The Great Escape: Intergenerational Mobility Since 1940*

[PRELIMINARY AND INCOMPLETE]

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Tax records indicate that intergenerational mobility (IM) has been stable for cohorts entering the labor market since the 1990s. I show that when using educational attainment as a proxy for adult income, stable IM is a new phenomenon: IM rose significantly for cohorts entering the labor market from 1940 to 1980. I measure IM directly in historical Census data for children still living with their parents at ages 22-25, and indirectly for other children using an imputation procedure that I validate in multiple data sets with parent-child links spanning the full 1940-2000 period. Post-war mobility gains were much larger in the South and for blacks, and were driven by gains in high school rather than college enrollment. Controlling for region and year, states with higher IM have had lower income inequality, higher income levels, more educational inputs, higher minimum dropout ages, and lower teen birth rates. IM gains plausibly increased aggregate annual earnings growth by 0.125-0.25 percentage points over the 1940-1980 period.

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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. A number of 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). While new administrative data sets are improving IM estimation in more recent periods, they do not shed light on long-term historical trends and they lack information on race and parental education (Chetty et al., 2014a). The lack of reliable, longer-term trends is unfortunate because the high school movement, early GI Bills, Great Society programs, several key Supreme Court decisions, and the Civil Rights movement all predate availability of leading panel data sets.

In this paper I develop a new method to estimate IM statistics on the U.S. census. 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 meaningfully observed (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 schooling as of ages 22-25 with respect to parental income or education.[1] Figure 1 illustrates these CEFs or "schooling gradients." 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.[2] 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 children have either "high-income" or "low-income" parents. Among 22-year-olds, I

[1] I use the terms "schooling" and "education" interchangeably in the text.

[2] The terminology of "upward absolute mobility" is more forced in my application because I am working with child outcome levels rather than outcome ranks, but nonetheless has some intuitive appeal.

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 22-year-old children have high-income parents and 200 have low-income parents. Let h equal average schooling of low-income 22-year-old independent children. We can now solve for h: $12 = \frac{100}{300}(h+2) + \frac{200}{300}h \implies h = 11.67$. Total schooling of low-income children is therefore $\frac{100}{300} \cdot 12 + \frac{200}{300} \cdot 11.67 = 11.78$ and total schooling of high-income children is $\frac{100}{200} \cdot 14 + \frac{100}{200} \cdot 13.67 = 13.835$. The *total* schooling gradient therefore has intercept of 11.78 and slope of 2.055.

Below I formalize and slightly weaken these two assumptions of parallel trends and smooth cohorts, generalize the method to more than two groups, and present strong empirial evidence that both assumptions are valid in the U.S. historical context.[3] I focus on ages 22-25. At these ages almost all children have completed schooling, but only about one third still live with their parents. The "parallel trends" assumption requires that children's final schooling depends on parental group status in some way that is known up to a constant for dependent and independent children. This assumption would be violated if, for example, low-income children do not attend college, while high-income children who attend college move back in with their parents in early adulthoold and high-income children who forego college live independently. I verify the parallel trends assumption directly in multiple data sets that contain parental group status for all children–not just dependents–including a matched panel linking the 1930 and 1940 100% census microdata, the Panel Study of Income Dynamics (PSID), the National Longitudinal Study of Youth 1979 (NLSY), the Occupational Change in a Generation Surveys of 1962 and 1973 (OCG62, OCG73), and the General Social Survey (GSS). These data sets together span the entire sample period of 1940-2000. Remarkably, I fail to reject the parallel trends assumption in every data set and every subgroup I examine.

[3]In ongoing joint work with Ofer Malamud and Christian Pop-Eleches, we find that the assumptions also hold up internationally across a wide range of countries.

I can also directly validate the smooth cohorts assumption, which is milder and less surprising than the parallel trends assumption. The smooth cohorts assumption would be violated if, for example, the share of children born to high-income parents in the U.S. changed in large, sharp ways over time.[4] I verify the smooth cohorts assumption in census data directly using cohort trends for children during ages of near-100% dependent status. As the toy example above illustrates, the parallel trends and smooth cohorts assumptions together allow me to allocate the "de-linked" pool of age 22-25 independent children across parental groups, impute their final schooling, and average them with dependent children–thereby recovering the total CEF of final schooling with respect to parental group status.

The method opens up a wide range of new possibilities for research on IM because cross-sectional data sets are both larger and more common over time and space than panel data sets. As a first application of the method, I examine long-term trends in IM in the U.S. I find that IM increased dramatically after 1940, may have increased modestly 1960-1980, and stabilized thereafter. The level and stability of IM after 1980 line up with recent findings in the PSID (Lee and Solon, 2009) and tax records (Chetty et al., 2014b). However, the major increase after 1940 indicates that recent levels of IM are not a "deep" fact about the US economy, and that IM increased over the same period that inequality declined (Goldin and Margo, 1992). I also replicate recent findings of lower IM in the South, but place this finding in historical context of long-term regional convergence over many decades. I also show that IM gains were only slightly larger for men and much larger for blacks.

Post-war gains in IM were economically large. Using the OCG surveys, I show that IM gains in terms of children's education likely caused IM gains in terms of children's income; cross-sectional returns to education do not vary significantly by parental education levels, and 75% of the intergenerational income elasticities can be accounted for by intergenerational education elasticities at observed returns to schooling. Back-of-the-envelope calculations suggest that the relative educational gains of poor children implied by the increase in IM raised aggregate annual earnings growth over the 1940-1980 period by 0.125-0.25 percentage points.[5]

I also shed some light on causes of these mobility gains. I show that increases in relative IM were driven by high school attandance rather than college attendance. Along with the facts above that gains were similar by gender and concentrated in years before

[4] I focus on native-born children to eliminate concerns about changes in selective immigration.

[5] This improvement in the education of poor children is reminiscent of the gains in allocative efficiency among women and blacks after 1960 (Hsieh, Hurst, Jones and Klenow 2013).

1960, the results argue against simple explanations based on the GI Bill, the War on Poverty or the Civil Rights Acts. Conditional on region, I show that IM by state-of-birth has historically been higher for children born in states with higher income levels, lower income inequality, greater educational inputs, higher minimum dropout ages, and lower teen birth rates. Inequality and dropout ages appear particularly important, suggesting some but not all of the educational gains may have been compelled.

Prior literature has found mixed results on post-war trends in IM in the U.S. Hertz (2007) and Lee and Solon (2009) document stable mobility in child earnings since the 1970s, while Chetty et al. (2014b) confirm stable mobility in child income since 2000.[6] I replicate these findings for recent decades, but place them in broader historical context. Aaronson and Mazumder (2008) develop a different method to estimate IM statistics on census data back to 1950 by instrumenting for parental income with cohort and state of birth. They find mixed results on IM trends before 1980 and declining IM after 1980. Olivetti and Paserman (2014) and Clark (2014) estimate trends in IM before 1940 using information about SES contained in children's first and last names, respectively. As these authors carefully point out, their approaches depend on strong and unverifiable assumption that instruments (state of birth, last name, first name) only affect child outcomes through parental characteristics, or that violations of this assumption do not bias trends over time.[7] The approach outlined here relies on weaker and more verifiable assumptions, at the cost of focusing on children's final schooling rather than earnings or income. The method complements Olivetti and Paserman (2014) in that names data currently end in 1940, while income and education data begin in 1940. Finally, Long and Ferrie (2013) have compared long-term trends in intergenerational occupational persistence using both census and OCG data. As they point out (Long and Ferrie, 2013, footnote 14), occupational categories–unlike income and educational attainment–cannot be ranked in a clear cardinal fashion over long periods of time. Moreover, reliance on OCG data precludes analysis of many subgroups due to sample size limitations.

The paper proceeds as follows. Section 2 outlines the problem of independent children that has prevented prior researchers from estimating IM on census data, develops a methodology for overcoming this problem, and validates the two assumptions underlying this method. Section 3 discusses the data sets I use in the analysis. Section 4 presents a simple model of parental income, children's schooling, and borrowing constraints, and uses this model to show how measures of IM based on children's schooling relate to each

[6] Chetty et al. (2014b) estimate actual and predicted IM for children born 1970-1990, using child outcomes observed in years 1999-2012.

[7] 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 (2014).

other and to more traditional measures of IM based on children's income. Section 5 presents the main results, and explores potential explanations of new findings. Section 6 presents evidence from the OCG that higher educational mobility likely implies higher income mobility. Section 7 conducts several robustness checks on the main results. Section 8 tests the empirical predictions of the stylized model in more detail. Section 7 concludes.

2 A Solution to the Problem of Independent Children

2.1 The Problem of Independent Children

One useful class of IM statistics I refer to as "intergenerational *educational* mobility" (IEM) statistics relies on estimation of the CEF of children's final schooling with respect to parental characteritics; below I show that IEM statistics relate closely to IM statistics in a simple economic model. Unfortunately, in many cross-sectional data sets, researchers only observe parental characterstics for the subset of children who still live with their parents. This is problematic because many children leave the parental home before they finish schooling, and the decision to move out may relate to schooling decisions in complex ways. This "problem of independent children" makes it hard to interpret IEM statistics calculated directly in most cross-sectional data sets (Cameron and Heckman, 1993).

Figure 2 displays this problem in census data in 1980 by plotting the fraction of children still living with their parents by age, alongside average schooling by age. The figure shows that children virtually all live with their parents up through age 16, but then begin to move out rapidly. Moreover, average schooling rises diffentially across races long after many children have left the parental household. By the ages at which average schooling stabilizes, nearly two-thirds of children have left the parental home, leaving researchers with a highly selected subsample of dependent children on which to calculate IEM statistics directly in census data.

2.2 Solution to the Problem of Independent Children

I correct for the problem of independent children as follows. 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, $h_{a,y}$.

To proceed, I make and validate two simple 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. 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:

$$f\left(h^D_y, h^I_y\right) = \rho \tag{2}$$

where $f(.)$ can be any known function. I refer to this as "parallel trends" assumption because in practice I assume the function $f\left(h^D_y, h^I_y\right) = h^D_y - h^I_y$. 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. One simple way in which this assumption could be valid is if dependent status after age 22 is exogenous to children's final schooling decisions conditional on parental group status. In fact, this stronger assumption turns out to be a reasonable approximation for whites in most years. However, the weaker assumption allows for a limited type of endogeneity and fits the data better more generally. The economic underpinnings of this assumption depend on complex, unobserved relationships between schooling, dependency, and parental group status. However, the assumption captures a simple and plausible intuition that parental income and education may correlate with children's schooling in ways that are not strongly related to residency choices in young adulthood, especially at ages after 22 when relatively few children are still attending college. Below I provide strong empirical support for this assumption.

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 $N_{a,y} = N_{a,y}^D + N_{a,y}^I$. The assumption is 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 $g\left(.\right)$ 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 Figure 2 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 17. 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 $g\left(.\right)$ 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 $\hat{h}_{a,y}^I = h_{a,y}^D - \rho$. I then estimate final schooling gradients using equation (1).

A final problem with the estimator for ρ in equation (4) is that $\sum_{j=1}^{K} \frac{\hat{N}_{a,j}^I}{N_a^I}$ will not generally equal one due to measurement error in the $\hat{N}_{a,j}^I$ 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 ($\hat{N}_a^I \equiv \sum_{j=1}^{K} \hat{N}_{a,j}^I$) for observed total independents at age a (N_a^I) in equation (4). This assures that $\sum_{j=1}^{K} \frac{\hat{N}_{a,j}^I}{\hat{N}_a^I} = 1$ and implies that $\hat{\rho}$ will be unbiased even if population growth changes parental group sizes across cohorts proportionally.

2.3 Validation of the Parallel Trends Assumption

Figure 3 presents non-parametric visual evidence on the validity of the parallel trends assumption in two leading panel data sets, the PSID and the NLSY79, both both parental income and parental education. The assumption appears approximately true. In addition to being parallel, the curves are not far apart from each other. This implies that errors in allocation of independent children to parental income groups due to violations of the smooth cohorts assumption are unlikely to significantly alter the final schooling gradients.

Figure 3 suggests that schooling gradients are approximately linear in parental income deciles, and in parental schooling above 10th grade. I therefore test the parallel trends assumption more formally using regressions of the following form:

$$h_{i,y}^{j} = \beta_0 + \beta_1 \cdot y + \beta_2 \cdot 1\{j = D\} + \beta_3 \cdot y \cdot 1\{j = D\} + e_{i,y}^{j} \tag{5}$$

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 1 presents estimates from this regression in parental income deciles. Columns (1)-(9) are based on the PSID. Column (1) finds no evidence to reject the assumption, pooling all years. Note that the overall gradient is large and highly significant, while the interaction term is small and insignificant. Columns (2)-(5) find no evidence to reject the assumption in any decade from 1970-2010. Columns (6)-(9) indicate the assumption holds for boys and girls, and for whites and non-whites. Column (10) finds no evidence to reject the assumption on the NLSY79, once again pooling all available years. I am unaware of any other data set in the U.S. with reliable information on parental income during adolescence and children's dependency status in young adulthood.[8]

Table 2 presents analogous evidence for schooling trends in parental education rather than income. Note that for these estimates to correspond to Figure 3, it is ncessary to reweight the data to give equal weight to each parental education category. I take a simple approach and simply collapse the data (using sample weights) to the level of dependent status by parental education group prior to estimating equation (5). I also restrict to parental education of at least 10 years in order to focus on the linear region of the curve in keeping with this specification. Columns (1)-(10) correspond to the analogous columns in Table 1. Columns (11)-(13) present additional evidence from the GSS, OCG62, and OCG73, all of which line up with results from the PSID and NLSY. The parallel trends assumption for both parental income and education therefore appears valid over the 1970-2010 period, with no evidence that this validity has varied substantially over time or demographic groups.

In order to assess parallel trends before the 1960s, I create a matched panel by linking

[8] The Wisconsin Longitudinal Study does not track children's dependent status (personal correspondence with survey administrators). The GSS contains qualitative, retrospective parental income categories reported by children, and the OCG surveys contain quantitive, retrospective parental income categories reported by children. Parallel trends also hold in these data sets, but I omit them because they have no quantitative interpretation. Results available from author upon request.

children ages 10-17 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; parental income and education are not available in 1930. I also restrict to boys due to changes in last names names of girls after marriage.[9]

Figure 4 plots children's final schooling at ages 22-25 by parental home value and rent deciles, and for both whites and blacks. For whites, dependent and independent children at ages 22-25 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.[10] These results line up well with the results for later decades. Therefore, there is no evidence to reject the parallel trends assumption at any point in time since WWII, or for any subgroup for which sufficient data are available to implement a test.

2.3.1 Intuition for Parallel Trends

Some of the findings above support an even stronger assumption than parallel trends: overlapping trends. This would arise, for example, if determinants of dependent status at ages 22-25 were exogenous to final schooling conditional on parental group status. It is therefore interesting to note that the primary determinant of dependent status is marital status. Appendix Table A.1 displays the share of children age 22-25 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 children tend to leave the parental home when they find a spouse. It seems plausible that the exact age at which 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

[9] This exercise takes advantage of new 100% digitized samples of both 1930 and 1940 censuses. Following a stricter version of IPUMS practice, I link children based on five variables: birth cohort, state of birth, sex, race, first name and last name. I require exact, unique matches. Out of $7,284,262$ children in the 1940 census, I match $1,483,889$ or 20%, and about 70% of these matches are unique for a final match rate of about 14%. The resulting panel contains over 4 million children aged 20-27 with outcomes observed in 1940 matched to their age 10-17 parental characteristics in 1930.

[10] One might wonder why schooling declines 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.

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

$$\frac{d\,(h_D - h_I)}{dg} = 0, \tag{6}$$

which can be shown to imply that

$$h'_H - h'_L = -\frac{(h_H - h_L)^2}{\rho}\left(p'_D - p'_I\right) \tag{7}$$

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 differences between groups, even if composition of types varies differentially for dependent and independent children across parental groups.

2.4 Validation of the Smooth Cohorts Assumption

I exploit the smooth assumption to predict total parental group cohort sizes–including both dependent and independent children–at ages 22-25 using cohort sizes prior to age 17, when virtually all children live with parents and can therefore be linked to parental groups. This prediction requires selection of an estimator.[11]

I employ a simple method to select an estimator and evaluate its accuracy. The approach I take is to evaluate potential estimators of total group cohort sizes ten years earlier at ages 12-15 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 22-25, 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 12-15 and the "prediction" ages 22-25.

The approach is easy to understand visually. Figure 5 plots the number of children living with parents in different income deciles by age in 1940. The figure suggests that we could predict cohort size at ages 12-15 quite well using cohort sizes at ages prior to 8. This suggests that in 1950, we can predict cohort sizes at ages 22-25 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 22-25.

Tables 3 and 4 present results of this exercise more formally for parental income and education groups, respectively. Each column displays results from a regression of group cohort size share at ages 12-15 on some estimator based on group cohort size shares before age 8.[12] Columns 1-3 experiment with different estimators, pooling all years 1940-2000. The simplest estimator based on cohort size at age 7 performs reasonably well relative

[11]It might seem that I could observe parental group cohort sizes almost perfectly in the prior census. 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. Second, parental group status may change in systematic ways over ten-year intervals. For example, parents of 12-15 year-olds in the bottom income decile in 1960 may not systematically be in the bottom income decile as parents of 22-25 year-olds in 1970. This consideration is less important for parental education, but still may exist due to variation in survey methodology or recall bias Neal (2006), and it is preferable 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 12-15 year-old children die before turning 22-25 during this period, and because restrictions to native-born eliminate most international migration for whites and blacks during this period.

[12]Recall that gradient estimation only depends on group cohort shares, not group cohort levels.

to more complex estimators. I 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.[13] 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 no particularly alarming pattern over time. The predictions for the parental education groups are somewhat more accurate than for parental income groups, though both are excellent. Appendix Tables A.2 and A.3 display similar patterns for black children.

The results in this section support the smooth cohorts assumption. Group cohort sizes evolve in predictable ways and thereby permit fairly good estimates of parental group composition among independents.

3 Data

The decennial census is the only large-scale, nationally representative source of data on income and education before the 1960s in the U.S.[14] I rely on census data from 1940-2000, when income and education are both available. I also make full use of the recently-available 100% digitized sample of the 1940 census.[15]

I also incorporate data from the Panel Study of Income Dynamics (PSID), the National Longitudinal Survey of Youth 1979 (NLSY), the Occupational Change in a Generation 1962 and 1973 surveys (OCG62 and OCG73), and the General Social Survey (GSS) to assess some of the assumptions underlying the empirical strategy. These PSID and NLSY79 are panel data sets that track children after they split into new households, and for my purposes cover children back to 1968 and 1994, respectively. The OCG surveys are one-time cross-sectional data sets that collect information on adults and their parental characteristics during adolescence. The GSS is an annual cross-sectional survey that collects information on parental income and education, and begins in 1972 for the US.

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

[13] Similar patterns by year hold for all of the estimators.

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

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

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 22-25, and very few 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 primary results I count all of these children as independents; in the Robustness section I show results are similar if I omit them from the analysis.

A final issue is how to count zeros in reported income and earnings in census data. I choose to exclude zeros from all my baseline analysis because they likely represent a combination of genuine zeros and measurement error, and the exact mix may vary across demographic groups and years. Including zeros as a separate group does not significantly alter the main results.

4 Economic Interpretation of Gradients

How should we interpret measures of mobility based on children's schooling rather than children's earnings? And how do mobility measures based on parental income and parental schooling relate to each other? In this section I present a simple model of borrowing constraints and schooling developed in Solon (2004) to address these questions, modified to allow for parent-child income transmission through both human capital and other factors.

Let a parent with one child maximize a Cobb-Douglas utility function

$$U_i = (1-\alpha)\ln C_{i,t-1} + \alpha \ln y_{i,t} \tag{8}$$

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

$$(1-\tau)\cdot y_{i,t-1} = C_{i,t-1} + I_{i,t-1} \tag{9}$$

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

$$h_{i,t} = \delta + \theta \ln(I_{i,t-1} + G_{i,t-1}) + e_{i,t} \tag{10}$$

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

$$\frac{G_{i,t-1}}{y_{i,t-1}} \approx \varphi - \gamma \ln\left(y_{i,t-1}\right), \tag{11}$$

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:

$$\ln y_{i,t} = \mu + ph_{i,t} + \varepsilon_{i,t} \tag{12}$$

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.[16]

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

$$e_{i,t} = \lambda e_{i,t-1} + \nu_{i,t} \tag{13}$$

$$\varepsilon_{i,t} = \lambda \varepsilon_{i,t-1} + u_{i,t} \tag{14}$$

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.

Letting $\beta_{x,x'}$ denote the OLS coefficient from a regression of x on x', it can be shown that in steady state

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

This result suggests that intergenerational relative mobility in terms of education should

[16]Note parents cannot increase child income directly with bequests in this model; human capital is the only instrument for transfers. To add savings, let $\ln\left(y_{i,t} - S_{i,t}\right) = \mu + ph$ and augment the budget constraint to $y_{i,t} = C_{i,t} + I_{i,t} + S_{i,t}$. In this extended model, for incomes above a critical value savings are 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.

be similar in magnitude to intergenerational relative mobility in terms of income. This key finding is consistent with evidence I present below comparing my estimates of $\beta_{h_t,h_{t-1}}$ with estimates of $\beta_{\ln y_t,\ln y_{t-1}}$ from other sources.

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

$$\beta_{h_t,\ln y_{t-1}} = -\left(\frac{1}{p}\lambda\sigma_\varepsilon^2\right) + \frac{1}{p}\cdot\beta_{h_t,h_{t-1}}. \tag{15}$$

This result clarifies why the relationship between the two types of gradient might vary over time and groups. For example, if the conditional variance of income rises, then $\beta_{h_t,y_{t-1}}$ will fall relative to $\beta_{h_t,h_{t-1}}$. Second, the result suggests that the relationship between the two gradients contains information about other parameters in the model. This relationship is more complicated than it appears because some of these parameters also enter into $\beta_{h_t,\ln y_{t-1}}$ and $\beta_{h_t,h_{t-1}}$. Below I explore the plausibility of the estimates implied by this relationship under some additional assumptions. Finally, the model also suggests a number of comparative statics for the education gradients: $\beta_{h_t,h_{t-1}}\left(\theta\uparrow,\gamma\downarrow,\lambda\uparrow,p\uparrow\right)$, $\beta_{h_t,\ln y_{t-1}}\left(\theta\uparrow,\gamma\downarrow,\lambda\leftrightarrow,p\leftrightarrow,\sigma_\varepsilon^2\downarrow\right)$. In theory, these comparative statics can shed light on potential causes of mobility variation over time and groups.

5 Results

Figure (6) 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 due to stability of dependence rates across parental groups at ages 22-25. It also affects blacks more than whites due to the larger share of young adult blacks living independently. Note that adjusted gradients remain precise for blacks despite having only one tenth the population of whites. In a separate paper, I show the correction also yields precise estimates for Asian Americans, the prototypical "model minority" (**?**), and a group that is only 1% as large as whites and therefore, to my knowledge, impossible to study intergenerationally outside the census.

Figure (6) previews two other patterns of interest. First, relative mobility is strongly correlated with absolute upward mobility because the gradients "pivot" at high levels of parental income and education. In other words, poor children vary much more across groups than rich children. Second, the gradients are approximately linear in parental income rank. These patterns echo recent findings on intergenerational mobility in administrative data in **?**, but 60 years earlier in time.

I now examine final schooling gradients non-parametrically by year. Figure 7 plots corrected schooling gradients in parental education for whites.[17]The gradients display a clear increase in both absolute and relative mobility over time. Figure 8 plots analogous gradients in three functions of parental income: deciles, levels and logs. The figure displays a striking increase in schooling levels among the poorest children from 1940-1980, with little change thereafter. Schooling is concave in the level of parental income, slightly convex or S-shaped in log of parental income, and linear up through the 90th percentile in rank of parental income. These curvature patterns are similar to patterns for children's income gradients in the 2000s found by **?** in administrative data, and are roughly consistent with the model except for the rejection of gradient linearity in the log of parental income. The rank specification is well-suited to furnish reliable measures of mobility because it does not depend on changes in the parental income distribution and because it happens to be approximately linear.

Having established that gradients are approximately linear in parental education levels and income deciles, I now report estimated intercepts and slopes of schooling gradients. Tables 5-8 display estimates of intercepts and slopes for schooling gradients in parental education levels and income deciles, for whites and blacks separately, i.e., the estimates displayed in Figures 7 and 8.a. Each column represents estimates from a regression of the form

$$h_{y,t} = \sum_{t=1940,1960,\ldots,2000} \alpha_t \cdot 1\left\{\text{year} = t\right\} + \sum_{t=1940,1960,\ldots,2000} \beta_t \cdot 1\left\{\text{year} = t\right\} \cdot y, \qquad (16)$$

where $h_{y,t}$ represents a child outcome measure, t indexes census year, and y indexes parental group (either education or income decile) normalized such that the lowest-SES group has $y = 0$. Note that the constant terms are omitted from these regressions. The coefficients α_t and β_t represent absolute upward mobility and relative mobility, respectively, in year t.

Column (1) from these four tables contains estimated intercepts and slopes for the two gradients, and for whites and blacks separately. I display these estimates graphically in Figures 9 and 10. These figures additionally compare the estimates with corresponding statistics from the PSID based on actual parent-child links. The census and PSID estimates line up reasonably well. The estimated slope of the gradient in parental education in 2000 for whites is 0.278 (SE=0.020), which is in the range of the estimated intergen-

[17]Gradients cannot be constructed for the 1950 census because only one individual per family received the census long form with questions about income and education, making it impossible to relate parental income to child outcomes.

erational income elasticity on tax data in years 2011-12 reported in Chetty et al. (2014a, Figure 1.B) of 0.344 (SE=0.000), as predicted by the model above.[18] In addition, the estimate of the intergenerational elasticity for whites in 1940 of 0.398 (SE=.0218) in Table 5 is very close to the estimate obtained for whites in 1940 by Olivetti and Paserman (2014).

The figures replicate recent findings of stable IM since the 1980s (Lee and Solon, 2009; **?**), but indicate this period of stability followed a dramatic increase in IM after WWII.[19] The long-term trend emerges in both types of gradients, in both absolute and relative IM measures, and in virtually all demographic groups. However, the increase was larger for some groups than others. The gain was much larger for blacks than whites. Both absolute and relative mobility were much lower for blacks than whites in 1940, with approximate convergence achieved in both measures by 1980. The increase in absolute mobility is especially remarkable: the lowest-income black children nearly *doubled* their final schooling from 6 to 12 years between 1940 and 2000, about twice the gain made by the lowest-income white children, which is large in its own right.

Columns (2)-(3) of Tables 5-8 display results for boys and girls separately. The increase in absolute and relative IM affected boys somewhat more than girls. Columns (4)-(5) break out results regionally into the South and Non-South, where "South" indicates states formerly composing the Confederacy. These results replicate findings that the South exhibits lower IM than other parts of the country in the most recent period (**?**). However, the longer time-frame allowed by census data reveals that this IM gap is very small in historical context, and follows in the wake of dramatic regional IM convergence from radically different initial conditions. Unsurprisingly, the mobility gains in the South were particularly large for blacks, though they were also substantial for whites. Figure 11 displays the estimated slopes of both gradients for whites only in the South and Non-South and vividly conveys the long-term convergence pattern.

These increases in IM since WWII have not previously been documented, and they 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 1980 remained at the 1940 level, so that schooling at the top decile in 1980 were held constant at its

[18]The difference could arise due to a violation of the model equating the two statistics, or to measurement error in education data in the census, or to instability of the intergenerational elasticity due to underlying nonlinearities in the CEF. In contrast to the gradient in parental education, the gradient in parental *income* that I estimate here has a much higher intercept and lower slope than that reported in Chetty et al. (2014a) based on five-year averages of parental income, as would be expected from substantial measurement error in census income data.

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

observed value but schooling of all lower deciles were decreased to reflect the steeper slope from 1940. This would reduce average schooling in 1980 by about one year. One year of schooling during this period increased earnings by around 5-10%. This would account for about 0.125-0.25 percentage points of economic growth over the 1940-1980 period. This transitory growth effect is conceptually related to that obtained from improving occupational opportunities for women and minorities, as in Hsieh et al. (2013).

5.1 What Caused the Increase in Intergenerational Mobility?

What accounts for the increase in educational attainment of poor children after 1940? The largest changes took place between 1940-1960, before the Great Society programs and the Civil Rights movement. Given that the poorest children had zero years of high school education on average in 1940, high schools rather than colleges most likely account for this increase in schooling among the poor. In order to explore the different contributions of high school and college I examine school enrollment rates separately for high school ages 16-18, and college ages 19-21.[20]

Columns (6)-(7) of Tables 5-8 display the results. For whites, both absolute and relative mobility in high school enrollment increased substantially. For college enrollment, however, only absolute mobility increased, while relative mobility actually fell significantly. After sixty years of policy initiatives designed to increase college affordability including the GI Bills, the community college movement and large expansions of federal financial aid, poor children have made substantial gains in college access, but have made no gains at all *relative* to rich children. For blacks the story is similar, though estimates are less precise. These estimates place recent work on college access into longer-term historical perspective (Bailey and Dynarski, 2011; Belley and Lochner, 2007).

The fact that mobility in the South started at a lower level and increased by more over time suggests an important role for geographic variables, as emphasized in Chetty et al. (2014a). To explore this I calculate mobility statistics separately by state and year, again focusing on four types of statistics: intercepts and slopes of gradients in both income and education. Inspection revealed that the magnitude of the slopes increased steadily in the R-squared of the underlying gradients, suggesting an important role for specification or measurement error in these statistics. I therefore restrict to statistics based on gradients with an R-squared of at least 0.5.

[20]Note that for enrollment gradients in parental education, I exclude parental education levels below 9, whereas I exclude parental education levels below 6.5 in other columns. The reason for this is that enrollment gradients are nonlinear; they are largely flat or downward-sloping below parental grade 9, but linearly increasing after grade 9. The main qualitative lessons here hold for the full gradients but cannot be captured by linear regressions.

Table (9) presents correlations of mobility statistics with various state-level, time-varying characteristics for whites. I first regress mobility statistics and all characteristics on a complete set of region by year interaction dummies, using the four census regions. FollowingChetty et al. (2014a), I then convert all residuals into z-scores to obtain correlations and standard errors by regressing these residual z-scores of mobility statistics on residual z-scores of state characteristics. This approach makes it easier to compare the role of many diverse characteristics on mobility. Each column contains correlations of the named state characteristic with four different measures of mobility that correspond to absolute and relative mobility using parental education and income groups. Column (1) uses the average of log income in state by year cells as a measure of development. Column (2) uses the interquartile gap in log incomes within state by year cells. Columns (3)-(5) use minimum school dropout age, average class size, and relative teacher pay. Columns (6) and (7) use the teen birth rate and share black, respectively. All significant coefficients go in the directions one would expect based on prior research. Absolute and relative mobility both increase in state income levels, lower inequality, higher school dropout ages, smaller class sizes, higher teacher pay, and lower teen birth rates.[21]

Given the significant correlations of so many spatial variables with multiple measures of mobility, we might wonder if they all capture the same latent variable or if they contain separate information. I therefore regress each of the four mobility statistics used in the bivariate correlations on all of the spatial variables at once to see whether any variables dominate. Table 10 displays the results. For mobility statistics based on parental education, only parental income inequality significantly predict higher absolute and higher relative mobility. For mobility statistics based on parental income, both dropout ages and class size predict absolute upward mobility, while income levels, income inequality, dropout ages and class size all predict relative mobility. The results therefore suggest that compulsory schooling is not the dominant explanation, and that other factors affecting the private costs and benefits of schooling also play important roles.

6 Did Higher Educational Mobility Lead to Higher Income Mobility?

A key finding of the paper is that intergenerational mobility in terms of children's schooling increased significantly after 1940. This raises the quation of how these changes in educational mobility translate into income mobility. I address this question using data

[21] The equality of correlations in row (4), columns (1) and (2) is not exact at higher decimals and appears to be driven by chance.

from the OCG1962 and OCG1973 surveys, which contain parental education levels and larger samples than the PSID or NLSY79.[22] I first ask if education affects income of children from different parental groups in similar ways. If that were the case, it would suggest that IEM likely translates into IM, and that 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

$$\text{logEarnings}_{g,\text{educ}} = \alpha + \beta \cdot \text{educ} + \gamma_g \cdot 1\{\text{fatherEduc} = g\} + \delta_g \cdot 1\{\text{fatherEduc} = g\} \cdot \text{educ} \tag{17}$$

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 quality of education or academic ability.

Table 11 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

$$\text{childOutcome}_{g,\text{c}} = \pi + \phi \cdot \text{fatherEduc} + \eta_c \cdot 1\{\text{cohort} = c\} + \lambda_c \cdot 1\{\text{cohort} = c\} \cdot \text{fatherEduc} \tag{18}$$

[22] An alternative approach is to use the 1930-40 matched census data, which contains parental home value and rent in 1930, and compare children's income mobility in that panel data to income mobility in more recent panel data such as the PSID, which also contains parental home value and rent. The main problem with this approach is that the housing market underwent a transformation from 1940-60 and the homeownership rate increased by 50%. It is therefore difficult to compare mobility among children of homeowners or renters in 1940 with mobility among children of homeowners or renters after 1960 due to this large shift in the composition of both owner and renter samples.

for individuals in the same 10-year cohorts as before, where 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 11 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.

7 Robustness Checks

I have focused on solving the problem of independent children. Another problem with census data are that they only contain information about parental income in a single year. This may be especially problematic when comparing gradients across races with very different permanent incomes (Rothstein and Wozny, 2014). In Appendix A.1 I develop a simple method to correct for permanent income differences across races in the PSID. I conclude that permanent income differences are not likely to be driving my estimated differences in relative mobility across racial groups. And of course the mobility gradients that rely on parental education are not affected by permanent income differences across races.

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 results I count all children living in dormitories, prisons and military barracks at ages 22-25 as independents. Appendix Figures A.1 and A.2 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 schooling-schooling gradient in 1970, which reflects an oddly low level of estimated final schooling among children of high-education parents in that year.

8 Model Validation

Recall that the model presented in Section 4 yielded three implications. The first implication is that the intergenerational education elasticity ($\beta_{h_t,h_{t-1}}$) should equal the intergenerational income elasticity (IGE). This implication is approximately bourne out in the data, though the education elasticity is somewhat lower than the income elasticity. The second implication is that the CEF of children's human capital with respect to parents' human capital should be linear. Above in Figure 8.b I presented evidence that this CEF is in fact convex in every year, not linear.

The third implication is that the two types of schooling gradients I estimate, with respect to parental income and parental schooling, are related to each other in steady state by Equation (15), reproduced here for convenience:

$$\beta_{h_t,\ln y_{t-1}} = -\left(\frac{1}{p}\lambda\sigma_\varepsilon^2\right) + \frac{1}{p} \cdot \beta_{h_t,h_{t-1}}. \tag{19}$$

One way to exploit this result is to regress estimates of $\beta_{h_t,\ln y_{t-1}}$ on estimates $\beta_{h_t,h_{t-1}}$ in a sample of groups that are in different steady states. Unfortunately, this regression only identifies the intercept and slope in Equation (15) under the strong exclusion restriction that steady state variation stems only from θ or γ, and not at all from variation in p, λ , or σ_ε^2.

The situation improves slightly if I assume fixed, reasonable values for the parameters p and σ_ε^2. Now λ is allowed to vary across groups along with θ and γ. This assumption implies empirical distributions for λ and $\theta(1-\gamma)$ based on variation in gradients across groups. I define groups by state of birth and race, controlling for year, and I explore values of $p \in [0.05, 0.25]$. Using census data, I find that σ_ε^2, the variance of log family earnings conditional on schooling, falls in the range of 0.4-0.6 for all schooling groups over the 1940-2000 period. This is an upper bound on σ_ε^2, which refers to variance in lifetime income, because lifetime income variation tends to be significantly smaller than annual income variation (Aguiar and Bils, 2011). I therefore explore values of $\sigma_\varepsilon^2 \in [0.2, 0.5]$.

I find that these assumptions yield plausible distributions for λ in the range of $[0.2, 1]$. I also find that implied distributions for $\theta(1-\gamma)$ are largely negative, which implies that either financial investments in human capital are counterproductive ($\theta < 0$), or schooling subsidies have an elasticity less than -1 with respect to parental income ($\gamma > 1$). Neither of these implications are plausible for the period 1940-2000. Therefore the new facts generated here reject the steady state implications of the model. Either the steady state assumption is too strong, which greatly weakens the empirical predictions of the model, or the many strong underlying assumptions are wrong.

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 final schooling gradients in these parental characteristics and show that some of these gradients are linear. I exploit this linearity to estimate intercepts and slopes as robust measures of absolute and relative IM, respectively, that are comparable over time, places and groups. I use a simple economic model to relate some of these implied mobility statistics to each other and to more traditional measures of IM based on children's earnings and income.

I use this new methodology to document a range of important new historical facts about IM. IM increased dramatically after WWII before stabilizing in the 1960-80 period when panel data sets first become available. This increase in IM was economically large; relative mobility by itself may have increased aggregate annual earnings growth by up to a quarter of a percentage point over the 1940-80 period. The increase in IM was broad-based but slightly larger for men, much larger for blacks, and much larger in the

South. The IM disadvantage of the U.S. South in recent decades turns out to be the tip of a much larger historical iceberg.

Turning to causes, I show that the increase in IM likely stemmed from increased high school enrollment, not college enrollment. I show that relative IM in terms of college enrollment has declined since 1940 despite decades of reforms seeking to equalize college access across socioeconomic groups. I calculate IM statistics separately by state of birth and find that IM positively correlates with higher state income, greater equality, and greater school inputs. Compulsory schooling laws only partly account for the increase in IM, suggesting the increased high school attendance among poor children partly represented voluntary human capital investment. Finally, I show that several predictions of the stylized model of borrowing constraints and parental income receive only partial support in the data. In ongoing and future research, the method developed here can also shed new light on mobility of groups that are too small to examine in panel data (such as Asian-Americans and other "model minorities"), as well as on mobility across a wider range of countries and time periods than has previously been possible.

ONLINE APPENDICES

A.1 Correction for Transitory Income

Each census only contains one year of parental income per family, potentially leading to biased estimates of parental Engel curves in permanent income due to measurement error. This is a larger problem here because measurement error in permanent income may vary systematically across racial groups, leading to differential mismeasurement of μ_g and θ_g and therefore underestimating the importance of income in explaining group schooling differentials. Recent research, for example, has shown that income may explain a much larger share of black-white test score gaps than previously thought (**?**).

To clarify the problem, let annual income of individual i in group g in year t equal permanent income plus noise

$$y_{igt} = y_{ig} + e_{igt}. \tag{20}$$

Let h_{ig} indicate final schooling for child i in group g. For simplicity I here assume the schooling gradient in question is linear, as I find to be the case for schooling gradients in parental income deciles. I would like to estimate the slope and intercept of the Engel curve with respect to permanent income, separately for each group g:

$$h_{ig} = \theta_g + \beta_g y_{ig} + \varepsilon_{ig} \tag{21}$$

Instead, we are forced to estimate Engel curves using annual income or annual earnings:

$$h_{ig} = \theta_g^* + \beta_g^* y_{igt} + \varepsilon_{ig}^* \tag{22}$$

The relationships between the coefficients in the feasible and the best-case regressions are

$$\begin{aligned} \beta_g^* &= (\alpha_g \lambda_g) \cdot \beta_g \\ \theta_g^* &= \theta_g + \left(1 - \alpha_g^2 \lambda_g\right) \mu_g \beta_g \end{aligned} \tag{23}$$

where α_g is the coefficient in a regression of transitory income on permanent income in group g, λ_g is the ratio of permanent to transitory income for group g, and μ_g is average permanent income in group g. The measurement error arises from respondents' recall mistakes, but also from transitory income shocks, lifecycle trends, and non-labor income.

Equation (23) shows that the use of annual rather than permanent income creates two problems. First, if groups differ in permanent income levels μ_g, then lower-income group schooling gradients will be shifted down relative to higher-income group schooling gradients, due to differential mean-reversion. Intuitively, if a black family and a white family are equally poor in a given year, the white family likely has higher income before and after this year than the black family, and therefore higher permanent income and child schooling. Figure A.3 illustrates this problem in the PSID by plotting average percentile in permanent income by annual earnings deciles for black and white families separately in 1969. At all levels of annual earnings, black families have significantly lower permanent income, with a gap ranging from 5-15 percentiles. This gap arises from both differential non-labor income and differential mean-reversion of transitory shocks.

The second problem highlighted by Equation (23) is that if groups differ in the variance share of transitory income λ_g, then schooling gradients in annual income will have slopes differentially attenuated by measurement error. For example, if black workers are more likely to be laid off, this will flatten the black Engel curve relative to the white Engel curve as estimated in cross-sectional data.

I address these problems directly by estimating the terms in Equation (23).[23]A first step is to exploit the focus on parental income percentiles, rather than income levels or logs. This choice mechanically sets $\lambda_g \equiv 1$ if no individuals have identical incomes and incomes are not censored. In practice this is a good approximation in this application. Note that λ_g is less stable over time for income in levels or logs, because income volatility increases over this period as other researchers have documented (e.g., Shin and Solon, 2011). I estimate μ_g in census data for each race in each year as the average percentile of annual parental income, which will equal the average percentile of permanent parental income if transitory and lifetime shocks cancel out within racial groups. The term α_g can be estimated directly in the PSID, but not in the census. If this moment is stable in the PSID, then it suggests extrapolation to earlier census years and to Asians has some credibility. To calculate α_g in the PSID, I regress total family earnings percentile in each year on permanent income percentile, restricting to families with at least 10 years of income in the PSID. Figure A.4 displays α_g for whites and blacks separately in the PSID by year. The moment appears highly stable over time, and roughly similar across races. I therefore estimate this parameter at around 0.8.[24]

Implementation

I implement the correction for these permanent income differences using Equation (23). To give a concrete example, the adjustment in 1940 uses the values $\alpha_g = 0.8$ for all races based on Figure A.4, $\lambda_g = 1$ for all races, and $\mu_{white} = 6.6$ and $\mu_{black} = 4.3$ based on average decile of annual total parental earnings in the census. The only thing changing over the years are these average parental earnings deciles.

This adjustment turns out to make little difference to the results, and I therefore do not report the adjusted results. The results are also similar when I estimate the schooling gradients using parental home mortgage and rent expenditures rather than family income, and when I reduce black annual earnings by plausible amounts to reflect permanent income gaps prior to constructing income deciles. It thus appears that unobserved permanent income differences are unlikely to drive the main patterns documented above.

[23]I experimented with using home values and rents as measures of permanent income. These measures are appealing because they exist in census data for all races. However, home values and rents do not predict permanent income percentile as well as annual income or earnings percentiles in PSID data. In practice none of these choices affect the main results of the paper.

[24]Haider and Solon (2006)show this parameter in individual earnings regressions will not generally equal 1 under realistic lifecycle earnings processes.

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	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dep Var: Child Schooling	All Years	1970-79	1980-89	1990-99	2000-2009	Male	Female	White	Non-White	NLSY 94-10
Parental Educ	0.202**	0.225**	0.176**	0.217**	0.243**	0.215**	0.191**	0.195**	0.181**	0.172**
	(0.00652)	(0.0136)	(0.0100)	(0.0134)	(0.0196)	(0.00984)	(0.00870)	(0.00742)	(0.0177)	(0.0273)
Dependent	0.248**	0.983**	-0.0785	0.501**	0.223	0.160	0.489**	0.278**	0.170	-0.326
	(0.0724)	(0.372)	(0.103)	(0.139)	(0.180)	(0.0938)	(0.115)	(0.0958)	(0.110)	(0.297)
Parental Educ*Dependent	-0.00584	-0.0465	0.0442*	-0.0536*	-0.0363	-0.0218	0.00291	-0.00765	0.00172	-0.00665
	(0.0122)	(0.0595)	(0.0185)	(0.0227)	(0.0299)	(0.0163)	(0.0185)	(0.0147)	(0.0301)	(0.0445)
Constant	11.74**	11.50**	11.77**	11.74**	11.83**	11.61**	11.86**	11.82**	11.60**	13.41**
	(0.0377)	(0.0830)	(0.0573)	(0.0758)	(0.114)	(0.0568)	(0.0503)	(0.0455)	(0.0657)	(0.192)
Observations	18,413	3,194	8,110	4,553	2,176	8,819	9,594	10,473	7,124	1,062
R-squared	0.108	0.127	0.094	0.115	0.149	0.111	0.114	0.098	0.089	0.086

Robust standard errors in parentheses

** p<0.01, * p<0.05

Table 1: Tests for Parallel Schooling Trends in Parental Income

Notes: Table documents that dependent status primarily alters level of children's schooling, not slope with respect to parental group status. Displays estimates of equation (5) by subsample for parental income groups. Columns (1)-(9) use the PSID, column (10) uses children from the NLSY79. Parental characteristics measured when children are age 17. Children's schooling at ages 23-25 is set to missing when lower than six years. Children with zero parental income at age 17 excluded. Income deciles calculated separately by year. Sample weights used in construction of deciles and in regressions.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Dep Var: Child Schooling	All Years	1970-79	1980-89	1990-99	2000-2009	Male	Female	White	Non-White	NLSY 94-10	GSS 72-12	OCG 73	OCG 62
Parental Educ	0.347**	0.363**	0.321**	0.351**	0.359**	0.358**	0.342**	0.332**	0.373**	0.397**	0.392**	0.382**	0.383**
	(0.0293)	(0.0888)	(0.0827)	(0.0323)	(0.0337)	(0.0323)	(0.0395)	(0.0394)	(0.0502)	(0.0521)	(0.0474)	(0.0901)	(0.0619)
Dependent	0.865	1.380	1.033	0.936	0.136	0.835	1.285	0.714	0.333	0.346	0.427	0.0469	-0.0295
	(0.568)	(1.735)	(1.602)	(0.625)	(0.653)	(0.626)	(0.764)	(0.762)	(0.972)	(1.009)	(0.290)	(1.745)	(1.229)
Parental Educ*Dependent	-0.0618	-0.0792	-0.0587	-0.0519	-0.0323	-0.0659	-0.0773	-0.0468	-0.00741	-0.0392	-0.0261	-0.0355	0.0291
	(0.0415)	(0.128)	(0.117)	(0.0456)	(0.0477)	(0.0457)	(0.0558)	(0.0557)	(0.0710)	(0.0737)	(0.0212)	(0.127)	(0.0875)
Constant	8.448**	8.560**	8.680**	8.319**	8.327**	8.194**	8.616**	8.726**	7.843**	8.818**	8.452**	8.692**	8.062**
	(0.402)	(1.216)	(1.133)	(0.442)	(0.462)	(0.443)	(0.540)	(0.539)	(0.688)	(0.713)	(0.649)	(1.234)	(0.869)
Observations	16	15	16	16	16	16	16	16	16	16	16	16	10
R-squared	0.951	0.711	0.683	0.946	0.947	0.944	0.912	0.912	0.902	0.899	0.956	0.745	0.934

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

Table 2: Tests for Parallel Schooling Trends in Parental Education

Notes: Table documents that dependent status primarily alters level of children's schooling, not slope with respect to parental group status. Displays estimates of equation (5) by subsample for parental education groups. Columns (1)-(9) use the PSID, columns (10)-(14) use children from the NLSY79, the General Social Survey, and Occupational Change in a Generation 1973 and 1962 samples, respectively. Note that PSID and NLSY are based on parental self-reported characteristics, while GSS and OCG are based on children's recalled parental education. Parental characteristics measured when children are age 17 in PSID and NLSY, at 16 in GSS and OCG data. Children's schooling at ages 23-25 is set to missing when lower than six years. Parental schooling set to missing if under 10 years. Sample weights used in construction of collapsed data but not in regressions, in order to correspond more closely to Figure 3.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dep Var: Income Group										
Cohort Share Ages 12-15	All Years	All Years	All Years	1940	1950	1960	1970	1980	1990	2000
Age 7	0.938**			0.840**	0.638**	0.785**	0.818**	0.879**	1.236**	1.253**
	(0.0421)			(0.0773)	(0.0382)	(0.0864)	(0.116)	(0.169)	(0.104)	(0.0407)
Ages 4-7, linear		0.915**								
		(0.0250)								
Ages 1-7, linear			0.932**							
			(0.0315)							
Constant	0.00885	0.00985**	0.00865*	0.0163*	0.0370**	0.0231*	0.0220	0.0178	-0.0222	-0.0252**
	(0.00463)	(0.00282)	(0.00351)	(0.00787)	(0.00442)	(0.00953)	(0.0130)	(0.0185)	(0.0114)	(0.00457)
Observations	280	280	280	40	40	40	40	40	40	40
R-squared	0.641	0.828	0.759	0.757	0.880	0.685	0.569	0.416	0.787	0.962

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

Table 3: Validation of Group Cohort Size Predictors: Parental Income Groups

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 12-15 on predicted parental income group cohort shares at ages 12-15 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 12-15	All Years	All Years	All Years	1940	1950	1960	1970	1980	1990	2000
Age 7	0.996**			0.951**	0.756**	0.962**	0.939**	1.046**	1.029**	1.047**
	(0.00595)			(0.0136)	(0.0315)	(0.0133)	(0.00588)	(0.00912)	(0.0195)	(0.0174)
Ages 4-7, linear		0.921**								
		(0.00480)								
Ages 1-7, linear			0.911**							
			(0.00498)							
Constant	0.000593	0.0125**	0.0142**	0.00684*	0.0379**	0.00673	0.00748**	-0.00647*	-0.00584	-0.00903
	(0.00158)	(0.00133)	(0.00139)	(0.00272)	(0.00720)	(0.00342)	(0.00168)	(0.00263)	(0.00459)	(0.00469)
Observations	244	244	244	36	36	36	36	36	32	32
R-squared	0.991	0.993	0.993	0.993	0.944	0.994	0.999	0.997	0.989	0.992

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

Table 4: Validation of Group Cohort Size Predictors: Parental Education Groups

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 12-15 on predicted parental education group cohort shares at ages 12-15 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)
Dependent Var	Highest Grade Attained, Age 22-25					Enrollment	
Sample	All	Boys	Girls	South	Non-South	Age 16-18	Age 19-21
Intercept 1940	9.826**	9.822**	10.08**	8.865**	10.07**	0.605**	0.159**
	(0.0718)	(0.0689)	(0.0944)	(0.0821)	(0.0815)	(0.0263)	(0.0267)
Intercept 1960	10.69**	10.78**	10.81**	10.02**	10.89**	0.625**	0.161**
	(0.0808)	(0.0581)	(0.0856)	(0.0920)	(0.0913)	(0.0185)	(0.0164)
Intercept 1970	11.05**	11.23**	11.11**	10.69**	11.12**	0.770**	0.218**
	(0.0820)	(0.0845)	(0.110)	(0.0883)	(0.0876)	(0.0148)	(0.0152)
Intercept 1980	11.29**	11.33**	11.40**	10.86**	11.42**	0.662**	0.145**
	(0.0946)	(0.0780)	(0.105)	(0.112)	(0.111)	(0.0148)	(0.0138)
Intercept 1990	11.14**	11.16**	11.31**	10.74**	11.30**	0.765**	0.208**
	(0.137)	(0.121)	(0.168)	(0.171)	(0.170)	(0.0189)	(0.0180)
Intercept 2000	11.28**	11.01**	11.59**	10.94**	11.42**	0.794**	0.247**
	(0.146)	(0.148)	(0.188)	(0.179)	(0.178)	(0.0185)	(0.0184)
Slope 1940	0.398**	0.425**	0.356**	0.506**	0.365**	0.0385**	0.0572**
	(0.0218)	(0.0221)	(0.0273)	(0.0248)	(0.0246)	(0.00751)	(0.00752)
Slope 1960	0.293**	0.310**	0.261**	0.376**	0.262**	0.0287**	0.0543**
	(0.0166)	(0.0126)	(0.0196)	(0.0186)	(0.0185)	(0.00484)	(0.00445)
Slope 1970	0.308**	0.311**	0.303**	0.349**	0.300**	0.0173**	0.0543**
	(0.0140)	(0.0147)	(0.0200)	(0.0151)	(0.0150)	(0.00347)	(0.00359)
Slope 1980	0.277**	0.280**	0.266**	0.311**	0.263**	0.0295**	0.0632**
	(0.0146)	(0.0124)	(0.0162)	(0.0171)	(0.0170)	(0.00326)	(0.00305)
Slope 1990	0.290**	0.287**	0.284**	0.317**	0.276**	0.0234**	0.0738**
	(0.0196)	(0.0180)	(0.0241)	(0.0243)	(0.0241)	(0.00393)	(0.00379)
Slope 2000	0.278**	0.284**	0.260**	0.299**	0.269**	0.0204**	0.0648**
	(0.0203)	(0.0201)	(0.0259)	(0.0245)	(0.0244)	(0.00372)	(0.00366)
Observations	58	58	58	58	58	52	52
R-squared	1.000	1.000	1.000	1.000	1.000	0.999	0.998

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

Table 5: Mobility Estimates in Parental Education, Whites

Notes: Displays estimated intercepts and slopes of children's schooling gradients with respect to parental education for whites. Presents estimates of α_t and β_t from Equation (16). Regressions weighted by square root of estimated cell sizes.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Var	Highest Grade Attained, Age 22-25					Enrollment	
Sample	All	Boys	Girls	South	Non-South	Age 16-18	Age 19-21
Intercept 1940	8.377**	8.225**	8.573**	7.302**	8.880**	0.407**	0.0184
	(0.251)	(0.244)	(0.288)	(0.243)	(0.281)	(0.0288)	(0.0455)
Intercept 1960	10.34**	10.30**	10.49**	9.965**	10.61**	0.558**	0.123**
	(0.162)	(0.153)	(0.196)	(0.132)	(0.196)	(0.0255)	(0.0349)
Intercept 1970	11.24**	11.34**	11.17**	10.80**	11.38**	0.714**	0.191**
	(0.190)	(0.194)	(0.205)	(0.245)	(0.199)	(0.0211)	(0.0337)
Intercept 1980	11.83**	11.82**	11.86**	11.41**	12.04**	0.680**	0.210**
	(0.158)	(0.162)	(0.171)	(0.137)	(0.184)	(0.0218)	(0.0319)
Intercept 1990	11.94**	11.89**	12.00**	11.66**	12.10**	0.794**	0.304**
	(0.209)	(0.205)	(0.240)	(0.151)	(0.270)	(0.0255)	(0.0392)
Intercept 2000	12.25**	12.10**	12.44**	12.08**	12.37**	0.803**	0.346**
	(0.231)	(0.239)	(0.244)	(0.176)	(0.287)	(0.0258)	(0.0411)
Slope 1940	0.401**	0.430**	0.355**	0.547**	0.338**	0.0391**	0.0295**
	(0.0440)	(0.0438)	(0.0492)	(0.0433)	(0.0491)	(0.00523)	(0.00815)
Slope 1960	0.277**	0.309**	0.222**	0.368**	0.231**	0.0204**	0.0271**
	(0.0272)	(0.0261)	(0.0320)	(0.0249)	(0.0317)	(0.00430)	(0.00587)
Slope 1970	0.245**	0.247**	0.238**	0.289**	0.229**	0.0203**	0.0319**
	(0.0293)	(0.0303)	(0.0313)	(0.0370)	(0.0308)	(0.00344)	(0.00537)
Slope 1980	0.193**	0.196**	0.186**	0.245**	0.168**	0.0165**	0.0294**
	(0.0245)	(0.0254)	(0.0261)	(0.0219)	(0.0282)	(0.00351)	(0.00503)
Slope 1990	0.194**	0.188**	0.197**	0.229**	0.174**	0.0141**	0.0385**
	(0.0323)	(0.0321)	(0.0365)	(0.0242)	(0.0410)	(0.00413)	(0.00622)
Slope 2000	0.174**	0.164**	0.178**	0.187**	0.163**	0.0128**	0.0369**
	(0.0363)	(0.0376)	(0.0386)	(0.0291)	(0.0443)	(0.00419)	(0.00658)
Observations	60	60	60	60	60	60	60
R-squared	1.000	1.000	1.000	1.000	1.000	0.998	0.987

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

Table 6: 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 (16). Regressions weighted by square root of estimated cell sizes.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Var	Highest Grade Attained, Age 22-25					Enrollment	
Sample	All	Boys	Girls	South	Non-South	Age 16-18	Age 19-21
Intercept 1940	7.730**	7.298**	8.352**	7.256**	9.354**	0.597**	0.155*
	(0.139)	(0.142)	(0.227)	(0.136)	(0.145)	(0.0711)	(0.0692)
Intercept 1960	9.937**	9.894**	9.744**	9.604**	10.62**	0.565**	0.129**
	(0.124)	(0.0969)	(0.174)	(0.130)	(0.138)	(0.0402)	(0.0344)
Intercept 1970	10.70**	10.78**	11.13**	10.40**	10.74**	0.718**	0.229**
	(0.0987)	(0.110)	(0.160)	(0.114)	(0.121)	(0.0243)	(0.0255)
Intercept 1980	11.32**	11.34**	11.61**	11.25**	11.61**	0.702**	0.215**
	(0.101)	(0.0868)	(0.112)	(0.108)	(0.115)	(0.0197)	(0.0176)
Intercept 1990	11.12**	11.05**	11.38**	11.07**	11.41**	0.773**	0.227**
	(0.166)	(0.150)	(0.183)	(0.178)	(0.189)	(0.0242)	(0.0214)
Intercept 2000	10.89**	10.89**	10.91**	10.95**	10.95**	0.677**	0.199**
	(0.192)	(0.183)	(0.257)	(0.208)	(0.222)	(0.0246)	(0.0226)
Slope 1940	0.526**	0.541**	0.464**	0.581**	0.345**	0.0331	0.0403
	(0.0603)	(0.0597)	(0.0810)	(0.0638)	(0.0680)	(0.0227)	(0.0213)
Slope 1960	0.271**	0.255**	0.217**	0.316**	0.168**	0.0222	0.0396**
	(0.0390)	(0.0324)	(0.0482)	(0.0459)	(0.0483)	(0.0142)	(0.0130)
Slope 1970	0.276**	0.314**	0.257**	0.313**	0.250**	0.0190*	0.0456**
	(0.0251)	(0.0334)	(0.0498)	(0.0277)	(0.0294)	(0.00835)	(0.00902)
Slope 1980	0.205**	0.198**	0.196**	0.208**	0.180**	0.0223**	0.0497**
	(0.0205)	(0.0192)	(0.0236)	(0.0223)	(0.0238)	(0.00590)	(0.00532)
Slope 1990	0.215**	0.211**	0.204**	0.221**	0.186**	0.0181**	0.0595**
	(0.0277)	(0.0258)	(0.0313)	(0.0305)	(0.0324)	(0.00632)	(0.00564)
Slope 2000	0.258**	0.217**	0.296**	0.253**	0.251**	0.0326**	0.0617**
	(0.0303)	(0.0282)	(0.0411)	(0.0334)	(0.0355)	(0.00607)	(0.00555)
Observations	58	55	53	58	58	52	52
R-squared	1.000	1.000	1.000	1.000	1.000	0.998	0.993

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

Table 7: Mobility Estimates in Parental Education, Blacks

Notes: Displays estimated intercepts and slopes of children's schooling gradients with respect to parental education for blacks. Presents estimates of α_t and β_t from Equation (16). Regressions weighted by square root of estimated cell sizes.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Var	Highest Grade Attained, Age 22-25					Enrollment	
Sample	All	Boys	Girls	South	Non-South	Age 16-18	Age 19-21
Intercept 1940	6.145**	5.526**	6.812**	5.827**	8.312**	0.392**	0.0651**
	(0.185)	(0.189)	(0.227)	(0.249)	(0.290)	(0.0209)	(0.0197)
Intercept 1960	9.358**	9.194**	9.482**	9.006**	10.66**	0.565**	0.168**
	(0.108)	(0.101)	(0.164)	(0.152)	(0.146)	(0.0167)	(0.0138)
Intercept 1970	10.69**	10.44**	10.89**	10.56**	10.77**	0.697**	0.191**
	(0.124)	(0.134)	(0.138)	(0.215)	(0.107)	(0.0126)	(0.0121)
Intercept 1980	11.75**	11.58**	11.90**	11.69**	11.87**	0.702**	0.258**
	(0.0929)	(0.108)	(0.0973)	(0.143)	(0.0952)	(0.0135)	(0.0114)
Intercept 1990	11.92**	11.79**	12.06**	11.86**	12.02**	0.790**	0.327**
	(0.113)	(0.132)	(0.118)	(0.169)	(0.120)	(0.0155)	(0.0134)
Intercept 2000	11.92**	11.68**	12.07**	11.92**	11.92**	0.806**	0.311**
	(0.147)	(0.190)	(0.139)	(0.212)	(0.175)	(0.0161)	(0.0152)
Slope 1940	0.679**	0.729**	0.596**	0.706**	0.311**	0.0479**	0.0263**
	(0.0610)	(0.0649)	(0.0698)	(0.0949)	(0.0712)	(0.00722)	(0.00666)
Slope 1960	0.331**	0.334**	0.271**	0.380**	0.0204	0.0122*	0.0127**
	(0.0335)	(0.0349)	(0.0419)	(0.0626)	(0.0319)	(0.00518)	(0.00436)
Slope 1970	0.298**	0.317**	0.273**	0.382**	0.251**	0.0208**	0.0312**
	(0.0387)	(0.0419)	(0.0423)	(0.0793)	(0.0300)	(0.00337)	(0.00339)
Slope 1980	0.182**	0.184**	0.180**	0.201**	0.154**	0.0205**	0.0255**
	(0.0213)	(0.0251)	(0.0219)	(0.0384)	(0.0189)	(0.00305)	(0.00259)
Slope 1990	0.149**	0.139**	0.156**	0.164**	0.129**	0.0141**	0.0314**
	(0.0235)	(0.0273)	(0.0247)	(0.0383)	(0.0225)	(0.00332)	(0.00284)
Slope 2000	0.162**	0.150**	0.188**	0.174**	0.153**	0.0128**	0.0353**
	(0.0284)	(0.0352)	(0.0281)	(0.0430)	(0.0314)	(0.00344)	(0.00312)
Observations	58	59	57	58	58	60	59
R-squared	1.000	1.000	1.000	0.999	1.000	0.999	0.995

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

Table 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. Presents estimates of α_t and β_t from Equation (16). Regressions weighted by square root of estimated cell sizes.

X VARIABLE	(1) Avg Log Income	(2) Ineq: p75-p25	(3) Dropout Age	(4) Class Size	(5) Rel Teacher Pay	(6) Teen Birth Rate	(7) Share Black
				Mobility in Parental Education			
Corr(Intercept, X)	0.0774	0.0756	-0.00303	-0.102	0.147	-0.167*	-0.0977
	(0.0793)	(0.0793)	(0.0967)	(0.113)	(0.113)	(0.0787)	(0.0794)
	[N=160]	[N=160]	[N=109]	[N=79]	[N=79]	[N=159]	[N=159]
Corr(Slope, X)	-0.402**	0.385**	-0.189	-0.0161	-0.191	0.257*	0.0855
	(0.128)	(0.118)	(0.156)	(0.173)	(0.178)	(0.121)	(0.120)
	[N=206]	[N=206]	[N=132]	[N=99]	[N=99]	[N=204]	[N=204]
				Mobility in Parental Income			
Corr(Intercept, X)	0.254**	-0.187*	0.292**	-0.257*	0.105	-0.145	-0.123
	(0.0865)	(0.0879)	(0.104)	(0.113)	(0.116)	(0.0885)	(0.0888)
	[N=127]	[N=127]	[N=87]	[N=75]	[N=75]	[N=127]	[N=127]
Corr(Slope, X)	-0.527**	0.527**	-0.237*	0.111	-0.365**	0.180*	0.103
	(0.0760)	(0.0760)	(0.105)	(0.116)	(0.109)	(0.0880)	(0.0890)
	[N=127]	[N=127]	[N=87]	[N=75]	[N=75]	[N=127]	[N=127]

Standard errors in parentheses, number of underlying state by year cells in brackets
** p<0.01, * p<0.05

Table 9: Correlations of Mobility with State Characteristics, 1940-2000

Notes: Table displays bivariate correlations of mobility statistics listed in row titles for state-of-birth by year cells with characteristics listed in column titles for state-of-residence by year cells from the previous census when children would have been 12-15 years old. Correlations constructed using residuals of all variables from regressions on full set of region by year dummies, using four regions based on census definitions. Correlations restricted to intercepts and slopes estimated with $R^2 \geq 0.5$, and with full coverage over parental income and education groups. Restricted to whites. Parental education groups defined as highest grade attained range 6.5-17 of household head. Parental income groups defined as total parental earnings decile, excluding households with zero parental earnings. Variables in column headings defined as follows. "p75-p25" defined as difference between 75th and 25 percentiles of log total parental earnings distribution. "Dropout age" refers to minimum school dropout age. Relative teacher pay refers to the ratio of the average public teacher salary in a state to the average salary of non-teacher college-educated workers in that state. "ln(Income)" refers to average of the log of the sum of husband's and wife's earnings. Income data are calculated from the census for household heads aged 40-55 in years 1940-2000, and for 1930 are taken from "State Personal Income 1929-99 CD-ROM" (U.S. Department of Commerce, Bureau of Economic Analysis) as displayed in (Garret and Wheelock, 2005). "Teen birth rate" defined as mean of dummy for any children for girls age 15-19 in census data. "Share black" refers to share black of all state residents in census data. Note p75-p25 in 1940 is matched to the year when children would have been 22-25 years old, rather than 12-15, due to the lack of state-level income inequality measures prior to 1940. Sample sizes smaller for dropout age, class size, and relative teacher pay because these variables were taken from Stephens and Yang (2014) and were not available for the full 1940-2000 period.

VARIABLES	(1) Educ: Intercept	(2) Educ: Slope	(3) Income: Intercept	(4) Income: Slope
Avg Log Income	0.0305	-0.0956	0.201	-0.231*
	(0.0606)	(0.112)	(0.127)	(0.101)
Inequality: p75-p25	0.156*	0.465**	-0.0459	0.348**
	(0.0618)	(0.107)	(0.120)	(0.0957)
Dropout Age	0.0576	-0.197	0.393**	-0.348**
	(0.0580)	(0.109)	(0.125)	(0.0995)
Class Size	-0.0278	0.102	-0.407**	0.233*
	(0.0716)	(0.131)	(0.138)	(0.110)
Rel Teach Pay	0.160	0.127	-0.0238	0.0346
	(0.0821)	(0.141)	(0.171)	(0.137)
Teen Birth Rate	-0.0886	-0.0775	-0.162	-0.0120
	(0.0597)	(0.107)	(0.130)	(0.103)
Share Black	-0.0491	0.121	0.345	-0.203
	(0.0690)	(0.118)	(0.173)	(0.138)
Constant	-0.171**	-0.148	-0.118	0.0695
	(0.0579)	(0.103)	(0.128)	(0.102)
Observations	79	97	75	75
R-squared	0.169	0.305	0.295	0.535

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

Table 10: Correlations of Mobility with State Characteristics, 1940-2000

Notes: Displays estimates from regression of mobility statistics on multiple state characteristics on data collapsed to state by year level. All variables and procedures are identical to those described in notes to Table 9. As described there, all variables have been residualized on a full set of region by year interactions for all years 1940-2000, and then converted to z-scores.

	(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 11: 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.

Decade	Dep Status	Income Quintile 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.1: 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 12-15	All Years	All Years	All Years	1940	1950	1960	1970	1980	1990	2000
Age 7	0.942**			0.999**	1.027**	0.866**	0.804**	0.868**	0.954**	1.009**
	(0.00750)			(0.0148)	(0.102)	(0.0351)	(0.0270)	(0.0187)	(0.0136)	(0.00618)
Ages 4-7, linear		0.900**								
		(0.00639)								
Ages 1-7, linear			0.911**							
			(0.00580)							
Constant	0.0101**	0.0161**	0.0136**	-0.00177	0.0136	0.0216**	0.0376**	0.0248**	0.00434	-0.00372
	(0.00227)	(0.00199)	(0.00180)	(0.00324)	(0.0217)	(0.00754)	(0.00679)	(0.00547)	(0.00398)	(0.00221)
Observations	241	241	241	36	33	36	36	36	32	32
R-squared	0.985	0.988	0.990	0.993	0.767	0.947	0.963	0.985	0.994	0.999

Standard errors in parentheses

** p<0.01, * p<0.05

Table A.2: Validation of Group Cohort Size Predictors: Parental Education Groups for Blacks

Notes: Columns (1)-(3) regress actual parental education group cohort shares at ages 12-15 on predicted parental education group cohort shares at ages 12-15 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 12-15	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.3: Validation of Group Cohort Size Predictors: Parental Income Groups for Blacks

Notes: Columns (1)-(3) regress actual parental income group cohort shares at ages 12-15 on predicted parental income group cohort shares at ages 12-15 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.

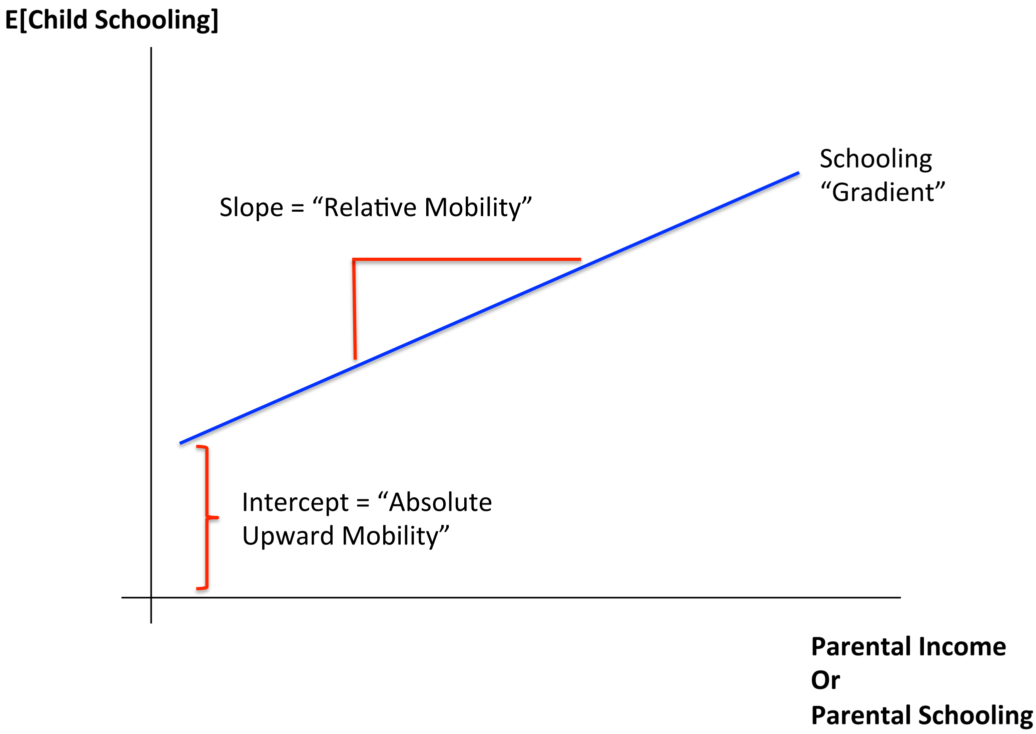


Figure 1: Illustration of Schooling Gradient and IM Statistics

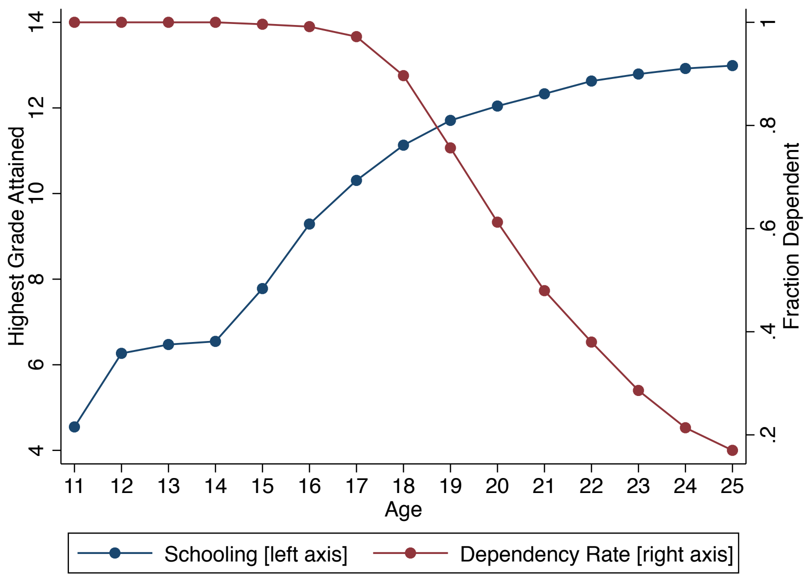
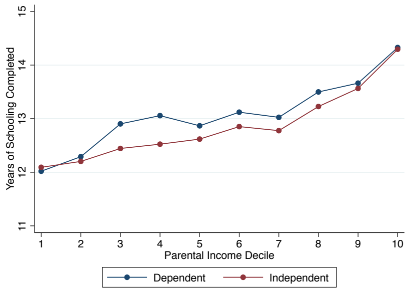
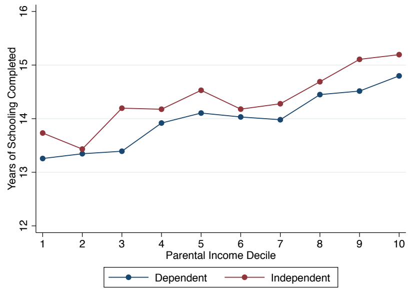


Figure 2: Schooling and Dependency Status by Age in 1980

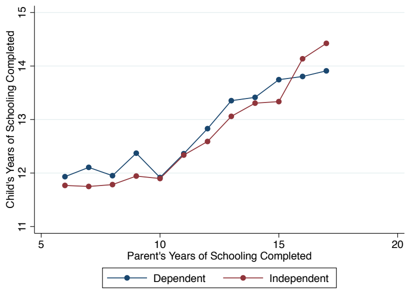
Notes: Red line plots fraction of native-born children living with parents by age in 1980. Blue line plots average schooling of native-born children by age in 1980. Whites only, excluding Alaska and Hawaii.



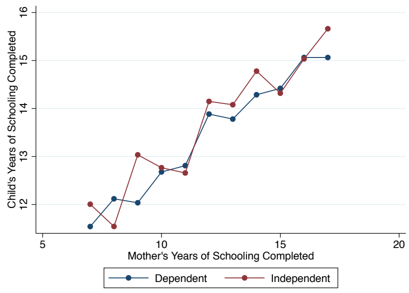
(a) Gradient in Parental Income, PSID



(b) Gradient in Parental Income, NLSY79



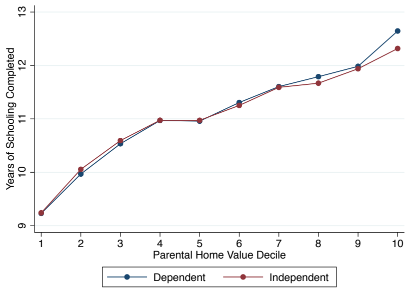
(c) Gradient in Parental Education, PSID



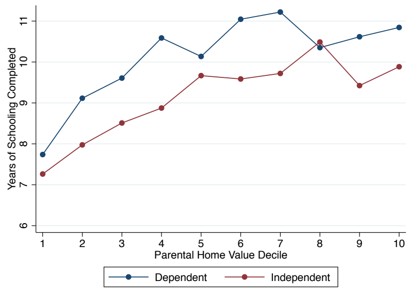
(d) Gradient in Parental Education, NLSY79

Figure 3: Highest Grade Attained at Ages 22-25 by Parental Characteristics at Age 17

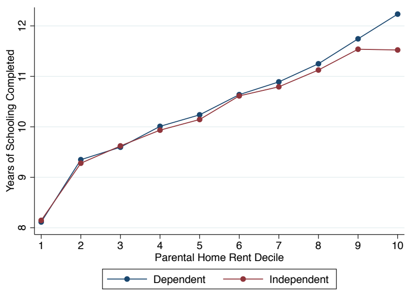
Notes: Figures based on data from PSID and NLSY79, pooling years 1968-2011 and 1994-2010, respectively. Parental characteristics measured when children are age 17. Children's schooling at ages 22-25 is set to missing when lower than six years. Children with zero parental income at age 17 excluded. Income deciles calculated separately by year. Sample weights used in all calculations.



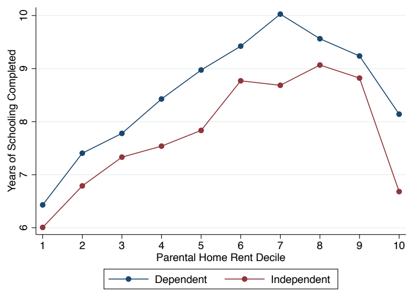
(a) Schooling by Parental Home Values, Whites



(b) Schooling by Parental Home Values, Blacks



(c) Schooling by Parental Rents, Whites



(d) Schooling by Parental Rents, Blacks

Figure 4: Final Schooling at Ages 22-25 in 1940 by Parental Group Status

Notes: Figures plot highest grade attained for ages 22-25 by parental home value or rent deciles based on matched 1930-1940 census data. Families with zero rent and earnings in 1930 excluded. Underlying data count each cohort aged 22-25 in 1940 equally for each parental group. 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.

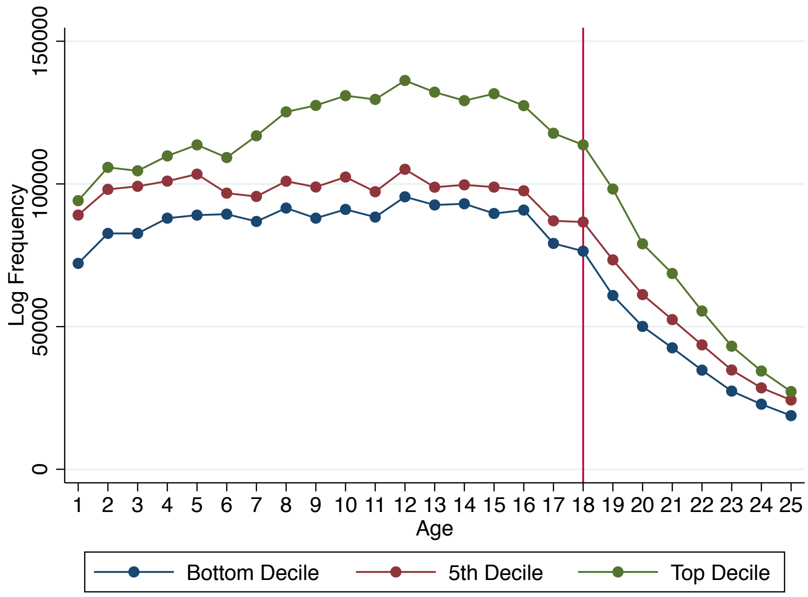
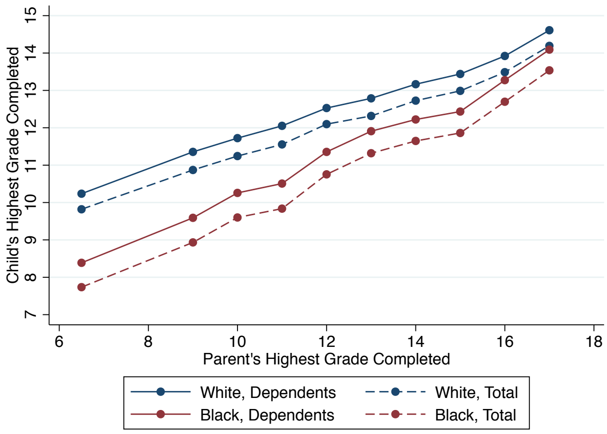
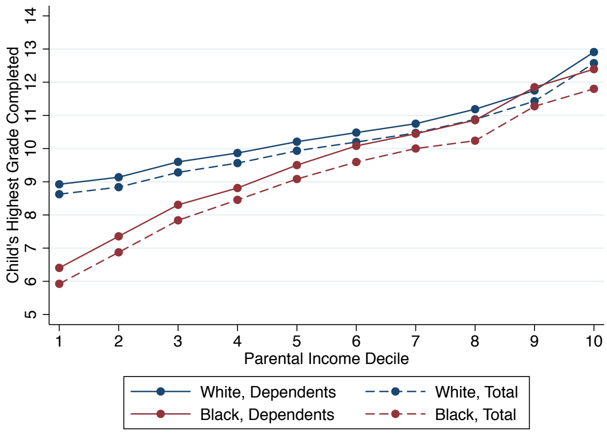


Figure 5: Number of Dependent Children by Age for Parental Income Decile Groups, 1940

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



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



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

Figure 6: Final Schooling Attainment at ages 22-25 by Parental Group Status, 1940

Notes: Figure plots estimated final schooling pooling separate estimates for ages 22-25, using the correction for independent children described in the text. Uncorrected estimates restrict to dependent children who can be linked with parents directly. Hawaii and Alaska excluded.

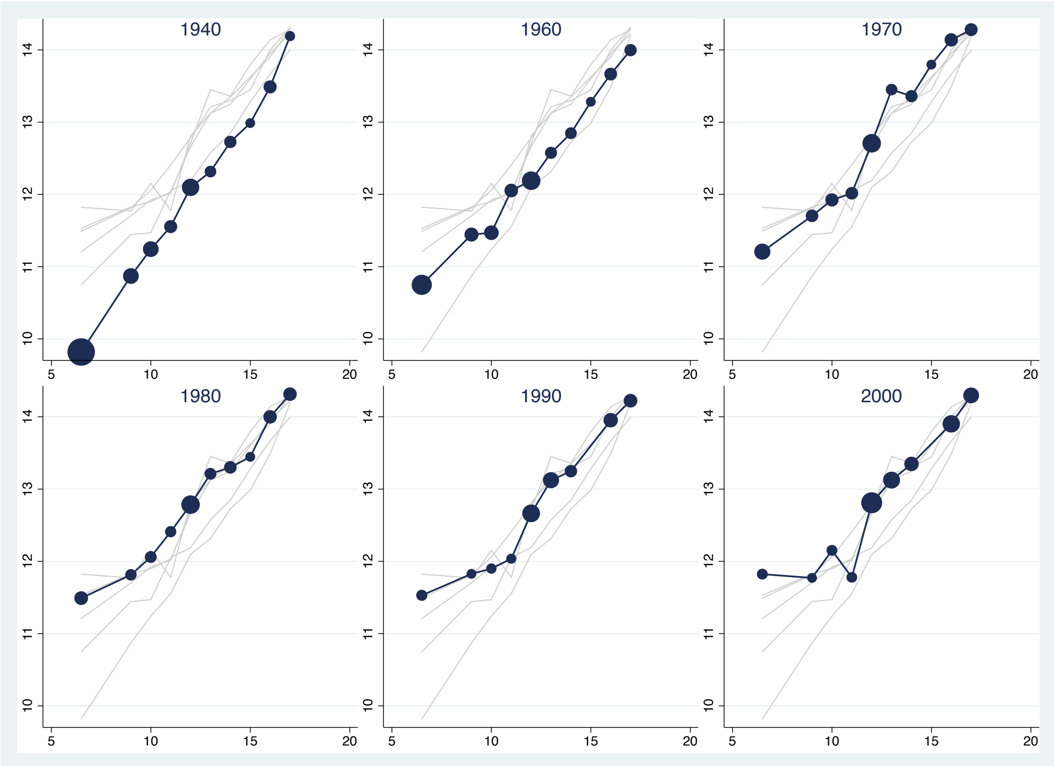
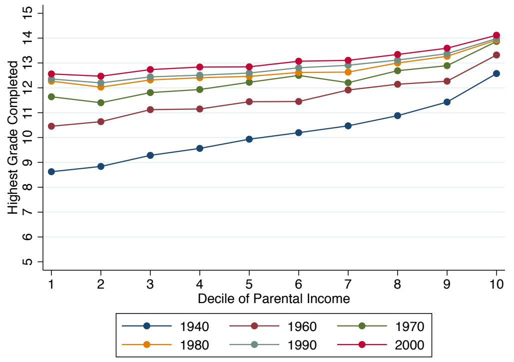
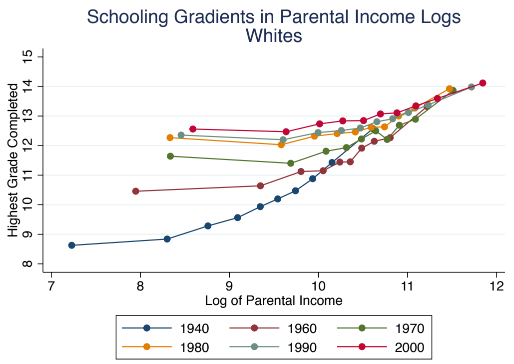


Figure 7: Final Schooling Attainment by Parental Schooling Attainment, 1940-2000

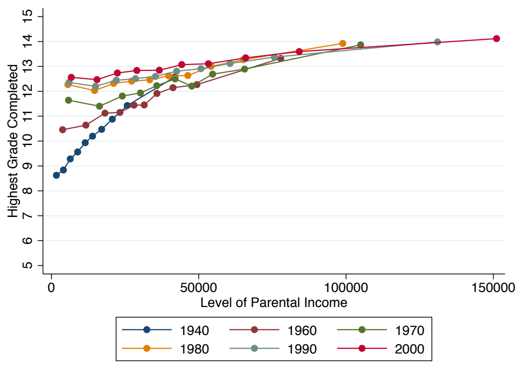
Notes: Figures plot highest grade attained by parental highest grade attained for whites age 22-25, by year. Figure adjusts for independent children as described in text.



(a) Decile of Parental Income



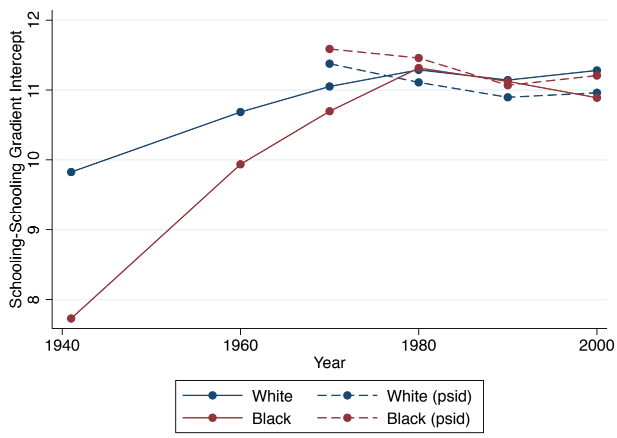
(b) Log of Parental Income



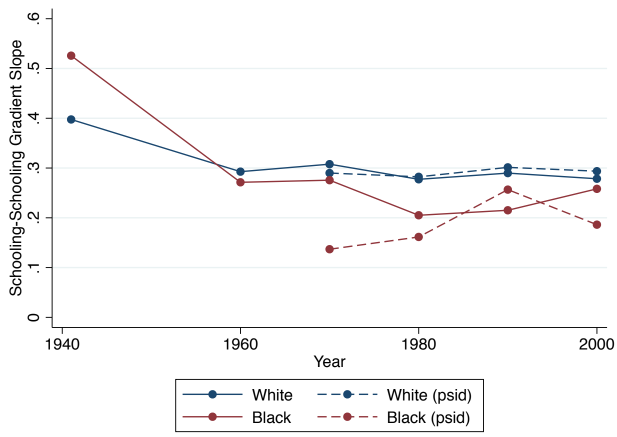
(c) Level of Parental Income

Figure 8: Final Schooling Attainment by Parental Income, 1940-2000

Notes: Figure adjusts for independent children and pools ages 22-25 as described in text. Parental income calculated as sum of head and spouse. Parents restricted to ages 25-65. Hawaii and Alaska excluded.



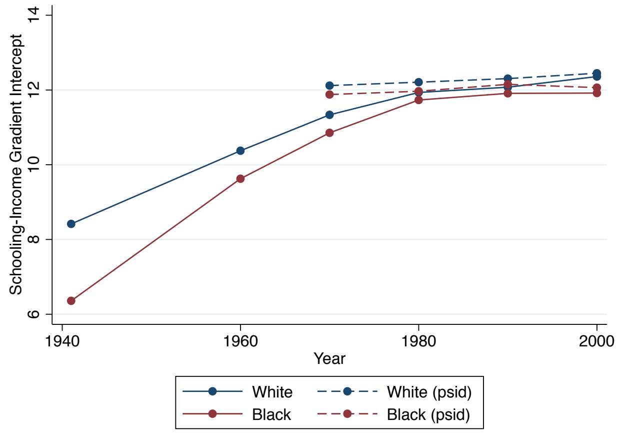
(a) Intercepts = Absolute Upward Mobility



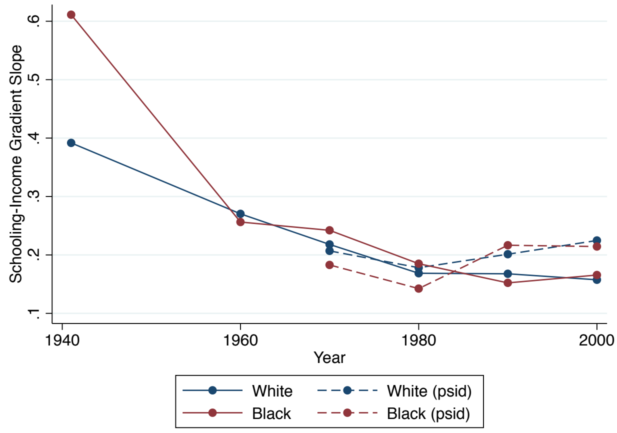
(b) Slopes = Relative Mobility

Figure 9: Intercepts and Slopes of Linear Schooling Gradients in Parental Education by Race and Year

Notes: 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.



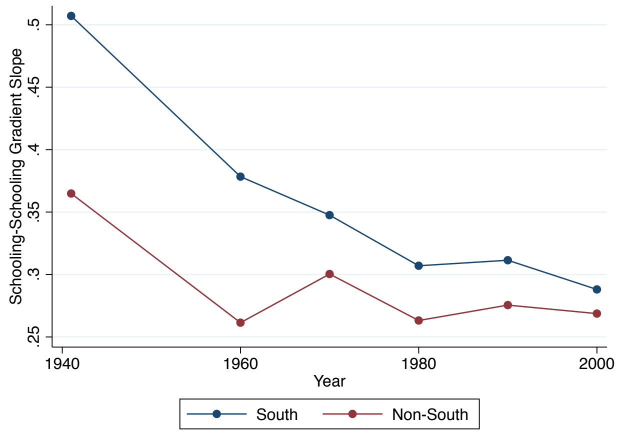
(a) Intercepts = Absolute Upward Mobility



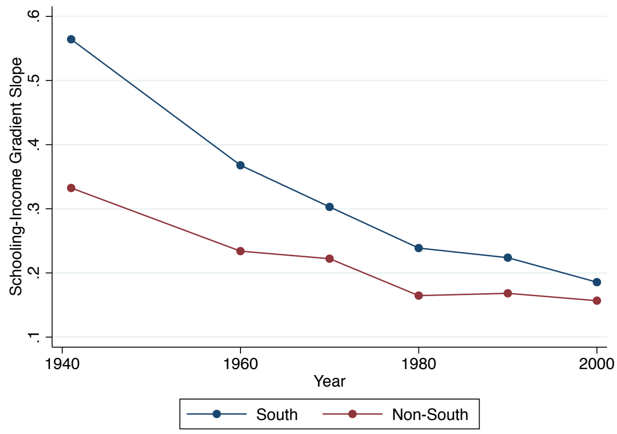
(b) Slopes = Relative Mobility

Figure 10: Intercepts and Slopes of Linear Schooling Gradients in Parental Income Deciles by Race and Year

Notes: 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.



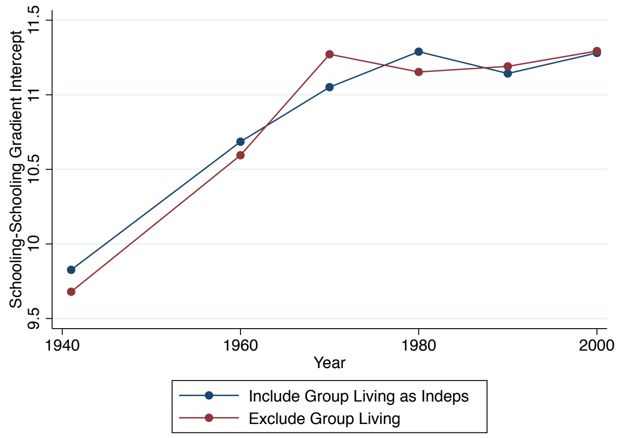
(a) Slopes of Schooling-Schooling Gradients



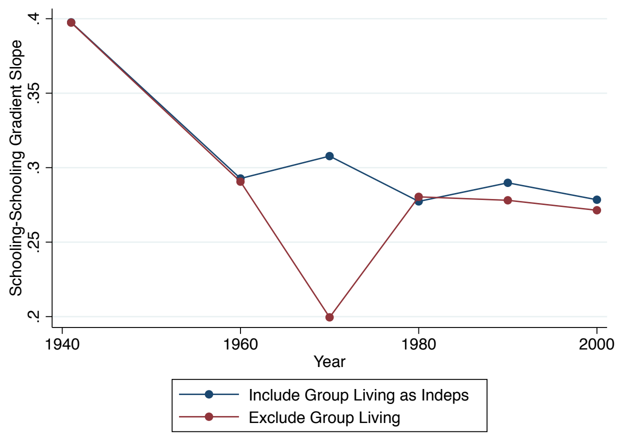
(b) Slopes of Schooling-Income Gradients

Figure 11: Slopes of Linear Schooling Gradients in South and Non-South

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 income decile level. Adjustment for independent children ages 22-25 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 5 and 6.



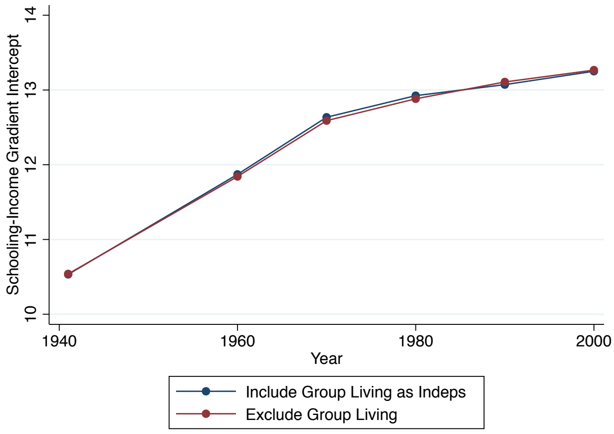
(a) Intercepts = Absolute Upward Mobility



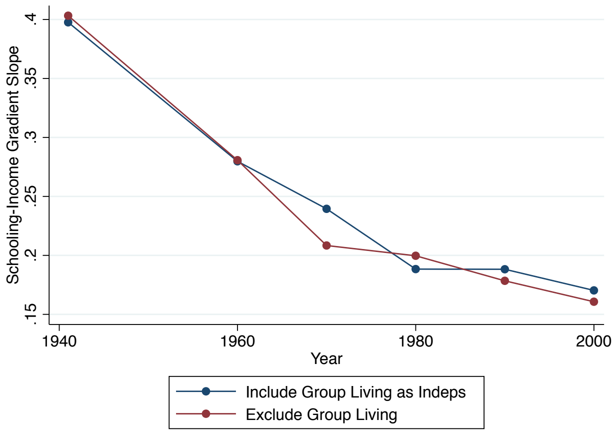
(b) Slopes = Relative Mobility

Figure A.1: Intercepts and Slopes of Linear Schooling Gradients in Parental Education by Sample and Year

Notes: 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.2: Intercepts and Slopes of Linear Schooling Gradients in Parental Income Deciles by Sample and Year

Notes: 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.

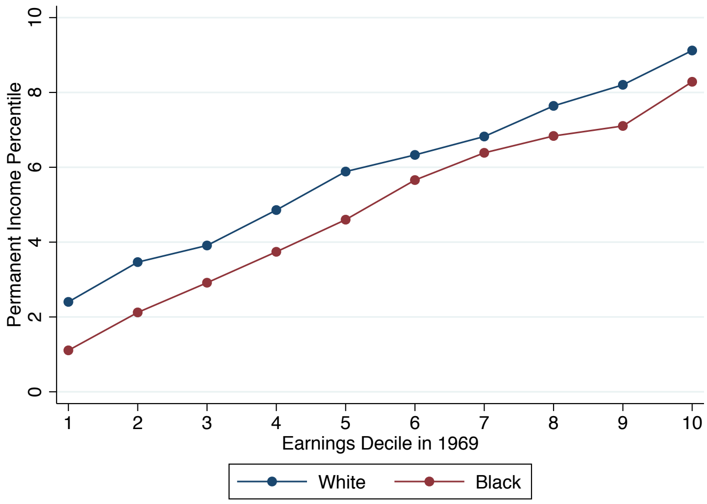


Figure A.3: Average Permanent Income Percentile by Annual Earnings Percentile in 1969

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.

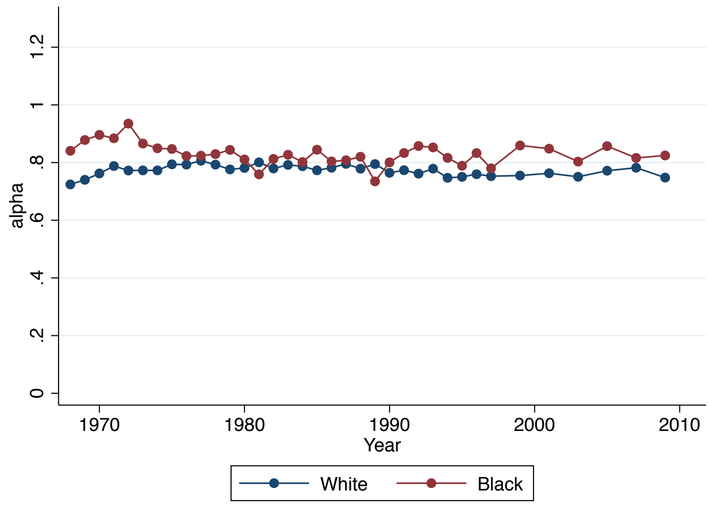


Figure A.4: Estimated α_g by Year

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.