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THE GREAT ESCAPE: INTERGENERATIONAL MOBILITY IN THE UNITED STATES SINCE 1940

Nathaniel G. Hilger

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The Great Escape: Intergenerational Mobility in the United States Since 1940
Nathaniel G. Hilger
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ABSTRACT

I develop a new method to estimate intergenerational mobility (IM) in educational attainment on U.S. census data spanning 1940-2000. I measure IM directly for children still living with parents at ages 26-29, and indirectly for other children using an imputation procedure that I validate in multiple datasets spanning the full sample period. Educational IM increased significantly 1940-1970 and declined after 1980. Post-1940 IM gains were economically large, driven by high school rather than college enrollment, and were larger for blacks primarily due to all-race IM gains in the South. I discuss potential causes of these patterns.

Nathaniel G. Hilger
Department of Economics
Brown University
64 Waterman Street
Providence, RI 02912
and NBER
nathaniel_hilger@brown.edu

1. Introduction

Intergenerational mobility (IM) is an important social objective for many individuals and policymakers, and may affect public attitudes toward other social objectives such as equality and growth (Piketty, 1995; Benabou and Ok, 2001; Corak, 2013). However, surprisingly little is known about IM variation over time, space and groups. The main empirical problem is that measuring IM requires data on labor market outcomes for both parents and children. No standard government data set has collected this information historically. Several panel data sets contain this information, but they begin in the 1960s and are too small to examine mobility over time or subgroups with precision (e.g., Lee and Solon, 2009). Tax records have greatly improved understanding of IM in the recent period but only link children beginning in the 1990s (Chetty et al., 2014a). The lack of reliable, longer-term trends both overall and for various subgroups is unfortunate because many interventions often thought to increase equality of opportunity such as the high school movement, early GI Bills, Great Society programs, several key Supreme Court decisions, and the Civil Rights movement all predate availability of most panel data sets.

In this paper I develop a new method to estimate IM statistics on U.S. census data back back to 1940. Prior research on IM has largely ignored census data. This is because the census only links parent and child outcomes while children still live with parents, and children rapidly become independent after age 17 but before any adult outcomes can be observed meaningfully (Cameron and Heckman, 1993). I develop a simple, semi-parametric adjustment for these "missing" independent children that allows me to estimate the conditional expectation function (CEF) of children's final educational attainment as of ages 26-29 (birth cohorts 1911-14 in 1940, and 1971-74 in 2000) with respect to parental income or education. Adopting the terminology of Chetty et al. (2014a), I define the intercepts and slopes of schooling gradients as measures of "absolute upward" and "relative" intergenerational educational mobility, respectively.[1] Below I show in a stylized economic model that these relative IM statistics are closely related to each other and to more traditional IM statistics based on children's earnings rather than children's schooling.

The adjustment for independent children rests on two simple and verifiable assumptions. To illustrate, consider a toy example with two parental groups in a fixed year. Let

[1]The terminology of "absolute upward mobility" is more appropriate when I measure education in ranks rather than levels.

children have either "low-income" or "high-income" parents labelled 0 and 1, respectively. Among 27-year-olds, I observe 100 children living with high-income parents, 100 with low-income parents and 300 living independently, with average highest grade attained of 14, 12 and 12, respectively. I therefore observe a schooling gradient intercept of 12 years of schooling and a slope of 2 years of schooling across parental groups, but only for *dependent* children. I need to know two things to account for the remaining 60% of children who are independent: their parental group composition, and their average schooling by group. I first make a "parallel trends" assumption that the schooling gradient among independent children has the same slope as the gradient among dependent children: here 2 years. Now observe that virtually 100% of children up through age 17 still live with their parents. Suppose I observe 200 high-income and 300 low-income 17-year-olds. Under a second "smooth cohorts" assumption that parental group shares do not change across cohorts, I infer that 100 of the independent 27-year-old children have high-income parents and 200 have low-income parents. Let h equal average schooling of low-income 24-year-old independent children. We can now solve for h: $12 = \frac{100}{300}(h+2) + \frac{200}{300}h \implies h = 11.33$. Total schooling of low-income children is therefore $\frac{100}{300} \cdot 12 + \frac{200}{300} \cdot 11.33 = 11.55$ and total schooling of high-income children is $\frac{100}{200} \cdot 14 + \frac{100}{200} \cdot 13.33 = 13.665$. The *total* schooling gradient therefore has intercept of 11.55 and slope of 2.11. Below I formalize these two assumptions of parallel trends and smooth cohorts, generalize the method to more than two groups, and use multiple datasets spanning the entire 1940-2000 period to validate both the two assumptions and the resulting gradients in the U.S. historical context.

The method opens up many new possibilities for research on IM over time, space and subgroups due to the widespread availability of census data. As a first application, I examine long-term IM trends in the U.S. by race and state-of-birth. The main new finding is that IM increased significantly in the U.S. after 1940.[2] These IM gains were economically large, plausibly increasing aggregate annual earnings growth by 0.25 percentage points over the 1940-70 period.[3] IM gains were similar for men and women. I replicate findings of lower IM in the South for both whites and blacks in recent years, but show these differences are small in historical context after many decades of regional IM convergence. Largely as a result of these regional trends, blacks achieved much larger IM gains than whites nationally. I also show that post-1940 IM gains were driven entirely by high school enrollment; college enrollment has actually detracted from IM gains since

[2]This result holds up for IM statistics relating children's education to parental education and parental income, both in levels and ranks, and for both regression coefficients and correlations.

[3]These gains are reminiscent of the gains in allocative efficiency from reductions in occupational barriers facing women and minorities after 1960 (Hsieh et al., 2013).

1940.

I examine several potential explanations for post-1940 IM gains. The results reject simple explanations based on the Great Migration, the GI Bills, the War on Poverty, the Civil Rights Acts. Temporary constraints on schooling during the Great Depression may account for part of the increase in IM after 1940 for whites but not for blacks. Using newly digitized data, I show that the late introduction of black high schools across southern states is unlikely to account for black mobility gains. Constructing a novel long-term panel dataset of IM by state of birth and year, I find evidence that "broad-based" economic growth, such as that experienced in the transformation of the U.S. Southern economy after 1940 (Wright, 1986), may play a role in the long-term evolution of IM. However, the divergence in high school and college enrollment mobility since 1940 also suggests a role for educational institutions such as private finance, active enrollment, and voluntary attendance.

2. Prior Literature

This study is the first to estimate educational IM on birth cohorts spanning 1911-1971 in a consistent fashion. While a growing literature examines mobility variation across space (Erikson and Goldthorpe, 1992; Jantti et al., 2006; Corak, 2006; Hertz et al., 2008; Chetty et al., 2014a), to my knowledge this is also the first study to estimate long-term trends in any type of mobility across regions and demographic subgroups in the U.S.

Research on IM trends has typically focused on income mobility. Hertz (2007), Lee and Solon (2009) and Harding et al. (2009) document that intergenerational elasticities of income have remained stable for children born between 1950 and 1970. Chetty et al. (2014b) document stable rank-rank income mobility for cohorts born between 1970 and 1990, and suggest that national IM statistics based on ranks and logs are likely comparable in practice, implying stable income mobility for cohorts born 1950-1990. Aaronson and Mazumder (2008) use a different method to estimate income mobility in census data back to 1940 by instrumenting for parental income with child's state of birth. In contrast with these other studies, they find that income mobility decreased sharply after 1980, while also finding that income mobility increased 1940-1980. As they acknowledge, their method yields biased results if places have causal effects on children's income not captured by parental income, as strongly indicated by recent work (Chetty et al., 2015; Chetty and Hendren, 2015) and by state-of-birth IM heterogeneity documented below.

Hertz et al. (2008) estimate educational IM in the U.S. for cohorts born as early as 1932, as well as for many other countries, using the World Bank Living Standards Measurement

Surveys. They find a gradual increase in IM according to intergenerational elasticities, but no change in intergenerational correlations. I find that IM increased significantly for cohorts born before 1932 in both elasticities and correlations, and I estimate trends for cohorts born after 1932 that are not consistent with their results. Several factors may explain this discrepancy. First, Hertz et al. (2008) estimate time trends off of cohort variation from individuals age 20-69 in surveys conducted 1994-2000. Their long-term trends are therefore subject to unknown bias from selective mortality attrition as well as changes in recall errors in own or parental education by age (Goldin, 1998). Hertz et al. (2008) are also unable to study sub-national IM variation across places or demographic groups due to sample size limitations.

Olivetti and Paserman (2015) and Clark (2014) estimate trends in occupational income mobility before 1940 using information about SES contained in children's first and last names, respectively. As these authors point out, this approach requires that instruments (state of birth, last name, first name) only affect child occupational income through parental characteristics, or that biases are constant over time.[4] The method here complements Olivetti and Paserman (2015) in the sense that names data are only available up through 1940, while income and education data begin in 1940. Finally, Long and Ferrie (2013a) have compared long-term changes in intergenerational occupational persistence using both census and Occupational Change in a Generation (OCG) data. As they point out (Long and Ferrie, 2013a, footnote 14), occupational categories–unlike income and educational attainment–cannot be ranked cardinally over long periods of time. Moreover, reliance on OCG data precludes analysis of many subgroups due to sample size limitations.

The method developed here can inform theoretical work on dynastic human capital investment and the distribution of income (e.g., Becker and Tomes, 1979; Loury, 1981), as well as macroeconomic models that link IM to other social objectives (e.g., Murphy et al., 1991; Galor and Tsiddon, 1997; Owen and Weil, 1998). These theories have relied on a limited set of moments from small panel datasets or administrative data from recent decades and high-income countries (Solon, 1999; Black and Devereux, 2011). IM estimates across a broader set of countries, regions, demographic groups, and time periods should help to refine these models.

In this paper I focus on two-generation mobility statistics. Such statistics are sufficient statistics for IM under the assumption that dynastic transmission of outcomes follows an AR(1) process. Recent work rejects the AR(1) transmission process due to mecha-

[4] This problem may be especially acute when focusing on subsets of unusual or prominent names (Chetty et al., 2014b, Appendix B). This is the case in Clark (2014), but not Olivetti and Paserman (2015).

nisms such as an inherited latent factor that is only partially reflected in socioeconomic outcomes, and direct grandparent effects (e.g., Clark, 2014; Olivetti et al., 2014; Stuhler, 2014; Braun and Stuhler, 2015; Solon, 2015). This literature implies that two-generation mobility statistics such as those estimated here should not be geometrically iterated to forecast long-term dynastic regression to the mean. Nonetheless, two-generation mobility statistics can still shed important light on social processes, and are likely to remain the primary measures of equality of opportunity in practice due to data limitations in much the same way that the GDP, Gini coefficients and poverty rates remain benchmark measures of social progress despite their many well-known flaws.[5]

Finally, Nybom and Stuhler (2014) show that events in prior generations should be expected to have long-lasting, possibly non-monotonic effects on two-generation mobility statistics. The key insight is that large institutional and technological changes affect mobility in two ways: they change structural links between parental SES and children's human capital, and they reshuffle winners and losers in ways that are not necessarily correlated with parental SES. The first effect permanently changes mobility statistics, but the second effect only temporarily changes mobility statistics in the generations in which parent and child outcomes emerge from different structural environments. Nybom and Stuhler (2014) point out that the temporary effect may take many decades to dissipate as dynasties reach steady states in which both parents and children grow up in the "new" environment. These insights can be kept in mind when I explore potential explanations for mobility trends below.

3. Data and Methodology

3.1. Data Sources and Variable Definitions

The decennial census is the only large-scale, nationally representative source of microdata on earnings and education before the 1960s in the U.S.[6] I rely primarily on census data from 1940-2000, when income and total years of education are both available.[7] I make use of recently-available 100% digitized versions of the 1930 and 1940 censuses both in the main results and in order to construct a panel of children in 1940 linked to their parental characteristics in 1930. Gradients cannot be constructed for the 1950 census

[5] In particular, recent research has not found support for the claim of Clark (2014) that variation in two-generation mobility statistics belies high levels of intergenerational transmission that are constant over time and place (e.g., Braun and Stuhler, 2015; Solon, 2015).

[6] The Annual Social and Economic Supplement of the Consumer Population Survey (the March CPS) begins in 1962 and excludes military and incarcerated individuals from its sample (Neal, 2006).

[7] All Census data sets obtained from Ruggles et al. (2010).

because only one individual per family received the census long form with questions about income and education.

"Educational attainment" is based on the more detailed IPUMS variable "EDUCD" and represents highest grade completed in all years. Categories change slightly over the 1940-2000 period. I count GEDs and regular high school degrees as 12 years of schooling, associate's degrees as 14 years of schooling, college degrees as 16 years, and graduate or professional school as 17 years. Results are not sensitive to counting GEDs as 11 years, or counting "some college" anywhere in the 13-15 range. For "children's education" I focus on ages 26-29. At these ages most children have completed education, and experimentation in panel datasets revealed that educational mobility statistics stabilize around these ages. For "parental education" I use average education of a child's mother and father, or education of the available parent in one-parent families.[8] I drop families with zero parental education. There are few such families, and inspection revealed that many of them likely represent measurement error. I also find that the bottom 2% of the parental education distribution, excluding zeros, often yields zero or wrong-signed associations with child outcomes, and I therefore drop these parents as well. Card and Krueger (1992a) also find that the bottom 2% of the education distribution behaves anomolously in a different application, and drop these observations from their sample.

I define parental income as the sum of mother's and father's labor earnings (all wage and salary income, tips, *etc.*). I exclude capital income because it is not available in 1940, apart from an indicator for the presence of capital income over $50. Parental earnings are missing or reported as zero for a significant share of families in many years. I exclude families with zero earnings from the baseline estimates of mobility with respect to parental earnings because zeros likely represent a combination of genuine zeros and measurement error, and the exact mix may vary across demographic groups and years. In the robustness section I show these choices do not drive any of the main results. Throughout the text I focus on parental earnings in deciles both to facilitate comparability across years, and because schooling gradients turn out to be more linear in parental income deciles than levels or logs.

I also incorporate data from the Panel Study of Income Dynamics (PSID), the National Longitudinal Survey of Youth 1979 and 1997 (NLSY79 and NLSY97), the Occupational Change in a Generation 1973 survey (OCG73), and the General Social Survey (GSS) both to assess the key "parallel trends" assumption underlying the empirical strategy, and to

[8] All main results are similar when I use mother's education, father's education, or head's education. Average of mother's and father's education is preferable because it incorporates maximum information about parental SES while also permitting inclusion of all family types.

compare IM estimates with those obtained from census data. The PSID, NLSY79 and NLSY97 are panel data sets that track children after they split into new households and go back to 1968, 1979 and 1997, respectively. The OCG73 is a cross-sectional data set that collected information on adults and their retrospective parental characteristics during adolescence. The GSS is an annual cross-sectional survey that collects retrospective information on parental income and education during adolescence, and begins in 1972 for the US.[9]

There is some ambiguity in dependent status of young adults in "group living" situations such as college dormitories, prisons, and military barracks in census data. Since 1850, instructions to enumerators (1850-1950) and to survey respondents (1960-2000) have indicated that children who are living away from home for college should be counted at their college residence and not as part of their family (e.g., Bureau of the Census, 1988; National Research Council, 2006, p. 47). Note that living away at colleges is not a major issue for my results because I focus on ages 26-29 in all of my main analysis, and almost no children live in college dormitories at these ages. However, prisons and military barracks may be important, especially for black men in more recent decades (Neal, 2006). For my main results I count all children living in dormitories, prisons and barracks as independents; in the robustness section I show results are similar if I omit these children from the analysis.

Educational attainment is subject to two additional types of measurement error due to (1) ungraded schools and (2) biased recall of educational attainment. Margo (1986) documents that before 1920, many blacks, especially in the South, attended ungraded schools. Whites may also have attended ungraded schools early in the 20th century in rural areas. While educational attainment in the census is supposed to represent highest grade completed, enumerators were instructed to elicit years of school attendance for individuals who had attended ungraded schools. In practice, this means that many black parents, and possibly rural white parents, in the 1940 and possibly 1960-70 censuses who report low levels of educational attainment probably have even lower levels of attainment due to slower annual progress in ungraded schools. This problem would shift some parents with very low attainment into the moderate attainment region and generate a more U-shaped estimated gradient in 1940 relative to later years. I do not find evidence for this, and I therefore do not focus on this problem. Moreover, this problem has no impact

[9] I omit several other datasets for various reasons. The Wisconsin Longitudinal Survey does not contain information on children's dependent status. The NLSY Original Cohorts have highly incomplete data on parental income and education. The OCG62 survey only contains father's education, and only in 2-year bins. The Children of the NLSY79 survey only contains children with unusually young mothers, and therefore do not yield a representative sample.

on gradients measured in parental *income*, which exhibit similar qualitative patterns as gradients measured in parental education.

Goldin (1998), consistent with earlier evidence in Denison (1985) and Folger and Nam (1967), documents a broader education recall bias whereby older cohorts report inflated high school graduation rates in the 1940 census. This pattern would tend to flatten my estimated relationships between child and parent education in 1940 under the plausible assumption that children's education correlates more strongly with actual parental education than with factors that motivate parents to exaggerate their education. Once again, this problem does not affect gradients measured in parental income.

3.2. The Problem of Independent Children

Census data only contain parental characteristics for the subset of children who still live with their parents, and these children may not be representative at ages when most children have completed education (Cameron and Heckman, 1993). Appendix Figure A.1 displays the problem in 1980: average education increases from 10 grades of schooling at age 17 to a plateau of 13 grades at age 26, while over these same ages the share of children living with their parents falls from over 90% to well under 20%.

I here develop a simple correction for the problem of independent children, which I then validate empirically in detail. Let $h_{a,y}$ represent average years of completed schooling for children of fixed age a with parental income or education group y, with $h^D_{a,y}$ and $h^I_{a,y}$ indicating average years of schooling for dependent children still living with parents at age a and independent children, respectively. Similarly, let $N^D_{a,y}$ and $N^I_{a,y}$ indicate the number of dependent and independent children at age a. By definition,

$$h_{a,y} = d_{a,y} h^D_{a,y} + (1 - d_{a,y})\, h^I_{a,y}. \tag{1}$$

where $d_{a,y} = \frac{N^D_{a,y}}{N^D_{a,y} + N^I_{a,y}}$, or the "dependency rate" for children at age a in parental group y. Only a subset of these terms can be estimated directly in census data. For dependent children, I observe both average schooling and number of children for each parental group, h^D_y and $N^D_{a,y}$. For independent children, I only observe the total number of children N^I_a and overall average schooling h^I_a, pooling all parental groups. Because the census does not keep track of intergenerational links after children become independent, we do not observe schooling or frequencies for independent children by parental group, $h^I_{a,y}$ and $N^I_{a,y}$. I therefore need to estimate these unobserved terms in order to impute overall schooling by parental groups, $h_{a,y}$.

I make and validate two assumptions: (1) a ***parallel trends*** assumption for dependent and independent children by parental group status, and (2) ***smooth group cohort size trends*** for parental groups. If there are K parental groups, these assumptions generate a system of $2K+1$ equations in $2K+1$ unknowns that can be solved to identify average final schooling of children by age and parental group.

The ***parallel trends*** assumption states that:

$$f\left(h_y^D, h_y^I\right) = \rho \tag{2}$$

where $f(.)$ can be any known function. I refer to this as "parallel trends" because in practice I use $f\left(h_y^D, h_y^I\right) = h_y^D - h_y^I$. This function places no restriction on the shape of children's schooling gradients in parental income or education; it simply requires this shape to be equal up to a constant across dependent and independent children, where this constant is free to vary as determined by the data across time, space, race, etc. The economic underpinnings of this assumption depend on complex, unobserved relationships between schooling, dependency, and parental group status. However, the assumption captures a simple intuition: rich children exhibit better outcomes than poor children, wherever they happen to live in early adulthood.

The second assumption is ***smooth cohorts***. Denote the total number of children in each parental group in cross-sectional data as $N_{a,y}$, where $N_{a,y} = N_{a,y}^D + N_{a,y}^I$. I assume that

$$N_{a,y} \approx g\left(N_{a-k-1,y}, N_{a-k-2,y}, ..., N_{1,y}\right) \tag{3}$$

for a function $g(.)$ that is smooth enough to be approximated by some parametric functional form, and where k captures the distance between the target age and the ages used in estimation. As shown for 1980 in Appendix Figure A.1 and is true for other years, children do not leave home until after age 17. This implies that $N_y^I \approx 0$ before age 18. Under smooth cohorts, we can therefore estimate group cohort sizes at ages k years after 17 when schooling has been largely completed by estimating the function $g(.)$ on group cohorts younger than 17. I then estimate parental group cohort sizes for independent children as $\hat{N}_{a,y}^I = \hat{N}_{a,y} - N_{a,y}^D$.

Under the assumption of parallel trends with $h_{a,y}^D - h_{a,y}^I = \rho$ and smooth cohorts, and for ages a at which children have completed schooling, I can estimate ρ as

$$\hat{\rho} = \left(\sum_{j=1}^{K} \frac{\hat{N}_{a,j}^I}{N_a^I} h_{a,j}^D\right) - h^I \tag{4}$$

I can therefore estimate average schooling for independent children in parental group y

as $\hat{h}^I_{a,y} = h^D_{a,y} - \rho$. I then estimate final schooling gradients using equation (1).

A final problem with the estimator for ρ in equation (4) is that $\sum_{j=1}^{K} \frac{\hat{N}^I_{a,j}}{N^I_a}$ will not generally equal one due to measurement error in the $\hat{N}^I_{a,j}$ terms. The primary concern here is population growth, which would alter all parental group sizes (approximately) proportionally. I address this problem by substituting *estimated* total independents at age a ($\hat{N}^I_a \equiv \sum_{j=1}^{K} \hat{N}^I_{a,j}$) for *observed* total independents at age a (N^I_a) in equation (4). This assures that $\sum_{j=1}^{K} \frac{\hat{N}^I_{a,j}}{\hat{N}^I_a} = 1$ and implies that $\hat{\rho}$ will be unbiased even if population growth changes parental group sizes across cohorts proportionally.

3.3. Validation of the Parallel Trends Assumption

Figure I presents non-parametric visual evidence on the validity of the parallel trends assumption in the PSID, NLSY79 and NLSY97 for gradients of children's education with respect to parental income deciles and parental education levels, pooling child ages 26-29. The assumption appears highly plausible. In addition to being approximately parallel, the curves are not far apart from each other in levels. This implies that results will be relatively insensitive to the smooth cohorts assumption.

Figure I suggests that schooling gradients are approximately linear in parental income deciles, and in parental schooling levels. I therefore test the parallel trends assumption more formally and quantify potential violations using regressions of the following form:

$$h^j_{i,y} = \beta_0 + \beta_1 \cdot y + \beta_2 \cdot 1\{j = D\} + \beta_3 \cdot y \cdot 1\{j = D\} + e^j_{i,y} \qquad (5)$$

where β_1 captures a linear trend in children's schooling by parental group status, β_2 captures a level shift in schooling across dependent and independent children, β_3 captures differences in the trend in parental group status across dependent and independent children. The parallel trends assumption can now be stated as the null hypothesis that $\beta_3 = 0$.

Table I presents estimates from this regression in parental education for every available dataset with reliable information on parental income during adolescence and children's dependency status in young adulthood. Estimates in every dataset and sample indicate the gradient slope is large and highly statistically significant, while the interaction term is small and statistically insignificant in all cases but one, which is consistent with random chance due to the large number of estimates. Table A.1 presents analogous results for regressions in parental income deciles rather than parental education levels. Once again, the gradients are large and significant, while the interaction terms are small and insignificant. The parallel trends assumption for both parental education and income is therefore surprisingly plausible over the 1980-2010 period. An important caveat is that the interaction terms are not estimated precisely enough to rule out some economically significant deviations from parallel trends. I return to this point below.

In order to test parallel trends before the 1980s, I create a panel dataset by linking children ages 10-17 (when dependency rates are near 100%) in the 1930 census with children ages 20-27 in the 1940 census. This allows me to plot children's schooling outcomes by parental home value and rent groups.[10] I also restrict to boys due to changes in surnames of girls after marriage.[11]

Figure II plots children's final schooling at ages 24-27 by parental home value and rent deciles, and for both whites and blacks. For whites, dependent and independent children at ages 24-27 have virtually identical schooling gradients. For blacks, the parallel trends assumption also holds, though the data are noisy in higher deciles. For blacks, though not for whites, allowing for a level shift fits the data significantly better.[12] Similar patterns arise when cutting each race on region of birth. These results line up well with the results for later decades. Therefore, the parallel trends assumption is plausibly valid over the entire sample period and for all subgroups and datasets with sufficient power to implement a meaningful test.[13]

Why might parallel trends in education arise? In Appendix A, I show that parallel trends requires the qualitatively plausible assumption that individual characteristics such as ability have smaller impacts on educational attainment in higher-status families. Appendix Table A.2 also shows that *timing of marriage* is the primary determinant of dependency status in children's late 20s. Timing of marriage may stem from noise in the

[10] Income and education are not available in the 1930 census.

[11] This exercise takes advantage of new digitized 100% samples of both 1930 and 1940 censuses. Following a stricter version of IPUMS practice, I link children based on five variables: year of birth, state of birth, sex, race, first name and last name. I require exact, unique matches, except for allowing year of birth to be off by one year in either direction. Out of 8.8 million children in the 1940 census, I match 2.5 million or 28%, and about 60% of these matches are unique for a final match rate of about 17%. The resulting panel contains 1.5 million children aged 20-27 with outcomes observed in 1940 matched to their age 10-17 parental characteristics in 1930. I forego more sophisticated matching algorithms (e.g., Feigenbaum, 2015) for simplicity; typically these methods are used to match a small dataset to a large dataset, whereas I am matching millions of children in the 1930 census to their records in the 1940 census.

[12] Why does schooling decline so dramatically for blacks with the highest parental rent expenditures? There are very few blacks in these cells, and many of them may have reported rent incorrectly, for example reporting annual rent in place of monthly rent. This type of measurement error would generate the observed pattern, and is also consistent with the lack of a similar decline for blacks with the highest home values.

[13] Note that while parallel trends appears to be a reasonable assumption for education of children in their 20s, it is not an artifact of the data. For example, I strongly reject parallel trends in children's early-career earnings and income.

spouse matching process or other idiosyncratic shocks rather than factors deeply related to educational attainment.

3.4. Validation of the Smooth Cohorts Assumption

I exploit the smooth cohorts assumption to predict total parental group cohort size shares–including both dependent and independent children–at ages 26-29 using cohort sizes prior to age 18, when virtually all children live with parents and can therefore be linked to parental groups. This prediction requires selection of an estimator.[14] In Appendix B, I show that group cohort size shares at age 17 perform as well as more complex estimators based on cohort trends before age 17, and that all of these estimators do an excellent job of predicting group cohort shares ten years in the future, both in terms of mean effects being close to one, and high R-squared, in every census year.

3.5. Direct Validation with Panel Data

Having validated the underlying assumptions, I now compare resulting mobility estimates directly with alternative sources in two ways. First, I compare the results by state of birth to income mobility estimates in tax data for the 2000s. Second, I compare the results by race and decade to analogous mobility estimates in the panel/retrospective datasets I used to assess parallel trends.

Chetty et al. (2014a) have recently estimated rank-rank income gradient slopes and intercepts by "commuter zone" (CZ) in the U.S. using the population of U.S. tax records spanning 1996-2012. For comparison, I average their income-based rank-rank slopes up

[14] It might seem that I could observe parental group cohort sizes almost perfectly for children under age 28 in the prior census, since ten years previously these children were still dependents under age 18. This is not true for several reasons. First, both income and education are not observed in 1930, preventing the use of this method to estimate parental group cohort sizes in 1940. Since gradients cannot be estimated in the 1950 census, it is critical that I develop a method that can be applied to the 1940 census if I am to significantly extend the historical record for IM. Second, parental group status may change in systematic ways over ten-year intervals. For example, parents of 16-17 year-olds in the bottom income decile in 1960 may not systematically be in the bottom income decile as parents of 26-27 year-olds in 1970. This consideration is less important for parental education, but still may exist due to variation in survey methodology and due to non-classical measurement error in retrospective education measures (Denison, 1962; Goldin, 1998), and it is useful to construct all gradients in a similar way for comparability. A less serious problem is that ten years of death and migration take place between censuses. This problem would be small in my application because few 16-17 year-old children die before turning 26-27 during this period, and because I restrict samples to native-born children. The problem of immigration would be more serious for other demographic groups such as Hispanics and Asian Americans, especially because some immigrant families strategically misrepresent their children's place of birth to census enumerators.

to the state level.[15] I then construct schooling rank-rank slopes on census data by state, adjusted to account for independent children.[16] Note that Chetty et al. (2014a) measure children's residential location around age 15. I can either measure children's location at ages 26-29, or at time of birth. I choose time of birth because many college graduates will have left their home states as of ages 26-29.

Figure V plots educational rank-rank mobility estimates from census data against income rank-rank mobility estimates from tax data. The correlation between the intercepts in Panel (a) is 0.61, while the correlation between the slopes in Panel (b) is 0.55. While rank-rank mobility statistics based on education and income are conceptually distinct and need not correlate perfectly even if measured without error, these results provide direct evidence that the method developed here for constructing mobility statistics generates meaningful results.

Second, I compare my gradient slopes by race and decade to analogous estimates in the survey datasets used above to assess parallel trends. Figure V plots the slope estimates from linear regressions of children's education at ages 26-29 on parental average education, for whites and blacks separately, pooling ten-year intervals into "decade" observations for comparison to census data (so "1980" for annual datasets pools 1980-89, "1990" pools 1990-99, etc.). Note that survey data estimates before 1970 rely on elderly respondents and may suffer mortality attrition, and retrospective reports of parental education are subject to substantial measurement error (e.g., National Center for Education Statistics, 1984).

The figure previews the main results and indicates that estimates from these various datasets are broadly consistent: they are similar in magnitude and exhibit a decline in slopes after 1940 that is larger for blacks. However, there are some important discrepancies. First, the decline for whites in census data is somewhat larger over the 1940-60 period (ignoring 1930) than that in the OCG73 and GSS55, for both whites and blacks. Second, the overall magnitude of the slopes in most survey datasets appear somewhat

[15] The resulting state-level intercepts and slopes should come close to what would be obtained from a rank-rank regression on state-wide micro-data. Such a regression would average the CZ-level slopes with weights proportional to the variance of parental income rank in each CZ (Angrist and Pischke, 2009). I use unweighted averages, although I experimented with weighting by CZ-level Gini coefficients and interquartile ranges as two proxies for parental income rank variance, and found these alternative weights had virtually no impact on the results.

[16] When ranking child and parent education, I break ties by assigning the midpoint of probability mass intervals. Parents are ranked separately by year. Children are ranked separately by age 26-29 and year. Note that state-level schooling mobility estimates in census data are quite noisy because IPUMS only provides a 5% sample in 2000, the education distribution is much lumpier than the income distribution, and the state size distribution is skewed. To increase precision, I therefore average the gradients from the 1980, 1990 and 2000 censuses before estimating slopes and intercepts.

higher in most years for whites, relative to census estimates. Most strikingly, survey datasets suggest an increase in slopes 1980-2000 for whites that is not captured by census estimates.[17] Examination of the underlying gradients revealed that children of parents with college degrees or higher in census data have conspicuously "too little" education in many decades compared both to similar children in panel datasets and to children of less educated parents in census data. If I exclude children of parents with college degrees, the estimated trend in slopes remains similar up through 1990 but exhibits a significant increase 1990-2000 that is consistent with the time trend in panel data (increasing from 0.39 in 1990 to 0.47 in 2000, nearly as high as the slope of 0.51 in 1940). I conclude that the imputation method is accurately detecting a post-1940 mobility increase, but failing to detect a decrease in mobility in recent decades due to a particular violation of parallel trends by children of the highest-educated parents.

Figure VI repeats the comparison using estimated slopes from regressions of children's education on parental *income deciles*. There is no other nationally representative sample with credible information on parental income for cohorts who reach their late 20s before 1970. Reassuringly, mobility with respect to parental income also exhibits an increase after 1940 for both whites and blacks. Once again, the census estimates appear to miss a possible decline in mobility after 1980.

Note that young adults in the U.S. since 1980 are unusually highly-educated and residentially independent, and in that sense a challenging test case for the method. The fact that the method performs reasonably well in this context bodes well for applications in other times, places and groups with less education and greater dependency rates.

4. Comparing Different Mobility Statistics

How do educational mobility statistics relate to earnings mobility statistics? And how does mobility with respect to parental education relate to mobility with respect to parental income? In Appendix C, I derive expressions for these different mobility statistics in a stylized model of family borrowing constraints and educational investments developed in Solon (2004) based on Becker and Tomes (1979, 1986). I generalize the setup in Solon (2004) slightly to allow for heritable determinants of child income other than human capital (e.g., family connections). The model also incorporates parent-child income transmission through investment in human capital (e.g., financial support for

[17]A decrease in educational mobility since 1980 would be consistent with prior work documenting a post-1980 increase in gaps between high-income and low-income children in test scores (Reardon, 2011) and various measures of educational attainment (Acemoglu and Pischke, 2001; Belley and Lochner, 2007; Bailey and Dynarski, 2011b).

college), and heritable determinants of human capital other than parental monetary investments (e.g., genetic IQ transmission). The main assumptions include one parent and one child in each generation, no financial bequests, Cobb-Douglas parental preferences for own consumption and child income, a Mincerian child earnings function, and a log-linear human capital production function. I further assume all dynasties are in steady state.

Denote the "intergenerational income elasticity" $\beta_{\ln y_t, \ln y_{t-1}}$ as the coefficient from a regression of log child's income on log of parental income. Likewise denote $\beta_{h_t, h_{t-1}}$ as the coefficient from a regression of child's education on parental education, and $\beta_{h_t, \ln y_{t-1}}$ as the coefficient from a regression of child's education on log of parental income. In Appendix C, I derive two useful results. First, intergenerational education "elasticities" should equal intergenerational income elasticities:

$$\begin{aligned} \beta_{h_t, h_{t-1}} &= \beta_{\ln y_t, \ln y_{t-1}} \\ &= g(\Phi) \end{aligned}$$

where $g(.)$ is a nonlinear function of a parameter vector Φ containing the returns to schooling, the progressivity of public education spending, the productivity of parental investments in children, and non-financial heritability of human capital and earnings shocks. This result suggests that intergenerational education and income elasticities reflect similar underlying features of social systems and should be equal in magnitude. Of course, the result cannot be taken literally in the 20th century U.S. context due to, for example, changes in the distribution of earnings Autor et al. (2008) and the curvature of earnings functions (Lemieux, 2006; Heckman et al., 2006; Goldin and Katz, 2010) that directly violate the steady state and functional form assumptions of the model.

Empirically, my estimated education elasticities since 1980 (around 0.4) are in fact similar to intergenerational income elasticities in prior literature (Solon, 1999). Figure IV.b is also consistent with this result, establishing a strong correlation between rank-rank education and income elasticities across U.S. states in recent decades. In Appendix D, I use the OCG62 and OCG73 to document three facts supporting the idea that educational mobility gains entailed income mobility gains: (1) returns to education do not vary by parental education, (2) the effect of parental education on child education can account for most of the effect of parental education on child earnings, and (3) the increase in educational mobility with respect to parental education across cohorts is consistent with the increase in income mobility with respect to parental education across cohorts. Of course, educational mobility is interesting in its own right even if its relation to income mobility cannot be known with certainty.

The second useful result relates my two measures of educational mobility, $\beta_{h_t,h_{t-1}}$ and $\beta_{h_t,\ln y_{t-1}}$ to each other:

$$\beta_{h_t,\ln y_{t-1}} = m\left(\Phi, \sigma_\varepsilon^2\right) \tag{6}$$

where $m(.)$ is a different nonlinear function of the same parameter vector Φ contained in $\beta_{h_t,h_{t-1}}$, as well as a new parameter σ_ε^2 denoting the variance of income conditional on human capital. Due to this additional parameter, comparative statics of $\beta_{h_t,\ln y_{t-1}}$ with respect to Φ are more ambiguous than for $\beta_{h_t,h_{t-1}}$. The finding that these two mobility statistics exhibit similar historical trends therefore does have some empirical content in the model, suggesting for example that trends are not driven by changes in σ_ε^2.[18]

5. Results

5.1. Regression Estimates

Figure VII presents the two estimated gradients in 1940 before and after the correction for independent children. The correction turns out to affect levels much more than slopes because dependence rates turn out to be roughly stable across parental groups at ages 26-29 in census data. The correction also affects blacks more than whites due to the larger share of young adult blacks living independently. The figure suggests that relative mobility is strongly correlated with absolute upward mobility because the gradients "pivot" at high levels of parental income and education, and that education is approximately linear in parental education and parental income rank. These patterns echo recent findings in Chetty et al. (2014a), but 60 years earlier in time.

Tables II-III display estimated intercepts and slopes for schooling gradients in parental education levels, for whites and blacks separately, i.e., the estimates displayed in Figures V and VI for census data. Tables A.7-A.8 present analogous estimates for mobility in parental income decile. Each column represents estimates from a regression of the form

$$h_{y,t} = \sum_{t=1940,1960,\ldots,2000} \alpha_t \cdot 1\{\text{year} = t\} + \sum_{t=1940,1960,\ldots,2000} \beta_t \cdot 1\{\text{year} = t\} \cdot y, \tag{7}$$

where $h_{y,t}$ represents a child outcome measure in census year t for children in parental group y (either education or income). I focus primarily on the slope coefficients β_t as measures of relative IM, because intercepts depend largely on secular trends in schooling.

[18] I have confirmed more directly that the conditional variance of income does not evolve in a way that would explain trends in $\beta_{h_t,\ln y_{t-1}}$.

When I re-estimate equation (7) in ranks below, intercepts can be interpreted as meaningful measures of absolute upward IM. By running these regressions on data binned at the level of year, race and parental income or education groups, my standard errors conservatively assume perfect intra-class correlation within these cells.

Column (1) from these two tables contains estimated intercepts and slopes for the two gradients, for whites and blacks separately. For whites, the slope in parental education falls from .50 in 1940 to .39 in 1960, or about 20%, and remains relatively stable up through 2000.[19] The slope in parental income deciles similarly falls from .37 in 1940 to .25 in 1960, or by about 25%. The post-1940 mobility gains of black children are especially remarkable, with slopes in parental income and education both falling by over 50%. These results show that 20th century black-white economic convergence (see, for example, Smith, 1984; Margo, 1986) can be understood not only as blacks converging toward whites, but as poor blacks converging toward rich blacks, and rising mobility more generally.

Columns (2)-(3) shows IM gains were similar for boys and girls, casting doubt on a central role for the G.I. Bills. Columns (4)-(5) break out results into South and North, where "North" includes all non-southern regions. These results replicate lower IM in the South in the 2000s (Chetty et al., 2014a), but reveal that this gap follows in the wake of dramatic regional IM convergence from radically different conditions in 1940. Surprisingly, IM gains in the South were similarly large for both whites and blacks with respect to parental education, and only slightly larger for blacks with respect to parental income. Figure IX displays the estimated slopes of both gradients for *whites* in the South and Non-South and vividly conveys this long-term mobility convergence. Given that over 75% of blacks in 1940 lived in the South compared to 25% of whites, these regional convergence trends account for most of the larger mobility gains of blacks nationally. I return to this finding below when discussing potential explanations for national mobility trends. Wright (1986) argues that the South represented an isolated, low-wage, low-productivity labor market for unskilled workers of all races from the end of the Civil War until the New Deal and World War II began a transformative process of integration, mechanization, and convergence.

Columns (6)-(7) compare mobility in areas defined by the census as "Urban" and "Rural" in years where this variable is available. Both absolute and relative mobility are higher in urban areas for both whites and blacks. The urban/rural mobility gap has also tended to decline over time, much like the North-South mobility gap.[20]

[19]Formal tests for equality of parameter estimates across years with very different point estimates generally yield p-values well below 5%.

[20]The one exception to this convergence pattern is that the urban/rural mobility gap increased for

Columns (8)-(9) plot *annual enrollment* at "high school ages" 16-18 and "college ages" 19-21, rather than plotting highest grade attained at ages 26-29 as in columns (1)-(7). I plot the slopes of gradients in parental income ranks from Tables A.7 and A.8 in Figure IX for convenience. For both whites and blacks, in both parental income and education, high school enrollment accounts for *all* of the increase in relative educational mobility after 1940. In contrast, relative mobility in terms of college enrollment has remained constant. After sixty years of policy initiatives attempting to increase college affordability including the GI Bills, the community college movement and large expansions of federal financial aid, lower-SES children have certainly made substantial gains in college access, but have fallen further behind relative to high-SES children. For blacks the story is similar. These results also place recent work on college access into longer-term historical perspective (e.g., Lochner and Monge-Naranjo, 2011; Bailey and Dynarski, 2011a; Belley and Lochner, 2007), and inform the discussion of causes below.

The post-1940 mobility gains are economically large. To see this, consider the impact of the increase in relative mobility with respect to parental income. Suppose relative educational mobility in 1970 remained at the 1940 level, so that schooling at the top decile in 1970 were held constant at its observed value but schooling of all lower deciles were decreased to reflect the steeper slope from 1940. This would reduce average schooling in 1970 by about 0.75 years. If annual earnings increase by 10% for each additional year of education, this change would account for about 0.25 percentage points of aggregate earnings growth over the 1940-1980 period. I estimate that total household earnings over this period grew at an annual rate of 3.4%, suggesting increasing relative mobility increased annual earnings growth by 8% over this high-growth period. Depending on mechanisms, this growth effect may relate to that obtained from expanding occupational opportunities for women and minorities as in Hsieh et al. (2013).

5.2. Mobility or Inequality? Rank-Rank Elasticities and Correlation Coefficients

I have focused on educational elasticities for convenience and ease of interpretation. However, elasticity trends depend both on trends in positional mobility and trends in cross-sectional inequality. For some purposes, we may wish to measure these two social objectives–mobility and inequality–with statistics that are *mechanically* independent. I estimate two such additional mobility statistics: rank-rank elasticities, and correlation coefficients.

blacks between 1940-60 before converging 1960-1990. Note that intercept and slope estimates for rural blacks in 1960 only cover income deciles 1-7 due to a lack of sufficient high-income rural blacks.

I use the same method developed above to estimate intergenerational elasticities in education *ranks*.[21] In Appendix Tables A.9-A.10, I show that the parallel trend assumption looks reasonable for these outcomes as well. This use of education ranks has two main advantages. Ranks facilitate comparison of gradients over time as the underlying distributions of educational attainment evolve. Second, rank gradients allow interpretation of intercepts as measures of absolute upward mobility distinct from secular gains in education. However, these advantages come at a cost: rank gradient properties cannot be interpreted in terms of human capital units, and ranks can be unstable for discrete random variables with lumpy distributions such as educational attainment.

Table IV displays gradient intercept and slope estimates analogous to those in Tables II-III, but now in education ranks on a scale of $0-100$ for both children and parents, with ranks calculated separately at each age for children age 26-29 and for all heads of household pooling ages 26-65. For whites (Columns 1 and 3), rank gradients in parental income suggest limited gains in absolute upward mobility, but significant gains in relative mobility. Rank gradients in parental education indicate no gains in absolute upward mobility, but do suggest gains in relative mobility that are roughly consistent with results for education levels. For blacks (Columns 2 and 4), rank gradients exhibit large improvements in both absolute upward and relative mobility over time, especially during the 1940-60 period.

While ranks do a better job than levels at distinguishing mobility trends from inequality trends, due to the lumpiness of the education distribution ranks are still not conclusive in this respect. The intergenerational *correlation* also abstracts from changes in educational inequality, and equals the intergenerational elasticity multiplied by the ratio of the standard deviation in parental education over the standard deviation in children's education. I construct these standard deviation ratios for whites and blacks in every year, and adjust the estimated elasticities accordingly. Figure A.2 displays the time trends in child-parent educational correlations for both whites and blacks.[22] The trends are similar to those displayed in Figure V for education elasticities.

[21] Educational attainment can be mapped into ranks by choosing a method to resolve ties. I choose the midpoint of the probability mass interval occupied by an educational category. I rank parental education (average of mother plus father, as described in text) separately by year, and I rank children's education during ages 26-29 separately by age and year.

[22] For correlation coefficients, the conservative standard errors based on collapsed data used above result in confidence intervals that are too wide to be useful. In this case I therefore make a less conservative assumption of a Moulton structure in children's education with respect to parental education, and adjust standard errors of correlation coefficients with Moulton factors by race and year.

6. Why Did Mobility Increase After 1940?

What accounts for the the gains in relative educational mobility 1940-70, and the stagnation or reversal of those gains after 1980? Key findings above suggest the importance of increasing supply of and/or demand for *high school* education for cohorts reaching high school ages 1930-1950 (born ~1915-35), especially factors affecting all genders and races in the South.

The G.I. Bills, Civil Rights Acts, and Great Migration. Several potential explanations for the post-1940 mobility gains are at odds with observed patterns. The post-1940 G.I. Bills almost exclusively benefited men, and yielded few benefits for southern blacks (Turner and Bound, 2003). The Civil Rights gains of the 1960s arrive too late to explain changes in high school attendance behavior between 1930-50. The Great Migration and other regional population shifts are not appealing explanations for the simple reason that black and white mobility both increased dramatically *within* the South and rural areas.[23]

The Great Depression. The Great Depression may have temporarily constrained educational demand among lower-SES families, leaving room for "catch-up" after 1940. Note the central role of North-South mobility convergence casts doubt on this explanation, because the Depression was not a disproportionately southern phenomenon (Rosenbloom and Sundstrom, 1999). I see if mobility was anomalously low in 1940, I estimate high school (age 16-18) and college (age 19-21) enrollment gradients in parental home value and rent deciles, which are available 1930-2000. [24] Figures A.4-A.5 display non-parametric gradients for high school and college enrollment, respectively, for whites. The figures indicate that mobility in high school enrollment was already low in 1930, but it did decline somewhat by 1940. Mobility in college enrollment is nearly identical in 1930 and 1940, in both sets of gradients. Goldin (1998) shows that overall high school enrollment and graduation rates increased significantly during the Great Depression due to the decline in adolescent work opportunities. For low-SES children, borrowing constraints may have dominated these lower opportunity costs of schooling. Therefore low educational mobility in 1940 may partly reflect abnormal constraints imposed by the Great Depression.

[23] For example, I have constructed mobility trends for whites and blacks holding constant state-of-birth population shares at their 1940 level. The resulting national mobility trend is barely affected. Results available upon request. Of course, migration may have played an important role in sustaining wage gains in the South by tightening labor markets. These results hold defining the South in terms of state of birth or state of residence.

[24] While composition of renters and owners changes dramatically 1940-60 due to the large post-war increase in homeownership, homeownership rates and home prices were relatively stable over the 1930s (Shiller, 2015).

Figures A.6-A.7 suggest that IM for blacks did not change significantly 1930-40.

The Black High School Movement. An appealing explanation for the sharp increase in black mobility is the extreme scarcity of black high schools in Southern states with segregated school systems.[25] Anderson (1988) writes: "Blacks in the rural South were excluded from the revolution in public secondary education that characterized the nation and the region during the period 1880 to 1935." In 1940, many blacks would have had to pay out of pocket for private and often faraway high schools. Much prior research has documented the importance of supply-side educational quality improvements in accounting for black-white education and earnings gaps (e.g., Smith and Welch, 1989; Margo, 1990; Card and Krueger, 1992b; Donohue III et al., 2002; Aaronson and Mazumder, 2011), but I am not aware of any research attempting to quantify impacts of the black high school movement in southern states. Once again, the black high school movement is unlikely to provide a full explanation given the similar IM gains of whites in the South.

To explore the role of black high schools in more detail I have compiled archival data on the evolution of black high schools by state from the series "Accredited Secondary Schools in the United States" from years 1928, 1930, 1933, 1934, 1937, 1939, 1942, and 1944 (Phillips and United States Office of Education, 1929; Carr and United States Office of Education, 1930; United States Office of Education, 1933; Carr and United States Office of Education, 1934; United States Office of Education, 1937; Carr and of Education, 1939; United States Office of Education, 1943; Carr and of Education, 1944) and the series "Directory of Secondary Day Schools" from years 1949 and 1952 (Rice and United States Office of Education, 1949, 1952). The volumes from 1928-44 contain data on the number of *accredited* white and black high schools in every state, while the last two volumes from 1949-52 contain data on all white and black high schools, allowing me to assess whether accredited public high schools proxy well for all public high schools. The share of all black high schools that are accredited in 1949 and 1952 varies around 30-50% across states with segregated schools, though it is lower in a few states. To obtain a measure of high school density, I divide the number of white and black high schools by the number of age 14-17 white and black children, respectively. I then regress total public black high schools per-capita on accredited black high schools per capita. I obtain coefficients of 1.03 (SE=.18) in 1949 and .82 (SE=.15) in 1952. These findings suggest that accredited black high schools per capita are plausibly a good proxy for total black high schools per capita in earlier years. I match high school density in 1928 and 1952 to

[25] I thank Robert Margo for suggesting this explanation.

26-29 year-olds by state of birth in the 1940 and 1970 censuses, respectively.[26]

Figure A.3 plots black and white public high schools per capita, by state, over the years 1928-1952. The figure illustrates the extreme relative scarcity of black high schools in the U.S. South in 1928, along with striking heterogeneity in convergence over the next 24 years. For example, as of 1952 blacks had made almost no progress in Mississippi and South Carolina, whereas in Texas and Oklahoma the black-white high school gap had vanished. I exploit this variation using a difference-in-differences approach by plotting changes in black mobility across southern states with above- and below-median changes in black high school density. The "treatment" implied by this comparison is large: an additional increase of .002 high schools per black high-school age child, which is about two-thirds of average white high school density in 1952 in the South. Figure X plots non-parametric education gradients with respect to parental education in these two groups of states in 1940 and 1970. The figure shows roughly equal gains in mobility for blacks in states with small vs. large gains in black high school density. This result casts further doubt on the idea that the black high school movement accounts for black educational mobility gains 1940-70.[27]

Income, Inequality, and other Factors: State-Level Panel Data Analysis. Many other factors could plausibly account for observed mobility trends, including per-capita income, income inequality, urbanization, educational inputs, the demand for teen labor, and migration. In order to explore these variables in a systematic way, I leverage the panel dimension of the mobility statistics constructed by state of birth and year. I estimate three bivariate regression models for each explanatory variable: OLS, fixed effects (FE) and first-differences (FD). Formally, for mobility statistic $M_{s,t}$ and covariate $X_{s,t}$ on individuals born in state s and age 20-29 in year $t \in [1940, 2000]$, I estimate the following regressions:[28]

$$\begin{aligned} M_{s,t} &= \beta_{OLS} X_{s,t} + e_{s,t} \text{ (OLS)} \\ M_{s,t} &= \beta_{FE} X_{s,t} + \gamma_s + \lambda_t + \epsilon_{s,t} \text{ (FE)} \\ M_{s,t} - M_{s,t-1} &= \beta_{FD} \left(X_{s,t} - X_{s,t-1} \right) + \eta_t + \varepsilon_{s,t} \text{ (FD)} \end{aligned}$$

[26] I use the 1970 census rather than the 1960 census for reasons of statistical power; results are similar in either case.

[27] I find similar results using a variety of additional difference-in-difference estimators that compare states with high vs. low growth in black high school density, high vs. low initial black high school density in 1940, high vs. low growth in relative density of black vs white high schools.

[28] I combine ages 20-29 in this section, as opposed to 26-29 in the main section, in order to maximize statistical power.

These regressions are intended to explore associations in the data, not as estimates of causal relationships. Table A.11 presents summary statistics for the dependent and independent variables in the analysis for whites and blacks separately. The large variation in mobility and all independent variables highlights the novel 60-year timeframe of the analysis. I define mobility $M_{s,t,}$ as slopes with respect to parental education; results are similar for mobility in parental income.

Many of these covariates relate to prior literature on mobility and group outcome gaps. A number of theories predict correlations between income, inequality and mobility with causality running in both directions (Loury, 1981; Becker and Tomes, 1986; Murphy et al., 1991; Galor and Tsiddon, 1997; Owen and Weil, 1998). Income inequality has been shown empirically to correlate with mobility across countries and over space within the U.S. in what has been labeled the "Great Gatsby Curve" Corak (2013); Krueger (2012); Chetty et al. (2014a). Black population share has been shown to correlate with black-white school input disparities over space in segregated school systems due to the lower "price" of converting black per-pupil funding into white per-pupil funding (Margo, 1990; Card and Krueger, 1992b). Urbanization may alter demand for education by altering, for example, transportation costs of schooling (Goldin, 1998). Teen birth rates correlate inversely with upward mobility in the modern period (Chetty et al., 2014a), possibly because girls in low-mobility areas perceive low opportunity costs of early motherhood Edin and Kefalas (2011); Kearney and Levine (2012). Teen employment may reflect opportunity costs of schooling or other labor market factors. Cogan (1982) suggests that cotton mechanization and minimum wages may have pushed southern black children into additional schooling rather than work over the 1950-70 period, but Margo and Finegan (1993) demonstrate that these changes mainly continued longer-term secular trends. Compulsory schooling laws affected some children over the 1910-40 period, especially low-SES white children (Acemoglu and Angrist, 2001; Lleras-Muney, 2002), though without clear impacts on earnings (Stephens and Yang, 2014). Class size and relative teacher wages have been shown to affect the returns to education (Card and Krueger, 1992b,a), and vary considerably over this period due to large-scale philanthropic investments in southern black schools, legal activism, and other factors (Smith and Welch, 1989; Margo, 1990; Donohue III et al., 2002; Aaronson and Mazumder, 2011). The share of children who leave their birth states reflects migration rates, which contributed to high rates of occupational mobility in the 19th century (Long and Ferrie, 2013b).

Table V presents estimated determinants of relative educational mobility (slopes of gradients with respect to parental education) in the three panel data regressions separately for whites and blacks.[29] For both races, higher mobility is quite robustly associated with higher state household earnings, lower household earnings inequality, lower black population share, higher minimum school dropout age, higher relative teacher wages, and to some extent higher migration rates. [30]

It is striking that IM correlates positively with earnings levels and negatively with earnings inequality, both cross-sectionally and over time.[31] These correlations are also consistent with North-South regional IM convergence, which coincided with a transformation of the southern economy that raised wages and reduced inequality (Wright, 1986). These patterns suggest a possibility that *broad-based economic growth* can account for historical trends in IM.

To probe this relationship I run multivariate regressions of IM on earnings levels and inequality jointly in Table VI. While precision declines, the point estimates are robust. Figure XI plots national changes in state earnings levels and inequality along with predicted IM using estimated coefficients from the FE specification in column (2) of Table VI. Due to rapid, broad-based economic growth 1940-70 and slower, narrower growth after 1980, predicted IM captures the observed trend reasonably well, though it overpredicts the post-1940 IM gains and fails to predict the IM decline after 1980, most likely due to the problem discussed in Section 3.5.

Educational Institutions. Other explanations for the post-1940 IM trends highlight educational institutions. Public high schools, in contrast with colleges, are characterized by public finance, compulsary initiation, and automatic enrollment. It is possible that mobility gains stalled and reversed after 1980 as the operative margin for educational mobility transitioned from high school to college institutions, if college institutions reduce access among disadvantaged groups. This idea may help account for the slowdown in the relative supply of college-educated workers since 1980 (Goldin and Katz, 2010).

Some of the evidence I present above is consistent with this story. While average college enrollment in 2000 is similar to average high school enrollment in 1930, college enrollment has a steeper gradient with respect to parental SES in 2000 than high school enrollment

[29] Results are similar for absolute mobility (intercepts of gradients), and for gradients in parental income deciles.

[30] The results presented above almost all become statistically insignificant if I allow for linear time trends that vary by state. Some of the results survive time trends that vary by region. The results are largely robust to dropping 1940, and dropping the South, but become underpowered when dropping both 1940 and the South together. The results become more significant if I weight by the precision of the regressions used to estimate the mobility statistics.

[31] The absence of a strong correlation of IM with local income levels in Chetty et al. (2014a) in the 2000s could stem from a difference in time periods, unit of analysis (states vs. commuter zones), or IM statistics (educational elasticities vs. rank-rank income elasticities).

at any point over the 1930-2000 period. Moreover the college gradient has only gotten steeper over the past 70 years, in sharp contrast with the high school gradient. These facts are consistent with a view that institutions characterizing college are relatively more disadvantageous to low-SES students than institutions characterizing high school. Large literatures on college access document the importance of private finance (Dynarski and Scott-clayton, 2013; Fack and Grenet, 2015) and small frictions related to the voluntary and active nature of college (Dynarski and Scott-Clayton, 2008; Bettinger et al., 2012; Carrell and Sacerdote, 2013), while another literature emphasizes that defaults can be sticky even for high-stakes decisions (e.g., Carroll et al., 2009; Chetty et al., 2013).

A related explanation is that primary and secondary school *quality* may have declined in the U.S. since the 1960s, possibly affecting low-SES children disproportionately. Table V indicates that higher relative teacher pay strongly correlates with higher IM. Much research documents a long-term decline in relative teacher pay and teacher quality as measured by test scores, college quality, and other proxies for human capital since 1940 (Hoxby and Leigh, 2004; Bacolod, 2007), and high school graduation rates have been stagnant for decades after their steep ascent in the first half of the 20th century (Heckman and Lafontaine, 2010). It is possible that the public K-8 primary school system prepared many low-SES children to attend high school in the 1940-70 period, but the public K-12 school system is not preparing low-SES children to attend college in recent decades. Bound et al. (2010), for example, show that college completion rates conditional on entry have declined in part due to a decline in the preparation of marginal college entrants.[32]

Summary of Potential Mechanisms. While I can rule out many plausible explanations for the post-1940 rise in mobility and the post-1980 decline in mobility, I am unable to provide more than speculative evidence on the remaining candidates. The most likely candidates involve broad-based economic growth, in particular the rapid transformation of the southern economy after 1940, changes in the institutions governing marginal increases in educational mobility (finance, compulsion, defaults), and a failure of the K-12 system to prepare low-SES students for the separate college system.

7. Intergenerational Decomposition of Racial Earnings Gaps

Schooling gradients estimated here allow a new nonlinear decomposition of historical racial earnings gaps. Let r index racial group and t index generations, and let y_{t-1} indicate parental SES (income or education). Let $y_{r,t}$ indicate average adult group earn-

[32]These authors find an even larger role for changes in the types of colleges attended by these students.

ings, and $h_{r,t}$ indicate average adult education. Let $f_r(y_{t-1})$ indicate the probability density function for parental SES in group r. Adult group earnings depend on final education, $y_{r,t} = y_{r,t}(h_{r,t})$, and final education depends on parental SES in childhood $h_{r,t} = h_{r,t}(y_{r,t-1})$, where both of these relations may vary by race. Group earnings can be written as:

$$y_{r,t} = \int_{y_{t-1}} y_{r,t}\left(h_{r,t}\left(y_{t-1}\right)\right) f_{r,t}\left(y_{t-1}\right) dy_{t-1}. \tag{8}$$

This decomposition breaks group average earnings into three terms. The term $y_{r,t}(h_{r,t})$ captures the group's "earnings function" and can differ across races due to factors such as school quality, labor market discrimination, or family skills not captured by educational attainment. The term $f_{r,t}(y_{t-1})$ captures a group's parental income or education distribution. The term $h_{r,t}(y_{t-1})$ is more novel and captures educational mobility gradients, which may vary across races due to opportunities and cultures.

It is now possible to assess the mechanical impacts of racial differences in each of these three terms on racial earnings gaps. Figure XII displays the three terms of the decomposition for whites and blacks in 1940. Qualitatively, all three terms contribute to the black-white earnings gap; blacks have lower earnings at every education level, lower-income parents, and less education at every level of parental income. Figure XIII quantifies the importance of these three terms. After 1950, by far the most important factor in black-white earnings gaps has been earnings conditional on education; parental income and educational mobility have played comparatively minor roles. This finding emphasizes factors such as school quality, labor market discrimination, and family skills not captured by educational attainment, and downplays the idea that black families have made dramatically different schooling decisions or suffered from dynastic poverty traps. While I have ignored multiplier effects beyond one generation for this empirical exercise, Appendix E shows formally that multiplier effects beyond the first generation are negligible.[33]

8. Robustness of Main Results

As discussed above in the "Data" section, there is some ambiguity in dependent status of young adults in "group living" situations such as college dormitories, prisons, and military barracks in census data (National Research Council, 2006). For my primary

[33] I cannot address the separate problem that two-generation mobility statistics likely overstate multi-generational mobility.

results I count all children living in dormitories, prisons and military barracks at ages 26-29 as independents. Figures A.8 and A.9 compare the estimated slopes and intercepts of mobility gradients in parental income and education for the primary sample and an alternative sample that excludes children in "group living" situations. The results are nearly identical with the one exception of an anomalously flat slope of the education gradient in 1970, which reflects an oddly low level of estimated final schooling among children of high-education parents in that year.

There is substantial variation in the fraction of children with zero or missing values for parental characteristics. Table A.13 displays this variation for whites and blacks, restricting to dependent children age 26-29. The fraction of parental education values that are missing or zero is small in all years for both whites and blacks. The fraction of missing parental income observations follows a U-shaped time trend for both whites and blacks, and therefore does not seem likely to explain the observed decline in mobility. The fraction of children with parents reporting zero income raises more serious concerns, as these shares are very high in 1940 and fall significantly and steadily over time.[34] These changes in the composition of families reporting zero income over time are unlikely to be driving the decline in IM for three reasons. Most persuasively, I examine mobility with respect to parental *education* separately for families reporting zero and positive income. If mobility with respect to parental education is similar in these two groups, it suggests that exclusion of zero-income families should not bias the estimated mobility trends. Figure A.10 plots education elasticities for children with positive and zero/missing parental income, by year, for whites and blacks separately, and indicates that mobility patterns are indistinguishable in these two samples.

Second, a simple exercise suggests the increase in mobility for whites is too large to be accounted for by the decline in the share of families reporting zero income. Note that the share of families reporting zero income falls by 14 percentage points between 1940 and 1960. The worst-case scenario is that these 14 percentage points of families are perfectly mobile with a gradient slope of zero. In that case the true 1940 gradient would actually be $(0.14\,(0) + 0.86\,(0.5) =)$ 0.43, which is still higher than the estimated slope of 0.39 in 1960. It should be clear from the result in Figure A.10 just discussed that this worst-case scenario is extremely conservative. Finally, note that the trend in share of missing and zero income is similar for whites and blacks, despite the large differences in white and black estimated mobility trends.

[34] Adults reporting zero income have very similar occupational composition as adults with missing income. Both of these groups are much more likely to report occupation "unclassifiable," "farmer," and "proprietor or manager" than adults with positive income.

9. Conclusion

In this paper I develop a new method to estimate intergenerational educational mobility on cross-sectional U.S. census data. The method overcomes the problem that most children cannot be linked to parents by ages of school completion, and thereby allows for estimation of final educational outcomes by parental income and education. I construct non-parametric schooling gradients yielding IM statistics that are comparable over time, places and groups. Using a standard dynastic model of human capital investment under borrowing constraints, I show how educational IM in parental income and education relate to each other and to intergenerational income elasticities.

The new methodology paired with several other datasets generate several important and robust historical facts. Educational IM increased significantly after 1940 (born 1910) before stabilizing and then declining after 1980 (born 1950). This increase in IM was economically large; relative IM gains plausibly increased aggregate annual earnings growth by 0.25 percentage points over the 1940-70 period. The increase in IM was very large in the South for both whites and blacks, implying larger IM gains for blacks nationally due to their greater representation in the South. Today's North-South IM gap is a legacy of much larger IM gaps earlier in the century, and incorporates many decades of IM convergence.

Turning to causes, I find the increase in relative IM after 1940 stemmed from greater high school enrollment, not college enrollment. Strikingly, college enrollment has only become less equitable since 1940 despite decades of reforms seeking to expand college access. The GI Bills, the Civil Rights Movement, school desegregation, the black high school movement, the Great Migration, and the Great Depression do not account for post-1940 mobility gains. I construct the first long-term sub-national panel dataset on IM to explore additional explanations. While I am unable to establish causes conclusively, overall patterns are consistent with a role for broad-based economic growth, especially in the South; differences in educational institutions governing K12 and college systems including finance, defaults and compulsion; and quality problems in the K12 system. The methods here should help to improve understanding of IM by expanding its measurement to more groups, times and places in future research.

FOR ONLINE PUBLICATION

A. Intuition for Parallel Trends

Some of the findings above support an even stronger assumption than parallel trends: exogeneity of educational attainment with respect to dependency status at ages 26-29, conditional on parental income or education. It therefore may be useful to note that the primary determinant of dependent status is timing of marriage. Appendix Table A.2 displays the share of children age 26-29 who are married by decade and income decile in the PSID. Virtually no dependent children are married in any year, while 40-70% of independent children in every group are married. This suggests that some children tend to leave the parental home when they find a spouse. It seems plausible that the exact age at which these children find their spouses may not correlate strongly with factors mediating transmission of parental economic status to final schooling.

Other findings above only support parallel (not overlapping) trends. What is the intuition for this restricted form of endogeneity? A simple two-type example provides some insight. Let g represent a continuous measure of parental group status such as income or education. Suppose there are two types of children: high types H disposed toward higher levels of schooling $h_H(g)$, and low types L disposed toward lower levels of schooling $h_L(g) < h_H(g)\,\forall g$. Assume both types exhibit higher schooling in higher-status parental households such that $h'_H, h'_L > 0$. Let $p_D(g) \in [0,1]$ indicate the prevalence of high types among dependent children, and likewise let $p_I(g)$ indicate the prevalence of high types among independent children. Suppose that high types are more prevalent among dependent children, i.e. $p_D > p_I$.

We can now write average schooling among dependent and independent children as

$$\begin{aligned} h_D &= p_D(g)\,h_H(g) + (1 - p_D(g))\,h_L(g) \\ h_I &= p_I(g)\,h_H(g) + (1 - p_I(g))\,h_L(g)\,. \end{aligned}$$

We can then express the parallel trends assumption as

$$\frac{d\,(h_D - h_I)}{dg} = 0, \tag{9}$$

which can be shown to imply that

$$h'_H - h'_L = -\frac{(h_H - h_L)^2}{\rho}\left(p'_D - p'_I\right) \tag{10}$$

where $\rho = h_D(g) - h_I(g)$ equals the constant gap between parallel schooling gradients. Suppose $\rho > 0$ as we observe for blacks in 1940 with respect to parental home value and rent groups. Suppose that prevalence of high types increases more rapidly in parental status g for dependents than independents, i.e. $p'_D - p'_I > 0$. Now schooling of high types must increase *less* rapidly than low types. In other words, parallel but non-overlapping trends require that *behavior converges as composition diverges.* The required convergence of behavior across types per unit of differential change in prevalence is decreasing in the gap between dependent and independent schooling ρ, and increasing in the level of behavioral differences across types.

At least qualitatively, this is a natural assumption to make in the context of schooling gradients and parental group status. For example, ability and many other determinants of schooling may change differentially among dependents and independents as parental status increases. But ability likely has smaller impacts on final schooling outcomes in higher-status families. This type of force may serve to stabilize dependent-independent child outcome differences across parental groups, even if selection on child type into dependent status also varies across parental groups.

B. Validation of Smooth Group Cohort Trends: Details

I employ a simple method to select and validate an estimator of group cohort size shares using only information about cohort size shares of children under age 18, who all have dependency rates well over 90%. The approach I take is to evaluate potential estimators of total group cohort sizes at ages 26-27 ten years earlier at ages 16-17 using group cohort sizes up through age 7. If the best estimators perform well at these ages when true group cohort sizes are observed, then these estimators will likely perform well when using group cohort sizes up through age 17 to predict group cohort sizes at ages 26-27, when true group cohort sizes are not observed. The assumption here is that parents do not change income and education groups in sharp ways over the ten years that elapse between the "validation" ages 16-17 and the "prediction" ages 26-27. In practice, I combine ages 26-29 in census data to increase statistical power, despite only being able to verify the smooth cohorts assumption in this way up through age 27.

The approach is easy to understand visually. Figure III plots the log of the number of children living with parents in different income deciles by age in 1940. The figure suggests that we could predict cohort sizes at ages 16-17 quite well using cohort sizes at ages prior to 8. This suggests that in 1950, we can predict cohort sizes at ages 26-27 (and hopefully 26-29) using cohort sizes at ages prior to 18. While no income data is available in the 1930 census to perform this exercise, the figure also suggests that cohort sizes before age 17 appear likely to perform well as predictors of cohort sizes at ages 26-29.

In Tables A.4 and A.3, I present results of this exercise more formally for parental education and income groups, respectively. Each column displays results from a regression of group cohort size share at ages 16-17 on some estimator based on group cohort size shares before age 8.[35] Columns 1-3 experiment with different estimators, pooling all years 1940-2000. The simplest estimator based on cohort size at age 7 performs better than more complex estimators. I therefore rely on this simple estimator for all main results for this reason and because it is more stable for smaller subgroups. Columns 6-12 examine this estimator by year.[36]

[35] Recall that gradient estimation only depends on group cohort shares, not group cohort levels.
[36] Similar patterns by year hold for all of the estimators.

Several lessons are apparent from these tables. First, the estimators are highly statistically significant in every year, indicating substantial power to identify the parental group composition of independent children. Second, the coefficients on the estimators are typically close to one, with some variation over time that does not line up sharply with the main patterns documented below in any particularly disturbing way. The predictions for parental education groups are somewhat better than for parental income groups in the sense of having coefficients close to one and high R-squared, though both are quite good. Tables A.5 and A.6 display similar patterns for black children. These results broadly support the smooth cohorts assumption. Group cohort sizes evolve in predictable ways and thereby permit credible estimates of parental group composition among independents.

C. Mobility Statistics in a Model of Parental Borrowing Constraints

Following Solon (2004); Becker and Tomes (1979, 1986), let a parent with one child maximize a Cobb-Douglas utility function

$$U_i = (1-\alpha)\ln C_{i,t-1} + \alpha \ln y_{i,t} \tag{11}$$

where i indexes individuals, t indexes a generation, $C_{i,t-1}$ denotes parent's own consumption, $y_{i,t}$ denotes the child's future pre-tax income, and α governs the trade-off between own consumption and children's income. The parent maximizes utility subject to a budget constraint

$$(1-\tau)\cdot y_{i,t-1} = C_{i,t-1} + I_{i,t-1} \tag{12}$$

where τ is the average and marginal tax rate on parental income, $y_{i,t-1}$ denotes parental pre-tax income, and $I_{i,t-1}$ denotes financial investments in children's human capital. These financial investments yield decreasing marginal returns subject to the human capital production function

$$h_{i,t} = \delta + \theta \ln\left(I_{i,t-1} + G_{i,t-1}\right) + e_{i,t} \tag{13}$$

where δ represents the minimum schooling level in society, θ represents the productivity of financial investments in human capital, $G_{i,t-1}$ represents government spending on human capital of child i, and $e_{i,t}$ captures human capital transmitted to children from parents through channels other than financial investment. Assume that government education spending is allocated progressively such that

$$\frac{G_{i,t-1}}{y_{i,t-1}} \approx \varphi - \gamma \ln\left(y_{i,t-1}\right), \tag{14}$$

where φ indicates the universal subsidy as a share of income, and γ captures progressivity of the subsidy schedule.

Assume a log-linear earnings equation in schooling in the tradition of Mincer:

$$\ln y_{i,t} = \mu + p h_{i,t} + \varepsilon_{i,t} \tag{15}$$

where p indicates the return to schooling, μ is the minimal income level in society, and $\varepsilon_{i,t}$ captures income transmitted to children from parents through channels other than observed human capital.

Let heritability of both $e_{i,t}$ and $\varepsilon_{i,t}$ be governed by the same AR(1) process such that

$$e_{i,t} = \lambda e_{i,t-1} + \nu_{i,t} \tag{16}$$

$$\varepsilon_{i,t} = \lambda \varepsilon_{i,t-1} + u_{i,t} \tag{17}$$

where λ indicates the degree of human capital and income inherited from parents outside of monetary investment channels. The assumption that one parameter governs both these inheritance processes is made for analytical convenience.

As pointed out in Becker and Tomes (1986), if parental income y_{t-1} exceeds a certain cutoff level, then parents in this model will leave financial bequests to children, and marginal parental income has no causal effect on children's human capital or income.[37] I assume that parental income is below this threshold, such that parents wish to borrow from their children's future income but are prevented from doing so by a complete failure of the human capital loans market.

Assume all dynasties are in steady state. Using known results from the econometrics literature on autoregressive models (Greene, 2002, pg. 266), and letting $\beta_{x,x'}$ denote the OLS coefficient from a regression of x on x', it can be shown that

$$\begin{aligned} \beta_{h_t,h_{t-1}} &= \frac{p\theta(1-\gamma)+\lambda}{1+p\theta(1-\gamma)\lambda} \\ &= \beta_{\ln y_t, \ln y_{t-1}}. \end{aligned}$$

Again assuming steady state, it can also be shown that gradients in parental education and parental income are related by:

$$\beta_{h_t,\ln y_{t-1}} = \frac{1}{p}\left(\beta_{h_t,h_{t-1}} - \frac{\lambda}{1-\lambda\theta(1-\gamma)}\sigma_\varepsilon^2\right). \tag{18}$$

[37] As the model is written, parents cannot increase child income directly with bequests because human capital is the only instrument available for transfers. To add savings, augment the parent's budget constraint to $y_{i,t} = C_{i,t} + I_{i,t} + S_{i,t}$ and augment the child's earnings function to $\ln(y_{i,t} - S_{i,t}) = \mu + ph$. In this extended model, for parental income above a critical value saving is positive and parental income has no causal impact on children's schooling but still has a positive regression coefficient due to the non-financial transmission parameter λ, as expected.

D. Did Higher Educational Mobility Lead to Higher Income Mobility?

How have changes in educational mobility translated into income mobility? Validation of state-level estimates against income mobility in tax data indicates the answer is yes for recent cohorts (see Section 3.5). Here I also address this question for earlier cohorts in the OCG1962 and OCG1973 surveys, which contain parental education levels and larger samples than the PSID or NLSY79. I first ask if education affects income of children from different parental groups in similar ways. If that were the case, it would suggest we can link these two concepts together with this shared return to schooling as assumed in the model of Section (4).

To proceed I decompose children's earnings into three factors: returns to education, returns to parental group status unrelated to education, and differential returns to education by parental group status, by estimating regressions of the form

$$\text{logEarnings}_{g,\text{educ}} = \alpha + \beta \cdot \text{educ} + \gamma_g \cdot 1\{\text{fatherEduc} = g\} + \delta_g \cdot 1\{\text{fatherEduc} = g\} \cdot \text{educ} \tag{19}$$

for individuals in 10-year birth cohort groups separately on OCG73 and OCG62 data. Here β captures a shared return to schooling, γ_g captures effect of parental background on earnings through non-education channels such as family connections, and δ_g captures differential returns to schooling by parental status due to factors such as educational quality.

Table A.12 Columns (1)-(5) present the results. I do not reject the hypothesis that returns to schooling (β) are the only determinant of children's earnings for any cohort in either OCG data set. I am unable to reject the hypothesis that other factors (γ_g and δ_g) changed in ways that could have offset the educational gains of children from low-SES parents. However, the point estimates decline across cohorts, which would amplify effects of increasing educational mobility on income mobility.

Therefore changes in children's education likely imply changes in children's earnings across all parental status groups. I now ask if higher income mobility can be observed directly in the OCG data. For this exercise, I estimate children's education and income gradients separately with respect to father's education, allowing the intercept and slope of this relationship to change across cohorts. Specifically, I estimate equations of the form

$$\text{childOutcome}_{g,\text{c}} = \pi + \phi \cdot \text{fatherEduc} + \eta_c \cdot 1\{\text{cohort} = c\} + \lambda_c \cdot 1\{\text{cohort} = c\} \cdot \text{fatherEduc} \tag{20}$$

for individuals in the same 10-year cohorts as before, where $\text{childOutcome}_{g,c}$ is either log earnings or education, father's education varies from 7 to 17 years of completed schooling, π and ϕ capture the intercept and slope of the outcome gradient, respectively, and the η_c and λ_c terms capture changes in the intercept and slope, respectively, across cohorts. I select cohorts that correspond roughly to cohorts of 22-25 year-olds in the 1940, 1950 and 1960 censueses. These cohorts have earnings that can be observed after age 27 in OCG data sets (except for the 1940 birth cohort in OCG62) and span the key educational mobility gains documented above.

Table A.12 Columns (6)-(9) present the results. The education gradients are similar to those estimated above on census data, and display similar increases in intercepts and decreases in slopes as in census data, although much less precisely. First note that a return to schooling around 10% per year suggests that education by itself can explain about 75% ($= 0.1 \times 0.429/0.055$) of the gains from having higher-education parents. I have also replicated this pattern in the 1930-40 matched census panel for parental groups defined by home value and rent; there education by itself can explain about 50% of the gains from having higher-status parents.

Second, note that the gains in educational mobility with respect to parental education that I document above suggest the gradient rotated up by about one year for children of the lowest-education parents. Returns to schooling of 10% per year therefore imply that the income gradient in parental education should increase by 0.1 log points in the intercept and, given the domain of father's education from 7-17 years, should decrease the slope by about 0.01 log points. This is close to the results in Column (7) for the OCG1973, though again results are imprecise. In OCG1962, I cannot observe income for the cohort corresponding to the 1960 census with precision, and results are too imprecise to be useful for the cohort corresponding to 1950. Overall, these results do suggest that gains in educational mobility imply gains in income mobility, but are too noisy to demonstrate this conclusively. This is not surprising given that the motivation for this paper stems from a lack of any precise, long-term historical time series data on intergenerational income mobility.

E. Dynamic Intergenerational Decomposition of Racial Earnings Gaps

In this appendix explores the effects of ignoring dynamics of equation (8) in Section 7. In order to study dynamics I make additional assumptions in the spirit of the Conlisk (1974). Ignoring group subscripts, I linearize educational mobility gradients as $h_t(y_{t-1}) = \theta + \gamma y_{t-1}$ and adult earnings functions as $y_t(h_t) = \alpha + \beta h_t$. I can then rewrite equation (8) as

$$\mathbb{E}[y_t] = \int_{y_{t-1}} f(y_{t-1}) \{\alpha + \beta(\theta + \gamma y_{t-1})\} dy_{t-1}. \tag{21}$$

I thereby assume that each group has a constant parental Engel curve and adult earnings function. I also assume that each parent has one child and each child has one parent. I thereby abstract from the marriage market, fertility choices, and different earnings functions for men and women.

This recursive relation leads to the following formula for average group earnings in generation T:

$$\mathbb{E}\left[y_T\right] = \int_{y_{t-1}} f\left(y_{t-1}\right) \left\{ \left(\alpha + \beta\theta\right) \sum_{j=1}^{T-(t-1)} \left(\beta\gamma\right)^{j-1} + \left(\beta\gamma\right)^{T-(t-1)} y_{t-1} \right\} dy_{t-1} \tag{22}$$

In the main text, I alter these parameters one generation at a time, ignoring effects beyond the first generation. For example, totally differentiating average child education with respect to a group's educational mobility parameters θ and γ yields

$$d\mathbb{E}\left[y_t\right] = \beta d\theta + \beta \mathbb{E}\left[y_{t-1}\right] d\gamma. \tag{23}$$

In contrast, the analogous formula for any future generation is

$$d\mathbb{E}\left[y_T\right] = \beta \Gamma_T d\theta + \left(\Phi_T + \Omega_T \beta \mathbb{E}\left[y_{t-1}\right]\right) d\gamma \tag{24}$$

where

$$\begin{aligned}
\Gamma_T &\equiv \sum_{j=1}^{T-(t-1)} \left(\beta\gamma\right)^{j-1} \\
\Phi_T &\equiv \beta\left(\alpha + \beta\theta\right) \sum_{j=1}^{T-(t-1)} \left(j-1\right)\left(\beta\gamma\right)^{j-2} \\
\Omega_T &\equiv \left(T - \left(t-1\right)\right)\left(\beta\gamma\right)^{T-t}
\end{aligned}$$

which is equivalent to 23 when $T = t$. The multiplier effects ignored in the static decomposition are therefore captured by Γ_T, Φ_T, and Ω_T for $T > t$. Under any realistic parameter values, these terms are small. For example, denominating θ in years of schooling, β in log of real dollars divided by years of schooling, and γ in years of schooling divided by log of real dollars, the data suggest upper bound estimates of $\beta \approx .1$ and $\gamma \approx .5$ for any race in any year, while $\theta \approx 7$, $\alpha \approx 8$ and $\mathbb{E}\left[y_{t-1}\right] \approx 9$ are reasonable approximations for 1940. This leads to values of $\Gamma_T \approx 1.05$, $\Phi_T \approx 1$, and $\Omega_T < 0.1$ for any $T > t$. Now suppose $d\theta = 3$ and $d\gamma = 0.3$, which approximates the difference between black and white educational mobility gradients in 1940, the year with the largest racial differences. Under these assumptions, $d\mathbb{E}\left[y_t\right] = 0.57$ using 23, and $d\mathbb{E}\left[y_T\right] < 0.63$ using 24 for any $T > t$, implying a bias of at most 10%.

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	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
					PSID						NLSY79					NLSY97			GSS	OCG73
Child Educ	All	1980-89	1990-99	2000-09	Male	Female	White	Black	All	Male	Female	White	Black	All	Male	Female	White	Black	All	All
Par Educ	0.437**	0.393**	0.489**	0.519**	0.432**	0.396**	0.441**	0.259**	0.452**	0.464**	0.446**	0.459**	0.315**	0.453**	0.453**	0.456**	0.477**	0.351**	0.338**	0.442**
	(0.0211)	(0.0232)	(0.0256)	(0.0345)	(0.0206)	(0.0433)	(0.0190)	(0.0365)	(0.0243)	(0.0262)	(0.0267)	(0.0235)	(0.0517)	(0.0321)	(0.0365)	(0.0367)	(0.0323)	(0.0521)	(0.0368)	(0.0281)
Dep	0.495	0.238	1.239	-0.520	0.155	1.256	-0.399	0.393	-0.144	-0.141	0.275	-0.222	-1.406	0.311	0.213	0.632	0.478	-3.033*	1.268	-0.0795
	(0.777)	(0.843)	(0.911)	(1.336)	(0.658)	(0.735)	(0.787)	(0.917)	(0.810)	(0.788)	(1.019)	(0.818)	(1.166)	(0.929)	(0.965)	(1.203)	(0.996)	(1.301)	(1.730)	(0.860)
Par Educ * Dep	-0.0662	-0.0360	-0.128	0.0161	-0.0489	-0.101	0.00239	-0.0516	-0.00673	-0.0117	-0.0275	3.19e-06	0.101	-0.0490	-0.0416	-0.0633	-0.0642	0.228*	-0.112	-0.0469
	(0.0628)	(0.0718)	(0.0744)	(0.0999)	(0.0531)	(0.0612)	(0.0617)	(0.0826)	(0.0660)	(0.0645)	(0.0823)	(0.0660)	(0.103)	(0.0704)	(0.0728)	(0.0914)	(0.0744)	(0.102)	(0.132)	(0.0803)
Constant	7.974**	8.502**	7.315**	6.734**	7.992**	8.614**	7.973**	9.805**	8.292**	8.006**	8.493**	8.207**	9.767**	8.136**	7.911**	8.323**	7.842**	9.160**	9.267**	8.196**
	(0.264)	(0.277)	(0.320)	(0.469)	(0.259)	(0.519)	(0.243)	(0.404)	(0.303)	(0.330)	(0.331)	(0.295)	(0.586)	(0.437)	(0.497)	(0.498)	(0.444)	(0.673)	(0.488)	(0.307)
Observations	24	24	24	23	24	24	24	24	24	24	24	24	24	24	24	24	24	24	24	30
R-squared	0.960	0.941	0.952	0.933	0.963	0.867	0.968	0.747	0.953	0.951	0.939	0.957	0.752	0.927	0.912	0.900	0.931	0.819	0.814	0.916

Robust standard errors in parentheses

** p<0.01, * p<0.05

Table I: Parallel Trends in Parental Education

Notes: Each column displays estimates of equation (5) for children's schooling at ages 26-29 by parental income deciles. Parental characteristics measured when children are age 17, or earlier in adolescence if not observed at age 17. Children's schooling excludes zeros. Parental education defined as average of mother's and father's educational attainment, or educational attainment of the single parent when second parent not observed. Children with parental education in the bottom 2% of the parental education distribution excluded. Sample weights used in all calculations. Regressions on collapsed data weighted by square of cell size.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent Var	Highest Grade Attained, Age 26-29							Enrollment	
Sample	All	Boys	Girls	South	North	Urban	Rural	Age 16-18	Age 19-21
Intercept 1940	7.267**	7.065**	7.558**	6.043**	7.881**	8.128**	6.454**	0.373**	-0.0791*
	(0.141)	(0.170)	(0.161)	(0.208)	(0.186)	(0.140)	(0.152)	(0.0129)	(0.0377)
Intercept 1960	9.311**	8.886**	9.171**	7.825**	9.457**	9.430**	8.119**	0.553**	-0.0426
	(0.146)	(0.211)	(0.169)	(0.232)	(0.216)	(0.161)	(0.195)	(0.0143)	(0.0391)
Intercept 1970	10.12**	9.546**	10.14**	9.735**	9.764**			0.712**	-0.00600
	(0.133)	(0.184)	(0.168)	(0.282)	(0.179)			(0.0128)	(0.0357)
Intercept 1980	10.84**	10.57**	10.46**	10.11**	10.72**			0.642**	-0.0444
	(0.135)	(0.181)	(0.171)	(0.224)	(0.196)			(0.0129)	(0.0360)
Intercept 1990	10.72**	10.70**	10.80**	10.57**	10.86**	10.99**	10.05**	0.728**	0.0706
	(0.172)	(0.209)	(0.196)	(0.248)	(0.231)	(0.163)	(0.216)	(0.0165)	(0.0460)
Intercept 2000	11.45**	11.08**	11.14**	10.54**	10.89**			0.802**	0.168**
	(0.151)	(0.207)	(0.187)	(0.281)	(0.253)			(0.0146)	(0.0403)
Slope 1940	0.499**	0.516**	0.469**	0.647**	0.432**	0.428**	0.562**	0.0442**	0.0411**
	(0.0216)	(0.0263)	(0.0244)	(0.0340)	(0.0278)	(0.0203)	(0.0247)	(0.00198)	(0.00578)
Slope 1960	0.388**	0.414**	0.334**	0.533**	0.329**	0.340**	0.453**	0.0344**	0.0487**
	(0.0218)	(0.0278)	(0.0222)	(0.0326)	(0.0278)	(0.0202)	(0.0289)	(0.00217)	(0.00582)
Slope 1970	0.371**	0.426**	0.295**	0.361**	0.372**			0.0255**	0.0577**
	(0.0196)	(0.0238)	(0.0216)	(0.0367)	(0.0230)			(0.00190)	(0.00523)
Slope 1980	0.387**	0.391**	0.371**	0.422**	0.358**			0.0322**	0.0675**
	(0.0206)	(0.0243)	(0.0228)	(0.0308)	(0.0259)			(0.00199)	(0.00550)
Slope 1990	0.365**	0.363**	0.363**	0.373**	0.353**	0.343**	0.399**	0.0238**	0.0665**
	(0.0244)	(0.0298)	(0.0277)	(0.0362)	(0.0323)	(0.0224)	(0.0330)	(0.00236)	(0.00652)
Slope 2000	0.404**	0.380**	0.411**	0.406**	0.378**			0.0220**	0.0707**
	(0.0271)	(0.0320)	(0.0287)	(0.0387)	(0.0336)			(0.00263)	(0.00723)
Observations	77	80	81	82	80	43	43	77	77
R-squared	1.000	1.000	1.000	0.999	1.000	1.000	1.000	1.000	0.988

Standard errors in parentheses
** p<0.01, * p<0.05

Table II: Mobility Estimates in Parental Education, Whites

Notes: Displays estimated intercepts and slopes of children's schooling gradients with respect to parental education for whites. Estimates correspond to α_t and β_t from Equation (7). Regressions weighted by square root of estimated cell sizes.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent Var	Highest Grade Attained, Age 26-29							Enrollment	
Sample	All	Boys	Girls	South	North	Urban	Rural	Age 16-18	Age 19-21
Intercept 1940	4.193**	3.697**	4.607**	3.901**	6.291**	5.117**	3.700**	0.336**	0.0115
	(0.141)	(0.144)	(0.219)	(0.151)	(0.352)	(0.203)	(0.149)	(0.0121)	(0.0324)
Intercept 1960	7.694**	6.103**	8.248**	6.684**	8.589**	8.183**	6.050**	0.574**	0.130**
	(0.144)	(0.176)	(0.236)	(0.179)	(0.321)	(0.203)	(0.204)	(0.0128)	(0.0326)
Intercept 1970	9.242**	9.217**	9.336**	9.105**	9.312**			0.727**	0.0391
	(0.135)	(0.148)	(0.206)	(0.195)	(0.149)			(0.0119)	(0.0309)
Intercept 1980	10.45**	9.822**	9.996**	10.11**	10.25**			0.711**	0.103**
	(0.124)	(0.159)	(0.227)	(0.162)	(0.194)			(0.0113)	(0.0285)
Intercept 1990	11.04**	10.37**	10.85**	11.01**	11.10**	10.55**	11.00**	0.748**	0.131**
	(0.154)	(0.209)	(0.296)	(0.190)	(0.207)	(0.191)	(0.338)	(0.0136)	(0.0352)
Intercept 2000	10.98**	10.72**	10.92**	10.64**	10.81**			0.794**	0.123**
	(0.145)	(0.167)	(0.248)	(0.203)	(0.219)			(0.0130)	(0.0334)
Slope 1940	0.635**	0.639**	0.627**	0.632**	0.419**	0.530**	0.585**	0.0371**	0.0228**
	(0.0282)	(0.0291)	(0.0431)	(0.0326)	(0.0533)	(0.0333)	(0.0357)	(0.00242)	(0.00648)
Slope 1960	0.409**	0.583**	0.285**	0.464**	0.250**	0.289**	0.519**	0.0174**	0.0119*
	(0.0244)	(0.0275)	(0.0341)	(0.0299)	(0.0396)	(0.0271)	(0.0423)	(0.00210)	(0.00540)
Slope 1970	0.285**	0.291**	0.275**	0.314**	0.273**			0.0115**	0.0298**
	(0.0181)	(0.0198)	(0.0278)	(0.0283)	(0.0192)			(0.00159)	(0.00415)
Slope 1980	0.275**	0.277**	0.271**	0.283**	0.269**			0.0162**	0.0340**
	(0.0177)	(0.0180)	(0.0255)	(0.0217)	(0.0230)			(0.00161)	(0.00406)
Slope 1990	0.239**	0.242**	0.227**	0.236**	0.240**	0.244**	0.139**	0.0186**	0.0482**
	(0.0243)	(0.0254)	(0.0360)	(0.0308)	(0.0316)	(0.0229)	(0.0451)	(0.00216)	(0.00557)
Slope 2000	0.374**	0.339**	0.362**	0.371**	0.346**			0.0208**	0.0662**
	(0.0299)	(0.0288)	(0.0430)	(0.0358)	(0.0369)			(0.00270)	(0.00687)
Observations	85	88	89	88	83	46	45	85	85
R-squared	1.000	1.000	0.999	0.999	1.000	1.000	0.998	1.000	0.984

Standard errors in parentheses
** p<0.01, * p<0.05

Table III: Mobility Estimates in Parental Education, Blacks

Notes: Displays estimated intercepts and slopes of children's schooling gradients with respect to parental education for blacks. Estimates correspond to α_t and β_t from Equation (7). Regressions weighted by square root of estimated cell sizes.

	(1)	(2)	(3)	(4)
Dependent Var:	Child's Education Rank, Ages 26-29			
Parental Groups:	Parental Income Rank		Parental Education Rank	
Sample:	White	Black	White	Black
Intercept 1940	31.74**	14.24**	34.01**	13.83**
	(2.981)	(2.181)	(1.386)	(1.514)
Intercept 1960	38.85**	24.79**	36.79**	26.75**
	(2.628)	(1.735)	(1.469)	(1.458)
Intercept 1970	37.55**	30.92**	33.54**	29.82**
	(2.149)	(1.288)	(1.390)	(1.327)
Intercept 1980	37.25**	33.01**	33.14**	30.69**
	(2.175)	(1.356)	(1.372)	(1.146)
Intercept 1990	36.26**	31.32**	32.70**	29.93**
	(2.459)	(1.541)	(1.518)	(1.328)
Intercept 2000	35.77**	31.39**	32.87**	28.55**
	(2.549)	(1.578)	(1.525)	(1.350)
Slope 1940	0.374**	0.520**	0.439**	0.554**
	(0.0548)	(0.0766)	(0.0310)	(0.0546)
Slope 1960	0.260**	0.431**	0.321**	0.350**
	(0.0446)	(0.0555)	(0.0266)	(0.0448)
Slope 1970	0.268**	0.262**	0.352**	0.307**
	(0.0356)	(0.0354)	(0.0231)	(0.0327)
Slope 1980	0.249**	0.234**	0.365**	0.310**
	(0.0361)	(0.0318)	(0.0234)	(0.0267)
Slope 1990	0.246**	0.271**	0.369**	0.328**
	(0.0413)	(0.0344)	(0.0262)	(0.0291)
Slope 2000	0.259**	0.239**	0.363**	0.339**
	(0.0420)	(0.0349)	(0.0259)	(0.0287)
Observations	60	60	59	59
R-squared	0.996	0.995	0.998	0.997

Standard errors in parentheses
** p<0.01, * p<0.05

Table IV: Rank-Rank Educational Mobility Estimates in Parental Income and Education

Notes: Displays estimated intercepts and slopes of age 26-29 children's schooling rank gradients with respect to parental income and education ranks. All ranks on scale from 0 to 100. Ranks computed on full population for each age and year for children, and for each year for parents pooling ages 26-65. Parental income rank ignores zeros. Ties in all rankings are assigned midpoints of rank intervals. Bottom 2% of reported educational attainment in each year dropped from sample. Estimates correspond to α_t and β_t from Equation (7). Regressions weighted by square root of estimated cell sizes.

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Whites				Blacks			
Covariate	Model	Estimate	SE	N	R-squared	Estimate	SE	N	R-squared
Log Household Earnings	OLS	-0.120**	(0.00943)	248	0.389	-0.152**	(0.0132)	159	0.519
	FE	-0.289**	(0.0445)	248	0.691	-0.349**	(0.0811)	159	0.730
	FD	-0.276**	(0.0800)	191	0.167	-0.216**	(0.0357)	104	0.550
Earnings Inequality: p75-p25	OLS	0.305**	(0.0377)	248	0.290	0.476**	(0.0410)	159	0.481
	FE	0.308**	(0.0578)	248	0.691	0.372**	(0.0768)	159	0.731
	FD	0.201	(0.121)	191	0.143	0.304**	(0.0914)	104	0.547
Urban Share	OLS	-0.463**	(0.103)	130	0.337	-0.776**	(0.106)	77	0.483
	FE	-0.162	(0.153)	130	0.756	-0.921**	(0.307)	77	0.817
	FD	-0.129	(0.124)	42	0.021	-0.160	(0.312)	16	0.012
Share Black	Basic	0.345**	(0.0552)	249	0.128	0.264**	(0.0802)	157	0.074
	FE	0.650**	(0.191)	249	0.658	0.938*	(0.360)	157	0.743
	FD	1.081**	(0.254)	192	0.152	0.777*	(0.348)	104	0.519
Teen Birth Rate	OLS	1.359**	(0.292)	249	0.111	0.823*	(0.306)	157	0.028
	FE	1.282*	(0.548)	249	0.644	2.431**	(0.743)	157	0.691
	FD	0.303	(0.589)	192	0.117	-0.145	(0.973)	104	0.494
Teen Employment Rate	OLS	-0.339**	(0.101)	249	0.048	0.233	(0.169)	157	0.014
	FE	0.244	(0.138)	249	0.632	0.866**	(0.210)	157	0.723
	FD	0.0254	(0.175)	192	0.115	0.155	(0.294)	104	0.495
Dropout Age	OLS	-0.0305*	(0.0147)	178	0.035	-0.0711**	(0.0253)	106	0.125
	FE	-0.0319	(0.0180)	178	0.681	-0.0529	(0.0282)	106	0.753
	FD	-0.0373*	(0.0154)	122	0.164	-0.0322	(0.0321)	53	0.367
Class Size	OLS	0.0164**	(0.00209)	136	0.316	0.0263**	(0.00260)	77	0.520
	FE	0.00533	(0.00544)	136	0.690	0.0169	(0.0127)	77	0.844
	FD	0.00383	(0.00484)	84	0.092	0.0111	(0.0108)	32	0.193
Relative Teacher Wage	OLS	-0.224**	(0.0740)	136	0.114	-0.163	(0.0859)	77	0.056
	FE	-0.144*	(0.0576)	136	0.701	-0.257**	(0.0848)	77	0.875
	FD	-0.180**	(0.0558)	84	0.141	-0.241**	(0.0670)	32	0.267
Share Move State	OLS	0.168**	(0.0497)	248	0.083	0.131*	(0.0614)	159	0.033
	FE	0.242*	(0.115)	248	0.651	0.597**	(0.139)	159	0.739
	FD	0.139	(0.144)	191	0.117	0.253	(0.197)	104	0.501

Table V: Mobility Regressions on State-Year Panel 1940-2000

Notes: Table displays estimates from bivariate regressions of educational mobility gradient slopes (child education on parent education) on various covariates. OLS, fixed effect and first-difference models described in text. Standard errors clustered at state-of-birth level. All regressions unweighted. Mobility varies by year, state-of-birth and race. All other variables vary by year and state-of-birth, or year and state. White and black samples vary due to requirement that gradients underlying mobility statistics be non-missing for at least 60% of parental group levels. Mobility statistics for state-of-birth panel analysis constructed on children age 20-29. "HS per capita" and "HS PC" refer to number of public high schools per age 14-17 year-old children in state-of-birth and year, and are only matched to census years 1940, 1960 and 1970 from high school data sets in years 1928, 1949 and 1952, respectively to match age at high school attendance as closely as possible with available data. Dropout age, class size and relative teacher wage matched to to census from year at which age 20-29 year-old children would have been approximately age 14, and are only matched to censuses 1940-70 using data from Stephens and Yang (2014). Log household earnings and earnings inequality constructed from household earnings distribution (head + spouse) for heads of household age 30-65 in year children turn 20-29 and living in child's state of birth. Migration and urbanization constructed from age 20-29 year-olds by state of birth and year.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	White						Black					
Household Earnings	Slope w.r.t. Parental Education			Slope w.r.t. Parental Income Decile			Slope w.r.t. Parental Education			Slope w.r.t. Parental Income Decile		
Log Household Earnings	-0.0907**	-0.173*	-0.229*	-0.138**	-0.0459	-0.107	-0.0974**	-0.200	-0.148**	-0.154**	-0.368**	0.0207
	(0.00882)	(0.0706)	(0.111)	(0.0118)	(0.109)	(0.124)	(0.0143)	(0.109)	(0.0389)	(0.0243)	(0.132)	(0.0725)
Earnings Inequality: p75-p25	0.133**	0.185	0.107	0.109*	0.228	0.204	0.257**	0.221*	0.200**	0.305**	0.406**	0.440*
	(0.0400)	(0.0931)	(0.158)	(0.0422)	(0.122)	(0.137)	(0.0391)	(0.100)	(0.0696)	(0.0658)	(0.136)	(0.180)
Observations	248	248	191	294	294	234	159	159	104	204	204	151
R-squared	0.421	0.702	0.174	0.460	0.675	0.210	0.592	0.742	0.568	0.457	0.735	0.210
Model	OLS	FE	FD	OLS	FE	FD	OLS	FE	FD	OLS	FE	FD

Robust standard errors in parentheses
** p<0.01, * p<0.05

Table VI: Mobility Regressions on Income Level and Inequality in State-Year Panel, 1940-2000

Notes: Table presents OLS, FE and FD regressions of mobility by state-of-birth, year and race on average log of household earnings and interquartile gap in log of household earnings. Constants omitted from table for convenience.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
				PSID							NLSY79					NLSY97		
Child Educ	All	1980-89	1990-99	2000-09	Male	Female	White	Black	All	Male	Female	White	Black	All	Male	Female	White	Black
Par Income	0.238**	0.197**	0.256**	0.274**	0.258**	0.219**	0.230**	0.206**	0.229**	0.233**	0.224**	0.227**	0.170**	0.317**	0.312**	0.326**	0.317**	0.255**
	(0.0228)	(0.0338)	(0.0291)	(0.0194)	(0.0285)	(0.0233)	(0.0248)	(0.0342)	(0.0181)	(0.0252)	(0.0252)	(0.0189)	(0.0467)	(0.0153)	(0.0226)	(0.0266)	(0.0189)	(0.0525)
Dep	-0.116	-0.254	0.158	-0.853*	0.0741	-0.216	-0.110	0.0485	-0.605	-0.645	-0.448	-0.604	-0.353	-0.329	-0.250	-0.268	-0.445	-0.579
	(0.419)	(0.659)	(0.537)	(0.329)	(0.452)	(0.530)	(0.523)	(0.337)	(0.286)	(0.366)	(0.448)	(0.326)	(0.325)	(0.209)	(0.281)	(0.408)	(0.284)	(0.476)
Par Inc * Dep	-0.0400	-0.0128	-0.0985	0.0698	-0.0999	0.0407	-0.0344	-0.0844	0.0524	0.0435	0.0644	0.0541	0.00259	-0.0457	-0.0373	-0.0618	-0.0348	0.0203
	(0.0686)	(0.110)	(0.0859)	(0.0540)	(0.0750)	(0.0842)	(0.0805)	(0.0756)	(0.0480)	(0.0614)	(0.0754)	(0.0525)	(0.0956)	(0.0360)	(0.0483)	(0.0702)	(0.0471)	(0.102)
Constant	12.08**	12.08**	11.97**	12.34**	11.89**	12.25**	12.16**	11.95**	12.62**	12.47**	12.78**	12.65**	12.65**	12.37**	12.12**	12.56**	12.40**	12.62**
	(0.144)	(0.212)	(0.183)	(0.122)	(0.179)	(0.147)	(0.162)	(0.148)	(0.109)	(0.153)	(0.152)	(0.116)	(0.171)	(0.0953)	(0.143)	(0.165)	(0.123)	(0.248)
Observations	20	20	20	20	20	20	20	20	20	20	20	20	20	20	20	20	20	20
R-squared	0.884	0.707	0.840	0.942	0.854	0.861	0.856	0.717	0.928	0.879	0.856	0.918	0.571	0.972	0.941	0.918	0.958	0.698

Robust standard errors in parentheses

** p<0.01, * p<0.05

Table A.1: Parallel Trends in Parental Income

Notes: Each column displays estimates of equation (5) for children's schooling at ages 26-29 by parental income deciles. Parental characteristics measured when children are age 17, or earlier in adolescence if not observed at age 17. Children's schooling excludes zeros. Parental income defined as sum of mother's and father's earnings when possible, otherwise as total family income. Children with zero parental income at age 17 excluded from the parental income figures. Parental income deciles calculated separately by year. Sample weights used in all calculations. Regressions on collapsed data weighted by square of cell size.

				Income Quintile			
Decade	Dep Status	1	2	3	4	5	All
1970	Independent	0.63	0.7	0.71	0.65	0.57	0.65
	Dependent	0	0	0.01	0.01	0	0
1980	Independent	0.53	0.65	0.61	0.53	0.47	0.56
	Dependent	0.01	0.01	0.01	0.01	0	0.01
1990	Independent	0.5	0.5	0.5	0.53	0.44	0.49
	Dependent	0.01	0.03	0.01	0.01	0	0.01
2000	Independent	0.54	0.51	0.5	0.43	0.44	0.49
	Dependent	0.02	0	0.03	0.01	0.02	0.01

Table A.2: Percent Married at Ages 22-25 by Decade, Dependency and Parental Income

Notes: All statistics calculated with PSID data using sampling weights. Quintiles calculated on total annual parental income at age 17. Decades pool 10 years from 1970-70, 1980-89, etc.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dep Var: Educ Group Cohort Share Ages 16-17	All Years	All Years	All Years	1940	1950	1960	1970	1980	1990	2000
Age 7	0.950**			1.163**	0.800**	0.660**	0.780**	0.941**	1.046**	1.039**
	(0.0152)			(0.0441)	(0.109)	(0.0729)	(0.0188)	(0.0160)	(0.0220)	(0.0137)
Ages 4-7, linear		0.882**								
		(0.0104)								
Ages 1-7, linear			0.241**							
			(0.0275)							
Constant	0.00667*	0.0131**	0.115**	-0.0139*	0.0337	0.0386**	0.0227**	0.00682	-0.00716	-0.00566
	(0.00295)	(0.00211)	(0.00824)	(0.00585)	(0.0207)	(0.00957)	(0.00338)	(0.00345)	(0.00478)	(0.00323)
Observations	190	190	190	32	34	28	26	24	24	22
R-squared	0.954	0.975	0.291	0.959	0.629	0.759	0.986	0.994	0.990	0.997

Standard errors in parentheses

** p<0.01, * p<0.05

Table A.3: Validation of Group Cohort Size Predictors in Parental Education, Whites

Notes: Documents ability to predict group cohort sizes at later ages with group cohort sizes at earlier ages. Columns (1)-(3) regress actual parental education group cohort shares at ages 16-17 on predicted parental education group cohort shares at ages 16-17 and a constant, where each column uses a different predictor. Columns (4)-(10) run the regression in Column (1) separately by census year 1940-2000. Regressions for white-only sample. All regressions weighted by the square root of the cell size.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dep Var: Income Group										
Cohort Share Ages 16-17	All Years	All Years	All Years	1940	1950	1960	1970	1980	1990	2000
Age 7	0.929**			0.753**	0.569**	0.692**	0.768**	0.826*	1.398**	1.327**
	(0.0800)			(0.141)	(0.0556)	(0.161)	(0.219)	(0.318)	(0.216)	(0.0716)
Ages 4-7, linear		0.880**								
		(0.0444)								
Ages 1-7, linear			0.883**							
			(0.0568)							
Constant	0.0117	0.0145**	0.0152*	0.0252	0.0439**	0.0335	0.0291	0.0262	-0.0373	-0.0324**
	(0.00879)	(0.00509)	(0.00641)	(0.0144)	(0.00637)	(0.0176)	(0.0245)	(0.0347)	(0.0236)	(0.00809)
Observations	140	140	140	20	20	20	20	20	20	20
R-squared	0.494	0.740	0.636	0.613	0.853	0.508	0.406	0.273	0.700	0.950

Standard errors in parentheses
** p<0.01, * p<0.05

Table A.4: Validation of Group Cohort Size Predictors in Parental Income, Whites

Notes: Documents ability to predict group cohort sizes at later ages with group cohort sizes at earlier ages. Columns (1)-(3) regress actual parental income group cohort shares at ages 16-17 on predicted parental income group cohort shares at ages 16-17 and a constant, where each column uses a different predictor. Columns (4)-(10) run the regression in Column (1) separately by census year 1940-2000. Regressions for white-only sample. All regressions weighted by the square root of the cell size.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dep Var: Educ Group Cohort Share Ages 16-17	All Years	All Years	All Years	1940	1950	1960	1970	1980	1990	2000
Age 7	1.023**			0.974**	1.162**	1.094**	1.146**	1.078**	0.786**	0.647**
	(0.0292)			(0.0196)	(0.132)	(0.0877)	(0.0660)	(0.0373)	(0.0378)	(0.0412)
Ages 4-7, linear		0.833**								
		(0.0429)								
Ages 1-7, linear			0.919**							
			(0.0305)							
Constant	-0.00230	0.0167**	0.00811*	0.00262	-0.0162	-0.00944	-0.0146	-0.00784	0.0214**	0.0353**
	(0.00343)	(0.00520)	(0.00368)	(0.00269)	(0.0159)	(0.0110)	(0.00753)	(0.00397)	(0.00404)	(0.00449)
Observations	140	140	140	20	20	20	20	20	20	20
R-squared	0.899	0.732	0.868	0.993	0.811	0.896	0.944	0.979	0.960	0.932

Standard errors in parentheses
** p<0.01, * p<0.05

Table A.5: Validation of Group Cohort Size Predictors in Parental Education, Blacks

Notes: Columns (1)-(3) regress actual parental education group cohort shares at ages 16-17 on predicted parental education group cohort shares at ages 16-17 and a constant, where each column uses a different predictor. Columns (4)-(10) run the regression in Column (1) separately by census year 1940-2000. Regressions for black-only sample. All regressions weighted by the square root of the cell size.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dep Var: Income Group										
Cohort Share Ages 16-17	All Years	All Years	All Years	1940	1950	1960	1970	1980	1990	2000
Age 7	1.052**			1.022**	1.134**	1.205**	1.165**	1.051**	0.839**	0.761**
	(0.0162)			(0.0141)	(0.123)	(0.0552)	(0.0472)	(0.0192)	(0.0174)	(0.0234)
Ages 4-7, linear		0.903**								
		(0.0225)								
Ages 1-7, linear			0.983**							
			(0.0159)							
Constant	-0.00531*	0.0134**	0.00252	-0.00561	-0.0102	-0.0253*	-0.0174*	-0.00511*	0.0164**	0.0246**
	(0.00232)	(0.00329)	(0.00231)	(0.00290)	(0.0188)	(0.00970)	(0.00675)	(0.00231)	(0.00208)	(0.00288)
Observations	280	280	280	40	40	40	40	40	40	40
R-squared	0.938	0.853	0.933	0.993	0.692	0.926	0.941	0.987	0.984	0.965

Standard errors in parentheses
** p<0.01, * p<0.05

Table A.6: Validation of Group Cohort Size Predictors in Parental Income, Blacks

Notes: Columns (1)-(3) regress actual parental income group cohort shares at ages 16-17 on predicted parental income group cohort shares at ages 16-17 and a constant, where each column uses a different predictor. Columns (4)-(10) run the regression in Column (1) separately by census year 1940-2000. Regressions for black-only sample. All regressions weighted by the square root of the cell size.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent Var	Highest Grade Attained, Age 26-29							Enrollment	
Sample	All	Boys	Girls	South	North	Urban	Rural	Age 16-18	Age 19-21
Intercept 1940	8.123**	8.311**	8.704**	7.389**	8.972**	9.141**	8.144**	0.461**	0.0119
	(0.209)	(0.291)	(0.297)	(0.322)	(0.295)	(0.292)	(0.239)	(0.00865)	(0.0434)
Intercept 1960	10.25**	10.48**	10.39**	9.654**	10.84**	10.80**	9.986**	0.665**	0.120**
	(0.239)	(0.281)	(0.240)	(0.284)	(0.261)	(0.230)	(0.218)	(0.00818)	(0.0383)
Intercept 1970	11.13**	11.20**	11.55**	11.12**	11.37**			0.793**	0.194**
	(0.218)	(0.212)	(0.213)	(0.298)	(0.193)			(0.00651)	(0.0313)
Intercept 1980	12.01**	12.17**	12.08**	11.62**	12.34**			0.762**	0.220**
	(0.189)	(0.213)	(0.216)	(0.244)	(0.211)			(0.00660)	(0.0317)
Intercept 1990	12.20**	12.20**	12.48**	11.89**	12.52**	12.53**	11.64**	0.829**	0.331**
	(0.154)	(0.242)	(0.244)	(0.259)	(0.247)	(0.197)	(0.224)	(0.00746)	(0.0358)
Intercept 2000	12.49**	12.46**	12.69**	12.27**	12.79**			0.860**	0.355**
	(0.189)	(0.256)	(0.247)	(0.280)	(0.250)			(0.00767)	(0.0371)
Slope 1940	0.365**	0.397**	0.320**	0.536**	0.305**	0.338**	0.463**	0.0391**	0.0302**
	(0.0336)	(0.0539)	(0.0542)	(0.0662)	(0.0525)	(0.0610)	(0.0633)	(0.00160)	(0.00799)
Slope 1960	0.254**	0.286**	0.242**	0.420**	0.200**	0.217**	0.278**	0.0183**	0.0280**
	(0.0409)	(0.0481)	(0.0405)	(0.0548)	(0.0426)	(0.0366)	(0.0444)	(0.00140)	(0.00650)
Slope 1970	0.223**	0.289**	0.152**	0.266**	0.221**			0.0149**	0.0309**
	(0.0373)	(0.0352)	(0.0351)	(0.0512)	(0.0317)			(0.00108)	(0.00519)
Slope 1980	0.199**	0.227**	0.208**	0.292**	0.188**			0.0154**	0.0297**
	(0.0308)	(0.0355)	(0.0359)	(0.0421)	(0.0345)			(0.00110)	(0.00526)
Slope 1990	0.164**	0.184**	0.152**	0.232**	0.147**	0.162**	0.198**	0.0121**	0.0371**
	(0.0245)	(0.0408)	(0.0409)	(0.0451)	(0.0408)	(0.0319)	(0.0409)	(0.00126)	(0.00602)
Slope 2000	0.175**	0.187**	0.199**	0.223**	0.169**			0.0115**	0.0364**
	(0.0301)	(0.0422)	(0.0404)	(0.0475)	(0.0406)			(0.00127)	(0.00611)
Observations	60	60	60	60	60	28	28	60	60
R-squared	1.000	0.999	0.999	0.999	0.999	1.000	0.999	1.000	0.987

Standard errors in parentheses
** p<0.01, * p<0.05

Table A.7: Mobility Estimates in Parental Income Deciles, Whites

Notes: Displays estimated intercepts and slopes of children's schooling gradients with respect to parental income deciles for whites. Presents estimates of α_t and β_t from Equation (7). Regressions weighted by square root of estimated cell sizes.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent Var	Highest Grade Attained, Age 26-29							Enrollment	
Sample	All	Boys	Girls	South	North	Urban	Rural	Age 16-18	Age 19-21
Intercept 1940	5.170**	5.335**	6.214**	5.466**	7.850**	6.634**	5.059**	0.404**	0.0543**
	(0.314)	(0.327)	(0.344)	(0.251)	(0.398)	(0.339)	(0.208)	(0.00769)	(0.0171)
Intercept 1960	8.271**	8.265**	9.075**	8.088**	10.03**	9.350**	7.400**	0.623**	0.168**
	(0.264)	(0.287)	(0.254)	(0.213)	(0.262)	(0.239)	(0.193)	(0.00632)	(0.0136)
Intercept 1970	10.39**	10.59**	10.83**	10.57**	10.72**			0.764**	0.175**
	(0.253)	(0.195)	(0.204)	(0.199)	(0.119)			(0.00466)	(0.0101)
Intercept 1980	11.62**	11.82**	11.84**	11.66**	12.20**			0.789**	0.260**
	(0.203)	(0.208)	(0.211)	(0.186)	(0.142)			(0.00500)	(0.0106)
Intercept 1990	11.81**	11.82**	12.16**	11.87**	12.16**	12.03**	11.57**	0.838**	0.333**
	(0.160)	(0.233)	(0.244)	(0.206)	(0.167)	(0.179)	(0.247)	(0.00561)	(0.0121)
Intercept 2000	12.16**	12.15**	12.49**	12.33**	12.31**			0.865**	0.321**
	(0.203)	(0.243)	(0.242)	(0.212)	(0.170)			(0.00577)	(0.0124)
Slope 1940	0.648**	0.682**	0.624**	0.676**	0.291**	0.588**	0.744**	0.0475**	0.0270**
	(0.0867)	(0.116)	(0.120)	(0.104)	(0.0989)	(0.120)	(0.120)	(0.00271)	(0.00600)
Slope 1960	0.524**	0.688**	0.486**	0.833**	0.173**	0.336**	1.015**	0.0218**	0.0150**
	(0.0739)	(0.102)	(0.0818)	(0.100)	(0.0609)	(0.0671)	(0.142)	(0.00202)	(0.00434)
Slope 1970	0.255**	0.272**	0.200**	0.284**	0.243**			0.0166**	0.0279**
	(0.0639)	(0.0540)	(0.0575)	(0.0672)	(0.0299)			(0.00127)	(0.00277)
Slope 1980	0.207**	0.192**	0.217**	0.217**	0.149**			0.0135**	0.0260**
	(0.0451)	(0.0485)	(0.0499)	(0.0496)	(0.0297)			(0.00118)	(0.00249)
Slope 1990	0.183**	0.174**	0.194**	0.204**	0.157**	0.181**	0.189**	0.0114**	0.0321**
	(0.0325)	(0.0521)	(0.0544)	(0.0500)	(0.0339)	(0.0390)	(0.0642)	(0.00125)	(0.00269)
Slope 2000	0.166**	0.165**	0.163**	0.177**	0.159**			0.0104**	0.0358**
	(0.0381)	(0.0532)	(0.0541)	(0.0488)	(0.0356)			(0.00129)	(0.00273)
Observations	60	60	60	60	60	28	25	60	60
R-squared	0.999	0.999	0.999	0.999	0.999	0.999	0.998	1.000	0.996

Standard errors in parentheses
** p<0.01, * p<0.05

Table A.8: Mobility Estimates in Parental Income Deciles, Blacks

Notes: Displays estimated intercepts and slopes of children's schooling gradients with respect to parental income deciles for blacks. Estimates correspond to α_t and β_t from Equation (7). Regressions weighted by square root of estimated cell sizes.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
				PSID							NLSY79					NLSY97		
Child Educ	All	1980-89	1990-99	2000-09	Male	Female	White	Black	All	Male	Female	White	Black	All	Male	Female	White	Black
Par Income	0.350**	0.275**	0.391**	0.427**	0.371**	0.330**	0.342**	0.282**	0.276**	0.268**	0.283**	0.272**	0.262**	0.363**	0.365**	0.367**	0.366**	0.278**
	(0.0353)	(0.0484)	(0.0449)	(0.0321)	(0.0423)	(0.0363)	(0.0392)	(0.0476)	(0.0240)	(0.0347)	(0.0369)	(0.0249)	(0.0704)	(0.0167)	(0.0253)	(0.0275)	(0.0208)	(0.0592)
Dep	-0.237	-0.216	0.0268	-1.046	-0.0809	-0.211	-0.255	0.105	-0.916*	-1.056	-0.582	-1.010*	-0.339	-0.497*	-0.344	-0.483	-0.602	-0.902
	(0.649)	(0.944)	(0.829)	(0.543)	(0.671)	(0.825)	(0.827)	(0.469)	(0.379)	(0.504)	(0.654)	(0.429)	(0.489)	(0.228)	(0.314)	(0.421)	(0.312)	(0.538)
Par Inc * Dep	-0.0534	-0.0120	-0.128	0.0796	-0.112	0.0211	-0.0375	-0.158	0.0900	0.0995	0.0778	0.106	-0.0654	-0.0392	-0.0351	-0.0543	-0.0328	0.0537
	(0.106)	(0.157)	(0.133)	(0.0893)	(0.111)	(0.131)	(0.127)	(0.105)	(0.0637)	(0.0845)	(0.110)	(0.0692)	(0.144)	(0.0393)	(0.0539)	(0.0725)	(0.0517)	(0.115)
Constant	2.675**	3.015**	2.465**	2.415**	2.451**	2.880**	2.767**	2.584**	3.142**	2.978**	3.306**	3.178**	3.052**	2.731**	2.386**	3.001**	2.751**	3.124**
	(0.223)	(0.304)	(0.283)	(0.202)	(0.266)	(0.229)	(0.255)	(0.206)	(0.145)	(0.210)	(0.223)	(0.153)	(0.258)	(0.104)	(0.160)	(0.170)	(0.134)	(0.279)
Observations	20	20	20	20	20	20	20	20	20	20	20	20	20	20	20	20	20	20
R-squared	0.874	0.692	0.840	0.933	0.849	0.850	0.841	0.712	0.917	0.846	0.817	0.908	0.553	0.975	0.947	0.932	0.962	0.708

Robust standard errors in parentheses
** p<0.01, * p<0.05

Table A.9: Parallel Trends for Child Education Rank in Parental Income Decile

Notes: Each column displays estimates of equation (5) for children's education rank at ages 26-29 by parental income deciles. Parental characteristics measured when children are age 17, or earlier in adolescence if not observed at age 17. Children's schooling excludes zeros. Parental income defined as sum of mother's and father's earnings when possible, otherwise as total family income. Children with zero parental income at age 17 excluded from the parental income figures. Parental income deciles calculated separately by year. Sample weights used in all calculations. Regressions on collapsed data weighted by square of cell size.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
					PSID						NLSY79					NLSY97			GSS	OCG73
Child Educ	All	1980-89	1990-99	2000-09	Male	Female	White	Black	All	Male	Female	White	Black	All	Male	Female	White	Black	All	All
Par Educ	0.530**	0.498**	0.576**	0.533**	0.539**	0.527**	0.531**	0.402**	0.514**	0.524**	0.510**	0.516**	0.406**	0.386**	0.397**	0.379**	0.389**	0.310**	0.351**	0.498**
	(0.0174)	(0.0272)	(0.0292)	(0.0343)	(0.0246)	(0.0579)	(0.0187)	(0.0460)	(0.0302)	(0.0293)	(0.0367)	(0.0315)	(0.0699)	(0.0144)	(0.0206)	(0.0217)	(0.0134)	(0.0261)	(0.0449)	(0.0314)
Dep	0.185	0.289	0.681	-0.833	0.169	0.446	0.117	0.0496	0.00369	-0.128	0.424	0.0639	-0.925	-0.353	-0.407	-0.0656	-0.291	-1.100**	0.311	0.0909
	(0.296)	(0.493)	(0.489)	(0.551)	(0.357)	(0.508)	(0.369)	(0.409)	(0.456)	(0.403)	(0.627)	(0.502)	(0.605)	(0.179)	(0.236)	(0.298)	(0.180)	(0.240)	(0.889)	(0.523)
Par Educ * Dep	-0.107	-0.0835	-0.214*	0.0661	-0.128	-0.0993	-0.0946	-0.0876	-0.0840	-0.0482	-0.155	-0.0901	0.118	-0.0128	0.00252	-0.0431	-0.0323	0.204**	-0.0756	-0.105
	(0.0523)	(0.0869)	(0.0854)	(0.0978)	(0.0627)	(0.0819)	(0.0602)	(0.106)	(0.0827)	(0.0726)	(0.115)	(0.0886)	(0.150)	(0.0330)	(0.0430)	(0.0560)	(0.0320)	(0.0517)	(0.162)	(0.0911)
Constant	1.742**	1.828**	1.481**	1.920**	1.593**	1.818**	1.788**	2.095**	2.095**	1.821**	2.330**	2.082**	2.426**	2.927**	2.580**	3.244**	2.971**	2.967**	3.168**	2.082**
	(0.104)	(0.160)	(0.176)	(0.206)	(0.148)	(0.359)	(0.116)	(0.191)	(0.172)	(0.170)	(0.206)	(0.183)	(0.291)	(0.0853)	(0.123)	(0.127)	(0.0819)	(0.128)	(0.253)	(0.187)
Observations	20	20	20	19	20	20	20	20	16	16	16	16	15	16	16	16	16	16	20	20
R-squared	0.984	0.958	0.963	0.951	0.972	0.896	0.982	0.848	0.965	0.969	0.945	0.961	0.826	0.987	0.977	0.967	0.989	0.959	0.801	0.946

Robust standard errors in parentheses

** p<0.01, * p<0.05

Table A.10: Parallel Trends for Child Education Rank in Parental Education Rank

Notes: Each column displays estimates of equation (5) for children's education rank at ages 26-29 by parental education rank. Parental characteristics measured when children are age 17, or earlier in adolescence if not observed at age 17. Children's schooling excludes zeros. Parental education rank defined as the percentile of the average of mother's and father's educational attainment, where ties are assigned the minimum rank over the tied interval. Sample weights used in all calculations. Regressions on collapsed data weighted by square of cell size.

	White					Black				
Variable	Mean	Std Dev	Min	Max	Count	Mean	Std Dev	Min	Max	Count
Mobility: Parental Educ	0.37	0.11	0.00	0.72	256	0.31	0.13	0.05	0.69	159
Mobility: Parental Income	0.21	0.14	-0.28	0.70	305	0.22	0.20	-0.40	0.90	204
Log Household Earnings	10.21	0.56	8.31	11.01	297	10.20	0.60	8.31	11.01	246
Earnings Inequality: p75-p25	0.99	0.19	0.61	1.66	297	1.01	0.19	0.65	1.66	246
Urban Share	0.64	0.17	0.00	0.94	153	0.66	0.14	0.36	0.94	123
Share Black	0.10	0.13	0.00	0.71	297	0.12	0.13	0.00	0.71	241
Teen Birth Rate	0.07	0.03	0.01	0.17	297	0.07	0.03	0.01	0.16	241
Teen Employment Rate	0.36	0.08	0.16	0.56	297	0.36	0.08	0.17	0.53	241
Dropout Age	16.22	0.75	14.00	18.00	192	16.17	0.75	14.00	18.00	150
Class Size	24.85	4.23	17.30	37.59	147	25.79	4.31	17.30	37.59	109
Relative Teacher Wage	1.00	0.18	0.64	2.04	147	1.00	0.20	0.64	2.04	109
Share Move State	0.54	0.20	0.02	0.86	297	0.55	0.20	0.02	0.86	246
Black HS per Capita	0.0025	0.0019	0.0000	0.0091	143	0.0021	0.0017	0.0000	0.0091	105
White HS per Capita	0.0029	0.0016	0.0000	0.0076	143	0.0026	0.0014	0.0004	0.0068	105
Black HS PC - South	0.0015	0.0018	0.0000	0.0091	47	0.0016	0.0018	0.0000	0.0091	46
White HS PC - South	0.0027	0.0012	0.0006	0.0061	47	0.0027	0.0012	0.0006	0.0061	46

Table A.11: Summary Statistics for Variables in Panel Analysis

Notes: Table displays summary statistics for whites and blacks separately. Mobility varies by year, state-of-birth and race. All other variables vary by year and state-of-birth, or year and state. White and black samples vary due to requirement that gradients underlying mobility statistics be non-missing for at least 60% of parental group levels. Mobility statistics for state-of-birth panel analysis constructed on children age 20-29. "HS per capita" and "HS PC" refer to number of public high schools per age 14-17 year-old children in state-of-birth and year, and are only matched to census years 1940, 1960 and 1970 from high school data sets in years 1928, 1949 and 1952, respectively to match age at high school attendance as closely as possible with available data. Dropout age, class size and relative teacher wage matched to to census from year at which age 20-29 year-old children would have been approximately age 14, and are only matched to censuses 1940-70 using data from Stephens and Yang (2014). Log household earnings and earnings inequality constructed from household earnings distribution (head + spouse) for heads of household age 30-65 in year children turn 20-29 and living in child's state of birth. Migration and urbanization constructed from age 20-29 year-olds by state of birth and year.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		OCG1973			OCG1962	OCG1973		OCG1962	
	Cohort 1920	Cohort 1930	Cohort 1940	Cohort 1920	Cohort 1930				
VARIABLES			Log Earnings			Education	Log Earnings	Education	Log Earnings
Education	0.094**	0.085**	0.075**	0.075*	0.080**				
	(0.012)	(0.015)	(0.015)	(0.032)	(0.017)				
Father HS Grad	-0.188	-0.039	-0.016	-0.200	0.207				
	(0.223)	(0.264)	(0.279)	(0.599)	(0.322)				
Father Some College	0.197	-0.195	0.099	-0.406	0.144				
	(0.223)	(0.264)	(0.279)	(0.599)	(0.322)				
Education * Father HS Grad	0.017	0.002	0.003	0.021	-0.004				
	(0.017)	(0.021)	(0.022)	(0.045)	(0.024)				
Education * Father Some College	-0.006	0.010	-0.004	0.039	-0.000				
	(0.017)	(0.021)	(0.022)	(0.045)	(0.024)				
Father's Education						0.429**	0.055**	0.476**	0.057**
						(0.037)	(0.006)	(0.022)	(0.014)
Cohort1930						0.707	0.232*	1.151*	-0.131
						(0.648)	(0.100)	(0.385)	(0.242)
Cohort1940						1.070	0.087		
						(0.648)	(0.100)		
Cohort1930*Father's Education						-0.025	-0.014	-0.057	0.005
						(0.052)	(0.008)	(0.031)	(0.020)
Cohort1940*Father's Education						-0.041	-0.010		
						(0.052)	(0.008)		
Constant	8.104**	8.302**	8.298**	7.783**	7.566**	7.898**	8.733**	7.029**	8.118**
	(0.158)	(0.187)	(0.198)	(0.423)	(0.227)	(0.458)	(0.071)	(0.272)	(0.171)
Observations	30	30	30	18	18	33	33	10	10
R-squared	0.892	0.826	0.750	0.696	0.851	0.933	0.891	0.993	0.864

Standard errors in parentheses
** p<0.01, * p<0.05

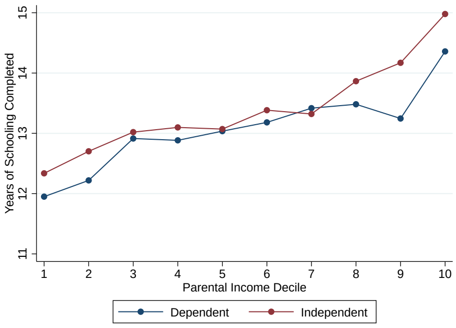
Table A.12: Returns to Schooling by Parental Group and Child Outcome Gradients in OCG

Notes: Documents shared returns to schooling across father's education groups, and children's schooling and log earnings gradients in father's education, together suggesting that educational mobility gains likely increased income mobility gains. Displays various regressions on OCG1962 and OCG1973 data. Columns (1)-(5) assess whether the returns to schooling differ by parental background. Columns (6)-(9) assess whether changes in mobility can be observed directly in OCG data. All regresssions restricted to whites. Underlying data collapsed prior to regression to child's education by father's education by cohort cells in columns (1)-(5) and to father's education by cohort level in columns (6)-(9). Sample weights used in collapse but not in regressions. Note father's education can take on any integer values between 7 and 17 in OCG1973 sample, but can only take on values of 8, 10, 12, 14, 16 in OCG1962. Columns (1)-(5) regress log of earnings on years of schooling, father's education, and the interaction of these two variables, separately for each 10-year birth cohort and OCG data set. Columns (6) and (7) regress education and log of earnings, respectively, on father's education, 10-year cohort dummies, and the interaction of these two variables in the OCG73 data. Columns (8) and (9) repeat the exercise in columns (6) and (7), respectively, on the OCG62 data.

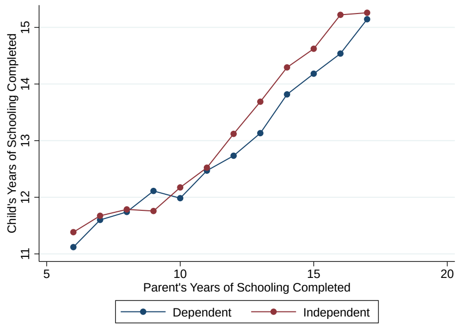
	White				Black			
	Parental Income		Parental Education		Parental Income		Parental Education	
Year	% Missing	% Zero	% Missing	% Zero	% Missing	% Zero	% Missing	% Zero
1940	11.7	40.1	3.8	6.1	13.1	41.3	5.2	11.9
1960	5.4	26.5	0.2	2.1	6.5	26.4	0.5	4.7
1970	4.2	16.1	4.8	0.5	5.4	21.0	8.3	2.3
1980	16.2	12.8	5.7	0.3	22.9	23.3	10.2	1.2
1990	18.3	9.7	2.7	0.5	24.2	18.7	4.2	1.2
2000	19.3	11.4	2.8	0.5	27.3	18.6	4.7	0.7

Table A.13: Percent of Children with Missing and Zero Values for Parental Characteristics, by Year and Race

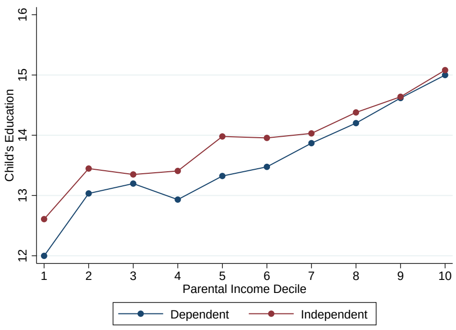
Notes: Sample restricted to children living with parents age 22-25 in census data.



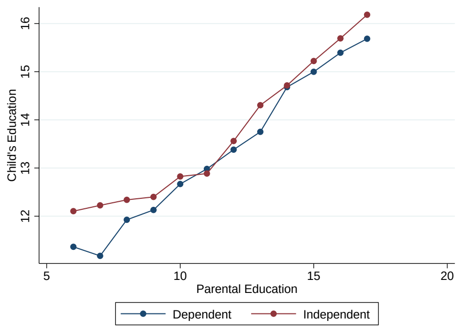
(a) Parental Income, PSID



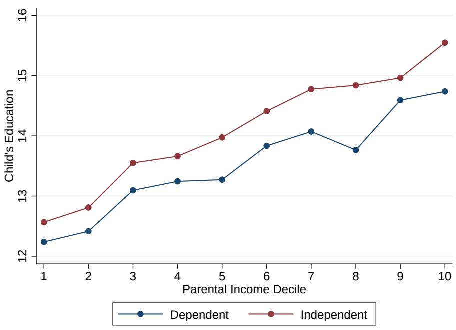
(b) Parental Education, PSID



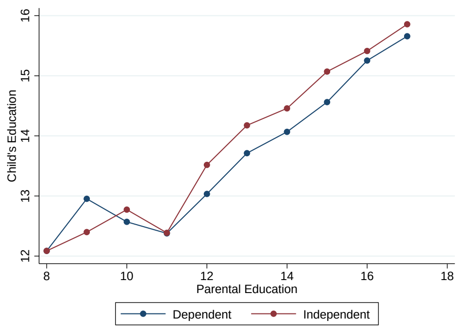
(c) Parental Income, NLSY79



(d) Parental Education, NLSY79



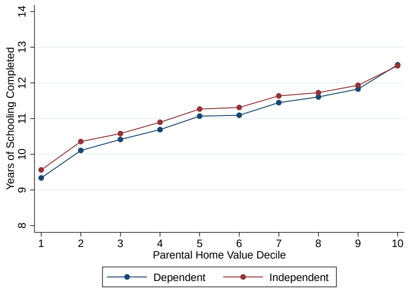
(e) Parental Income, NLSY97



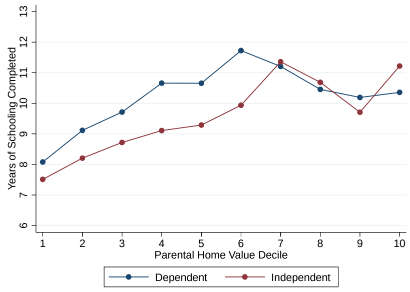
(f) Parental Education, NLSY97

Figure I: Highest Grade Attained at Ages 26-29 by Parental Characteristics at Age 17

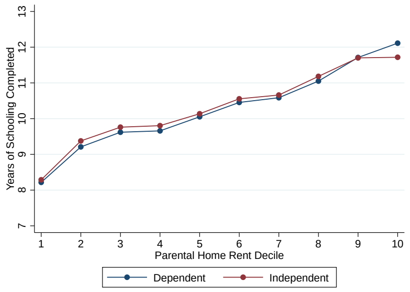
Notes: Figures based on data from PSID, NLSY79, and NLSY97 pooling years 1968-2011, 1994-2011, and 1997-2011 respectively. Parental characteristics measured when children are age 17, or earlier in adolescence if not observed at age 17. Children's schooling at ages 26-29 is set to missing when recorded as zero. Children with zero parental income at age 17 excluded from the parental income figures. Income deciles calculated separately by year. Parental education defined as average of mother's and father's educational attainment, or educational attainment of the single parent when second parent not observed. Children with parental education in the bottom 2% of the parental education distribution excluded from the parental education figures. Sample weights used in all calculations.



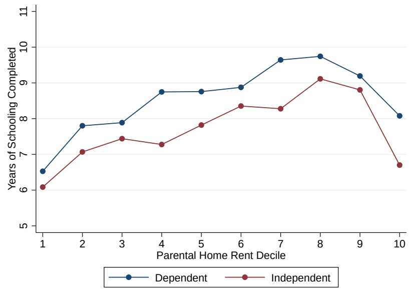
(a) By Parental Home Value, Whites



(b) By Parental Home Value, Blacks



(c) By Parental Rent, Whites



(d) By Parental Rent, Blacks

Figure II: Final Schooling at Ages 24-27 in 1940 by Parental Group Status

Notes: Figures plot highest grade attained for ages 24-27 by parental home value or rent deciles based on matched 1930-1940 census data. Families with zero rent and earnings in 1930 excluded. Deciles calculated on full population of parents with any children age 10-17 in 1930, including all non-farm owner-occupied or renter-occupied units, weighting by number of children.

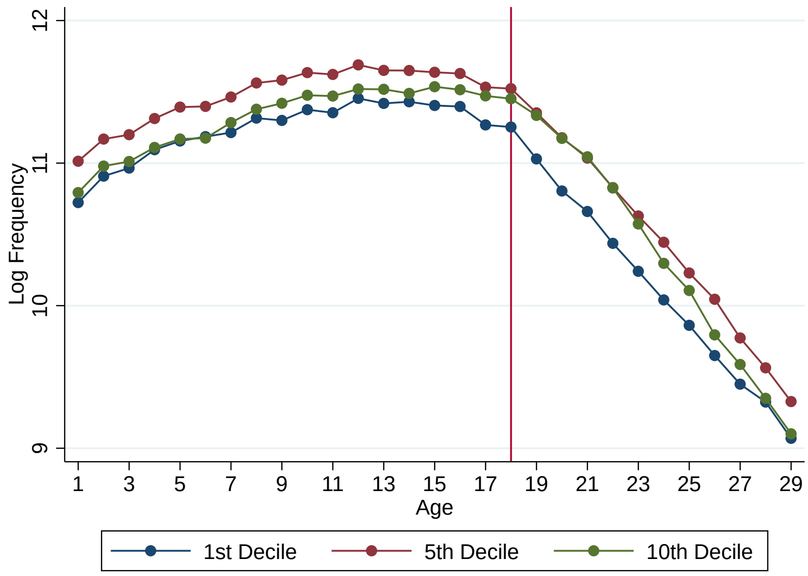
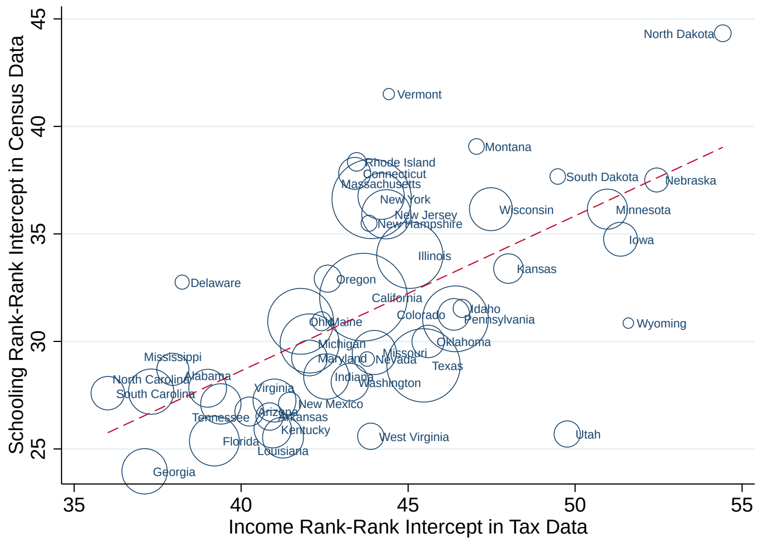
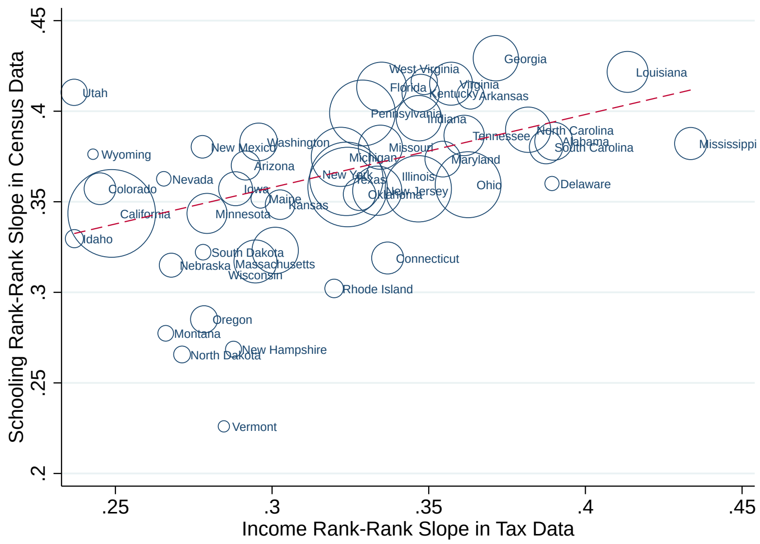


Figure III: Number of Dependent Children by Age and Parental Income Decile, 1940

Notes: Figures plot frequencies for white native-born children living with parents by age and race in 1940 100% IPUMS data sample.



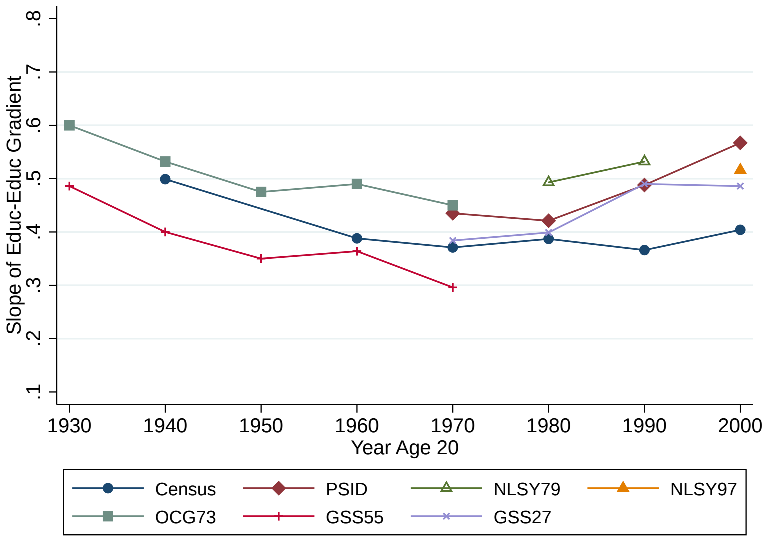
(a) Absolute Upward Mobility



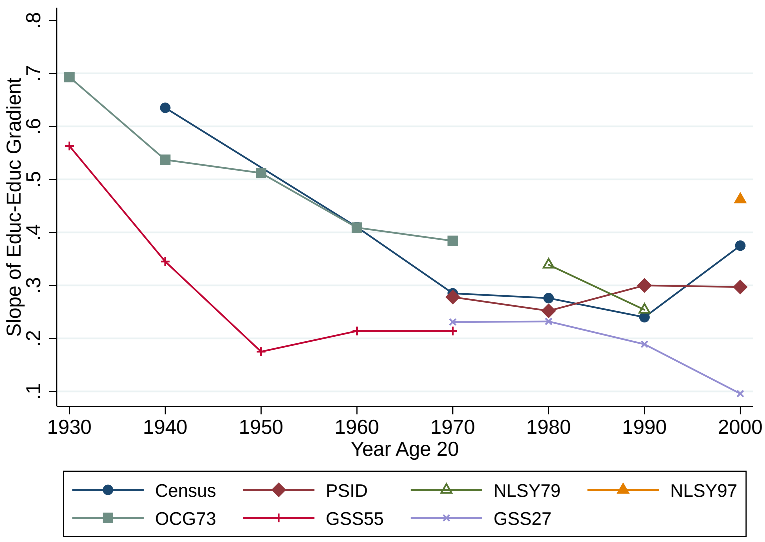
(b) Relative Mobility

Figure IV: Comparison of Mobility Estimates by State in Census and Tax Data, 2000

Notes: Panel (A) plots intercepts from regression of child education rank on parental education rank in census data with adjustment for independents, against intercepts from regression of child income rank on parental income rank in U.S. population tax records. Panel (B) plots slopes from the same regressions. All races pooled. Education measured as highest grade attained. Education ranks computed on national sample for each age and year separately with midpoints of rank intervals assigned to mass points. Children's education measured at ages 26-29. Points weighted by estimated total number of children age 26-29 in census. Census regressions pool data from 1980, 1990 and 2000 censuses. Description of income rank-rank mobility estimates available in Chetty et al. (2014a).



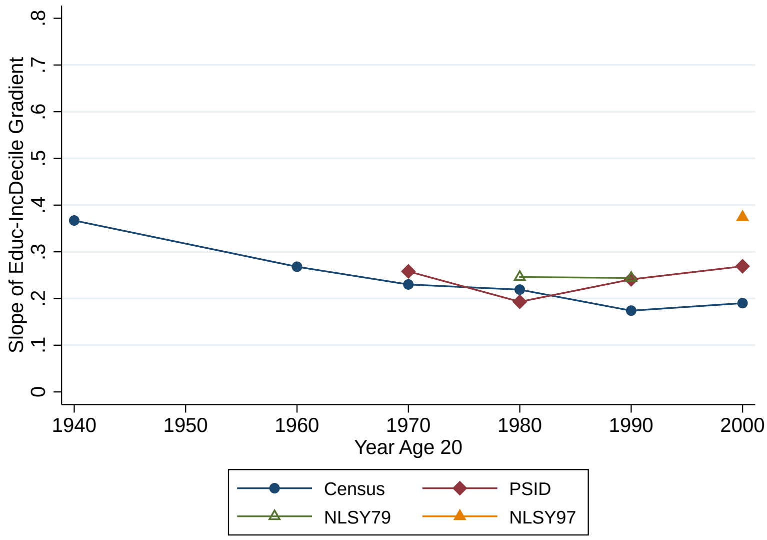
(a) Whites



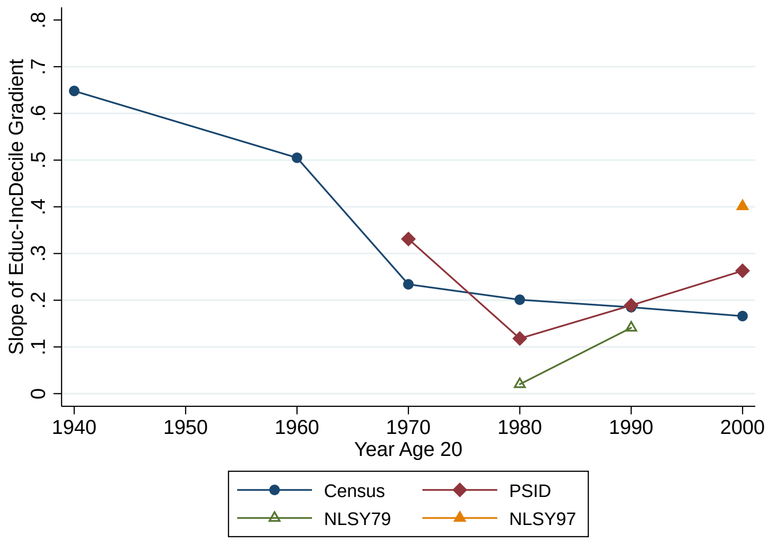
(b) Blacks

Figure V: Educational Mobility in Parental Education, 1930-2000

Notes: Figure plots slopes from regression of child education on parental education by year for whites in Panel A and blacks in Panel B. Child and parental education defined as described in Section 3.1. Census estimates adjusted for independents as described in text. "Year" in census defined as year available cohorts turn ages 26-29. "Year" in PSID, NLSY79, and NLSY97 defined as decades, e.g. "1980" reflects cohorts of children turning 26-29 during the years 1980-89. "Year" in OCG73, GSS55 and GSS27 defined as year cohorts would have turned 20-29. "GSS55" and "GSS27" refer to cohorts in the General Social Survey age 55-65 and 27-37, respectively, over the GSS sample period 1972-2012. All estimates make use of sample weights and exclude the bottom 2% of the parental education distribution.



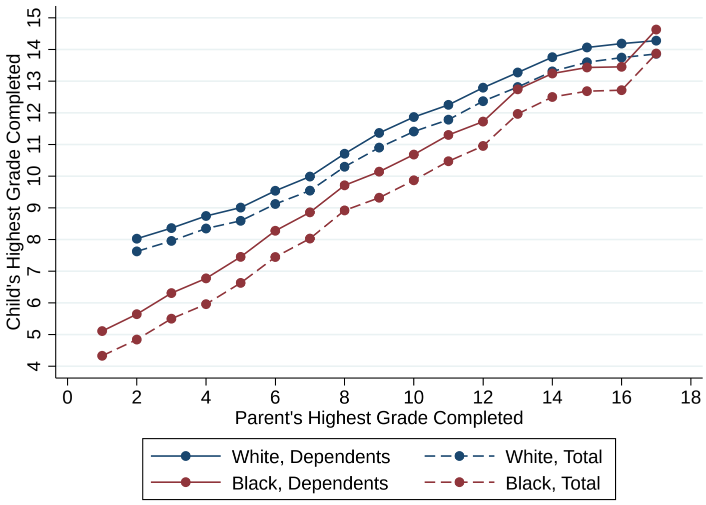
(a) Whites



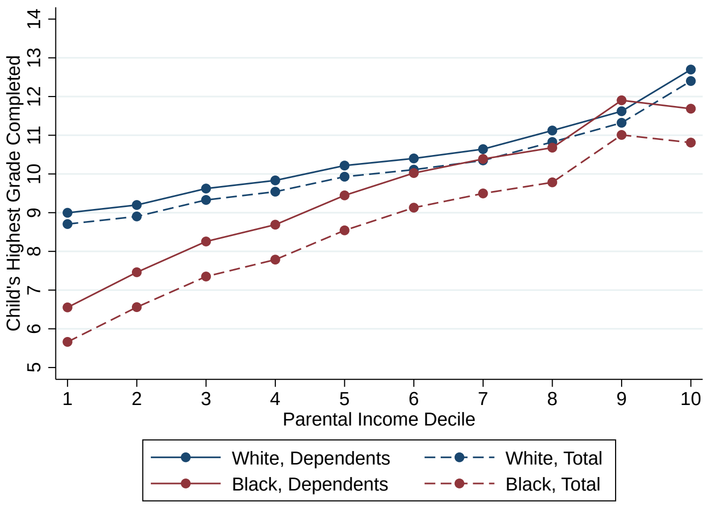
(b) Blacks

Figure VI: Educational Mobility in Parental Income Decile, 1940-2000

Notes: Figure plots slopes from regression of child education on population parental income decile by year for whites in Panel A and blacks in Panel B. Child education and parental income decile defined as described in Section 3.1. Census estimates adjusted for independents as described in text. "Year" in census defined as year available cohorts turn ages 26-29. "Year" in PSID, NLSY79, and NLSY97 defined as decades, e.g. "1980" reflects cohorts of children turning 26-29 during the years 1980-89. All estimates make use of sample weights and exclude zero incomes.



(a) Gradient in Parental Education, Before and After Adjustment



(b) Gradient in Parental Income Decile, Before and After Adjustment

Figure VII: Final Schooling Attainment at ages 26-29 by Parental Group Status, 1940

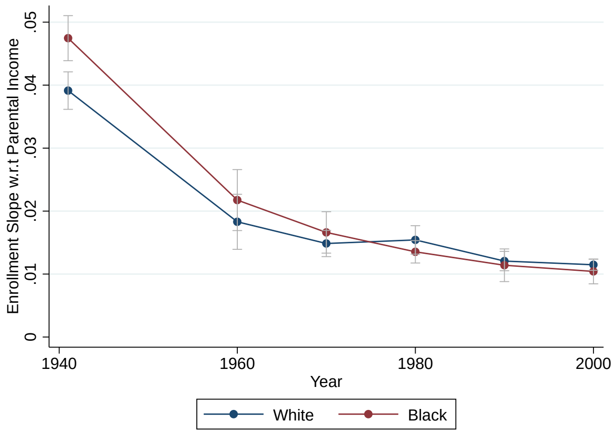
Notes: Figure plots estimated final schooling pooling separate estimates for ages 26-29, using the correction for independent children described in the text. Uncorrected estimates restrict to dependent children who can be linked with parents directly.

(a) Slopes of Gradients in Parental Education

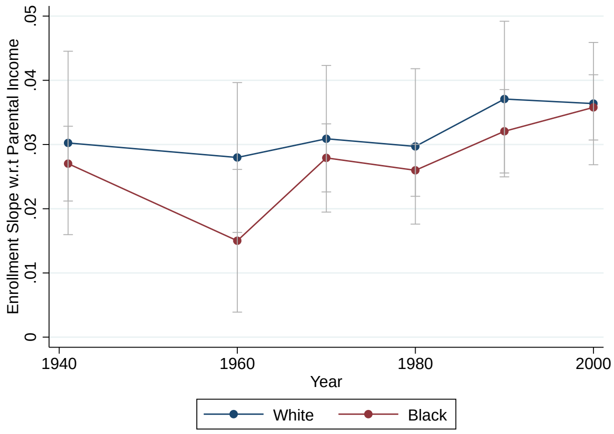
(b) Slopes of Gradients in Parental Income Deciles

Figure VIII: Slopes of Mobility Gradients by Region, 1940-2000

Notes: Restricting to whites. Presents estimated slopes from linear regressions of children's highest grade attained on parental highest grade attained or income decile, using data grouped at the year by race by parental status level. Adjustment for independent children ages 26-29 as described in text. Sample weights are used to construct cell means, and regressions on collapsed data are weighted by the square of cell size. Estimates correspond to slope estimates in Columns (4)-(5) in Tables II and A.7.



(a) "High School Enrollment," Ages 16-18



(b) "College Enrollment," Ages 19-21

Figure IX: Slopes of Enrollment Gradients in Parental Income, 1940-2000

Notes: Presents estimated slopes from linear regressions of children's annual enrollment on parental income decile, using data grouped at the year by race by parental status level. Adjustment for independent children ages 19-21 as described in text. Sample weights are used to construct cell means, and regressions on collapsed data are weighted by the square of cell size. Estimates correspond to slope estimates in Columns (6)-(7) in Tables A.7 and A.8.

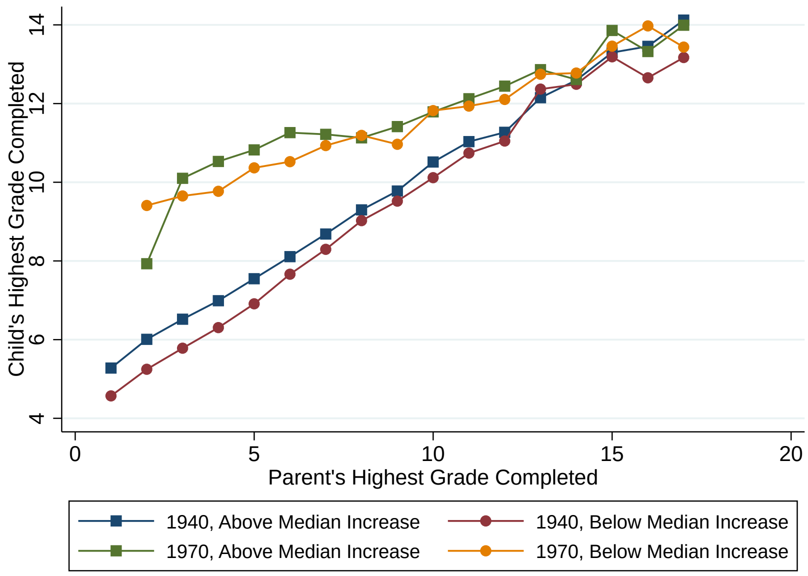


Figure X: Black Mobility in Southern States with Big vs. Small Increases in Black High School Density

Notes: Figure plots highest grade completed at age 20-29 in Southern states with above- and below-median increases in black public high schools per capita. High schools per capita measured in 1928 and 1952.

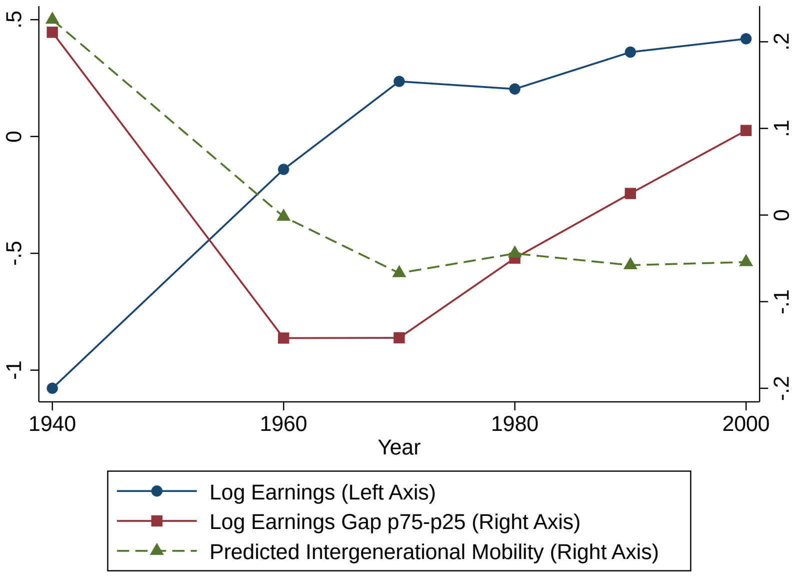
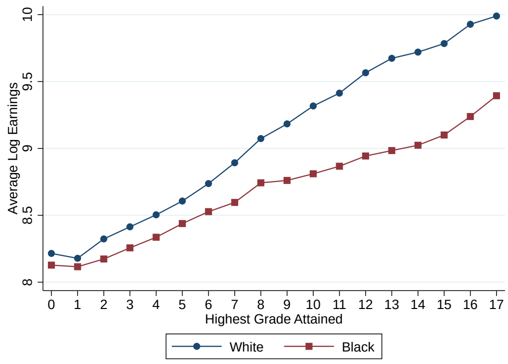
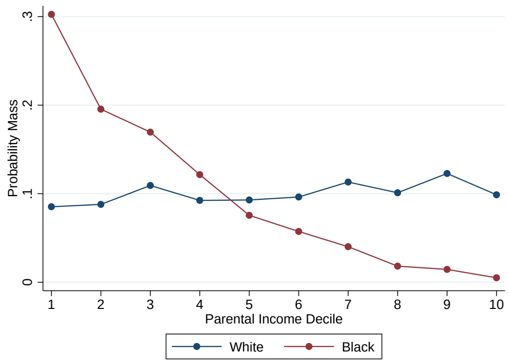


Figure XI: Broad-Based Economic Growth and Intergenerational Mobility

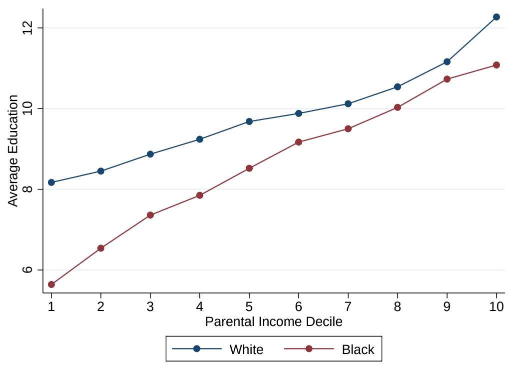
Notes: Restricting to whites. Figure plots weighted state-level averages of log household earnings, log household earnings interquartile gaps (p75-p25), and predicted intergenerational education elasticities using coefficients from fixed effects regression in column (2) of Table VI. Trend lines de-meaned for comparability.



(a) Earnings Functions



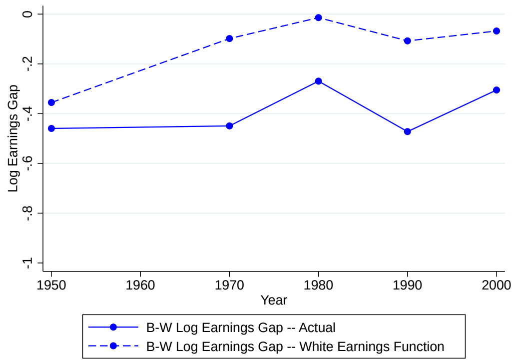
(b) Parental Income Distributions



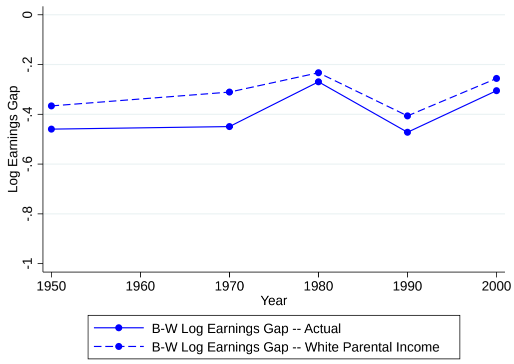
(c) Educational Mobility Gradients

Figure XII: Three Terms of Intergenerational Earnings Decomposition in 1940

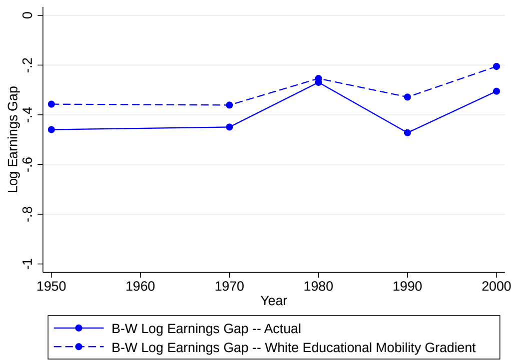
Notes: Earnings functions in panel (a) calculated on men ages 30-35. Parental income in panel (b) calculated for families with children age 13-18. Educational mobility gradients in panel (c) calculated for children age 26-29 using adjustment described in text. All figures reweight white sample to match black sample distribution of state of birth.



(a) Earnings Functions



(b) Parental Income Distributions



(c) Educational Mobility Gradients

Figure XIII: Actual and Counterfactual Black-White Log Earnings Gaps, 1950-2000

Notes: Figures present black-white log earnings difference as calculated from three estimated terms in decomposition, calculated as described in Figure XII. Counterfactuals nonparametrically replace black term by specified white term, one term at a time.

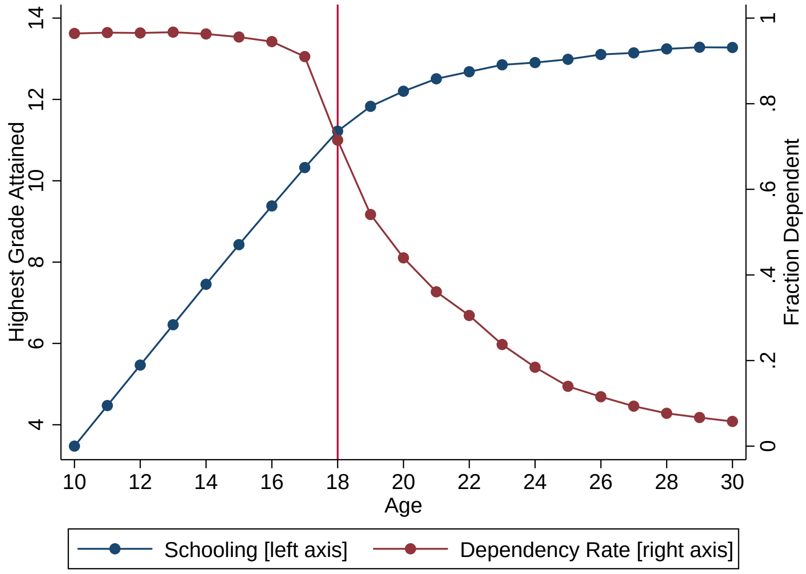


Figure A.1: Schooling and Dependency Status by Age in 1980

Notes: Red line plots fraction of native-born children living with parents by age in 1980. Blue line plots average schooling of native-born children by age in 1980. Whites only, excluding Alaska and Hawaii.

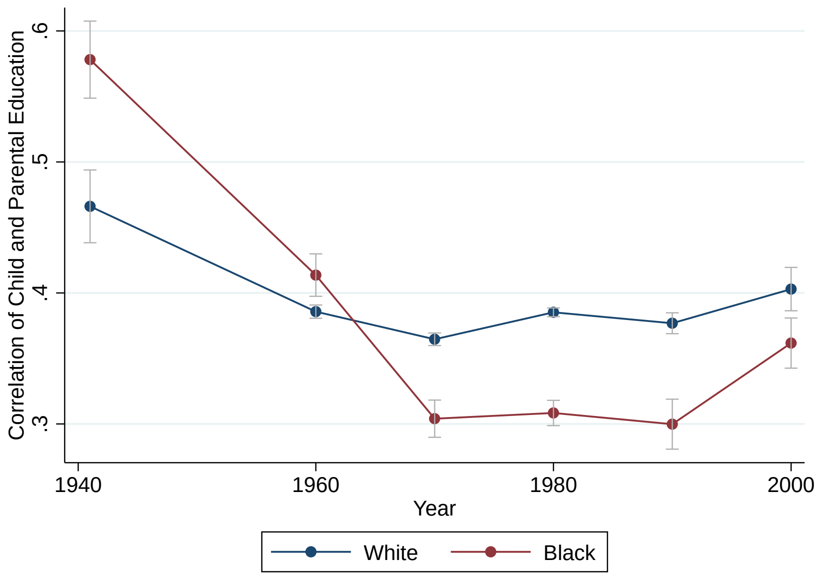


Figure A.2: Child-Parent Educational Correlations by Race, 1940-2000

Notes: Correlations constructed as elasticities multiplied by ratio of standard deviation of parental average education over standard deviation of child average education. Standard errors of correlations adjusted with Moulton factor.

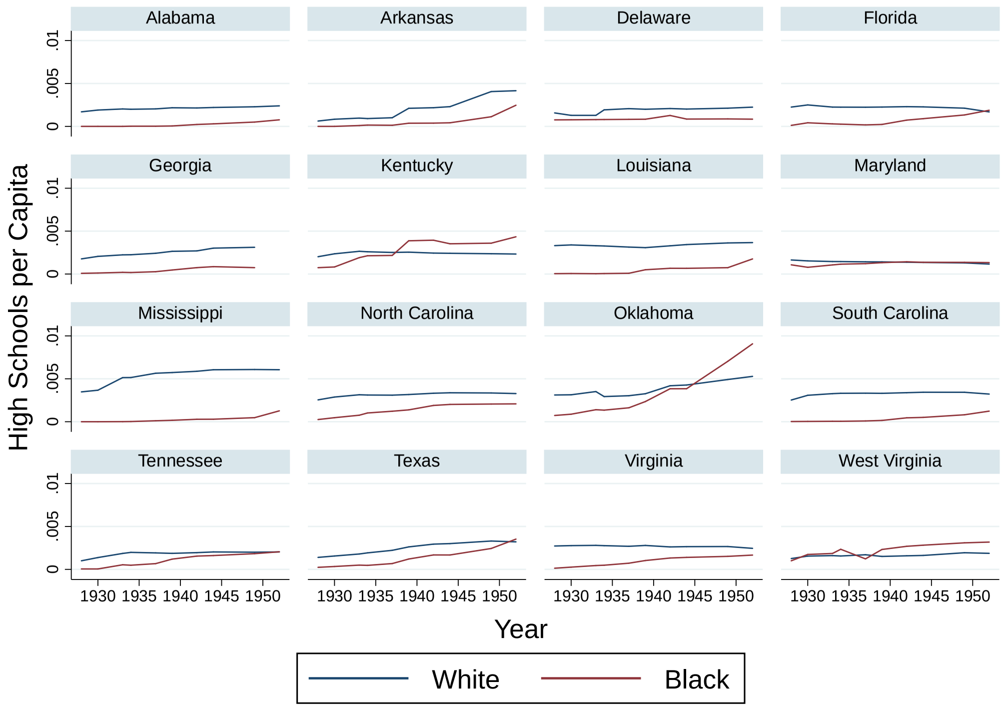
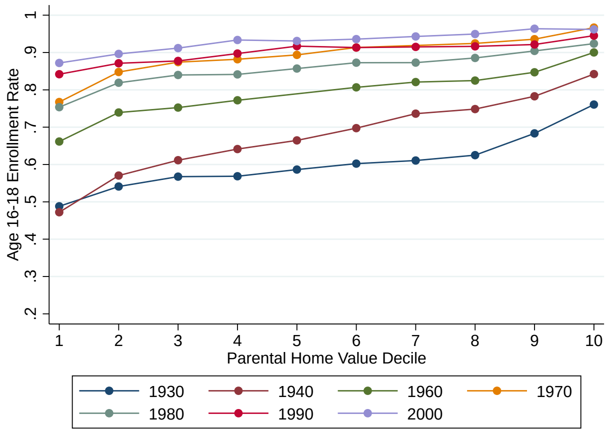
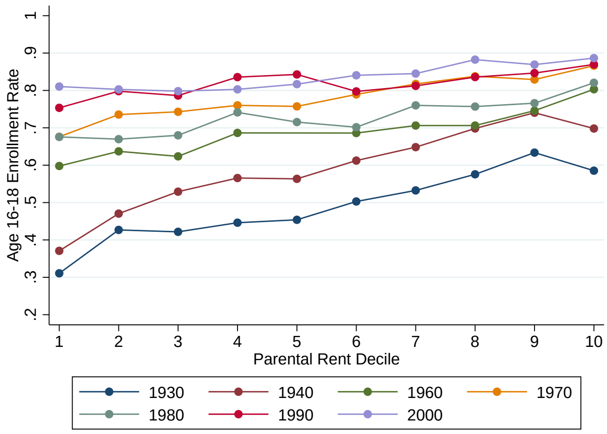


Figure A.3: Black and White High Schools per Capita in U.S. South by State, 1928-1952

Notes: Figure plots number of black and white high schools divided by number of black and white children, respectively, ages 14-17 in each state.



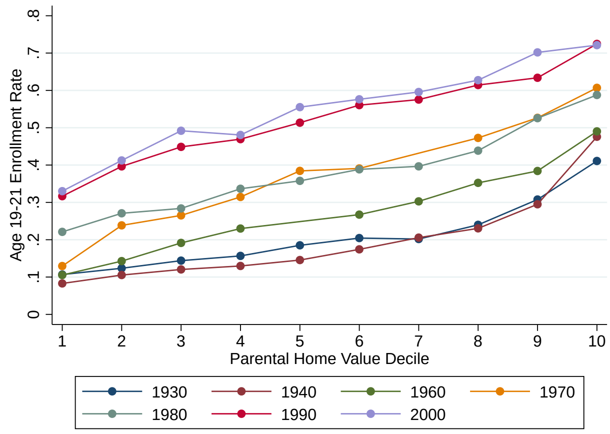
(a) Enrollment by Parental Home Value Deciles



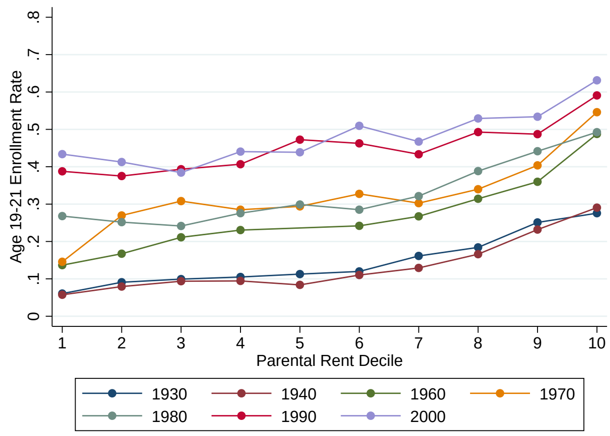
(b) Enrollment by Parental Rent Deciles

Figure A.4: High School Enrollment by Home Value and Rent, Whites 1930-2000

Notes: Restricting to whites. Plots average enrollment for dependent children ages 16-18 by parental home value and rent deciles, by year. Sample weights are used to construct cell means.



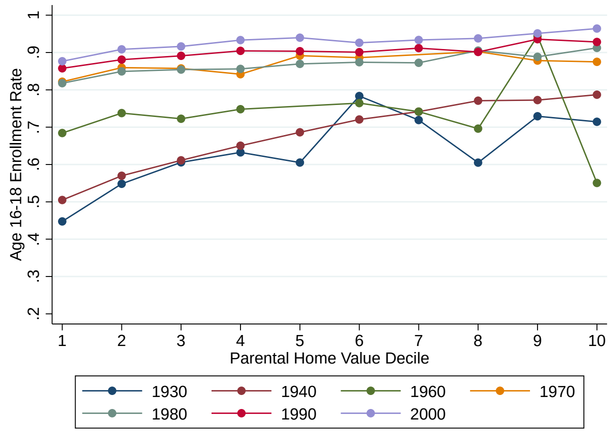
(a) Enrollment by Parental Home Value Deciles



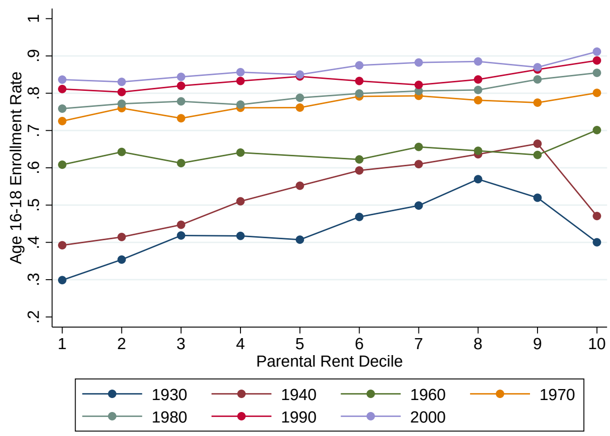
(b) Enrollment by Parental Rent Deciles

Figure A.5: College Enrollment by Home Value and Rent, Whites 1930-2000

Notes: Restricting to whites. Plots average enrollment for ages 19-21 by parental home value and rent deciles, by year. Adjustment for independent children as described in text. Sample weights are used to construct cell means.



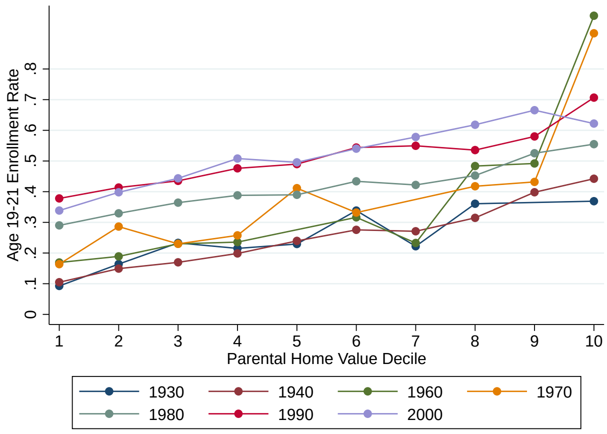
(a) Enrollment by Parental Home Value Deciles



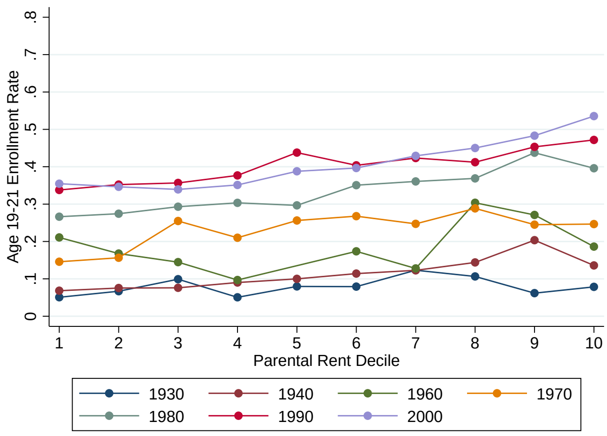
(b) Enrollment by Parental Rent Deciles

Figure A.6: High School Enrollment by Home Value and Rent, Blacks 1930-2000

Notes: Restricting to whites. Plots average enrollment for dependent children ages 16-18 by parental home value and rent deciles, by year. Sample weights are used to construct cell means.



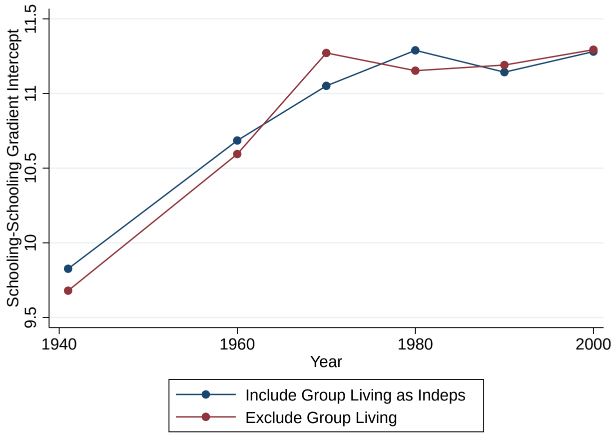
(a) Enrollment by Parental Home Value Deciles



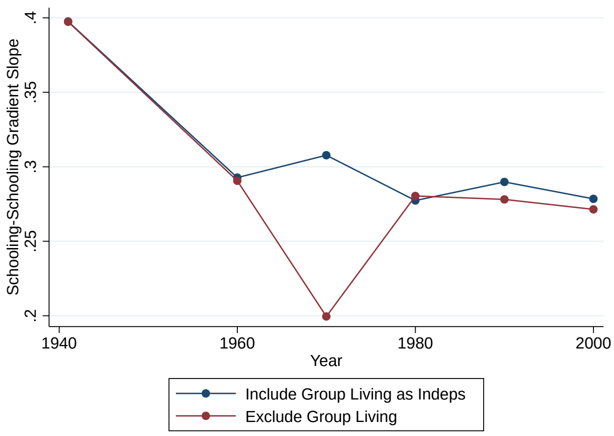
(b) Enrollment by Parental Rent Deciles

Figure A.7: College Enrollment by Home Value and Rent, Blacks 1930-2000

Notes: Restricting to whites. Plots average enrollment for ages 19-21 by parental home value and rent deciles, by year. Adjustment for independent children as described in text. Sample weights are used to construct cell means.



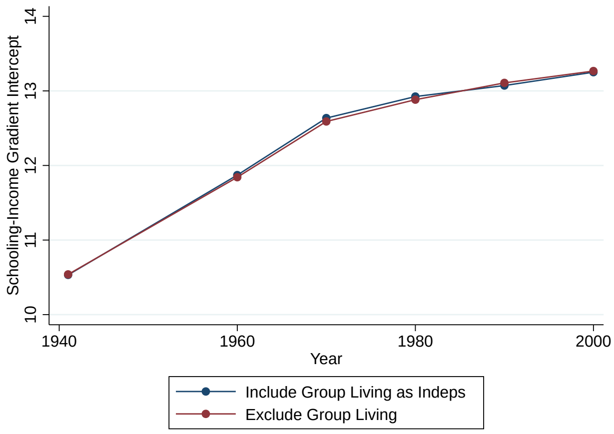
(a) Intercepts



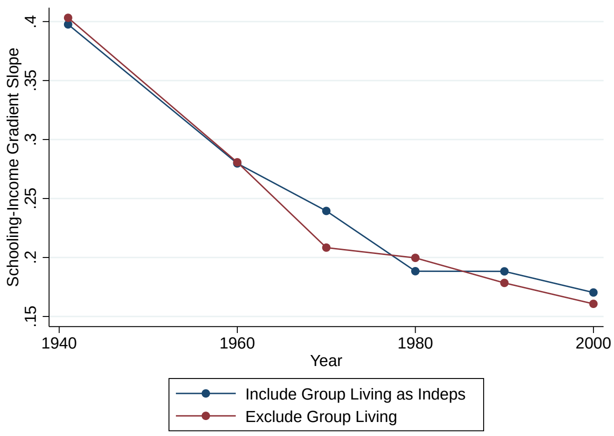
(b) Slopes

Figure A.8: Intercepts and Slopes of Schooling Gradients in Parental Education by Sample and Year

Notes: Figure documents that education gradients in parental education are not sensitive to different ways of classifying children as independent, by year, for whites and blacks. Presents estimated intercepts and slopes from linear regressions of children's highest grade attained on parent's highest grade attained, using data grouped at the year by race by parental education level. Sample weights are used to construct cell means, and regressions on collapsed data are weighted by the square of cell size.



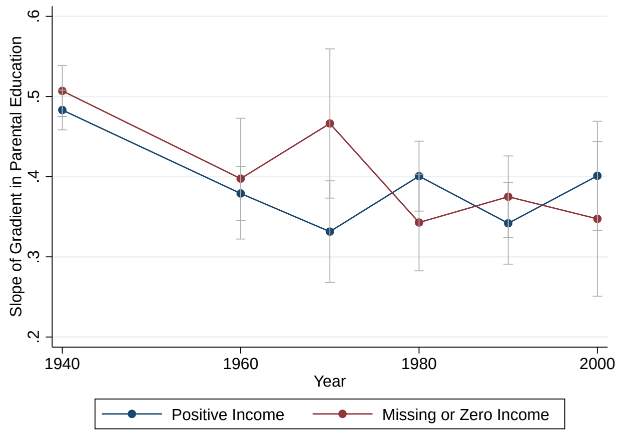
(a) Intercepts = Absolute Upward Mobility



(b) Slopes = Relative Mobility

Figure A.9: Intercepts and Slopes of Linear Schooling Gradients in Parental Income Deciles by Sample and Year

Notes: Figure documents that education gradients in parental income are not sensitive to different ways of classifying children as independent, by year, for whites and blacks. Presents estimated intercepts and slopes from linear regressions of children's highest grade attained on parental income decile, using data grouped at the year by race by parental income decile level. Sample weights are used to construct cell means, and regressions on collapsed data are weighted by the square of cell size.



(a) Whites

(b) Blacks

Figure A.10: Slopes of Schooling Gradients in Parental Education by Missing Income Status and Year

Notes: Figure documents that education elasticities are similar in families with positive household earnings and families with missing/zero household earnings, by year, for whites and blacks. Presents estimated slopes from linear regressions of children's highest grade attained on parent's highest grade attained, using data grouped at the year by race by parental education level. Sample weights are used to construct cell means, and regressions on collapsed data are weighted by the square of cell size.

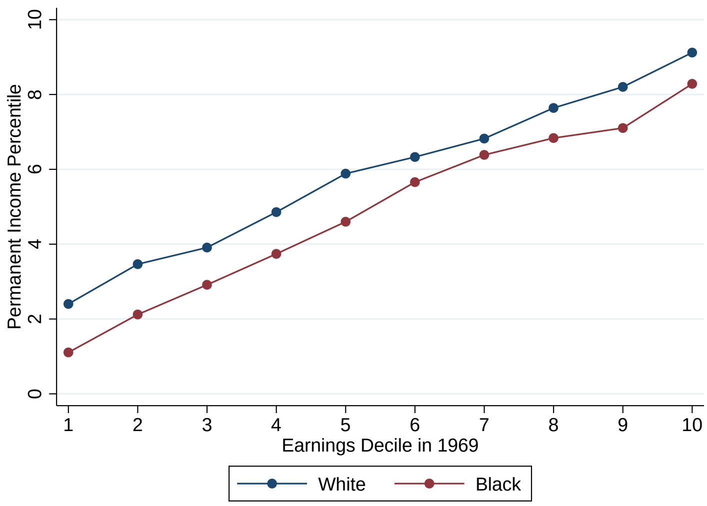


Figure A.11: Average Permanent Income Percentile by Annual Earnings Percentile in 1969

Notes: Sample includes household heads, ages 25-65. Income includes labor, business, transfer, interest, dividents, and other sources of total family income. Permanent income calculated by averaging annual income in all available years for each individual household head, then taking the log of this average. Annual earnings deciles constructed using 1970 survey sample weights. Zeros excluded from annual earnings deciles, but included in construction of permanent income.

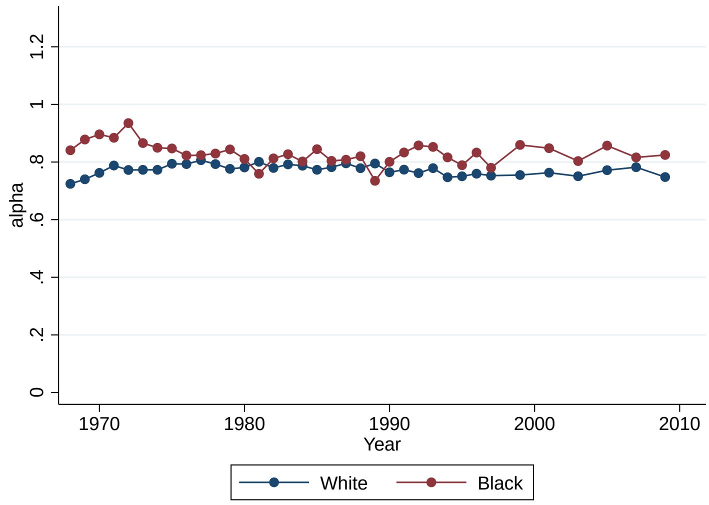


Figure A.12: Estimated α_g by Year

Notes: The term α_g represents the coefficient from a regression of annual total family earnings percentile on permanent total family income percentile, run separately on each year in the PSID using each year's PSID probability weights. Sample includes families with heads between ages 25-65. Income includes labor, business, transfer, interest, dividents, and other sources of total family income. Permanent income calculated by averaging annual income in all available years for each individual household head, then taking the log of this average. Annual earnings deciles constructed using each year's sample weights. Zeros excluded from annual earnings percentiles. Zeros included in construction of permanent income from annual incomes, and in construction of permanent income percentiles.