants were so heavily black compared to the native labor force in the North makes the Great Migration an informative empirical counterfactual to contemporary flows of foreign immigrants to the US, which tend to have skill distributions that are similar to natives (Card, 2009). In addition, that the Great Migration represented such a large shock to the Northern black labor force provides me with unusually high statistical power to detect the effect of immigration on wages. While previous immigration research has been challenged by concerns that outmigration among natives may dissipate immigration-induced labor-supply shocks across labor markets, attenuating estimates of the impact of immigration on wages, I find no evidence of an outmigration response among blacks. That I find similar results using both the local and national approaches lends credibility to the idea that such spatial arbitrage can help explain why previous studies of immigration have reached conflicting results.

1.A APPENDIX TABLES

See Tables 1.17, 1.18, 1.19, 1.20, 1.21 and 1.14 attached.

1.B MISSPECIFIED LAGGED-DEPENDENT-VARIABLES AND FIXED-EFFECTS MODELS

Guryan (2001) shows that, when x is a serially-uncorrelated treatment indicator, estimates from lagged-dependent-variables and fixed-effects specifications bound the treatment effect when the data-generating process is unknown. I extend his argument to the case of serially-correlated treatment. I also compare the biases of OLS and lagged-dependent variables when the data-generating processes exhibits permanent unobserved heterogeneity. I focus on the simplified case where the treatment effect is zero in order to emphasize the bias due to misspecification. I assume that all variables are area-level means with individual covariates (and other area-level covariates) partialled out.

Suppose that x has no causal effect, so that the the value of y in area j evolves according to the data-generating process

$$y_{jt} = a_j + e_{jt},$$

with $E(e_{jt}|a_j) = 0$, implying that $y_{jt} = y_{jt-1} + e_{jt} - e_{jt-1}$.

41 Ottaviano and Peri (2012) make a related point about the impact of immigration on the wages of previous of immigrants when natives and immigrants are imperfect substitutes.

If the regression is misspecified as including the lagged dependent variable, i.e. $y_{jt} = \alpha y_{jt-1} + \beta x_{jt} + u_{jt}$, the probability limit of $\hat{\beta}$ is

$$\begin{aligned}
\beta^*_{LDV} = \frac{Cov(y_{jt}, \tilde{x}_{jt})}{Var(\tilde{x}_{jt})} &= \frac{Cov(y_{jt-1} + e_{jt} - e_{jt-1}, \tilde{x}_{jt})}{Var(\tilde{x}_{jt})} \\
&= \frac{Cov(e_{jt} - e_{jt-1}, x_{jt} - \gamma(a_j + e_{jt-1}))}{Var(\tilde{x}_{jt})} = \gamma \frac{\sigma_e^2}{Var(\tilde{x}_{jt})} \\
&= \frac{Cov(x_{jt}, a_j)}{Var(y_{jt-1})} \cdot \frac{\sigma_e^2}{Var(\tilde{x}_{jt})},
\end{aligned}$$

where $\tilde{x}_{jt} = x_{jt} - \gamma y_{jt-1}$ is the residual from a population regression of x_{jt} on y_{jt-1} and σ_e^2 is the variance of e_{jt}, which is assumed to be iid across j and t. If x is positively correlated with a, then $\beta^*_{LDV} > 0$.

Now suppose the data-generating process is

$$y_{jt} = \delta y_{jt-1} + e_{jt}$$

with $\delta < 1$, but the model is misspecified as $y_{jt} = \beta x_{jt} + a_j + u_{jt}$. In addition, suppose that over short periods of time the process by which x_{jt} evolves can be described by $x_{jt} = \lambda x_{jt-1} + r_{jt}$, with $\lambda > 1$ (to reflect the accumulation of immigrants over time). In this case, the probability limit of $\hat{\beta}$ is

$$\begin{aligned}
\beta^*_{FD} &= \frac{Cov(y_{jt} - y_{jt-1}, x_{jt} - x_{jt-t})}{Var(x_{jt} - x_{jt-t})} \\
&= \frac{Cov(y_{jt}, x_{jt}) - Cov(y_{jt}, x_{jt-1}) - Cov(y_{jt-1}, x_{jt}) + Cov(y_{jt-1}, x_{jt-1})}{Var(x_{jt} - x_{jt-1})} \\
&= \frac{(\delta\lambda)^{t-1}(\lambda\delta - \lambda - \delta + 1)\sigma_{xa}}{Var(x_{jt} - x_{jt-1})} = \frac{(\delta\lambda)^{t-1}[(\lambda - 1)(\delta - 1)]\sigma_{xa}}{Var(x_{jt} - x_{jt-1})} < 0,
\end{aligned}$$

where $\sigma_{xa} = Cov(x_0, y_{j0})$ is positive by assumption.

We thus have that $\beta^*_{FD} < \beta = 0 < \beta^*_{LDV}$.

A similar result allows us to compare the OLS and lagged-dependent-variables estimators when there is permanent heterogeneity. If the data-generating process is $y_{jt} = a_j + e_{jt}$ but the regression is specified as $y_{jt} = \beta x_{jt} + u_{jt}$, the population regression coefficient will be

$$\beta^*_{OLS} = \frac{Cov(y_{jt}, x_{jt})}{Var(x_{jt})} = \frac{Cov(a_j, x_{jt})}{Var(x_{jt})} > 0.$$

Thus, the lagged-dependent-variable estimate will be less biased than standard OLS if

$$\frac{Var(x_{jt})}{Var(\tilde{x}_{jt})} \cdot \frac{\sigma_e^2}{Var(y_{jt-1})} < 1.$$

Note that, since $\sigma_e^2 / Var(y_{jt-1}) \leq 1$ by definition, this condition is testable. In my dataset, $Var(x_{jt}) / Var(\tilde{x}_{jt}) \approx 1$.

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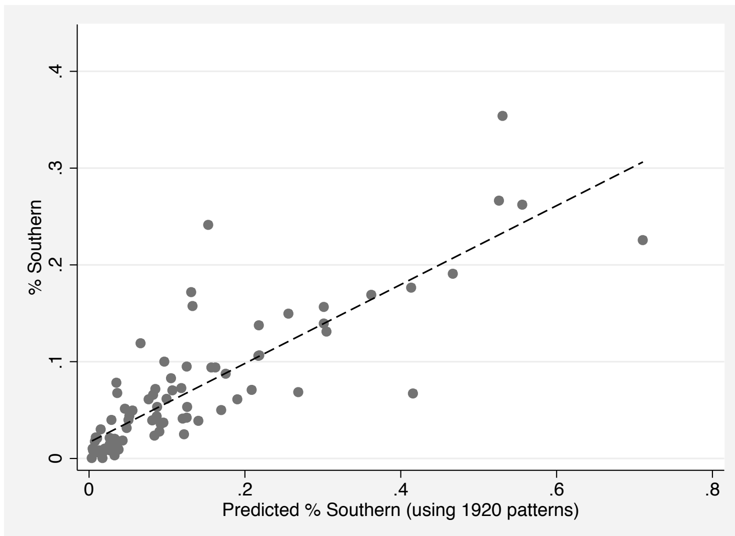
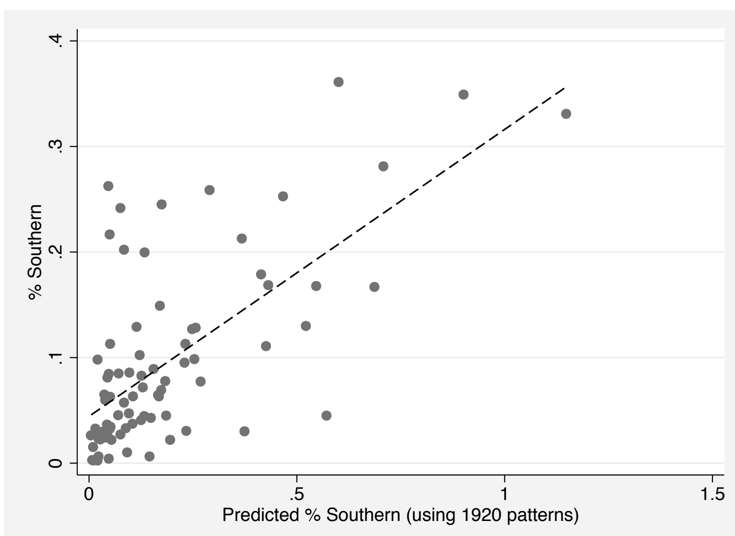
TABLES AND FIGURES

Figure 1.1: Actual and predicted immigration

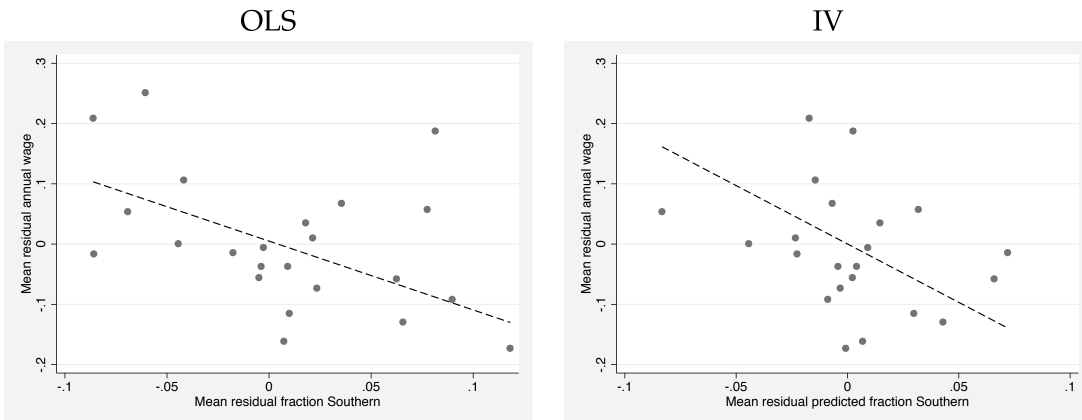


(a) 1940

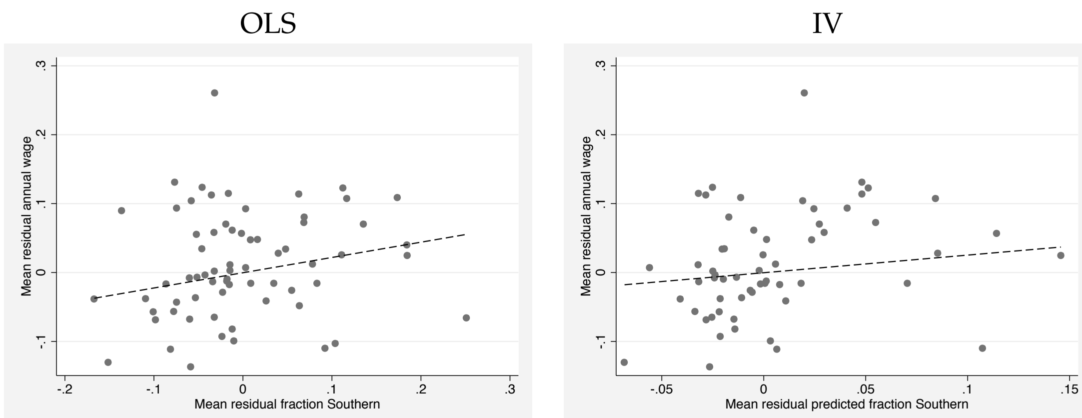


(b) 1950

Figure 1.2: Mean residual wages and immigration in 1950



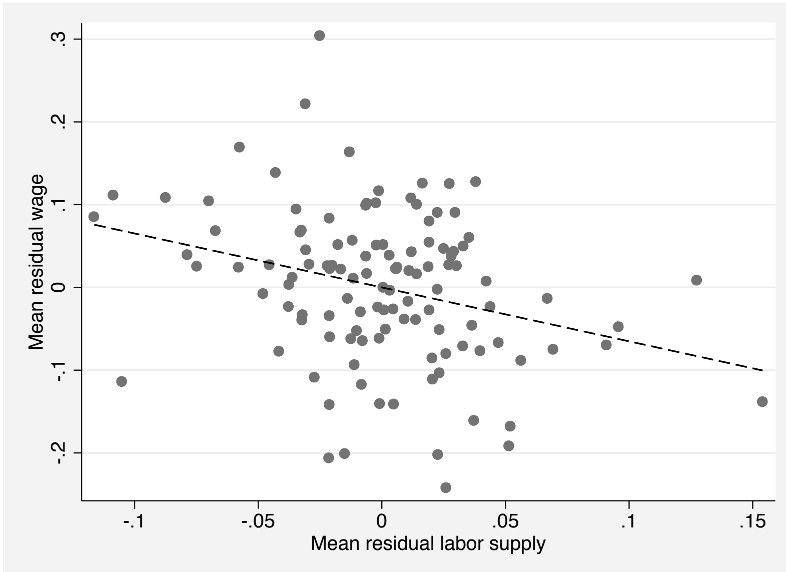
(a) Blacks



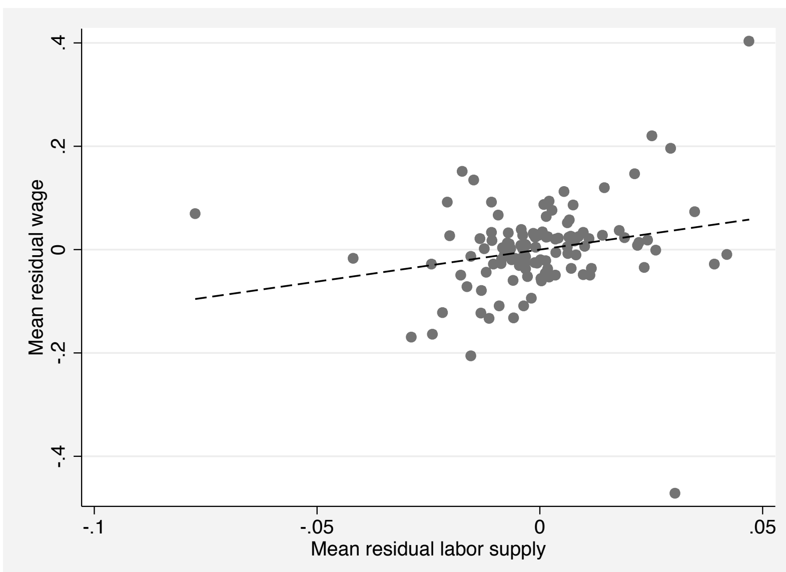
(b) Whites

Notes—All variables are expressed as metro-area mean residuals from regressions that include individual and metro-level covariates, as well as mean lagged log annual wages. The IV plots use the projection of actual immigration onto its prediction according to 1920 settlement patterns on the x-axis.

Figure 1.3: Wages and immigration within education-experience cells by race



(a) Blacks



(b) Whites

Notes—All variables are education-experience-group-level average residuals from a regression of the form $y_{ext} = \lambda_{ex} + \lambda_{et} + \lambda_{xt} + u_{ext}$; these figures relate variation in Southern labor supply within education-experience groups over time to that in log annual wages.

Table 1.1: Immigration by race and year

	1940	1950
	Prop. Southern	
Entire labor force	0.079	0.101
	(0.270)	(0.101)
Black labor force	0.632	0.661
	(0.482)	(0.473)
White labor force	0.054	0.068
	(0.227)	(0.253)
	Prop. black and Southern	
Entire labor force	0.027	0.036
	(0.162)	(0.187)
	Prop. white and Southern	
Entire labor force	0.052	0.065
	(0.222)	(0.246)
	Prop. recent Southern	
	1940 (5 years ago)	1950 (1 year ago)
Entire labor force	0.017	0.015
	(0.127)	(0.080)
Black labor force	0.052	0.006
	(0.223)	(0.080)
White labor force	0.015	0.008
	(0.121)	(0.088)

Notes—The labor force is defined as black and white men aged 16-64 who reported nonzero earnings in the year of enumeration. "Southern" means born in the South. Standard errors in parentheses.

Table 1.2: Education by race, year and immigrant status

		Black		White	
Year	Education group	Northern	Southern	Northern	Southern
1940	Less than 5th grade	0.10	0.16	0.02	0.05
	5th-8th grade	0.48	0.56	0.39	0.44
	Some high school	0.23	0.15	0.23	0.21
	High school degree	0.12	0.08	0.22	0.17
	Greater than high school	0.07	0.05	0.13	0.13
		1.00	1.00	1.00	1.00
1950	Less than 5th grade	0.05	0.12	0.02	0.04
	5th-8th grade	0.34	0.45	0.27	0.35
	Some high school	0.28	0.24	0.24	0.24
	High school degree	0.21	0.13	0.29	0.21
	Greater than high school	0.11	0.05	0.18	0.15
		1.00	1.00	1.00	1.00
1960	Less than 5th grade	0.03	0.07	0.01	0.04
	5th-8th grade	0.25	0.36	0.18	0.28
	Some high school	0.33	0.28	0.25	0.25
	High school degree	0.27	0.19	0.32	0.25
	Greater than high school	0.13	0.09	0.24	0.19
		1.00	1.00	1.00	1.00

Table 1.3: Annual wages and immigration

		OLS				IV			
		1940		1950		1940		1950	
Specification		Black	White	Black	White	Black	White	Black	White
(1)	Prop. Southern	0.152	0.0363	-0.661**	0.367	-0.110	-0.178	-1.116***	0.394
		(0.207)	(0.380)	(0.313)	(0.270)	(0.394)	(0.386)	(0.324)	(0.276)
	Observations	2,107	107,149	1,043	41,154	2,081	104,029	1,029	39,219
	Clusters	27	93	23	100	26	80	22	80
(2)	Prop. Southern	-0.243	0.329	-0.948**	0.475**	-0.774***	0.379	-1.369***	0.367
		(0.298)	(0.261)	(0.411)	(0.220)	(0.280)	(0.245)	(0.507)	(0.313)
	Observations	2,107	104,209	1,043	37,433	2,081	101,339	1,029	36,413
	Clusters	27	87	23	72	26	75	22	62
(3)	Prop. Southern			-1.185***	0.162			-1.827***	0.301
				(0.343)	(0.200)			(0.273)	(0.251)
	Observations			1,043	39,316			1,029	38,078
	Clusters			23	87			22	75
(4)	Prop. Southern			-1.245***	0.210**			-2.269***	0.104
				(0.291)	(0.0993)			(0.546)	(0.225)
	Observations			1,043	36,473			1,029	35,644
	Clusters			23	67			22	59

Notes—All specifications include indicators for age and education; specification (2) includes white and black metro-level percent employed in manufacturing, percent farming, and average years of education; specification (3) includes the metro level average dependent variable, lagged one decade; specification (4) includes metro-level average variables and a lagged mean dependent variable. Standard errors, clustered by metro area, reported in parentheses. "***", "**", and "*" denote significance at the 1, 5, and 10% levels, respectively.

Table 1.4: Annual wages and race-specific immigration

		OLS				IV			
		1940		1950		1940		1950	
Specification		Black	White	Black	White	Black	White	Black	White
(1)	Prop. Southern black	-0.486	1.943***	-0.702	1.750***	-1.504*	0.708	-2.917**	0.946*
		(0.769)	(0.493)	(1.161)	(0.279)	(0.837)	(0.883)	(1.245)	(0.519)
	Prop. Southern white	0.483	-0.579**	-0.638	-0.265*	0.730	-0.739**	-0.00155	-0.285*
		(0.416)	(0.242)	(0.401)	(0.136)	(0.585)	(0.301)	(0.606)	(0.171)
	Observations	2,107	107,149	1,043	41,154	2,081	104,029	1,029	39,219
	Clusters	27	93	23	100	26	80	22	80
(2)	Prop. Southern black	-1.905	2.151***	-1.920	1.764***	-4.806***	1.734***	-2.265*	0.914*
		(1.256)	(0.374)	(1.482)	(0.353)	(1.583)	(0.384)	(1.351)	(0.483)
	Prop. Southern white	0.385	-0.530**	-0.535	-0.229	1.049*	-0.557***	-0.646	-0.167
		(0.472)	(0.206)	(0.576)	(0.189)	(0.580)	(0.208)	(0.439)	(0.268)
	Observations	2,107	104,209	1,043	37,433	2,081	101,339	1,029	36,413
	Clusters	27	87	23	72	26	75	22	62
(3)	Prop. Southern black			-1.086	1.330***			-2.659***	0.787**
				(0.981)	(0.259)			(0.556)	(0.325)
	Prop. Southern white			-1.241***	-0.262***			-1.311**	-0.272*
				(0.381)	(0.0920)			(0.529)	(0.141)
	Observations			1,043	39,316			1,029	38,078
	Clusters			23	87			22	75
(4)	Prop. Southern black			-5.203***	0.845**			-5.437***	0.101
				(1.163)	(0.393)			(1.497)	(0.422)
	Prop. Southern white			-0.0541	-0.0360			-0.127	0.106
				(0.516)	(0.161)			(0.487)	(0.220)
	Observations			1,043	36,473			1,029	35,644
	Clusters			23	67			22	59

Notes—All specifications include indicators for age and education; specification (2) includes white and black metro-level percent employed in manufacturing, percent farming, and average years of education; specification (3) includes the metro level average dependent variable, lagged one decade; specification (4) includes metro-level average variables and a lagged mean dependent variable. Standard errors, clustered by metro area, reported in parentheses. "***", "**", and "*" denote significance at the 1, 5, and 10% levels, respectively.

Table 1.5: Weekly wages and immigration

		OLS				IV			
		1940		1950		1940		1950	
Specification		Black	White	Black	White	Black	White	Black	White
(1)	Prop. Southern	-0.301	0.164	-0.415	0.396**	-0.579	-0.259	-0.940**	0.120
		(0.197)	(0.307)	(0.294)	(0.153)	(0.362)	(0.333)	(0.466)	(0.241)
	Observations	2,106	107,104	1,029	40,751	2,080	103,986	1,015	38,839
	Clusters	27	93	23	100	26	80	22	80
(2)	Prop. Southern	-0.455	0.324	-0.582	0.379**	-0.832***	0.0717	-0.919	0.0892
		(0.323)	(0.256)	(0.422)	(0.181)	(0.282)	(0.286)	(0.592)	(0.234)
	Observations	2,106	104,167	1,029	37,065	2,080	101,299	1,015	36,055
	Clusters	27	87	23	72	26	75	22	62
(3)	Prop. Southern			-0.929***	0.164*			-1.221***	0.159
				(0.257)	(0.0860)			(0.258)	(0.185)
	Observations			1,029	38,933			1,015	26,290
	Clusters			23	87			22	22
(4)	Prop. Southern			-1.129***	0.145*			-1.256***	-0.0631
				(0.265)	(0.0767)			(0.425)	(0.177)
	Observations			1,029	36,114			1,015	26,290
	Clusters			23	67			22	22

Notes—All specifications include indicators for age and education; specification (2) includes white and black metro-level percent employed in manufacturing, percent farming, and average years of education; specification (3) includes the metro level average dependent variable, lagged one decade; specification (4) includes metro-level average variables and a lagged mean dependent variable. Standard errors, clustered by metro area, reported in parentheses. "***", "**", and "*" denote significance at the 1, 5, and 10% levels, respectively.

Table 1.6: Weekly wages and race-specific immigration

		OLS				IV			
		1940		1950		1940		1950	
Specification		Black	White	Black	White	Black	White	Black	White
(1)	Prop. Southern black	-1.066	1.328**	-1.294	1.091***	-1.501**	0.164	-3.607***	0.262
		(0.722)	(0.523)	(1.076)	(0.241)	(0.696)	(0.843)	(1.197)	(0.444)
	Prop. Southern white	0.0966	-0.212	0.0705	0.0776	-0.0224	-0.526**	0.718	-0.0536
		(0.392)	(0.222)	(0.332)	(0.131)	(0.470)	(0.257)	(0.564)	(0.141)
	Observations	2,106	107,104	1,029	40,751	2,080	103,986	1,015	38,839
	Clusters	27	93	23	100	26	80	22	80
(2)	Prop. Southern black	-0.938	1.198***	-2.206	1.007***	-2.887**	0.569	-2.626**	0.178
		(1.236)	(0.427)	(1.338)	(0.336)	(1.316)	(0.525)	(1.289)	(0.435)
	Prop. Southern white	-0.273	-0.0875	0.0998	0.0364	0.0964	-0.272	0.443	0.00247
		(0.617)	(0.247)	(0.394)	(0.173)	(0.638)	(0.290)	(0.412)	(0.233)
	Observations	2,106	104,167	1,029	37,065	2,080	101,299	1,015	36,055
	Clusters	27	87	23	72	26	75	22	62
(3)	Prop. Southern black			-1.279**	0.685***			-2.050***	0.374*
				(0.616)	(0.180)			(0.633)	(0.200)
	Prop. Southern white			-0.693	-0.0290			-0.583	-0.0911
				(0.451)	(0.0631)			(0.571)	(0.115)
	Observations			1,029	38,933			1,015	37,712
	Clusters			23	87			22	75
(4)	Prop. Southern black			-3.193***	0.466**			-2.518***	0.189
				(0.738)	(0.213)			(0.811)	(0.210)
	Prop. Southern white			-0.374	-0.0113			-0.128	0.00415
				(0.329)	(0.105)			(0.374)	(0.153)
	Observations			1,029	36,114			1,015	35,295
	Clusters			23	67			22	59

Notes—All specifications include indicators for age and education; specification (2) includes white and black metro-level percent employed in manufacturing, percent farming, and average years of education; specification (3) includes the metro level average dependent variable, lagged one decade; specification (4) includes metro-level average variables and a lagged mean dependent variable. Standard errors, clustered by metro area, reported in parentheses. "***", "**", and "*" denote significance at the 1, 5, and 10% levels, respectively.

Table 1.7: Native outmigration and recent immigration

		Overall							
		OLS				IV			
		1940		1950		1940		1950	
Specification		Black	White	Black	White	Black	White	Black	White
(1)	Prop. Southern	-0.428	0.343***	-0.784	0.423	-0.0468	1.101***	-0.219	0.513
		(0.351)	(0.102)	(1.149)	(0.315)	(0.267)	(0.346)	(0.277)	(0.757)
	Observations	2,018	97,043	954	38,073	1,996	94,370	940	36,456
	Clusters	27	92	21	97	26	79	20	79
(2)	Prop. Southern	-0.580***	-0.0283	0.411	-0.304	0.223	0.725***	0.295	1.356
		(0.187)	(0.0223)	(1.091)	(0.313)	(0.609)	(0.215)	(2.028)	(0.876)
	Observations	2,018	94,512	954	34,942	1,996	92,054	940	34,055
	Clusters	27	86	21	70	26	74	20	61

		By race							
		OLS				IV			
		1940		1950		1940		1950	
		Black	White	Black	White	Black	White	Black	White
(1)	Prop. Southern black	0.250	-1.165	-2.655**	-0.679	1.506	2.440	1.854	-2.992
		(1.119)	(0.956)	(1.248)	(0.958)	(3.607)	(2.420)	(5.019)	(3.348)
	Prop. Southern white	-0.472	0.728***	0.826	1.051***	-0.593	0.794***	-1.167	2.340***
		(0.404)	(0.206)	(0.592)	(0.369)	(1.011)	(0.295)	(1.993)	(0.853)
	Observations	2,018	97,043	954	38,073	1,996	94,370	940	36,456
	Clusters	27	92	21	97	26	79	20	79
(2)	Prop. Southern black	0.349	-0.589	-2.735	-0.290	7.386	0.558	2.627	-0.157
		(2.669)	(0.586)	(2.155)	(1.290)	(6.773)	(1.532)	(4.570)	(3.247)
	Prop. Southern white	-0.603*	0.492***	-0.319	0.527	-1.697	0.769**	-1.193	2.125***
		(0.307)	(0.152)	(1.287)	(0.438)	(1.128)	(0.361)	(1.562)	(0.723)
	Observations	2,018	94,512	954	34,942	1,996	92,054	940	34,055
	Clusters	27	86	21	70	26	74	20	61

Notes—All specifications include indicators for age and education; specification (2) includes white and black metro-level percent employed in manufacturing, percent farming, and average years of education. Standard errors, clustered by metro area, reported in parentheses. "***", "**", and "*" denote significance at the 1, 5, and 10% levels, respectively.

Table 1.8: Fixed-effects and first-differenced models

		Fixed effects							
		OLS				IV			
		Black		White		Black		White	
Annual wages	Prop. Southern	-0.276	0.212	1.354***	1.413***	-6.675*	-3.420	-1.373	-0.862
		(1.948)	(1.345)	(0.405)	(0.299)	(3.731)	(5.360)	(2.993)	(4.749)
	Observations	3,150	3,150	148,303	141,642	3,110	3,110	143,248	137,752
	Clusters	29	29	100	92	28	28	80	78
	Covariates	N	Y	N	Y	N	Y	N	Y
Weekly wages	Prop. Southern	-1.193	-0.488	0.847**	0.908***	-4.339	-6.411	-0.217	0.958
		(0.839)	(1.053)	(0.366)	(0.309)	(2.817)	(4.691)	(1.680)	(4.202)
	Observations	3,135	3,135	147,855	141,232	3,095	3,095	142,825	(0.839)
	Clusters	29	29	100	92	28	28	80	78
	Covariates	N	Y	N	Y	N	Y	N	Y
		First differences							
		OLS				IV			
		Black		White		Black		White	
Annual wages	Prop. Southern	-0.188	0.445	0.254	0.297	-9.796***	-7.385**	17.00	-6.143
		(2.154)	(2.150)	(0.253)	(0.372)	(2.584)	(2.905)	(135.7)	(8.574)
	Observations	21	21	93	67	20	20	59	59
	Covariates	Y	N	Y	N	Y	N	Y	N
Weekly wages	Prop. Southern	-0.144	0.154	0.0978	0.217	-5.490**	-5.104**	15.35	-6.362
		(1.261)	(1.294)	(0.190)	(0.275)	(2.215)	(2.262)	(122.0)	(8.788)
	Observations	21	21	93	67	20	20	59	59
	Covariates	Y	N	Y	N	Y	N	Y	N

Notes—Covariates include white and black metro-level percent employed in manufacturing, percent farming and average years of education and well as indicators for age and educational attainment. The dependent variable for the first-differenced models is the residual from a regression of wages on indicators for age and educational attainment. Standard errors for the fixed-effects estimates are clustered at the metro level, for the first-differenced estimates they are heteroskedasticity robust. "***", "**", and "*" denote significance at the 1, 5, and 10% levels, respectively.

Table 1.9: Annual wages and immigration, same metro areas

		Overall					By race			
		1940		1950			1940		1950	
Specification		Black	White	Black	White		Black	White	Black	White
(1)	Prop. Southern	-0.110	-0.422	-1.116***	0.239	Prop. Southern black	-1.504*	-0.132	-2.917**	0.687
		(0.394)	(0.424)	(0.324)	(0.302)		(0.837)	(1.040)	(1.245)	(0.608)
	Observations	2,081	73,638	1,029	26,562	Prop. Southern white	0.730	-0.598*	-0.00155	-0.295
	Clusters	26	26	22	22		(0.585)	(0.354)	(0.606)	(0.195)
						Observations	2,081	73,638	1,029	26,562
						Clusters	26	26	22	22
(2)	Prop. Southern	-0.774***	0.252	-1.369***	0.164	Prop. Southern black	-4.806***	0.923	-2.265*	0.522
		(0.280)	(0.240)	(0.507)	(0.385)		(1.583)	(0.582)	(1.351)	(0.616)
	Observations	2,081	73,638	1,029	26,562	Prop. Southern white	1.049*	-0.0756	-0.646	-0.0986
	Clusters	26	26	22	22		(0.580)	(0.187)	(0.439)	(0.265)
						Observations	2,081	73,638	1,029	26,562
						Clusters	26	26	22	22
(3)	Prop. Southern			-1.827***	0.316	Prop. Southern black			-2.659***	0.963***
				(0.273)	(0.318)				(0.556)	(0.324)
	Observations			1,029	26,562	Prop. Southern white			-1.311**	-0.357***
	Clusters			22	22				(0.529)	(0.131)
						Observations			1,029	26,562
						Clusters			22	22
(4)	Prop. Southern			-2.269***	-0.0549	Prop. Southern black			-5.437***	-0.398
				(0.546)	(0.241)				(1.497)	(0.678)
	Observations			1,029	26,562	Prop. Southern white			-0.127	0.171
	Clusters			22	22				(0.487)	(0.184)
						Observations			1,029	26,562
						Clusters			22	22

Notes—Instrumental variables estimates. All specifications include indicators for age and education; specification (2) includes white and black metro-level percent employed in manufacturing, percent farming, and average years of education; specification (3) includes the metro level average dependent variable, lagged one decade; specification (4) includes metro-level average variables and a lagged mean dependent variable. Standard errors, clustered by metro area, reported in parentheses. "***", "**", and "*" denote significance at the 1, 5, and 10% levels, respectively.

Table 1.10: Native outmigration and recent immigration, same metro areas

		Overall					By race			
		1940		1950			1940		1950	
Specification		Black	White	Black	White		Black	White	Black	White
(1)	Prop. Southern	-0.0468	1.214***	-0.219	0.572	Prop. Southern black	1.506	4.393	1.854	-2.366
		(0.267)	(0.394)	(0.277)	(0.804)		(3.607)	(3.363)	(5.019)	(3.320)
	Observations	1,996	68,564	940	24,704	Prop. Southern white	-0.593	0.493*	-1.167	2.173***
	Clusters	26	26	20	20		(1.011)	(0.299)	(1.993)	(0.824)
						Observations	1,996	68,564	940	24,704
						Clusters	26	26	20	20
(2)	Prop. Southern	0.223	0.768***	0.295	1.355	Prop. Southern black	7.386	6.803	2.627	-0.254
		(0.609)	(0.226)	(2.028)	(1.013)		(6.773)	(5.551)	(4.570)	(3.497)
	Observations	1,996	68,564	940	24,704	Prop. Southern white	-1.697	-0.597	-1.193	1.995***
	Clusters	26	26	20	20		(1.128)	(1.038)	(1.562)	(0.653)
						Observations	1,996	68,564	940	24,704
						Clusters	26	26	20	20

Notes—Instrumental variables estimates. All specifications include indicators for age and education; specification (2) includes white and black metro-level percent employed in manufacturing, percent farming, and average years of education. Standard errors, clustered by metro area, reported in parentheses. "***", "**", and "*" denote significance at the 1, 5, and 10% levels, respectively.

Table 1.11: Annual wages and immigration, including Southern workers

		Overall					By race			
		1940		1950			1940		1950	
Specification		Black	White	Black	White		Black	White	Black	White
(1)	Prop. Southern	-0.0649	-0.172	-0.567*	0.302	Prop. Southern black	-0.653	0.788	-1.046	0.846*
		(0.319)	(0.364)	(0.306)	(0.257)		(0.879)	(0.809)	(0.963)	(0.511)
	Observations	6,849	109,298	3,501	41,794	Prop. Southern white	0.274	-0.751***	-0.235	-0.314*
	Clusters	47	80	33	80		(0.490)	(0.286)	(0.447)	(0.162)
						Observations	6,849	109,298	3,501	41,794
						Clusters	47	80	33	80
(2)	Prop. Southern	-0.441*	0.361	-0.469*	0.293	Prop. Southern black	-3.777***	1.780***	-0.652	0.788
		(0.228)	(0.245)	(0.285)	(0.307)		(1.076)	(0.370)	(0.806)	(0.511)
	Observations	6,849	106,507	3,501	38,906	Prop. Southern white	0.895***	-0.612***	-0.326	-0.165
	Clusters	47	75	33	62		(0.338)	(0.195)	(0.352)	(0.272)
						Observations	6,849	106,507	3,501	38,906
						Clusters	47	75	33	62
(3)	Prop. Southern			-0.880*	0.183	Prop. Southern black			-0.891	0.649*
				(0.466)	(0.268)				(0.645)	(0.353)
	Observations			3,488	40,602	Prop. Southern white			-0.874**	-0.315**
	Clusters			32	75				(0.442)	(0.151)
						Observations			3,488	40,602
						Clusters			32	75
(4)	Prop. Southern			-0.620**	0.0207	Prop. Southern black			-1.731**	-0.0630
				(0.278)	(0.213)				(0.673)	(0.445)
	Observations			3,488	38,104	Prop. Southern white			0.0790	0.0982
	Clusters			32	59				(0.299)	(0.224)
						Observations			3,488	38,104
						Clusters			32	59

Notes—Instrumental variables estimates. All specifications include indicators for age and education; specification (2) includes white and black metro-level percent employed in manufacturing, percent farming, and average years of education; specification (3) includes the metro level average dependent variable, lagged one decade; specification (4) includes metro-level average variables and a lagged mean dependent variable. Standard errors, clustered by metro area, reported in parentheses. "***", "**", and "*" denote significance at the 1, 5, and 10% levels, respectively.

Table 1.12: Native outmigration and immigration, including Southern workers

		Overall					By race			
		1940		1950			1940		1950	
Specification		Black	White	Black	White		Black	White	Black	White
(1)	Prop. Southern	0.109	1.027***	-0.210	0.719	Prop. Southern black	0.205	2.146	-2.917	-1.985
		(0.198)	(0.341)	(0.383)	(0.860)		(2.083)	(2.236)	(4.304)	(3.736)
	Observations	6,335	98,378	3,286	38,680	Prop. Southern white	0.0777	0.763***	1.051	2.097**
	Clusters	46	79	31	79		(0.519)	(0.269)	(1.566)	(0.880)
						Observations	6,335	98,378	3,286	38,680
						Clusters	46	79	31	79
(2)	Prop. Southern	-0.0623	0.680***	-0.676	1.807	Prop. Southern black	-0.939	0.418	-0.949	1.258
		(0.317)	(0.226)	(1.444)	(1.247)		(6.423)	(1.518)	(3.239)	(4.524)
	Observations	6,335	95,987	3,286	36,217	Prop. Southern white	0.160	0.750**	-0.461	2.088**
	Clusters	46	74	31	61		(1.341)	(0.355)	(1.088)	(0.919)
						Observations	6,335	95,987	3,286	36,217
						Clusters	46	74	31	61

Notes—Instrumental variables estimates. All specifications include indicators for age and education; specification (2) includes white and black metro-level percent employed in manufacturing, percent farming, and average years of education. Standard errors, clustered by metro area, reported in parentheses. “***”, “**”, and “*” denote significance at the 1, 5, and 10% levels, respectively.

Table 1.13: Elasticity estimates

	1940-1960	1940-1970
White	-0.05	-0.05
	(0.06)	(0.04)
N	117	156
Black	0.05	0.02
	(0.08)	(0.05)
N	117	156
Constrained	-0.04	-0.04
	(0.06)	(0.04)
N	234	311

(a) Immigrant-native

		1940-1960			1940-1970		
Wage sample	Labor supply sample	OLS	IV (Immig. component)	IV (National stock)	OLS	IV (Immig. component)	IV (National stock)
All	All	-0.22	-0.10	-0.20	-0.13	-0.05	-0.16
		(0.09)	(0.11)	(0.10)	(0.06)	(0.08)	(0.06)
		117	117	117	155	155	155
All	Immigrant	-0.04		-0.13	-0.03		-0.14
		(0.08)		(0.09)	(0.06)		(0.07)
		117		117	155		155
Native	All	-0.18	-0.19	-0.16	-0.14	-0.12	-0.16
		(0.09)	(0.11)	(0.13)	(0.05)	(0.07)	(0.07)
		117	117	117	156	155	156
Native	Immigrant	-0.08		-0.11	-0.06		-0.14
		(0.08)		(0.10)	(0.06)		(0.07)
		117		117	155		155

(b) Black-white

	1940-1960		1940-1970	
	OLS	IV (Immig. component)	OLS	IV (Immig. component)
Experience	-0.26	-0.20	-0.23	-0.18
	(0.03)	(0.03)	(0.04)	(0.03)
	117	117	155	155
Education	-0.24	-0.28	-0.21	-0.25
	(0.14)	(0.06)	(0.10)	(0.05)
	15	15	20	20

(c) Education and experience

Notes—"Immig. component" refers to the immigrant component of labor supply and "National stock" refers to the labor supply among all Southern-born workers (including those living in the North). Except where noted, wages and labor supplies are measured using all Northern labor. Standard errors for the estimates of σ_i, σ_r, and σ_x are clustered by education-experience group (or race-education-experience groups for models that pool both races); standard errors for σ_e are heteroskedasticity-robust.. All regressions are weighted by the number of observations used to construct the dependent variable.

Table 1.14: Race-specific estimates of σ_r

			1940-1960			1940-1970		
Wage sample	Labor supply sample		OLS	IV (Immig. component)	IV (National stock)	OLS	IV (Immig. component)	IV (National stock)
All	All	log(L)	-0.25	-0.17	-0.26	-0.23	-0.17	-0.22
			(0.06)	(0.05)	(0.05)	(0.04)	(0.03)	(0.04)
		Black*log(L)	0.17	0.07	0.25	0.15	0.09	0.18
			(0.08)	(0.06)	(0.08)	(0.05)	(0.04)	(0.05)
		N	234	234	234	311	311	311
All	Immigrant	log(L)	-0.25		-0.26	-0.23		-0.22
			(0.06)		(0.05)	(0.04)		(0.04)
		Black*log(L)	0.11		0.20	0.12		0.14
			(0.07)		(0.07)	(0.04)		(0.04)
		N	234		234	311		311
Native	All	log(L)	-0.26	-0.18	-0.26	-0.23	-0.18	-0.22
			(0.06)	(0.05)	(0.05)	(0.04)	(0.03)	(0.04)
		Black*log(L)	0.19	0.06	0.27	0.12	0.06	0.14
			(0.09)	(0.08)	(0.12)	(0.06)	(0.05)	(0.06)
		N	234	234	234	312	311	312
Native	Immigrant	log(L)	-0.25		-0.26	-0.23		-0.22
			(0.06)		(0.05)	(0.04)		(0.04)
		Black*log(L)	0.09		0.21	0.09		0.11
			(0.09)		(0.10)	(0.05)		(0.05)
		N	234		234	311		311

Notes—The estimating equation is $\log w_{exrt} = \lambda_{exr} + \lambda_{ert} + \lambda_{ext} - \sigma_w^{-1} \log w_{exrt} - (\sigma_b^{-1} - \sigma_w^{-1})1_{r=b} \log w_{exrt} + u_{exrt}$. Immig. component refers to the immigrant component of labor supply and National stock refers to the labor supply among all Southern-born workers (including those living in the North). Standard errors for the estimates of σ_i, σ_r, and σ_x are clustered by education-experience group (or race-education-experience groups for models that pool both races). All regressions are weighted by the number of observations used to construct the dependent variable.

Table 1.15: The effects of South-North migration on wages

		1940-1960				1940-1970			
		1940 shares		1950 shares		1940 shares		1950 shares	
		OLS	IV	OLS	IV	OLS	IV	OLS	IV
White	Overall	0.00	0.00	0.01	0.01	0.00	0.00	0.01	0.01
Black	Overall	-0.14	-0.13	-0.22	-0.20	-0.09	-0.11	-0.19	-0.24
White	Less than 5th grade	0.00	0.00	0.02	0.02	0.03	0.03	0.05	0.05
	5th-8th grade	0.02	0.02	0.03	0.03	0.03	0.03	0.05	0.05
	Some high school	0.00	0.00	0.00	0.00	0.00	0.01	0.01	0.01
	High school degree	0.00	0.00	0.00	0.00	-0.01	-0.01	-0.01	-0.01
	Greater than high school	-0.01	-0.01	0.01	0.01	-0.03	-0.03	-0.01	-0.01
Black	Less than 5th grade	0.03	0.03	0.04	0.04	0.09	0.10	0.12	0.13
	5th-8th grade	-0.03	-0.03	-0.02	-0.02	0.05	0.05	0.06	0.06
	Some high school	-0.33	-0.30	-0.40	-0.36	-0.22	-0.27	-0.30	-0.37
	High school degree	-0.43	-0.39	-0.53	-0.49	-0.46	-0.57	-0.67	-0.82
	Greater than high school	-0.31	-0.28	-0.28	-0.25	-0.39	-0.48	-0.36	-0.44

Notes—Columns labeled "OLS" use the OLS estimate of the elasticity of substitution between blacks and whites; columns labeled "IV" use estimates of this elasticity obtained by instrumenting for relative labor supplies with relative supplies among the national stock of Southerners. All other elasticities are taken from IV estimates based on the labor supplied within skill groups by immigrants.

Table 1.16: Simulations

	Simulation 1		Simulation 2		Simulation 3		Simulation 4	
	White	Black	White	Black	White	Black	White	Black
Overall	0.00	0.02	0.01	-0.18	-0.06	0.06	0.00	0.01
Less than 5th grade	0.00	0.02	0.07	0.07	-0.05	0.07	0.02	0.02
5th-8th grade	0.02	0.02	0.05	0.01	0.03	0.08	0.03	0.03
Some high school	-0.01	0.02	-0.01	-0.35	-0.12	0.05	-0.02	-0.02
High school degree	-0.01	0.02	-0.02	-0.47	-0.18	0.05	-0.01	-0.01
Greater than high school	-0.01	0.02	0.01	-0.23	-0.11	0.06	0.00	0.00

Notes—All simulations use 1950 shares and the same IV estimates of the elasticities as Table 1.15. Simulation 1 assumes that there is no black immigration. Simulation 2 assumes that percent changes in black and white immigration are equal ($\Delta L_{exws}/L_{exws} = \Delta L_{exbs}/L_{exbs}$). Simulation 3 assumes that whites and blacks have equal wage-bill shares ($s_{exwi} = s_{exbi}$ for all e, x, and i) and that there is no black immigration. Simulation 4 uses actual shares and percent changes, but assumes perfect substitution between black and white labor.

Table 1.17: Metro areas used in black regressions

	1940					1950				
	Prop. Southern			# of natives		Prop. Southern			# of natives	
Metro	All	Black	White	Black	White	All	Black	White	Black	White
Atlantic City, NJ	0.094	0.085	0.009	23	181	0.030	0.022	0.008	13	54
Baltimore, MD	0.149	0.083	0.066	237	2062	0.213	0.116	0.097	85	735
Boston, MA-NH	0.015	0.007	0.007	41	4582	0.032	0.012	0.020	18	1725
Charleston, WV	0.161	0.057	0.104	26	543	0.113	0.059	0.054	14	204
Chicago, IL	0.095	0.062	0.032	155	9003	0.149	0.102	0.047	105	3308
Cincinnati-Hamilton, OH/KY/IN	0.266	0.082	0.184	30	996	0.281	0.085	0.196	12	390
Cleveland, OH	0.094	0.065	0.029	37	2368	0.113	0.085	0.028	29	877
Columbus, OH	0.106	0.061	0.045	38	799	0.098	0.043	0.055	19	349
Dayton-Springfield, OH	0.137	0.056	0.082	37	872	0.259	0.106	0.153	11	323
Des Moines, IA	0.027	0.005	0.023	11	418					
Detroit, MI	0.157	0.073	0.085	62	4226	0.245	0.135	0.110	51	1616
Indianapolis, IN	0.176	0.077	0.099	39	911	0.167	0.063	0.104	18	311
Kansas City, MO-KS	0.139	0.057	0.083	96	1311	0.179	0.081	0.098	21	492
Los Angeles-Long Beach, CA	0.171	0.024	0.147	44	4737	0.200	0.049	0.151	29	2246
Minneapolis-St. Paul, MN	0.020	0.007	0.013	15	1898					
New Haven-Meriden, CT	0.018	0.011	0.007	15	977					
New York-Northeastern NJ	0.066	0.045	0.020	427	19203	0.083	0.058	0.025	235	6835
Omaha, NE/IA	0.061	0.034	0.027	11	545	0.077	0.027	0.050	10	184
Philadelphia, PA/NJ	0.087	0.064	0.024	263	5729	0.128	0.094	0.033	136	2067
Pittsburgh, PA	0.061	0.044	0.017	79	4007	0.064	0.050	0.015	39	1436
St. Louis, MO-IL	0.131	0.070	0.061	97	2805	0.168	0.096	0.072	45	978
San Francisco-Oakland-Vallejo, CA	0.100	0.011	0.089	18	2719	0.202	0.059	0.143	13	1204
Seattle-Everett, WA						0.129	0.030	0.099	10	458
Topeka, KS	0.068	0.029	0.039	11	182					
Trenton, NJ						0.084	0.073	0.011	12	147
Washington, DC/MD/VA	0.354	0.151	0.202	232	1128	0.361	0.148	0.213	106	459
Wheeling, WV/OH	0.049	0.028	0.022	14	692					
Wilmington, DE/NJ/MD	0.050	0.017	0.032	37	353					
Youngstown-Warren, OH-PA	0.070	0.045	0.025	12	934	0.063	0.052	0.011	12	368

Table 1.18: First-stage regressions

		Individual covariates				Individual and metro-level covariates			
		1940		1950		1940		1950	
Dependent variable	Instrument(s)	Black	White	Black	White	Black	White	Black	White
Prop. Southern	Predicted Prop. Southern	0.544***	0.514***	0.453***	0.372***	0.559***	0.532***	0.289***	0.358***
		(0.0724)	(0.0739)	(0.0778)	(0.0852)	(0.0647)	(0.0618)	(0.0803)	(0.0697)
	Observations	2,081	104,029	1,029	39,219	2,081	101,339	1,029	36,413
	R-squared	0.852	0.606	0.628	0.272	0.932	0.772	0.836	0.570
	Clusters	26	80	22	80	26	75	22	62
Prop. Southern black	Predicted Prop. Southern black	0.322***	0.471***	0.300***	0.376***	0.313***	0.469***	0.233***	0.329***
		(0.0829)	(0.0918)	(0.0520)	(0.0733)	(0.0985)	(0.0653)	(0.0697)	(0.0546)
	Predicted Prop. Southern white	0.115	-0.0522	0.0676	0.0165	0.0863	-0.00589	0.0345	0.0206
		(0.0773)	(0.0518)	(0.0411)	(0.0569)	(0.0538)	(0.0563)	(0.0401)	(0.0523)
	Observations	2,081	104,029	1,029	39,219	2,081	101,339	1,029	36,413
	R-squared	0.834	0.732	0.555	0.452	0.933	0.833	0.877	0.677
	Clusters	26	80	22	80	26	75	22	62
Prop. Southern white	Predicted Prop. Southern black	-0.197	-0.289	-0.257	-0.413**	-0.616***	-0.209	-0.416***	-0.279**
		(0.139)	(0.175)	(0.158)	(0.173)	(0.182)	(0.126)	(0.142)	(0.105)
	Predicted Prop. Southern white	0.748***	0.874***	0.725***	0.807***	1.018***	0.779***	0.685***	0.653***
		(0.110)	(0.187)	(0.109)	(0.159)	(0.131)	(0.127)	(0.102)	(0.115)
	Observations	2,081	104,029	1,029	39,219	2,081	101,339	1,029	36,413
	R-squared	0.879	0.717	0.779	0.594	0.957	0.799	0.893	0.720
	Clusters	26	80	22	80	26	75	22	62

Notes—All specifications include indicators for age and education; metro-level covariates include white and black percent employed in manufacturing, percent farming, and average years of education. Standard errors, clustered by metro area, reported in parentheses. "***", "**", and "*" denote significance at the 1, 5, and 10% levels, respectively.

Table 1.19: Wages and immigration: State-specific IV estimates

		Annual wages				Weekly wages			
		1940		1950		1940		1950	
Specification		Black	White	Black	White	Black	White	Black	White
(1)	Prop. Southern	-0.308	-0.448	-1.134***	0.269	-0.722*	-0.384	-0.874**	0.187
		(0.494)	(0.394)	(0.307)	(0.288)	(0.428)	(0.320)	(0.421)	(0.216)
	Observations	2,081	104,029	1,029	39,219	2,080	103,986	1,015	38,839
	Clusters	26	80	22	80	26	80	22	80
(2)	Prop. Southern	-0.804***	0.121	-1.348***	0.325	-0.959***	-0.0790	-0.478	0.155
		(0.307)	(0.259)	(0.365)	(0.308)	(0.342)	(0.293)	(0.490)	(0.225)
	Observations	2,081	101,339	1,029	36,413	2,080	101,299	1,015	36,055
	Clusters	26	75	22	62	26	75	22	62
(3)	Prop. Southern			-1.570***	0.260			-0.859**	0.225**
				(0.233)	(0.198)			(0.402)	(0.114)
	Observations			1,029	38,078			1,015	37,712
	Clusters			22	75			22	75
(4)	Prop. Southern			-1.650***	0.244			-0.866**	0.157
				(0.331)	(0.198)			(0.419)	(0.137)
	Observations			1,029	35,644			1,015	35,295
	Clusters			22	59			22	59

Notes—All specifications include indicators for age and education; specification (2) includes white and black metro-level percent employed in manufacturing, percent farming, and average years of education; specification (3) includes the metro level average dependent variable, lagged one decade; specification (4) includes metro-level average variables and a lagged mean dependent variable. Standard errors, clustered by metro area, reported in parentheses. "***", "**", and "*" denote significance at the 1, 5, and 10% levels, respectively.

Table 1.20: Wages and race-specific immigration: State-specific IV estimates

		Annual wages				Weekly wages			
		1940		1950		1940		1950	
Specification		Black	White	Black	White	Black	White	Black	White
(1)	Prop. Southern black	-2.212*	0.584	-2.443**	1.155**	-1.809**	0.147	-2.911**	0.505
		(1.160)	(0.834)	(1.112)	(0.503)	(0.861)	(0.814)	(1.187)	(0.455)
	Prop. Southern white	0.694	-0.841***	-0.347	-0.303**	-0.150	-0.586**	0.352	-0.0182
		(0.765)	(0.291)	(0.545)	(0.149)	(0.559)	(0.255)	(0.494)	(0.140)
	Observations	2,081	104,029	1,029	39,219	2,080	103,986	1,015	38,839
	Clusters	26	80	22	80	26	80	22	80
(2)	Prop. Southern black	-4.391***	1.518***	-2.803*	1.091**	-2.773**	0.391	-2.275	0.351
		(1.325)	(0.419)	(1.522)	(0.460)	(1.361)	(0.559)	(1.422)	(0.379)
	Prop. Southern white	0.809*	-0.644***	-0.640	-0.195	-0.144	-0.336	0.381	0.0222
		(0.486)	(0.219)	(0.444)	(0.276)	(0.587)	(0.283)	(0.439)	(0.243)
	Observations	2,081	101,339	1,029	36,413	2,080	101,299	1,015	36,055
	Clusters	26	75	22	62	26	75	22	62
(3)	Prop. Southern black			-2.057***	1.067***			-1.552**	0.581***
				(0.702)	(0.309)			(0.721)	(0.198)
	Prop. Southern white			-1.300***	-0.240*			-0.381	-0.0111
				(0.446)	(0.125)			(0.603)	(0.0706)
	Observations			1,029	38,078			1,015	37,712
	Clusters			22	75			22	75
(4)	Prop. Southern black			-4.907***	0.370			-2.338**	0.263
				(1.335)	(0.461)			(1.035)	(0.240)
	Prop. Southern white			0.0116	0.160			-0.153	0.0802
				(0.471)	(0.195)			(0.423)	(0.130)
	Observations			1,029	35,644			1,015	35,295
	Clusters			22	59			22	59

Notes—All specifications include indicators for age and education; specification (2) includes white and black metro-level percent employed in manufacturing, percent farming, and average years of education; specification (3) includes the metro level average dependent variable, lagged one decade; specification (4) includes metro-level average variables and a lagged mean dependent variable. Standard errors, clustered by metro area, reported in parentheses. "***", "**", and "*" denote significance at the 1, 5, and 10% levels, respectively.

Table 1.21: Changes in South-North migration

		% change (1940-1960)		% change (1940-1970)	
Education group	Experience group	White	Black	White	Black
Less than 5th grade	6-10	0.10	-0.50	-0.15	-1.00
	11-15	-0.02	0.20	-0.48	-0.75
	16-20	0.58	0.24	-0.19	-0.65
	21-25	0.58	0.11	-0.23	-0.71
	26-30	0.90	0.08	0.33	-0.69
	31-35	0.25	0.14	0.30	-0.58
	36-40	0.98	0.21	1.21	-0.40
5th-8th grade	1-5	2.14	3.05	2.63	5.69
	6-10	0.04	0.13	-0.39	-0.60
	11-15	0.10	0.01	-0.37	-0.61
	16-20	0.25	0.09	-0.34	-0.65
	21-25	0.09	0.20	-0.24	-0.47
	26-30	0.15	0.19	-0.02	-0.37
	31-35	0.62	0.81	0.63	0.33
	36-40	0.83	1.19	0.72	0.78
Some high school	1-5	3.06	1.66	4.62	2.33
	6-10	0.83	1.92	0.70	1.36
	11-15	0.63	1.82	0.46	1.59
	16-20	0.88	2.72	0.59	2.41
	21-25	1.50	3.60	1.22	3.11
	26-30	2.11	4.79	2.58	6.97
	31-35	2.02	4.49	3.60	9.39
	36-40	3.87	5.48	5.94	11.88
High school degree	1-5	1.95	3.04	2.56	4.38
	6-10	1.18	2.29	2.22	4.47
	11-15	1.09	4.62	1.87	11.38
	16-20	1.92	4.34	3.09	9.02
	21-25	3.32	5.32	4.21	6.46
	26-30	3.28	6.31	5.26	16.27
	31-35	2.63	8.73	6.42	14.33
	36-40	2.75	4.20	8.65	8.27
Greater than high school	1-5	1.72	2.64	5.74	6.52
	6-10	1.94	3.54	3.81	8.23
	11-15	1.84	4.60	3.00	8.50
	16-20	2.24	2.71	3.50	6.20
	21-25	1.78	2.34	3.12	4.29
	26-30	1.31	2.19	4.74	4.89
	31-35	1.63	1.76	4.36	4.55
	36-40	1.90	2.35	5.78	4.18

2

INTERGENERATIONAL ALTRUISM IN THE MIGRATION DECISION CALCULUS: EVIDENCE FROM THE GREAT MIGRATION

2.1 INTRODUCTION

In this paper, I estimate the effect of intergenerational altruism on the propensity to migrate during the Great Migration—a period spanning 1915-1970 during which millions of Southern-born blacks left their birthplaces for cities in the Northern United States. Social scientists have long recognized that the Great Migration was integral to the economic gains realized by blacks during the 20th century (see Smith and Welch, 1989; Tolnay, 2003, for example); as we develop a greater understanding of the motives of the Great Migrants, so do we better understand the history of black progress and the intergenerational dynamics of inequality. While it is believed that the welfare of future generations helped motivate the Great Migrants to leave the South (cf. Wilkerson, 2010; Eichenlaub et al., 2010)—and that it figures prominently in decisions of many other migrants (see, for example, Borjas, 1993; Tcha, 1995, 1996; Deutsch, Epstein, and Lecker, 2006; Caponi, 2011)—there has been little empirical work addressing the sensitivity of migrant flows to intergenerational altruism, either in general or in the context of the Great Migration. Although, like all migration episodes, the Great Migration was a product of unique historical circumstances, the question faced by Southern blacks—whether the benefits of migrating outweighed the financial and psychological costs—is the same that, as Sjaastad (1962) first argued, all potential migrants face. Thus, evidence from the Great Migration can also inform contemporary migration policy, better-enabling policymakers to predict, and possibly influence, migrant flows.

Many studies have examined intergenerational dimensions of migration. Chiswick (1977), in an early contribution, finds that second generation immigrants fare better in the labor market than their parents and children. Borjas (1993) develops a model of dynastic selection into migration, which he tests against Census data on immigrants to the US. He finds evidence of negative selection among second-generation immigrants whose parents hail from source countries with high income inequality, as well as intergenerational earnings patterns that mirror those reported by Chiswick. Deutsch, Epstein, and Lecker (2006) argue that the intergenerational earnings patterns first identified by Chiswick can be explained by reciprocal altruism and use Oaxaca-Blinder decompositions to show that, consistent with their theoretical model, the differences in earnings across successive generations of immigrants to Israel cannot be explained by differences in observable characteristics. Caponi (2011) develops a structural, intergenerational model of the migration decision in order to explain several stylized facts about Mexican immigrants to the US (including the Chiswick earnings profile). His counterfactual exercises demonstrate that intergenerational altruism and migration-induced depreciation in human capital can explain why second-generation immigrants are more educated (and command higher wages) than their first- and third-generation counterparts.

There is, in addition, a small literature on whether altruism motivates migration. Tcha (1995, 1996) develop models of the migration decision when preferences exhibit reciprocal parent-child altruism. Tcha (1996) shows that, under such models, the earnings thresholds that characterize the migration decision rule depend on the degree of altruism, and that using "dynastic earnings thresholds" improves the ability of empirical models to explain flows of rural-urban migrants in Korea and the US. Berman and Rzakhanov (2000) test their hypothesis that immigrants are self-selected on intergenerational altruism using data on the fertility of Eastern European immigrants to Israel before and after a policy change that dramatically increased access to Israel. The relationship between altruism and migration has also been investigated in the context of remittances (see, e.g., Lucas and Stark, 1985; Shen, Docquier, and Rapoport, 2009). Papers in this strand of the literature present models in which some agents are willing to migrate in order to remit portions of their earnings back to family members in the source country. Lucas and Stark (1985), for example, find that the remittance behavior of rural-urban immigrants in Botswana is consistent with a combination of altruism and self-interest.

Thus, the literature has shown that immigration has implications for intergenerational mobility and that altruism plays a role in the decision to migrate, but has not provided evidence on the degree to which altruism motivates migrants, or consequently, the effect of intergenerational altruism on migrant flows. In order to quantify the effect of altruism on migration, I develop and estimate a dynastic model of residential location choice in which parents consider the welfare of future generations when deciding whether to migrate. My basic framework can be viewed as a simplification of the Barro-Becker model (1989, cf. Loury, 1981; Becker and Tomes, 1986; Becker and Barro, 1988), in which forward-looking parents can bequeath capital (in the form of time, human capital, etc.) to their children as an investment in future generations. In my model, current-generation utility depends only on exogenously-endowed characteristics and residential location, so that parents are able to invest in their children by choosing the location to which they expect their children, grandchildren, etc. to be best-suited (i.e, have the greatest earnings capacity or non-pecuniary taste). As I discuss below, Northward migrations are not well-explained by observable characteristics, suggesting that unobserved characteristics loom large in the migration decision process. Moreover, both observed and unobserved endowments may by correlated across generations, as in Becker and Tomes (1986). Empirical models that do not account for such unobserved heterogeneity will misstate agents' location preferences and, consequently, the effect of altruism on the propensity to migrate. In order to allow for this possibility, I use one of Arcidiacono and Miller's (2010) conditional choice probability estimators for dynamic models with unobserved heterogeneity. A unique aspect of my dataset is that it follows families for enough to generations to meet the conditions for the nonparametric identification of dynamic finite mixture models (Kasahara and Shimotsu, 2009).

I estimate that, over the sample period, altruism increased the probability of migrating by 4-6% on average (depending on the generation), with somewhat larger effects among those with more education or higher fertility, groups for which the potential benefits of migrating are greater. My estimates also suggest that correlated but unobserved location preferences play an integral role in determining migration behavior, masking considerable heterogeneity in the propensity of migrating. In particular, I find that roughly 65% of the Southern population is unlikely to migrate regardless of altruism towards future generations. Among the remainder of the population, the implied effect of altruism is substantially higher, on the order of 12-16%. Thus, the impact of altruism on the propensity to migrate is relatively small when averaged over the complete pool of potential migrants but appreciable among those for whom migrating is a legitimate possibility. Policy analysts seeking to predict the effects of a proposed change to migration policy may find the former impact more informative while social scientists concerned with understanding migration behavior may be more interested in the latter. Finally, by comparing the effects of a counterfactual subsidy that permanently offsets the cost of migrating to one which does so only for future generations, I show that altruistic migration—migration undertaken to the detriment of the current generation in order to increase the welfare of future generations—is the main channel through which intergenerational altruism impacts migration behavior.

2.2 MODEL

2.2.1 *Theoretical framework*

My empirical model can be viewed as simplifying Barro and Becker's (1989) model of intergenerational transfers (cf. Becker and Tomes, 1986; Becker and Barro, 1988) by making residential location choice the instrument through which parents can invest in their children and, in order to focus on the migration decision, abstracting away from human capital and fertility decisions by assuming that they evolve exogenously. In this model, successive members of a dynasty decide whether to live in the South or the North, taking into account how their location decisions affect their flow utility as well as the dynastic continuation utility that they receive by virtue of their altruistic concern for future generations' wellbeing.

If the gth generation of the dynasty is endowed with characteristics z_g, let the flow utility accruing to g from living in location $l \in \{s, n\}$ be denoted by $u_{lg}(z_g) + \epsilon_{lg}$, where the random component ϵ_{lg} is idiosyncratic across generations and independent of z_g. I assume that g expects all of her children to face the same endowments z_{g+1} and random location preferences ϵ_{lg+1}, so that if $\lambda \in (0, 1)$ is the intergenerational discount factor (i.e., the rate of intergenerational altruism) and $h(k)$ describes the rate at which continuation utility increases in fertility k (an element of z), expected dynastic utility from generation g on can be expressed

$$E\left\{\sum_{\gamma=g}^{G}\sum_{l\in\{s,n\}} 1[l_\gamma(z_\gamma,\epsilon_\gamma)=l]h(k_\gamma)^{1_{\gamma\neq g}}\lambda^{\gamma-g}\left[u_{l\gamma}(z_\gamma)+\epsilon_{l\gamma}\right]\right\}$$

where $1[l_\gamma(z_\gamma, \epsilon_\gamma) = l]$ indicates γ's location choice (a function of her endowments and idiosyncratic preferences) and the expectation is taken over the endowments z_γ and idiosyncratic preference terms $\epsilon_{l\gamma}$. Let $E[V_{g+1}(z_{g+1})|z_g, l_g]$ denote the expected continuation utility of the dynasty under the assumption that all generations after g choose their locations optimally and let g's conditional valuation functions

$$v_{lg}(z_g) + \epsilon_{lg} = u_{lg}(z_g) + \epsilon_{lg} + h(k_g)\lambda E[V(z_{g+1})|z_g, l_g]$$

represent the utility to g of choosing location l under this assumption, so that the expected valuation function can be defined recursively as

$$E[V_{g+1}(z_{g+1})|z_g, l_g] = E\left\{\max_{l\in\{s,n\}}\left[v_{l,g+1}(z_{g+1})+\epsilon_{l,g+1}\right]|z_g, l_g\right\}$$

and g's choice problem reduces to choosing the location that maximizes $v_{lg}(z_g) + \epsilon_{lg}$.

Several aspects of this framework warrant additional interpretation. First, like the theoretical model of Barro and Becker and the empirical models used in Caponi (2011) and Heckman and Raut (2013), this framework ignores life-cycle aspects of the choice problem, assuming that agents, upon observing their permanent endowments and preferences, choose the residential locations that maximize the discounted sum of lifetime utilities over the duration of the dynasty. In contrast, Gayle, Golan, and Soytas (2013) estimate a model that combines both life cycle and intergenerational considerations. Their model is better-equipped to explain features of the data such as the timing of fertility and the relationship between fertility and human capital decisions. Similarly, the dynamic location choice models of Kennan and Walker (2011) and Bishop (2012) allow individuals to choose many residences over the course of a lifetime, allowing them to capture common migration phenomena such as return and chain migration. My dataset consists of cross sectional observations from three generations which, as I discuss below, helps me identify correlated unobserved heterogeneity. However, since it contains little information about the timing of location and other choices over the life cycle, I am unable to incorporate these features into my model.

Second, because a model in which individuals simultaneously choose their characteristics and residential location is equivalent to one in which the location decision is made after observing characteristics drawn from a distribution obtained by margining location out of the equilibrium joint distribution over all choice variables, little generality is lost in assuming that the state variables evolve exogenously. One limitation of this assumption is that it precludes me from modeling altruism's impact on joint outcomes such as education-location and location-fertility.

Third, I focus on the location decisions of Southern-born blacks during the Great Migration. As Tolnay (2003) and Eichenlaub et al. (2010) note, the Great Migration was effectively over by the mid 1970s, making the dynamic residential location choice problem a non-stationary one. My stylized interpretation of this trend is that, after the 1970s, forces such as the Civil Rights Movement and the interregional diffusion of labor, capital and technology brought the Northern and Southern labor markets into a steady-state equilibrium, leaving agents indifferent between living in either location (at least before accounting for moving costs). In this case, after the 1970s (roughly the fourth generation in my dataset), Southern- and Northern-born agents would have faced the same expected lifetime utility, eliminating the dynastic aspect of the location choice problem. This time-horizon assumption, which I use in estimation, enables me to identify migration costs which, in turn, play an important role in estimating how intergenerational altruism affects migration behavior. As I discuss in Section 2.3, although misspecification of the time horizon faced by the agents in the model may lead me to draw incorrect inferences about the effects of altruism on migration, given information about the nature of the misspecification, the direction of the inconsistency can be determined and the resulting estimates can be viewed as bounds on the true effects.

Let

$$p_g(x,\lambda) = \iint 1[v_{ng}(x,w;\lambda) + \epsilon_{ng} > v_{sg}(x,w;\lambda) + \epsilon_{sg}] f_{w|x,g}(w|x,g) dw f_{\epsilon_g}(\epsilon_g) d\epsilon_g$$

be the conditional probability of migrating, where endowed characteristics are partitioned into $z = (x, w)$ and the dependence of the conditional valuation function v on λ has been made explicit, and

$$p_g(x,0) = \iint 1[u_{ng}(x,w) + \epsilon_{ng} > u_{sg}(x,w) + \epsilon_{sg}] f_{w|x,g}(w|x,g) dw f_{\epsilon_g}(\epsilon_g) d\epsilon_g$$

denote the probability that a myopic agent—one who places no weight on the utility of future generations and thus makes location decisions with respect to flow utility alone—

migrates to the North. A natural measure of the impact of intergenerational altruism on the propensity to migrate is

$$p_g(x,\lambda) - p_g(x,0) = \iint \{1[v_{ng}(x,w;\lambda) + \epsilon_{ng} > v_{sg}(x,w;\lambda) + \epsilon_{sg}] \\ - 1[u_{ng}(x,w) + \epsilon_{ng} > u_{sg}(x,w) + \epsilon_{sg}]\} f_{w|x,g}(w|x,g) dw f_{\epsilon_g}(\epsilon_g) d\epsilon_g.$$

The difference $p_g(x,\lambda) - p_g(x,0)$, or equivalently, the difference in the probability of migrating between otherwise-similar altruistic and selfish individuals, is my primary estimand of interest.

Insofar as endowed characteristics are heritable, parents and children are likely to have similar location preferences, in which case myopic and altruistic parents might be expected to make similar migration decisions. Within my theoretical framework, the source of migration-altruism dynamics is when accounting for the wellbeing of future generations alters the balance of relative location preferences, in which case

$$sign\left\{[v_{ng}(z;\lambda) + \epsilon_{ng}] - [v_{sg}(z;\lambda) + \epsilon_{sg}]\right\} \neq sign\left\{[u_{ng}(z) + \epsilon_{ng}] - [u_{sg}(z) + \epsilon_{sg}]\right\}.$$

Even when endowments are correlated across generations, altruism may increase or decrease the propensity to migrate. The flagship case under which altruism will make migration more likely is when the moving costs embedded in the relative flow utilities are prohibitive for selfish agents but, after the migrating generation has born the one-time cost, migration is sufficiently beneficial to future generations to induce altruistic agents to migrate. Altruism may increase or decrease the propensity to migrate when the deciding factor for a selfish agent is an unusually high or low idiosyncratic preference $\epsilon_{ng} - \epsilon_{sg}$ for living in the North. Here, altruistic agents will behave differently than their selfish counterparts because it is unlikely that future generations will share their idiosyncratic location tastes. The average impact of altruism will depend on the relative measures of these cases across populations with different endowments and idiosyncratic location preferences.

This framework can also be used to understand the value of incorporating dynamics and heterogeneity into empirical models of location choice. A static choice model will produce inconsistent estimates of the parameters of the current utility function. Because these estimates will reflect elements of both current and dynastic continuation utility, a static model may provide a reasonable approximation of the probability of migrating. However, such a model will not be able to estimate the impact of altruism on the propensity to migrate since its reduced form parameters will not distinguish between current and dynastic utility components. Similarly, when intergenerationally-correlated heterogeneity is part of the migration decision process, a model that ignores such heterogeneity will estimate parameters that are "averaged" over the unobserved types. Since the probability of migrating is nonlinear in the parameters of the conditional valuation function, however, the probability of migrating given "average" parameters will differ from the average probability of migrating given the actual parameters, with no a priori way to assess the error introduced by ignoring unobserved heterogeneity.

2.2.2 *Empirical implementation*

I estimate several migration models, each of which treats the location decision as depending on education, fertility, gender, an idiosyncratic shock and, in some cases, an unobserved type. Let (d, g) denote the gth generation of dynasty $d \in \{1, \ldots, D\}$. Let (d, g)'s education be described by $e_{dg} \in \{1, \ldots, 9\}$ where each element represents years of completed schooling in two-year intervals (so that 1 represents 0-1 years of school and 9 represents 16-17 years). I discretize fertility, letting f_{dg} equal one if (d, g) has greater than the median number of children among those in generation g, and zero otherwise. A gender indicator m_{dg} is equal to one if (d, g) is male and zero otherwise. I further assume that all generations of a dynasty belong to one of two unobserved types, indexed by $\tau_d \in \{1, 2\}$.

I parameterize the flow utility functions for each model as $u_{lg}(x_{dg}, \tau_d) = u_{lg}(x_{dg}, \tau_d; \beta, c)$, where β is a vector of parameters and c is the cost of migrating (the exact specifications are detailed in Section 2.6). The coefficients in these models should be interpreted as structural parameters that combine both real-income and non-pecuniary differences in relative location-specific utilities. Those with more education, e.g., may simultaneously command higher wages in the North because of regional differences in skill prices and prefer the North because education gives one a taste for Northern amenities.[1] The cost parameters should similarly be viewed as combining both the transportation and psychological costs of migrating. Additionally, since remaining in the South includes the possibility of moving within the South, moving costs in these models should be seen as describing the cost of migrating to the North net of the costs associated with potential smaller-scale intraregional moves. Finally, many studies (see, e.g., Ríos-Rull, 2002; Gayle et al., 2013) report evidence of assortative mating, providing an a posteriori justification for approximating the distribution of the correlated unobserved heterogeneity using two types that are transmitted perfectly across generations.

Nearly all of the Northern-born individuals in my sample choose to remain in the North, precluding me from modeling the location choices of Northern-born agents. Instead, I model the location choices of Southern-born agents under the assumption that the North is an absorbing state, so that a Northward migration terminates the intergener-

1 Another possibility is that Southerners obtain more education in preparation for a planned migration to the North. As mentioned previously, while my model does not capture life-cycle aspects of the migration decision problem, it is not inconsistent with such behavior.

ational decision problem. Normalizing the value to future generations of residing in the North to zero, I assume that the conditional valuation functions take the form

$$v_{sg}(x_{dg}, \tau_d; \beta, c) + \epsilon_{sdg} = \begin{cases} u_{sg}(x_{dg}, \tau_d; \beta, c) + \epsilon_{sdg} + \lambda[1 + (\mu_g - 1) f_g] E[V_{g+1}(x_{d,g+1}, \tau_d) | x_{dg}, s_{dg}, \tau_d] & \text{if } g < 3 \\ u_{sg}(x_{dg}, \tau_d; \beta, c) + \epsilon_{sdg} & \text{if } g = 3, \end{cases}$$

where $\mu_g = E(k|g, k \geq \tilde{k}) / E(k|g, k < \tilde{k})$ is the fertility ratio among those with above and below median fertility in generation g, and

$$v_{ng}(x_{dg}, \tau_d; \beta, c) + \epsilon_{ndg} = u_{ng}(x_{dg}, \tau_d; \beta, c) + \epsilon_{sdg}. \tag{2.1}$$

Note that, embedded equation (2.1), is the idea that the utility from migrating to the North is distinct from the utility of residing in the North, as only the generation that undertakes the migration bears the cost of doing so. This is the sense in which migration can properly be viewed as an investment in future generations, and the model a variation on Barro and Becker (1989).[2] Note also that my implementation abstracts away from the tradeoff between the quantity and quality of children.[3]

2.3 IDENTIFICATION

Hotz and Miller (1993) establish that under mild regularity conditions, given a distribution for the (serially uncorrelated) unobserved state variables and a value for the discount factor, there is a mapping from the data (i.e. the conditional choice probabilities and state transitions) to differences in the conditional valuation functions. Drawing on this work, Magnac and Thesmar (2002) show that these differences can be used to nonparametrically identify differences in flow utility functions, arguing that although the required assumptions about the unobserved state variables and discount factors render dynamic discrete choice problems generically underidentified, restrictions on the flow utility functions can restore identification in some cases. Arcidiacono and Miller (2013) extend these identification results, characterizing the degree of underidentification and, among other things, elucidating the conditions under which identification can be restored.

Absent serially-correlated unobserved heterogeneity, nonparametric identification of the flow utility functions for my model requires estimates of the conditional (on the

2 Strictly speaking, I normalize the utility of residing in the North to ϵ, so that the continuation value after migrating will be a discounted sum of Euler's constants under the distributional assumptions on the ϵ that I make below. However, this term will also appear in the valuation function for remaining in the South, with no effect on the probability of a move.

3 Gayle et al. (2013) estimate this tradeoff using data from the Panel Study of Income Dynamics, finding that, while there is evidence of a tradeoff, dynastic continuation utility is not too far from linear in fertility.

observed state variables) probability of migrating, estimates of the transitions over the observed state variables, a distribution from which the idiosyncratic location preference terms (i.e., the uncorrelated unobserved heterogeneity) are drawn, and an assumption about either the time horizon or the rate of intergenerational altruism (i.e., the dynastic discount factor). The paucity of North-South migrations in my dataset makes disentangling migration costs from pure location preferences another concern, and identifying the distribution of the unobserved types places additional requirements on the data and the model. I assume that the observed state variables evolve independently of the unobserved type and that the idiosyncratic preference terms are distributed Type-I Extreme Value. I discuss the remaining conditions for identification below.

2.3.1 *Altruism*

I assume that the intergenerational discount factor λ is equal to $.95^{25} \approx .28$. This assumption is consistent with two heuristic interpretations: either it takes 25 years for parents to learn what their children's lifetime utility will be, or, only after children have matured to adulthood do they provide their parents with altruistic utility. Similar assumptions have been used in previous research. Caponi (2011), in his study of Mexican immigration to the US between 1994 and 2008, assumes a value of $.9615^{30} \approx .31$. Glover and Heathcote (2011) use wealth data to calibrate a parameter that implies a discount factor of .31 in a two-period overlapping generations economy in which the progression of time is roughly comparable that in my model. Heckman and Raut (2013), using data from the Panel Study of Income Dynamics over a period of time that overlaps with my data, estimate a value of .44.[4] Gayle et al. (2013) also estimate an intergenerational altruism parameter, although since their model incorporates both life cycle and dynastic elements, it is difficult to compare their estimates to a pure intergenerational discount factor.[5]

Arcidiacono and Miller (2013) note that, in finite-horizon models, time preferences are identified directly from period-specific flow utilities, generalizing a result from Magnac and Thesmar (2002) that restrictions on the current utility functions can be used to identify the discount factor. Although the discount factor is in principle identified from the data, I prefer to use a value that is comparable to those estimated by or used in previous studies for two reasons. The first is that, in my model, identification of the altruism parameter relies heavily on assumptions about the time horizon. While it is the historical fact that the Great Migration had come to an end by the 1970s that motivates my assumption of a finite time horizon, any incongruity between this stylized assumption and the data-

4 Their model does not account for fertility, which may partially explain their comparatively high point estimate.

5 Estimates of a previous version of their paper that models future utility in a manner similar to mine are close to the value that I use.

generating process may cause me to misstate the altruism parameter and, consequently, the other structural parameters of the model.

To understand the source of potential inconsistency, suppose that the inversion and representation arguments of Arcidiacono and Miller (2013) have been used to obtain estimates of the South-North relative ex-ante conditional valuation functions $v_g(x) = v_{ng}(x) - v_{sg}(x)$. These functions can be parameterized by regressing them on a constant and a flexible function of the observed state variables x. Let k_g be the corresponding constant terms for each generation and suppose further that the relative ex-ante flow utility functions are assumed to take take the stationary parametric form $u_g(x;\beta,c) = \beta_0 + c + h(x;\beta)$. By Theorem 1 of Arcidiacono and Miller (2010), the conditional valuation functions can be represented in terms of the conditional probabilities that all successive generations remain in the South, in which case (ignoring fertility and unobserved types for simplicity)[6]

$$k_2 = \beta_0 + c + \lambda(\beta_0 + c)$$

and

$$k_3 = \beta_0 + c,$$

so that the altruism parameter can be recovered algebraically using

$$\lambda = \frac{k_2 - k_3}{k_3}. \tag{2.2}$$

Equation (2.2) can be used to sign the effect of misspecification of the time horizon on the estimated altruism factor.[7] For example, suppose that the dynamic decision problem actually lasts four generations but is misspecified as ending after the third. In this case, the estimator for λ becomes

$$\hat{\lambda} = \frac{k_2 - k_3}{k_3} = \frac{[\beta_0 + c + \lambda(\beta_0 + c) + \lambda^2(\beta_0 + c)] - [\beta_0 + c + \lambda(\beta_0 + c)]}{[\beta_0 + c + \lambda(\beta_0 + c)]} = \frac{\lambda^2}{1 + \lambda} < \lambda.$$

Here, the estimate of λ will be too low. In a parametric utility specification, this inconsistency will spill over to the other structural parameters as well, invalidating estimates of the effect of altruism on migration. Other types of misspecification will have a similar effect on the estimated altruism parameter.[8] Another concern is that, as I discuss below,

6 Normalizing the value of residing in the North to zero and maintaining the assumption of Gumbel errors, the generation-two relative conditional valuation function can be represented using

$$v_2(x) = \beta_0 + c + h(x;\beta) + \lambda\{E[\beta_0 + c + h(x;\beta)|x] - E[\log(1 - p_3(x_3))|x]\}$$

where $p_3(x)$ is the conditional probability of migrating for members of the third generation.

7 Note that the discount factor could also be identified by comparing the behavior of generations one and three; with three periods of data, it is overidentified.

8 For example, suppose the environment becomes a stationary infinite-horizon one after three generations, but the expected utilities of being born in the South and North are not the same. If the relative expected continuation value of being born in the South after the third generation is $E[V(x_{g>3})|x_{g-1}, l_{g-1} = s] = \varphi > 0$

the location decisions of some of the younger members of the third-generation in my sample may be right-censored, potentially overstating their preferences for remaining in the South. Within the foregoing identification procedure, right censoring is tantamount to overstating k_3, which in turn will understate λ (since $d\hat{\lambda}/dk_3 = -k_2/k_3^2 < 0$).

My second reason for preferring to assume, rather than estimate, λ is that my treatment of unobserved heterogeneity may generate estimates that understate the degree of altruism. To see this, consider the simplified case where preferences only depend on the type $\tau \in \{1, 2\}$ and an idiosyncratic preference term, and the type is observed. Repeating the preceding estimation procedure will recover constant terms k_g^τ for each type and generation. The altruism parameter, which I assume is type-independent, can then be recovered by averaging type-specific estimates $\hat{\lambda}^\tau = (k_2^\tau - k_3^\tau)/k_3^\tau$ according to $\hat{\lambda} = \pi\hat{\lambda}^1 + (1-\pi)\hat{\lambda}^2$, where π is the proportion of the population with $\tau = 1$. Now suppose that dynasties with $\tau = 2$ never migrate, in which case $k_g^2 = \infty$ for all g. Since the limit of $\hat{\lambda}^2 = (k_2^2 - k_3^2)/k_3^2$ as $(k_2^2, k_3^2) \to (\infty, \infty)$ does not exist, approaching λ only in the special case where k_2^2 and k_3^2 grow along the path $k_2^2 = k_3^2 + \lambda k_3^2$ (the path implied by the finite horizon specification), the altruism parameter is not identified for type-2 dynasties. If the estimates of k_2^2 and k_3^2 converge to infinity at the same rate, $\hat{\lambda}^2$ will approach zero, severely attenuating the overall estimate of λ, particularly if a large fraction of dynasties belong to the second type.

Intuitively, this attenuation arises because the altruism parameter is identified from differences in the migration behavior of successive generations, and these differences vanish when preferences for the South approach infinity. Since migrations are rare and not well-explained by observable characteristics in my data, families that never leave the South have a high posterior probability of belonging to a type with strong unobserved preferences for the South, similar to the type-2 dynasties in simplified example above. Since this group comprises a large fraction of the dynasties in the data, estimates of λ obtained from models that allow for unobserved heterogeneity place more weight on the group for which this parameter is the hardest to estimate. Indeed, when I estimate the altruism factor using a version of my primary specification (detailed below) that excludes unobserved heterogeneity, I obtain a point estimate of about .25, close to the value that I assume. However, this estimate is statistically insignificant and, for the reasons outlined previously, may still understate the degree of altruism.

For these reasons, my model is poorly suited to estimate the altruism factor. In contrast, both Heckman and Raut (2013) and Gayle et al. (2013) use models in which parents can make investments in children that only increase parental utility by affecting future generations' outcomes; such investment behavior directly conveys information about intergenerational altruism. Conceptually, then, what my results establish is the effect of

we will have (again representing the valuation functions in terms of all generations residing in the South) $k_2 = \beta_0 + c + \lambda(\beta_0 + c) + \lambda^2\varphi$ and $k_3 = \beta_0 + c + \lambda\varphi$ so that $\hat{\lambda} = [\lambda(\beta_0 + (\lambda - 1)\varphi)]/(\beta_0 + \lambda\varphi) < \lambda$.

altruism on the propensity to migrate, given an assumed level of altruism. That parents exhibit altruism towards future generations is not controversial, however, and I use a value for the altruism parameter that is comparable to those estimated by other studies. Furthermore, as I show in Section 2.6, my estimates are insensitive to large changes in the assumed degree of altruism. It thus appears that it is the existence, rather than the degree, of altruism that matters, and the former has been established by previous research.

2.3.2 *Moving costs*

In a model where migration is a terminal action, it is difficult to disentangle moving costs from pure location preferences. What enables me to distinguish between these utility components is the assumption that the dynamic decision problem ends after three periods.[9] To see this, represent the conditional valuation functions in terms of all future generations living in the North and let k_g be the constant term in $v_g(x)$, so that, using the parameterization described above,[10]

$$k_G = \beta_0 + c$$

and

$$k_g = \beta_0 + c - \lambda c.$$

Algebraically, the moving cost can be identified by estimating separate constant terms for generations $g < 3$ and $G = 3$ and letting

$$\hat{c} = \frac{k_G - k_g}{\lambda} = c.$$

In a stationary model, it would be impossible to identify the moving cost, since the constant term would be $k = \beta_0 + c - \lambda c$ for every generation. The intergenerational migration probabilities $p(x, \lambda)$, which would only depend on the composite constant term $\beta + (1 - \lambda)c$, could still be recovered. Estimating the effect of intergenerational altruism on migration, however, would require an estimate of $p(0, \lambda)$, which itself would require the moving cost to be identified separately from the rest of the constant term.

9 In a binary location choice model with return migration, moving costs can be identified by comparing the differential behavior of otherwise-similar individuals with different initial locations. Because I observe virtually no North-South moves, I am unable to use this approach to infer moving costs. While a larger dataset may contain more variation in the behavior of Northern-born individuals, an additional complication is that it is unclear whether the distribution over the unobserved types would remain nonparametrically identified. Allowing for bidirectional migration would require adding the initial state (i.e., the lagged location choice) to the state space. The identification approach of Kasahara and Shimotsu (2009) requires six time periods per panel unit in this case, which is clearly infeasible when the unit of time is a generation. The approach of Hu and Shum (2012) allows for lagged choice variables in the state vector, but requires four time periods when the setting is stationary and at least five when it is not.

10 In this case, the relative conditional valuation function would be $v_g(x_g) = \beta_0 + c + h(x_g; \beta) - \lambda\{c + E[\log p_g(x_{g+1})|x_g]\}$ where $p_g(x)$ is the conditional probability of migrating.

The time horizon assumption is consequently important to estimating the effect of altruism on the probability of migrating. My interpretation of the end of the Great Migration (Tolnay, 2003; Eichenlaub et al., 2010) is that a fourth-generation child born in either the South or the North faced the same expected utility, obviating the dynamic aspect of the location decision problem. That is, after three generations, the North ceases to be an absorbing state and, net of moving costs, the utilities of residing in the South and the North are the same (across observable and unobservable types), so that the constant from the generation-three relative conditional valuation function can be written

$$k_G = \beta_0 + c - \lambda \left[c + \log \left(\frac{p_{s,G+1}}{p_{n,G+1}} \right) \right],$$

where $p_{l,G+1}$ is the probability that a fourth-generation agent born in l chooses to live in the North.[11] If both locations yield the same utility, the only reason why a Southern-born agent would be less likely to live in the North than a Northern-born one is that the Southerner must pay the cost of migrating, in which case $\log(p_{s,G+1}/p_{n,G+1}) = -c$.

There are two potential complications to identifying the moving cost this way. First, right-censoring among third-generation respondents may overstate k_G (which represents their relative preferences for living in the South) and hence c. Second, the Northern and Southern labor markets may not have completely equilibrated by the end of the third generation. In either case, external information about the source of misspecification can be used to sign the resulting inconsistency in the estimated effects of altruism, in which case the estimates can be interpreted as bounding the true effects. For example, Tolnay (2003) notes that return migration to the South exceeded migration to the North during the mid-1970s. If Southerners found the North less attractive during this period (even before taking moving costs into account) then $\log(p_{s,G+1}/p_{n,G+1}) < -c$, implying $\hat{c} = (k_G - k_g)/\lambda > c$, in which case the estimated effects of altruism on migration can be viewed as upper bounds.[12] In fact, the coefficients from a parametric specification of the flow utility functions for a finite-horizon model can still be estimated consistently without estimating the moving cost as long as the constant term for the third generation is allowed to vary. Misspecification-induced inconsistency of the moving cost is therefore innocuous in comparison to that for the altruism parameter.

2.3.3 *Unobserved types*

While the identification approaches of Magnac and Thesmar (2002) and Arcidiacono and Miller (2013) require that all of the serially-correlated state variables are observed, my

11 This expression represents the valuation function in terms of all future generations residing in the North and exploits the fact that the model exhibits finite dependence (see Arcidiacono and Miller, 2010; Arcidiacono and Ellickson, 2011; Arcidiacono and Miller, 2013).

12 Since $dp(x,0)/dc < 0$, overstating the cost of moving will also overstate the impact of altruism on migration.

model allows for unobserved heterogeneity that persists across generations. The literature on the short-panel identification of dynamic choice models with unobserved heterogeneity demonstrates that, under suitable conditions, the type-dependent distribution functions that comprise the primitives of these models can be nonparametrically identified (see Kasahara and Shimotsu, 2009; Hu and Shum, 2012). In particular, that the elements of my model are, in principle, identified from the distributions governing the observable data follows from Proposition 4 of Kasahara and Shimotsu (2009).[13] Although in practice I use parametric specifications to smooth utilities over the state space and reduce the dimension of the estimation problem, this identification argument suggests that the information so-recovered is contained in the data itself.

2.4 DATA AND SUMMARY STATISTICS

The data that I use in this study are drawn from the Three-Generation National Survey of Black American Families, 1978-1981 (Jackson and Tucker, 1997). This dataset is a compendium of responses to several questionnaires administered as part of the National Survey of Black American Families, a nationally representative survey of black households in the US. When respondents to the cross-sectional survey reported having two living relatives belonging to different generations, these relatives were interviewed, and the initial respondent re-interviewed, resulting in a dataset containing responses to the same questions from members of three generations of 510 families. This intergenerational dataset includes information about educational attainment, fertility, birthplace, and residential location at the time of enumeration, the primary variables that I use in my empirical analysis.[14] Of the 1,425 individual observations that remain after omitting those with missing values for the key covariates, there are only 11 North-South migrations, making it virtually impossible to model the location decision problem for those born in the North. Instead, I model the decision of Southern-born agents to remain in the South or migrate North under the assumption that the North is absorbing—after a Northward migration, all future generations remain in the North, effectively terminating the dynamic decision problem. For this reason, I also omit respondents who were born in the North.

My empirical model is dynastic, treating the generation as the basic unit of time. Consequently, I also omit observations from members of the third generation who are younger than 18 at the time of the interview, as these respondents may have incomplete educa-

13 In addition to a rank condition (which is satisfied trivially), the proposition requires that (i) the choice probabilities do not depend on lagged choices, (ii) any (observable) element of the state space can be reached with positive probability, (iii) the transitions over the observable state variables do not depend on the unobserved type and (iv) there are at least three time periods. My model meets conditions (i)-(iv) and is thus nonparametrically identified subject to the rank condition.

14 I code respondents' current and birth locations as Southern according to the Census Bureau's definition of the Southern region. According to this definition, Alabama, Arkansas, Delaware, Florida, Georgia, Kentucky, Louisiana, Maryland, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Virginia and West Virginia are Southern states. I code all other locations within the US as belonging to the North.

tion and fertility histories and are less likely to have made their own residential location choices. This omission is unlikely to completely solve the problem of right-censoring among third-generation respondents. However, because the initial sample is already somewhat small, there is a tradeoff between including only those third-generation observations for which I can be reasonably sure that the data represent completed histories and retaining enough data to preserve variation in the observables and permit meaningful inference. Noting that it is generally straightforward to determine how incomplete histories will affect estimates of the utility of migrating, I include all third-generation respondents aged 18 or older.

After performing these sample selections, I am left with 443 families, from which observations from 1,099 individuals are used in estimation. Table 2.1 provides sample characteristics for these observations. The median respondent from the first generation was born in 1910 and 70 years old when the survey was administered between 1978 and 1981. The median second generation respondent was born in 1933 and 47 years old at the time of the survey. The median third generation respondent was born in 1954 and was 24.5 when surveyed. As the table shows, respondents, particularly those belonging to the first and second generations, are substantially more likely to be female. This is almost certainly a reflection of higher mortality rates among men, an explanation which is supported by the fact that 43%, a much larger proportion, of third-generation respondents are male. Because the intergenerational dataset was collected by interviewing relatives of respondents to the nationally-representative seed survey, the larger fraction of women may also partially reflect a greater willingness of females to participate in the follow-up surveys. Regardless of the underlying cause, the sample distribution of gender is not an accurate reflection of the population distribution across generations, a point to which I return in Section 2.5.

The summary statistics in Table 2.1 also provide a sense of the likelihood of Northward migration. Across generations, 86% of individuals choose to remain in the South. Members of the second generation are about 7% more likely to migrate than the sample average, although the similarity between the migration behavior of first- and third-generation respondents may partially owe to the younger ages at which the latter were interviewed. Unsurprisingly, the table also shows that successive generations obtained more education, with median years of completed schooling increasing from 7 to 11 to 12 across the three generations. Similarly, the median number of children declines from 6 to 4 to 1. Note well that right-censoring of the third generation plays a role in determining the observed rates of change in education and fertility.

To provide preliminary evidence on how the covariates are related to location preferences, I present in Table 2.2 a series of linear models of the probability that a Southern-born individual migrates North. For simplicity, these models use binary measures of education (an indicator for having more than a high-school education) and fertility (an indicator for having greater than the median number of children); using full sets of education and fertility indicators produces similar results. The first specification shows that, pooling across generations, migration is more likely among males, those with greater education, and those with more children. The covariates included in this regression only explain about 8% of the variation in migration. When the regressors are interacted with time, the main coefficients are similar, and the explanatory power of the model only increases by about 1%. For the third specification, which includes all interactions between time, gender, education and fertility, the R^2 is just over .1. Conditioning on gender and time as well as fertility and human capital decisions, two of the most important choices over the life cycle, only explains 10% of the variation in migration behavior. These regressions show that migration decisions are made, in significant measure, with respect to unobserved factors, illustrating the scope for empirical models that neglect this heterogeneity to be inconsistent, depending on how these unobservable factors are correlated with each other over time and with the included observables.

2.5 ESTIMATION

2.5.1 *Transitions*

In my model, there are nine education categories, two genders and two fertility levels, for a total of 36 observable states which evolve stochastically according to a Markov process that does not depend on either the unobserved type or the idiosyncratic preference shock. Estimating this transition function involves a tradeoff between capturing the mutual dependence between education, gender, and fertility and the precision with which the transitions can be estimated. I use a parsimonious multinomial logit specification in which, letting $\tilde{x}$ represent education and fertility (omitting gender), the probability of transiting from state x_g to state $\tilde{x}_{g+1}$ conditional on m_{g+1} is given by

$$\Pr(\tilde{x}_{g+1} = j | m_{g+1}, x_g) = \frac{\exp\left(\alpha_j(m_{g+1}, x_g)\right)}{\sum_{k \in \{1,\dots,K\}} \exp\left(\alpha_k(m_{g+1}, x_g)\right)},$$

where

$$\alpha_j(m_{g+1}, x_g) = \alpha_{j0} + \alpha_{j1} e_g + \alpha_{j2} f_g + \alpha_{j3} m_{g+1}.$$

I estimate the transitions conditional on gender because the greater proportion of women in the sample, as seen in Table 2.1, likely reflects a combination of earlier mortality among men and a higher willingness of women to participate in the follow-up survey. I then assume that male and female children are equally likely, estimating the unconditional transition function using

$$f(x_{g+1}|x_g) = \frac{1}{2}\Pr(\tilde{x}_{g+1}|m_{g+1}, x_g).$$

Because some of the state-space cells are sparsely populated, I pool data across generations and regions, estimating only one transition matrix to describe the evolution of the state variables over time. As the summary statistics presented above show, education in years is increasing over time. Thus, transitions from "low" to "medium" levels of education are estimated primarily using variation between generations one and two and transitions from "medium" to "high" levels are estimated primarily from the second and third generations, so that pooling over generations mainly helps estimate transitions between infrequently-obtained education levels. Similarly, the indicator for having above-median fertility is generation-specific, so intergenerational pooling only implies that the transmission of relative fertility from parent to child is stable over time. In addition, because (i) the majority of the sample is Southern-born, and (ii) I treat migration as a terminal action, so that birthplace-specific differences in the transitions do not factor into the future values of the options, including the Northern-born in the estimation sample for the transitions is unlikely to have much of an effect on the resulting estimates. At the same time, using this information allows me to better-capture the joint relationship between the state variables across generations and estimate the transitions with greater precision.

Because the resulting 36×36 transition matrix is somewhat unwieldy, I summarize the estimated transitions in Table 2.3 (the full matrix is presented in Appendix Table 2.14). The top panel of the table shows the estimated average unconditional transitions for education, obtained by margining over gender and fertility in generation $g+1$ and averaging over fertility in generation g. The estimates exhibit the expected pattern of conditionally increasing education over time. For example, the child of a parent with roughly a sixth grade education ($e_g = 4$) is most likely to have about a high-school education ($e_{g+1} = 7$), but relatively unlikely to complete additional schooling; the child of a parent with between 16 and 17 years of education remains most likely to obtain a high school degree, but is also relatively likely to obtain between 14 and 17 years of education. The bottom panel of the distribution shows the estimated average transitions over fertility. As expected, these estimates show that parents with below-median fertility are more likely to themselves have below-median fertility than children born to parents with above-median fertility.

2.5.2 *Conditional valuation functions*

Hotz and Miller (1993) establish that, under mild assumptions met by my empirical model, there is a mapping from the state-specific probabilities of selecting the elements of the choice set to the normalized (relative to a reference choice) conditional valuation functions, obviating the need to use backwards recursion or fixed point iteration to compute the future value terms. Arcidiacono and Miller (2010) further show that the valuation functions can be represented in terms of the conditional probabilities of selecting any (potentially counterfactual) sequence of decisions. I use these inversion and representation results to express the conditional valuation functions in terms of the conditional probabilities of migrating. Specifically, since the ϵ_{ldg} are distributed Type-I Extreme Value and the North is a terminating action, the (relative) valuation functions for living in the South can be expressed in terms of the choice sequence $(l_1 = s, l_2 = n, l_3 = n)$ as

$$v_g(x_{dg}, \tau_d) \equiv v_{sg}(x_{dg}, \tau_d) - v_{ndg}(x_{dg}, \tau_d) = u_{sg}(x_{dg}, \tau_d) - u_{ng}(x_{dg}, \tau_d) + [1 + (\mu_g - 1) f_{dg}]\lambda \left(u_{n,g+1}(x_{dg}, \tau_d) - \sum_{x_{g+1} \in X} f(x_{d,g+1} = x_{g+1} | x_{dg}) \log[p_{g+1}(x_{g+1})] \right), \tag{2.3}$$

where $p_{g+1}(x_{g+1}) = \Pr(l_{g+1} = n | x_{d,g+1} = x_{g+1})$ is the conditional probability of migrating to the North.

In equation (2.3), the conditional relative value of living in the South is simply relative current utility $u_{sg} - u_{ng}$ plus maximized relative future utility. Since the future utility associated with migrating is normalized to zero, the relative future utility term reduces to the maximized utility for future generations given that g remains in the South, discounted by λ and weighted by fertility. The maximized future value term for living in the South is represented in terms of all future generations living in the North. Under this sequence of future location choices, generation $g + 1$ receives flow utility $u_{n,g+1}$, to which a correction, $-E[\log p_{g+1}(x_{g+1}) | x_g]$, is added to account for the fact that living in the North may not, in fact, be optimal for generation $g + 1$. Given that $g + 1$ has chosen to migrate, a terminal action, future generations remain in the North, receiving flow utility that has been normalized to zero, and to which no further correction terms need to be added.[15]

2.5.3 *Estimation with unobserved types*

If τ_d were observed, the conditional choice probabilities could be estimated directly from the data, in which case representation (2.3) could be combined with the estimated tran-

15 That is, the model exhibits one-period finite dependence.

sitions and the model estimated by maximum likelihood. In order to estimate the model with unobserved types, I use one of the Arcidiacono and Miller (2010) estimators, which adapt the Expectation-Maximization algorithm (Dempster, Laird, and Rubin, 1977) for use in conditional choice probability estimation of dynamic discrete choice models. While the estimators are fully documented in Arcidiacono and Miller (2010) (see also Arcidiacono and Ellickson, 2011), I briefly outline the algorithm here.

Denote the probability of observing g's location choice and $(g+1)$'s observed state variables, conditional on g's observed state variables and d's type $\tau \in \{1,2\}$, by

$$L_g(l_{dg}, x_{d,g+1}|x_{dg}, \tau; \theta, p) = \left(\frac{\exp\left(v_g(x_{dg}, \tau; \theta, p)\right)}{1+\exp\left(v_g(x_{dg}, \tau; \theta, p)\right)}\right)^{1(l_{dg}=s)} \left(\frac{1}{1+\exp\left(v_g(x_{dg}, \tau; \theta, p)\right)}\right)^{1(l_{dg}=n)} f(x_{d,g+1}|x_{dg}),$$

where the dependence of v_g on the parameters θ of the utility functions and the vector p of conditional choice probabilities has been made explicit, and the logit form arises because the ϵ_{ldg} are Type I-Extreme Value (Rust, 1987, 1994). The $(m+1)$th stage of the algorithm proceeds in two steps. In the expectation step, the probability $q(\tau_d = \tau|x_d, l_d)$ that $\tau_d = \tau$ conditional on the sequences $x_d = (x_{d1}, x_{d2}, x_{d3})$ and $l_d = (l_{d1}, l_{d2}, l_{d2})$ of dynasty d's state variables and location choices is updated using

$$q^{(m+1)}(\tau_d = \tau|x_d, l_d) = \frac{\pi^{(m)} \prod_{g=1}^{3} L_g(l_{dg}, x_{d,g+1}|x_{dg}, \tau, \theta^{(m)}, p^{(m)})}{\sum_{\tau\in\{1,2\}} \pi^{(m)} \prod_{g=1}^{3} L_g(l_{dg}, x_{d,g+1}|x_{dg}, \tau, \theta^{(m)}, p^{(m)})},$$

where π is the fraction of dynasties with $\tau_d = 1$. The estimate of π is also updated in the step, according to

$$\pi^{(m+1)} = \frac{\sum_{d=1}^{D} q^{(m+1)}(\tau_d = \tau|x_d, l_d)}{D},$$

as are the conditional choice probabilities of migrating given x_{dg} and τ_d, using

$$p^{(m+1)}(x, \tau) = \frac{1}{1+\exp\left(v_g(x_{dg}, \tau; \theta^{(m)}, p^{(m)})\right)}.$$

In the maximization step, the parameters θ are updated by maximizing a sum of type-specific log-likelihood functions, weighted by the posterior probability that d belongs to either type:

$$\theta^{(m+1)} = \underset{\theta}{argmax} \sum_{d=1}^{D} \sum_{g=1}^{G} \sum_{\tau\in\{1,2\}} q^{(m+1)}(\tau_d = \tau|x_d, l_d) \log\left[L_g(l_{dg}, x_{d,g+1}|x_{dg}, \tau; \theta^{(m+1)}, p^{(m+1)})\right].$$

Iterations continue until convergence.

2.6 RESULTS

2.6.1 *Parameter estimates*

In the main specification of my empirical model, I parameterize the current utility functions as

$$u_{sg}(x_{dg},\tau_d)+\epsilon_{sdg}=\beta_0^{\tau}+\beta_1 e_{dg}+\beta_2 e_{dg}^2+\beta_3 m_{dg}+\beta_4 f_{dg}+\beta_5 g+\epsilon_{sdg}$$
$$u_{ng}(x_{dg},\tau_d)+\epsilon_{ndg}=-c+\epsilon_{ndg}.$$

That is, location preferences depend on education, gender, fertility and time, as well as moving cost and a fixed-effect that is specific to the unobserved type. Estimates of this specification are presented in the second column of Table 2.4 alongside estimates of the same model with no permanent unobserved heterogeneity (the first column). In interpreting the coefficient estimates, recall that the utility specifications relate the utility of living in the South relative to the North, which is normalized to zero. In the model with heterogeneity, the estimated constant term for those with $\tau = 2$ (hereafter the "low" type since they are relatively unlikely to migrate) is an order of magnitude larger than that for those with $\tau = 1$ (the high type). This result is unsurprising: families that reside in the South for all three generations are likely have the same unobserved type, and because they never migrate, must have strong preferences for living in the South.

More interesting are the effects of including unobserved heterogeneity on estimates of the other parameters of the model. In the model with no permanent heterogeneity, the main coefficient on grades is -.6, indicating that education decreases relative tastes for the South. In the model with heterogeneity, this coefficient is halved, although it remains negative. This pattern is due to textbook omitted variables bias: the high types ($\tau = 1$) tend to be better educated, leading the model without heterogeneity to overstate the role of education in determining tastes for the South. Accounting for unobserved heterogeneity has no clear impact on the gender coefficient, with both the point estimates and the standard errors increasing after including heterogeneity.

Including permanent heterogeneity has a notable effect on the coefficient on generation, causing it to switch signs and increase substantially in absolute value, a change which can be explained by dynamic selection bias. Since those remaining in the South over time are more likely to belong to the low type, and consequently less likely to migrate, the model without unobserved heterogeneity detects no trend in the utility of the South over time. When permanent heterogeneity is included, the utility of the South appears to decrease across generations, a finding more consistent with the increasing likelihood of migration over time evidenced in the summary statistics. This dynamic selection bias may also explain why the coefficient on fertility is so much lower in the model without heterogeneity.

If low types are more fecund, the fertility coefficient may be attenuated downward to reflect that, even among the low types, the probability of migrating is increasing over time (along with fertility among Southerners).

The estimated moving costs are clinically large. In the model without heterogeneity, for example, the relative lifetime utility of living in the South for a first-generation male with ten years of education and below-median fertility is about -4.3. The estimated cost of migrating is over half that sum. This finding accords well with those of Kennan and Walker (2011) and Bishop (2012), who also estimate that moving is very costly. The underlying explanation for these large cost estimates is that, even among those who, according to their observable characteristics, are the most likely to move, migrations are somewhat rare (among those with a high-school degree or greater, e.g., the average probability is on the order of about .2). Furthermore, allowing for some families to have systematically large relative tastes for the South does not drastically reduce the estimated cost (which drops from 2.7 to 2.1 when heterogeneity is included); even among the high types, migration is sufficiently uncommon that the cost of a move represents a substantial fraction of lifetime utility.

The estimated fraction π of the population who are of the high type is .35. As I discuss in Section 2.9, this estimate corresponds roughly with the fraction of dynasties that ever leave the South. This correspondence arises because the observables in the utility function do not explain much of the variation in migration, so its sequence of location choices is the most influential determinant of the posterior probability that a dynasty belongs to either type.

As the standard errors for the model with heterogeneity show, it is difficult to separately estimate the location preference parameters for the unobserved types and observed covariates. However, a Wald test for the joint significance of the observables rejects the hypothesis that they are not a part of the utility function. Table 2.4 also reports the results of a likelihood ratio test between the models with and without permanent heterogeneity. There are two sets of restrictions under which the model without heterogeneity can be nested into the model with heterogeneity: either the parameters are the same for both types or the parameters are type-dependent but the unobserved type distribution is degenerate. As Chen and Chen (2001) note (see also Chen, Chen, and Kalbfleisch, 2001), this multiplicity imbues the likelihood ratio test with a nonstandard limiting distribution. For this reason, I use the bootstrap procedure that they outline in order to test between these models. Let LR_D denote the likelihood ratio statistic and $\tilde{\theta}$ denote the parameters of the model with no unobserved heterogeneity, estimated using the full sample. I construct $B = 100$ bootstrap samples (l_{dg}^b, x_{dg}), $b \in \{1, \ldots, B\}$, $d \in \{1, \ldots, D\}$, $g \in \{1, 2, 3\}$, by drawing errors from a Type-I Extreme Value distribution for each b, d and g and computing the optimal location choices $l_{dg}^b(x_{dg}, \epsilon_{dg}^b; \tilde{\theta})$ implied by the null hypothesis of no heterogeneity. For each bootstrap sample, I estimate the model with and without heterogeneity and compute the likelihood ratio statistic LR_b. The p-value associated with the likelihood ratio test is the proportion of the LR_b that exceed LR_D.

As the table shows, none of the bootstrap samples generate a likelihood ratio statistic that exceeds that computed from the original sample; the model without permanent heterogeneity is easily rejected. The rarity of migrations can also explain why including unobserved heterogeneity improves the fit of the model. Because, regardless of their observables, people seldom move, the idiosyncratic location-preference errors play a pivotal role in rationalizing the migrations that do occur. Allowing for unobserved types reduces the implied magnitude of the random errors invoked to explain the observed migrations, increasing the likelihood of observing the data.

2.6.2 *The effect of altruism on migration*

In Table 2.5, I present estimates of the intergenerational and selfish migration probabilities, as well as the effects $p(x, \lambda) - p(x, 0)$ of altruism on migration, for this model among several different subpopulations x, computed using parameters estimated with and without permanent unobserved heterogeneity. Across specifications and subpopulations, the estimated probabilities vary as expected; they are increasing in education, decreasing in fertility, and higher for members of the second generation, reflecting the same patterns seen in the summary statistics. When permanent unobserved heterogeneity is accounted for, the estimated altruistic and selfish migration probabilities are close to zero for the low types. Although the stark difference between the types is a mechanical consequence of the use of two unobserved types (families that remain in the South must have high tastes for living there), the differences in migration probabilities between these types show that unobserved correlated preference components figure importantly in the migration decision process.

Accounting for heterogeneity, the average effect of altruism is to increase the probability of a migration by about 4% for members of the first generation and 6% for members of the second (since the horizon is finite, the effect is zero by construction for the third generation); I can reject the hypothesis that these effects are zero. Given that the average migration probabilities are about 10 and 20% in generations one and two, the magnitudes of these effects are nontrivial. The effects of altruism are greater-than-average among those with a high-school degree or better (between 5 and 6%, depending on the generation) and among those with above-median fertility (between 6 and 8%). The first effect clearly arises because education simultaneously increases both tastes for the North and the education of future generations. The second effect represents a combination of two forces: fertility decreases tastes for the North while increasing the number of children that can benefit from a Northward migration.

The corresponding effects estimated without accounting for heterogeneity are nearly all somewhat smaller. Even though there are marked differences in the parameter estimates between these models, the model without heterogeneity approximates the effects of altruism reasonably well. However, the differences between the effects of altruism estimated with and without heterogeneity are statistically significant among second generation individuals with low education or high fertility, groups with relatively low migration probabilities. High types with low education or high fertility may lie on a steeper part of the distribution function that determines migration probabilities, making their migration behavior more sensitive to intergenerational altruism.

Although the model without heterogeneity can approximate the average effect of altruism among the pool of all potential migrants, it cannot differentiate between those for whom migration is a realistic possibility and those who are unlikely to migrate regardless of altruism. As Table 2.5 shows, the average migration probability is over 50% among high types and close to zero among low types. The effect estimates in the table demonstrate that, among high types, the average effects of altruism on the probability of migrating are about 12 and 16 percent for the first and second generations, respectively. Because of their strong, and dynastically correlated, preferences for the South, the average effect of altruism among low types is virtually zero. Thus, the estimated effects of altruism presented in Table 2.5 answer two different questions. For policymakers concerned about the effects of a policy that would primarily benefit the children of would-be migrants, the relevant question is how much altruism would increase the total flow of migrants from the source location. The answer, at least insofar as the Great Migration is a generalizable case study, is not by much—on average over the entire pool of potential migrants, altruism increases migration by less than 6%. On the other hand, for a behavioralist interested in understanding what motivates migration decisions, the relevant question is the degree to which altruism encourages migration among those for whom migration is a realistic possibility (that is, among the high types). Among this group, altruism is much more influential, increasing the probability of a migration by as much as 16% on average.

2.7 ADDITIONAL SPECIFICATIONS AND ROBUSTNESS EXERCISES

2.7.1 *Moving cost heterogeneity*

The preference specification used to generate the results discussed above assumes that only pure location preferences, and not the cost of migration, depend on the unobserved type. In a stationary, infinite-horizon model, the estimated intergenerational migration probabilities are robust to misspecification of the role of unobserved heterogeneity. In a finite-horizon setting, the different implications of unobserved heterogeneity for agents in the terminal generation breaks this equivalence.[16] Moreover, as discussed in Section 2.3, what identifies moving costs in this model is the differential behavior of members of the third and members of the first and second generations, to whom these costs present different incentives. Misspecification of the way that the unobserved type enters the utility function may therefore distort estimates of the costs of moving and, consequently, the effects of altruism on migration.[17]

In order assess the sensitivity of my results to different assumptions about the role of unobserved heterogeneity, I also present, in the third column of Table 2.4, estimates of a second specification in which pure location preferences are type independent, but members of different unobserved types face different migration costs:

$$\begin{aligned} u_{sg}(x_{dg}, \tau_d) + \epsilon_{sdg} &= \beta_0 + \beta_1 e_{dg} + \beta_2 e_{dg}^2 + \beta_3 m_{dg} + \beta_4 f_{dg} + \beta_5 g + \epsilon_{sdg} \\ u_{ng}(x_{dg}, \tau_d) + \epsilon_{ndg} &= -c^{\tau} + \epsilon_{ndg}. \end{aligned}$$

The relationships between the coefficient estimates for the models with and without heterogeneity are similar to those for the first specification. The coefficient on education drops (by even more in this case) due to omitted variables bias and the coefficient on generation switches signs due to dynamic selection bias, causing the coefficient on fertility to increase in turn. Here as well, a Wald test rejects the hypothesis that the observable characteristics can be excluded from the utility function. The estimated moving cost for the high types is 1.93, lower than estimates from the models without heterogeneity and with location-preference heterogeneity, but still large. Partially compensating for this, the con-

16 To see this, first suppose that the environment is stationary and, in the data-generating process, both types have the same location preferences, but the high types have a lower moving cost. In this case (ignoring fertility for simplicity), the constant term in the conditional valuation function can be expressed $k^{\tau} = \beta_0 + (c - \Delta 1_{\tau=1}) - \lambda(c - \Delta 1_{\tau=1})$. If the empirical model is misspecified by allowing only for type-specific fixed effects, so that $\tilde{k}^{\tau} = \beta_0 + \tilde{\Delta} 1_{\tau=1} + c - \lambda c$, $\tilde{\Delta}$ will be estimated as $(1-\lambda)\Delta$, and the estimated constant and intergenerational migration probabilities will be the same even if the model is misspecified. When the time horizon is finite, the true and misspecified models are only equivalent when $\tilde{\Delta} = -\Delta = (\lambda - 1)\Delta$, a condition that requires that either Δ or λ is zero. This argument also applies to the case where both location preferences and moving costs are heterogeneous.

17 Misspecification of the dependence of moving costs on the covariates may also be a source of inconsistency. For example, suppose that the actual conditional valuation function is $v_g(x_g) = x_g'\beta + x_g'\gamma - \lambda\{E(x_{g+1}|x_g)'\gamma + E[\log p(x_{g+1})|x_g]\}$, where γ is a vector of cost function parameters (ignoring unobserved types and abusing notation by letting x be a vector rather than an element of the state space). If an interaction between β and an indicator for belonging to the terminal generation is included in the specification, the (population) parameter estimates from a model that ignores the dependence of moving costs on x will be $\tilde{\beta}_g = \beta + \gamma - \lambda\Lambda' x_g \gamma$ and $\tilde{\beta}_G = \beta + \gamma$, where Λ is the matrix of coefficients from a linear projection of the elements of x_{g+1} onto x_g. If $E(x_{g+1}|x_g)$ were linear in x_g, the altruistic migration probabilities $p(x, \lambda)$ could be estimated consistently (although the preference parameter estimates themselves would be inconsistent). If, in addition, this expectation were stationary, the combined location preference and moving cost terms could be recovered using $\beta + \gamma = \tilde{\beta}_g - (\tilde{\beta}_G - \tilde{\beta}_g)/\lambda$, and these composite parameters used to estimate the selfish migration probabilities $p(x, 0)$. Since this expectation is in general nonlinear, the projection of x_{g+1} onto x_g will introduce some approximation error. Note, however, that by this argument, omitting a time trend in the moving cost introduces no inconsistency as long as the preference specification includes a time trend, since g transitions linearly. (Note also that, even if the the projection error were negligible, a non-stationary transition function would imply that $(\tilde{\beta}_G - \tilde{\beta}_g)/\lambda \neq \Lambda' x_g \gamma$, rendering the estimated moving costs inconsistent.)

stant term is noticeably lower. For the low types, the estimated moving cost is prohibitive at roughly 7 (both costs are significantly different from zero). The fraction of high types is estimated at .29 and the likelihood ratio procedure described above yields a p-value of zero, easily rejecting the null that there is no unobserved heterogeneity.

The estimated effects of altruism on migration probabilities for this model are shown in Table 2.6. These effects are nearly identical to those estimated for the first specification. The average effects are between 4 and 6% (for generations one and two), with larger effects for the more educated and more fertile. Across subpopulations, the effects from the model with heterogeneity are larger, though not statistically significantly so. At between 13 and 15%, the average effects among the high types are substantially larger. The estimated effects of altruism are, evidently, not sensitive to specification of the role that unobserved heterogeneity plays in the utility function.

2.7.2 *The altruism parameter*

The value of $\lambda \approx .28$ that I assume is on the low end of those used in the literature on altruistic investment in children; Caponi (2011) assumes a value of about .31 while Heckman and Raut (2013) estimate this parameter at about .44. In order to assess the sensitivity of my parameter and effect estimates to the altruism parameter, I re-estimate the model using a value of $\lambda = .4$, an increase of over 40%. Because the estimated effects are similar under both specifications, I only estimate the model with pure location preference heterogeneity under this assumption. The coefficient estimates are displayed in the last two columns of Table 2.4. Regardless of whether permanent unobserved heterogeneity is included in the model, the coefficient estimates are very similar to those obtained using $\lambda \approx .28$. The exception to this pattern is that the estimated moving costs are lower (and the estimated constant terms are higher). This occurs because some of the observed migrations are undertaken only to unburden future generations of paying the costs of migrating, and a higher altruism parameter implies a lower cost threshold at which parents are willing to make this sacrifice.

The corresponding estimates of the effects of altruism are summarized in Table 2.7. Ultimately, assuming a higher altruism parameter causes a slight increase in the estimated effects of altruism. When this larger value of λ is used, the average effect increases by only .004 when heterogeneity is excluded and .001 when it is included. Within subpopulations, the differences are similarly small. These estimates show that my results are locally insensitive to the assumed level of intergenerational altruism (especially when allowing for permanent unobserved heterogeneity).

2.7.3 *A stationary, infinite-horizon model*

To underscore the role of time horizon assumptions and moving cost estimates in assessing the impact of altruism on migration, I present estimates in Table 2.8 of a stationary, infinite-horizon version of the model. Because, in this environment, moving costs cannot be estimated, this specification does not include a moving cost term. In the specification that excludes permanent unobserved heterogeneity, the estimated constant term is about 3, over twice its counterpart from the finite-horizon model summarized in Table 2.4. This difference arises because the constant term in the stationary model absorbs both location preferences and moving costs. Because I pool across generations to estimate the conditional choice probabilities for this model, the estimated coefficient on grades is smaller in absolute value, a reflection of the higher likelihood of migration among less-educated members of older generations. Unlike the finite-horizon case, the estimated fertility coefficient is negative, an artifact of dynamic selection bias introduced by the omission of a time trend in the location preference specification: the utility of living in the South is increasing in fertility (compared to the national median) and decreasing over time, so that successive generations of Southerners are simultaneously more fertile and more likely to migrate. When permanent heterogeneity is included in the model, the results are much the same. As expected, the constant term for the low types is much larger than when heterogeneity is excluded. It is lower, however, than the estimate from the finite-horizon model, presumably because using data on all generations to estimate the choice probabilities used in the value function representation reduces the influence of right-censoring among third generation observations. As in the finite-horizon model, the estimated coefficients on education and fertility are larger when unobserved heterogeneity is accounted for (although in this case the fertility coefficient remains negative, again a consequence of the stationarity assumption). With the exception of the fertility coefficients, the estimates from the stationary and finite-horizon models are broadly comparable.

The effects of altruism on migration depend on the moving costs, about which the estimates in Table 2.8 provide no guidance. Instead, Table 2.9 shows the implied effects of altruism when moving is costless. This hypothetical exercise illustrates the role of moving costs and unobserved heterogeneity in determining the dynamics of migration. In the model without unobserved heterogeneity, the estimated average effect of altruism is -15%. When migration is both costless and terminal, altruism decreases the likelihood of migrating because the only way to "invest" in future generations is to remain in the South to hedge against the possibility that they will be poorly-matched to the North. Including permanent unobserved heterogeneity substantially diminishes this effect by making location matches more persistent over generations. When moving is costly, the average effect of altruism combines this dynamic with the ability to migrate in order to spare future generations the cost of doing so.

2.8 POLICY ANALYSIS: SUBSIDIZED MIGRATION

A comparison of the estimated effects of altruism presented in Section 2.6 with the hypothetical effects under stationarity discussed in Section 2.7.3 reveals that moving costs are a central determinant of the role that altruism plays in the migration decision process. In this section, I study how a subsidy that offsets the cost of migration might affect welfare and immigrant flows. This analysis is instructive for several reasons. In the context of the Great Migration—and internal migration in general—a prolonged period of high migration flows suggests a role for policy to reduce the inefficiency associated with geographical disequilibrium in the labor market. I also analyze the effects of a subsidy that only offsets the cost of migration for future generations. As discussed in Section 2.2.1, altruism can impact migration behavior by encouraging members of the current generation to migrate at their own detriment in order to make future generations better off, a phenomenon I refer to as altruistic migration, or by preventing members of the current generation from making location decisions on the basis of idiosyncratic preferences that future generations are unlikely to share. When migrating terminates the dynamic decision problem, a subsidy that eliminates moving costs for future generations can only impact welfare and migration behavior by removing the incentive for altruistic migration. A comparison of the previously-discussed effects of altruism on migration to the effects of such a subsidy on migration enables me to identify the importance of altruistic migration in explaining the overall effect of intergenerational altruism on migration behavior. In addition to being interesting in its own right, understanding the importance of altruistic migration may also provide guidance to policymakers concerned about the intergenerational ramifications of changes to migration policy.

Table 2.10 summarizes the gains in expected indirect dynastic and current utility accruing to Southern-born individuals from a subsidy that reduces the cost of migrating to the North to zero. I calculate the welfare effects using the estimated parameters of the location-preference heterogeneity specification (Table 2.4).[18] The right panel displays the simulated welfare effects using parameters estimated without including permanent unobserved heterogeneity; the left displays effects after doing so. Within each panel, the first and second rows show the subsidy-induced increases in dynastic, and respectively, flow utility.[19] Ignoring heterogeneity, the increase in flow utility accounts for 60% of the aver-

18 With Type-I Extreme Value errors, indirect dynastic utility can be calculated using $EV_g(x,\tau) = -c + \gamma - \log p_g(x,\tau)$ where $p_g(x,\tau)$ is the probability of migrating and $\gamma \approx .5772$ is Euler's constant, and indirect flow utility can be calculated as

$$EU_g(x,\tau) = [1 - p_g(x,\tau)]\{(\tau,x)'\beta^\tau + \gamma - \log[1 - p_g(x,\tau)]\} + p_g(x,\tau)[-c + \gamma - \log p_g(x,\tau)].$$

These expressions use finite dependence and the property of the Type-I Extreme Value distribution that $E(\epsilon_n | v_n + \epsilon_n > v_s + \epsilon_s) = \gamma - \log p(n)$.

19 N.b.: these effects, which I calculate under the normalization that the utility of residing in the North is zero, have no cardinal interpretation.

age total gain to members of the first generation and over 70% of the gain to members of the second (and, by construction, 100% of the gain for the third). In subpopulations with high pre-subsidy migration probabilities (high education and low fertility), flow utility accounts for a greater share of the total welfare gains due to the subsidy. This occurs because children belonging to these subpopulation are likely to enjoy the benefits of residing in the North (without incurring the moving cost) even absent the subsidy, which therefore primarily benefits their parents.

As the left panel shows, these relationships persist when allowing for unobserved heterogeneity, with flow utility accounting for over 60% of the total average gain from the subsidy for all generations and an even greater fraction among subpopulations already more likely to migrate. However, including unobserved heterogeneity substantially decreases both the dynastic and flow utility gains due the subsidy, although it does so proportionally, preserving the share of the total welfare increase accruing to flow utility. The reason for this estimated decrease is that the subsidy only benefits those who prefer the North to the South. Under the parameter estimates from the model without heterogeneity, the typical Southerner has only a modest preference for living in the South, so remitting the cost of moving results in substantial utility gains for a large number of migrants and would-be migrants. In contrast, under the model with heterogeneity, two-thirds of the Southern-born population (the low types) have such large tastes for the South that they would never migrate, even if doing so were costless; this group receives no benefit from subsidized migration. At the same time, the remaining third of Southerners (the high types) have a high likelihood of migration even before the subsidy, placing them on a relatively shallow portion of the distribution function that relates location preferences to migration probabilities. Since expected indirect utility is convex and decreasing in the migration probability, accounting for unobserved heterogeneity also decreases the welfare gain due to the subsidy for high types relative to the model that ignores it.

In order to decompose the current-utility gains induced by the subsidy, I also compute the welfare effects of a notional policy under which moving costs are unchanged for the current generation, but future generations can migrate at no cost.[20] Because this policy does not affect the current generation's migration cost, the only way that the subsidy can influence current utility is through its effect on the probability that members of the current generation migrate in order to benefit future generations. Furthermore, because this policy presents future generations with precisely the same incentives as the policy that subsidizes all migrations, the differential effects of these two policies isolates the contribution of altruistic migration to the impact of the subsidy to all generations on

20 That is, for each generation, I (i) calculate relative conditional valuation functions $\hat{v}_g(x,\tau) = (\tau, x)'\beta^\tau + c - [1 + (\mu_g - 1) f_g]\lambda E[\log \tilde{p}(x_{g+1}, \tau)|x_g, \tau]$ using future choice probabilities $\tilde{p}_{g+1}(x,\tau)$ obtained under the assumption that migration is costless, (ii) use these valuation functions to estimate current-generation choice probabilities $\hat{p}_g(x,\tau) = [1 + \exp(\hat{v}_g(x,\tau))]^{-1}$ (under the assumption that the current generation does face migration costs), and (iii) use these choice probabilities to calculate current-period expected utility according to the expression given in footnote 18.

current-generation welfare. The third columns of the left and right panels of Table 2.10 present the results of this decomposition. Considering first the results when heterogeneity is ignored, it seems that altruistic migration does not contribute much to the welfare effects of the subsidy. On average, offering future generations a subsidy only increases flow utility by about 1% as much as subsidizing current and future migration. Including unobserved heterogeneity increases this figure, but not by much; the average increases to about 2%. As the left panel shows, the current-generation welfare gains from subsidizing future migration accrue entirely to members of the high type—the only group with the ability to forecast whether future generations will fare better in the North—among whom flow utility can account for between 6 and 7% of the welfare increase from subsidizing all migration. Reflecting the fact that they have more children upon whom to bestow the benefits of migration, the contribution of altruistic migration to the current-utility effects of the subsidy are largest among those with above-median fertility; in this population, eliminating the incentive to altruistically migrate accounts for between 11 and 17% of the current-generation welfare increase from subsidizing all generations' moving costs. Ultimately, the reason that the subsidy to future generations has such a small impact on flow utility (and, as I show below, the probability of migration) is that the discounted future benefit of migrating is simply not large enough relative to the cost of doing so to induce much migration, regardless of whether unobserved heterogeneity is included.

Of course, migration policy is often designed to target the amount of migration and not migrants' welfare. To this end, I summarize in Table 2.11 the impact of these subsidies on the probability of migrating. The left panel accounts for heterogeneity, the right does not; in each panel, the first column shows the effect of subsidizing migration for all generations and the second shows the effect of subsidizing only future generations' migration costs. When heterogeneity is ignored, the apparent effects of the permanent subsidy on the propensity to migrate are quite large, with the average probability increasing by between 38 and 56%. When these effects are calculated using parameter estimates that take permanent unobservable heterogeneity into account, the mean effects are much smaller, on the order of 9-12%. The left panel of the table shows that the effect of subsidizing migration for all generations is as high as 35% for high types, and that the subsidy has virtually no effect for low types. The differences between these estimates highlight the impact of ignoring unobserved heterogeneity. While models without heterogeneity approximate the effect of altruism reasonably well, the inconsistent parameter estimates from these models can perform poorly in other counterfactual exercises. Including unobserved heterogeneity changes the simulated impacts of the subsidy to all generations more than the subsidy to future generations because the former subsidy causes a much greater increase in the relative utility of migrating. Since the probability of migrating is a nonlinear function of the utility of doing so, for large utility increases, the resulting change in the probability of migrating given "averaged" parameters is a worse approximation to the average change given the actual parameters. The average effect of subsidizing future generations' migration costs is to decrease migration by about 5%, although the simulations that allow for heterogeneity show that this average is driven almost entirely by high types, among whom the subsidy decreases the probability of migrating by between 14 and 17%. As the hypothetical effects of altruism when migration is costless (Table 2.9) show, parents who are idiosyncratically well-suited to the North can also benefit their progeny by choosing not to migrate, since future generations are unlikely to share their tastes for the North. Since, when the North is an absorbing state, the incentive to avoid migrating on this basis remains, such parents are unaffected by a policy that only rebates future generations' moving costs. At the same time, since this subsidy leaves the flow utility of migrating unchanged, any effect of subsidizing the cost of moving for future generations must be due to altruistic migration. The estimated effects of this policy change are very similar to the estimated effects of intergenerational altruism on migration presented in Table 2.5, implying that altruistic migration is overwhelmingly the primary channel through which intergenerational altruism affects migration behavior.

2.9 IDENTIFYING THE UNOBSERVED TYPE DISTRIBUTION

As the hypothesis tests discussed in Section 2.6 demonstrate, allowing for permanent unobserved heterogeneity greatly increases the ability of empirical models to explain location choices over time. Because unobserved heterogeneity appears to feature prominently in the data-generating process, the question of how the distribution of this heterogeneity is uncovered from variation in the data becomes important.

The distribution of intergenerational location sequences $l_d = (l_{d1}, l_{d2}, l_{d3})$ is shown in Table 2.12 (after removing the small number of families with North-South migrations). Only about 35% of families ever leave the South, a fraction that corresponds with estimates of the proportion π of high types obtained from the empirical models presented above (which range between .29 and .36). This correspondence suggests that the location sequences are more influental than the other observables in identifying the distribution of the unobserved types.

To explore the different roles played by the observables, I present in Table 2.12 regressions of the estimated posterior probabilities $q(\tau_d = 1 | x_d, l_d; \hat{\theta})$ of being a high-migration-probability type on family-level observable characteristics (I estimate these regressions using probabilities obtained from both the location-preference and moving-cost heterogeneity specifications, although the results are similar for both). Unconditional on other observables, the estimated probability of being a high type declines when when some generations are more fertile and increases when they are more highly educated. These unconditional relationships are intuitive, although they are quite weak, with small estimated coefficients and low explanatory power (this is partially due to the misspecified linear functional form, which I use for simplicity).[21] When the estimated probabilities are regressed on a set of indicators for the location sequences l_d, the R^2 for the regressions increases to about .88; these sequences are capable of explaining about 88% of the likelihood that dynasties belong to either type, with the remaining variation explained by the observable characteristics included in the empirical models. Predictably, the probability of being a high type is decreasing in the number of generations the dynasty remains in the South. When education and fertility are added to this regression, they are more weakly related to the type probability than when considered unconditionally, with the coefficient on education actually changing sign. This counterintuitive sign change occurs because, although education is positively correlated with being of the high type, it also increases the utility of living in the North. Thus, better-educated families who remain long in the South are less likely to be high types (which explains why they do not migrate despite their high education), as are better-educated families who leave for the North (because their high education makes them more likely to migrate even if they are of the low type).

The high explanatory power of the location sequences reflects the relative magnitudes of the coefficients on the observed covariates and unobserved types; although the observable covariates influence location preferences, among low types this influence is dominated by that of the permanent location-preference or moving cost components, making them unlikely to migrate regardless of their covariates. Note that this does not imply that unobserved heterogeneity does not affect the parameters estimated from models that ignore it (the R^2 from a regression of the estimated type probabilities on a set of indicators for all generations' observable states is about .4, suggesting a strong correlation between the observed and unobserved preference components).[22]

2.10 CONCLUSION

In order to provide direct evidence on the impact of intergenerational altruism on migration, I use data from the Great Migration to estimate a dynastic model of residential choice, arguing that this migration episode can be viewed as a case study for migration in general. I find that altruism makes the average Southerner between 4 and 6 percent more likely to migrate. My estimates also imply that a large proportion of Southern dynasties have overriding preferences for remaining in the South, making them unlikely to migrate regardless of their concern for future generations; altruism has no impact on

21 The reported standard errors should be interpreted with care. Because they have not been adjusted to account for the fact that the posterior probabilities are estimated, the only source of "uncertainty" in these regressions is specification error.

22 In the limiting case where the covariates explained none of the variation in the type probabilities conditional on the location sequences (i.e. if the location fixed effect for low types was infinite), the preference parameters for the low types could not be identified (their migration probabilities would be exactly zero). Preferences for the high types, and therefore the effects of altruism, could still be recovered by estimating the model (without heterogeneity) on the sample of migrating families, weighting each family by its posterior probability of belonging to the high type.

members of these dynasties. In contrast, among those who would actually consider migrating, the impact of altruism is between 12 and 16%. The evident effect of altruism on migration depends crucially on the population over which the average is taken, with the correct population depending on the question being asked. For example, policymakers concerned about the response of migrant flows to a policy change that would make the children of potential migrants better off might be more interested in the effect of altruism among all members of a source country while social scientists seeking to understand the determinants of migration behavior may only be interested in the impact of altruism among those who might plausibly choose to migrate.

My estimates are robust several potential types of misspecification. Although, in a finite-horizon setting, heterogeneity in pure location preferences is distinct from heterogeneity in moving costs, I obtain similar estimates under either assumption. That parents exhibit altruism towards future generations has been established by previous research, and my effect estimates are locally insensitive to the assumed rate of intergenerational altruism. While moving costs play an integral role in determining the importance of altruism in the decision to migrate, it is difficult to disentangle these costs from pure location preferences when return migration is rare. However, the influence of the failure of these assumptions on the estimated impacts of altruism can be signed, in which case the estimates can be interpreted as bounding the actual impact.

I simulate the effects of a hypothetical subsidy that eliminates the cost of migrating. In addition to making future generations better off, such a subsidy increases direct utility by making those better-suited for the North more likely to migrate and by obviating the need for altruistic migration—migrations undertaken to increase future generations' wellbeing to the detriment of the current generation. By comparing this policy to one that subsidizes only future generations' moving costs, I decompose the direct utility impact of the original subsidy, finding that the role of altruistic migration is comparatively small. I also simulate the effects of these policies on the propensity to migrate, finding that altruistic migration, which decreases by about five percent under either subsidy, is the primary channel through which intergenerational altruism impacts migration behavior.

2.A APPENDIX TABLES

See Table 2.14 below.

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TABLES

Table 2.1: Descriptive statistics

	Generation	N	Mean	Median	Standard deviation	Min	Max
Age	1	443	70.48	70	9.05	49	96
	2	400	46.55	47	8.18	28	75
	3	256	25.49	24.5	5.80	18	53
	All	1099	51.29	51	19.42	18	96
Birth year	1	443	1909.40	1910	9.04	1883	1931
	2	400	1933.18	1933	8.08	1906	1951
	3	256	1954.19	1955	5.80	1927	1963
	All	1099	1928.49	1929	19.33	1883	1963
Male	1	443	0.28	0	0.45	0	1
	2	400	0.29	0	0.45	0	1
	3	256	0.43	0	0.50	0	1
	All	1099	0.32	0	0.47	0	1
South	1	443	0.89	1	0.32	0	1
	2	400	0.82	1	0.38	0	1
	3	256	0.88	1	0.32	0	1
	All	1099	0.86	1	0.35	0	1
Grades	1	443	6.81	7	3.36	0	17
	2	400	10.46	11	3.18	0	17
	3	256	12.54	12	2.15	5	17
	All	1099	9.47	10	3.83	0	17
Kids	1	443	6.33	6	3.97	0	23
	2	400	4.77	4	3.02	0	18
	3	256	1.11	1	1.36	0	7
	All	1099	4.55	4	3.76	0	23

Table 2.2: Migration probabilities

	(1)	(2)	(3)
Male	0.0768	0.0894	0.101
	(0.0208)	(0.0346)	(0.0349)
Education	0.126	0.193	0.222
	(0.0278)	(0.0875)	(0.118)
Fertility	0.0760	0.0441	0.0387
	(0.0171)	(0.0274)	(0.0273)
Male*Education			-0.156
			(0.189)
Education*Fertility			0.739
			(0.373)
Male*Education*Fertility			-0.944
			(0.533)
Male*2nd gen.		-0.0159	0.0846
		(0.0498)	(0.0594)
Male*3rd gen.		-0.0213	0.0358
		(0.0510)	(0.0637)
Education*2nd gen.		-0.000473	0.0210
		(0.0985)	(0.131)
Education*3rd gen.		-0.125	-0.153
		(0.0965)	(0.135)
Fertility*2nd gen.		0.0810	0.142
		(0.0398)	(0.0427)
Fertility*3rd gen.		0.0235	0.0903
		(0.0438)	(0.0525)
Male*Education*2nd gen.			0.0135
			(0.225)
Male*Education*3rd gen.			0.118
			(0.220)
Male*Fertility*2nd gen.			-0.271
			(0.0797)
Male*Fertility*3rd gen.			-0.232
			(0.0949)
Education*Fertility*2nd gen.			-0.863
			(0.395)
Education*Fertility*3rd gen.			-0.804
			(0.387)
Male*Education*Fertility*2nd gen.			0.873
			(0.583)
Male*Education*Fertility*3rd gen.			1.099
			(0.563)
Observations	1,099	1,099	1,099
R-squared	0.078	0.088	0.114

Notes—Dependent variable is an indicator for migrating North. Coefficients estimated by OLS.

Table 2.3: Average transition matrices

	Education in g+1								
Education in g	1	2	3	4	5	6	7	8	9
1	0.005	0.143	0.115	0.171	0.156	0.191	0.173	0.030	0.015
	(0.004)	(0.058)	(0.040)	(0.043)	(0.030)	(0.033)	(0.028)	(0.011)	(0.006)
2	0.006	0.065	0.069	0.146	0.169	0.231	0.243	0.046	0.025
	(0.004)	(0.022)	(0.020)	(0.029)	(0.024)	(0.027)	(0.026)	(0.012)	(0.008)
3	0.005	0.025	0.038	0.111	0.167	0.250	0.303	0.063	0.037
	(0.003)	(0.008)	(0.010)	(0.017)	(0.018)	(0.023)	(0.023)	(0.012)	(0.009)
4	0.004	0.009	0.021	0.078	0.157	0.252	0.347	0.080	0.052
	(0.003)	(0.004)	(0.006)	(0.010)	(0.014)	(0.018)	(0.019)	(0.012)	(0.010)
5	0.003	0.003	0.011	0.052	0.145	0.245	0.377	0.096	0.069
	(0.002)	(0.002)	(0.004)	(0.008)	(0.012)	(0.015)	(0.016)	(0.011)	(0.010)
6	0.002	0.001	0.006	0.034	0.133	0.232	0.392	0.112	0.089
	(0.002)	(0.001)	(0.003)	(0.007)	(0.012)	(0.015)	(0.017)	(0.012)	(0.010)
7	0.002	0.000	0.003	0.021	0.122	0.216	0.402	0.125	0.109
	(0.001)	(0.000)	(0.002)	(0.006)	(0.014)	(0.018)	(0.022)	(0.015)	(0.013)
8	0.001	0.000	0.002	0.013	0.111	0.199	0.396	0.140	0.137
	(0.001)	(0.000)	(0.002)	(0.005)	(0.016)	(0.022)	(0.030)	(0.021)	(0.022)
9	0.001	0.000	0.001	0.008	0.101	0.183	0.385	0.154	0.168
	(0.001)	(0.000)	(0.002)	(0.004)	(0.020)	(0.027)	(0.040)	(0.031)	(0.037)

(a) Education

	Fertility in g+1	
Fertility in g	Low	High
Low	0.674	0.326
	(0.019)	(0.019)
High	0.558	0.442
	(0.025)	(0.025)

(b) Fertility

Notes—Standard errors based on 100 nonparametric bootstrap replications.

Table 2.4: Parameter estimates

		Permanent unobserved het.		$\lambda = .4$	
	No het.	In preferences	In moving costs	No het.	Preference het.
Constant	1.35	0.79	0.51	1.52	1.18
	(1.00)	(1.63)	(1.57)	(0.96)	(1.49)
Constant (Type 2)		8.68			8.94
		(2.64)			(2.46)
Grades	-0.61	-0.28	-0.12	-0.58	-0.26
	(0.33)	(0.58)	(0.53)	(0.33)	(0.57)
Grades^2	0.03	0.00	-0.02	0.03	0.00
	(0.03)	(0.06)	(0.05)	(0.03)	(0.06)
Fertility	0.54	0.85	0.95	0.56	0.86
	(0.30)	(0.57)	(0.60)	(0.30)	(0.58)
Male	0.08	0.22	0.11	0.08	0.22
	(0.22)	(0.42)	(0.37)	(0.22)	(0.41)
Generation	0.05	-0.51	-0.56	0.01	-0.53
	(0.16)	(0.37)	(0.38)	(0.16)	(0.36)
Moving cost	2.77	2.11	1.93	2.57	1.67
	(0.59)	(1.05)	(1.17)	(0.43)	(0.77)
Moving cost (Type 2)			7.04		
			(2.06)		
π		0.35	0.29		0.35
		(0.04)	(0.05)		(0.04)
Wald	9138.29	6792.56	8801.49	6411.07	8984.02
Pr(Wald)	0.00	0.00	0.00	0.00	0.00
LR		78256.05	80533.06		
Pr(LR)		0.00	0.00		

Notes—Standard errors based on 250 nonparametric bootstrap replications. Wald statistic is for the hypothesis that the observables are jointly insignificant. LR tests the model with heterogeneity against the one without.

Table 2.5: Migration probabilities (location preference heterogeneity)

	With heterogeneity			Without heterogeneity			
	Altruistic	Selfish	Diff.	Altruistic	Selfish	Diff.	Diff. in diff.
All, 1st gen.	0.112	0.069	0.043	0.122	0.086	0.036	0.007
	(0.015)	(0.027)	(0.024)	(0.015)	(0.024)	(0.020)	(0.010)
All, 2nd gen.	0.195	0.140	0.055	0.161	0.123	0.038	0.017
	(0.021)	(0.025)	(0.030)	(0.018)	(0.021)	(0.027)	(0.013)
All, 3rd gen.	0.197	0.197	0.000	0.131	0.131	0.000	0.000
	(0.030)	(0.030)	(0.000)	(0.019)	(0.019)	(0.000)	(0.000)
All type 1, 1st gen.	0.318	0.196	0.122				
	(0.050)	(0.079)	(0.074)				
All type 1, 2nd gen.	0.554	0.397	0.157				
	(0.084)	(0.083)	(0.091)				
All type 1, 3rd gen.	0.559	0.559	0.000				
	(0.111)	(0.111)	(0.000)				
All type 2, 1st gen.	0.000	0.000	-0.000				
	(0.001)	(0.002)	(0.001)				
All type 2, 2nd gen.	0.000	0.000	-0.000				
	(0.005)	(0.006)	(0.002)				
All type 2, 3rd gen.	0.001	0.001	0.000				
	(0.013)	(0.013)	(0.000)				
≥12 years, 1st gen.	0.179	0.126	0.053	0.213	0.156	0.057	-0.004
	(0.027)	(0.036)	(0.029)	(0.031)	(0.039)	(0.030)	(0.013)
≥12 years, 2nd gen.	0.223	0.166	0.056	0.191	0.147	0.044	0.012
	(0.023)	(0.026)	(0.030)	(0.021)	(0.024)	(0.031)	(0.014)
≥12 years, 3rd gen.	0.202	0.202	0.000	0.135	0.135	0.000	0.000
	(0.030)	(0.030)	(0.000)	(0.019)	(0.019)	(0.000)	(0.000)
<12 years, 1st gen.	0.094	0.054	0.040	0.098	0.068	0.030	0.010
	(0.015)	(0.026)	(0.023)	(0.014)	(0.021)	(0.018)	(0.010)
<12 years, 2nd gen.	0.140	0.087	0.052	0.101	0.076	0.025	0.027
	(0.026)	(0.029)	(0.030)	(0.017)	(0.018)	(0.021)	(0.013)
<12 years, 3rd gen.	0.108	0.108	0.000	0.065	0.065	0.000	0.000
	(0.045)	(0.045)	(0.000)	(0.020)	(0.020)	(0.000)	(0.000)
High fertility, 1st gen.	0.100	0.040	0.060	0.105	0.057	0.048	0.012
	(0.018)	(0.031)	(0.033)	(0.016)	(0.025)	(0.027)	(0.016)
High fertility, 2nd gen.	0.175	0.092	0.082	0.133	0.084	0.049	0.033
	(0.024)	(0.038)	(0.044)	(0.022)	(0.027)	(0.036)	(0.019)
High fertility, 3rd gen.	0.161	0.161	0.000	0.100	0.100	0.000	0.000
	(0.039)	(0.039)	(0.000)	(0.025)	(0.025)	(0.000)	(0.000)
Low fertility, 1st gen.	0.122	0.100	0.022	0.136	0.116	0.020	0.002
	(0.019)	(0.024)	(0.013)	(0.020)	(0.023)	(0.011)	(0.004)
Low fertility, 2nd gen.	0.210	0.186	0.024	0.182	0.161	0.021	0.003
	(0.023)	(0.022)	(0.013)	(0.021)	(0.018)	(0.015)	(0.007)
Low fertility, 3rd gen.	0.237	0.237	0.000	0.165	0.165	0.000	0.000
	(0.034)	(0.034)	(0.000)	(0.023)	(0.023)	(0.000)	(0.000)

Notes—Standard errors based on 250 nonparametric bootstrap replications.

Table 2.6: Migration probabilities (moving cost heterogeneity)

	With heterogeneity			Without heterogeneity			
	Altruistic	Selfish	Diff.	Altruistic	Selfish	Diff.	Diff. in diff.
All, 1st gen.	0.112	0.070	0.042	0.122	0.086	0.036	0.006
	(0.015)	(0.028)	(0.026)	(0.015)	(0.024)	(0.020)	(0.022)
All, 2nd gen.	0.198	0.143	0.055	0.161	0.123	0.038	0.017
	(0.021)	(0.026)	(0.030)	(0.018)	(0.021)	(0.027)	(0.018)
All, 3rd gen.	0.204	0.204	0.000	0.131	0.131	0.000	0.000
	(0.029)	(0.029)	(0.000)	(0.019)	(0.019)	(0.000)	(0.000)
All type 1, 1st gen.	0.359	0.233	0.126				
	(0.058)	(0.113)	(0.088)				
All type 1, 2nd gen.	0.622	0.469	0.153				
	(0.085)	(0.114)	(0.099)				
All type 1, 3rd gen.	0.656	0.656	0.000				
	(0.120)	(0.120)	(0.000)				
All type 2, 1st gen.	0.009	0.002	0.007				
	(0.009)	(0.004)	(0.006)				
All type 2, 2nd gen.	0.022	0.007	0.015				
	(0.019)	(0.008)	(0.012)				
All type 2, 3rd gen.	0.016	0.016	0.000				
	(0.016)	(0.016)	(0.000)				
≥12 years, 1st gen.	0.184	0.131	0.053	0.213	0.156	0.057	-0.004
	(0.027)	(0.035)	(0.030)	(0.031)	(0.039)	(0.030)	(0.019)
≥12 years, 2nd gen.	0.229	0.171	0.057	0.191	0.147	0.044	0.013
	(0.023)	(0.026)	(0.030)	(0.021)	(0.024)	(0.031)	(0.018)
≥12 years, 3rd gen.	0.210	0.210	0.000	0.135	0.135	0.000	0.000
	(0.029)	(0.029)	(0.000)	(0.019)	(0.019)	(0.000)	(0.000)
<12 years, 1st gen.	0.093	0.054	0.039	0.098	0.068	0.030	0.009
	(0.014)	(0.028)	(0.025)	(0.014)	(0.021)	(0.018)	(0.023)
<12 years, 2nd gen.	0.139	0.088	0.051	0.101	0.076	0.025	0.025
	(0.026)	(0.031)	(0.031)	(0.017)	(0.018)	(0.021)	(0.022)
<12 years, 3rd gen.	0.106	0.106	0.000	0.065	0.065	0.000	0.000
	(0.046)	(0.046)	(0.000)	(0.020)	(0.020)	(0.000)	(0.000)
High fertility, 1st gen.	0.097	0.038	0.059	0.105	0.057	0.048	0.011
	(0.017)	(0.034)	(0.036)	(0.016)	(0.025)	(0.027)	(0.032)
High fertility, 2nd gen.	0.176	0.094	0.083	0.133	0.084	0.049	0.033
	(0.025)	(0.041)	(0.045)	(0.022)	(0.027)	(0.036)	(0.026)
High fertility, 3rd gen.	0.168	0.168	0.000	0.100	0.100	0.000	0.000
	(0.039)	(0.039)	(0.000)	(0.025)	(0.025)	(0.000)	(0.000)
Low fertility, 1st gen.	0.123	0.102	0.021	0.136	0.116	0.020	0.001
	(0.019)	(0.024)	(0.013)	(0.020)	(0.023)	(0.011)	(0.009)
Low fertility, 2nd gen.	0.215	0.191	0.024	0.182	0.161	0.021	0.003
	(0.024)	(0.021)	(0.013)	(0.021)	(0.018)	(0.015)	(0.009)
Low fertility, 3rd gen.	0.244	0.244	0.000	0.165	0.165	0.000	0.000
	(0.030)	(0.030)	(0.000)	(0.023)	(0.023)	(0.000)	(0.000)

Notes—Standard errors based on 250 nonparametric bootstrap replications.

Table 2.7: Migration probabilities ($\lambda = .4$, location preference heterogeneity)

	With heterogeneity			Without heterogeneity			
	Altruistic	Selfish	Diff.	Altruistic	Selfish	Diff.	Diff. in diff.
All, 1st gen.	0.112	0.068	0.044	0.123	0.083	0.040	0.004
	(0.015)	(0.028)	(0.025)	(0.015)	(0.023)	(0.020)	(0.011)
All, 2nd gen.	0.195	0.139	0.056	0.160	0.121	0.039	0.016
	(0.021)	(0.026)	(0.030)	(0.018)	(0.021)	(0.027)	(0.014)
All, 3rd gen.	0.197	0.197	0.000	0.132	0.132	0.000	0.000
	(0.030)	(0.030)	(0.000)	(0.019)	(0.019)	(0.000)	(0.000)
All type 1, 1st gen.	0.319	0.194	0.124				
	(0.048)	(0.082)	(0.078)				
All type 1, 2nd gen.	0.554	0.395	0.159				
	(0.083)	(0.085)	(0.092)				
All type 1, 3rd gen.	0.559	0.559	0.000				
	(0.110)	(0.110)	(0.000)				
All type 2, 1st gen.	0.000	0.000	-0.000				
	(0.001)	(0.002)	(0.001)				
All type 2, 2nd gen.	0.000	0.000	-0.000				
	(0.003)	(0.005)	(0.003)				
All type 2, 3rd gen.	0.001	0.001	0.000				
	(0.012)	(0.012)	(0.000)				
≥12 years, 1st gen.	0.179	0.124	0.055	0.213	0.148	0.065	-0.010
	(0.027)	(0.036)	(0.031)	(0.032)	(0.037)	(0.030)	(0.015)
≥12 years, 2nd gen.	0.223	0.165	0.058	0.190	0.143	0.046	0.011
	(0.023)	(0.027)	(0.031)	(0.021)	(0.024)	(0.030)	(0.015)
≥12 years, 3rd gen.	0.202	0.202	0.000	0.136	0.136	0.000	0.000
	(0.030)	(0.030)	(0.000)	(0.019)	(0.019)	(0.000)	(0.000)
<12 years, 1st gen.	0.094	0.054	0.041	0.099	0.066	0.033	0.008
	(0.015)	(0.027)	(0.024)	(0.014)	(0.020)	(0.018)	(0.011)
<12 years, 2nd gen.	0.139	0.087	0.052	0.101	0.076	0.025	0.027
	(0.026)	(0.030)	(0.030)	(0.018)	(0.018)	(0.021)	(0.014)
<12 years, 3rd gen.	0.109	0.109	0.000	0.066	0.066	0.000	0.000
	(0.045)	(0.045)	(0.000)	(0.020)	(0.020)	(0.000)	(0.000)
High fertility, 1st gen.	0.100	0.039	0.061	0.106	0.054	0.052	0.009
	(0.018)	(0.033)	(0.035)	(0.017)	(0.024)	(0.027)	(0.017)
High fertility, 2nd gen.	0.174	0.092	0.083	0.132	0.081	0.050	0.032
	(0.024)	(0.039)	(0.044)	(0.022)	(0.026)	(0.036)	(0.020)
High fertility, 3rd gen.	0.161	0.161	0.000	0.100	0.100	0.000	0.000
	(0.039)	(0.039)	(0.000)	(0.025)	(0.025)	(0.000)	(0.000)
Low fertility, 1st gen.	0.122	0.099	0.022	0.135	0.113	0.022	0.000
	(0.019)	(0.024)	(0.013)	(0.020)	(0.022)	(0.011)	(0.005)
Low fertility, 2nd gen.	0.210	0.186	0.025	0.181	0.159	0.022	0.002
	(0.023)	(0.022)	(0.014)	(0.021)	(0.018)	(0.015)	(0.008)
Low fertility, 3rd gen.	0.237	0.237	0.000	0.167	0.167	0.000	0.000
	(0.033)	(0.033)	(0.000)	(0.022)	(0.022)	(0.000)	(0.000)

Notes—Standard errors based on 250 nonparametric bootstrap replications.

Table 2.8: Stationary specification estimates

	With heterogeneity	Without heterogeneity
Constant	1.68	2.99
	(1.33)	(0.88)
Constant (Type 2)	6.68	
	(1.48)	
Grades	-0.27	-0.49
	(0.51)	(0.32)
Grades^2	-0.01	0.03
	(0.05)	(0.03)
Fertility	-0.15	-0.61
	(0.29)	(0.18)
Male	0.14	0.07
	(0.36)	(0.21)
π	0.36	
	(0.04)	
Wald	7968.29	10923.55
Pr(Wald)	0.00	0.00

Notes—Standard errors based on 250 nonparametric bootstrap replications. Wald statistic is for the hypothesis that the observables are jointly insignificant.

Table 2.9: Migration probabilities (stationary)

	With het.			Without het.			
	Altruistic	Selfish	Diff.	Altruistic	Selfish	Diff.	Diff. in diff.
All	0.161	0.195	-0.034	0.138	0.290	-0.152	0.118
	(0.161)	(0.195)	(-0.034)	(0.012)	(0.013)	(0.008)	(0.020)
All, type 1	0.452	0.530	-0.078				
	(0.452)	(0.530)	(-0.078)				
All, type 2	0.001	0.009	-0.009				
	(0.001)	(0.009)	(-0.009)				
≥12 years	0.207	0.241	-0.033	0.174	0.341	-0.166	0.133
	(0.207)	(0.241)	(-0.033)	(0.017)	(0.016)	(0.007)	(0.017)
<12 years	0.105	0.139	-0.034	0.095	0.229	-0.134	0.100
	(0.105)	(0.139)	(-0.034)	(0.013)	(0.023)	(0.013)	(0.024)
High fertility	0.144	0.194	-0.049	0.113	0.347	-0.234	0.185
	(0.144)	(0.194)	(-0.049)	(0.015)	(0.027)	(0.016)	(0.033)
Low fertility	0.175	0.188	-0.013	0.159	0.212	-0.052	0.039
	(0.175)	(0.188)	(-0.013)	(0.017)	(0.018)	(0.004)	(0.008)

Notes—Standard errors based on 250 nonparametric bootstrap replications.

Table 2.10: Welfare gains due to subsidized migration

	With heterogeneity			Without heterogeneity		
	Dynastic	Flow	Flow (future subsidy)	Dynastic	Flow	Flow (future subsidy)
All, 1st gen.	0.517	0.312	0.021	1.293	0.780	0.008
All, 2nd gen.	0.604	0.484	0.024	1.377	1.008	0.006
All, 3rd gen.	0.565	0.565	0.000	1.064	1.064	0.000
All type 1, 1st gen.	1.467	0.888	0.059			
All type 1, 2nd gen.	1.715	1.375	0.069			
All type 1, 3rd gen.	1.600	1.600	0.000			
All type 2, 1st gen.	0.001	0.000	0.000			
All type 2, 2nd gen.	0.002	0.001	0.000			
All type 2, 3rd gen.	0.004	0.004	0.000			
$\geq$12 years, 1st gen.	0.586	0.465	0.023	1.574	1.197	0.013
$\geq$12 years, 2nd gen.	0.634	0.539	0.024	1.485	1.147	0.008
$\geq$12 years, 3rd gen.	0.575	0.575	0.000	1.088	1.088	0.000
<12 years, 1st gen.	0.498	0.272	0.020	1.219	0.669	0.006
<12 years, 2nd gen.	0.545	0.375	0.024	1.161	0.733	0.004
<12 years, 3rd gen.	0.411	0.411	0.000	0.678	0.678	0.000
High fertility, 1st gen.	0.574	0.241	0.039	1.423	0.590	0.014
High fertility, 2nd gen.	0.610	0.411	0.046	1.387	0.795	0.011
High fertility, 3rd gen.	0.511	0.511	0.000	0.909	0.909	0.000
Low fertility, 1st gen.	0.471	0.368	0.006	1.192	0.928	0.003
Low fertility, 2nd gen.	0.600	0.539	0.007	1.369	1.170	0.003
Low fertility, 3rd gen.	0.625	0.625	0.000	1.237	1.237	0.000

Notes—“Dynastic” refers to the gain in dynastic indirect utility when the moving cost is subsidized, “Flow” refers to the gain in current-period utility under the subsidy, and “Flow (future subsidy)” refers to the gain in current-period utility when the subsidy is only applied to future generations.

Table 2.11: Migration probability differences due to subsidized migration

	With heterogeneity		Without heterogeneity	
	Subsidy	Future subsidy	Subsidy	Future subsidy
All, 1st gen.	0.095	-0.047	0.375	-0.050
All, 2nd gen.	0.088	-0.059	0.458	-0.053
All, 3rd gen.	0.124	0.000	0.561	0.000
All type 1, 1st gen.	0.271	-0.135		
All type 1, 2nd gen.	0.249	-0.167		
All type 1, 3rd gen.	0.345	0.000		
All type 2, 1st gen.	0.000	0.000		
All type 2, 2nd gen.	0.000	0.000		
All type 2, 3rd gen.	0.004	0.000		
≥12 years, 1st gen.	0.100	-0.058	0.489	-0.076
≥12 years, 2nd gen.	0.080	-0.060	0.494	-0.061
≥12 years, 3rd gen.	0.121	0.000	0.568	0.000
<12 years, 1st gen.	0.094	-0.045	0.345	-0.043
<12 years, 2nd gen.	0.102	-0.056	0.386	-0.038
<12 years, 3rd gen.	0.168	0.000	0.459	0.000
High fertility, 1st gen.	0.056	-0.066	0.264	-0.066
High fertility, 2nd gen.	0.073	-0.088	0.369	-0.070
High fertility, 3rd gen.	0.146	0.000	0.535	0.000
Low fertility, 1st gen.	0.127	-0.033	0.462	-0.038
Low fertility, 2nd gen.	0.099	-0.037	0.526	-0.041
Low fertility, 3rd gen.	0.099	0.000	0.591	0.000

Notes—"Subsidy" refers to the change in the probability of migrating when all generations' moving costs are subsidized. "Future subsidy" refers to this change when only future generations' moving costs are subsidized.

Table 2.12: Location sequences

	Number	Fraction
$l = (n, n, n)$	41	0.096
$l = (s, n, n)$	65	0.152
$l = (s, s, n)$	44	0.103
$l = (s, s, s)$	277	0.649
Total	427	1

Table 2.13: Determinants of $q(\tau|l, x)$

	Preference heterogeneity				Moving cost heterogeneity			
Constant	0.07	0.35			0.02	0.3		
	(0.13)	(0.03)			(0.12)	(0.03)		
Grades (gen. 1)	0.01			-0.01	0.01			-0.01
	(0.01)			(0.01)	(0.01)			(0.01)
Grades (gen. 2)	0.02			-0.02	0.02			-0.02
	(0.01)			(0.01)	(0.01)			(0.01)
Grades (gen. 3)	0.01			0	0.01			0
	(0.02)			(0.01)	(0.02)			-(0.01)
Fertility (gen. 1)		-0.05		-0.01		(0.06)		0
		(0.04)		(0.02)		(0.04)		(0.02)
Fertility (gen. 2)		-0.02		-0.01		-0.02		0
		(0.04)		(0.02)		(0.04)		(0.02)
Fertility (gen. 3)		0.02		-0.01		0.02		-0.01
		(0.04)		(0.02)		(0.04)		(0.02)
$l = (n, n, n)$			1	1.2			0.96	1.15
			(0.03)	(0.07)			(0.03)	(0.07)
$l = (s, n, n)$			0.84	1.03			0.76	0.93
			(0.02)	(0.07)			(0.02)	(0.06)
$l = (s, s, n)$			0.43	0.61			0.34	0.5
			(0.03)	(0.07)			(0.03)	(0.06)
$l = (s, s, s)$			0.09	0.26			0.05	0.21
			(0.01)	(0.06)			(0.01)	(0.06)
R^2	0.01	0.01	0.88	0.88	0.02	0.01	0.87	0.88
N	427	427	427	427	427	427	427	427

Table 2.14: Full transition matrix

		Generation g+1																	
		Male																	
Generation g		Low Fertility									High fertility								
	Education group	1	2	3	4	5	6	7	8	9	1	2	3	4	5	6	7	8	9
Low fertility	1	0.000	0.000	0.016	0.033	0.020	0.045	0.048	0.012	0.005	0.000	0.023	0.034	0.057	0.075	0.057	0.055	0.012	0.009
	2	0.000	0.000	0.012	0.028	0.025	0.057	0.064	0.018	0.009	0.000	0.010	0.017	0.041	0.066	0.058	0.068	0.015	0.012
	3	0.000	0.000	0.009	0.022	0.029	0.067	0.079	0.024	0.014	0.000	0.004	0.008	0.027	0.054	0.053	0.078	0.019	0.015
	4	0.000	0.000	0.006	0.016	0.032	0.074	0.090	0.032	0.020	0.000	0.001	0.003	0.017	0.041	0.046	0.083	0.021	0.018
	5	0.000	0.000	0.004	0.011	0.034	0.077	0.099	0.039	0.028	0.000	0.000	0.001	0.010	0.030	0.038	0.086	0.023	0.020
	6	0.000	0.000	0.002	0.007	0.034	0.078	0.105	0.047	0.038	0.000	0.000	0.001	0.006	0.021	0.031	0.085	0.024	0.022
	7	0.000	0.000	0.001	0.005	0.034	0.077	0.108	0.054	0.049	0.000	0.000	0.000	0.003	0.014	0.024	0.082	0.024	0.024
	8	0.000	0.000	0.001	0.003	0.032	0.074	0.109	0.061	0.063	0.000	0.000	0.000	0.002	0.009	0.018	0.077	0.024	0.025
	9	0.000	0.000	0.001	0.002	0.031	0.070	0.107	0.068	0.080	0.000	0.000	0.000	0.001	0.006	0.014	0.071	0.023	0.026
High fertility	1	0.000	0.038	0.010	0.021	0.013	0.022	0.034	0.006	0.003	0.000	0.046	0.021	0.058	0.072	0.089	0.055	0.008	0.002
	2	0.000	0.016	0.009	0.019	0.019	0.031	0.050	0.010	0.005	0.000	0.021	0.012	0.046	0.071	0.099	0.076	0.011	0.003
	3	0.000	0.006	0.007	0.016	0.024	0.039	0.065	0.015	0.009	0.000	0.009	0.006	0.033	0.061	0.098	0.093	0.014	0.005
	4	0.000	0.002	0.005	0.012	0.027	0.045	0.079	0.021	0.014	0.000	0.003	0.003	0.021	0.049	0.090	0.106	0.017	0.006
	5	0.000	0.001	0.003	0.009	0.030	0.050	0.091	0.027	0.020	0.000	0.001	0.001	0.013	0.037	0.078	0.113	0.019	0.007
	6	0.000	0.000	0.002	0.006	0.032	0.052	0.100	0.033	0.028	0.000	0.000	0.000	0.008	0.027	0.065	0.117	0.021	0.008
	7	0.000	0.000	0.001	0.004	0.032	0.053	0.107	0.040	0.038	0.000	0.000	0.000	0.005	0.019	0.053	0.117	0.022	0.009
	8	0.000	0.000	0.001	0.003	0.032	0.053	0.111	0.047	0.051	0.000	0.000	0.000	0.003	0.013	0.041	0.113	0.022	0.009
	9	0.000	0.000	0.000	0.002	0.031	0.052	0.113	0.054	0.065	0.000	0.000	0.000	0.001	0.009	0.032	0.108	0.022	0.010
		Female																	
		Low Fertility									High fertility								
	Education group	1	2	3	4	5	6	7	8	9	1	2	3	4	5	6	7	8	9
Low fertility	1	0.004	0.000	0.011	0.068	0.033	0.053	0.055	0.008	0.003	0.000	0.032	0.082	0.033	0.039	0.034	0.031	0.007	0.006
	2	0.004	0.000	0.009	0.060	0.044	0.072	0.077	0.013	0.006	0.000	0.014	0.043	0.025	0.036	0.036	0.041	0.010	0.009
	3	0.004	0.000	0.006	0.048	0.053	0.085	0.096	0.019	0.009	0.000	0.006	0.020	0.017	0.030	0.035	0.048	0.012	0.012
	4	0.003	0.000	0.004	0.035	0.059	0.095	0.111	0.024	0.014	0.000	0.002	0.009	0.011	0.023	0.030	0.052	0.014	0.014
	5	0.003	0.000	0.003	0.025	0.062	0.099	0.122	0.030	0.019	0.000	0.001	0.004	0.006	0.017	0.025	0.053	0.015	0.016
	6	0.002	0.000	0.002	0.017	0.063	0.101	0.130	0.036	0.026	0.000	0.000	0.001	0.004	0.012	0.020	0.053	0.016	0.018
	7	0.002	0.000	0.001	0.011	0.062	0.100	0.134	0.042	0.035	0.000	0.000	0.001	0.002	0.008	0.016	0.051	0.016	0.020
	8	0.001	0.000	0.001	0.007	0.061	0.097	0.136	0.048	0.045	0.000	0.000	0.000	0.001	0.005	0.012	0.049	0.016	0.021
	9	0.001	0.000	0.000	0.004	0.058	0.093	0.136	0.054	0.057	0.000	0.000	0.000	0.001	0.004	0.009	0.045	0.016	0.021
High fertility	1	0.000	0.068	0.007	0.043	0.023	0.027	0.039	0.004	0.002	0.007	0.065	0.052	0.034	0.038	0.054	0.032	0.005	0.002
	2	0.000	0.031	0.007	0.044	0.036	0.041	0.063	0.008	0.004	0.007	0.033	0.032	0.029	0.041	0.066	0.048	0.007	0.003
	3	0.000	0.012	0.005	0.038	0.046	0.054	0.086	0.012	0.007	0.006	0.014	0.016	0.022	0.037	0.068	0.062	0.010	0.004
	4	0.000	0.004	0.004	0.030	0.054	0.063	0.105	0.017	0.010	0.004	0.006	0.007	0.014	0.030	0.063	0.071	0.012	0.005
	5	0.000	0.001	0.003	0.022	0.059	0.069	0.120	0.022	0.015	0.003	0.002	0.003	0.009	0.023	0.055	0.075	0.014	0.006
	6	0.000	0.000	0.002	0.015	0.062	0.072	0.131	0.027	0.021	0.002	0.001	0.001	0.005	0.016	0.045	0.077	0.015	0.007
	7	0.000	0.000	0.001	0.010	0.063	0.073	0.139	0.033	0.028	0.002	0.000	0.001	0.003	0.012	0.036	0.077	0.015	0.007
	8	0.000	0.000	0.001	0.007	0.063	0.072	0.145	0.038	0.037	0.001	0.000	0.000	0.002	0.008	0.029	0.074	0.016	0.008
	9	0.000	0.000	0.000	0.004	0.061	0.070	0.148	0.044	0.048	0.001	0.000	0.000	0.001	0.005	0.022	0.071	0.016	0.008

3

ROY-MODEL BOUNDS ON GROUP DIFFERENCES IN TREATMENT EFFECTS: THEORY WITH AN APPLICATION TO THE GREAT MIGRATION

3.1 INTRODUCTION

In this paper, I study the conditions under which information about the causal effect of a treatment can be identified by applying difference-in-differences regression to two groups that both self-select into treatment. I show that, in many cases when selection and counterfactual outcomes can be described by a Roy model, differences-in-differences provide a lower bound on the difference in the average effect of the treatment on the treated between the group that is more likely to receive the treatment and the group that is less likely to do so. This group difference in treatment effects is particularly informative when interest centers on treatment effect heterogeneity or when it is reasonable to assume that the average treatment on the treated is nonzero for both groups, in which case differences-in-differences also provide a lower bound on the treatment effect for the group with the higher treatment rate. My identification results apply under quite general selection and outcome processes and do not require an instrument for treatment status. Since they provide a framework for interpreting treated-untreated outcome differences in terms of the causal effects of treatment, they may also be useful when the data contain no credibly exogenous sources of variation in the probability of receiving treatment.

The logic of the identification argument is straightforward. Suppose that, within each of two groups, the treated population is positively selected in the sense that those for whom the effect of treatment is greater are more likely to enroll in treatment. In this case, the within-group average difference between the outcomes of the treated and untreated populations represents a combination of the effect of the treatment on the treated and a bias term that reflects positive selection. When one group has a lower probability of being treated—that is, treatment is more selective—we might expect the selection bias component to be larger for this group. In this case, subtracting the average treated-untreated difference for the low-probability group from that for the high-probability group overcontrols for the selection bias of the high-probability group. Consequently, the group difference in average treated-untreated differences represents a lower bound on the group difference in the average effect of the treatment on the treated. In other words, although treatment is not randomly assigned, the low-probability group may be viewed as a quasi-control group whose outcomes can be used to partially identify the causal effect of the treatment on the high-probability group.

The formal identification results that I present below clarify the conditions under which this informal argument is correct. Identification is not assumption-free. Rather, the identification procedures that I develop require restrictions on group-specific treatment rates and the processes that determine treatment and counterfactual outcomes. Only the former is directly verifiable. Of the second set of restrictions, the most stringent requirement is that the selection and outcomes processes follow a generalized Roy model under which those for whom the treatment effect is greater are more likely to enroll. Because it imposes a rational decision-making structure on the data, this requirement is strong. At the same time, because this structure underlies many social-scientific theories and econometric methodologies, it is not unusually strong. More importantly, this same rational-choice framework is almost always the source of concerns that treated-untreated comparisons are contaminated with selection bias. If it is unreasonable to presume that the data were generated by a Roy model, selection bias is unlikely to be a problem.

As usual, functional form assumptions can assist in identification. In the simplest of the models that I develop, treated and untreated outcomes are linear functions of an unobserved random variable that also influences selection into treatment, imposing further structure on the data-generating process. Similar assumptions are invoked in procedures for estimating truncated regressions, switching regressions, treatment effects and sample selection models that are implemented in popular statistical packages such as Stata and R (Toomet and Henningsen, 2008; StataCorp, 2013). While these estimators typically assume that the unobservable determinants of selection and counterfactual outcomes are normally distributed, my identification results hold under much weaker distributional assumptions that are met by many distributions frequently used in empirical models of discrete choice, sample selection, duration and reliability. I also present identification results that hold under more general forms for the functions that determine outcomes and enrollment. This additional generality requires further restrictions on the processes that determine selection and counterfactual outcomes. Because these extra restrictions are unlikely to be acceptable on purely theoretical grounds, I present a falsification test for whether they are consistent with the data, although the test is weak in that it may fail to reject when the conditions are not met.

The standard methods for estimating treatment effects when unobservable factors influence both treatment and outcomes are instrumental variables and switching regression estimators based on Heckman's (1979) sample-selection correction (see, e.g., Wooldridge, 2002). As previously discussed, my identification results hold under weaker distributional assumptions, or more flexible functional forms, than the canonical switching regression model. Moreover, both instrumental variables methods and switching regression estimators require an exclusion restriction (a variable that appears in the selection equation but not the outcome equations) in order to identify treatment effects.[1] Because my identification results exploit between-group variation in treatment rates to bound (group differences in) treatment effects, they do not require an exclusion restriction.

What affords my results this flexibility is that they only partially recover treatment effects. Rather than point-identifying the average effect of treatment on the treated, differences-in-differences identifies a lower bound on the group difference in this average. In many treatment effect estimation contexts, this is not the primary object of interest. However, there are several settings in which this estimand provides useful information. The first, and most salient, of these is when heterogeneity in treatment effects is of direct interest. In Section 3.4, for example, I use the identification results to interpret wage differentials between the Northern and Southern US in terms of the causal impact of migration on wages. Because Northward migration is thought to have played a crucial role in black relative economic progress during the 20th century, the black-white difference in the effect of migration is arguably more relevant than the causal effect for either group alone. Other examples abound: an educator may be interested in knowing whether a new program helps struggling students catch up to their peers, a clinician may be interested in gender or racial differences in the efficacy of a new drug, etc. Second, when it is reasonable to assume that the treatment effect is nonnegative for both groups, a lower bound on the group difference in treatment effects is also a lower bound on the average treatment effect itself for the group with the higher treatment rate. Third, because group differences in treatment effects are identified under relatively weak conditions, partial information about the causal effect of the treatment may be preferable to treatment effect estimates that are point-identified from invalid instruments. Fourth, information about group differences in treatment effects obtained from differences-in-differences may be cross-validated against the results of other treatment effect estimators.

This paper builds on a number of seminal theoretical and methodological results on sample-selection problems. Roy's "Some Thoughts on the Distribution of Earnings" was published in 1951 and has provided social scientists with a framework for thinking about self-selection ever since. Tobin (1958) demonstrated the effects of truncation on estimates of models with linear errors and developed an estimation procedure to circumvent the resulting bias. Heckman (1979) showed similar biases arising due to sample-selection can be viewed as specification error and accounted for using his celebrated sample-selection correction. Heckman (1976) and Amemiya (1984) showed that many sample-selection esti-

1 It is possible to estimate the switching regression model without an exclusion restriction. Since identification in this case is achieved purely through the nonlinearity of the inverse Mills ratio, however, the credibility (and precision) of the resulting estimates may be low. Olsen (1980) presents an alternative to Heckman's selection correction in which the errors in the selection equation are uniformly distributed, in which case the model cannot be estimated without an exclusion restriction. Although my identification results also rely on distributional assumptions, since only bounds on group differences in treatment effects can be recovered, identification is robust to very general misspecification of the distributions of the random determinants of selection and outcomes.

mators can be viewed as extensions of the Tobin's (1958) truncated regression procedure. My results draw on these authors' methodological contributions and can be applied to sample-selection models similar to theirs. Furthermore, although my results are novel and my emphasis is on treatment effects, my identification approach is also conceptually related to work by Borjas (1988) and Heckman and Honoré (1990), in which Roy models of self-selection (into immigration and occupations, respectively) are used to draw inferences about the underlying models whose equilibria generate observed descriptive relationships.

In Section 3.2, I present identification results for a simple model, similar to those used in standard sample-selection and truncated regression estimators, in which selection is determined entirely by the realization of an unobserved type that is also linearly related to counterfactual outcomes. In Section 3.3, I extend these results to allow for the possibilities that other random factors influence the enrollment decision and that selection and outcomes depend nonlinearly on the unobserved type. In Section 3.4, I apply the identification results to interpret wage differentials between the Northern and Southern US in terms of the black-white difference in the causal effect of Northward migration on migrants' wages. I summarize and conclude in Section 3.5.

3.2 PURE SELECTION

I begin by analyzing the impacts of treatment in the case where the treatment effect is a linear function of an unobserved, normally distributed random variable, a setup similar to those studied by Roy (1951), Borjas (1988) and Heckman and Honoré (1990). In the interest of clarity, and to avoid using nonstandard notation, I present the model and theoretical results in the context of the empirical application given at the end of this paper: the identification of black-white differences in the causal effect of treatment. The extension of the results to other kinds of group differences is straightforward.

Assume that members of group $r \in \{b, w\}$ are characterized by their unobserved type a with corresponding distribution and density functions $F(a)$ and $f(a)$ that do not depend on r.[2] Individuals may choose to receive treatment (that is, make the enrollment choice $d = 1$) or remain untreated ($d = 0$). Counterfactual outcomes in the treated and untreated conditions are given by $y_{dr} = \gamma_{dr} a$, with $\gamma_{dr} \geq 0$ for $r \in \{b, w\}$ and $d \in \{0, 1\}$.

2 This implies that any group differences in the average treatment effect and the probability of treatment arise because of group differences in the utility of receiving treatment. If one group's type distribution is a shift-and-spread of the other's, the identification arguments that follow still be apply as long as the treatment effect model presented below is still applicable when viewed as a function of the standardized type variable.

Reflecting the hypothesis of positive selection into treatment, I assume that the selection (equivalently, enrollment) decision rule takes the form

$$d(a,r) = \begin{cases} 1 & \text{if } a \geq \hat{a}_r \\ 0 & \text{otherwise} \end{cases}.$$

This decision rule subsumes the standard Roy-model selection process in which individuals select into treatment solely on the basis of the counterfactual outcomes y_{dr}. It also allows for the possibility that individuals receive utility from being treated that is unrelated to the outcome variable, as long as this utility is increasing in the unobserved type a. Note that if the treatment and selection equations also depend on observable characteristics x, the argument that follows can be repeated within strata of these variables. In this case, the decision thresholds become $\hat{a}_{rx}$ and the outcome equations become $y_{dr}(x) = \gamma_{drx}a$.

Unlike traditional difference-in-differences designs, since blacks and whites both self-select into treatment, there is no formal control group. However, if there are group differences in the probability of being treated, information about the group difference in average treatment effects may still be identified. In particular, suppose that blacks are more likely than whites to be treated, so that $\hat{a}_b < \hat{a}_w$. In this case, whites can be viewed as a quasi-control group. Under conditions outlined below, treated-untreated comparisons for whites may be used to bound the degree to which treated-untreated comparisons among blacks are contaminated by selection bias.

Given the above selection and treatment models, and letting $y = dy_1 + (1-d)y_0$ denote the observed outcome, the group-r mean treated-untreated difference can be written

$$\begin{aligned} E(y|r, d=1) - E(y|r, d=0) &= \gamma_{1r}E(a|a \geq \hat{a}_r) - \gamma_{0r}E(a|a < \hat{a}_r) \\ &= (\gamma_{1r} - \gamma_{0r})E(a|a \geq \hat{a}_r) + \gamma_{0r}\left[E(a|a \geq \hat{a}_r) - E(a|a < \hat{a}_r)\right]. \end{aligned} \quad (3.1)$$

The second equality in (3.1) decomposes the mean difference in outcomes between treated and untreated members of group r into the average causal effect of treatment on the outcomes of the treated, $(\gamma_{1r} - \gamma_{0r})E(a|a \geq \hat{a}_r)$, and the selection bias, $\gamma_{0r}[E(a|a \geq \hat{a}_r) - E(a|a < \hat{a}_r)]$ that arises because the average type is higher in the treated population. Similarly, the black-white difference in average treated-untreated differences can be written

$$\begin{aligned} &[E(y|b, d=1) - E(y|b, d=0)] - [E(y|w, d=1) - E(y|w, d=0)] = \\ &\quad \{(\gamma_{1b} - \gamma_{0b})\, E(a|a \geq \hat{a}_b) - (\gamma_{1w} - \gamma_{0w})\, E(a|a \geq \hat{a}_w)\} \\ &\quad + \{\gamma_{0b}\left[E(a|a \geq \hat{a}_b) - E(a|a < \hat{a}_b)\right] - \gamma_{0w}\left[E(a|a \geq \hat{a}_w) - E(a|a < \hat{a}_w)\right]\}. \end{aligned} \quad (3.2)$$

In (3.2), the difference-in-differences is expressed as the sum of the black-white differences in treatment effects and selection biases. Therefore, a difference-in-differences estimator (i.e., the left-hand size of (3.2)) will identify a lower bound on the black-white difference $(\gamma_{1b} - \gamma_{0b})E(a|a \geq \hat{a}_b) - (\gamma_{1w} - \gamma_{0w})E(a|a \geq \hat{a}_w)$ in treatment effects if

$$\frac{\gamma_{0b}}{\gamma_{0w}} < \frac{E(a|a \geq \hat{a}_w) - E(a|a < \hat{a}_w)}{E(a|a \geq \hat{a}_b) - E(a|a < \hat{a}_b)}. \quad (3.3)$$

As long as $\gamma_{0b} \leq \gamma_{0w}$, we can replace the left-hand side of condition (3.3) with unity. What this simplification requires is that, when untreated, a black individual's outcome will be no better than that of a white individual of the same type. Since, by assumption, the black treatment rate exceeds the white treatment rate, this assumption is natural. For example, in the context of the effect of South-North migration on wages, that blacks face greater discrimination (and consequently lower wages) in the South is mutually consistent with the observation that they are also more likely to migrate to the North. Given this simplification, a sufficient condition for (3.3) is that

$$[E(a|a \geq \hat{a}) - E(a|a < \hat{a})]' \geq 0 \quad \text{whenever} \quad \hat{a} \geq \hat{a}_b. \quad (3.4)$$

Consider the effect of an increase in the enrollment threshold $\hat{a}_r$. Due to this change, an individual at the margin of the enrollment decision switches from the treated state to the untreated state. Since, were this individual to enroll, he would have the lowest type in the treated group, increasing the threshold therefore increases the type of the mean treated unit. Because this marginal individual is now the untreated unit with the highest type, increasing the threshold also increases the type of the mean untreated unit. What (3.4) ensures is that the former effect dominates, so that subtracting the mean treated-untreated outcome difference for whites from that for blacks effectively over-controls for selection bias among blacks, resulting in a lower bound on the black-white difference in the average effect of treatment on the treated. The following result establishes that, if the type distribution is normal, this dominance is automatic if the probability of treatment is less than 1/2.

Proposition 3.1. *Suppose that a is normally distributed. Then $[E(a|a \geq \hat{a}) - E(a|a < \hat{a})]' \geq 0$ when the treatment probability is less than 1/2.*

Proof. A standard result on truncated normal variables is that (supposing without loss of generality that a is standard normal) $E(a|a \geq \hat{a}) = \lambda(a) = \phi(a)/\Phi(-a)$ and $E(a|a < \hat{a}) = -\lambda(-a) = -\phi(a)/\Phi(a)$, where $\lambda(a)$, $\phi(a)$ and $\Phi(a)$ are the inverse Mill's ratio, and the standard normal density and distribution functions, respectively (see, e.g., Heckman, 1979; Wooldridge, 2002; Greene, 2011). Thus, $E(a|a \geq \hat{a}) - E(a|a < \hat{a}) = \lambda(a) + \lambda(-a)$. By inspection, this function has a critical point at zero. Furthermore, Heckman and Honoré (1990) showed that $\lambda(a)$ is strictly convex, so $\lambda(a) + \lambda(-a)$ is strictly convex as well, implying that the function reaches its unique minimum at zero and is increasing for all $\hat{a} > 0$. □

The implication of Proposition 3.1 is that, when both groups receive treatment with probability less than 1/2, the selection bias component of the treated-untreated difference is increasing in the enrollment threshold for both groups, so subtracting this difference for whites from that for blacks bounds the group difference in treatment effects from below. Defining the group-r proportional treatment effect as $y_{1r}/y_{0r} = \gamma_{1r}/\gamma_{0r}$, noting that the difference-in-differences for log outcomes is

$$[E(\log y_{1b}|a \geq \hat{a}_b) - E(\log y_{0b}|a < \hat{a}_b)] - [E(\log y_{1r}|a \geq \hat{a}_w) - E(\log y_{0r}|a < \hat{a}_w)]$$
$$= \log\left(\frac{\gamma_{1b}/\gamma_{0b}}{\gamma_{1w}/\gamma_{0w}}\right) + [E(\log a|a \geq \hat{a}_b) - E(\log a|a < \hat{a}_b)]$$
$$- [E(\log a|a \geq \hat{a}_w) - E(\log a|a < \hat{a}_w)],$$

and recalling that, when $a \sim LN(\mu, \sigma^2)$ the median of a is e^μ, the following result is immediate:

Corollary 3.1. *Suppose that* $\log a$ *is normally distributed. Then differences-in-differences identifies a lower bound on the proportional black-white difference in proportional treatment effects whenever both groups are treated with probability less than 1/2.*

Although I show below that this bounding result holds under many type distributions, the normality assumption is common in sample selection models and natural in cases where the empiricist has no a priori guidance about the distribution of the unobservables. Moreover, the actual type distribution only has to resemble the normal distribution insofar as it inherits condition (3.4) in order for differences-in-differences to bound the group difference in treatment effects from below. This set identification approach is thus more robust to misspecification of the skill distribution than Tobit-type estimators that attempt to point-identify the parameters of an outcome equation, which, as Arabmazar and Schmidt (1982) illustrate, can suffer from considerable inconsistency when the data-generating process is non-normal.

3.2.1 *Robustness to non-normality*

The following result establishes two sets of sufficient conditions under which $E(a|a \geq \hat{a}) - E(a|a < \hat{a})$ is increasing. The conditions are quite different and appear restrictive on face. As illustrated below, however, most distributions used in sample selection, truncation, discrete choice, duration and reliability models meet one of these two sets of conditions.

Proposition 3.2. *Suppose that a is distributed over* $[L, \infty]$ *with density* $f(a)$ *satisfying* $\lim_{a\to\infty} f(a) = 0$. *Then*

1. *If $E(a|a \geq \hat{a})$ is convex and $E(a|a < \hat{a})$ is concave, there exists an a^* such that*

$$[E(a|a \geq \hat{a}) - E(a|a < \hat{a})]' \geq 0$$

for all $\hat{a} \geq a^*$. If $f(a)$ is symmetric, then a^* is the mean. If the mean exceeds the median, then a^* is less than the median.

2. *If $f(a)$ is log convex and $f'(a) \leq 0$ for all a,*

$$[E(a|a \geq \hat{a}) - E(a|a < \hat{a})]' \geq 0$$

for all $\hat{a}$.[3]

Proof. To prove the first part, suppose that $\lim_{\hat{a}\to\infty} E(a|a < \hat{a})' > 0$. Then, since a has infinite support, there exists an $\hat{a}$ such that $E(a|a \leq \hat{a}) > E(a)$, a contradiction. Thus $\lim_{\hat{a}\to\infty} E(a|a < \hat{a})' = 0$. Since $E(a|a \geq \hat{a})' \geq 0$ by definition, there is an a^* such that $E(a|a \geq \hat{a})' = E(a|a < \hat{a})'$. Then by assumption, $E(a|a \geq \hat{a}) - E(a|a < \hat{a})$ is convex, reaching its unique minimum at a^* and increasing thereafter. Further,

$$[E(a|a \geq \hat{a}) - E(a|a < \hat{a})]' = \frac{f(\hat{a})}{1-F(\hat{a})}\left(\frac{\int_{\hat{a}}^{\infty} tf(t)dt}{1-F(\hat{a})} - \hat{a}\right) - \frac{f(\hat{a})}{F(\hat{a})}\left(\hat{a} - \frac{\int_L^{\hat{a}} f(t)dt}{F(\hat{a})}\right).$$

At the median, $\tilde{a}$, of a, this expression becomes $4f(\tilde{a})\,[E(a) - \tilde{a}]$. Thus, for $f(a)$ symmetric, $a^* = E(a) = \tilde{a}$. Instead, if $E(a) > \tilde{a}$, $[E(a|a \geq \tilde{a}) - E(a|a < \tilde{a})]' > 0$, so $a^* < \tilde{a}$.

To prove the second part, note that $f(a)$ log convex with $\lim_{a\to\infty} f(a) = 0$ implies that $1 - F(a)$ is log convex and that $f'(a) \leq 0$ for all a implies that $F(a)$ is log concave (Bagnoli and Bergstrom, 2005). But $1 - F(a)$ log convex implies $E(a|a \geq \hat{a})' \geq 1$ while $F(a)$ log concave implies $E(a|a < \hat{a})' \leq 1$ (see, e.g., Heckman and Honoré, 1990). Thus $[E(a|a \geq \hat{a}) - E(a|a < \hat{a})]' \geq 0$ for all $\hat{a}$. □

The first part of Proposition 3.2 clarifies the sense in which a type distribution needs to approximate the normal distribution in order for the identification result to hold over some range of treatment probabilities—the left- and right-truncated expectations must be convex and, respectively, concave. Note that if the mean of an asymmetric type distribution exceeds its median then the condition that the treatment probability is less than 1/2 is conservative: the difference between the left- and right-truncated expectations will be increasing at even higher treatment rates. The second part of the proposition shows that, even when the type density does not resemble the normal distribution in this way (i.e., log convex distributions with monotone decreasing densities), difference-in-differences regression may still recover lower bounds on group differences in treatment effects. More-

3 A function is log concave (log convex) if its logarithm is concave (convex).

over, for distributions that satisfy the second set of criteria, the bounding result holds at any treatment probability.[4]

Figure 3.1 plots the density functions, left- and right-truncated expectations, and the difference between these expectations, for the (standard) normal, logistic, uniform, gamma (with shape parameter 1.5), Weibull (with shape parameter 1.5), exponential (with rate parameter 1.5), and (standard) lognormal distributions. For each of these distributions, the left-truncated expectation is at least weakly convex and the right-truncated expectation is at least weakly concave. Accordingly, for each distribution, there exists a range over which the difference $E(a|a \geq \hat{a}) - E(a|a < \hat{a})$ is nondecreasing. For the normal and logistic distributions, this difference is increasing whenever $\hat{a}$ exceeds the mean (i.e., the treatment probability is less than 1/2). Under, for example, the gamma and Weibull distributions, the threshold at which this difference reaches its minimum is lower (so that the corresponding treatment probability is greater).

Figure 3.2 plots the same functions for the gamma distribution (with shape parameter .75), the Weibull distribution (with shape parameter 1.5), the Pareto distribution (with shape parameter 1.1) and the lognormal distribution (with $a \sim LN(1,2)$). Each of these distributions has concave left- and right-truncated expectations with the slope of the left-truncated expectation exceeding that of the right-truncated expectation over the entire support. For these distributions, the difference $E(a|a \geq \hat{a}) - E(a|a < \hat{a})$ is increasing everywhere increasing, implying that differences-in-differences identifies a lower bound on the group difference in average treatment effects at any enrollment threshold.

A comparison of Figures 3.1 and 3.2 reveals that log concave distributions tend to satisfy the first set of conditions given in Proposition 3.2 while log convex distributions tend to satisfy the second set. The density functions for the normal, logistic and exponential densities are log concave, while the gamma and Weibull densities are log concave when their shape parameters exceed one.[5] As Figure 3.1 illustrates, these densities have convex (concave) left-truncated (right-truncated) expectations. In contrast, the Pareto density is log convex and the gamma and Weibull distributions are log convex when their shape parameters are less than one. Figure 3.2 shows that these distributions have monotone decreasing density functions (and hence log concave distribution functions), concave left-truncated expectations with slopes that exceed one, and concave right-truncated expectations with slopes less than one. The uniform and lognormal densities straddle these cases: the uniform density is both log concave and log convex while the lognormal density is log concave when a is small and log convex with a is large.

Proposition 3.3 shows that this pattern is not coincidental. Under certain conditions, met by many distributions, log concave densities generate convex and concave left- and,

4 Note also that if the distribution of the log of the type satisfies one of these conditions, then a result similar to Corollary 3.1 applies.

5 Bagnoli and Bergstrom (2005) give expressions for $[\log f(a)]''$ for the normal, lognormal, exponential, gamma, Weibull and Pareto densities.

respectively, right-truncated expectations while monotone decreasing log convex densities generate concave left-truncated expectations with slopes that are everywhere smaller than those of their concave right-truncated expectations.

Proposition 3.3. *Suppose that a is distributed over $[L, H]$ and*

$$\left| \frac{[\log f(a)]''}{\left\{[\log f(a)]'\right\}^2} \right|' \lesseqgtr 0 \quad \textit{when} \quad f'(a) \lesseqgtr 0. \tag{3.5}$$

1. *If $f(a)$ is log concave and $\lim_{a\to L} f(a) = \lim_{a\to H} f(a) = 0$, then $E(a|a \geq \hat{a})$ is convex and $E(a|a < \hat{a})$ is concave.*
2. *If $f(a)$ is log convex and $\lim_{a\to H} f(a) = 0$ then $E(a|a \geq \hat{a})$ is concave.*

The proof is presented in Appendix 3.A. In addition to explaining the pattern exhibited by the distributions shown in Figures 3.1 and 3.2, Proposition 3.3 provides easily verifiable conditions under which a hypothesized distribution will satisfy the first criterion given in Proposition 3.2 (the second criterion can already be verified given an expression for the density function).[6] For example, when a is distributed standard normal or logistic, $|[\log f(a)]''/\{[\log f(a)]''\}^2|$ is $1/a^2$ or $2\exp(a)/[1-\exp(a)]^2$, respectively. These functions are decreasing when a exceeds zero, and the left-truncated expectations for these distributions are convex. When a is Pareto with shape parameter β, this ratio is $1/(\beta+1)$ and the left-truncated expectation is linear.

Although the previous proposition clarifies the relationship between the shape of the density function and the shape of the truncated expectation functions, it is Figures 3.1 and 3.2 that provide the most persuasive evidence of the utility of the foregoing identification procedure. The fact that the sufficient conditions for the identification of a lower bound presented in Proposition 3.2 are met by a large collection of distributions that are commonly used for econometric and reliability modeling strongly suggests that the distributional assumptions upon which the identification argument ultimately relies are relatively innocuous.[7]

6 That the lognormal density, which is neither log concave nor log convex over its entire support, can be configured to satisfy the either set of conditions given in Proposition 3.2 underscores the fact that the conditions enumerated in Proposition 3.3 are sufficient, and not necessary, to determine the shapes of the truncated expectations.

7 The linear functional form for counterfactual outcomes also suggests a natural falsification test for whether the type variable meets the conditions of Proposition 3.2. Although observed outcomes are of the form $y = d\gamma_1 a + (1-d)\gamma_0 a$, if there exists a subpopulation among whom treatment is very unlikely, the distribution of y will be close to the distribution of a, making it possible to estimate the curvature of the truncated expectations or test whether the empirical distribution of a approximates some hypothesized distribution.

3.3 NOISY SELECTION

Although the identification results in Section 3.2 hold under a wide variety of type distributions, the assumptions that selection is determined entirely by the unobserved type and that outcomes are linear in this type may be unreasonably restrictive in some contexts. In this section, I extend the identification argument to allow for the possibilities that idiosyncratic factors influence the decision to enroll in treatment and that the outcome functions are nonlinear. This additional flexibility comes at the cost of the imposition of greater structure on the treatment enrollment decision and, given outcome and selection mechanisms, more restrictive assumptions on the type distribution. However, the enrollment decision model remains quite general and I develop a falsification test that can detect the failure of the distributional assumptions in some cases. I also show that, when enrollment is idiosyncratic but outcomes remain linear, the results of the previous section can be applied under restrictions that are stronger than those required for the pure selection model but weaker than those required when outcomes are nonlinear.

3.3.1 *Two types*

I first present a simplified selection model in which the type distribution is binary, with $a \in \{l, h\}$ and $\Pr(a = h) = \pi$. The race- and treatment-condition-specific outcome equations take the form $y_{dr} = y_{dr}(a)$, $y'_{dr}(a) \geq 0$, $y_{1r}(a) > y_{0r}(a)$, $y_{0b}(a) \leq y_{0w}(a)$ and, as before, $r \in \{b, w\}$, $d \in \{0, 1\}$. These assumptions are the nonlinear counterparts to those presented in the setup of the pure selection model developed in the previous section. The relative utility of receiving the treatment is given by $\Delta_r + \gamma 1_{a=h} - \epsilon$, where ϵ is drawn from a distribution with density $f(\epsilon)$ and distribution function $F(\epsilon)$, so that the enrollment decision is of the form

$$d(a, r, \epsilon) = \begin{cases} 1 & \text{if } \Delta_r + \gamma 1_{a=h} - \epsilon \geq 0 \\ 0 & \text{otherwise} \end{cases}.$$

To reflect the notion that those who enroll in treatment are likely to have higher outcomes even absent the treatment, I assume that $\gamma > 0$. Note that a higher probability of treatment among blacks implies $\Delta_b > \Delta_w$. Because the main purpose of this model is to provide intuition about a more general selection model presented below, I postpone further discussion of the implications of these assumptions until Section 3.3.2.

Under this model, the mean treated-untreated outcome differential for group r is

$$\begin{aligned} &E(y|r,d=1) - E(y|r,d=0) \\ &\quad = [P(h|r,1)y_{1r}(h) + P(l|r,1)y_{1r}(l)] - [P(h|r,0)y_{0r}(h) + P(l|r,0)y_{0r}(l)] \\ &\quad = P(h|r,1)\,[y_{1r}(h) - y_{0r}(h)] + P(l|r,1)\,[y_{1r}(l) - y_{0r}(l)] \\ &\qquad + [P(h|r,1) - P(h|r,0)]\,y_{0r}(h) + [P(l|r,1) - P(l|r,0)]\,y_{0r}(l), \end{aligned}$$

where $P(a|r,d)$ denotes probability that an individual has type a given that they belong to group r and make treatment decision d. As in the model of Section 3.2, this expression decomposes the treated-untreated outcome differential into an average treatment on the treated term, $P(h|r,1)[y_{1r}(h) - y_{0r}(h)] + P(l|r,1)[y_{1r}(l) - y_{0r}(l)]$, and a selection bias term, $[P(h|r,1) - P(h|r,0)]y_{0r}(h) + [P(l|r,1) - P(l|r,0)]y_{0r}(l)$, that reflects differences in the type distribution, and hence untreated outcomes, between the treated and untreated groups.

The black-white difference in treated-untreated differences will bound the black-white difference in mean treatment effects from below if the selection bias component for whites exceeds that for blacks, or if

$$\begin{aligned} &[P(h|b,1) - P(h|b,0)]\,y_{0b}(h) + [P(l|b,1) - P(l|b,0)]\,y_{0b}(l) \\ &\quad < [P(h|w,1) - P(h|w,0)]\,y_{0w}(h) + [P(l|w,1) - P(l|w,0)]\,y_{0w}(l). \end{aligned} \tag{3.6}$$

Since $\Delta_b > \Delta_w$, condition (3.6) will hold if

$$\begin{aligned} &\frac{\partial}{\partial \Delta_r}\left\{[P(h|r,1) - P(h|r,0)]\,y_{0r}(h) + [P(l|r,1) - P(l|r,0)]\,y_{0r}(l)\right\} \\ &\quad = \left(\frac{\partial P(h|r,1)}{\partial \Delta_r} - \frac{\partial P(h|r,0)}{\partial \Delta_r}\right)[y_{0r}(h) - y_{0r}(l)] < 0, \end{aligned} \tag{3.7}$$

where the equality arises because $\partial P(h|r,1)/\partial \Delta_r = -\partial P(l|r,0)/\partial \Delta_r$. Since $y_{0r}(h) > y_{0r}(l)$ for $r \in \{b, w\}$ by assumption, (3.7) is tantamount to

$$\frac{\partial}{\partial \Delta_r}[P(h|r,1) - P(h|r,0)] < 0. \tag{3.8}$$

The following result establishes sufficient conditions for (3.8) to hold.

Proposition 3.4. *Suppose that $F(\epsilon)$ is log concave and that members of group r receive treatment with probability less than 1/2. Then*

$$\frac{\partial}{\partial \Delta_r}[P(h|r,1) - P(h|r,0)] \leq 0$$

if

$$f(\Delta_r + \gamma) - f(\Delta_r) \leq 0. \tag{3.9}$$

The proof, which is given in Appendix 3.A, shows that, when the treatment probability is less than 1/2, log concavity enables us to to bound the derivative in (3.8) from above while the density condition (3.9) establishes that the upper bound is negative, signing the derivative. The behavioral interpretation of the proposition is that the density condition $f(\Delta_r+\gamma) < f(\Delta_r)$ implies that high types are already sufficiently likely to enroll that making the treatment more appealing (increasing Δ_r) induces a disproportionate number of low types to enroll, decreasing the gap between the types of the average treated and untreated individuals.

3.3.2 *A continuum of types*

Although it is easier to understand how difference-in-differences can identify a lower bound on the difference in treatment effects when the type distribution is binary, for many applications, a binary type distribution may be inappropriate. To generalize the model to allow for a continuous type distribution, assume now that types are drawn from a distribution with density $\pi(a)$, outcomes are given by $y_{dr}(a)$ where $y'_{dr}(a) \geq 0$, and the selection rule is

$$d(a,r,\epsilon) = \begin{cases} 1 & \text{if } \Delta_r + \gamma(a) - \epsilon \geq 0 \\ 0 & \text{otherwise} \end{cases}, \qquad (3.10)$$

where $\gamma'(a) \geq 0$ and the distribution and density functions for ϵ are $F(\epsilon)$ and $f(\epsilon)$.[8] Note that the assumption that outcomes depend only on a is without loss of generality: we can equivalently assume that $y_{dr}(a)$ is the outcome that a type-a member of group r expects to receive when her treatment status is d, with the difference between the observed and expected outcomes being due to forecasting or measurement error, or otherwise uncorrelated with a and the stochastic component ϵ of the selection equation. Put differently, the statistical relationship between the selection and outcome equations owes to the mutual dependence of these functions on the unobserved factor a; this dependence is the ultimate source of selection bias in treated-untreated comparisons. As before, $\Delta_b > \Delta_w$ when the black treatment rate exceeds the white rate.

As in the linear treatment effect model of Section 3.2, the assumption that both groups have the same type distribution can be relaxed as long as the selection and treatment models are still applicable after the types has been standardized. The assumption that the random preference components ϵ are drawn from the same distribution for both groups is innocuous; any systematic differences in the attractiveness of treatment should be absorbed into the deterministic component $\Delta_r + \gamma(a)$ of the selection equation.

8 I assume without loss of generality that ϵ has support over the real line.

For parsimony, I impose in the selection equation (3.10) the simplification that the function $\gamma(a)$ that determines the relationship between the type and the utility from treatment is the same for blacks and whites. This assumption accommodates two important parametric models of group-differences in the utility of treatment. In the first model, the expected treatment-induced utility gains for blacks and whites of the same type differ only by a constant. This will be appropriate if Δ_r represents a source of between-group variation in the attractiveness of treatment that is uncorrelated with the effect of treatment on the outcome measure (in other words, if Δ is an instrument that is constant within groups but varies between them) or if, at least to an approximation, the outcome functions for blacks and whites are vertical translates. In the second model, the utility from treatment to a type-a black individual whose random preference component is ϵ is proportional to that for an otherwise-identical white individual. That is, the utility associated with treatment status d is $\Delta_{dr}\gamma_d(a){\epsilon_d}^{-1}$, so that an individual from group r will enroll if

$$\log(\Delta_{1r} - \Delta_{0r}) + \log[\gamma_1(a) - \gamma_0(a)] - \log(\epsilon_1 - \epsilon_0) \equiv \Delta_r + \gamma(a) - \epsilon \geq 0.$$

This model of the treatment decision is simple and flexible; absent prior empirical or theoretical information about the selection process, it is also a priori reasonable. However, the identification argument can also be modified to allow more general selection equations of the form $\gamma(\Delta_r, a) - \epsilon$ subject to the conditions that $\partial\gamma(\Delta_r, a)/\partial\Delta_r > 0$ and $\sup_a\{\partial\gamma(\Delta_r, a)/\partial\Delta_r\} < \infty$.[9]

Like the pure selection model, the noisy selection model can accommodate the case where the unobserved type only enters the enrollment decision through its influence on outcomes—that is, when $\Delta_r + \gamma(a) = y_{1r}(a) - y_{0r}(a)$—as well as the more general case where preferences over treatment conditions also depend on non-outcome factors that are correlated with unobserved types. Finally, as in the pure selection model, if treatment and selection also depend on observable characteristics x, the identification argument can be repeated within x-strata.

When the type distribution is continuous, the mean treated-untreated outcome difference can be written

$$\begin{aligned} E(y|r, d=1) - E(y|r, d=1) &= \int y_{1r}(a)P(a|r,1)da - \int y_{0r}(a)P(a|r,0)da \\ &= \int \left[y_{1r}(a) - y_{0r}(a)\right] P(a|r,1)da + \int \left[P(a|r,1) - P(a|r,0)\right] y_{r0}(a)da, \end{aligned}$$

9 To see this, replace all instances of $f(\Delta_r + \gamma(a))$ in the proof (given in Appendix 3.A) with $f(\gamma(\Delta_r, a))[\partial\gamma(\Delta_r, a)/\partial\Delta_r]$; the result still holds as long as $\partial\gamma(\Delta_r, a)/\partial\Delta_r$ is positive and bounded over a. When this derivative is unbounded, whether the argument still applies depends on the rate at which it grows. Note also that, when $\partial\gamma(\Delta_r, a)/\partial\Delta_r \neq 1$, the falsification test developed below must be modified accordingly.

where, as before, the second equality decomposes the treated-untreated difference into the average effect of treatment on the treated, $\int [y_{1r}(a) - y_{0r}(a)]P(a|r,1)da$, and a selection bias term, $\int [P(a|r,1) - P(a|r,0)]y_{0r}(a)da$.[10] In this case, the group difference in treated-untreated differences will bound the group difference in the average treatment effect for the treated from below if

$$\int [P(a|w,1) - P(a|b,0)]\, y_{b0}(a)da < \int [P(a|w,1) - P(a|w,0)]\, y_{0w}(a)da. \qquad (3.11)$$

Condition (3.11) will hold if $\Delta_b > \Delta_w$ and

$$\frac{\partial}{\partial \Delta_r} \int [P(a|r,1) - P(a|r,0)]\, y_{0r}(a)da < 0. \qquad (3.12)$$

Proposition 3.5 presents sufficient conditions for (3.12):

Proposition 3.5. *Suppose that $F(\epsilon)$ is log concave, $\lim_{\epsilon \to \infty} f(\epsilon) = 0$, and that members of group r receive treatment with probability less than 1/2. Then*

$$\frac{\partial}{\partial \Delta_r} \int [P(a|r,1) - P(a|r,0)]\, y_{0r}(a)da \leq 0$$

if

$$Cov\left[f\left(\Delta_r + \gamma(a)\right), y_{0r}(a)\right] \leq 0. \qquad (3.13)$$

The proof, which is given in Appendix 3.A, is similar that of Proposition 3.4. When the distribution of the random preference terms is log concave and the treatment probability is less than one half, the derivative in (3.12) can be bounded from above (when the type distribution is continuous, this also requires the tail condition that the density of the random preference terms is zero at infinity). When, in addition, the covariance condition (3.13) is satisfied, the upper bound is negative, signing the derivative. The covariance condition is analogous to the density condition required for the binary type distribution case. Informally, it requires that treatment is sufficiently common among those with high realizations of the unobserved type that large untreated outcomes are associated with small changes in the probability of treatment.

Several aspects of Proposition 3.5 (and Proposition 3.4) warrant further comment. The log concavity of $F(\epsilon)$ is weaker than the log concavity of $f(\epsilon)$ (which was required for the identification results developed in Section 3.2), since the log concavity of the latter implies that of the former. Second, although the proposition requires log concavity (as opposed to either log concavity or log convexity as in Section 3.2), it is only the distribution of the idiosyncratic component ϵ of the relative utility of receiving the treatment that must be log concave; the normal and logistic distributions, which are frequently used to model differences in utilities, satisfy this condition. Third, the derivative (3.12), and hence the

10 I abuse notation here by letting $P(a|r,d)$ denote the density of a conditional on race and treatment status.

covariance condition (3.13), only needs to hold for values of Δ_r in the set (Δ_w, Δ_b) for the lower bound condition (3.11) to be satisfied at the observed group-specific treatment probabilities.

3.3.3 *Assessing the covariance condition*

Proposition 3.5 shows that the essential identification result of the pure selection model—that differences-in-differences can provide a lower bound on the group difference in treatment effects when treatment rates are unequal—can still be obtained when outcomes are nonlinear in the unobserved type and the enrollment decision is stochastic. Although extending the result to these cases requires the imposition of additional structure on the model, the extra requirements are minimal: the distribution function governing the random preference components must be log concave, the associated density must be zero at infinity, and the selection equation must fall into a large class of parametric functions.

However, the unobserved types (combined with the selection and outcome equations) must also meet the more stringent condition (3.13) that the covariance between the "treatment density" $f(\Delta_r + \gamma(a))$ and untreated outcomes $y_{0r}(a)$ is nonpositive. Since this covariance depends simultaneously on four potentially nonlinear functions (the distributions of ϵ and a as well as the selection and outcome equations), it seems reasonable to ask whether it is likely, or even possible, that this condition will hold. By assumption, f is log concave (and hence unimodal) and y_{0r} is increasing. Therefore, large values of $y_{0r}(a)$ will be associated with large values of the treatment density $f(\Delta_r + \gamma(a))$ when this density is increasing and small values of $y_{0r}(a)$ when this density is decreasing. Informally, the covariance between these functions will therefore be negative if Δ_r is large enough that the treatment density tends to be decreasing, if the distribution of a is such that it places more weight on points in the support of a where the treatment density is decreasing, or if y_{0r} is increasing more rapidly when the treatment density is decreasing.

A simple numerical example illustrates this logic. Suppose that the outcome equations take the linear forms $y_1 = 3a$ and $y_0 = a$ and that the selection equation is a probit in the difference in counterfactual outcomes, so that $d = 1(\Delta + 2a - \epsilon)$ with ϵ standard normal. When $\log(a)$ is standard normal, simulation reveals that $Cov[f(\Delta + 2a), a]$ is negative when Δ is large enough to generate an average treatment probability $E[F(\Delta + 2a)]$ greater than about .3. Instead, if $\log(a) \sim N(0, 2)$, in which case the right tail of the type distribution is fatter (and hence places more weight on points of the support of a where the treatment density is decreasing), the simulated covariance is negative when Δ is large enough to generate an average treatment probability of about .15 or greater.

Thus, the covariance condition (3.13) depends in a complex way on functions whose realizations are unobserved and about which theory or prior empirical evidence may provide little guidance. Furthermore, although specifying parametric forms for these functions may provide insight about the plausibility of the covariance condition, the fewer restrictions placed on these functions, the more credible the identification strategy. The availability of a proxy z for a suggests a natural falsification test for the covariance condition: use z to estimate $g(z) = G'(z)$ where $G(z) = \Pr(d = 1|z)$ and compute the empirical covariance between outcomes in the untreated condition (which are observed in the data) and the estimate $\hat{g}(z)$. If, for example, z is discrete, a simple estimator for $g(z)$ is

$$\hat{g}(z) = \hat{G}(z) - \hat{G}(z-1) = \frac{\sum_{i=1}^{N} 1(d_i = 1)1(z_i = z)}{\sum_{i=1}^{N} 1(z_i = z)} - \frac{\sum_{i=1}^{N} 1(d_i = 1)1(z_i = z-1)}{\sum_{i=1}^{N} 1(z_i = z-1)}. \quad (3.14)$$

Alternatively, one may specify a parametric form (say a logit or probit) for $G(z) = \Pr(d_i = 1|z_i = z)$ and construct $\hat{g}(z)$ by differentiating $\hat{G}(z)$.

Note that assuming that z proxies for the unobserved type is much weaker, and consequently more defensible, than the assumptions that either (i) the covariance condition holds, (ii) the forms of the untreated outcome equation, selection equation and density for the stochastic preference terms are known, or (iii) that there is an instrument for d. Since z is a proxy (and not an estimator) for a, and since the untreated outcomes $y_{r0}(a)$ are only observed for untreated units, this procedure only provides a falsification test, and not a formal hypothesis test, of the covariance condition. If the data fail the falsification exercise—that is, if the empirical covariance between $\hat{g}(z_i)$ and y_{0ri} is positive—we should be skeptical that condition (3.13) will hold. However, since f is nonlinear (and perhaps so is y_{0r}) and y_{0r} is not observed for treated individuals, it is possible that the empirical covariance will be nonpositive even when $f(\Delta_r + \gamma(a))$ is positively correlated with $y_{0r}(a)$.[11]

When z is thought to be correlated with a but also have an independent impact on the probability of selection into treatment, the falsification test can still be applied, but only with reduced ability to detect the failure of the covariance condition. For example, if the enrollment probability is actually $F(\beta_0 + \beta_1 a + \beta_2 z)$ and the falsification exercise is carried out under the specification $G(\gamma_0 + \gamma_1 z)$, the estimate of γ_1 will reflect both the effect of z on the probability of selection and the relationship between z and a. Proposition 3.5 requires that the treatment density and the untreated outcomes are negative correlated at each level of z, which implies that the average (across values of z) correlation will be negative as well.[12] A finding that $Cov[g(\hat{\gamma}_0 + \hat{\gamma}_1 z_i), y_{0i}] < 0$ lends credibility to the assumption that the covariance condition holds on average, but this does not imply that it holds at each level of the proxy z.

11 The second of these concerns can be addressed with an additional falsification test if there is a subpopulation for whom the treatment probability is close to zero, and consequently, the distributions of observed and untreated outcomes are approximately the same. Also note that, although the covariance condition needs to hold over the range (Δ_w, Δ_b), if it holds at Δ_w it is also likely to hold at values of $\Delta_r > \Delta_w$ since, as Δ_r increases, the part of the support of a over which $f(\Delta + \gamma(a))$ is increasing becomes smaller.

12 This follows from the bilinearity of the covariance operator.

3.3.4 *Relating the pure and noisy selection models*

While the noisy selection model allows for more flexible selection and outcome processes, the key criterion required to ensure identification is stronger than the analogous conditions on the type distribution required in the pure selection model. However, when the selection and outcome equations can be well-approximated by linear functions, the results from the pure selection model of Section 3.2 can be applied to the case where the enrollment decision is random under milder distributional assumptions, obviating the need to verify the covariance condition required by Proposition 3.5.

Standard estimators of sample selection and switching regression models (see Amemiya, 1984; Wooldridge, 2002, for example) assume counterfactual outcome equations similar to

$$y_{dr}(a) = \gamma_{dr} a$$

and that selection into treatment is determined by a process similar to

$$d_r(\epsilon) = \begin{cases} 1 & \text{if } \Delta_r - \epsilon \geq 0 \\ 0 & \text{otherwise} \end{cases}.$$

In this model, the mean treated-untreated differences can be decomposed, as before, according to

$$\begin{aligned} E(y|d=1,r) - E(y|d=0,r) &= \gamma_{1r} E(a|\epsilon \leq \Delta_r) - \gamma_{0r} E(a|\epsilon > \Delta_r) \\ &= (\gamma_{1r} - \gamma_{0r}) E(a|\epsilon \leq \Delta_r) + \gamma_{0r}[E(a|\epsilon \leq \Delta_r) - E(a|\epsilon > \Delta_r)]. \end{aligned}$$

If, as in Olsen (1980) and Wooldridge (2002), we assume that $E(a|\epsilon) = \rho\epsilon$ (with $\rho < 0$ and, for exposition, a and ϵ mean zero), then $E(a|\epsilon \leq \Delta_r) = \rho E(\epsilon|\epsilon \leq \Delta_r)$ and this difference becomes

$$\begin{aligned} &E(y|d=1,r) - E(y|d=0,r) \\ &\quad = (\gamma_{1r} - \gamma_{0r})\rho E(\epsilon|\epsilon \leq \Delta_r) - \gamma_{0r}\rho[E(\epsilon|\epsilon > \Delta_r) - E(\epsilon|\epsilon \leq \Delta_r)]. \end{aligned} \tag{3.15}$$

When blacks are more likely to enroll in treatment than whites, $\Delta_b > \Delta_w$, so the results of Section 3.2 can be applied to the random component ϵ of the selection equation to determine whether the selection bias term $-\gamma_{0r}\rho[E(\epsilon|\epsilon > \Delta_r) - E(\epsilon|\epsilon \leq \Delta_r)]$ from (3.15) is decreasing, in which case differences-in-difference once again recovers a lower bound on the black-white difference in the average treatment on the treated, even though the enrollment decision is random conditional on the type.

The assumption that the expectation of a given ϵ is linearly decreasing in ϵ (which implies that treated units have lower values of ϵ and hence higher values of a) is a simple way of capturing the notion of positive selection into treatment when the selection equation is random. Olsen (1980) shows that, given a distribution for ϵ, the linearity assumption may not impose unreasonable requirements on the distribution of a.[13] Indeed, when a and ϵ are multivariate normal, this linearity is automatic, the (modified) pure selection model reduces to the canonical switching regression or Type 5 Tobit model (Amemiya, 1984), and Proposition 3.1 can be applied.

3.4 APPLICATION: THE WAGES OF THE GREAT MIGRANTS

3.4.1 *The Great Migration*

The Great Migration refers to a period of US history spanning roughly 1915-1970 during which a tremendous number of Southern-born blacks left the South in favor of cities in the North. This episode is widely believed to have had profound social and economic consequences, both for the migrants themselves and for the areas to which they moved (Tolnay, 2003 provides a detailed review of the Great Migration and its effects). In particular, it is widely acknowledged (see, e.g., Smith and Welch, 1989; Donohue and Heckman, 1991) that Northward migration played an important role in the relative economic progress experienced by blacks during the 20th century. Although the Great Migration is therefore rightfully seen as holding special significance for black history, accounts of the magnitude of this migration often overlook the fact—which I document below—that many whites also moved North, albeit at considerably smaller rates. On average between 1940 and 1970, for example, 14% of Southern-born white men, compared to 28% of Southern-born black men, migrated to the North.

Smith and Welch (1989) and Donohue and Heckman (1991) decompose changes in black-white log wage gaps into components explained by racial differences in residential location, education and other factors. The aim of these studies is to understand the contribution of, inter alia, migration to declines in the black-white wage gap. This estimand can easily be recovered from black-white differences in North-South wage differentials. However, since characteristics not observed in the data may have contributed to the decision to migrate, and since they might have done so differently for blacks and whites, descriptive decompositions may not be informative about the causal effects of migration

13 Olsen (1980) notes that when ϵ is uniform and $E(a|\epsilon)$ is linear, a can be decomposed into two random variables, one of which is independent of ϵ. If the component that is independent of ϵ is normal, the distribution of a will be the convolution of a normal variable with a uniform variable. Since log concavity is preserved by multiplication and linear transformations (Bagnoli and Bergstrom, 2005), it is also preserved by convolution. It is therefore not unreasonable to believe that the results of Section 3.2 will apply in this case.

on wages. As Smith and Welch (1989) noted in their survey of the determinants of black economic progress,

> Even among men who have the same amount of education and job experience, large geographic wage differentials prevail among regions. Identifying their underlying causes is a complex empirical problem. Some of these wage disparities reflect cost-of-living differences between regions, or compensating payments for the relative attractiveness or undesirability of locational attributes (e.g. climate, crime, and density). Given the magnitude of the regional wage differentials we estimate, it is also likely that they proxy for unobserved indices of skill. Finally, the large black-white gap in the South may well reflect the historically more intense racial discrimination there.

and

> If they proxy for unobserved skill differences, cross-sectional wage differentials would not represent the wage gain an individual would receive by moving from the South to the North.

The identification results of Sections 3.2 and 3.3 provide a framework for understanding racial differences in North-South wage differentials in terms of the causal impact of migration on wages. In fact, because they recover group-differences in average treatment effects, the identification procedures in this paper are particularly well-suited to the analysis of the impacts of the Great Migration. As Smith and Welch (1989) argue, equilibrium North-South wage differentials may partially reflect regional variation in amenities, so that subtracting the effect of migration among whites from that for blacks removes the component of the wage effect of migration that is due to amenities, at least insofar as this component is similar for blacks and whites.[14] Further, while between-group heterogeneity in treatment effects is of interest in many settings, in the context of the Great Migration its interpretation is particularly stark: it represents extent to which black migrants earned more in the North because its denizens were less discriminatory than their Southern counterparts.

3.4.2 *Migration rates and North-South wage differentials*

Figure 3.3 plots Northward migration rates by year of birth for black and white men born in the Southern US. The data for this graph, and all further results in this section,

14 If the North-South amenity differential were greater for blacks (say because of reduced discrimination in the North), they would be willing to accept smaller wage increases in exchange for migrating. Subtracting the regional wage differential for whites (of which the decrease in the treatment effect due to amenities would be smaller) would still result in a lower bound on the black-white difference in the component of the treatment effect not due to amenity differences.

are based on 1% Integrated Public Use Microdata Samples (IPUMS) of the 1940-1970 US Censuses (Ruggles, Alexander, Genadek, Goeken, Schroeder, and Sobek, 2010), from which I include only Southern-born black and white men.[15] For birth years prior to 1880, the sample sizes are small and the estimated migration rates are imprecise for men of both races. Past 1880, as the figure shows, black migration rates dominate white rates at all birth years, and exhibit a steeper trend. For example, a white man born in the South around 1940 had about a 20% chance of migrating to the North, while his black counterpart had a 40% chance of migrating. To examine these differences in migration rates in greater detail, I present in Table 3.1 linear models of the probability of migrating as a function of race and, in some specifications, indicators for age, educational attainment and, in models that pool across time, decade. Pooling across all four decades of Census data, the average probability of migrating for whites was about 14%; for blacks, it was twice as high at about 28%. Including covariates (which are not race-specific) increases the coefficient on Black, most likely reflecting racial differences in educational attainment over time.[16] The decade-specific regressions show that the white migration rate increased from about 15% in 1940 to 17% in 1950, leveling off around 19% during the 60s and 70s. The black-white difference in migration rates increased as well, increasing from 8% in 1940 to nearly 16% during the 60s and 70s. Within decades, the trend in black-white differences in migration rates are similar. Thus, Figure 3.3 and Table 3.1 demonstrate that both groups migrated North with probability less than one half and that blacks were substantially more likely to migrate than whites, both overall and within age-education strata.

Table 3.2 presents regression estimates of the black-white difference in North-South (annual) wage and log wage differentials over the same periods.[17] The top panel of the table shows the log wage results. On average between 1940 and 1970, black wages were 70 log points smaller than white wages, white migrants earned wages that were 25 log points higher than whites working in the South, and black migrants earned wages that were another 34 log points higher. Conditioning on age and education reduces each of these differentials: the black-white wage gap drops to 43 log points, the North-South

15 I classify states as Southern using the Census Bureau's definition of the South: Alabama, Arkansas, Delaware, Florida, Georgia, Kentucky, Louisiana, Maryland, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Virginia and West Virginia are Southern states. I define the North as any other state in the US. Although the Great Migration started well-before 1940, that decade is the first for which wage data are available.

16 This hypothesis is supported by the fact that, when covariates are included in the regression, the coefficient on black decreases between 1960 and 1970, a period of large gains in black relative educational attainment (Smith and Welch, 1989). As the estimates of the model without covariates shows, the raw difference in migration rates was stable during this period.

17 The wage measure consists of all income from wages and salary in the year before enumeration (this variable is named INCWAGE in the IPUMS dataset). The self-employed are included but business and farm income are not. All wages are inflated to 1999 dollars using the CPI weights supplied with the IPUMS. To make the wage and log wage regressions comparable, I restrict the sample to include only those reporting nonzero wages. In addition, to focus on men who are likely to have completed educational histories, I restrict the estimation sample to those older than 30.

wage gap drops to 22 log points and the black-white difference in this gap drops to 24 log points. The within-decade results are similar. The raw and covariate-adjusted racial log wage gaps drop from 75 and 44 in 1940 to 63 and 43 in 1970 (both of these gaps actually increase between 1950 and 1960, providing an explanation for this trend is beyond the scope of this paper); the raw and adjusted white North-South log wage gaps drop from 32 and 23 in 1940 to 21 and 17 by 1970. In addition, the black-white difference in North-South wage gaps increase from 30 (without covariates) and 20 (with covariates) log points to 37 and 27 in 1950, then decrease to 31 and 23 log points in 1970. These descriptive results are similar to those reported by Smith and Welch (1989) and Donohue and Heckman (1991).

The bottom panel of Table 3.2 presents the results for wages in levels. On average between 1940 and 1970, blacks earned \$12,700 less than whites (in 1999 dollars), a figure which drops to \$8,300 after accounting for age and education. White migrants earned \$5,400 more than whites working in the South, or \$4,600 after controlling for covariates. Black migrants earned an additional \$2,400 compared to blacks living in the South, or \$1,100 with covariates. The results are similar within and across decades, with the racial gap in absolute North-South wage differentials increasing from \$374 when covariates are excluded and -\$509 when they are included to around \$4000 and \$2000, respectively, between 1940 and 1970. Regardless of how wages are measured, these difference-in-differences estimates show that the North-South wage differential was substantially higher for blacks than for whites, over time and across covariate strata.

3.4.3 *Applying the identification results*

Suppose, as in the above caveat from Smith and Welch (1989), that the North-South wage differentials detailed in Table 3.2 represent a combination of the treatment effect of migration on migrants' wages and selection bias arising because those with greater skill find migration less costly, more beneficial, or both, and thus are more likely to migrate. Put differently, equilibrium wages and migration behavior are determined by a Roy model, in which case the identification results developed in this paper may be applicable.

First, suppose that the pure selection model of Section 3.2 provides a reasonable approximation to the data-generating process for migration and wages. That is, at least within covariate strata, region-specific wages are a linear function of skill and individuals migrate if their skill exceeds a threshold beyond which migrating is more attractive than remaining in the South. Since, as evidenced in Table 3.1, the black and white migration rates are less than one-half and the black migration rate exceeds the white rate, the difference-in-differences estimates presented in Table 3.2 can be interpreted as lower bounds on the black-white difference in treatment effects as long as the skill distribution satisfies one of the sufficient conditions given in Proposition 3.2. For example, if the skill distribution is lognormal, then the black-white differences in both the absolute and proportional average effects of migration on migrants' wages are bounded from below by differences-in-differences estimates. In this case, the estimates presented in Table 3.2 imply that, on average between 1940 and 1970, whatever the proportional change that a white Southerner would have experienced as a consequence of migrating to the North, a black Southerner would have experienced an increase in wages that was at least 24% greater, after accounting for the variation in wages explained by age and educational differences between migrants and nonmigrants. Similarly, whatever the absolute effect of migrating on the white Southerner's wages was, the wages of his black counterpart would have increased by at least an additional $1,100.

While the assumption imposed by the pure selection model that wages are linear in unobserved skill is comparable to the assumption, invoked in standard sample selection and switching regression estimators, of an additively separable error term, it may seem an oversimplification to suppose that skill is the only random determinant of migration behavior. However, if the linearity assumptions are reasonable, the selection model may be weakened according to the discussion in Section 3.3.4 to allow the migration decision to depend on a general random preference component. As long as the expectation of skill given the random preference component is linear in that component and the distribution of the random component satisfies one of the criteria listed in Proposition 3.2, the above interpretation of the differences-in-differences estimates of Table 3.2 remains applicable.

If the functional form assumptions of the pure selection model are incorrect, these differences-in-differences may still have a causal interpretation as long as the relationship between migration and wages can be viewed through the lens of the noisy selection model of Section 3.3. This less restrictive model allows for the wage and selection equations to depend on unobserved skill in very general ways, as long as the observed black-white difference in migration probabilities can be modeled as a function of a single preference parameter and the error term in the selection equation satisfies mild regularity conditions. The more stringent identification requirement is that, given selection and wage equations and a distribution for the random preference terms, the skill distribution meets the covariance condition (3.13).

Of course, because this condition places restrictions on the distribution of an unobserved random variable, it is not directly testable. Education, which is often viewed as a proxy for skill, is observed, allowing me to implement the falsification test developed in Section 3.3.4. I conduct two versions of this test. In the first, I estimate the covariance between wages and the frequency estimate (3.14) of discrete changes in the probability of migrating conditional on educational attainment for those working in the South. In the second, I estimate a logit model in which the migration decision depends on quadratics in age and education. I then estimate the covariance between wages and the estimated derivative of the migration probability, $\Lambda(x_i\hat{\beta})[1-\Lambda(x_i\hat{\beta})]$ where Λ is the logistic density and $\hat{\beta}$ are the logit estimates, among those working in the South. Because discrimination and other socioeconomic factors may have altered the relationship between skill and education among blacks, reducing the fidelity of education as a proxy for unobserved skill, I estimate these covariances using samples of white men. I perform separate tests for absolute wages and log wages by decade.

The results of the falsification tests are presented in Table 3.3. In each decade, the empirical covariance between log wages in the South and the frequency estimate of the migration density is close to zero. The empirical covariance between absolute wages and this estimate is negative in each decade. The bottom panel of the table shows the results of the test when the migration density is estimated using a logit (the logit parameter estimates are given in Table 3.4). For log wages, the empirical covariances are positive but close to zero. For absolute wages, the empirical covariances are positive and numerically larger, casting doubt on the validity of the identification procedure when wages are measured in levels. Still, given the scale of wages, even these covariances, which are on the order of about 100, are comparatively small. Thus, the results of both sets of falsification tests are consistent with the hypothesis that, on average over the covariate distribution, the covariance between skill and the treatment density is nonpositive. This, in turn, is consistent with the hypothesis that this covariance is nonpositive within covariate cells. Consequently, there is no evidence that the difference-in-difference estimates presented above cannot be interpreted as lower bounds on the black-white difference in the impact of migration on wages when the wage and selection equations depend on skill in a nonlinear way. At the same time, this does not imply that the covariance condition (and hence the identification result) holds, since, for reasons outlined in Section 3.3.3, these falsification tests cannot detect all failures of the covariance condition.

3.5 CONCLUSION

The identification results developed in this paper demonstrate that group differences in treated-untreated average outcome differences can often be interpreted as a lower bound on the difference between the average causal effect of the treatment on the treated for the group with the higher treatment rate and the group with the lower rate. When outcomes are a linear function of an unobserved type that also determines enrollment, the bounding result applies when when the type is drawn from a large class of distributions commonly used in econometric modeling and group differences in treatment rates meet verifiable restrictions. The result can also be applied when there are additional random determinants of the enrollment decision. When the unobserved type enters the selection and outcome equations nonlinearly, the requirements for identification are more stringent, and may not be acceptable on purely theoretical grounds. However, if the empiricist has access to a proxy for the unobserved type, a weak falsification test can be used to determine whether the data are inconsistent with these additional requirements.

Although it is not the typical estimand of treatment effect studies, the group difference in average treatment effects is particularly informative when researchers are interested in treatment effect heterogeneity or when it is believed that the average treatment effect is nonzero for both groups (since in this case a lower bound on the difference in treatment effects is also a lower bound on the treatment effect itself for the high-treatment-rate group). Moreover, since this difference is identified under comparatively mild conditions, it may also be of interest when there are no exogenous sources of variation in treatment status, or as a consistency check on treatment effect estimates obtained using other methods.

I use these results to impose a causal interpretation on regional wage differentials during the Great Migration. Southern-born blacks migrated North at higher rates than their white counterparts, at least between 1940 and 1970, the period that I study, and earned wages that were about 23% higher, on average after controlling for age and education. If selection and wages are well-approximated by the pure selection model presented in Section 3.2, this cross-sectional difference-in-differences estimate implies that migration increased wages for blacks by at least 23% more than for whites. I also provide evidence that the data are consistent with the more rigorous requirements of the general noisy selection model of Section 3.3, implying that black-white differences in North-South wage differences retain the same interpretation even under less restrictive assumptions on the enrollment and outcome processes.

The Great Migration neatly illustrates the applicability of the identification results. Since Northward migration was an important instrument for black relative economic progress, and since the causal effect of migration on wages may reflect a combination of factors including productivity, regional amenity differences and discrimination, the black-white difference in the effect of migration on migrants' wages is of direct interest. In fact, at least to an approximation, it may be interpreted as representing the contribution of regional differences in racial discrimination to the impact of migration on the wages of black migrants. Additionally, historical wage microdata are scarce and difficult to connect with ancillary datasets that contain information about plausibly exogenous sources of variation in the propensity to migrate.[18] My identification results, in concert with standard economic models of migration behavior and wage determination, allow me to draw inferences about the causal effects of the Great Migration from the available data.

18 Collins and Wanamaker (2014) use linked Census data and imputed wages constructed from occupational averages to provide evidence on the causal effect of migration on black migrants' wages during earlier waves of the Great Migration. That they find little evidence of selective migration is consistent with my interpretation of black-white differences in regional wage differentials, although no direct comparison can be made since they only examine wages for blacks.

3.A PROOFS

3.A.1 *Proof of Proposition 3.3*

Proposition. *Suppose that a is distributed over $[L, H]$ and*

$$\left|\frac{[\log f(a)]''}{\left\{[\log f(a)]'\right\}^2}\right|' \lesseqgtr 0 \quad \textit{when} \quad f'(a) \lesseqgtr 0. \tag{3.16}$$

1. *If $f(a)$ is log concave and $\lim_{a\to L} f(a) = \lim_{a\to H} f(a) = 0$, then $E(a|a \geq \hat{a})$ is convex and $E(a|a < \hat{a})$ is concave.*

2. *If $f(a)$ is log convex and $\lim_{a\to H} f(a) = 0$ then $E(a|a \geq \hat{a})$ is concave.*

The proof relies on an extension of the Prékopa-Borell theorem (Prékopa, 1971, 1973; Borell, 1975) due to Mares and Swinkels (2011).[19] Define the local ρ-concavity of $g(a)$ at a by

$$\rho_g(a) = 1 - \frac{g(a)g''(a)}{[g'(a)^2]}.$$

The justification for this definition is that if the local ρ-concavity of $g(a)$ at a is t, then g^t/t is linear at a. Mares and Swinkels (2011, Theorem 2) show that the local ρ-concavity of g can be used to bound the local ρ-concavity of the function $\bar{G}(a) = \int_a^1 g(t)dt$. In the course of establishing this result, they also provide the following corollary:

Corollary (Mares and Swinkels, 2011)**.** *If on some interval $[\tilde{a}, 1]$, g is decreasing and $g(1) = 0$ while ρ_g is decreasing, then $\rho_{\bar{G}}$ is decreasing on $[\tilde{a}, 1]$. If g is decreasing, while ρ_g is increasing on $[\tilde{a}, 1]$, then $\rho_{\bar{G}}$ is increasing on $[\tilde{a}, 1]$.*

Proof of Proposition 3.3. For the log concave case, I prove the result for $E(a|a \geq \hat{a})$. That $-E(a|a < \hat{a})$ is convex follows by analogy. First, note that, since $E(a|a \geq \hat{a}) - \hat{a} = [\int_{\hat{a}}^H 1 - F(t)dt]/[1 - F(\hat{a})]$ (this follows from integration by parts, see Bagnoli and Bergstrom, 2005), we can write

$$E(a|a \geq \hat{a})' = \frac{f(\hat{a})}{1 - F(\hat{a})} \frac{\int_{\hat{a}}^H 1 - F(t)dt}{1 - F(\hat{a})}.$$

Since

$$\rho_{\int 1-F}(\hat{a}) = 1 - \frac{f(\hat{a}) \int_{\hat{a}}^H 1 - F(t)dt}{[1 - F(\hat{a})]^2},$$

19 See Caplin and Nalebuff (1991) for an introduction to ρ-concavity and the Prékopa-Borell theorem.

$E(a|a \geq \hat{a})$ convex is equivalent to $\rho'_{\int 1-F}(\hat{a}) \leq 0$. By the corollary, since $1 - F(\hat{a})$ is decreasing, $\rho'_{1-F}(\hat{a}) \leq 0$ implies $\rho'_{\int 1-F}(\hat{a}) \leq 0$. Because log concave densities are unimodal (see An, 1995), $\rho_{1-F}(\hat{a})' \leq 0$ whenever $\hat{a}$ is less than or equal to the mode of a, since

$$\rho_{1-F}(\hat{a}) = 1 - \frac{[-f'(\hat{a})][1-F(\hat{a})]}{[f(\hat{a})]^2} = 1 + \frac{f'(\hat{a})[1-F(\hat{a})]}{[f(\hat{a})]^2}$$

and, when $f'(\hat{a}) > 0$, $f'(\hat{a})/f(\hat{a})$ and $[1-F(\hat{a})]/f(\hat{a})$ are positive and, by log concavity, they are always decreasing.

When a exceeds the mode, so that $f'(\hat{a}) < 0$, we can apply the corollary once again in order to infer the sign of $\rho'_{1-F}(\hat{a})$ from that of $\rho'_f(\hat{a})$. Noting that, since $f(\hat{a})$ is log concave, it can be written $f(\hat{a}) = \exp[h(\hat{a})]$ where $h(\hat{a})$ is a concave function,

$$\rho_f(\hat{a}) = 1 - \frac{f''(\hat{a})f(\hat{a})}{[f'(\hat{a})]^2} = 1 - \frac{\exp[h(\hat{a})]\left\{\exp[h(\hat{a})]h'(\hat{a})^2 + \exp[h(\hat{a})]h''(\hat{a})\right\}}{\left\{\exp[h(\hat{a})]h'(\hat{a})\right\}^2} = -\frac{h''(\hat{a})}{[h'(\hat{a})]^2}.$$

Since log concavity implies $h''(\hat{a}) < 0$, $-h''(\hat{a})/[h'(\hat{a})]^2$ is positive and (weakly) decreasing, so $\rho_f(\hat{a})' \leq 0$, implying that $\rho_{1-F}(\hat{a})' \leq 0$ and hence $\rho_{\int 1-F}(\hat{a})' \leq 0$, establishing the result.

For the log convex case, note that if $f(\hat{a})$ is log convex then $f'(\hat{a})/f(\hat{a})$ is increasing and, since $f(H) = 0$ implies that $1 - F(\hat{a})$ is also log convex (see Theorem 2 of Bagnoli and Bergstrom, 2005), $[1-F(\hat{a})]/f(\hat{a})$ is increasing as well. Thus, $\rho_{1-F}(\hat{a})$, and consequently $\rho_{\int 1-F}(\hat{a})$ are positive and increasing when $f'(\hat{a}) > 0$. When $f'(\hat{a}) < 0$, by the conditions of the proposition, we have $h''(\hat{a})/[h'(\hat{a})]^2$ positive and decreasing, so that $\rho_f(\hat{a})$, $\rho_{1-F}(\hat{a})$ and hence $\rho_{\int 1-F}(\hat{a})$ are increasing, implying that $E(a|a \geq \hat{a})$ is concave. □

3.A.2 *Proof of Proposition 3.4*

Proposition. *Suppose that $F(\epsilon)$ is log concave and that members of group r receive treatment with probability less than 1/2. Then*

$$\frac{\partial}{\partial \Delta_r}\left[P(h|r,1) - P(h|r,0)\right] \leq 0$$

if

$$f(\Delta_r + \gamma) - f(\Delta_r) \leq 0.$$

Proof. Dropping the r subscripts for simplicity and applying Bayes' rule,

$$P(h|1) = \frac{\pi F(\Delta+\gamma)}{\pi F(\Delta+\gamma) + (1-\pi)F(\Delta)}$$

and

$$P(h|0) = \frac{\pi\left[1 - F(\Delta+\gamma)\right]}{\pi\left[1-F(\Delta+\gamma)\right] + (1-\pi)\left[1-F(\Delta)\right]}.$$

Differentiating,

$$\frac{\partial P(h|1)}{\partial \Delta} = \frac{\pi(1-\pi)}{P(1)^2}\left[F(\Delta)f(\Delta+\gamma) - F(\Delta+\gamma)f(\Delta)\right] \equiv \frac{\pi(1-\pi)}{P(1)^2}c_1$$

and

$$\frac{\partial P(h|0)}{\partial \Delta} = \frac{\pi(1-\pi)}{P(0)^2}\left\{[1-F(\Delta+\gamma)]\,f(\Delta) - [1-F(\Delta)f(\Delta+\gamma)]\right\} \equiv \frac{\pi(1-\pi)}{P(0)^2}c_0,$$

where $P(1) = 1 - P(0) = \pi F(\Delta+\gamma) + (1-\pi)F(\Delta)$.

Notice that $\partial P(h|n)/\partial\Delta$ (and hence c_1) have the same sign as

$$\frac{f(\Delta+\gamma)}{F(\Delta+\gamma)} - \frac{f(\Delta)}{F(\Delta)} < 0,$$

where the inequality is a consequence of the log concavity of F, which implies that $(f/F)' < 0$.

Since $P(1) < P(0)$ and $c_1 < 0$,

$$\begin{aligned}\frac{\partial P(h|1)}{\partial \Delta} - \frac{\partial P(h|0)}{\partial \Delta} &= \pi(1-\pi)\left[\frac{c_1}{P(1)^2} - \frac{c_0}{P(0)^2}\right] \\ &< \frac{\pi(1-\pi)}{P(0)^2}\left(c_1 - c_0\right) \\ &= \frac{\pi(1-\pi)}{P(0)^2}\left[f(\Delta+k) - f(\Delta)\right],\end{aligned}$$

proving the proposition. □

3.A.3 *Proof of Proposition 3.5*

Proposition. *Suppose that $F(\epsilon)$ is log concave, $\lim_{\epsilon\to\infty} f(\epsilon) = 0$, and that members of group r receive treatment with probability less than 1/2. Then*

$$\frac{\partial}{\partial \Delta_r}\int \left[P(a|r,1) - P(a|r,0)\right] y_{0r}(a)da \leq 0$$

if

$$Cov\left[f\left(\Delta_r + \gamma(a)\right), y_{0r}(a)\right] \leq 0.$$

Proof. First, note that, dropping the r indices for notational simplicity,

$$\frac{\partial P(a|1)}{\partial \Delta} = \frac{\partial}{\partial \Delta}\frac{\pi(a)F\left(\Delta+\gamma(a)\right)}{\int \pi(a)F\left(\Delta+\gamma(a)\right)da} = \frac{c_1(a)}{P(1)^2}$$

and

$$\frac{\partial P(a|0)}{\partial \Delta} = \frac{\partial}{\partial \Delta} \frac{\pi(a)\left[1 - F\left(\Delta + \gamma(a)\right)\right]}{\int \pi(a)\left[1 - F(\Delta + \gamma(a)\right] da} = \frac{c_0(a)}{P(0)^2}$$

where $P(1) = 1 - P(0) = \int \pi(a) F\left(\Delta + \gamma(a)\right) da$,

$$c_1(a) \equiv \pi(a) f\left(\Delta + \gamma(a)\right) \int \pi(a) F(\Delta + \gamma(a)) da \\ - \pi(a) F\left(\Delta + \gamma(a)\right) \int \pi(a) f\left(\Delta + \gamma(a)\right) da,$$

and

$$c_0(a) \equiv -\left[\int \pi(a)\left[1 - F\left(\Delta + \gamma(a)\right)\right] da\right] \pi(a) f\left(\Delta + \gamma(a)\right) \\ + \pi(a)\left[1 - F\left(\Delta + \gamma(a)\right)\right] \int \pi(a) f\left(\Delta + \gamma(a)\right) da.$$

Second, observe that $c_1(a)$ has the same sign as

$$\frac{f\left(\Delta + \gamma(a)\right)}{F\left(\Delta + \gamma(a)\right)} - \frac{E\left[f\left(\Delta + \gamma(a)\right)\right]}{E\left[F\left(\Delta + \gamma(a)\right)\right]}. \tag{3.17}$$

Since $F(\epsilon)$ is log concave with $\lim_{\epsilon \to \infty} f(\epsilon) = 0$, f/F decreases monotonically to zero. Furthermore, since (3.17) determines the sign of $c_1(a)$ and $E[f(\Delta + \gamma(a)]/E[F(\Delta + \gamma(a)] \geq 0$ is constant, there exists an a^* such that $c_1(a) \lesseqqgtr 0$ as $a \gtreqqless a^*$.

Third, since $y_0'(a) > 0$ and

$$\int c_1(a) da = E\left[f\left(\Delta + \gamma(a)\right)\right] E\left[F\left(\Delta + \gamma(a)\right)\right] - E\left[F\left(\Delta + \gamma(a)\right)\right] E\left[f\left(\Delta + \gamma(a)\right)\right] = 0,$$

we must have

$$\int c_1(a) y_0(a) da < \int_{-\infty}^{a^*} c_1(a) y_0(a^*) da + \int_{a^*}^{\infty} c_1(a) y_0(a^*) da < y_0(a^*) \int c_1(a) da = 0,$$

where a^* is the unique solution to $c_1(a^*) = 0$. As long as limit operations can be interchanged, this implies that

$$\begin{aligned} \frac{\partial}{\partial \Delta} \int \left[P(a|1) - P(a|0)\right] y_0(a) da &= \int \left[\frac{\partial P(a|1)}{\partial \Delta} - \frac{\partial P(a|0)}{\partial \Delta}\right] y_0(a) da \\ &= \int \left[\frac{c_1(a)}{P(1)^2} - \frac{c_0(a)}{P(0)^2}\right] y_0(a) da \\ &< \frac{1}{P(0)^2} \int \left[c_1(a) - c_0(a)\right] y_0(a) da \end{aligned}$$

since $P(1) < P(0)$.

Finally, the sign of the last expression is the same as that of

$$\begin{aligned}\int \left[c_1(a) - c_0(a)\right] y_0(a) &= \int \left[\pi(a) f\left(\Delta + \gamma(a)\right) da + \pi(a) \int \pi(a) f\left(\Delta + \gamma(a)\right) da\right] y_0(a) da \\ &= E\left[f\left(\Delta + \gamma(a)\right) y_0(a)\right] - E\left[y_0(a)\right] E\left[f\left(\Delta + \gamma(a)\right)\right] \\ &= Cov\left[f\left(\Delta + \gamma(a)\right), y_0(a)\right].\end{aligned}$$

□

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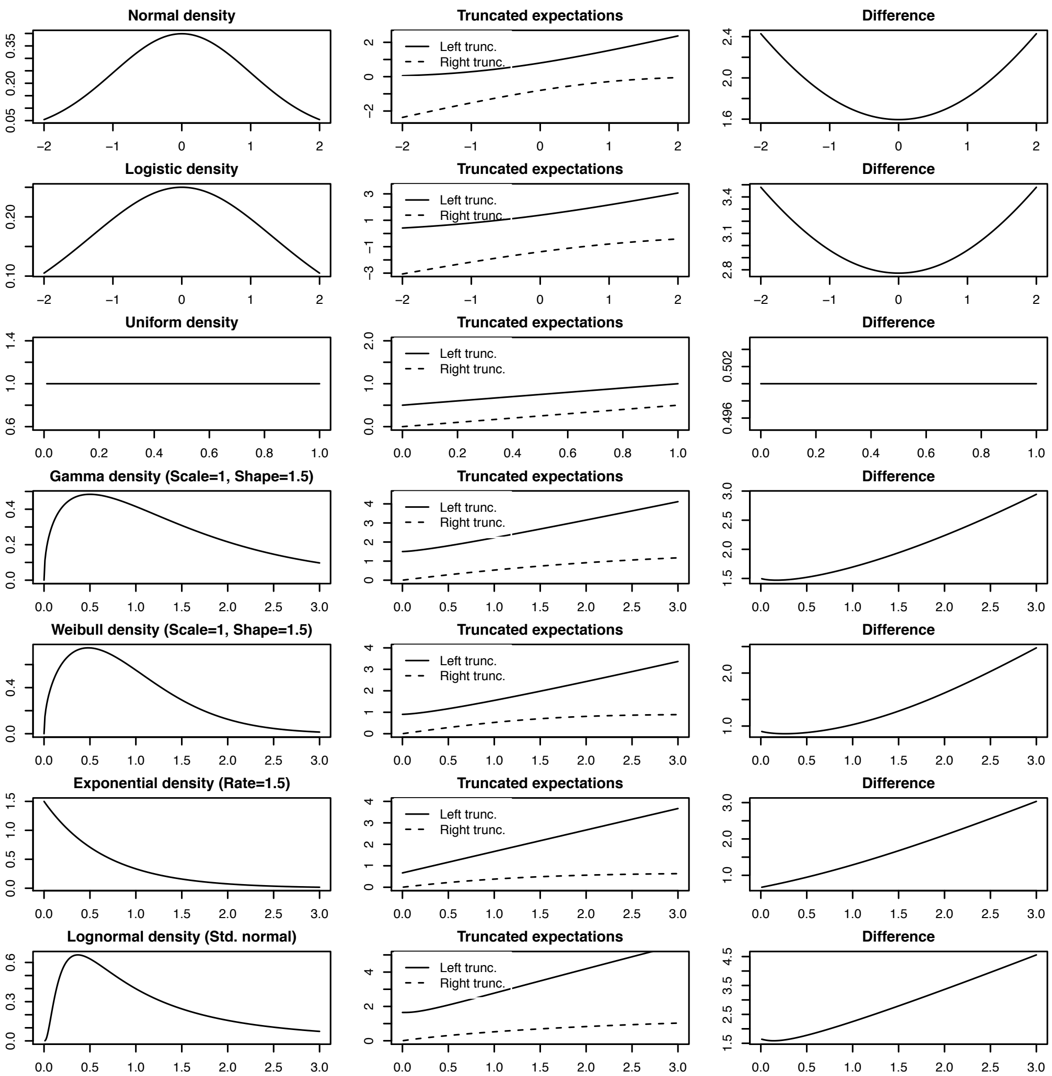
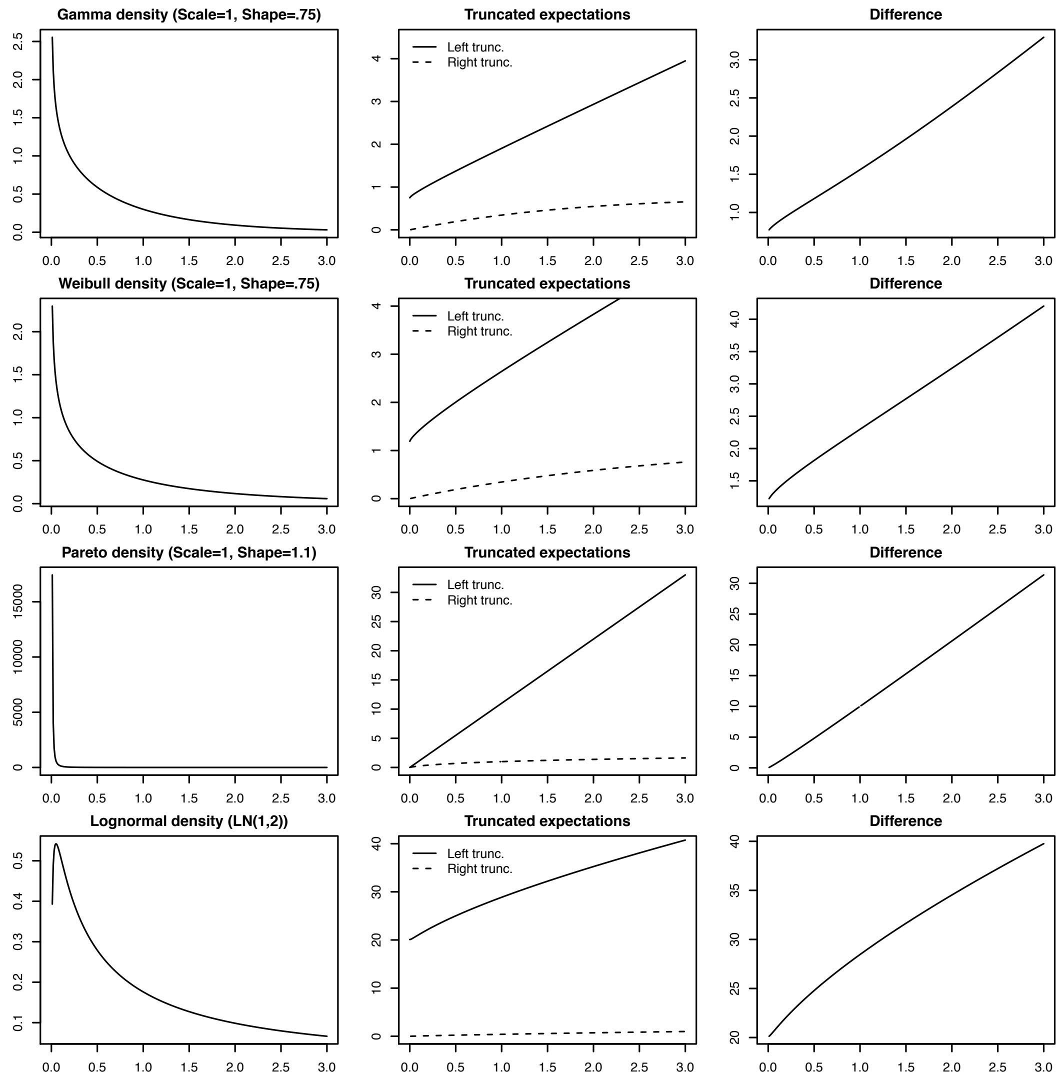
TABLES AND FIGURES

Figure 3.1: Distributions with convex (concave) left- (right-) truncated expectations



Notes—Difference denotes $E(a|a \geq \hat{a}) - E(a|a < \hat{a})$. Density formulae taken from Bagnoli and Bergstrom (2005). Expressions for the left- and right-truncated moments of the normal, logistic, gamma, Weibull and lognormal densities can be found in Arabmazar and Schmidt (1982), Heckman and Honore (1990) and Jawitz (2004). By direct calculation, if $a \sim U[0,1]$ then $E(a|a \geq \hat{a}) = 1/2 + \hat{a}/2$ and $E(a|a < \hat{a}) = \hat{a}/2$. If a is exponential with rate parameter λ, then it can be shown (integrate by parts and apply L'Hôpital's rule) that $E(a|a \geq \hat{a}) = 1/\lambda + \hat{a}$ and $E(a|a < \hat{a}) = 1/\lambda - \hat{a}/(e^{\lambda\hat{a}} - 1)$ (see also Head, 2011).

Figure 3.2: Distributions with concave truncated expectations



Notes—Difference denotes $E(a|a \geq \hat{a}) - E(a|a < \hat{a})$. Density formulae taken from Bagnoli and Bergstrom (2005). Expressions for the left- and right-truncated moments of the gamma, Weibull and lognormal densities can be found in Jawitz (2004). If a is Pareto distributed with shape parameter β then it can be shown that $E(a|a \geq \hat{a}) = \beta\hat{a}/(\beta - 1)$ and $E(a|a < \hat{a}) = [\beta/(\beta-1)](1-\hat{a}^{1-\beta})/(1-\hat{a}^{-\beta})$ (see also Head, 2011).

Figure 3.3: Migration rates by year of birth

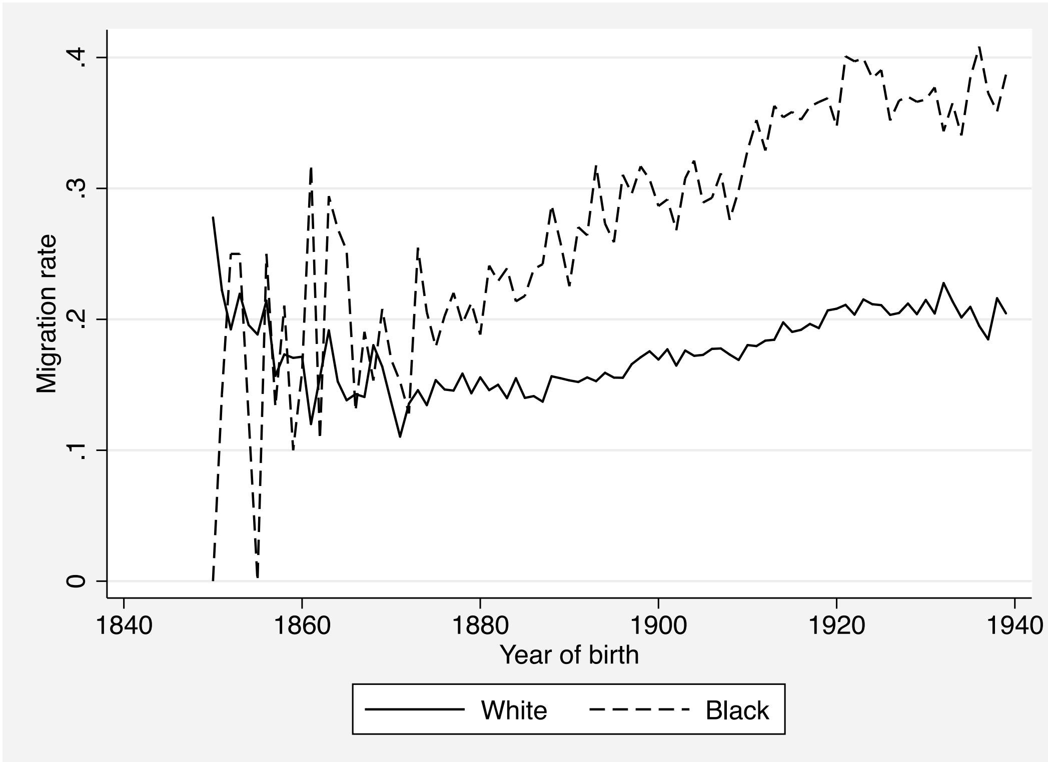


Table 3.1: Migration rates

	All decades		1940		1950		1960		1970	
Black	0.137***	0.173***	0.0805***	0.134***	0.144***	0.188***	0.156***	0.189***	0.156***	0.180***
	(0.00797)	(0.00803)	(0.00973)	(0.00999)	(0.0115)	(0.0117)	(0.00993)	(0.00990)	(0.00906)	(0.00917)
Constant	0.139***	-0.0177	0.152***	-0.0313	0.170***	-0.00573	0.193***	0.0148	0.191***	0.0563
	(0.00911)	(0.0331)	(0.00897)	(0.0528)	(0.0103)	(0.0783)	(0.0113)	(0.0651)	(0.0109)	(0.0643)
Covariates	N	Y	N	Y	N	Y	N	Y	N	Y
Observations	365,785	365,785	82,226	82,226	31,297	31,297	122,638	122,638	129,624	129,624
R-squared	0.023	0.043	0.008	0.049	0.023	0.050	0.024	0.044	0.023	0.037

Notes—Covariates include indicators for age, educational attainment, and in pooled models, decade. Standard errors clustered on state-year of birth. Sample consists of Southern-born black and white men greater than 30 years of age. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 3.2: Difference-in-difference regressions

	Log wage									
	All decades		1940		1950		1960		1970	
Black	-0.697***	-0.428***	-0.755***	-0.440***	-0.653***	-0.419***	-0.733***	-0.484***	-0.631***	-0.432***
	(0.00876)	(0.00882)	(0.0148)	(0.0143)	(0.0221)	(0.0212)	(0.0128)	(0.0109)	(0.0116)	(0.00966)
North	0.248***	0.216***	0.318***	0.231***	0.289***	0.250***	0.250***	0.212***	0.207***	0.165***
	(0.00972)	(0.0146)	(0.0167)	(0.0146)	(0.0208)	(0.0175)	(0.0140)	(0.00876)	(0.0143)	(0.00813)
Black*North	0.339***	0.236***	0.294***	0.203***	0.374***	0.273***	0.359***	0.248***	0.314***	0.228***
	(0.0120)	(0.0136)	(0.0227)	(0.0210)	(0.0317)	(0.0304)	(0.0179)	(0.0156)	(0.0175)	(0.0152)
Covariates	N	Y	N	Y	N	Y	N	Y	N	Y
Observations	249,591	249,591	49,013	49,013	20,785	20,785	85,547	85,547	94,246	94,246
R-squared	0.254	0.272	0.105	0.244	0.078	0.188	0.098	0.258	0.076	0.266

	Wage									
	All decades		1940		1950		1960		1970	
Black	-12,695***	-8,257***	-7,807***	-4,214***	-8,608***	-5,289***	-13,326***	-8,351***	-16,502***	-10,593***
	(160.8)	(128.7)	(122.4)	(112.1)	(188.4)	(176.7)	(158.6)	(130.4)	(236.6)	(187.9)
North	5,403***	4,598***	3,480***	2,594***	3,649***	3,166***	5,244***	4,699***	6,676***	5,712***
	(248.1)	(171.7)	(229.2)	(187.7)	(295.8)	(249.8)	(280.7)	(198.2)	(391.9)	(256.5)
Black*North	2,380***	1,141***	374.2	-509.3**	2,361***	1,233***	2,884***	1,040***	4,148***	2,138***
	(260.4)	(220.0)	(245.2)	(218.2)	(363.7)	(345.4)	(294.8)	(252.9)	(415.9)	(350.2)
Covariates	N	Y	N	Y	N	Y	N	Y	N	Y
Observations	249,591	249,591	49,013	49,013	20,785	20,785	85,547	85,547	94,246	94,246
R-squared	0.227	0.371	0.103	0.684	0.097	0.756	0.098	0.762	0.071	0.752

Notes—Covariates include indicators for age, educational attainment, and in pooled models, decade. Sample consists of Southern-born black and white men greater than 30 years of age. Wage is defined as all income from wages in the year before enumeration, and the sample is restricted to those with nonzero wages. Standard errors clustered on state-year of birth. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 3.3: Covariance tests

	Frequency estimator			
	1940	1950	1960	1970
Log wage	0.000	0.000	-0.001	-0.001
Wage	-4.108	-8.990	-17.491	-28.169

	Logit			
	1940	1950	1960	1970
Log wage	0.011	0.007	0.005	0.004
Wage	116.698	97.992	79.409	106.361

Notes—Frequency estimates computed using the sample analogs of $\Pr(\text{North}|\text{Ed.} = e) - \Pr(\text{North}|\text{Ed.} = e - 1)$. Logit estimates computed using $\Lambda(x'\beta)[1 - \Lambda(x'\beta)]$ where Λ is the logistic CDF and x includes quadratics in age and education. All estimates are derived from samples of whites.

Table 3.4: Logit estimates

	1940	1950	1960	1970
Age	0.0243***	0.0151**	0.00216	0.0236***
	(0.00488)	(0.00751)	(0.00361)	(0.00333)
Age^2	-0.000173***	-0.000149**	-7.67e-05**	-0.000274***
	(4.92e-05)	(7.52e-05)	(3.57e-05)	(3.27e-05)
Ed	0.316***	0.180***	0.161***	0.0781***
	(0.0156)	(0.0216)	(0.0106)	(0.0106)
Ed^2	-0.0114***	-0.00553***	-0.00604***	-0.00219***
	(0.000724)	(0.000993)	(0.000480)	(0.000459)
Constant	-4.307***	-3.097***	-2.278***	-2.468***
	(0.141)	(0.211)	(0.103)	(0.101)
Observations	76,594	28,524	109,323	118,250

Notes—Logit models of migration among Southern-born whites. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.