Mobility for All: Representative Intergenerational Mobility Estimates over the 20th Century

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Abstract

We estimate long-run trends in intergenerational relative mobility for representative samples of the U.S.-born population. Harmonizing all surveys that include father's occupation and own family income, we develop a mobility measure that allows for the inclusion of non-whites and women for the 1910s–1970s birth cohorts. We show that mobility increases between the 1910s and 1940s cohorts and that the decline of Black-white income gaps explains about half of this rise. We also find that excluding Black Americans, particularly women, considerably overstates the level of mobility for twentieth-century birth cohorts while simultaneously understating its increase between the 1910s and 1940s.

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1 Introduction

Intergenerational relative mobility—how tied an individual's place in the income distribution is to her parents' place in the income distribution while she was growing up—has long been an object of interest, especially in the United States. While analysis of modern data shows that the US is less mobile than its rich peers (Bratsberg *et al.*, 2007; Jantti *et al.*, 2006), much less is known about trends in U.S. mobility over the 20th century.[1]

The main contribution of this paper is simple: we present, to the best of our knowledge, the first estimates of long-run intergenerational relative mobility trends for a *representative* sample of U.S.-born individuals. In particular, we show mobility estimates for children born in the 1910s through the 1970s.[2] As Table 1 shows, a handful of papers have made important contributions to our understanding of long-run trends in intergenerational relative mobility, typically relating occupational standing of one generation to the next (*Occ.-Occ.* mobility). However, for data reasons, they include only subsets (and typically *advantaged* subsets) of the population. Song *et al.* (2020) shows mobility of occupational prestige from 1830 to 1980, but only for white men. Using a clever synthetic-panel strategy based on the status information conveyed by first names, Olivetti and Paserman (2015) can compare occupational mobility between fathers and sons to that of fathers and sons-in-law, but only for white men and married white women. Collins and Wanamaker (2022) and Ward (2023) include Black Americans, but only men.[3]

We begin by locating (to the best of our knowledge) all surveys that ask individuals their current family income, as well as their race, father's occupation and region of birth or childhood. Instead of relying on the traditional *Occ.-Occ.* mobility measure (which complicates looking at women, as few formally worked after marriage in the historical period), we relate the *family income* reported by prime-age adults in these surveys to

[1] As Song *et al.* (2020) write: "evidence of long-term trends in intergenerational mobility is largely absent" (p. 251). Similarly, Mazumder (2018, p. 225-226) writes: "One active topic of research that has not yet been resolved is whether there have been major changes in intergenerational mobility in the United States over time."

[2] Note that we do not examine intergenerational *absolute* mobility, which captures the probability that a child's income as an adult surpasses her parents' income (in real dollars) while she was a child. Specifically, we do not have detailed income measures for the parents' generation (and instead rely on measures of predicted income, detailed in Section 3) so we cannot accurately calculate the share of adult children earning more than their parents. For recent work on intergenerational absolute mobility, see Chetty *et al.* (2017), Berman (2018), Berman (2022) and Manduca *et al.* (2021).

[3] As Table 1 shows, many important papers do include representative samples, but either for more modern birth cohorts only (Chetty *et al.*, 2014b; Solon, 1992; Chetty *et al.*, 2020; Mazumder, 2018) or short snapshots of time (Massey and Rothbaum, 2020; Card *et al.*, 2018).

their *predicted family income* during childhood.[4] We directly observe contemporaneous family income of the adult child as it is a question asked in many surveys and—unlike own occupation—can be answered by male as well as female respondents. Moreover, unlike occupation alone, self-reported family income naturally reflects income gaps by race or other characteristics. Similar to a two-sample instrumental variable approach, we predict a respondent's childhood income by using their race, region and father's occupation and calculating the average family income conditional on these characteristics among households with children in the Census or other auxiliary data sources (from as close as possible to the respondent's tenth birthday).[5]

Our main finding is that both the IGE and rank-rank correlation fell (meaning that mobility rose) between the 1910s and 1940s birth cohorts. The IGE (rank-rank correlation) falls from 0.75 (0.37) for those born in the 1910s to 0.42 (0.25) for those born in the 1940s. Between the 1940s and 1970s birth cohorts, the IGE measure drifts upward again, while there is little change in the rank-rank correlation. Both because the trends after the 1950s are more sensitive to the measure (IGE or rank-rank) and because alternative data sources and methodologies are available for these more modern birth cohorts, we mainly focus on the 1910s–1940s cohorts.

Importantly, we do not claim to have estimated *causal* effects of childhood income on adult income, which would require us to identify sources of exogenous variation in parental income in each of our birth decades. We view our results as descriptive. Moreover, given the variety of approaches authors have taken to overcome various data limitations inherent in estimating historical mobility, we do not claim to have estimated "the" IGE or rank-rank from this period (e.g., comparing the magnitudes of our results to those of an *Occ.-Occ.* estimation should be done with caution). Rather, the main goal of our various robustness checks is to show that the biases of our approach relative to the ideal (and infeasible) OLS regression of child on parental income are not changing over time in a manner that would produce our result as an artifact.

To the best of our knowledge, the significant *increase* in U.S. intergenerational relative mobility from the 1910s to the 1940s birth cohorts that we find is novel in the literature. Much of the existing historical literature emphasizes either rising persistence or stability (e.g., Song *et al.*, 2020; Olivetti and Paserman, 2015; Davis and Mazumder,

[4] Note from Table 1 that Collins and Wanamaker (2022) and Ward (2023) both take an income-score to income-score approach (in essence, predicting income using information on occupation, race, and region for both generations). This approach remains problematic for women, however, for the same reason as *Occ.-Occ.* measures: a woman's own occupation is both endogenous to marriage and not highly predictive of economic well-being in the historical period.

[5] Björklund and Jäntti (1997), Bloise *et al.* (2021) and others use a two-sample instrumental variable approach to estimate intergenerational income mobility, but this approach has been less common in the U.S. context given limited historical microdata.

2022), especially in the U.S. context. Our uncovering a period of significantly *rising* mobility is due to two factors. First, the 1910s–1940s birth cohorts have been understudied, as they are born too recently for Census-based linking but also too long ago for study using IRS or PSID data.[6] Second, we show that the more traditional statistic—the *Occ.-Occ.* mobility estimate for white men—misses much of the rise in mobility for these cohorts. Indeed, we show that using additional inputs beyond father's occupation improves the prediction of childhood income and offers additional insights on the evolution of mobility than the traditional *Occ.-Occ.* mobility measure. This period of rising mobility in the U.S. complements recent studies (Karlson and Landersø, 2021; Nybom and Stuhler, 2023; Pekkarinen *et al.*, 2017) documenting increases in mobility in Denmark, Sweden, and Norway following educational reforms in the mid-twentieth century.

In the second part of the paper, we focus on subgroups (mostly the four subgroups defined by Black/white race and male/female sex) and in particular how movements of these subgroups contributed to (or slowed) the increase in mobility from the 1910s to 1940s cohorts. Like inequality measures and unlike means, the full-population IGE (or rank-rank) slope is *not* a weighted average of subgroup slopes. In particular, in societies with two very unequal and endogamous subgroups (a description that applies to white and Black Americans over much of the 20th century), between-group differences in *mean* incomes play a major role in determining overall relative mobility for any given birth cohort and *changes* in those mean differences play an important role in determining *trends* in overall mobility.

We show that between the 1910s–1920s and the 1940s–1950s birth cohorts, Black Americans exhibit significant (though still partial) convergence to whites in both (predicted) childhood income and adult income. Whites also enjoy income growth in real terms (though slower than Black individuals), *and* their IGE and rank-rank slopes become flatter (meaning that, *within the white population*, parental income matters less in predicting own adult family income). Our decomposition, applying Hertz (2008) to our historical data, shows that the Black-white convergence in mean income accounts for half of the rise in overall mobility (and the flattening of the white slope accounts for the remainder). This result is quite striking given that Black Americans are a relatively small share of the U.S. population—roughly twelve percent for much of our sample period. But because they are drawn from an extremely low part of the childhood and adult income distribution and in our historical period did not intermarry with whites, changes in their average income exert great statistical influence on the

[6] As of today, the most recent Census that can be linked is that of 1940, when the 1910s birth cohorts would be only in their twenties and the 1940s cohorts not yet born.

overall regression line.

In this paper we pay particular attention to Black women. First, because of data limitations, there has been almost no work on historical intergenerational mobility that includes this group. Second, we show that because Black women tend to grow up in the bottom of the income distribution (as do their male counterparts) and in our historical period are the lowest-income group as adults (substantially poorer than Black men), they play an especially outsize role in increasing full-population intergenerational persistence measures. As just one example, in 1920, the IGE increases from 0.51 to 0.59 (with non-overlapping confidence intervals) when Black women (only six percent of the population) are added to the rest of the sample. Excluding even this small share of the population overstates early twentieth-century U.S. mobility considerably. At the same time, we show that excluding Black Americans or even only Black women significantly reduces the *rise* in mobility in the first half of the twentieth century and, as we note, is one reason that past studies of this period focusing on white men have not found large declines in persistence.

We close the paper with a brief analysis of what role modern racial income gaps play in explaining low levels of U.S. mobility relative to rich peers. Decomposition exercises show that modern levels of racial inequality set a very high lower bar on U.S. intergenerational persistence: to attain an IGE of 0.20 (roughly that in Denmark) while maintaining current racial differences in income, U.S. within-race IGEs would have to fall below 0.05, a remarkably high and likely unattainable level of within-race mobility.

The remainder of the paper is organized as follows. In the next section we describe the various datasets we use. In Section 3, we describe our methodology, in particular the adult-family-income-to-predicted-childhood-income mobility measure. Section 4 presents our results for the full, representative population and Section 5 probes the robustness of these results. Section 6 presents a decomposition of the full-population mobility measures and then decomposes the rise in mobility into differential mobility by race and gender. Section 7 concludes.

2 Data

In this section, we briefly describe the datasets that we use in this paper and share summary statistics. Far greater detail can be found in Appendix E.

2.1 Datasets and sampling rules

We have located to the best of our knowledge all surveys that ask respondents their current family income, their fathers' occupation while they were growing up (with sufficient detail), their race, and the region of the country where the respondent was born or grew up (at least to the level of South versus other regions). We end up locating 15 different surveys, with details on all of them provided in Appendix E. Most readers will be familiar with some (e.g., the General Social Survey or the American National Election Survey), but others are not as well known (e.g., the National Survey of Black Americans or Americans View their Mental Health).[7]

We restrict attention to U.S.-born men and women in the 30–50 age range in order to ensure that we are measuring life-cycle earnings as closely as possible.[8] Because advantaged children spend on average more time in formal education, their earnings tend to be disproportionately depressed in the late twenties relative to their prime-age earnings, so measuring the adult child's income at these ages may lead to downward bias of persistence measures. Haider and Solon (2006) suggest as a rule-of-thumb to observe adult children as close to age forty as possible.

All of the surveys used in this analysis ask respondents about their total family income. Many of the surveys ask respondents to report their income by choosing an interval (e.g., \$8,000–\$10,000), whereas others allow respondents to provide an exact value. To be consistent across surveys and over time, we transform the variables in the latter group to resemble those in the former group, so that our baseline measure of an adult child's family income is a categorical variable with a similar number of income bins over time. For more details on the construction of this harmonized variable, we refer the reader to Appendix E.

Our baseline sample spans the 1910s–1970s birth cohorts, and consists of respondents with non-missing family income and with available information on race, childhood location, and father's occupation (used to predict parental income, as described in Section 3). In the earliest cohorts in our sample, the share of children living without fathers is very small. Later in the paper, we present various robustness checks to assess sensitivity of the more modern results to various assumptions about missing fathers

[7] In some cases, the data we use are in fact panel datasets that follow individuals and families over time (e.g., the Panel Study of Income Dynamics [PSID] and the National Longitudinal Surveys of Mature Women and Older Men) and have often been used to measure mobility for more modern periods. To remain consistent within our methodology, however, we do not use the *panel* components of these datasets. In the first wave, these panel datasets often ask the adult respondent questions about their own childhood, and it is this linkage that we use to predict the respondent's family income in childhood.

[8] We restrict the sample to U.S.-born men and women because we want to ensure that our measures of childhood income—which are derived from U.S. sources—are relatively accurate approximations of income in the parental generation. The share of adult children that are excluded because of this restriction is relatively small: the share of adults ages 30–50 who were born outside of the U.S. ranges from 5% to 9% in the 1950–1980 Censuses, which correspond to our time period of interest. We do note however that first-generation immigrant parents (a sizable group in this time period) would be included in the analysis as long as their children were born in the U.S.

(i.e., including non-working or retired fathers, or using information about the mother's occupation).

In many cases, the data collection for these surveys was explicitly meant to be representative and provides survey weights to correct deviations due to sampling error. In those cases, we use the provided sampling weights. Of course, some of these surveys target one sex (e.g., the National Fertility Survey) or one race (e.g., the National Survey of Black Americans) and so are clearly not representative of the full U.S.-born population. In the early cohorts, we also have a substantially lower share of women in our data relative to the general population. For this reason, we will always re-weight the pooled dataset so that each cohort has weighted shares for white women, white men, Black women and Black men that match the corresponding shares in the Census (Ruggles *et al.*, 2021).[9] In Appendix B, we show that our main results barely change under other weighting schemes, including not weighting at all.

2.2 Summary statistics

The first panel of Table 2 shows summary statistics of the fathers of the respondents in our main dataset, separately by decade of birth. In this table we do not weight at all so that readers can get a sense of the raw data.

The decline of agriculture as a dominant occupation for fathers is readily apparent for children in the 1910s–1950s birth cohorts, falling from over one-third to less than one-tenth. We do not have father's education in every survey, but the table shares summary statistics from those surveys that do include father's education. In our earliest birth cohorts, the fathers in our data are born in the last few decades of the nineteenth century and thus grew up before the high school movement, which is reflected in their low levels of secondary education. Less than twenty percent of the fathers of our 1910s and 1920s birth cohorts graduated from high school. College graduation was a rarity for these fathers and as late as the 1950s birth cohort less than one in six of respondents have fathers who completed college.

Summary statistics for the adult children (i.e., the survey respondents) appear in the second panel of the table. The age of respondents is relatively similar and always close to forty, as we would expect from our 30–50 age restriction. In contrast to past historical work on U.S. mobility—which either excludes non-whites or uses linkage techniques that significantly under-sample non-whites—our samples have coverage of

[9] We only focus on individuals whose race is classified as white or Black. Individuals of other races account for tiny shares of the surveys' samples in these historical time periods (1% or less of the sample in the pre-1950 cohorts). The decomposition in Section 6 also highlights that groups with very small population shares are unlikely to affect the full-population measures of persistence.

Black individuals very close to their population shares even before weighting.

A number of well-known trends among the children are apparent in our data. The rise of educational attainment from the 1910s to the 1950s birth cohorts is striking and consistent with Goldin and Katz (2010). High school attainment increases from one-half to 90 percent, and college graduation rates nearly triple from ten to twenty-eight percent. The increase in education from one generation to the next is massive as well: for the 1910s to 1930s birth cohorts, the likelihood our survey respondents graduate from high school is triple that of their fathers.[10]

Appendix Table A.1 separates our data (unweighted, as in the previous table) by time period, race and sex and compares survey respondents to the relevant population in the Census. As before, we see that in all periods and separately for men and women, our data are very close to representative on race (roughly ten to fifteen percent of the sample). In fact, one of the only variables on which there are small discrepancies between our raw survey data and the Census data is education in the earliest birth cohorts (we later show robustness to using weights that adjust for these differences). Otherwise, our raw survey data is remarkably similar to the Census in terms of age, the share living or originating from the South (an especially important variable for Black respondents), and marital patterns.

3 Methodology

With ideal data, we would regress log permanent household income of the adult child on log permanent income of her household while she was growing up. As is well understood in the U.S. historical mobility literature, such a regression is not feasible, so the next subsections describe the approach we follow instead.

3.1 Specifications

With ideal data, we would estimate changes in intergenerational mobility over time using the classic log-log specification (Becker and Tomes, 1979):

$$\log(y_{ic}) = \beta_c^{\text{OLS}} \log(y_{ic}^p) + \epsilon_{ic}, \tag{1}$$

[10] Another marked trend for the adult children in our data is the decline in veteran status (which the table reports only for men in surveys that asked about veteran status). While over seventy percent of men in our 1920s cohort report military service, by the 1950s cohort military service has become relatively rare. Finally, another noticeable trend is union membership: while it holds steady in the high-twenties to low-thirties for our early cohorts, it begins a steady decline with the 1950s cohort, consistent with Farber *et al.* (2021).

where y_{ic} is the permanent household income of respondent i born in cohort c; y_{ic}^p is permanent family income of respondent i's parents, and ϵ_{ic} is the error term. Here, the coefficient β_c^{OLS} is the intergenerational elasticity (IGE) and it is a descriptive coefficient that does not take on causal interpretation. Estimating this equation separately by birth cohort would allow us to see how β_c^{OLS} changes across cohorts c.

Because our surveys do not include information about parental income, estimating this ideal β^{OLS} is not feasible. The preferred approach is thus a two-sample two-stage least squares (TS2SLS; Inoue and Solon, 2010) estimation, using auxiliary data sources as well as information about the respondents' upbringing to predict their log parental income. This empirical strategy has been a common approach in the intergenerational mobility literature (see, e.g., Aaronson and Mazumder, 2008; Bloise *et al.*, 2021; Björklund and Jäntti, 1997; Olivetti and Paserman, 2015). Because the fifteen surveys in our baseline sample include information about the respondent's race, childhood location, and father's occupation, we can use these variables in auxiliary datasets (e.g., U.S. Census microdata) to predict the log income of individuals with those same characteristics.

Our surveys include respondents born between the 1910s and 1970s birth cohorts, so implementing a TS2SLS strategy requires microdata that spans this time period. Nevertheless, as is well-known in the U.S. economic history literature, there are limited sources of microdata that include income measures prior to the 1940 Census, especially for representative samples of the population. Given that we do not have microdata to predict parental income for the 1910s birth cohort, we instead implement a modified TS2SLS strategy. In Section 5, we present numerous robustness checks, including standard TS2SLS estimates.

We use available sources of microdata as well as historical records of income from the early 20th century (described in Section 3.2) to calculate average income conditional on occupation, race, and location. We then apply a log transformation, and use these imputed measures of log parental income as our right-hand-side variable.[11] This imputation approach is frequently used in U.S. economic history papers, including Collins and Wanamaker (2022) and Ward (2023). Our baseline specification is thus:

$$\log(y_{ic}) = \beta_c \widetilde{\log(y_{ic}^p)} + \epsilon_{ic}. \tag{2}$$

[11] The difference between the TS2SLS and imputation approaches is the order of prediction versus log transformation of parental income. TS2SLS predicts average log parental income using microdata in the first stage of the estimation, whereas the imputation approach calculates average income for each cell and *then* applies the log transformation. In Section 5.3 we present results from a levels-based specification that avoids these issues, as well as TS2SLS estimates of our main specifications, though we cannot extend the analysis as far back in time with this approach.

In Appendix D we more formally compare our estimated $\hat{\beta}_c$ and the ideal β_c^{OLS}, but in this and the next subsection, we focus on simply describing our estimation procedure.

As an alternative to the IGE, we follow the rank-rank approach in Chetty *et al.* (2014a).[12] The rank of the adult child, Rank_{ic}, is the rank of predicted family income among all adult children born in the same year. Similarly, the rank of the parents, Rank_{ic}^{p}, is the percentile (based on predicted parental income) among all parents having a child born in the same year. The mapping of child's rank to parental rank (the copula of the joint distribution) tends to be linear and can handle zeros, which may be missed in the (logarithmic) IGE specification. Chetty *et al.* (2014a) focus on this specification:

$$\text{Rank}_{ic} = \gamma_c \widetilde{\text{Rank}}_{ic}^{p} + \delta_{sy} + \epsilon_{ic}. \tag{3}$$

In this estimation, γ_c is an estimate of the rank-rank correlation for cohort c. Again, we estimate this equation separately by birth decade.[13]

While the rank-rank measure has become a fixture of the intergenerational mobility literature, it is unwieldy for decomposing changes in the full-population mobility measure into subgroup-specific changes, as changing a subgroup's mobility will affect the ranks of the whole population. As decompositions of changes in mobility along the lines of race and gender is a key focus of our paper, we also show results for the intergenerational correlation (IGC), which is the same as the IGE but standardizes the log income of children and parents by the mean and standard deviation. The IGC thus measures a positional mobility concept, similar to the rank-rank correlation, while retaining the tractability of the IGE specification. Appendix D discusses the relationship between these three concepts as well as a specification that utilizes levels of income for both generations.[14]

3.2 Predicting parental income

IPUMS provides 1950-based occupational income scores, which calculate the median total income of people (pooling men and women) in each occupation in the 1950 Census.

[12] Discussions of the relative merits of different measures of mobility can be found in Fields and Ok (1999), Deutscher and Mazumder (2023), and Ray and Genicot (2023).

[13] Note that unlike the log specification, one cannot implement a TS2SLS procedure with ranked income. It is, of course, possible to construct predicted rank income for fathers in auxiliary data (i.e., estimate a first stage). However, computing the average rank for each cell implies compressing the rank distribution, so that in the second stage, the distribution of ranked parental income is no longer uniform and its variance affects the level of the rank-rank correlation.

[14] We show that TS2SLS and OLS imputation-based estimates are numerically equivalent with a levels-on-levels specification, and that one can transform the β^{levels} coefficient using the income distributions to approximate the IGE and rank-rank correlation.

These income scores have been used to approximate the income of individuals in earlier (or later) Censuses who have the same occupations. Our approach to constructing income predictions is similar in spirit to that of the IPUMS *occscore* variable, but we differ in four notable ways.

First, not all of our surveys have father's occupation categories that are as detailed as those in the Census. Across all of our surveys, we can harmonize occupations into 28 categories. We thus build and use crosswalks that map the occupations in our surveys into these 28 categories. These coarsened bins include broad occupations like doctors, clerical workers, craftsmen, and farm laborers, and the full list is in Appendix E.

Second, when constructing measures of predicted parental income, we limit the samples whenever possible to men between the ages of 30 and 50 who are living with a biological child younger than 18 years old (these men are almost always living with a wife as well). This sample restriction should better proxy household income of *fathers* with a given occupation, which is the population of interest when we try to predict income during the respondent's childhood.

Third, we calculate the average household income (summing across all working adults in the household) by father's occupation, race (Black versus white) and region (South versus elsewhere). We follow recent papers (Abramitzky *et al.*, 2021; Collins and Wanamaker, 2022; Ward, 2023; Saavedra and Twinam, 2020) that utilize characteristics beyond occupation to improve measures of predicted income. Given widespread discrimination and occupational segregation, using occupational scores computed from pooled Black and white populations will substantially mis-measure childhood incomes. Similarly, the South is far poorer than other regions during our sample period, so pooling across all regions throws out valuable information, especially for Black respondents who are vastly over-represented in the region. The choice of South versus elsewhere for the construction of predicted income is motivated by the fact that this level of detail is present in every survey. However, in Section 5.1, we check the robustness of the main results to using the four Census regions as predictors of income for the subsample of respondents for whom we have this level of information.

Finally, instead of only relying on the 1950 Census, we use multiple datasets spanning the 20th century to approximate parental income based on when the survey respondent was growing up. Specifically, we use income information from the 1901 Cost of Living Survey, the full-count sample of the 1940 Census, as well as the 1960–1990 Censuses (Ruggles *et al.*, 2021).[15] We combine our data sources so that families are

[15] Similar to Collins and Wanamaker (2022), we do not use the 1950 Census to construct measures of predicted income, as only sample-line respondents are asked about their income. For our purposes, this smaller sample size means that we are unable to calculate the average income for eleven *occupation* $\times$ *race* $\times$ *South* cells pertaining to Black fathers. Moreover, the sample-line restriction makes it impossible to

assigned measures of predicted income that come from the data sources closest in time to when the respondent is ten years old. That is, the 1910s–1920s cohorts are assigned measures of predicted income that are weighted averages of the 1901- and 1940-based predictions; the 1930s–1940s cohorts are assigned measures that are weighted averages of the 1940- and 1960-based predictions; and the 1950s–1970s birth cohorts are similarly assigned income predictions that are weighted averages of measures constructed using the 1960–1990 Censuses.

One feature of historical measurement of occupational incomes is that farm income is notoriously difficult to impute, as it is both highly volatile (being subject to weather and price shocks) as well as difficult to measure (as comprehensive measurement of agricultural costs is difficult to capture). More than other occupations, farmers have also declined in relative status over the first half of the 20th century; using data from Iowa, Feigenbaum (2018) shows farmer families have median household income in 1915, but are at the tenth percentile by 1950, so their status in one decade cannot proxy for their status earlier or later.

For our earliest cohorts, we follow the approach in Goldenweiser (1916) and Abramitzky *et al.* (2012) and use the 1900 Census of Agriculture to calculate farmers' net earnings. In our calculations, we allow for variation at the *race* $\times$ *South* level and take into account the share of each group that is not farm owners (i.e., part owners, or cash or share tenants). Moreover, because the 1940 Census income variable excludes income from self-employment, which includes most farmers, we supplement the 1940-based predictions with the Bureau of Labor Statistics' 1936 Expenditure Survey, which includes family income for farmers and the self-employed. To our knowledge, this data source is the earliest microdata to include total family income for these categories. More detail on this additional data source for farmers and the self-employed is available in Appendix E. One reassuring comparison is that in our data, white respondents born in 1910–1929 outside of the South to farmer fathers are estimated to be growing up around the 37–47$^{\text{th}}$ percentiles of the childhood income distribution, consistent with results in Feigenbaum (2018) for Iowa.

While we show robustness to many modifications of this prediction methodology in Section 5, the measure described in this section serves as our baseline approach for predicting childhood income, as we can calculate it for the respondents in all fifteen of our surveys. The third panel of Table 2 displays summary statistics related to predicted parental income.

calculate average *household* income.

3.3 Assessing the accuracy of predicted parental income

There are at least two challenges in predicting parental income using our methodology. First, adult children may not accurately recall their father's occupation. Second, even if recall is perfect, the way in which we assign parental income to survey respondents—based on occupation, race, and region cells in auxiliary datasets—may not be reliable for predicting income (or the predictive power of the cells may change differentially over time).

Appendix C provides greater detail on the accuracy of adult children's recall, but we summarize some key results here. First, we show that predicted childhood incomes calculated for male and female respondents are indistinguishable (as we would expect, given that there is no documented evidence of sex selection in the U.S. in our historical period and thus boys and girls on average grow up in the same families) and show no differential trends over time. This equivalence by sex across decades holds for the full sample, as well as for white and Black respondents separately (Appendix Figures C.1 and C.2). Second, Appendix Figure C.3 shows that in surveys, like the NLS, where multiple siblings from the same household are sampled, siblings' recall of their father's occupation is extremely highly correlated.

Third, we can perform a direct evaluation in modern data, as the PSID (beginning in 1997) asks household heads to recall their father's occupation and in many cases we directly observe the fathers of these respondents in earlier waves of the surveys (i.e., the 1960s and 1970s) when they are asked to report their own occupations. Over 80 percent of these household heads report an occupation that the father also reports and the most common mistakes are small and understandable (e.g., one party reporting "craftsmen" and the other reporting "operatives"). Indeed, the correlation between logged predicted income based on father's self-reported occupation and those based on the child's recall is 0.83 and the relationship is very linear across the entire support of father's predicted income (so we do not see, for example, that the children of the lowest-status dads tend to overstate their father's occupational status or that children of the highest-status dads tend to understate it; see Appendix Figure C.5). Further, the coefficient from a regression of the five-year average of log father's income on our income prediction (using the retrospective question) results in a coefficient very close to 1, suggesting that our retrospective measures of predicted income are quite closely correlated with father's actual permanent income. While these data pertain to birth cohorts more modern than our years of interest, it is nonetheless reassuring that recall appears highly reliable.

Finally, if our surveys of adult children are representative and their recall is accurate, then the fathers described by our respondents and the fathers in the Census when these respondents were growing up should be drawn from the same underlying population and thus appear similar on observables. Indeed, we show that the average (predicted) parental income as well as the types of occupations reported by our survey respondents are similar to the occupations of actual fathers in the Census (see Appendix Figure C.4 and Tables C.2 and C.3). These comparisons help alleviate concerns that children tend to inflate the status of their father when they are asked to recall their upbringing and that the implied distribution of parental income in the surveys will not correspond to the distribution of parental income in the same time period. Importantly, these exercises suggest that respondents' recall was not improving or deteriorating over time in a way that would drive the mobility trends we uncover.

As noted, even if recall is perfect, the predictions may be so noisy as to convey little information. Another concern, especially for earlier cohorts for whom we cannot use the Census to predict childhood income, is that the auxiliary datasets are not representative. As a check on these concerns, we show that our predicted childhood income tracks known trends in overall inequality over the 20th century. Appendix Figure A.1 shows the Gini coefficient as well as the top-10-to-bottom-50 ratio based on our predicted childhood income measure, separately by respondents' birth decades, as well as the analogous statistics of national income throughout this time period from the World Inequality Database (WID). Recall that comparisons of *levels* are not helpful, since by construction our measures will miss all household inequality arising from within *father occupation* $\times$ *race* $\times$ *South*-cell variation. But for both the Gini and the top-10-to-bottom-50 ratio, inequality measures based on our predicted childhood income and those based on the WID data track each other remarkably well. Panel (*c*) of this figure plots the Black-white income gap for the past 150 years (based on series compiled by Margo (2016)) and shows that our prediction of childhood income captures Black-white convergence at mid-century.

3.4 Comparing our approach to an ideal OLS coefficient

The evidence in the previous section reassures us that the fathers in the auxiliary datasets and the fathers of our survey respondents are drawn from the same or very similar underlying populations. However, even under this assumption, our imputation approach—which is akin to a two-sample two-stage least squares (TS2SLS) estimation—produces known biases relative to the target OLS coefficient. In Appendix D.1.1, we show that β_c^{TS2SLS} can be expressed as a function of the ideal β_c^{OLS} and two bias terms: a prediction-error term and an exclusion-restriction-violation term. The sign of this second bias is generally believed to be positive (Zimmerman, 1992), as missing factors that positively influence parental income conditional on the set of instruments are likely to also positively influence the adult child's income conditional on parental income.

As our main claim in the paper is that intergenerational persistence is *declining* between the 1910s and 1940s cohorts, the key concern with our approach is that these two biases may be changing across cohorts in a manner that produces a decline in our mobility measures whereas the true measure of mobility actually trends differently. In Section 5 and Appendix D.1.2, we present a variety of evidence against this concern.[16]

3.5 Comparison to past measures of parental income

Data limitations have long plagued the study of mobility in the United States, and our approach is no exception. We briefly review the main approaches in the literature, highlighting their advantages and disadvantages to better put our approach and results in context.

Papers using historical data. The Census provides identified data for those in the 1940 and earlier Censuses (and is in the process of releasing the 1950 Census). Recent papers have used linking algorithms to find the same individual across Censuses based on their name, year of birth and place of birth. This approach faces several challenges, the most important in our context being that, except for white men, linking rates are poor.[17] Most obviously, the linked sample is not representative by sex, as women during this period almost all changed their names upon marriage. To date, all published mobility papers using Census linking drop all women.[18] While in principle Black men are link-able, in practice match rates are very low for them. For example, an important contribution of Ward (2023) is the inclusion of Black men, but his linked sample is only two-percent Black before those observations are up-weighted. Similarly, Collins and Wanamaker (2022) are able to find reliable adult matches for three and five percent of Black children in the 1880 and 1900 Census, respectively. Moreover, Black Americans, and particularly Black men, are systematically under-counted in Censuses even before any linking is performed.[19] Even beyond gender and race, certain types

[16] To highlight just one example here, we show that for the 1940s and 1950s cohorts—when we can use the NLS and PSID to directly observe parental income, instead of having to predict it—the imputation approach and the ideal OLS approach move together in changes (and especially in the rank-rank specification, are very close in levels as well). While we cannot directly verify that the two move together in changes over the 1910s to 1940s cohorts, we are reassured that they covary in the period of overlap.

[17] See Ferrie (1996) for an important and early contribution to this literature. There is an active literature on the correct linking methodology and the preferred tolerance for rates of falsely matching and missing true matches (see, e.g., Abramitzky *et al.*, 2019; Bailey *et al.*, 2020). Matching methodologies are still in flux and best practices will likely evolve as machine-learning techniques improve.

[18] Recent papers studying historical intergenerational mobility have begun to include women via linking, including Althoff *et al.* (2023), Bailey and Lin (2022), Buckles *et al.* (2023), and Eriksson *et al.* (2023).

[19] O'Hare (2019) calculates that the net under-count rate for the Black population has gone from 8.4% in 1940 to 2.5% in 2010.

of names are very hard to link with precision (such as very common names like John Smith or long, foreign names that might have changed over time).

Our approach circumvents many of the challenges associated with linking. In most cases, the "link" to the father's occupation and other childhood characteristics are merely included as questions answered by the adult child respondent in the survey. Most of our surveys aim to be nationally representative (see Appendix Table E.1) so the percent of Black respondents in our (unweighted) data is very close to that in the full U.S. population, even for our earliest cohorts.

That said, there are important subgroups that may be missed even in our surveys. Given our focus on representativeness of the U.S. population, especially by race, the fact that incarcerated or otherwise institutionalized people are unlikely to complete the surveys in our sample may bias our estimates of intergenerational mobility. Appendix Figure A.2 shows the share of individuals ages 30–50 who are institutionalized (e.g., in correctional facilities or mental hospitals), separately by subgroup and cohort. The stark increase in the Black male incarceration rate for cohorts born since the 1960s is clear in the Census data. But there is little differential trend for Black male institutionalization for those born prior to 1960, which are the cohorts that are the focus of our study.

Beyond linking individuals across time, another challenge for historical work on mobility is the lack of individual or family income data until the 1940 Census. Most historical U.S. mobility research focuses on the occupational status of the father (as we do, though we adjust it along additional dimensions) and often the son as well. Relative to a single snapshot of parental or father's income, which is a very noisy proxy for average childhood income and thus leads to severe attenuation bias (Solon, 1992), a single snapshot of father's occupation may have the advantage of being more stable over time. But a single observation of a father's occupation has noise from two sources, as Ward (2023) recently highlights. First, fathers change occupations from year to year, especially when occupations are measured at the three-digit level that is often used in this literature. While this attenuation bias is likely smaller than that from year-to-year changes in family income, it could still be substantial. Ward (2023) shows that mobility estimates using father's occupation as observed in a single Census year substantially over-estimate mobility relative to those that use multiple observations across different Censuses. Second, Census-takers appear to record occupations with substantial error, at least in the historical period.[20]

[20] As Ward (2023) details, in a special case when a re-Census was required in St. Louis in 1880, one-third of occupations were reported differently only five months later, despite the reference date for the occupation being unchanged. Given the challenges of linking, researchers have turned to creative solutions. We noted in the introduction the synthetic-panel approached used by Olivetti and Paserman (2015) to study white

We do not observe fathers for just one (or two) Census snapshots, but rather observe them in the recollections of their adult children during their prime-age years. In that sense, we avoid the problem that Census researchers face of potentially observing the father in a particularly unrepresentative year in terms of his occupation. By contrast, it seems natural to assume that the adult child would remember the father's main occupation over her entire childhood, so the retrospective nature of our data likely aids in identifying the chief occupation of the father.

Papers using more modern data. The PSID and NLS datasets have many advantages for modeling intergenerational mobility (papers that use these data to estimate mobility for the 1950s through 1970s cohorts include Davis and Mazumder (2022), Mazumder (2015) and Bratberg *et al.* (2017)). First, they tend to have multiple observations of father or family income while the child is growing up, alleviating concerns about attenuation bias. Second, they have been fielded over decades, so the children can now be observed in their prime-age years, alleviating concerns about life-cycle bias. However, it is difficult for long panels such as these to avoid attrition, which typically results in non-representative samples as the most disadvantaged respondents prove harder to track over time and across generations.[21]

Chetty *et al.* (2014b) pioneered the use of administrative data, available since the 1990s, to study U.S. mobility. These data obviate the need for linking (the observations have identification numbers) and are much less susceptible to attrition and attenuation bias, as many years of income of both parents and children are available. Even with these administrative data, there are numerous challenges for mobility research. First, roughly seven percent of children cannot be linked to parents for various reasons (in our main sample, for the 1910s to 1940s cohorts, roughly twice that share of children are missing information for father's occupation, and we show robustness to various adjustments in the appendix). Second, to date, only the 1980s cohort can be studied (as they are young enough to have lived with their parents in the 1990s when IRS data becomes available and old enough to be observed today in prime earning years) and

men and married white women. To the extent that children stay in their parents' households as adults, then household surveys like the Census allow researchers to observe both child and parents *without* needing to link, an insight Card *et al.* (2018) and Hilger (2015) have used to study intergenerational mobility with respect to education. But this approach only works for periods in which most children have completed their education while living with their parents and of course does not provide a workable solution when the outcome of interest is the adult child's family income, as few children remain with their parents during their prime-age years.

[21] Schoeni and Wiemers (2015) show that the patterns of attrition by parent and child income result in biased estimates of intergenerational mobility. Indeed, as we show in Appendix Table A.2, individuals for whom we observe five or even ten years of childhood household income in the PSID have fathers who are more likely to be white and much more educated than the general population of fathers.

even for these individuals, early-childhood income is not observed.[22] Therefore, these data cannot track changes in mobility over decades.

Relative to these data sources, our approach allows us to reach further back in history (though not as far back as Census linking—as in Ward (2023), Song *et al.* (2020), Collins and Wanamaker (2022), or Olivetti and Paserman (2015)—because the types of surveys we use only become common in the 1940s, so will not capture 19th century cohorts at prime age). However, relative to IRS data, our sample sizes are orders of magnitude smaller, preventing us from breaking the data into neighborhoods or single percentiles as in Chetty *et al.* (2014a), Chetty and Hendren (2018a), and Chetty and Hendren (2018b).

4 Results for representative samples

4.1 Main results

The first series of Figure 1 shows the IGE for survey respondents over time, pooling across surveys and applying our baseline population-adjusted weights. We show the IGE separately by decade of birth and report the corresponding estimates in Appendix Table A.3. Between the 1910s and 1940s birth cohorts, the IGE falls markedly, from roughly 0.75 to 0.42. We then see an increase in this measure in subsequent birth cohorts, so that the IGE appears to take on a *u*-shape over time.

The second series shows the results from the rank-rank specification. As is typically found in other papers, our rank-rank coefficients are lower in magnitude than our IGEs: it begins the sample period around 0.37 and declines to a low of 0.25 for the 1940s birth cohorts. While the IGE and rank-rank track each other in changes very closely between the 1910s and 1940s cohorts, the IGE drifts upward for more modern cohorts while the rank-rank correlation stays relatively flat.[23]

For several reasons, we focus on the *decline* in the IGE and rank-rank measures that occurs from the 1910s to the mid-century birth cohorts, instead of the subsequent rise in the IGE or the stabilization of the rank-rank correlation thereafter. First, as we noted in our discussion of Table 2, the share of data with missing information about fathers increases over time, so levels and trends of mobility estimates toward the latter

[22] See Heckman *et al.* (2013), Uguccioni (2021), as well as cites therein for evidence that *early* childhood resources are especially important to later-life outcomes.

[23] Appendix Figure A.3 plots the estimates separately for each survey in order to give readers a sense of which surveys contribute to each decade's estimate and their relative magnitudes. Given the focus on representativeness, we exclude surveys whose respondents are only of one race or not representative of the 30–50 age group.

part of our sample period might reflect sample selection.[24] Second, beginning in the 1960s, data sources with income information for both generations (i.e., modern panel data such as the PSID and later on linked administrative IRS data) become increasingly available.[25] The availability of parental income data is particularly important in these more modern cohorts given rising residual wage inequality since the 1970s (Lemieux, 2006), which would likely increase the degree of bias in our estimates through incorrect predictions and omitted variable bias.

Figure 2 shows the decline in intergenerational persistence between the 1910–1919 cohorts and the 1940–1949 cohorts as bin-scatter figures. The first panel shows the change in the IGE relationship: a large shift rightward and upward (reflecting real income growth for both generations) as well as a significant flattening of the slope (because the upward shift is especially large among individuals growing up with less family income). The second panel of Figure 2 shows that the decline in the rank-rank is also large and precisely estimated. Given that by construction there can never be an overall increase in parents' or children's ranks (their average must always be 50) we see only a flattening of the slope.[26] Appendix Table A.4 quantifies the decline between the 1910s and 1940s birth cohorts, showing that the IGE (rank-rank correlation) falls roughly 0.007 (0.004) percentage points per year in the 1910s–1940s period.

A natural question that Figure 1 raises is why the IGE increases in more recent decades, while the rank-rank correlation stays relatively constant. The diverging paths of these measures can be explained by the fact that, holding the copula fixed, the IGE will rise with the ratio of children's to parents' inequality, whereas, by definition, the rank-rank will not. Specifically, using our baseline approach, the variance of log parental income declines over time, implying that the IGE will increase over time even if the covariance of log income across generations is relatively unchanged. In

[24] Appendix Figure A.4 plots the variance of logged (predicted) parental income in the baseline sample. To the extent that the lower variance in the 1950s–1970s cohorts partially stems from sample selection, then the IGE estimates for these later cohorts will be biased upward. Indeed, the robustness checks in Section 5.4 show that once we incorporate respondents who provide information about their mothers' occupations (when fathers' occupations are missing), the magnitudes of the IGE in this later time period are significantly reduced.

[25] Davis and Mazumder (2022) find an increase in persistence between the 1950s and 1960s cohorts (we find an increase in the IGE and a modest rise in the rank-rank correlation for the same cohorts). We view their findings as consistent with ours in terms of implying relatively high levels of mobility for cohorts born in the middle of the twentieth century.

[26] While caution is warranted in terms of comparing the levels of our rank-rank estimates (which use predicted parental income) to those from modern administrative data (which use actual income data averaged over several years from the parents), we use the modern estimates as rough benchmarks to assess the importance of the changes. The rank-rank slope we find for the 1910s–1920s cohorts is roughly equal to the modern US (see Chetty *et al.*, 2014a), whereas the slopes we find for mid-century cohorts are close to the modern estimates in Canada (Connolly *et al.*, 2019) and Denmark (Helsø, 2021a).

contrast, the variance of ranked parental income is fixed by construction, so changes in the rank-rank correlation will only reflect changes in the covariance of ranked income across generations. Indeed, the third series of Figure 1 illustrates that when we instead compare the rank-rank correlation to the intergenerational correlation—where IGC = IGE $\times \frac{\sigma_{y^p}}{\sigma_y}$ and σ_y and σ_{y^p} are the standard deviations of adult children's and parental logged income, respectively—the trends in the two measures coincide throughout the 20th century.

Overall, we believe the decline in intergenerational relative persistence from the 1910s to the 1940s cohorts is a novel finding, though there have been hints of it in past work. Using a dynamic *Occ.-Occ* mobility approach for white men (where status is fixed for all men within an occupation-decade but occupational status can change over time), Song *et al.* (2020) find a modest decline for those born around 1946–1955, which we also find as roughly the nadir of our IGE and rank-rank series. They write "we consider the deviation of the 1950 birth cohort best interpreted as suggestive." Similarly, including white and Black men, Ward (2023) finds that mobility is significantly lower in 1910 than in 1960 (though he does not have data for the intervening years), again consistent with our results for representative samples.

4.2 Comparison to occupational mobility

We adopted a *Self-reported inc.-Predicted inc.* approach to better include women and non-whites, but a natural question is how our results compare to the more traditional *Occ.-Occ.* measures. Note that we can only perform this comparison for men. In Appendix Figure A.5 we show that the standard *Occ.-Occ.* approach using the Census *occscore* variable shows only limited decline in intergenerational persistence (panel (a) includes all men and (b) just white men). Similarly, there is little decline when using an *Occ.-Predicted inc.* approach (second series). The third and fourth series show results for *Predicted inc.-Predicted inc.* and *Self-reported inc.-Predicted inc.*, and in both cases a large decline in intergenerational persistence appears. The differences between the later (third and fourth) and earlier (first and second) series suggest that an important part of the rise in male-only mobility comes from *within-occupation* upgrading of men with low-status fathers. As such, while the motivation of adjusting the *Occ.-Occ.* measure was in large part to include women, the adjustments also provide new insights for male-only mobility during this period and further show why the decline in persistence was harder to detect with more traditional, occupation-based mobility measures.

As an alternative way to see the difference between our approach and the more traditional occupation-based measures, we can study how our full-sample mobility estimates change as we transition from only using father's occupation to incorporating information about respondents' race and geography in the income prediction. (Note that we use reported family income as the dependent variable in this exercise in order to include women.) The first series of Appendix Figure A.6 shows the occupation-only estimates, confirming a decline in persistence between the 1910s and 1940s cohorts. Incorporating race and Southern residence into the prediction of childhood income accelerates the 1910–1940 decline in the IGE (statistically significantly, $p < 0.01$). Its effect on the rank-rank is also visually evident and in fact using only father's occupation to predict childhood income would have reduced the overall decline by roughly one-third.[27] This figure thus shows that using a richer set of predictors than merely father's occupation increases in a statistically significant manner (and for the rank-rank, economically significant) the estimated decline in intergenerational persistence from the 1910s to 1940s cohorts.

In summary, we have so far provided evidence of a significant decline in IGE and rank-rank persistence measures between the 1910s and 1940s birth cohorts. Importantly, these results reflect samples that are representative of the full U.S.-born population, including women and non-white respondents. In the next Section, we show robustness of this result to what we consider to be the most central concerns.

5 Robustness of the full-population result

We divide our robustness checks into three main concerns: measurement error of childhood predicted income; life-cycle bias; and econometric challenges related to the two-stage estimation. The final subsection summarizes robustness checks that do not fit into these main categories.

While more details on all of these results are provided in various appendices, Table 3 summarizes more succinctly how the main result—the decline in the IGE and rank-rank correlation between the 1910s and 1940s cohorts—holds up after changing methodological choices.

5.1 Measurement error in predicted childhood income

As noted earlier, a key challenge for our approach is measurement error in estimating the respondent's parental income during her childhood. This measurement error can

[27] In particular, as we add predictors of parental income, both the variance of logged parental income as well as the covariance of logged income across generations increase, so that the trends in the IGE remain relatively unchanged. By contrast, the covariance of ranked income across generations increases, so that the rank-rank correlation always increases as we include predictors beyond occupation. Appendix Table A.5 shows the R-squared from regressing logged income on the predictors. This table highlights that occupation is certainly an important predictor of income, but incorporating race, region, and education all improve the power of our measures to predict household income.

arise from several sources, which we address in turn below.

Recall bias. Section 3.2 already provided evidence that the recall of father's occupation appears reasonable—men and women give the same answers on average and the answers given match the occupational mix of actual fathers in the Census during the period the respondent grew up. We also performed a direct validation using the PSID, where we can observe the father reporting his own occupation and then decades later observe the adult child's recollection of that occupation. Appendix Figure B.1 shows that if the types and frequencies of recall errors made in the PSID were made in all of our other surveys, our main result of declining intergenerational persistence between the 1910s and 1940s cohorts would still hold.

Unobserved within-cell variance. Our baseline approach assigns each respondent a childhood income based on the mean family income in a *father occupation* $\times$ *race* $\times$ *South* cell from the appropriate Census or other auxiliary dataset, and it thus ignores within-cell variation. To the extent that some within-cell variation in a single Census year is merely transitory, excluding within-cell variation will better approximate permanent average childhood income. But to the extent that within-cell variation reflects systematic income differences missed by *father occupation* $\times$ *race* $\times$ *South*, our measure of predicted childhood income will bias us away—in an *a priori* unclear direction—from the persistence measure of interest.

We begin to address this concern by re-estimating predicted childhood income on a subset of our data that include more information on childhood background, namely father's education and detailed childhood region. *A priori*, father's education is one of the most likely factors to create systematic deviation from our *father occupation* $\times$ *race* $\times$ *South*-based mean family income. Indeed, adding information about father's education to our standard approach significantly increases predictive power (e.g., in 1960, the R-squared rises from 0.29 to 0.33; see Appendix Table A.5). Appendix Figures B.2 and B.4 show that when we improve our childhood income measures with important predictors, the trends in mobility remain unchanged, providing some reassurance that systematic, unobserved within *father occupation* $\times$ *race* $\times$ *South* cell variation in income is not driving our results.[28]

[28] Note that incorporating this information in the imputation implicitly tests the robustness of the two-sample approach, as different predictions will emit different prediction-error and exclusion-restriction-violation bias terms. In Appendix D, we also show that our main result of a 1910s–1940s decline in persistence is robust to *reducing* the number of variables used to impute parental income. As our estimated coefficient is a function of the true (unobserved) OLS target parameter and the two bias terms, the fact that we consistently find a decline from the 1910s to 1940s cohorts—despite the bias terms changing with each variation of the predictions—suggests that a decline in the true target parameter is driving the estimated

We now take a different approach to assessing the extent of potential bias due to unobserved within-cell variance. Essentially, we ask, even if we assume that *all* within-cell variance reflects true, permanent differences in childhood income, can we still detect a decline in intergenerational persistence between the 1910s and 1940s cohorts? For each *father occupation* × *race* × *South* cell, we observe the actual family income values of all observations in that cell (i.e., in microdata from the appropriate Census or 1936 Expenditure Survey). We thus re-estimate the IGE using both a multiple-imputation estimation (see, e.g., Little and Rubin, 2019; Rubin, 1987) as well as direct draws from the empirical distribution of all observed family income values (Appendix Figure B.5). We find that even when we make maximal assumptions—that *all* within-cell variation reflects permanent variation in childhood income—we find a decline in intergenerational persistence between the 1910s and 1940s cohorts.

Farmer income. Our baseline measure of parental income acknowledges the difficulty in estimating farmer (and self-employed) income in the first half of the 20th century using conventional survey or Census data. We therefore use the 1900 Census of Agriculture (for farmers) as well as the 1936 Expenditure Survey (for farmers and self-employed) given the limitations of the 1940 Census for these groups. In Appendix Figure B.6, we show that our main result is unchanged when imputing farmer and self-employed income using an alternative approach that follows Collins and Wanamaker (2022) and when dropping farmers from the sample.

5.2 Life-cycle bias

Various papers in this literature have noted that using current income to proxy for the adult children's lifetime earnings may bias estimates of mobility (see, e.g., Haider and Solon, 2006; Lee and Solon, 2009; Nybom and Stuhler, 2016). Recall we already restrict the sample to ages 30–50 to limit life-cycle effects. However, Appendix Figure B.7 shows the robustness of the main result to alternative specifications and sample restrictions that attempt to further minimize this life-cycle bias (e.g., including polynomials in adult children's age and restricting the sample to older respondents whose total family income may be better approximations of their lifetime earnings).

5.3 Robustness to econometric approach

Functional form. One concern with the empirical approach is that we rely on the log or rank transformations for estimating relative mobility. Following Dahl and

decline.

Lochner (2012) and Løken *et al.* (2012), Appendix Table D.1 instead considers levels of income for both the survey respondents and their parents. This table confirms the weakening relationship between income across generations, with the main decline occurring between the 1910s and 1940s cohorts.[29] In Appendix D, we also show that the coefficient from the levels-on-levels regression can be transformed using the first and second moments of the parent and child marginal income distributions to generate close approximations of the IGE and rank-rank measures, confirming that the non-linear transformations embedded in the latter two measures are not driving the rise in mobility (Appendix Figure D.7).

Connection to TS2SLS. As noted in Section 3.1, the baseline empirical approach is similar in spirit to a two-sample two-stage least squares (TS2SLS) approach. In Appendix D we implement the TS2SLS approach using the nearest source of microdata (i.e., the 1936 Expenditure Survey and the 1940–1980 Censuses) to predict parental logged income. Due to the lack of first-stage microdata for the 1910s cohorts, we cannot replicate the entire 1910s–1940s persistence decline in this exercise.

Panel (a) of Appendix Figure D.4 shows that when you refrain from using non-linear transformations and instead use *levels* of income for both generations, the TS2SLS estimator and the OLS estimator using imputed averages are numerically identical. The levels-on-levels specification exhibits a strong decline from the 1920s–1940s.[30]

In panel (b), we implement TS2SLS using the log-log functional form, which is *not* numerically identical to the OLS imputation approach. The former uses the most contemporaneous source of microdata to predict logged income in the parental generation, whereas the latter computes the average predicted income for each cell and *then* applies the log transformation.[31] The third series in this figure displays robust TS2SLS standard errors (Choi *et al.*, 2018; Pacini and Windmeijer, 2016). This panel high-

[29] Appendix Table D.2 estimates quadratic specifications using levels of income, finding that the slope of the relationship at the 25$^{\text{th}}$ percentile of the parental income distribution also flattens between the 1910s and 1940s cohorts. Appendix Tables D.3 and D.4 show analogous results for the IGE and rank-rank correlation, confirming a decline in persistence between the 1910s and 1940s birth cohorts for individuals throughout the (predicted) parental distribution (i.e., at the mean and at the 10th and 90th percentiles).

[30] In fact, the one difference between the levels-based specification and our baseline results is that for the former, the persistence decline continues through the 1950s birth cohort before plateauing and then reversing, whereas the nadir using our baseline methodology occurs for the 1940s cohort. The divergence between the levels, logs, and rank specifications in 1950 comes from different standardizations of income. We refer the reader to Appendix D for a full treatment.

[31] As noted in Section 3, an analogous approach for estimating the rank-rank correlation via TS2SLS is not desirable. Instead, we show an approximation of the rank-rank correlation based on the coefficient from a levels specification (i.e., based on the normal distribution, the rank-rank measure is $\beta^{\text{RR}} = \frac{6}{\pi}\arcsin(\frac{\beta^{\text{IGC}}}{2})$, in which β^{IGC} is the intergenerational correlation calculated from β^{levels}). We show that this approximation is quite close to the estimates in the main text.

lights that although the levels differ slightly—resulting from the different moment in which the log transformation is applied—the mobility trends are very consistent with our baseline results (a u-shape with a 1940s nadir) and in fact the TS2SLS approach displays a somewhat more marked 1920s–1940s decline.

In Appendix Figure D.1 we present results showing robustness to varying the set of parental income predictors. We show that the 1910–1940 decline in persistence holds using any subset of instruments. Further, any subset of instruments that includes occupation yields very similar results, and it is only when race and region are used without occupation—a case where the exclusion restriction is much more likely to be violated—that mobility estimates diverges from our baseline estimates.

5.4 Other robustness checks

We also conduct a variety of other exercises in Appendix B that check the robustness of the 1910s–1940s decline to other sampling and specification choices. Notably, we incorporate into the sample respondents whose fathers were present but not working (e.g., retired) as well as respondents who provided information about their mother's occupation. We also consider the sensitivity of the results to alternative weighting schemes, to including survey-year fixed effects, and to changes in household size (i.e., adjusting measures of income using reported household size).

As a final plausibility check on our main result, we examine a different outcome variable for the adult children: education. While the exact return to education varies over time, on average, more educated individuals have significantly higher earnings and family income. Thus, it would be somewhat surprising if the predictive power of parental income *on children's education* did not fall given that its predictive power over adult family income did. In Appendix Figure A.7 we estimate variants of equations (2) and (3) where we put the adult child's self-reported years of schooling as the outcome variable (available in all of our datasets). The figure shows that the relationship between father's predicted income and respondent's educational attainment declines sharply between the 1910s and 1950s birth cohorts.[32] Panel (b) uses the subset of twelve surveys for which we have father's education, and shows that the declining education-

[32] Using data from the modern period, Landersø and Heckman (2017) has questioned whether mobility is truly lower in places such as Scandinavia than in the US, because when education of the adult child is the outcome of interest instead of earnings or income, mobility measures in the US and Scandinavia look more similar. In our analysis, both family income and years of education appear to have a decreasing dependence on predicted childhood income over the first half of the 20th century. Appendix Figure A.8 illustrates these changes using bin-scatter figures, highlighting that this weakening relationship is largely driven by the rapid increase in respondents' high school completion in the bottom half of the income distribution, rather than the later rise in college completion.

on-predicted-income correlation is mirrored by a declining education-education correlation.[33] Recall that father's education is not used as a predictor of father's income, so the observed decline in education-education correlations over time is an independent check on our main IGE and rank-rank results showing declines in persistence over the first half of the century. Moreover, while we have tried to address concerns about using auxiliary data in a two-step process in Section 5.3, the education-education results are further reassuring because no first-stage prediction is required.

6 Decomposing the rise in mobility

In this section, we show how to decompose the overall IGE and rank-rank relationships into factors related to subgroups, building on Hertz (2008). We then use this decomposition to show how much changes in mobility or income among subgroups, particularly by race and gender, explain the overall rise of mobility over the first half of the 20th century.

6.1 Decomposing the IGE and rank-rank slopes

Consider any partition of the full sample, emitting subgroups $g \in G$ with subgroup g's share of the total sample given by p_g. Further, let β_g^{IGE} be equal to β from estimating equation (2) on subgroup g.

From the OLS formula and the law of total covariance, the whole-population IGE is given by:

$$\beta^{\text{IGE}} = \underbrace{\sum_{g \in G} p_g \frac{\text{Var}(y^p \mid g)}{\text{Var}(y^p)} \beta_g^{\text{IGE}}}_{\text{Weighted average of subgroup slopes}} + \underbrace{\frac{\text{Cov}(\text{E}[y \mid g], \text{E}[y^p \mid g])}{\text{Var}(y^p)}}_{\text{Between-group covariance of subgroup averages}} . \tag{4}$$

The formula remains unchanged for β^{IGC} save for setting $\text{Var}(y^p) = 1$, if the y and y^p are considered to be standardized (de-meaned and divided by standard deviation) versions of logged income. A slight modification gives a similar (and more novel) expression for the whole-population rank-rank slope γ^{RR}, equal to γ from equation (3). Assuming that both the parental and adult children's ranked incomes have a uniform distribution, the same application of the law of total covariance gives:

[33] These patterns mirror the rise and subsequent decline in relative educational mobility documented in Hilger (2015), which restricts attention to adult children living with their parents at the time of the Census.

$$
\begin{aligned}
\gamma^{\mathrm{RR}} =& \sum_{g\in G} p_g \frac{\mathrm{Var}(\mathrm{Rank}^p \mid g)}{\mathrm{Var}(\mathrm{Rank}^p)} \gamma_g^{\mathrm{RR}} + \frac{\mathrm{Cov}(\mathrm{E}[\mathrm{Rank} \mid g], \mathrm{E}[\mathrm{Rank}^p \mid g])}{\mathrm{Var}(\mathrm{Rank}^p)} \\
=& 12 \times \Big(\sum_g p_g \mathrm{Var}(\mathrm{Rank}^p \mid g) \gamma_g^{\mathrm{RR}} + \sum_g p_g \mathrm{E}[\mathrm{Rank}^p \mid g] \mathrm{E}[\mathrm{Rank} \mid g] - 0.25 \Big)
\end{aligned} \tag{5}
$$

To ease intuition and to focus on one of the key applications for our paper, we rewrite the IGE decomposition for two groups: namely, white W and Black B respondents.

$$
\begin{aligned}
\beta^{\mathrm{IGE}} =& p_W \frac{\mathrm{Var}(y^p \mid W)}{\mathrm{Var}(y^p)} \beta_W^{\mathrm{IGE}} + (1 - p_W) \frac{\mathrm{Var}(y^p \mid B)}{\mathrm{Var}(y^p)} \beta_B^{\mathrm{IGE}} \\
&+ \frac{p_W \mathrm{E}[y^p \mid W] \cdot \mathrm{E}[y \mid W] + (1 - p_W) \mathrm{E}[y^p \mid B] \cdot \mathrm{E}[y \mid B] - \mathrm{E}[y^p]\mathrm{E}[y]}{\mathrm{Var}(y^p)}.
\end{aligned} \tag{6}
$$

The decomposition helps clarify two points. First, because population shares act as weights in the first two terms of equation (6), changes in the within-group IGE of the large majority group, β_W^{IGE}, will, perhaps not surprisingly, affect the full-population IGE. By the same logic, while the Black-only slope, β_B^{IGE}, may be of interest in other applications, it will not play a large role in determining the overall slope.

Second, and less obviously, the decomposition highlights the important role of between-group differences in parental income y^p in determining the full-population IGE. To see this point, assume for the moment that W and B are two distinct subgroups, but are drawn independently *from the same distribution* of parental income y^p. In this special case of no between-group differences in parental income, $\beta^{\mathrm{IGE}} = p_W \beta_W^{\mathrm{IGE}} + (1 - p_W) \beta_B^{\mathrm{IGE}}$, or in other words, the full-population IGE is the average of the two subgroup IGE slopes weighted by the subgroup share of the total population. This result holds regardless of the adult childhood outcomes (e.g., even if the mean adult income y of group B is well below that of group W).[34] However, if there exist large differences in parental income between the two groups (as there are for Black and white Americans), then this third term will be heavily weighted and will play a key role in determining the full-population IGE.[35]

[34] In the less-extreme case in which the two groups have the same average parental income but different variances, then the third-term still cancels out, and the full-population IGE is a weighted average of the subgroup IGEs, where the weights are a function of population shares and (conditional and unconditional) variances of parental income.

[35] This decomposition thus highlights that income changes in the parental generation will affect the subsequent generation's level of mobility, a point explored in greater detail in Nybom and Stuhler (2023).

6.2 Decomposing mobility by race and gender

In this subsection, we show visually the mappings of (predicted) parental income to adult children's incomes, separately by the race and gender of the respondent, and how these mappings change over time. As we will split our data into small $race \times sex$ subgroups in the following analyses, to gain power and precision we will typically compare the "low mobility" 1910s–1920s birth cohorts to the "high mobility" 1940s–1950s cohorts.

Decomposing mobility by race. Given the discussion in Section 6.1, we expect that the between-group component will prove important for a decomposition along racial subgroups, given that Black Americans grow up with far less parental income than whites in our period (as well as today). For this reason, we show results as binscatter graphs, as it is important to examine the means of childhood and adult income by group and over time.

Figure 3 shows Black and white mobility for the earlier, less-mobile cohorts compared to the later, more-mobile cohorts (IGE in panel (a) and rank-rank correlation in (b)). Perhaps the most striking aspect of the graph is how little overlap there is in the support of the Black versus white distributions: Black and white childhood income overlaps only modestly, especially in the early period. In the rank-rank figure, almost no white respondents grow up in the bottom ten percent of predicted childhood income and few Black respondents grow up above the 30$^{\text{th}}$ percentile, so the overlap of the two groups mostly occurs over an interval of approximately twenty percentiles.[36]

Another notable result is the significant progress Black respondents make relative to their white counterparts in both the parents' and children's generations. In the IGE graph, both the Black and white regression lines shift rightward, denoting substantial average real income growth during these respondents' childhood, but more so for Black Americans. The rank-rank graph cannot capture average real income growth given its zero-sum nature, so the overall support is fixed between zero and 100. The catch-up of Black adult income here is striking. A Black child in the earlier cohorts growing up at the 15$^{\text{th}}$ percentile (which we choose as a point of maximal overlap between Black and white children) would be predicted to have an adult family income at the 28.5$^{\text{th}}$

[36] One feature of our "small data" is that the vast differences between how Black and white children grow up is readily apparent in the support of these figures: with full-population administrative data one can capture the tiny number of Black children who grew up in rich families and thus extend the regression lines over the entire 0–100 domain of parental income rank. But even today prime-age Black adults are vastly under-represented in the upper parts of the parental income distribution while growing up. The tiny share of Black children in the upper ranks of parental income distribution even in modern data can be seen in the appendix figures of Chetty *et al.* (2020).

percentile, compared to the 41.9$^{\text{th}}$ percentile for a similarly situated white child. But for mid-century cohorts, Black children born at the 15$^{\text{th}}$ percentile are predicted to appear at the 34.5$^{\text{th}}$ percentile as adults compared to 43.7$^{\text{th}}$ for whites (closing the gap with their white counterparts from around 13.4 to 9.2 percentile ranks).

While we have so far focused on Black-white convergence, the regression lines depicting white-only mobility also change over this period. In both the IGE and rank-rank estimates, the slopes flatten significantly. The rank-rank slope falls from 0.27 to 0.20. As the large majority group, the flattening of the mobility slope among white individuals will have an important effect on the overall full-cohort IGE and rank-rank estimates (while the Black-only slope also flattens over time in both graphs, given that this component is weighted by a small population share, the effect on overall mobility will be very small).

By gender. A major motivation for our family-income-to-predicted-childhood-income mobility concept is that it enables us to perform intergenerational-mobility estimation including women. The decomposition in Section 6.1 suggests that the key elements of a decomposition of mobility by gender will differ from that by race. Because women and men grow up on average in the same households in the US, the between-group component of equation (6) should be close to zero and thus the full-population IGE is well approximated by the simple mean of the within-gender IGE slopes (as each sex is roughly half of the population). Put differently, the male-only IGE will be a biased measure of the full-population IGE only if the female *slope* is significantly different than the male slope, and differences in adult-income means between the two groups will not matter.

Of course, *a priori*, there is no reason to assume that the mobility slopes of men and women will coincide. For example, marriage patterns could differ by parental income and they will tend to matter more for women's family income than for men's, especially in the historical period when most married women did not work.

Figure 4 (as well as Tables A.6 and A.7) compares male and female slopes over time, instead of using bin-scatter graphs, as between-group mean childhood income differences are trivial. For both measures and for all birth decades, persistence measures for women are greater than or equal to those for men. The male-female gap appears to be relatively stable over time, especially for the 1910s–1940s cohorts.[37]

[37] As noted in Section 2, some of our datasets include only women (e.g., the National Longitudinal Surveys of Mature or Younger Women) or only men (the Occupational Changes in a Generation datasets), so a possible concern is that the differences in mobility by sex are an artifact of using different datasets. In Appendix Figure A.9 we show robustness to restricting the baseline sample to datasets that include both men and women (roughly 47% of the baseline sample).

Why do women's adult family income depend more on their parents' income than is the case for men? To answer this question, we turn again to differences by race.

By race *and* gender. We now consider differences by race separately for men and for women. In particular, Figures 5 and 6 further break down the by-race results in Figure 3 by gender. Figure 5 shows that among men, Black Americans closed much of the mobility gap with whites by mid-century (of course, as the supports of the regression lines make clear, Black men still grew up in far poorer households, so their average adult income in either logs or ranks is still much lower than that of whites). By mid-century, there is considerable overlap in adult outcomes between Black and white men born to similarly advantaged parents. For example, in the more mobile mid-century cohorts, Black men born at the 15^{th} percentile are predicted to appear at the 37.6^{th} percentile as adults, just slightly below their white counterparts at the 43.6^{th} percentile. This 6.0 percentile point gap is 10.4 points in the earlier cohorts, with Black men born at the 15^{th} percentile predicted to appear at the 32.4^{th} percentile as adults, compared to the 42.8^{th} percentile for their white counterparts.

Figure 6 paints a different picture for women. First, comparing Figures 5 and 6, it is clear that Black adult women are simply poorer than their male counterparts. Their entire regression line is below that of Black men. In the early cohorts, a Black woman born at the 15^{th} percentile is predicted to barely climb upward at all (an expected adult family-income percentile rank of 25.2, compared to 41.0 for a similarly situated white woman). While Black women make progress over time, even at mid-century the corresponding prediction is only the 31.9^{th} percentile (compared to 43.7 for white women).

Thus, for mid-century cohorts, while the racial mobility gap at the 15^{th} percentile for men is down to 6.0 percentiles (from 10.4) it remains at 11.8 (down from 15.8) for women. While Black women make considerable *progress* over time, given their low starting point, even in the most mobile mid-century period, a Black girl is predicted to grow up to be significantly poorer than any other group born to similar circumstances.[38]

By contrast, white boys and girls both grow up with the same childhood income, but, conditional on their place in the childhood income distribution, they also enjoy similar family income as adults. Indeed, comparing Figures 5 and 6 shows that the white-only mobility slopes are nearly identical for men and women. For the rank-rank correlation, the male and female slopes are both 0.27 in the early period and 0.20 in

[38] Note that the lack of gender gaps by family income among white respondents and the large gaps (favoring men) among Black respondents is apparent in the basic summary statistics shown in Table A.1, both in our surveys and in the Census.

the later period. That white men and women's family incomes were equally tied to the status of their fathers in an era when most married white women did not work suggests that they were marrying individuals very similar in earnings to their brothers.

In summary, the higher IGE and rank-rank persistence measures for women relative to men in Figure 4 are not driven by white individuals. Instead, the fact that Black women do poorly relative to Black men in adulthood pulls down the overall female mobility regression line for the lowest percentiles of parental income and results in a steeper slope for full-population female mobility relative to male mobility throughout much of the 20th century.

6.3 Decomposing the *decline* in intergenerational persistence

As already discussed, the full-population persistence slope is approximately equal to the (simple) mean of the male-only and female-only slopes. Because the gap between those two slopes is quite stable between the 1910 and 1940 cohorts (shown in Figure 4), a decomposition by sex is unlikely to help us explain the *decline* in full-population persistence over this period. So we consider the decomposition by race instead.

Returning to Figure 3 with the decomposition in mind allows us to assess the effects of the various movements in the by-race IGE and rank-rank mappings. Figure 3 depicts a number of different changes over time, some of which will increase mobility (the income growth for Black respondents, the flattening slope for the white majority), some of which will reduce mobility (the income growth for whites), and some of which should have minimal effect (the flattening of the Black-only slope). The decomposition can quantify the various contributions.

We begin by considering the role of Black-white income convergence over the first half of the twentieth century for the increase in the IGE. Figure 7 shows (second set of bars) that if Black individuals had instead experienced the same real income growth as white individuals during adulthood (without changing the slopes for either group or the averages or variances of parental incomes), then 57% of the IGE decline would not have been realized. Thus, Black respondents' catch-up to whites in income levels over this period explains a large share of the total decline in persistence, despite Black Americans only being a small share of the population.

The flattening of the white slope also plays a major role in the decline of the IGE—had it retained its 1910s level and otherwise allowing all other factors to move as they actually did between the 1910s and 1940s cohorts—then the IGE would have only fallen five points instead of fourteen (0.58 vs. 0.49). This result also emphasizes the importance of the Black-white income convergence: even though white individuals experienced no increase in mobility, the convergence in average income across races still yields more than a third of the decline in persistence.

We repeat this analysis for the rank-rank correlation as well as the intergenerational correlation. Separating the decline of the rank-rank measures into within- and between-group components is slightly complicated by the fact that changing either the slope of white respondents' income or altering Black mean income to grow at the same, lower rate as white income will mechanically change the ranked income of individuals in the other group. Hence, for the rank-rank measure we account for the effects of the whole distribution by first conducting the IGE counterfactual, re-ranking adult children in the counterfactual distribution, and then estimating γ^{RR} in the counterfactual late period. For the IGC, we compute β^{IGE} and multiply it by $\frac{\sigma_{y^p}}{\sigma_y}$ where σ_y and σ_{y^p} are the standard deviations of counterfactual adult children's and actual parental logged income, respectively. Results for the IGC and the rank-rank are similar both qualitatively and quantitatively. As with the IGE, both Black-white catch-up and the flattening of the white-only slope each explain a sizable portion of the decline.

In summary, while the exact shares are sensitive to the use of the IGE, the IGC, or the rank-rank, in all cases we find that Black-white income convergence and the flattening of the white-only slope are the key changes that drove the decline in full-population intergenerational persistence in the first half of the 20th century.

6.4 Convergence in racial income gaps using Census data

The analysis above suggests that convergence in white-Black means—the third term of the decomposition—is a major factor in the decline in overall intergenerational persistence. One implication is that we can calculate this component of mobility *without access to data that links adult children to their parents.* Appendix Figure A.12 thus plots the third term (i.e., the between-group term) of the IGE and rank-rank decomposition using (unlinked) Census data as an additional robustness check for the full-population mobility decline from Section 4. The same *u*-shape appears when considering logged income, as in the IGE in Figure 1. Similarly, the *L*-shape of the rank-rank correlation also emerges when using ranked income. Thus, readers who remain skeptical of adult children's recall or have other concerns about measurement error in childhood income can observe that the (large) component of mobility estimates that do *not* rely on linking can be replicated using completely different data than our 15 surveys. Note that Margo (2016) and others have already documented much of this Black-white convergence, though it was not organized by birth cohorts, did not focus on fathers (and thus did not have a direct intergenerational link), and was not parameterized in the same manner so as to directly relate it to mobility decompositions.

6.5 Comparing representative versus subgroup mobility estimates

While we have shown which components of the decomposition play the largest roles in affecting both the levels and changes of full-population persistence measures, a separate question is how biased sub-group (e.g., white men) estimates are relative to representative estimates. If we had performed our family-income-to-childhood-income mobility estimation on, say, only white men, how biased (in levels and changes) would these estimates be relative to a representative sample?

In Figure 8, we show how the mobility estimates change as we sequentially add various subgroups (as usual, the IGE is depicted in panel (a) and the rank-rank in panel (b)). We begin with white men (first series), the group most often studied in the existing mobility literature. In some decades, adding white women (second series) increases estimated persistence and in other decades it reduces it, but in all cases confidence intervals overlap.

We then add Black respondents, first men (third series) and then women (fourth series). Both additions increase the estimated persistence measures, as we would expect from the evidence already presented. And, again as expected, the change tends to be larger once we add Black women. As they are born to families at the bottom of the distribution (like their male counterparts) and tend to remain poor as adults (more so than their male counterparts) excluding this group significantly biases downward measures of intergenerational persistence, despite being just over five percent of the population.[39]

In terms of the actual effects of using representative samples versus only white men on various point estimates, consider the 1920s cohort as an example. The white-male rank-rank slope is 0.25 and does not change after adding white women. Adding Black men—just over five percent of the population—increases it an additional three percentage points to 0.28 and adding the similarly small group of Black women increases it to 0.31. Similarly, the IGE in for this cohort rises from 0.42 for white men to 0.59 for the representative population. Excluding Black men and especially Black women paints an overly *optimistic* picture about the *level* of intergenerational mobility in the first half of the 20th century.

Considering a representative population instead of only white men also changes our view of the *evolution* of mobility over this period. For white men, the IGE falls roughly 0.003 percentage-points per year from 1910–1940 (Appendix Table A.8). For the full population, it falls considerably faster over this period—0.007 points per year—and we can reject equality of these two rates at the 5-percent significance level. The analogous

[39] Appendix Figure D.6 shows that these patterns remain in the TS2SLS levels-based estimates.

rates are 0.003 and 0.004 for the rank-rank and equality can also be rejected at the 5-percent level. In summary, including only white men misses a substantial part of the decline in the slope and thus paints an overly *pessimistic* picture of the *rise* in intergenerational mobility over this same period.

7 Discussion and conclusion

We provide, to the best of our knowledge, the first evidence on long-run intergenerational relative mobility trends for representative samples of the U.S.-born population. We find a robust decline in IGE and rank-rank persistence measures from the 1910s to the 1940s birth cohorts. Previous studies that have examined historical mobility have overwhelmingly focused on white men, which both overstates mobility relative to the full population—a point also made by Ward (2023) in the context of male-only mobility—but at the same time understates the *rise* in mobility from the 1910s to the 1940s. Including only white men misses out on the important progress Black Americans, particularly Black women, make relative to whites, which has large implications for full-population mobility given the extreme disadvantage of Black children over our sample period. In short, the United States starts the 20th century much further from the "American Dream" ideal of a mobile society but also improves more significantly when the full population is considered rather than only white men.

While we avoid comparing our 1910s–1970s survey data with the 1980s IRS data *in levels* (given that the latter data source has income information for both generations), we compare the relative positions of the four groups. Figure 9 considers individuals growing up at the 25^{th} percentile of the income distribution, separately by race and sex for each birth cohort in our data. We include an additional data point from Chetty *et al.* (2020) labeled 1980s (though technically these individuals are born between 1978 and 1983). The results from the 1910s–1970s reflect findings we have already presented. For example, Black women are the poorest as adults but also show the most dramatic progress of any group. Similarly, Black-white convergence appears to peak around the 1940s (a brief moment where a Black and white boy born at the 25^{th} percentile would be predicted to end up at a similar family income rank as adults). In the 1960s and 1970s, Black-white adult income gaps for those born at the 25^{th} percentile regain much of their earlier magnitude. The 1980s data shows that Black women have continued their progress, in this case *overtaking* Black men (there is gender reversal among whites as well, but much less dramatic both because the 1910–1970s differences were always close to zero and because the female advantage in the 1980s is small). But, overall, Black-white adult income gaps for those born at the 25^{th} percentile continue to grow, continuing the trend we saw in the 1960s and 1970s in our data.

As Black-white convergence helped drive the rise in mobility over the first half of the 20th century, it is natural to ask how important racial income gaps are today in shaping overall U.S. intergenerational mobility. In particular, if modern-day *between-racial-group* income gaps remain unchanged, how much would *within-racial-group* IGEs have to fall for the overall U.S. IGE to reach 0.20 (roughly that in Denmark; see Helsø, 2021b)? Using the decomposition in Section 6 and statistics from Chetty *et al.* (2020) on contemporary income distributions, we find that within-group IGEs would have to fall below 0.05, an implausibly high level of mobility.[40] Put differently, U.S. intergenerational persistence faces a high lower bound unless major income convergence across racial groups occurs.

The comparison to modern data also suggest at least two areas for further work, both related to racial gaps given their centrality to overall mobility levels. First is that declining marriage rates and diverging outcomes by gender interact to produce changing patterns of mobility. The lower marriage rates of Black Americans relative to whites throughout our sample period (see Table A.1) and continuing today permit large mobility gaps between men and women (as they are not married to each other and thus do not mechanically share a family income). In addition to studying the implications of declining marriage rates for intergenerational mobility, future work might also examine the rise of *interracial marriage*—while rare during our sample period, today 18 percent of recently married Black Americans have a spouse of a different race.[41]

Second, any candidate explanation for the reversal of Black progress in closing the mobility gap with whites would need to have a large gender-specific component, given the relative progress Black women have made. Mass incarceration, a phenomenon that largely post-dates our historical cohorts but has important implications for modern cohorts of Black men, and deindustrialization, which impacted Black men earlier than whites (see Wilson, 1997), are two natural candidates.

We close with some final thoughts on what our paper suggests about the persistence of advantage across generations. On the one hand, the decline in intergenerational persistence over the first half of the 20th century we document challenges scholarship that has concluded that intergenerational mobility remains relatively stable even in the face of large political and structural changes (see, e.g., Ager *et al.*, 2019; Alesina *et al.*,

[40] To simplify this calculation, we assume that all racial groups would have the same within-group IGE. We use summary statistics from Chetty *et al.* (2020) to approximate modern within-racial-group income distributions. We find similar results using the rank-rank correlation and find that the between-group term accounts for over 25% of the overall rank-rank coefficient. For more details on these calculations, see Appendix Section E.8.

[41] See https://www.pewresearch.org/social-trends/2017/05/18/1-trends-and-patterns-in-intermarriage.

2020; Clark, 2015, 2023; Erikson and Goldthorpe, 2002; Olivetti and Paserman, 2015; Song *et al.*, 2020).[42] On the other hand, the return of early twentieth-century race-specific mobility gaps is cause for pessimism (perhaps suggesting that the mid-century convergence we document was a mere aberration).

Overall, we view the twentieth-century patterns as providing evidence that policy and institutions can increase U.S. intergenerational mobility. The birth cohorts in our paper span the mechanization and declining importance of American agriculture, the high school movement, two World Wars, the Great Depression, the New Deal, the "Great Compression," and the Civil Rights movement. Even the modern return of the race-specific mobility gaps present evidence of dynamism—Black women reversed a large gender gap that existed for at least seven decades. These documented changes across time suggest that mobility patterns are not set in stone and we hope will inspire future research to better understand the underlying institutional and policy determinants of intergenerational transmission of advantage.

[42] In contrast, recent studies of Scandinavian countries have also documented periods of rising mobility in the twentieth century following nationwide educational reforms (see, e.g., Karlson and Landersø, 2021; Nybom and Stuhler, 2023; Pekkarinen *et al.*, 2017).

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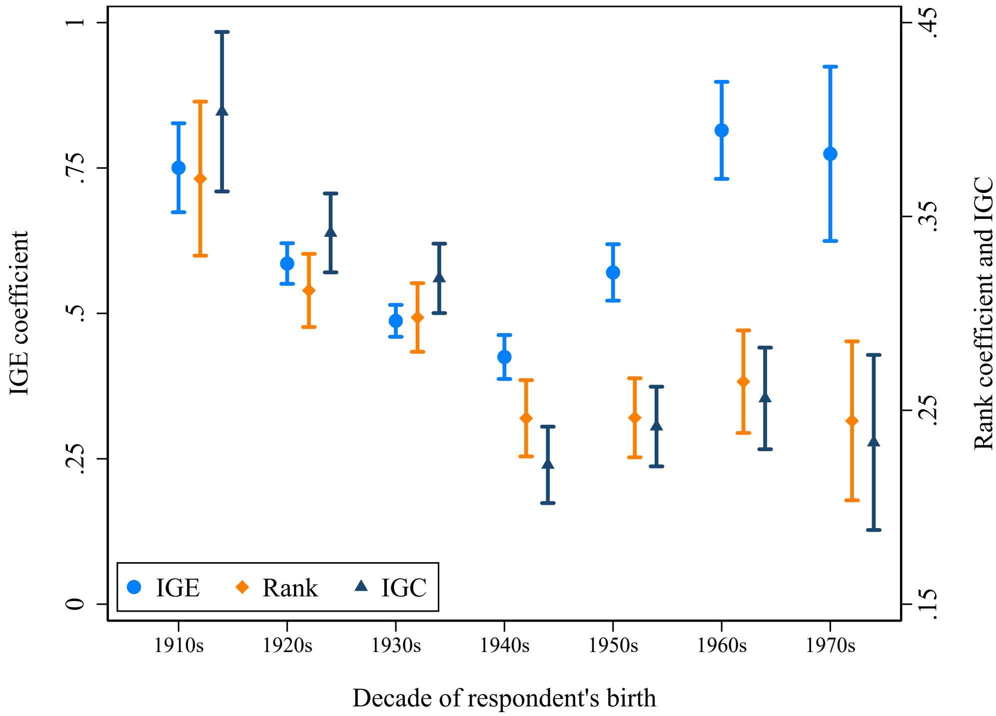
Figure 1: IGE and rank-rank measures by birth decade

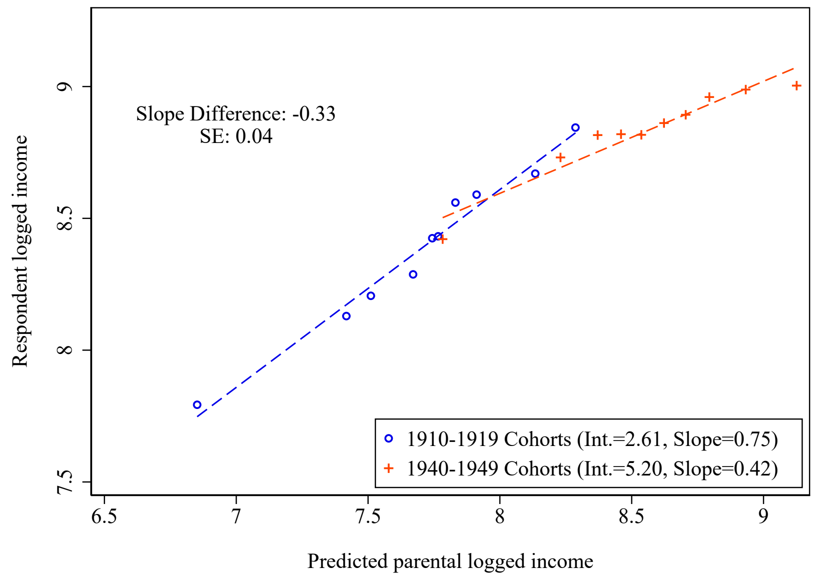


Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

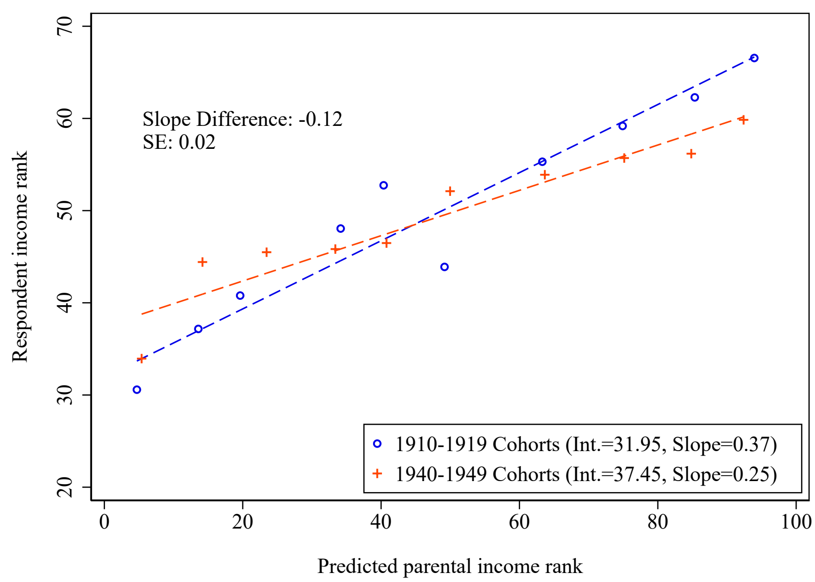
Notes: The IGE and rank-rank estimates are based on the baseline sample of respondents ages 30–50 using equations (2) and (3). The intergenerational correlation is equal to IGE $\times \frac{\sigma_{y^p}}{\sigma_y}$ and σ_y and σ_{y^p} are the standard deviations of adult children's and parental logged income, respectively. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* $\times$ *sex* shares.

Figure 2: Bin-scatter depictions of the decline in intergenerational persistence

(a) Intergenerational elasticities



(b) Rank-rank relationships

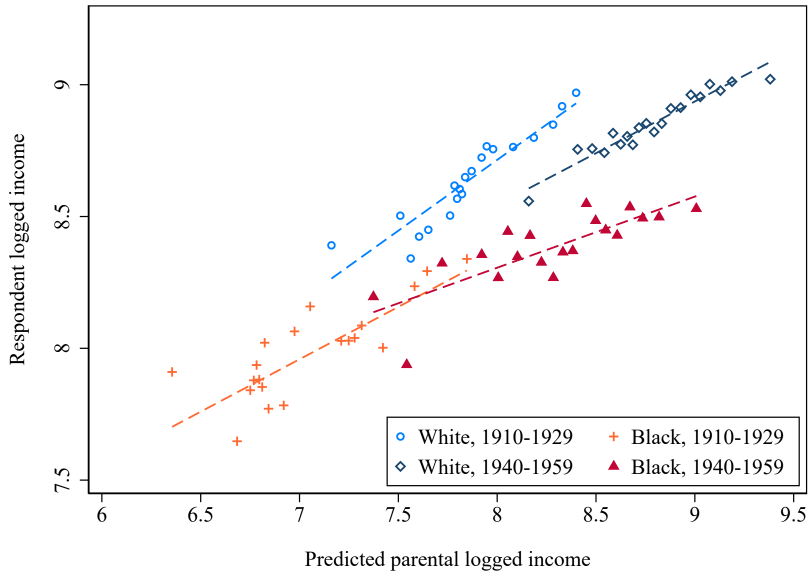


Sources: Data come from 15 different surveys, described in Section 2 and in further detail in Appendix E.

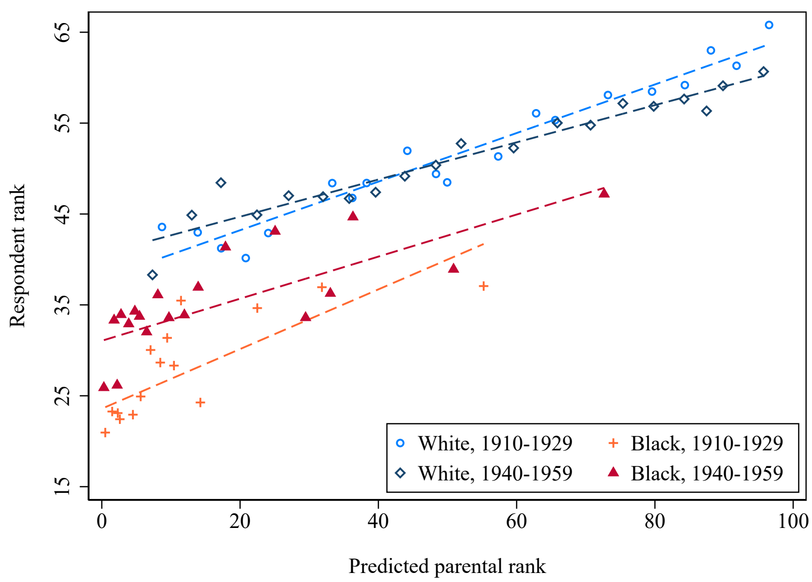
Notes: The IGE and rank-rank are based on the baseline sample of respondents ages 30–50. The estimated slope difference and its standard error come from regressions similar to equations (2) and (3), but which allow the slope and intercept to differ by cohort. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares.

Figure 3: Mobility by race, 1910s–1920s versus 1940s–1950s

(a) Intergenerational elasticities



(b) Rank-rank relationships



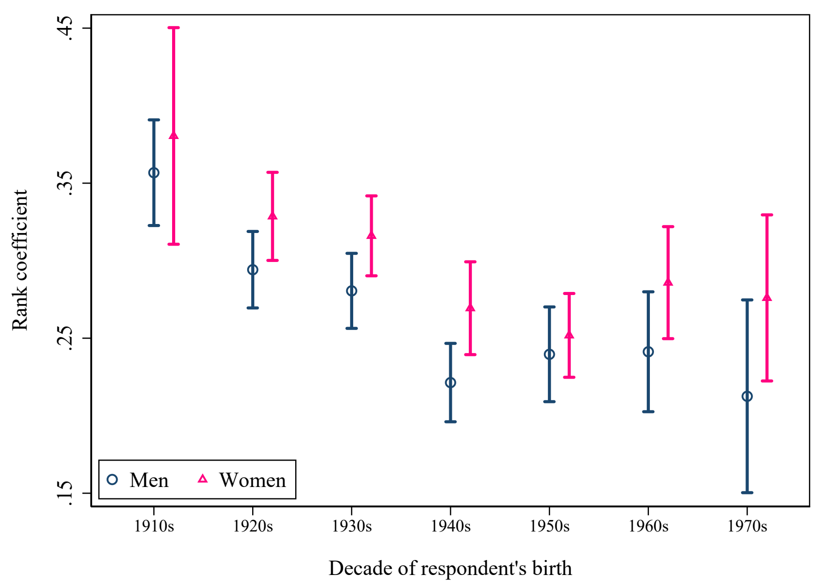
Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

Notes: The IGE and rank-rank are based on the baseline sample of respondents ages 30–50. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares. To account for pooling of cohorts, specifications include birth decade fixed effects.

Figure 4: IGE and rank-rank measures by birth decade, by sex

(a) Intergenerational elasticity

(b) Rank-rank coefficient

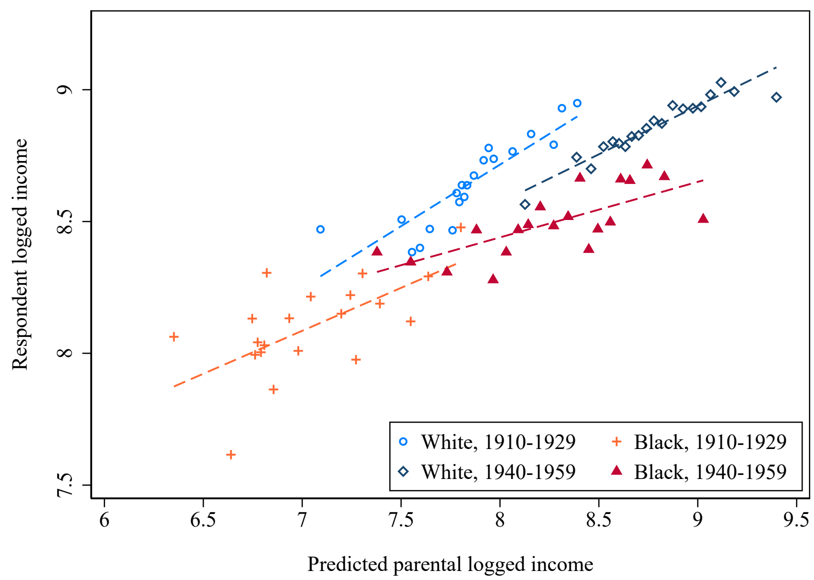


Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

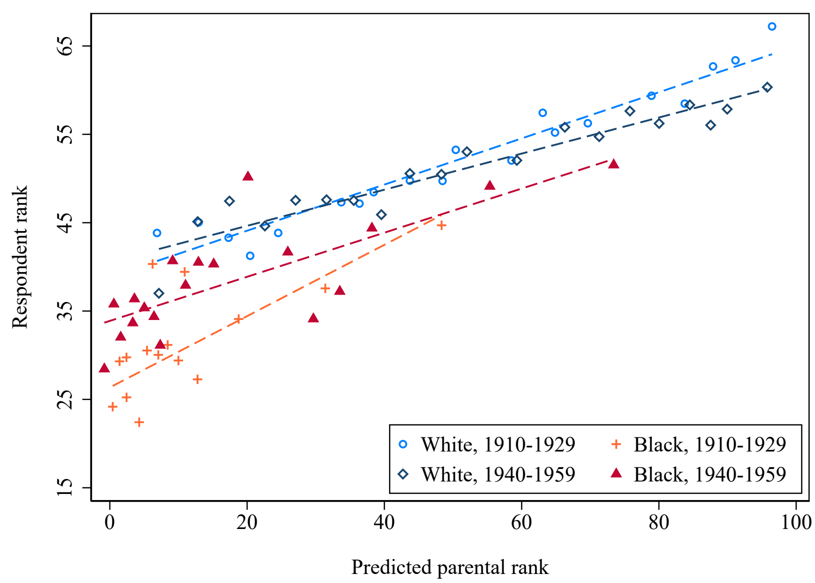
Notes: The IGE and rank-rank are based on the baseline sample of respondents ages 30–50. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares.

Figure 5: Mobility by race for men, 1910s–1920s versus 1940s-1950s

(a) Intergenerational elasticities



(b) Rank-rank relationships

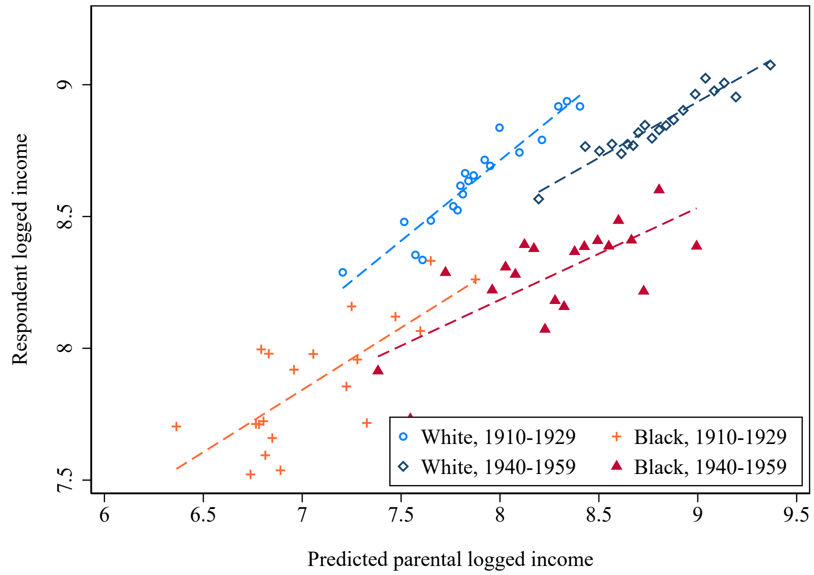


Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

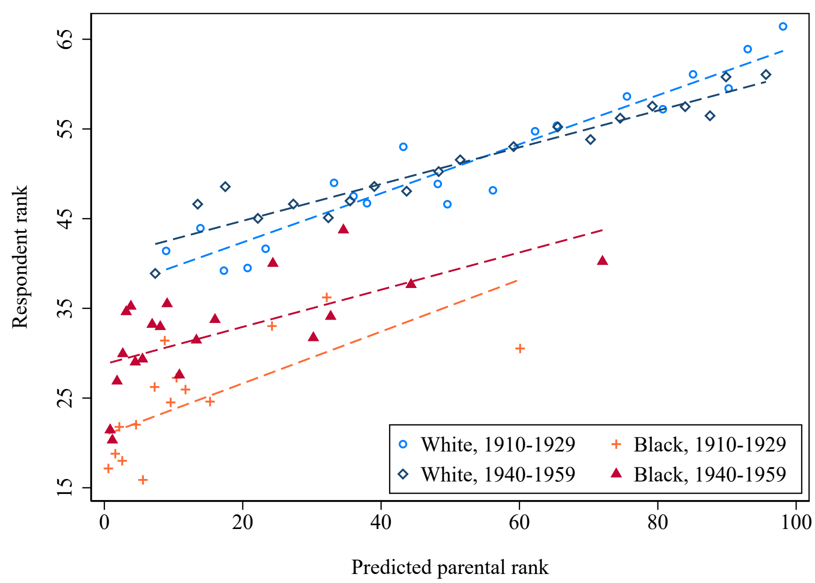
Notes: The IGE and rank-rank are based on the baseline sample of respondents ages 30–50. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares. To account for pooling of cohorts, specifications include birth decade fixed effects.

Figure 6: Mobility by race for women, 1910s–1920s versus 1940s-1950s

(a) Intergenerational elasticities



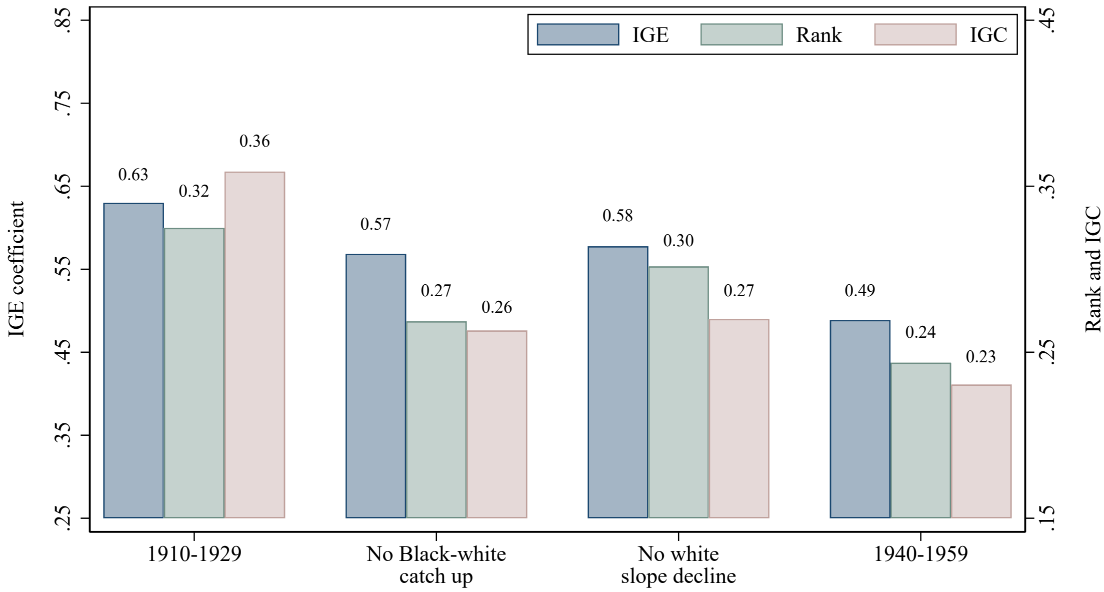
(b) Rank-rank relationships



Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

Notes: The IGE and rank-rank are based on the baseline sample of respondents ages 30–50. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares. To account for pooling of cohorts, specifications include birth decade fixed effects.

Figure 7: Decomposing the rise in mobility from the 1910s–1920s to 1940s–1950s

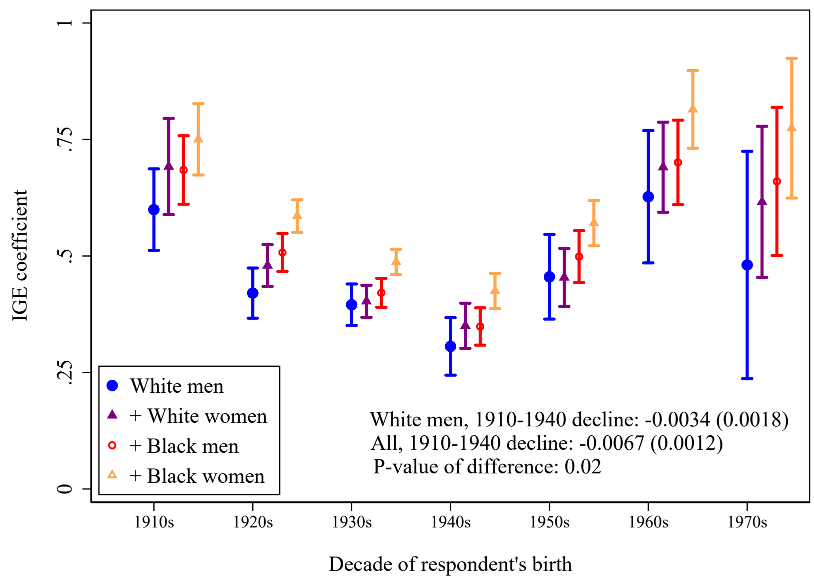


Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

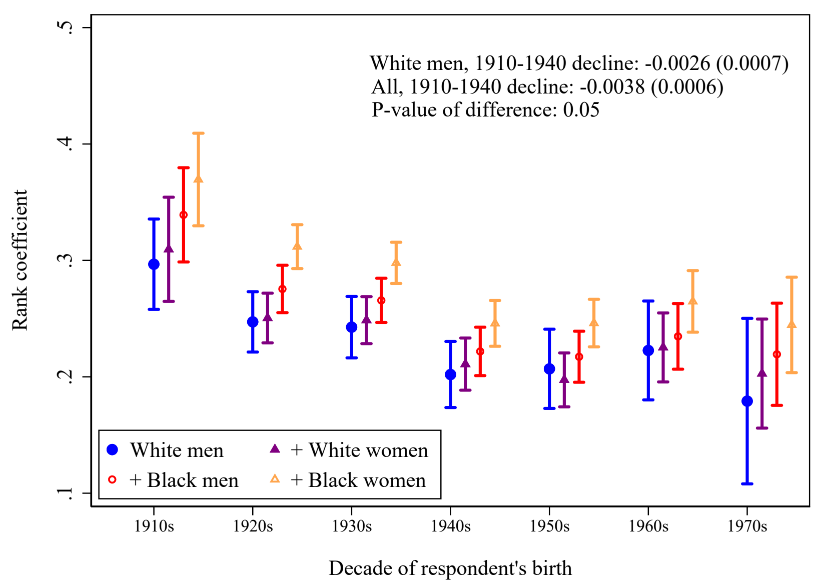
Notes: This figure shows the contribution of different components of the decomposition in Section 6 to the increase in intergenerational mobility that occurred between the 1910s–1920s cohorts and the 1940s–1950s cohorts. Specifically, the figure shows the contribution of the reduction in the white-only persistence measure and the contribution of the between-group convergence in income levels. "No Black-white catch up" refers to assuming that Black respondents had the same income growth as white respondents in log points in the adult children's generation. "No white slope decline" refers to white individuals in the 1940s birth cohorts having the same slope as the 1910s cohorts (without altering the average incomes of white and Black adult children). The IGE and IGC bars display the intergenerational elasticity and correlation, respectively, in the early and late period as well as under these two scenarios. The "Rank" bars display the rank-rank correlation in the early and late period as well as under these two scenarios (re-ranking individuals after altering their logged incomes to reflect both scenarios). To account for pooling of cohorts, specifications include birth decade fixed effects.

Figure 8: Mobility patterns over the 20th century including under-represented groups

(a) Intergenerational elasticities



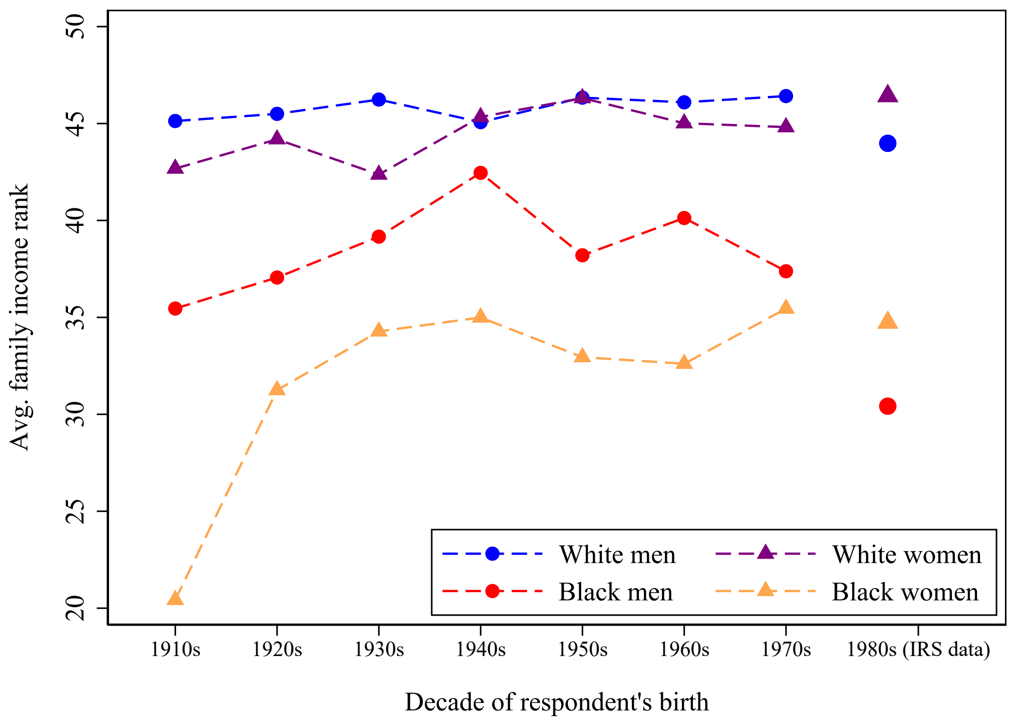
(b) Rank-rank relationships



Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

Notes: Estimates are based on the baseline sample of respondents. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares. Each panel reports the decline between the 1910s–1940s cohorts from a specification that models the decline in the slope linearly. Specifically, we run regressions in which we interact predicted parental income (or rank) with a variable that measures the number of years between a respondent's birth and 1911 (including birth-year fixed effects). In both panels, the p-values correspond to a test of whether the two coefficients (using white men vs. representative samples) are equal using seemingly unrelated regressions.

Figure 9: Average income rank of individuals born to the 25$^{\text{th}}$ percentile of the parental income distribution, by subgroup and birth cohort



Sources: Data for the 1910s–1970s birth cohorts combine 15 different surveys, which are described in Section 2 and in further detail in Appendix E. Data for the 1980 birth cohort come from Chetty *et al.* (2020), available at `https://opportunityinsights.org/data/`.

Notes: This figure plots plots the average adult income rank for individuals growing up at the 25$^{\text{th}}$ percentile of the parental income distribution, separately by race, sex, and birth cohort. For survey respondents, we use equation (3) to compute the expected income rank for individuals growing up at the 25$^{\text{th}}$ percentile of the parental income distribution. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). For the 1980 cohort, we use the average percentile rank in the national distribution of household income (measured in 2014–2015) for individuals growing up at the 25$^{\text{th}}$ percentile of the parent household income distribution (measured in 1994–2000).

Table 1: Select review of intergenerational mobility papers using U.S. data

		Income/status proxy			
Paper	Cohorts	Parent(s)	Child	Links	Sample
Ward (2023)	1850–1910	*Occ.*×*Race*×*Reg.*	*Occ.*×*Race*×*Reg.*	Match	All ♂
Collins and Wanamaker (2022)	1880–1970	*Occ.*×*Race*×*Reg.*	*Occ.*×*Race*×*Reg.*	Match & Retr.	All ♂
Song *et al.* (2020)	1830–1980	Occ.	Occ.	Match & Retr.	White ♂
Abramitzky *et al.* (2021)	1860–1900	*Occ.*×*Reg.*	*Occ.*×*Reg.*	Match	White ♂
Long and Ferrie (2013)	1840, 1930	Occ.	Occ.	Match & Retr.	White ♂
Olivetti and Paserman (2015)	1840–1910	Occ.	Occ.	Synthetic panel	White ♂, married ♀
Feigenbaum (2018)	1900	Inc.	Inc.	Match	Iowa ♂
Feigenbaum (2015)	1900–1910	Inc.	Inc.	Match	Urban ♂
Card *et al.* (2018)	1920	Edu.	Edu.	Same HH	Representative
Bowles (1972)	1930	Inc.	Inc.	Retrospective	CPS ♂
Mazumder (2015)	1950–1970	Inc.	Inc.	Panel data	Representative
Davis and Mazumder (2022)	1950–1960	Inc.	Inc.	Panel data	Representative
Chetty *et al.* (2014a)	1980–1991	Inc.	Inc.	Claim dep.	Representative
Chetty *et al.* (2020)	1978–1983	Inc.	Inc.	Claim dep.	Representative
Our paper	1910–1970	*Occ.*×*Race*×*South*	Inc.	Retrospective	Representative

Notes: Since many papers do not explicitly consider birth cohorts, the "cohorts" column refers to the birth decade(s) that most of the sample comes from, given the age restrictions used in the paper. In the "Links" column, "Match" refers to matching across datasets (e.g., Census matching by name, age and state of birth); "Synthetic panel" refers to matching based on characteristics but not individual identity; "Claim dep." refers to matching by whether the parent ever claims the child as a dependent to the IRS; "Retrospective" (or "Retr.") refers to adult children being asked retrospectively about the characteristics of their parents (e.g., occupation and education). Abramitzky *et al.* (2021) also considers modern cohorts corresponding to those in Chetty *et al.* (2020).

Table 2: Summary statistics, by birth decade

	1910s	1920s	1930s	1940s	1950s	1960s	1970s
Father demographics:							
Foreign-born	0.22	0.17	0.11	0.05	0.03	0.03	0.05
High school educated	0.17	0.19	0.26	0.45	0.60	0.70	0.82
College educated	0.04	0.04	0.06	0.09	0.16	0.20	0.26
Farming occupation	0.37	0.29	0.24	0.15	0.09	0.05	0.03
Respondent demographics:							
Female	0.12	0.33	0.45	0.44	0.57	0.52	0.56
Age	45.89	41.52	36.95	38.49	38.05	38.46	38.92
Black	0.12	0.13	0.15	0.15	0.18	0.16	0.24
High school educated	0.50	0.61	0.71	0.85	0.90	0.91	0.91
College educated	0.10	0.14	0.16	0.28	0.28	0.30	0.39
Moved regions	0.21	0.21	0.22	0.24	0.22	0.21	0.22
Union member (men)	0.31	0.31	0.29	0.28	0.22	0.17	0.13
Veteran (men)	—	0.77	0.59	0.46	0.21	0.15	0.14
Parental income:							
Predicted income (1950$)	2,340	2,575	3,292	5,373	7,687	9,107	9,479
Missing income	0.13	0.15	0.16	0.15	0.16	0.24	0.21
Rank	45.37	45.44	45.50	45.94	45.61	46.87	44.87
Respondent income:							
Family income (1950$)	5,506	6,803	7,292	7,895	7,620	7,887	8,487
Missing income	0.15	0.10	0.06	0.06	0.08	0.08	0.04
Bottom coded	0.08	0.05	0.03	0.02	0.04	0.05	0.06
Top coded	0.07	0.07	0.08	0.14	0.12	0.09	0.08
Family income rank	49.00	48.23	47.09	46.30	46.41	47.62	46.02
Observations	5,207	13,328	12,446	11,580	10,964	6,605	3,132

Notes: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E. All of the shares in this table are unweighted and are based on the baseline sample of respondents ages 30–50 (i.e., those with non-missing family income and predicted parental family income). The two exceptions are the "Missing income" rows, which consider all U.S.-born respondents ages 30–50 in the 15 surveys. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). For characteristics that are unavailable in every survey (e.g., father's educational attainment), the average is computed using only the baseline-sample respondents in the surveys with the available information. When considering union membership and veteran status, we restrict the sample to male respondents. "Bottom coded" and "Top coded" refers to the share of individuals that had family income values in the bottom or top bins, respectively.

Table 3: Differences between 1910 and 1940 IGE, using various adjustments

	IGE			Rank-rank		
	Ratio (1)	Difference (2)	Racial Convergence (3)	Ratio (4)	Difference (5)	Racial Convergence (6)
Baseline	0.57 (0.04)	0.33 (0.04)	0.19	0.67 (0.05)	0.12 (0.02)	0.05
Alt. parental inc. measures:						
Using CW farm fix	0.58 (0.04)	0.30 (0.04)	0.16	0.64 (0.04)	0.14 (0.02)	0.05
Dropping farmers	0.57 (0.06)	0.30 (0.07)	0.18	0.69 (0.07)	0.10 (0.03)	0.06
Only occ. in prediction	0.57 (0.05)	0.31 (0.06)	0.03	0.72 (0.05)	0.09 (0.02)	0.01
Using IPUMS occscore	0.61 (0.05)	0.21 (0.04)	0.01	0.66 (0.05)	0.10 (0.02)	0.01
Using father's income	0.54 (0.04)	0.31 (0.04)	0.17	0.63 (0.04)	0.14 (0.02)	0.05
Using nearest Census	0.63 (0.04)	0.27 (0.04)	0.16	0.64 (0.04)	0.14 (0.02)	0.04
Alt. weights in prediction	0.57 (0.04)	0.32 (0.04)	0.19	0.67 (0.05)	0.12 (0.02)	0.05
Including missing income:						
Includes non-working fathers	0.56 (0.04)	0.33 (0.04)	0.20	0.67 (0.05)	0.12 (0.02)	0.05
Using mother's occupation	0.54 (0.04)	0.33 (0.04)	0.19	0.66 (0.05)	0.13 (0.02)	0.05
+ Moms & non-working dads	0.54 (0.04)	0.33 (0.04)	0.20	0.67 (0.05)	0.12 (0.02)	0.05
Additional predictors:						
Using father's education	0.66 (0.05)	0.20 (0.04)	0.09	0.67 (0.05)	0.12 (0.02)	0.05
Using childhood region	0.54 (0.04)	0.33 (0.04)	0.20	0.60 (0.05)	0.15 (0.02)	0.06
Different age ranges:						
Ages 30–45	0.53 (0.04)	0.38 (0.06)	0.20	0.64 (0.06)	0.14 (0.03)	0.05
Ages 35–50	0.65 (0.05)	0.26 (0.05)	0.17	0.71 (0.05)	0.11 (0.02)	0.05

Notes: Columns 1 and 4 report the ratio of the IGE and rank-rank correlation estimates, respectively, for the 1940s cohort relative to the estimates for the 1910s cohort. Columns 2 and 5 report the difference in the IGE and in the rank-rank correlation, respectively, between the 1910s and 1940s cohorts. Columns 3 and 6 report the difference in the third term of the decomposition (i.e., the third term of equation (4), denoting average income differences by race) between the 1910s and 1940s cohorts. "Using CW fix" refers to using the adjustments to farmer and self-employed income from Collins and Wanamaker (2022). "Using father's income" refers to using the father's personal, rather than household, income. "Using nearest Census" refers to using the nearest Census to childhood (i.e., 1940 with 1936 adjustments for the 1910s–1930s birth cohorts and 1950–1980 for the 1940s–1970s cohorts, respectively). "Alt. weights in prediction" refers to using the number of children a father has when constructing average predicted parental income.

Mobility for All: Appendix Materials

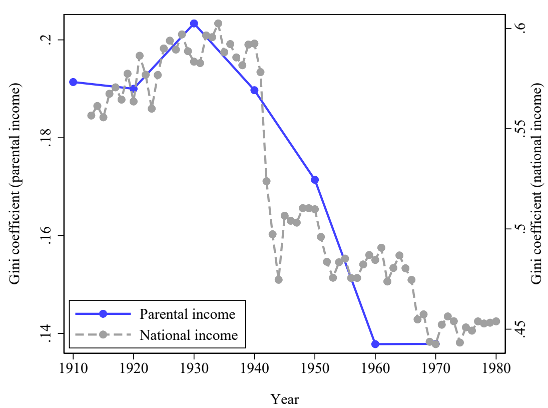
List of Appendix Figures

List of Tables

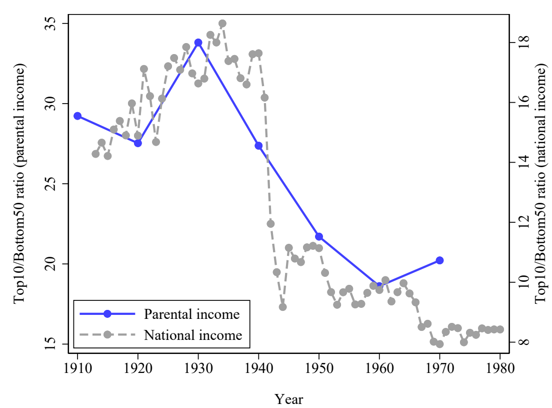
A Additional figures and tables referenced in the text

Figure A.1: Measures of inequality and Black-white income gap of predicted parental income, by birth cohort

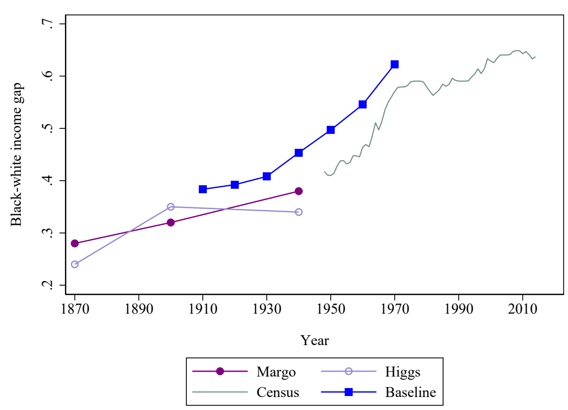
(a) Gini coefficient



(b) Top 10 to Bottom 50 ratio



(c) Black-white ratio of (predicted) parental income

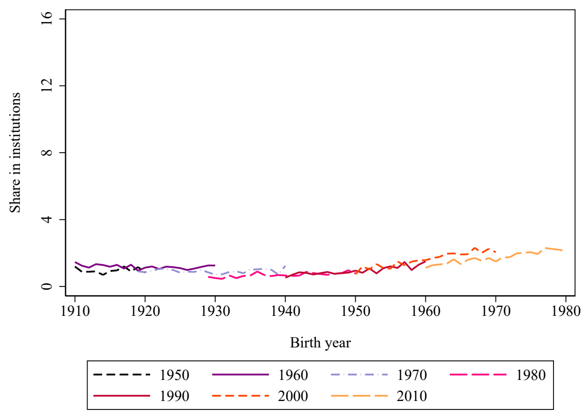


Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E. Data on national income comes from the World Inequality Database. Data on the Black-white income gap comes from Margo (2016).

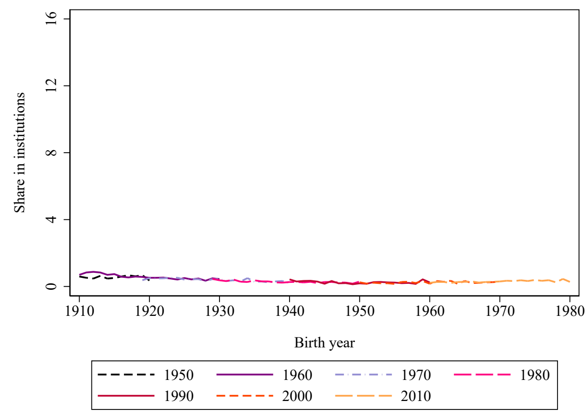
Notes: The first panel plots the Gini coefficient of predicted parental income for each birth cohort in the baseline sample as well as the Gini coefficient of national income. The second panel plots the ratio of total income in the top 10% of the income distribution relative to the total income in the bottom 50%, using predicted parental income for each cohort as well as national income. The third panel plots the ratio of the average parental income of Black respondents to that of white respondents in each birth cohort as well as Black-white income gaps from other data sources. In the first two panels, to predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). To be consistent with other data sources, in the third panel we use father's personal income (conditional on occupation, race, and region) to predict parental income.

Figure A.2: Share of Census respondents in institutions, by birth year and Census year

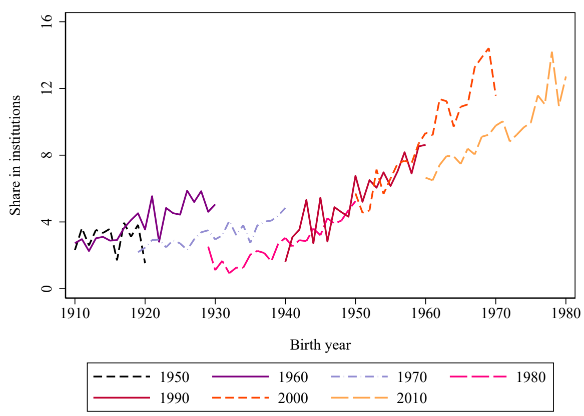
(a) White men



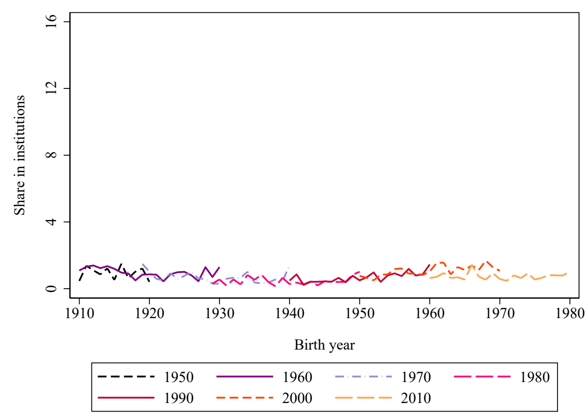
(b) White women



(c) Black men



(d) Black women

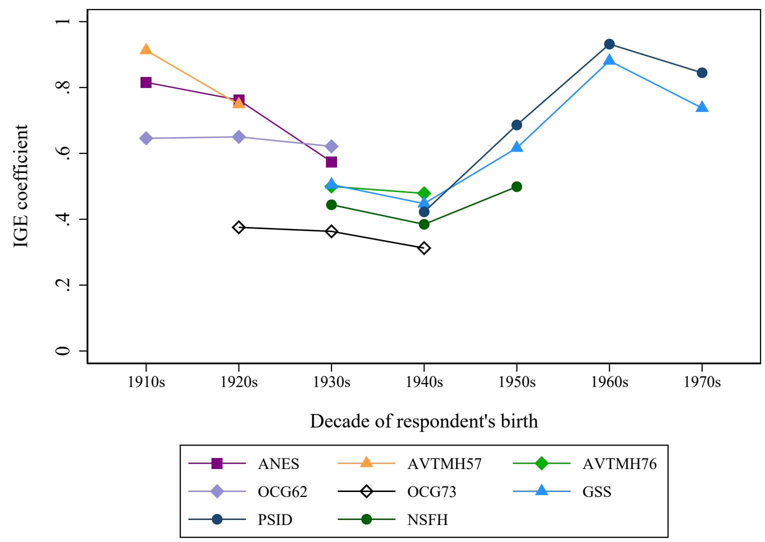


Sources: 1950–2000 1% Census samples and 2010 American Community Survey (Ruggles *et al.*, 2021).

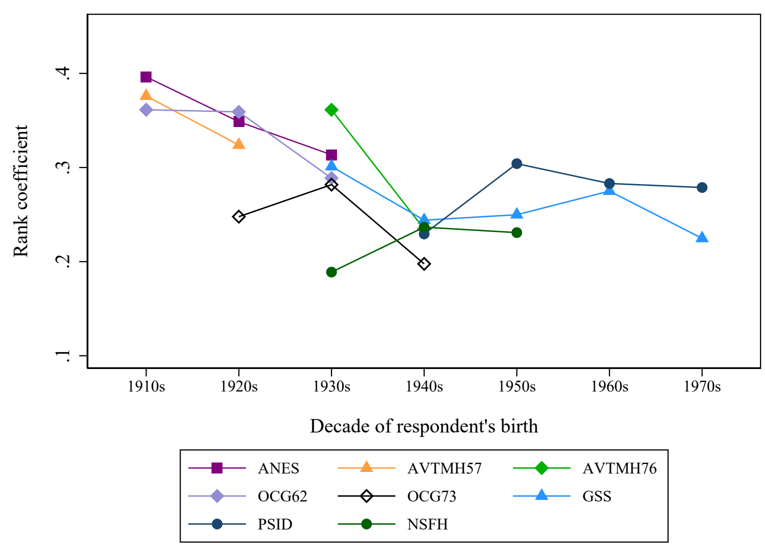
Notes: This figure plots the share of individuals born in a specific year that are living in institutions (measured using group quarter status), separately by Census year. The sample is restricted to white and Black U.S.-born Census respondents.

Figure A.3: Mobility measures by birth decade and by survey

(a) Intergenerational elasticity



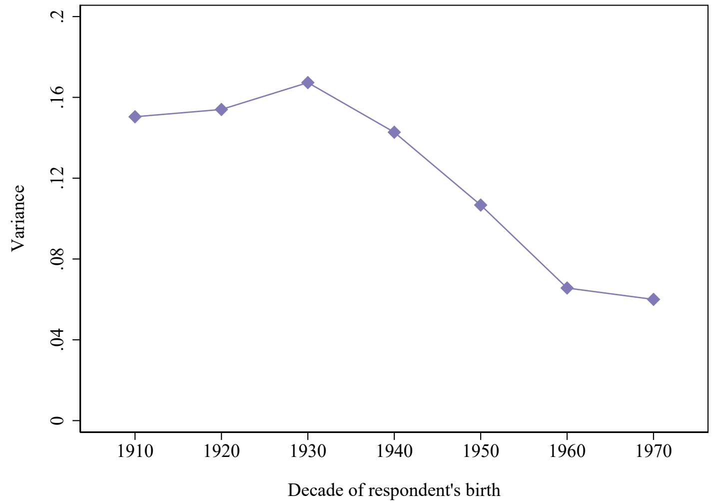
(b) Rank-rank coefficient



Sources: This figure uses 8 of the 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

Notes: This figure plots the IGE and rank-rank coefficient estimated on each survey separately. We exclude surveys whose respondents are only of one race as well as surveys that are not representative of the 30–50 age group. We also exclude cohorts within a survey if there were fewer than 200 respondents born in that decade. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). In both panels, we use the baseline population-adjusted weights and in the bottom panel, we maintain the same ranking for respondents and their parents as in the baseline approach.

Figure A.4: Variance of parental income by birth cohort

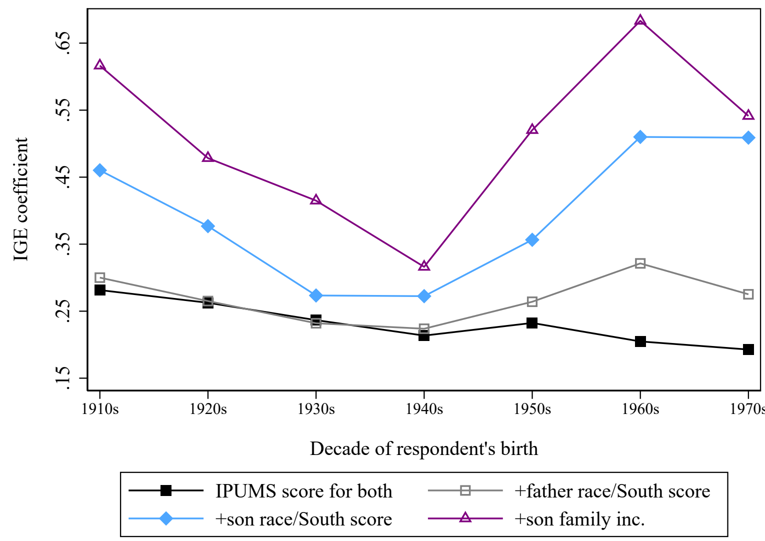


Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

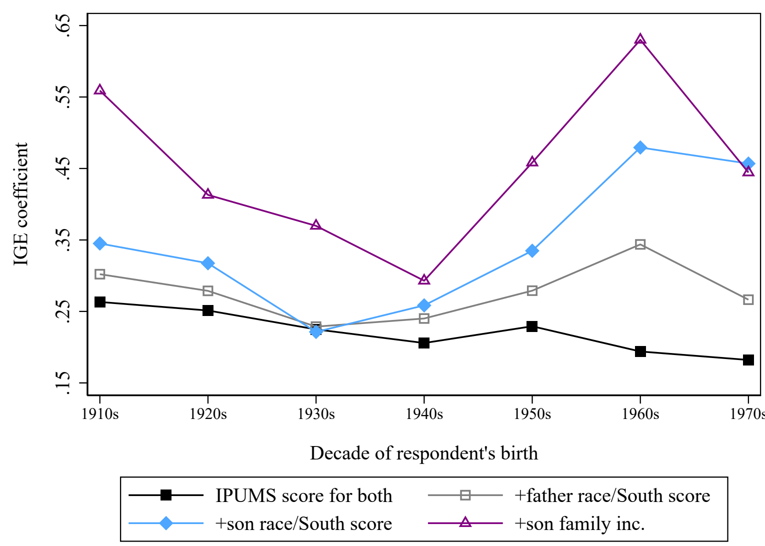
Notes: This figure plots the variance of predicted parental income for the baseline sample of respondents ages 30–50. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares.

Figure A.5: IGE measure for men by birth cohort, using various ways of measuring parental and adult children's incomes

(a) All men



(b) White men

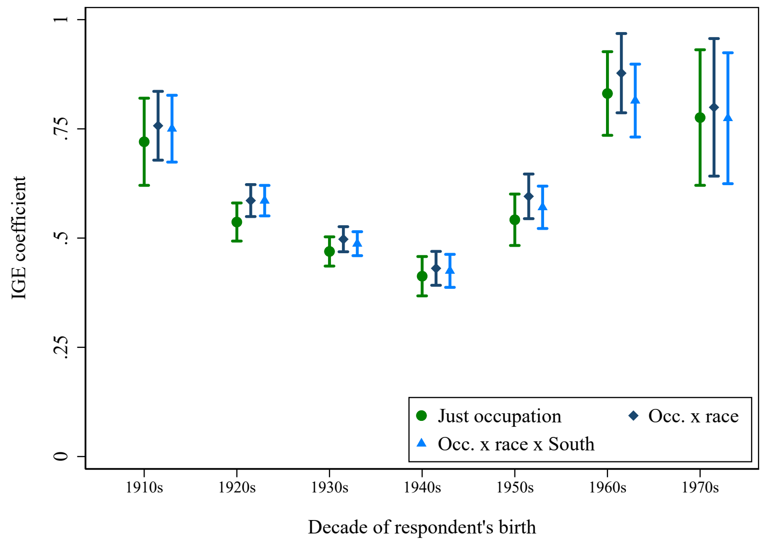


Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

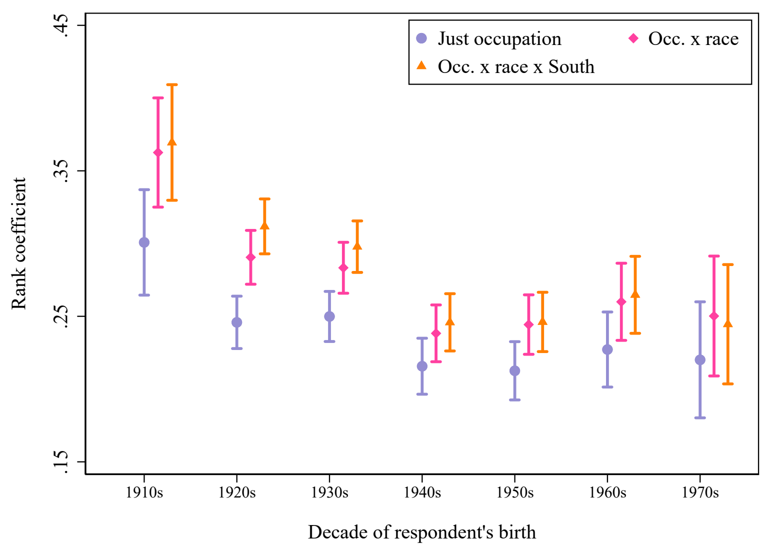
Notes: This figure plots the IGE coefficient as we alter how we measure respondent and parental income. Both panels only consider male respondents who reported their occupations as well as their fathers' occupations. The first series uses the IPUMS *occscore* variable to measure income in both generations. The second series replaces predicted parental income with the baseline family income prediction at the *occupation* × *race* × *South* level. The third series replaces the *occscore*-based income prediction for sons with an income prediction that varies at the *occupation* × *race* × *South* level using the two Censuses closest in time to when the respondent was 40 years old (i.e., using weighted averages of predicted income that are constructed using the 1940–2000 Censuses as well as the 2010 and 2019 American Community Survey from Ruggles *et al.* (2021)). The fourth series replaces the son's income prediction with the son's reported family income.

Figure A.6: Mobility measures by birth decade, adding detail to parental family income prediction

(a) Intergenerational elasticity



(b) Rank-rank coefficient

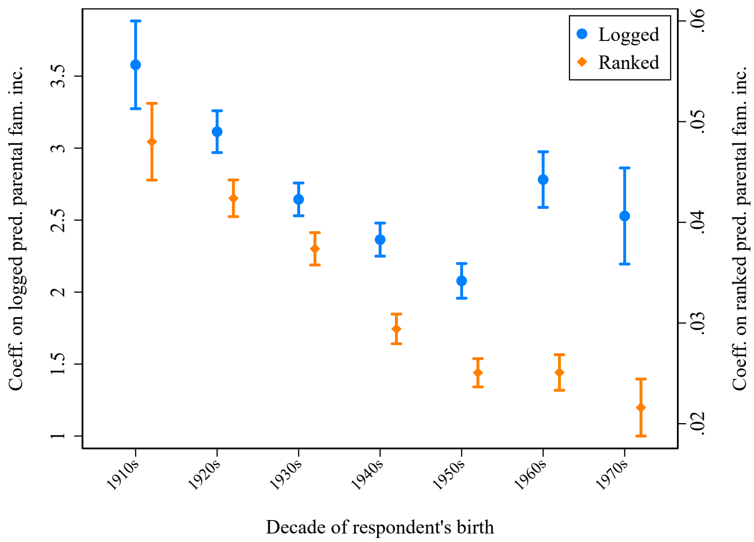


Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

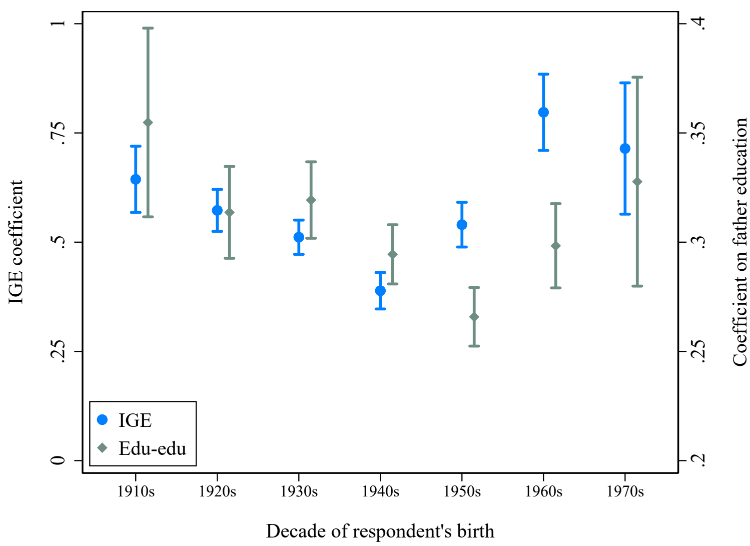
Notes: The IGE and rank-rank are based on the baseline sample of respondents ages 30–50. The first series uses predicted parental income that only varies by a father's occupation. The second series allows income to vary by father's occupation and race. The third series allows income to vary by father's occupation, race, and Southern residence. To predict parental income, we use family income conditional on father's characteristics from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) in this sample so that they have representative *race* $\times$ *sex* shares.

Figure A.7: Weakening intergenerational relationship using educational attainment

(a) Respondent education & (predicted) parental income



(b) Respondent & parental education

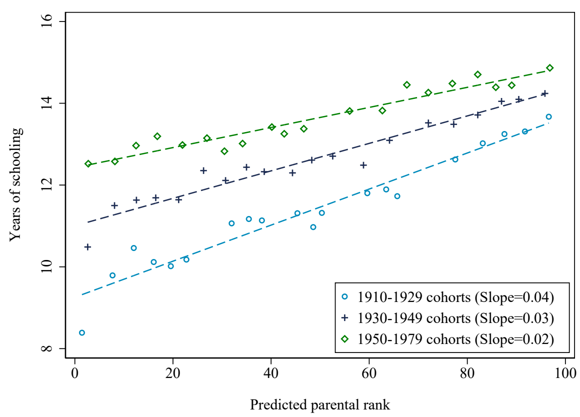


Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

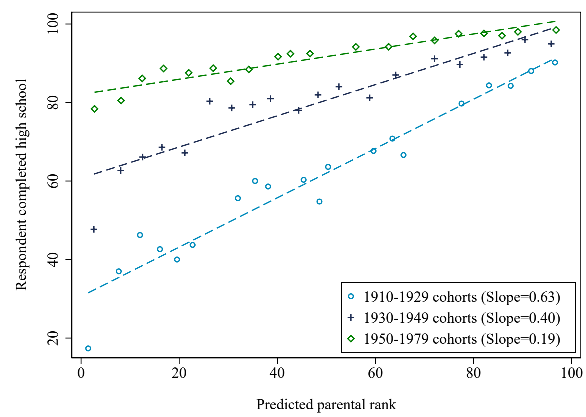
Notes: In the top panel, we use a respondent's years of schooling as the dependent variable and regress it on logged or ranked predicted parental income—similar to equations (2) and (3)—using the baseline sample of respondents ages 30–50. In the bottom panel, we restrict the sample to the 12 surveys that include information on father's education. The first series plots the IGE for this subsample and the second series plots the estimates from a regression of respondent years of schooling on parental years of schooling. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares.

Figure A.8: Bin-scatter depictions of the weakening relationship between respondent education and parental rank

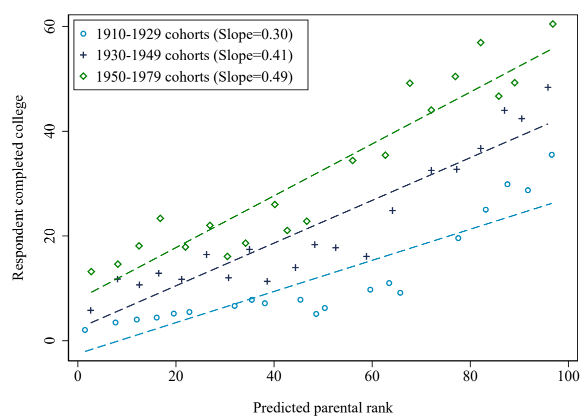
(a) Years of schooling



(b) High school completion



(c) College completion

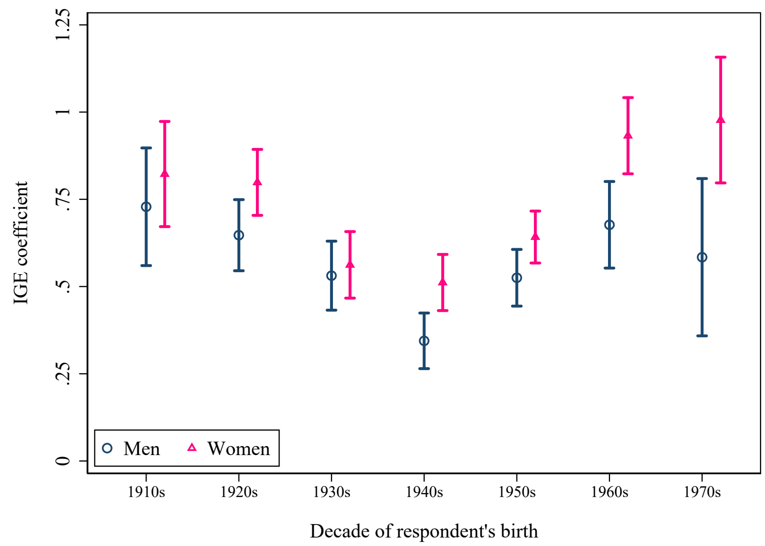


Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

Notes: The estimates are based on the baseline sample of respondents ages 30–50. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares. Further details on the construction of education variables are available in Appendix E.

Figure A.9: Mobility measures by birth decade, by sex (restricted to common surveys)

(a) Intergenerational elasticity



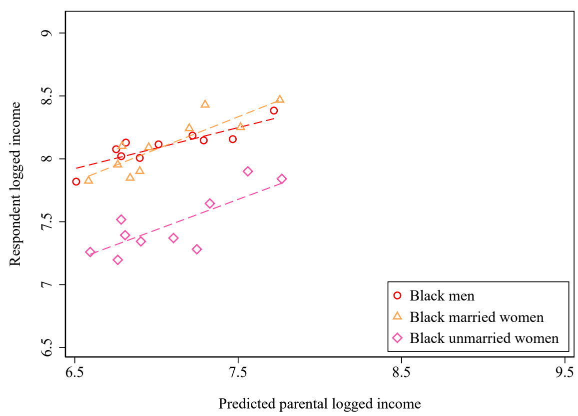
(b) Rank-rank coefficient

Sources: This figure combines the 7 surveys that include both male and female respondents.

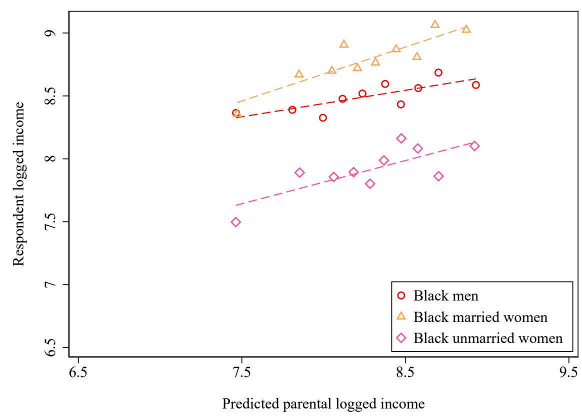
Notes: This figure is identical to Figure 4 except that in this figure, we only use surveys that include both men and women. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further weight each birth cohort in this sub-sample so that they have representative *race* $\times$ *sex* shares.

Figure A.10: Mobility by marital status for Black women and men, 1910s–1920s versus 1940s–1950s

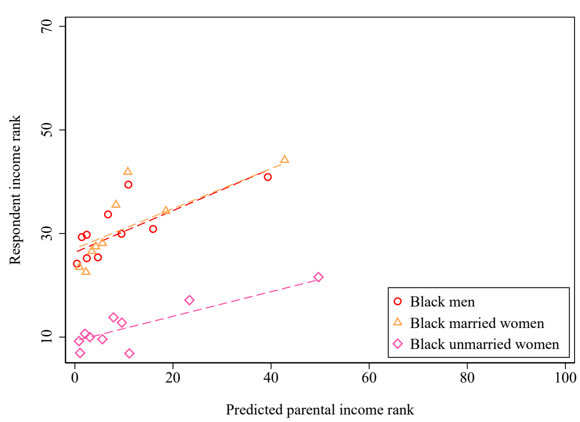
(a) IGE: 1910s–1920s



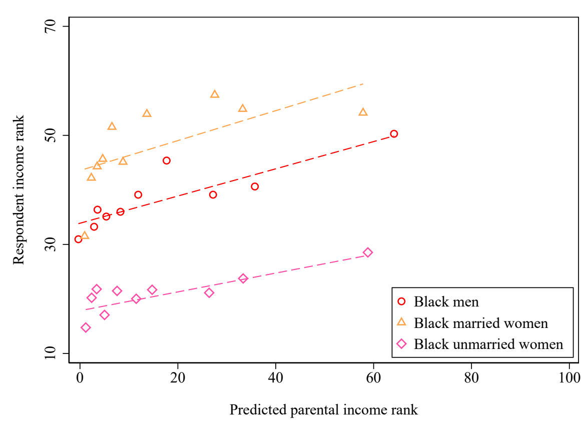
(b) IGE: 1940s–1950s



(c) Rank-rank correlation: 1910s–1920s



(d) Rank-rank correlation: 1940s–1950s



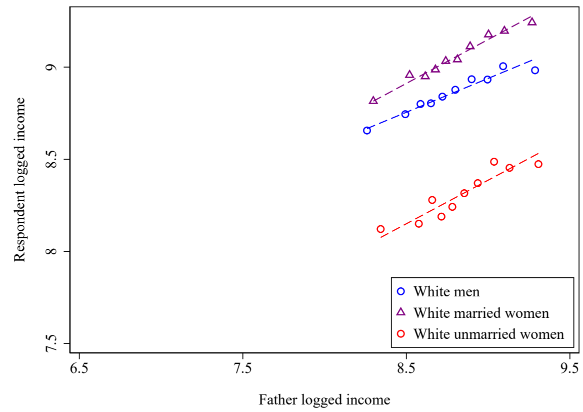
Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

Notes: The IGE and rank-rank are based on the baseline sample of respondents ages 30–50. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares. To account for pooling of cohorts, specifications include birth decade fixed effects.

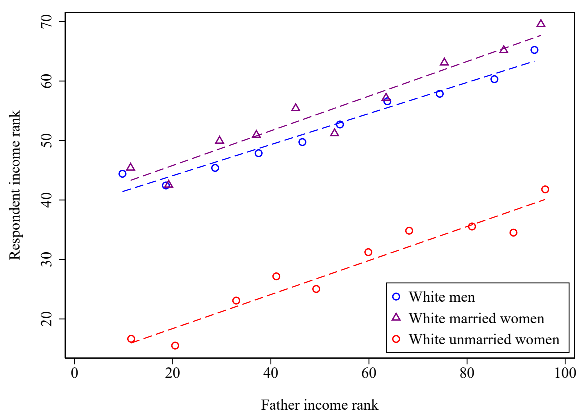
Figure A.11: Mobility by marital status for white women and men, 1910s–1920s versus 1940s–1950s

(a) IGE: 1910s–1920s

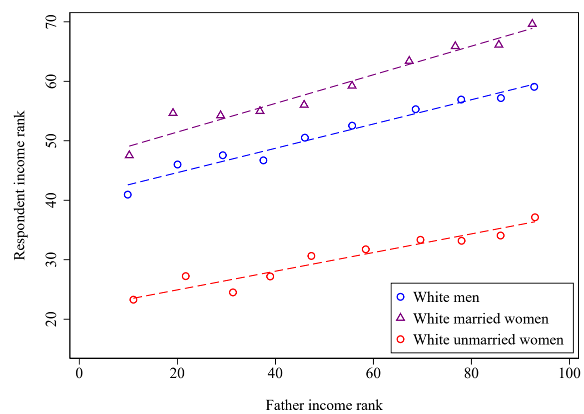
(b) IGE: 1940s–1950s



(c) Rank-rank correlation: 1910s–1920s



(d) Rank-rank correlation: 1940s–1950s

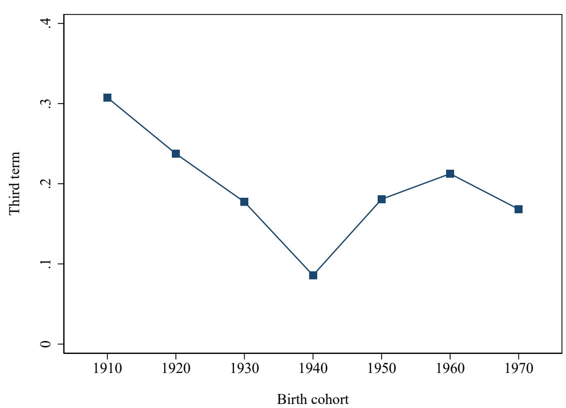


Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

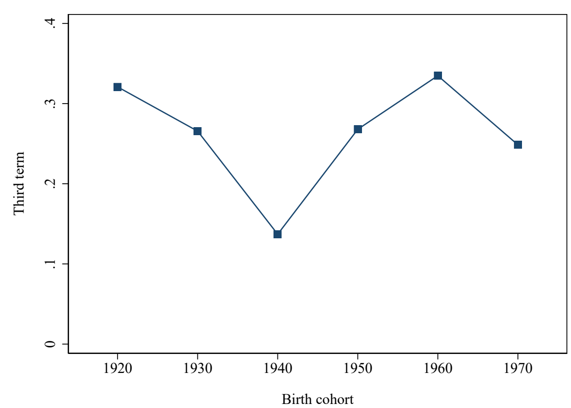
Notes: The IGE and rank-rank are based on the baseline sample of respondents ages 30–50. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares. To account for pooling of cohorts, specifications include birth decade fixed effects.

Figure A.12: Black-white intergenerational convergence across cohorts using Census data (third term of decomposition)

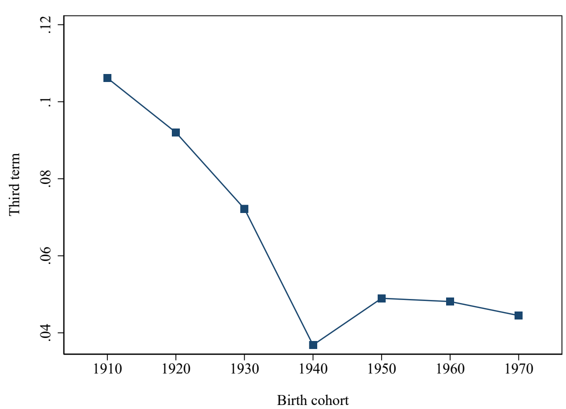
(a) Logged income, men



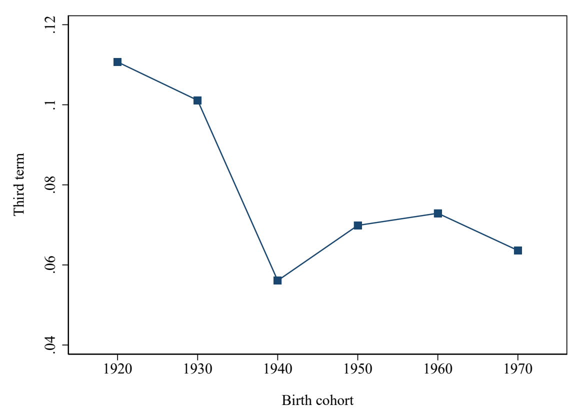
(b) Logged income, women



(c) Ranked income, men



(d) Ranked income, women



Sources: 1920–2000 1% Census samples as well as the 2010 and 2019 American Community Survey (Ruggles *et al.*, 2021).

Notes: This figure plots the third term from equation (6) for both logged and ranked income using Census data. To identify the fathers of children born in a given cohort, we consider the two Censuses in which that cohort would be aged 0–18 (e.g., for the 1920s cohort, we look at fathers in the 1930 and 1940 Censuses). We then restrict the sample to men ages 30–50 with a child present in the household who was born in that cohort. To identify individuals born in a given cohort, we consider the two Censuses in which that cohort would be aged 30–50 and restrict the sample to U.S.-born individuals. Fathers are assigned predicted income using their occupation, race, and Southern location and using the closest Census to that year (i.e., fathers in the 1920–40 Censuses are assigned a 1940-based income prediction with 1936 fixes, and fathers from later Censuses are assigned an income prediction constructed using later Censuses). Adult children are assigned their total family income. Women in the 1910s cohort are excluded because their total family income cannot be accurately measured in the 1950 Census (because of the sample-line restriction). In both generations, we restrict the sample to individuals whose race is classified as white or Black. Fathers are re-weighted so that the subgroup population share is the same for both generations. In panels (c) and (d), adult children are ranked relative to other adult children of the same age and fathers are ranked relative to fathers of the same age.

Table A.1: Summary statistics, comparing survey respondents to Census respondents

	1910–1929		1930–1949		1950–1969	
	Census	Survey	Census	Survey	Census	Survey
Panel A: White Men						
Share of Men	0.90	0.91	0.90	0.90	0.87	0.86
Age	39.51	43.30	38.69	37.05	40.59	38.13
High school graduate	0.51	0.60	0.81	0.80	0.92	0.91
College graduate	0.12	0.15	0.27	0.27	0.30	0.33
Southern born/grew up	0.30	0.28	0.31	0.31	0.28	0.27
Resides in the South	0.28	0.27	0.33	0.31	0.34	0.31
Married	0.87	0.90	0.81	0.84	0.68	0.66
Widowed	0.01	0.01	0.00	0.01	0.00	0.01
Family income, 1950$	6,124	6,762	7,712	8,145	8,519	8,362
Respondent rank	52.57	51.64	53.20	52.60	52.20	53.50
Observations	195,091	12,281	214,612	11,942	297,783	6,745
Panel B: Black Men						
Share of Men	0.10	0.09	0.10	0.10	0.13	0.14
Age	39.41	44.57	38.54	37.66	40.13	38.01
High school graduate	0.21	0.28	0.62	0.60	0.85	0.82
College graduate	0.03	0.04	0.10	0.12	0.13	0.14
Southern born/grew up	0.86	0.84	0.77	0.73	0.60	0.62
Resides in the South	0.54	0.54	0.51	0.57	0.57	0.61
Married	0.75	0.82	0.63	0.69	0.50	0.53
Widowed	0.02	0.02	0.01	0.02	0.01	0.01
Family income, 1950$	3,817	4,257	5,738	6,109	6,318	6,129
Respondent rank	27.59	31.53	39.19	38.09	38.72	40.18
Observations	21,002	1,212	24,293	1,393	38,206	1,127
Panel C: White Women						
Share of Women	0.89	0.79	0.88	0.80	0.86	0.81
Age	39.50	40.97	38.74	38.57	40.64	38.35
High school graduate	0.55	0.66	0.81	0.82	0.94	0.93
College graduate	0.07	0.09	0.17	0.19	0.30	0.31
Southern born/grew up	0.30	0.31	0.31	0.31	0.28	0.27
Resides in the South	0.28	0.30	0.32	0.32	0.34	0.31
Married	0.86	0.86	0.79	0.77	0.70	0.65
Widowed	0.03	0.03	0.02	0.02	0.01	0.01
Family income, 1950$	6,033	6,865	7,527	7,737	8,469	8,061
Respondent rank	51.06	51.64	51.45	50.73	51.75	51.62
Observations	201,503	3,977	217,061	8,537	302,610	7,810
Panel D: Black Women						
Share of Women	0.11	0.21	0.12	0.20	0.14	0.19
Age	39.27	40.88	38.70	37.81	40.18	38.01
High school graduate	0.25	0.32	0.63	0.59	0.88	0.83
College graduate	0.04	0.05	0.09	0.11	0.17	0.16
Southern born/grew up	0.86	0.84	0.77	0.73	0.61	0.66
Resides in the South	0.55	0.60	0.51	0.57	0.58	0.64
Married	0.66	0.64	0.50	0.52	0.40	0.37
Widowed	0.08	0.09	0.06	0.06	0.03	0.03
Family income, 1950$	3,560	3,598	4,962	4,806	5,706	4,966
Respondent rank	23.72	23.81	32.87	29.15	34.65	32.51
Observations	24,081	1,065	29,808	2,154	45,166	1,887

Notes: Survey shares are based on the baseline sample of respondents ages 30–50 and are unweighted. We use the 1% samples of the 1960, 1980, and 2000 Censuses from Ruggles *et al.* (2021) and keep Census respondents born in the same years as survey respondents.

Table A.2: Summary Statistics in Panel Study of Income Dynamics

				Father's Income		
	1968 Men	1968 Fathers	With Child in Survey	1 year	5 years	10 years
Age	40.06	39.98	39.75	39.61	38.39	36.26
Black	0.09	0.08	0.07	0.07	0.06	0.06
HS educated	0.56	0.58	0.61	0.62	0.62	0.65
College edu.	0.16	0.17	0.19	0.19	0.20	0.20
Family income	10,986	11,109	11,363	11,399	11,346	11,264
Observations	1,765	1,472	1,077	959	802	558

Notes: This table uses the Panel Study of Income Dynamics dataset from 1968 through 2015. The first column considers all men ages 30–50 in the 1968 wave of the PSID. Column 2 restricts that sample to household heads with children present in the family unit, away from home, or in an institution. Column 3 further restricts the sample to those who were identified by the PSID as the biological or adoptive fathers of other survey respondents using the Family Identification Mapping System (FIMS). Columns 4–6 then restrict the sample to fathers with 1, 5, and 10 years of available income between the ages of 30–50, respectively, and whose children had at least one year of available income between ages 30–50.

Table A.3: IGE and rank coefficient, by birth cohort

(a) Intergenerational elasticity

	(1) 1910	(2) 1920	(3) 1930	(4) 1940	(5) 1950	(6) 1960	(7) 1970
IGE coefficient	0.750	0.586	0.487	0.425	0.570	0.815	0.774
	[0.039]	[0.018]	[0.014]	[0.019]	[0.025]	[0.043]	[0.076]
Lower & Upper Bound	(0.67, 0.83)	(0.55, 0.62)	(0.46, 0.51)	(0.39, 0.46)	(0.52, 0.62)	(0.73, 0.90)	(0.62, 0.92)
Observations	5,207	13,328	12,446	11,580	10,964	6,605	3,132

(b) Rank-rank coefficient

	(1) 1910	(2) 1920	(3) 1930	(4) 1940	(5) 1950	(6) 1960	(7) 1970
Rank coefficient	0.369	0.312	0.298	0.246	0.246	0.265	0.245
	[0.020]	[0.010]	[0.009]	[0.010]	[0.010]	[0.013]	[0.021]
Lower & Upper Bound	(0.33, 0.41)	(0.29, 0.33)	(0.28, 0.32)	(0.23, 0.27)	(0.23, 0.27)	(0.24, 0.29)	(0.20, 0.29)
Observations	5,207	13,328	12,446	11,580	10,964	6,605	3,132

Notes: The IGE and rank-rank estimates—calculated using equations (2) and (3), respectively—are based on the baseline sample of respondents ages 30–50. "Lower & Upper Bound" refers to the 95% confidence interval of the corresponding estimate. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares.

Table A.4: Quantifying the decline between the 1910s and 1940s birth cohorts

	IGE		Rank-rank	
	(1)	(2)	(3)	(4)
	1910–1940 difference	Linear difference	1910–1940 difference	Linear difference
Difference	-0.3254	-0.0067	-0.1236	-0.0038
	[0.0435]	[0.0012]	[0.0226]	[0.0006]
Observations	16,787	42,561	16,787	42,561

Notes: This table quantifies the decline in the IGE and rank-rank correlation between the 1910s and 1940s birth cohorts using the baseline sample of respondents ages 30–50. "1910–1940 difference" considers the difference between respondents born in the 1910s birth cohorts and those born in the 1940s cohorts, using specifications like equations (2) and (3), but allowing the slope and intercept to differ by cohort. The reported coefficient and standard error correspond to the interaction term, which measures the difference in the slope between the two cohorts. "Linear difference" considers all respondents born in the 1910s–1940s cohorts and models the decline in the slope linearly. Specifically, we run specifications in which we interact predicted parental income (or rank) with a variable that measures the number of years between a respondent's birth and 1911 (including birth-year fixed effects). To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* $\times$ *sex* shares.

Table A.5: Predictive power (R-squared) of occupation, race, region, and education on 1940–1990 Census measures of logged household income

	Occupation	Race	Occ. × race	Race × South	Occ. × race × South	Occ. × race × South × edu.
1940 Census	0.27	0.05	0.28	0.09	0.31	0.34
1940 Census, 1936 fix	0.24	0.05	0.25	0.08	0.28	–
1960 Census	0.23	0.07	0.27	0.11	0.29	0.33
1970 Census	0.20	0.03	0.21	0.06	0.23	0.28
1980 Census	0.15	0.01	0.15	0.02	0.17	0.21
1990 Census	0.19	0.01	0.20	0.02	0.21	0.26

Notes: This table regresses predicted logged family income of fathers ages 30–50 on various characteristics and reports the R-squared from these regressions. Each row uses a different Census sample. The first and second columns regress income on (coarsened) occupation fixed effects and race fixed effects, respectively. The third column allows the occupation fixed effects to vary by the race of the father. The fourth column regresses income on race fixed effects that vary by Southern location. The fifth column regresses income on occupation × race × South fixed effects and the sixth column uses occupation × race × South × education fixed effects. The first row uses the 1940 Census for all occupations. The second row uses the 1940 Census in conjunction with farmers and self-employed workers from the 1936 Expenditure Survey (for whom educational level is not available).

Table A.6: IGE estimates by cohort and subgroup

	1910s	1920s	1930s	1940s	1950s	1960s	1970s
By sex:							
Men	0.65	0.49	0.44	0.32	0.52	0.68	0.58
	(0.03)	(0.03)	(0.02)	(0.02)	(0.04)	(0.06)	(0.12)
Women	0.85	0.68	0.54	0.54	0.61	0.93	0.98
	(0.07)	(0.03)	(0.02)	(0.03)	(0.03)	(0.06)	(0.09)
By race:							
White	0.69	0.48	0.40	0.35	0.45	0.69	0.62
	(0.05)	(0.02)	(0.02)	(0.02)	(0.03)	(0.05)	(0.08)
Black	0.33	0.42	0.41	0.26	0.28	0.49	0.76
	(0.13)	(0.06)	(0.05)	(0.05)	(0.07)	(0.13)	(0.28)
By subgroup:							
White men	0.60	0.42	0.40	0.31	0.46	0.63	0.48
	(0.04)	(0.03)	(0.02)	(0.03)	(0.05)	(0.07)	(0.12)
+ white women	0.69	0.48	0.40	0.35	0.45	0.69	0.62
	(0.05)	(0.02)	(0.02)	(0.02)	(0.03)	(0.05)	(0.08)
+ Black men	0.68	0.51	0.42	0.35	0.50	0.70	0.66
	(0.04)	(0.02)	(0.02)	(0.02)	(0.03)	(0.05)	(0.08)
+ Black women	0.75	0.59	0.49	0.42	0.57	0.81	0.77
	(0.04)	(0.02)	(0.01)	(0.02)	(0.02)	(0.04)	(0.08)

Notes: The IGE estimates are based on the baseline sample of respondents ages 30–50. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* $\times$ *sex* shares.

Table A.7: Rank-rank correlations by cohort and subgroup

	1910s	1920s	1930s	1940s	1950s	1960s	1970s
By sex:							
Men	0.36	0.29	0.28	0.22	0.24	0.24	0.21
	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.03)
Women	0.38	0.33	0.32	0.27	0.25	0.29	0.28
	(0.04)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.03)
By race:							
White	0.31	0.25	0.25	0.21	0.20	0.23	0.20
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
Black	0.13	0.41	0.42	0.27	0.20	0.17	0.24
	(0.10)	(0.06)	(0.07)	(0.05)	(0.04)	(0.05)	(0.06)
By subgroup:							
White men	0.30	0.25	0.24	0.20	0.21	0.22	0.18
	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.04)
+ white women	0.31	0.25	0.25	0.21	0.20	0.23	0.20
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
+ Black men	0.34	0.28	0.27	0.22	0.22	0.23	0.22
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
+ Black women	0.37	0.31	0.30	0.25	0.25	0.26	0.24
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)

Notes: The rank-rank correlations are based on the baseline sample of respondents ages 30–50. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares.

Table A.8: Differences in the 1910–1940 decline when looking at subgroups versus representative samples

	IGE	Rank-rank
Difference between 1910s and 1940s cohorts:		
White men	-0.294	-0.095
	(0.055)	(0.025)
All whites	-0.342	-0.099
	(0.058)	(0.026)
All whites & Black men	-0.336	-0.117
	(0.043)	(0.023)
Representative sample	-0.325	-0.124
	(0.043)	(0.023)
P-value (white men vs. rep. sample)	0.513	0.199
P-value (all whites vs. rep. sample)	0.708	0.032
P-value (all whites & Black men vs. rep. sample)	0.724	0.480
Linear decline using 1910s–1940s cohorts:		
White men	-0.0034	-0.0026
	(0.0018)	(0.0007)
All whites	-0.0053	-0.0028
	(0.0016)	(0.0007)
All whites & Black men	-0.0066	-0.0035
	(0.0012)	(0.0006)
Representative sample	-0.0067	-0.0038
	(0.0012)	(0.0006)
P-value (white men vs. rep. sample)	0.021	0.048
P-value (all whites vs. rep. sample)	0.232	0.00070
P-value (all whites & Black men vs. rep. sample)	0.953	0.183

Notes: The top panel considers the difference between respondents born in the 1910s birth cohorts and those born in the 1940s cohorts, using specifications like equations (2) and (3), but allowing the slope and intercept to differ by cohort. The reported coefficient and robust standard error correspond to the interaction term, which measures the difference in the slope between the two cohorts. In the second panel, we consider all respondents born in the 1910s–1940s cohorts and model the decline in the slope linearly. Specifically, we run specifications in which we interact predicted parental income (or rank) with a variable that measures the number of years between a respondent's birth and 1911 (including birth-year fixed effects). In both panels, the p-values correspond to a test of whether the two coefficients (using white men vs. representative samples, using all whites vs. representative samples, and using whites + Black men vs. representative samples) are equal using seemingly unrelated regressions.

B Robustness of 1910–1940 mobility increase

In this Appendix, we present alternative estimates of intergenerational mobility over the 20th century to consider the robustness of the full-population decline in persistence we document between the 1910s and 1940s cohorts. For more details on the construction of alternative income measures, we refer the reader to Appendix E.

B.1 Recall-adjusted estimates of mobility

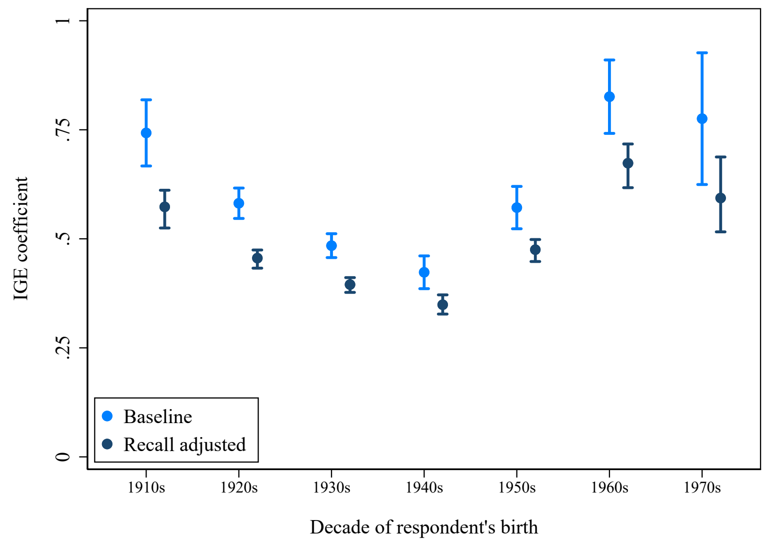
In this subsection, we perform an exercise to consider whether our main result might be driven by recall deteriorating over time (i.e., the decline in persistence between the 1910s and 1940s cohorts stemming from measurement error, rather than true changes in mobility). Specifically, one concern might be that the distribution of fathers' occupations is changing during this time period (e.g., away from agricultural occupations) in such a way that survey respondents might have a more difficult time recalling their fathers' occupations. To consider this possibility, we turn to the sample of households heads in the PSID for whom we have both retrospective answers about fathers' occupations as well as their father's self-reported answers in earlier survey waves (described in detail in Section C.3).

We begin by using this sample to calculate a matrix that denotes the likelihood that a respondent who reports occupation i for their father has their father report occupation j. As an example, among respondents who said their fathers were accountants, 75% of the corresponding fathers reported being accountants, while 15% and 6% of fathers said they were businessmen and clerical workers, respectively. This matrix thus allows us to get a sense of which occupations are easier to recall and importantly, the types of mistakes that are commonly made for each occupation.

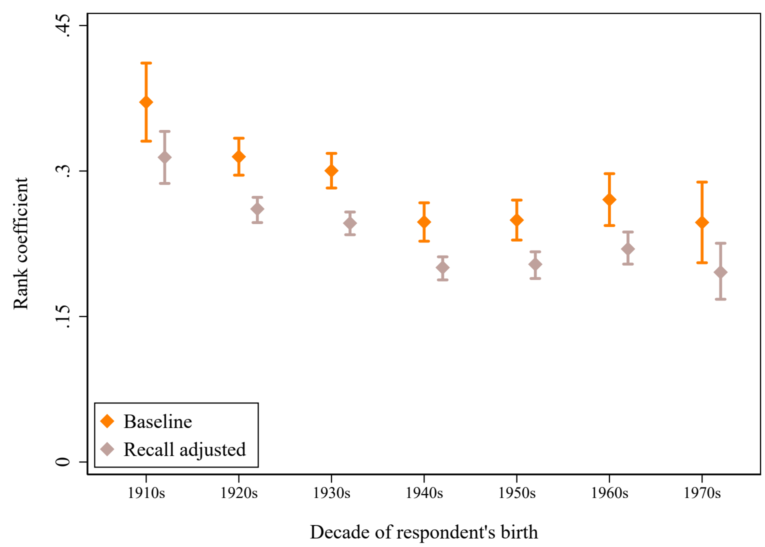
We then return to the baseline sample of respondents from our surveys and use this matrix to alter the occupation of a random share of fathers. Using the accountant example above, we allow a random 75% of respondents who said their fathers were accountants to keep their answer, but we change the fathers' occupations for the remaining 25%. Importantly, the changes we make reflect the distribution of mistakes in the PSID (i.e., 15% and 6% would be changed to businessmen and clerical workers, respectively). We then predict parental income using these recall-adjusted occupations as well as race and Southern location, and we re-calculate the estimates of mobility. We repeat this exercise 200 times, so that we change the occupation of a different share of fathers and allow for different types of mistakes in recall. Appendix Figure B.1 plots the baseline estimates of mobility alongside the estimates from this simulation. The recall-adjusted estimates are generally attenuated, but the main finding of an increase in mobility in the first half of the 20th century is unchanged.

Figure B.1: Mobility measures by birth decade, robustness to recall bias

(a) Intergenerational elasticity



(b) Rank-rank coefficient



Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

Notes: The first series in both panels reproduces the main IGE and rank-rank estimates using our baseline sample and income prediction. In the second series, we allow for the possibility of mistakes in recall. Specifically, we use the matrix of mistakes from the PSID to change the occupation of a random share of fathers, and we then re-estimate the IGE and rank-rank correlation. We repeat this process 200 times. The point estimate in the second series plots the average value obtained, while the upper and lower bound of the confidence intervals plot the values corresponding to the 2.5^{th} and 97.5^{th} percentiles of the distribution of estimates. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) in all samples so that they have representative *race* × *sex* shares.

B.2 Unobserved within-cell variance

By construction, our baseline approach assigns each respondent a childhood income based on the mean family income in a *father occupation* × *race* × *South* cell from the appropriate Census or other auxiliary dataset, and it thus ignores within-cell variation. As noted, to the extent that some within-cell variation in a single Census year is merely transitory, excluding within-cell variation will better approximate permanent average childhood income. But to the extent that within-cell variation reflects systematic income differences missed by *father occupation* × *race* × *South*, our measure of predicted childhood income will bias us away—in an *a priori* unclear direction—from the persistence measure of interest.

We begin to address this concern by re-estimating predicted childhood income on subsets of our data that include more information on childhood background. As noted earlier, for more than half of our surveys, respondents were asked about their fathers' education. We can thus re-calculate measures of predicted childhood income using father' occupation, race, Southern location, *and* father's education for this subsample. *A priori*, father's education is one of the most likely factors to create systematic deviation from our *father occupation* × *race* × *South*-based mean family income. Indeed, adding information about father's education to our standard approach significantly increases predictive power (e.g., in 1960, the R-squared rises from 0.29 to 0.33; see Appendix Table A.5).

The first panel of Appendix Figure B.2 compares the IGE with the original predictions at the *father occupation* × *race* × *South* level—using the baseline sample as well as the sub-sample of respondents who are asked fathers' education—to the IGE using these augmented measures in this restricted sub-sample. Of the 15 surveys in our baseline sample, 12 include information about a father's educational attainment, representing nearly 80 percent of the baseline sample. The three series are very comparable in both levels and trends: in particular, they show the marked decline between the 1910s and 1940s birth cohorts. The second panel shows that the decline in the rank-rank measure is also unchanged by augmenting the income prediction with father's education. Thus, when we significantly improve our childhood income measures with an important predictor, the trends in mobility remain unchanged, providing some reassurance that systematic, unobserved within *father occupation* × *race* × *South* cell variation in income is not driving our results.

In Figure B.3, we again focus on the sub-sample of respondents that provide information about their father's educational attainment, showing how the IGE and rank-rank estimates change as we add more detail to the parental family income prediction. (This figure is analogous to Appendix Figure A.6, but it uses a sub-sample of respondents and specifically considers the importance of education as a predictor.) The first series only allows the measures to vary by occupation, but the second, third, and fourth series successively incorporate detail on the race, region, and educational attainment of the father. We see that the decline in the IGE and rank-rank estimates between the 1910s and 1940s cohorts is remarkably unchanged despite adding important information to the income prediction.[43] Table B.1 summarizes the results from this exercise, quantifying the decline between the 1910s and 1940s birth cohorts using these alternative ways of approximating parental income.

Similar to the previous exercise, for nine of our fifteen surveys, we have information on the *Census region* of birth or childhood. For respondents in this subsample, we can thus predict childhood income at the *father occupation* × *race* × *Census region* level, instead of collapsing region to South versus other. Appendix Figure B.4 shows how the baseline estimates vary as we transition to this subsample and to measures of predicted childhood income that vary at the regional level. The decline in persistence and the overall trends in mobility remain unchanged.

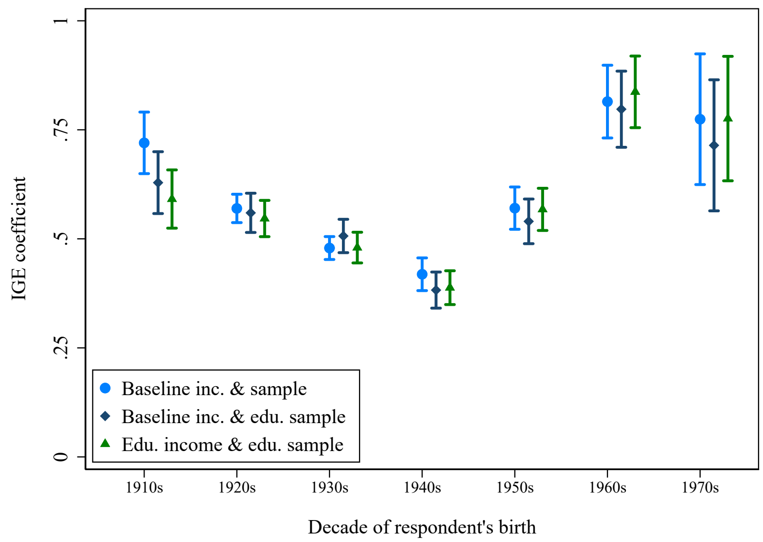
While we have shown robustness of our main result to using a richer set of predictors whenever our data allow, we now take a different approach to assessing the extent of potential bias due to unobserved within-cell variance. Essentially, we ask, even if we assume that *all* within-cell variance reflects true, permanent differences in childhood income, can we still detect a decline in intergenerational persistence between the 1910s and 1940s cohorts?

For each *father occupation* × *race* × *South* cell, we observe the actual family income values of all observations in that cell (i.e., from the appropriate Census or 1936 Expenditure Survey). We begin with a multiple-imputation estimation (see, e.g., Little and Rubin, 2019; Rubin, 1987), which uses the observed distribution of data in the Census to calculate various plausible values for the respondents' childhood income (conditional on the father's occupation, race, and Southern location). Note that for these exercises, we use the 1936 Survey and 1940 Census to impute parental income for respondents born in the 1910s–1930s birth cohorts (unlike our baseline approach). The first series of Appendix Figure B.5 shows the baseline IGE results and the second series shows the multiple-imputation-based results. While the multiple-imputation-based results are unsurprisingly attenuated (shown formally in Cortes-Orihuela *et al.* (2022)) and thus make it harder to detect changes over time, we can nonetheless see a decline in persistence between the 1910s and 1940s cohorts. Similarly, when we simply draw directly and non-parametrically from the empirical distribution of all observed family income values in a cell (the third series), we find similar results.

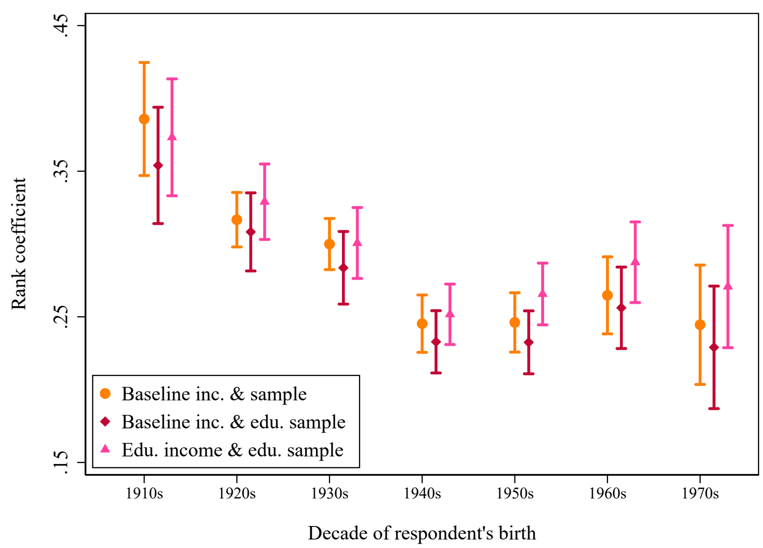
[43] An increase in the level of the rank-rank coefficient is unsurprising in this setting given that incorporating additional information into the income prediction likely increases the covariance between the rank of children and the rank of their fathers, while leaving the variance of the father's ranks fixed (by construction, given that ranks range from 0 to 100).

Figure B.2: Mobility by birth decade, adjusting predicted parental income for education

(a) Intergenerational elasticity



(b) Rank-rank coefficient

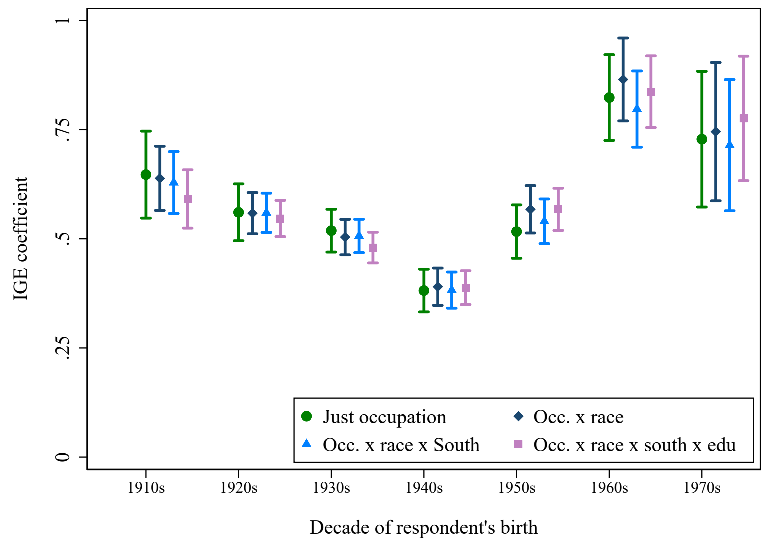


Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

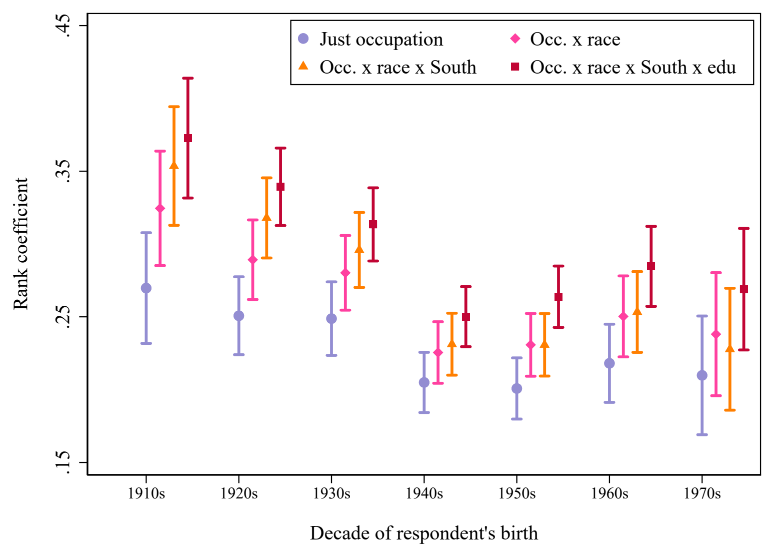
Notes: The first series in both panels reproduces the main IGE and rank-rank estimates using our baseline sample and income prediction. In the second series, we continue to use the baseline income prediction, but restrict the sample to respondents ages 30–50 who provided information on their fathers' education (available in 12 of the 15 surveys). In the third series, we use this smaller sub-sample in conjunction with income predictions that vary by a father's educational attainment. To predict parental income, we use family income conditional on father's characteristics from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) in all samples so that they have representative *race* $\times$ *sex* shares.

Figure B.3: Mobility measures by birth decade, adding detail to parental income prediction

(a) Intergenerational elasticity



(b) Rank-rank coefficient

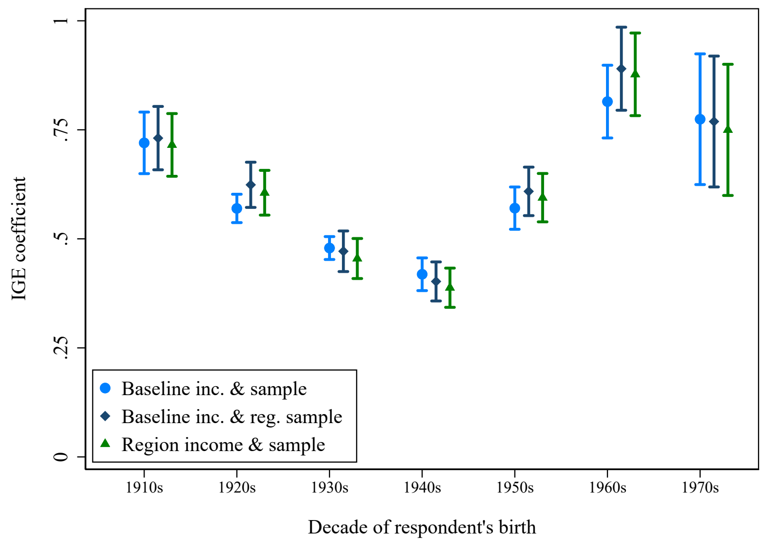


Sources: This figure combines 12 surveys in which respondents provide information on a father's educational attainment. Further detail is available in Appendix E.

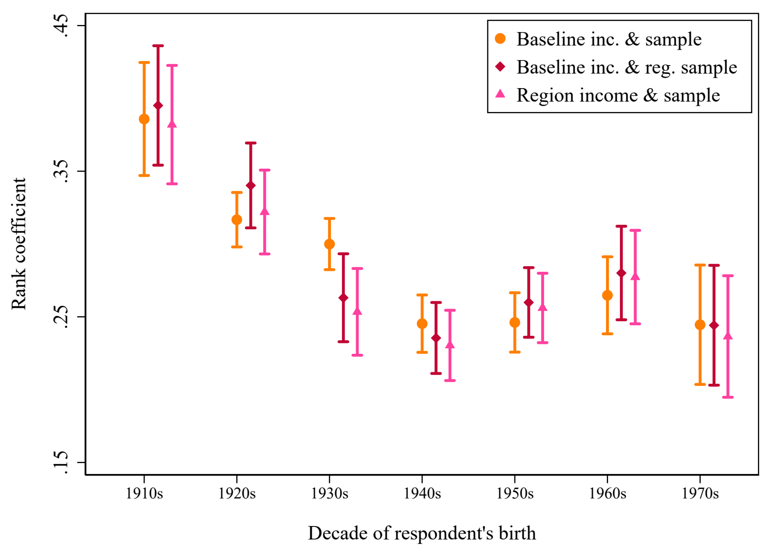
Notes: The IGE and rank-rank are based on the sample of respondents ages 30–50 who provided information about their fathers' educational attainment. The first series the parental income prediction only varies by a father's occupation. The second series allows predicted income to vary by father's occupation and race. The third series allows predicted income to vary by father's occupation, race, and Southern residence. The fourth series allows predicted income to vary by father's occupation, race, Southern residence, and father's educational level. To predict parental income, we use family income conditional on father's characteristics from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) in this sample so that they have representative *race* × *sex* shares.

Figure B.4: Mobility measures by birth decade, robustness to regional differences in parental income

(a) Intergenerational elasticity



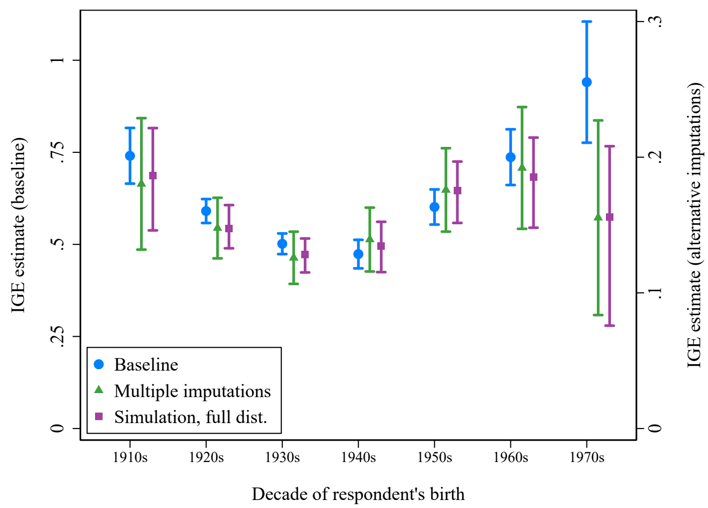
(b) Rank-rank coefficient



Sources: Data come from 15 different surveys, described in Section 2 and in further detail in Appendix E.

Notes: The first series in both panels reproduces the main IGE and rank-rank estimates using our baseline sample and income prediction. In the second series, we continue to use the baseline income prediction, but restrict the sample to respondents ages 30–50 who provided more detailed information on their fathers' region (available in 9 of the 15 surveys). In the third series, we use this smaller sub-sample in conjunction with income predictions that vary by a father's region (using the four Census regions). To predict parental income, we use family income conditional on father's characteristics from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) in all samples so that they have representative *race* × *sex* shares.

Figure B.5: Intergenerational elasticity by cohort, comparing baseline results with alternative imputation approaches



Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

Notes: The first series uses our baseline income prediction based on the average household incomes at the *occupation* × *race* × *South* level. The second and third series then allow for more variability in the income prediction. The second series imputes parental logged income based on occupation, race, and Southern residence using a multiple imputation approach with 100 imputations (see, e.g., Little and Rubin, 2019; Rubin, 1987; Schafer, 1997). In the third series, the parental generation is randomly assigned an income value within their *occupation* × *race* × *South* cell before estimating the IGE. This process is repeated 100 times, and the upper and lower bound of the confidence intervals plot the values corresponding to the 2.5$^{\text{th}}$ and 97.5$^{\text{th}}$ percentiles of the distribution of estimates. In all series, we predict parental income using the nearest Census to the respondent's childhood. Specifically, for the 1910–1930s birth cohorts, we use the 1940 Census with farmer and self-employed income from the 1936 Expenditure Survey. For the 1940s–1970s birth cohorts, we use the 1950–1980 Censuses, respectively.

Table B.1: Quantifying the 1910–1940 decline in the IGE and rank coefficient as we add information about fathers to their family income prediction

(a) Intergenerational elasticity

	(1) Occupation	(2) Occ. x race	(3) Occ. x race x South	(4) Occ. x race x South x edu
Parental income	0.6109***	0.6204***	0.6218***	0.5871***
	[0.0437]	[0.0326]	[0.0311]	[0.0286]
Inc. x (Year-1910)	-0.0035**	-0.0045***	-0.0047***	-0.0037***
	[0.0017]	[0.0014]	[0.0013]	[0.0012]
Observations	31,093	31,093	31,093	31,093

(b) Rank-rank coefficient

	(1) Occupation	(2) Occ. x race	(3) Occ. x race x South	(4) Occ. x race x South x edu
Parental rank	0.2869***	0.3405***	0.3813***	0.3986***
	[0.0169]	[0.0174]	[0.0177]	[0.0172]
Rank x (Year-1910)	-0.0022***	-0.0032***	-0.0042***	-0.0041***
	[0.0007]	[0.0007]	[0.0007]	[0.0007]
Observations	31,093	31,093	31,093	31,093

Notes: All estimates are based on the sample of respondents ages 30–50 who provided information about their fathers' educational attainment and were born before 1950. Each column varies the information used to predict parental income. In all specifications, we interact father family income (or rank) with a variable that measures the number of years between a respondent's birth year and 1911. All specifications include birth-year fixed effects. To predict parental income, we use family income conditional on father's characteristics from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares.

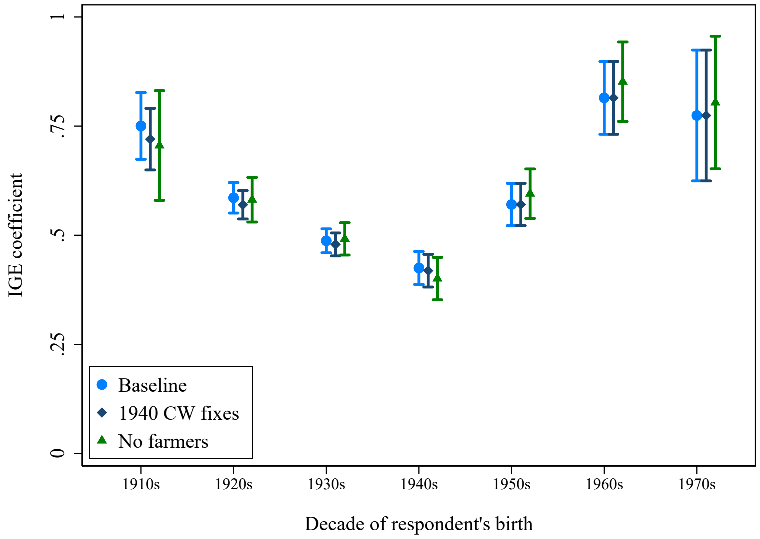
B.3 Farmer income

Our baseline measure of parental income acknowledges the difficulty in estimating farmer (and self-employed) income in the first half of the 20th century using conventional survey or Census data. We therefore use the 1900 Census of Agriculture (for farmers) as well as the 1936 Expenditure Survey (for farmers and self-employed) given the limitations of the 1940 Census for these groups.

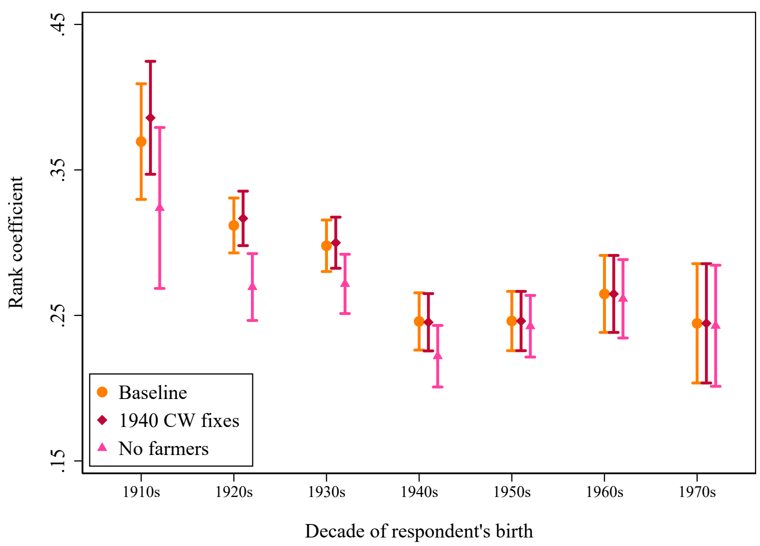
In this section we perform additional checks related to the prediction of income for farmer fathers. First, we follow Collins and Wanamaker (2022) and impute the income of farmers in 1940 using the income of farm laborers in 1940 as well as the ratio of farmer-to-farm-laborer income in the 1960 Census. We adjust the income of self-employed businessmen in 1940 using a similar approach. Appendix Figure B.6 shows that our main result of a marked decline in persistence remains unchanged when we follow this differing methodology. Second, also shown in Appendix Figure B.6, we simply drop farmers to ensure that our mobility patterns are not being entirely driven by this sizable population for which it is hard to estimate childhood income. Again, the conclusion that mobility increased substantially between the 1910s and 1940s birth cohorts is unchanged.

Figure B.6: Mobility by birth decade, incorporating various adjustments for farmers

(a) Intergenerational elasticity



(b) Rank-rank coefficient



Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

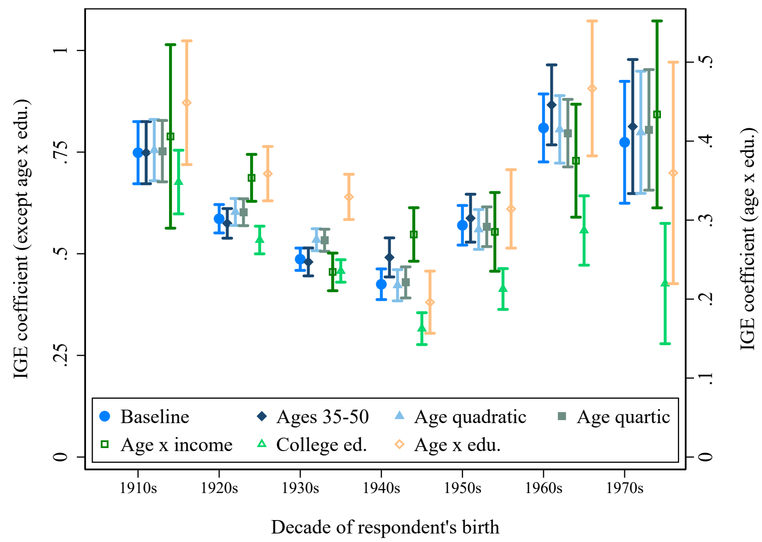
Notes: The first series in both panels reproduces the main IGE and rank-rank estimates using our baseline approach for predicting parental income (see Section 3.2 for more details). The second series uses the same methodology as Collins and Wanamaker (2022) to estimate the parental income of farmer and self-employed fathers in the 1910s–1940s birth cohorts. In both the first and second series, the IGE and rank-rank estimates are based on the baseline sample of respondents aged 30–50. We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares. The third series excludes all respondents whose fathers work in agricultural occupations; the remaining respondents are re-ranked in this sub-sample, and weights are constructed so that each birth cohort in this sub-sample also has representative *race* × *sex* shares.

B.4 Life-cycle bias

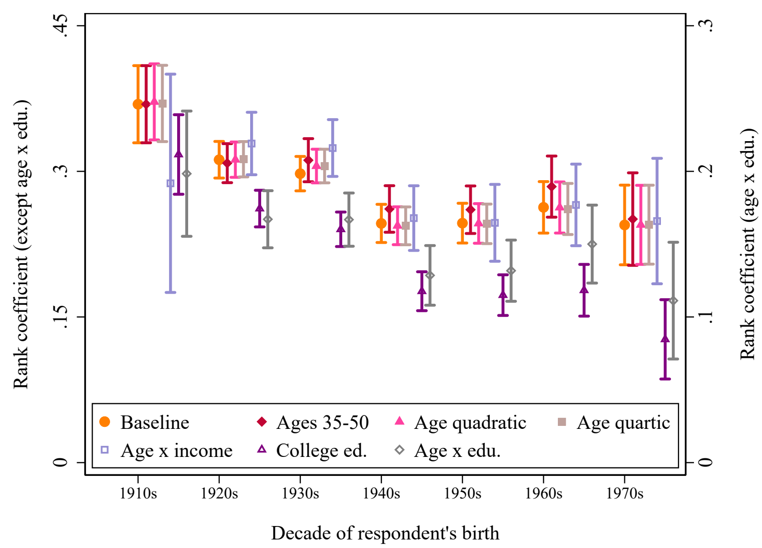
Various papers in this literature have noted that using current income to proxy for the adult children's lifetime earnings may bias estimates of mobility (see, e.g., Haider and Solon, 2006; Lee and Solon, 2009; Nybom and Stuhler, 2016). Recall we already restrict the sample to be observed at ages 30–50 to limit life-cycle effects. However, Appendix Figure B.7 shows the robustness of the main result to alternative specifications and sample restrictions that attempt to further minimize this life-cycle bias. We begin by restricting the sample to older respondents (i.e., those aged 35–50) whose total family income may be better approximations of their lifetime earnings. We also consider how the results change when we include quadratic age controls. We also follow Lee and Solon (2009) and include quartic age controls (relative to age 40) as well as interaction terms of the quartic terms with parental income. Finally, acknowledging the possibility of heterogeneous age-earnings profiles, we follow Nybom and Stuhler (2016) and include controls for college education, and we allow income to grow differentially with age depending on a respondent's education. Although none of these exercises can definitively eliminate life-cycle bias (i.e., cohorts likely have different life-cycle trajectories, and thus likely suffer from different degrees of bias), it is reassuring that the decline in persistence between the 1910s–1940s cohorts remains marked in all of these specifications.

Figure B.7: Mobility measures by birth decade, robustness to life-cycle bias

(a) Intergenerational elasticity



(b) Rank-rank coefficient



Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

Notes: The first series reproduces the baseline estimates in Figure 1. The second series restricts the sample to respondents ages 35–50. The third series includes a quadratic polynomial in age. The fourth series uses quartic polynomial in adult children's age minus 40. The fifth series uses the quartic polynomial in age as well as interactions of the adult child's age quartic with parental income. The coefficients displayed from this specification represent the age-40 measure of mobility as each cohort passes through that age. The sixth series returns to the quadratic specification, but controls for the respondent's college education. The seventh series (secondary y-axis) uses a quadratic polynomial in age and allows these coefficients to vary by a respondent's level of education (using six categories of educational attainment, as discussed in Appendix E).

B.5 Other robustness checks

Table 2 shows that the information needed to calculate predicted childhood income is not available for all respondents. This situation arises almost always because the respondent does not report father's occupation (presumably because she doesn't remember, chooses not to report it, or grew up without her father). In Appendix Figure B.8 we incorporate the roughly 2,000 respondents whose fathers were present but not working (e.g., retired, unemployed). The next series in this figure instead incorporates the roughly 4,750 respondents who provided information about their mother's occupation, assigning them measures of predicted childhood income based on mothers who were household heads in the 1940–1990 Censuses. Not surprisingly, the estimates hardly change for the 1910s–1940s cohorts, but from the 1950s onward we find more precise and slightly smaller persistence estimates (so the increase in the IGE from the 1950s to 1970s is smaller).[44] We also show robustness to a particularly extreme assumption about respondents who provided no information about either parent: that their household had zero income, or in other words, that their family had the lowest possible rank for predicted childhood income. Under all these various assumptions, the overall shape of the IGE and rank-rank estimates are unchanged over our sample period.

Next, one concern is that the decline in mobility might stem from pooling various different surveys across multiple years in our baseline sample. Appendix Figure B.9 shows that the results are quite stable after incorporating survey-by-year fixed effects (i.e., fixed effects for each of our fifteen surveys and a separate control for each survey-year for the surveys that span multiple years). Table 2 also shows that the share of survey respondents with income values that are top coded varies by cohort. Appendix Figure B.10 thus drops the three surveys that have the highest share of top coding in the adult children's generation and confirms that the 1910–1940 result is robust to their exclusion.

Appendix Figure B.11 shows the robustness of the main result to alternative weighting schemes: namely, using the provided survey weights without any additional adjustments for population shares and using no weights at all. We also consider robustness to re-weighting survey weights so that each birth cohort has representative *race* × *sex* × *education* × *age* shares where education refers to having a high school education and age refers to five-year age bins. In all of these checks, we continue to find an increase in mobility between the 1910s and 1940s cohorts.

One concern with this long-term view of mobility might be that mortality rates were high for early cohorts (for men and Black men in particular; see, e.g., Preston *et al.*, 2003), so that selection into the sample—i.e., remaining alive at ages 30–50—might be changing over time. If individuals who grow up in poorer households are those with higher mortality rates, then if anything, this decline in mortality would likely

[44] The second panel of Appendix Figure B.8 shows that the rank-rank correlation barely changes with this expanded sample, confirming the important role of the variance of parental income in inflating the IGE in our baseline sample.

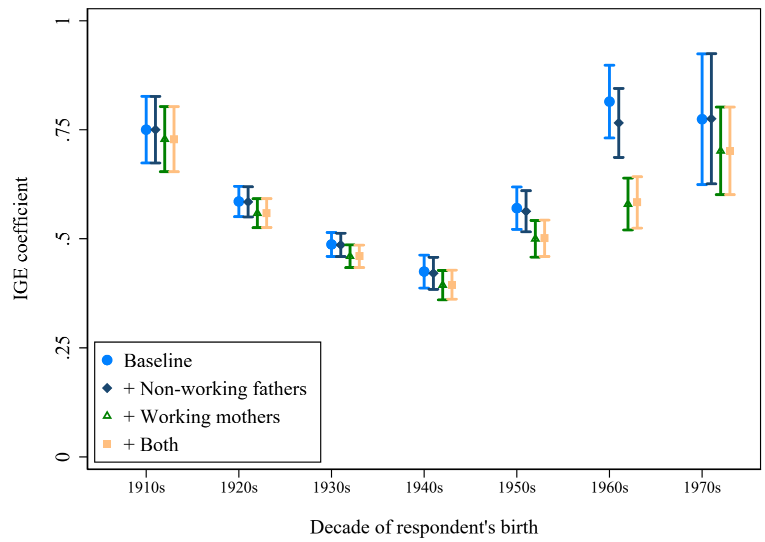
bias us against finding a decline in intergenerational persistence between the 1910s and 1940s birth cohorts (in the earliest cohorts, individuals born poor who are still alive at prime age would likely be positive selected, which would bias intergenerational persistence downward). Nevertheless, we still take seriously this consideration and Appendix Figure B.12 compares our baseline results to those that focus on individuals ages 30–45 and 30–40, both of which are less affected by differential mortality rates. The rise in mobility is unchanged in these sub-samples. Nonetheless, selection into fertility and mortality remains a concern for all intergenerational mobility estimates using historical data, including our recall-based method.

Another notable demographic change that took place in the 20th century was the change in household size for the mid-century cohorts (i.e., the Baby Boom). Appendix Figure B.13 adjusts the family income of adult children by self-reported household size and adjusts predicted parental income by household size in that *father occupation* × *race* × *South* cell when the respondent is ten years old. We see that this adjustment does not affect the rise in mobility between the 1910s and 1940s birth cohorts.

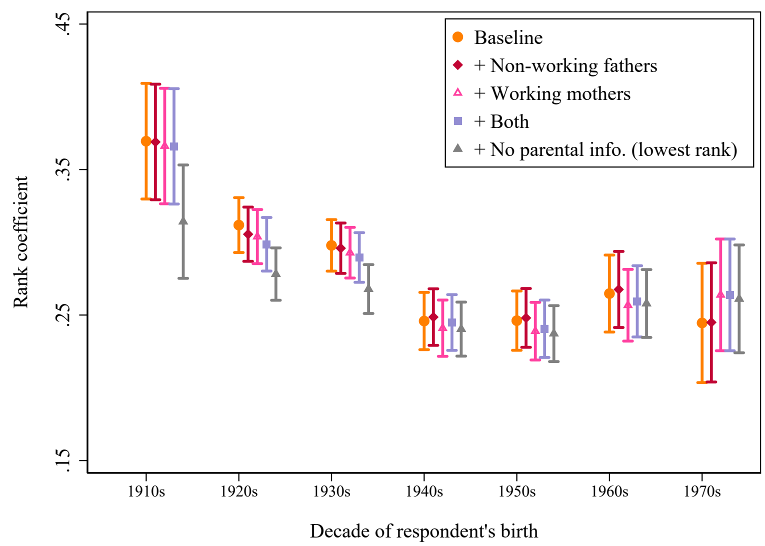
In Figure B.14 we present results that use alternative data sources for or alternative ways of predicting parental income. Recall that our baseline measure of predicted parental family income relies on measures of household income from various Censuses and other sources in the 20th century. We alter this preferred measure in a number of ways. First, instead of using measures of *household* income, we simply use individual-level wage information for fathers ages 30–50 to estimate parental income (second series). This measure more closely mirrors the construction of predicted income in related studies. Next, we use data from the closest Census to the survey respondents' childhood to approximate parental income (i.e., the 1940 Census with 1936 adjustments for the 1910s–1930s cohorts and the 1950–1980 Censuses for the 1940–1970 cohorts, respectively). Third, we return to our baseline measure and allow fathers with more children to receive greater weight when calculating average incomes at the *occupation* × *race* × *South* cell (i.e., weighting fathers by the number of children younger than 18 present in the household). Finally, we compare our results to simply using the 1950 IPUMS *occscore* variable (which, recall, pooled all adults and computed the nation-wide median income for each occupation). In all of these exercises, the decline in the IGE remains salient and in all but the last series, the *u*-shape of the IGE remains stark. The levels of the rank-rank measures all look quite comparable to each other, with the exception being the series that uses the 1950 IPUMS *occscore* variable, thus highlighting the value of incorporating information about race and region when predicting parental income.

Figure B.8: Mobility measures by birth decade, incorporating respondents with missing predicted parental income

(a) Intergenerational elasticity



(b) Rank-rank coefficient

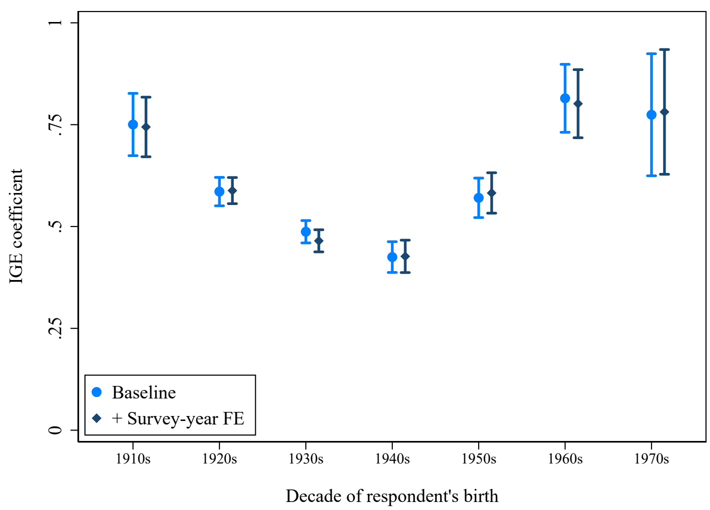


Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

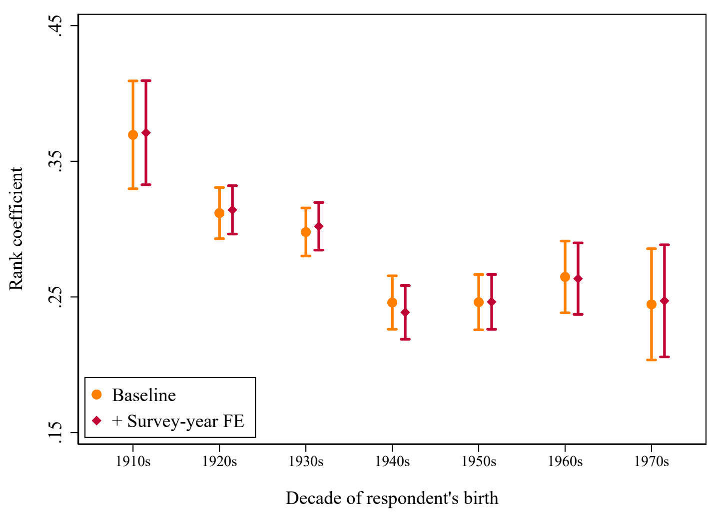
Notes: The first series in both panels reproduces the main IGE and rank-rank estimates using our baseline sample and income prediction (see Section 3.2 for more details). In the second series, we include respondents from the 15 surveys whose father was present but not working (e.g., unemployed, retired). In the third series, we instead include respondents from the 15 surveys who provided information about their mother's occupation. The fourth series includes respondents who provided information about their non-working father *or* about their mother's occupation (and if both pieces of information were provided, we predict family income based on the mother's occupation). More detail on the income prediction for non-working fathers and working mothers are in Appendix E. In the final series of the bottom panel, we assign all U.S.-born respondents ages 30–50 in our 15 surveys that still have missing predicted parental income the lowest possible rank (i.e., assuming that their household had zero income in childhood).

Figure B.9: Mobility by birth decade, robustness to including survey × year fixed effects

(a) Intergenerational elasticity



(b) Rank-rank coefficient

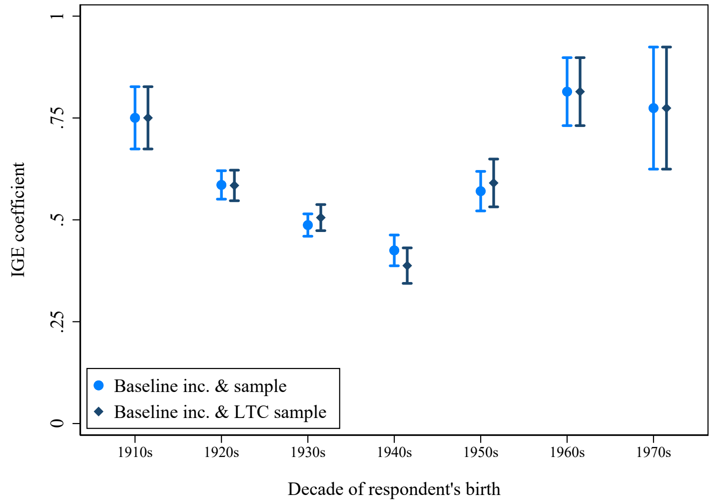


Sources: This figure combines 12 different surveys, which are described in Section 2 and in further detail in Appendix E.

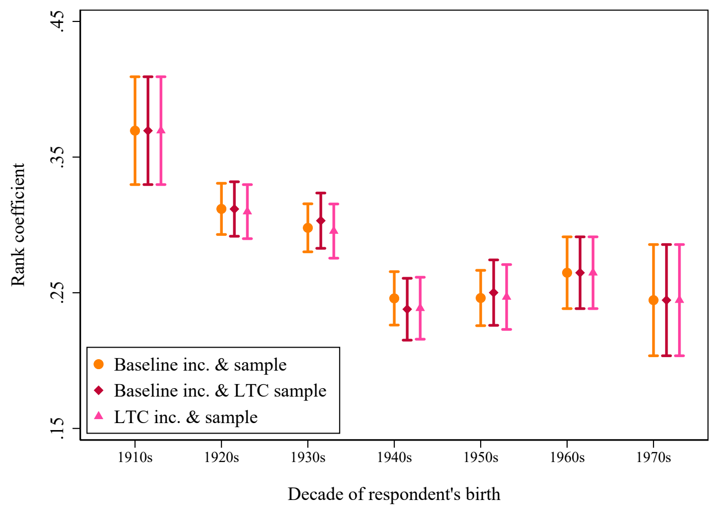
Notes: The first series in both panels reproduces the main IGE and rank-rank estimates using our baseline sample and measures of predicted parental family income. In the second series, we include survey-by-year fixed effects in the specification. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) in all samples so that they have representative *race* × *sex* shares.

Figure B.10: Mobility by birth decade, robustness to excluding surveys with high share of top coding of respondent income

(a) Intergenerational elasticity



(b) Rank-rank coefficient

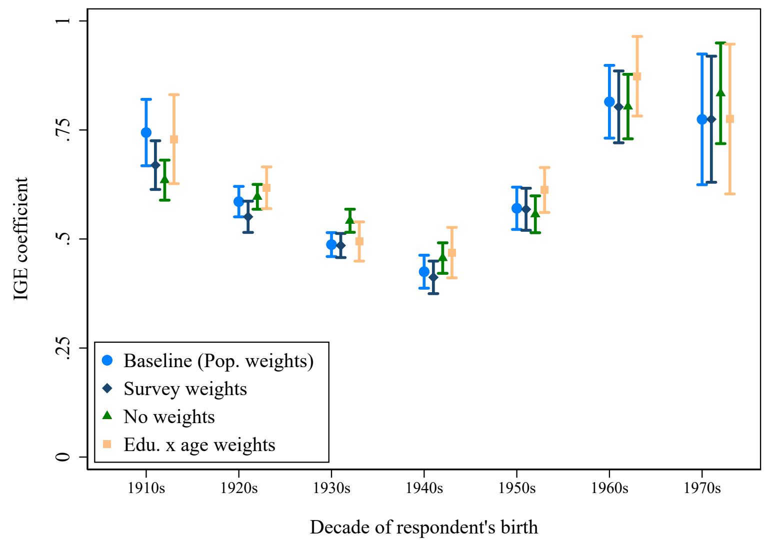


Sources: This figure combines 12 different surveys, which are described in Section 2 and in further detail in Appendix E.

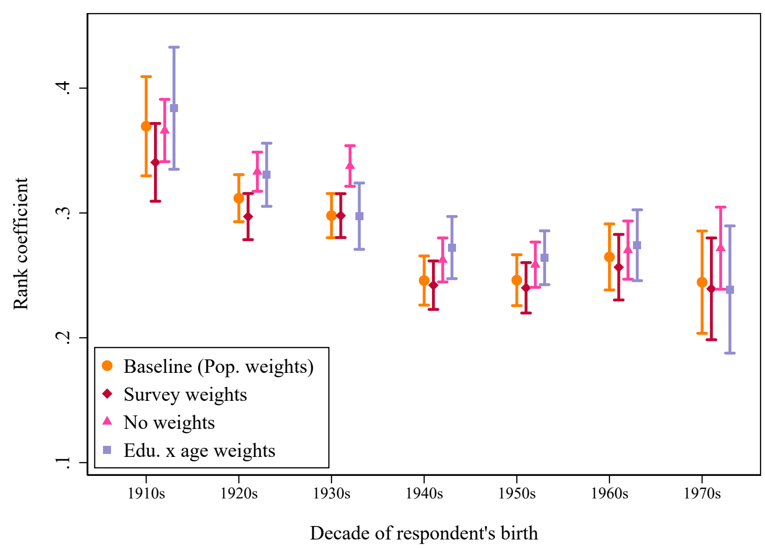
Notes: The first series in both panels reproduces the main IGE and rank-rank estimates using our baseline sample and measures of predicted parental family income. In the second series, we continue to use the baseline income measures, but restrict the sample to the twelve surveys with the lowest shares of top coding of respondents' income (i.e., excluding respondents who are not in the National Fertility Study, the National Survey of Families and Households, and the National Longitudinal Survey of Young Women). In the third series of panel (b), we use this smaller sub-sample and as well as measures of ranked parental income that are only based on this sub-sample. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) in all samples so that they have representative *race* × *sex* shares.

Figure B.11: Mobility by birth decade, robustness to weights

(a) Intergenerational elasticity



(b) Rank-rank coefficient

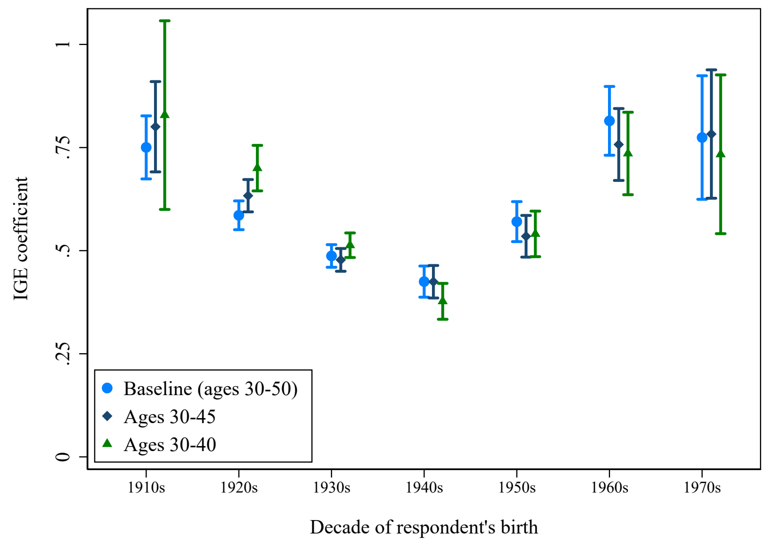


Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

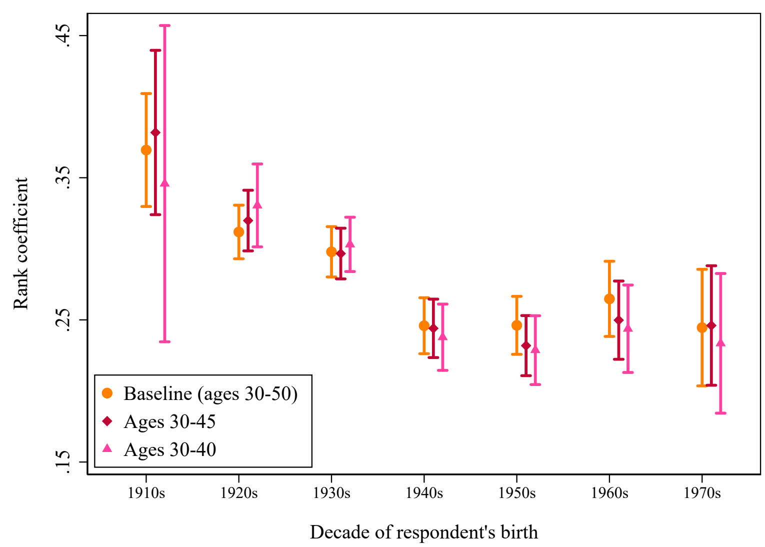
Notes: The estimates are based on the baseline sample of respondents ages 30–50. The first series in both panels reproduces the main IGE and rank-rank estimates using the baseline population-adjusted weights. In other words, in the first series, we re-weight survey weights so that each birth cohort has representative *race* × *sex* shares. The second series uses the provided survey weights (or a weight of one when no survey weight is available). The estimates from the third series are unweighted. The fourth series uses alternative population-adjusted weights in which the *race* × *sex* × *education* × *age* shares vary over time. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details).

Figure B.12: Mobility by birth decade, robustness to age group

(a) Intergenerational elasticity



(b) Rank-rank coefficient

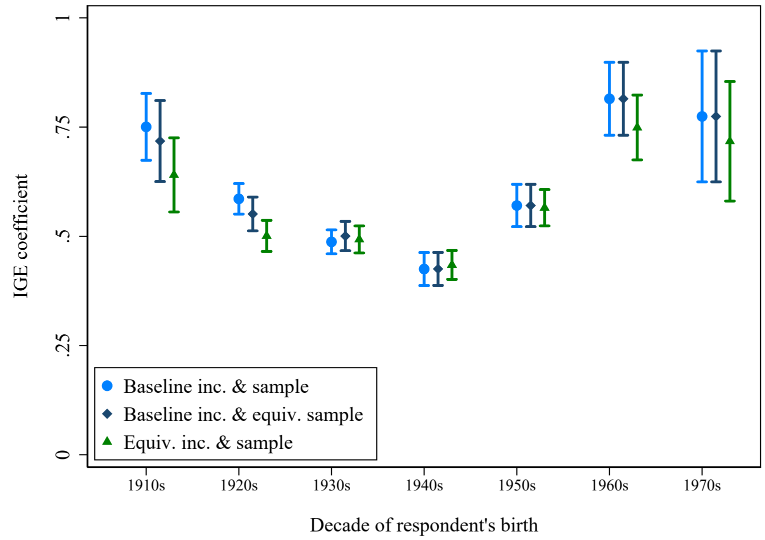


Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

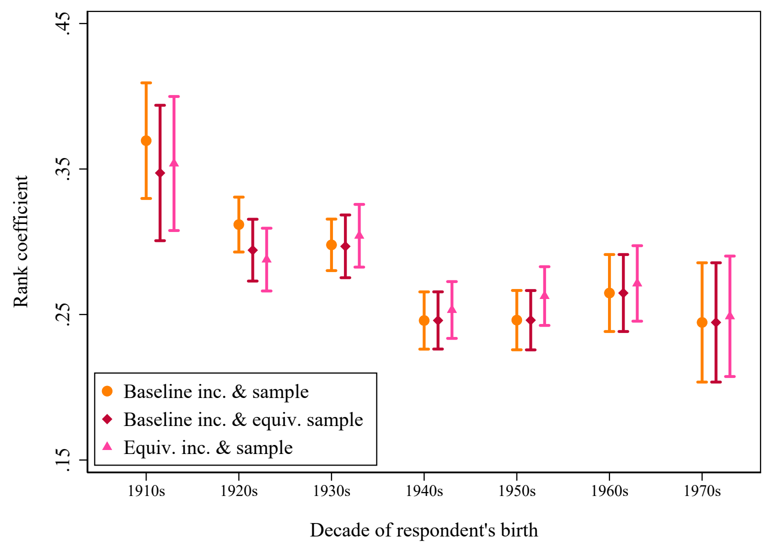
Notes: The estimates are based on the baseline sample of respondents aged 30–50. The first series in both panels reproduces the main IGE and rank-rank estimates using the baseline population-adjusted weights. In the second series, we restrict the sample to respondents aged 30–45. The third series further restricts the sample to those aged 30–40. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details).

Figure B.13: Mobility by birth decade, robustness to family size

(a) Intergenerational elasticity



(b) Rank-rank coefficient

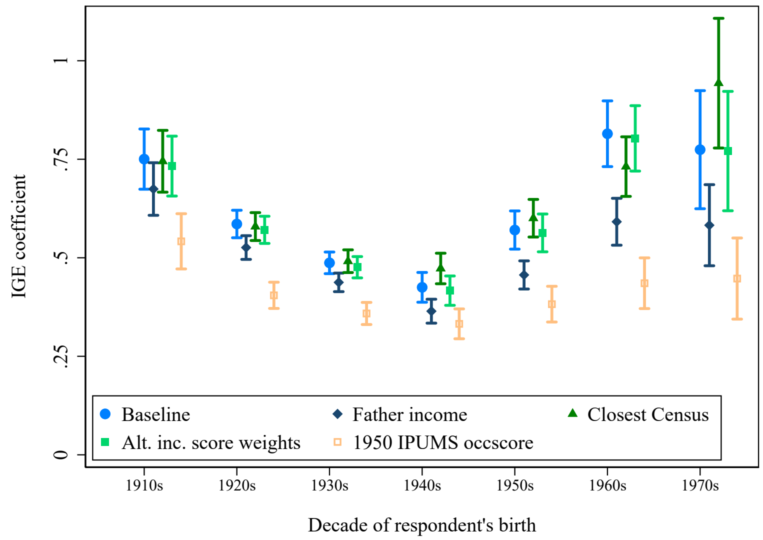


Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E. We utilize data from Ruggles *et al.* (2021) to construct income predictions and measures of household size.

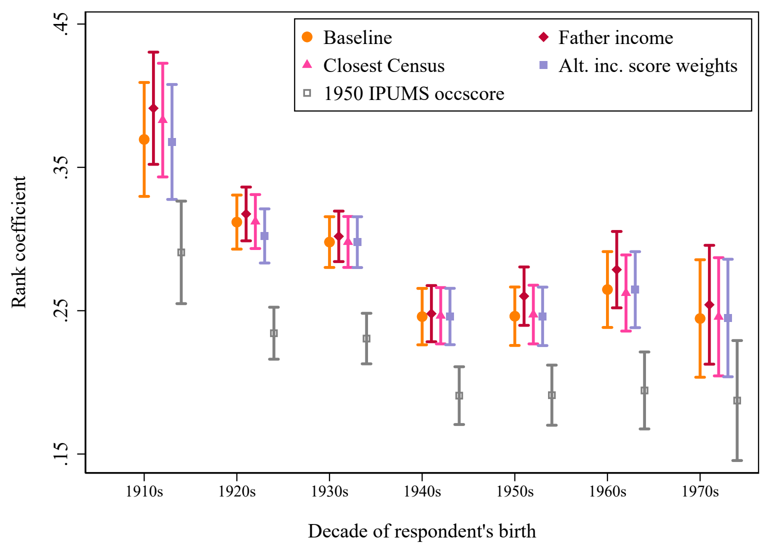
Notes: The first series in both panels reproduces the main IGE and rank-rank estimates using our baseline sample and income prediction (see Section 3.2 for more details). In the second series, we restrict the sample to respondents that provided information about their household size (84% of the baseline sample). In the third series, we use this sub-sample and adjust the income prediction to account for differences in household size. For the respondent's generation, we divide own family income by the square root of a respondent's household size at the time of the interview. For the parental generation, we divide the baseline income prediction by the square root of the median household size. Specifically, we use the 1920–1990 Censuses to construct the median household size when the respondent is 10 years old (taking the weighted average of the median household size in that *occupation* × *race* × *South* cell and allowing the weights to reflect the year in which the respondent is 10).

Figure B.14: Mobility by birth decade, incorporating various adjustments to predicted income

(a) Intergenerational elasticity



(b) Rank-rank coefficient



Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

Notes: All estimates in this figure are based on the baseline sample of respondents ages 30–50. The first series reproduces the main IGE and rank-rank estimates using our baseline income prediction (see Section 3.2 for more details). "Father income" refers to using individual (as opposed to household) income for fathers. "Closest Census" refers to using the closest Census relative to the survey respondent's childhood to calculate predicted income. In particular, we use the 1940 Census with the 1936 Expenditure Survey for the 1910s–1930s cohorts, the 1960 Census for the 1940s–1950s cohorts, the 1970 Census for the 1960s cohort, and the 1980 Census for the 1970s cohort. "Alt. inc. score weights" refers to using an income prediction in which fathers are weighted by the number of children in the household in the calculation of average family income. "1950 IPUMS occscore" refers to using the *occscore* variable from IPUMS. For more detail on the construction of these income predictions, see Appendix E.

C Assessing recall bias

Our estimates of mobility rely on survey respondents' recollection of their fathers' occupations. In this section, we consider the extent to which recall bias might be present in our estimates. We begin by comparing the fathers' occupations provided by male versus female respondents in our surveys. We then compare the fathers' occupations in our surveys to those of fathers in the decennial Censuses at the time that the respondents were growing up. We conclude by looking at the PSID—a survey that includes both retrospective questions as well as self-reported information about fathers' occupations when the respondent was growing up—to gauge the extent to which adult children's retrospective answers match fathers' self-reported occupations.

C.1 Comparing male and female survey respondents

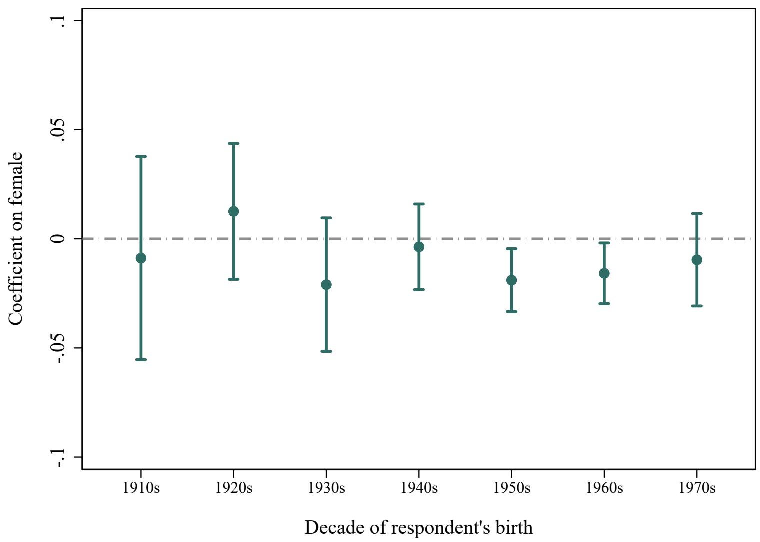
We begin by comparing the fathers' occupations reported by male and female respondents in our surveys. Roughly speaking, brothers and sisters grow up in the same families in the US, so adult men and women should report similar occupations for their fathers. Strictly speaking, small differences could arise between the predicted parental family income of men and that of women. If parents have sex-based stopping rules when making fertility decisions, then boys and girls might wind up growing up in systematically different families (as in Asher *et al.* (2018), using data from India). However, evidence for sex-based fertility patterns in the US is much weaker. Second, even if boys and girls grow up in identical families in terms of parental income, small differences might arise because men have higher mortality rates than women and thus selection into surviving into prime age could differ by gender (especially in our oldest cohorts, men are less likely to live until age 50).

These small potential discrepancies notwithstanding, we would be suspicious of any parental income estimate that gives significantly different estimates for male and female respondents. We thus regress the log as well as the rank of estimated parental income on a female dummy, separately for each of our birth decades, and report the results in Appendix Figure C.1. The coefficient on the female dummy is always close to zero and has no consistent sign. We repeat this analysis separately for white and Black respondents and report the results in Appendix Figure C.2. Again, we find no notable patterns or significant differences beyond what might be expected by chance. Importantly, these figures do not indicate that recall deteriorated between the 1910s and 1940s cohorts in a way that would drive our main result. Appendix Table C.1 shows the top five occupations reported by male and female respondents in each birth cohort. In all birth cohorts, at least four—if not all five—of the top occupations coincide between male and female respondents and in roughly the same order.

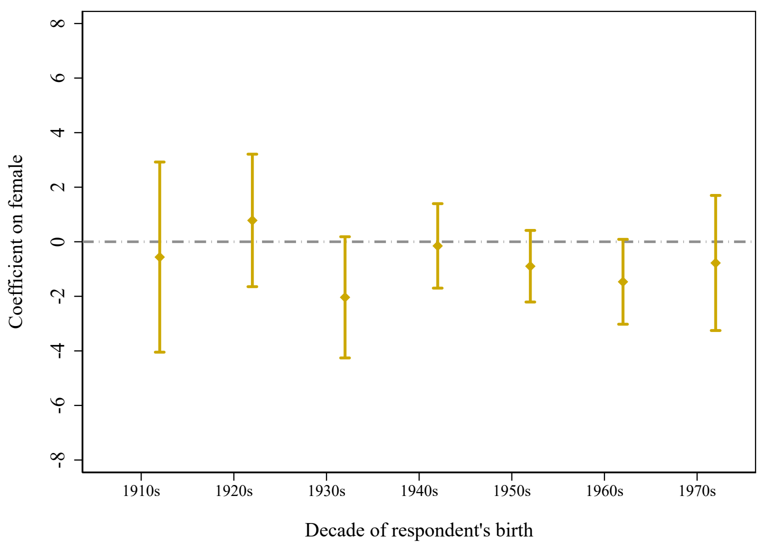
Finally, we can more directly compare survey respondents who are siblings, which occurs in four of our surveys and corresponds to around 7% of the baseline sample. As seen in Appendix Figure C.3, the predicted parental incomes implied by siblings' answers are highly correlated, providing another piece of evidence that individuals' recall of their fathers' occupation provides relatively accurate information about their upbringing.

Figure C.1: Differences in income prediction, by respondent sex and birth cohort

(a) Logged parental income



(b) Ranked parental income

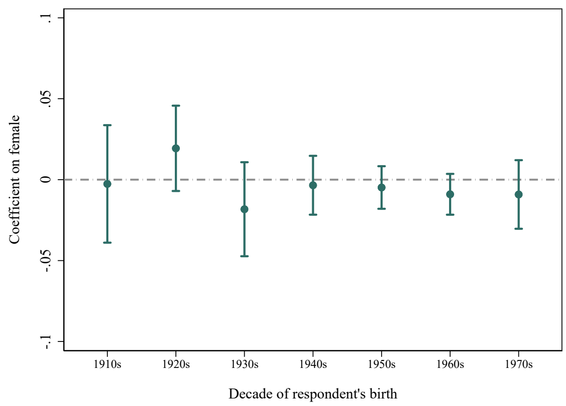


Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

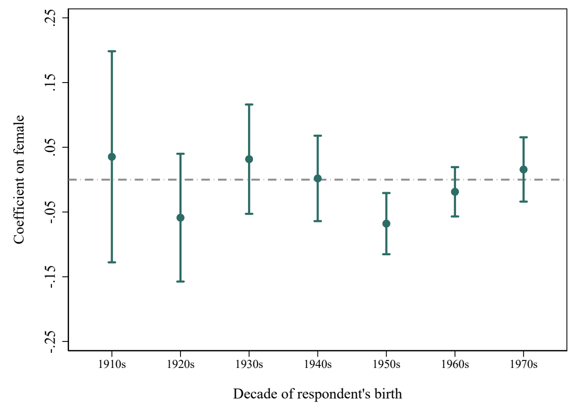
Notes: This figure uses our baseline sample ages 30–50 to regress logged and ranked predicted parental income on an indicator variable for whether a respondent is female. Survey-year fixed effects are included in both panels. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* $\times$ *sex* shares.

Figure C.2: Differences in income prediction, by respondent sex, race, and birth cohort

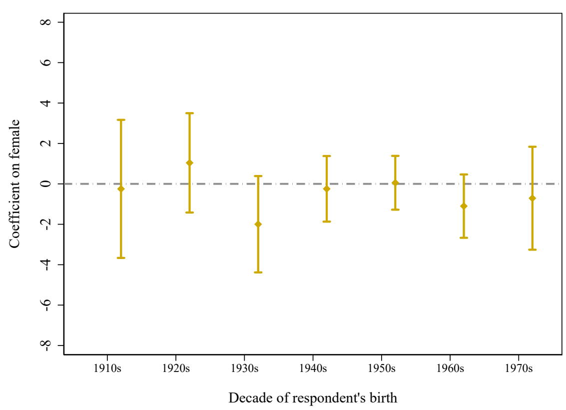
(a) Logged parental income, white respondents



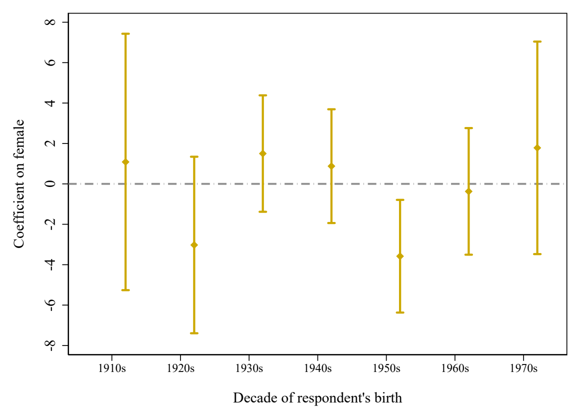
(b) Logged parental income, Black respondents



(c) Ranked parental income, white respondents



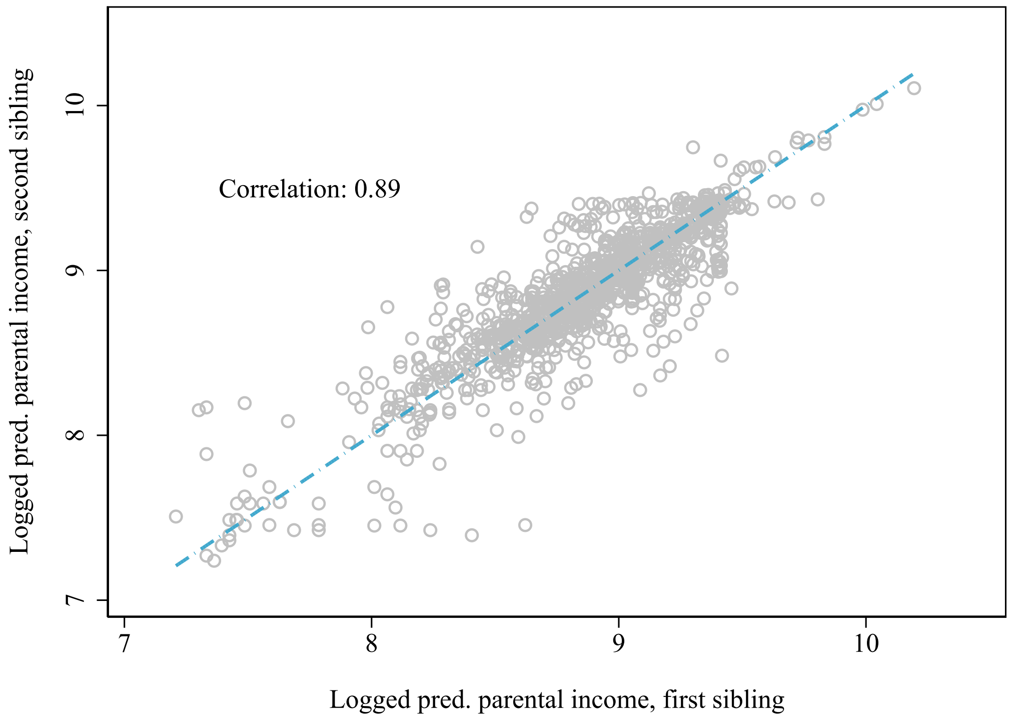
(d) Ranked parental income, Black respondents



Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

Notes: This figure uses our baseline sample of ages 30–50 to regress logged and ranked predicted parental income on an indicator variable for whether a respondent is female, separately by respondent race. Survey-year fixed effects are included in all panels. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares.

Figure C.3: Scatterplot of correlation in parental income prediction among siblings



Sources: This figure combines data from four different surveys which can include respondents who are siblings (the PSID, the NLS of Youth, and the NLS Young Men and Young Women surveys).

Notes: This figure restricts the baseline sample of respondents ages 30–50 to individuals with one sibling in the baseline sample (4% of the baseline sample). The figure plots the parental income prediction of one sibling against that of the other sibling based on their responses about their father's occupation. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). The 45-degree line is shown.

Table C.1: Top five occupations reported by male and female respondents, by birth cohort

Birth Cohort	Male Respondents	Share of male sample	Female Respondents	Share of female sample
1910s	1. Farm operator	0.35	1. Farm operator	0.36
	2. Craftsman (skilled)	0.15	2. Craftsman (skilled)	0.16
	3. Craftsman (semi-skilled)	0.14	3. Craftsman (semi-skilled)	0.12
	4. Unskilled laborer (non-farm)	0.07	4. Unskilled laborer (non-farm)	0.08
	5. Businessman (self-employed)	0.05	5. Businessman (self-employed)	0.08
1920s	1. Farm operator	0.24	1. Farm operator	0.28
	2. Craftsman (skilled)	0.18	2. Craftsman (semi-skilled)	0.18
	3. Craftsman (semi-skilled)	0.17	3. Craftsman (skilled)	0.13
	4. Unskilled laborer (non-farm)	0.07	4. Businessman (not self-employed)	0.08
	5. Businessman (self-employed)	0.06	5. Unskilled laborer (non-farm)	0.08
1930s	1. Farm operator	0.19	1. Farm operator	0.21
	2. Craftsman (skilled)	0.19	2. Craftsman (semi-skilled)	0.19
	3. Craftsman (semi-skilled)	0.17	3. Craftsman (skilled)	0.16
	4. Unskilled laborer (non-farm)	0.07	4. Businessman (not self-employed)	0.10
	5. Businessman (self-employed)	0.06	5. Unskilled laborer (non-farm)	0.08
1940s	1. Craftsman (skilled)	0.20	1. Craftsman (semi-skilled)	0.18
	2. Craftsman (semi-skilled)	0.17	2. Craftsman (skilled)	0.18
	3. Farm operator	0.12	3. Farm operator	0.11
	4. Businessman (not self-employed)	0.11	4. Businessman (not self-employed)	0.11
	5. Unskilled laborer (non-farm)	0.06	5. Unskilled laborer (non-farm)	0.07
1950s	1. Craftsman (skilled)	0.20	1. Craftsman (skilled)	0.18
	2. Craftsman (semi-skilled)	0.17	2. Craftsman (semi-skilled)	0.18
	3. Businessman (not self-employed)	0.13	3. Businessman (not self-employed)	0.11
	4. Farm operator	0.07	4. Unskilled laborer (non-farm)	0.07
	5. Unskilled laborer (non-farm)	0.06	5. Farm operator	0.07
1960s	1. Craftsman (skilled)	0.20	1. Craftsman (skilled)	0.20
	2. Craftsman (semi-skilled)	0.17	2. Craftsman (semi-skilled)	0.19
	3. Businessman (not self-employed)	0.14	3. Businessman (not self-employed)	0.12
	4. Unskilled laborer (non-farm)	0.05	4. Unskilled laborer (non-farm)	0.06
	5. Protective service officer	0.05	5. Protective service officer	0.05
1970s	1. Craftsman (skilled)	0.18	1. Craftsman (semi-skilled)	0.19
	2. Craftsman (semi-skilled)	0.15	2. Craftsman (skilled)	0.18
	3. Businessman (not self-employed)	0.13	3. Businessman (not self-employed)	0.13
	4. Protective service officer	0.08	4. Unskilled laborer (non-farm)	0.07
	5. Unskilled laborer (non-farm)	0.07	5. Protective service officer	0.07

Notes: Estimates are based on the baseline sample of respondents ages 30–50. We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares.

C.2 Comparing survey respondents' fathers to Census fathers

Next, we compare the occupations of fathers in the surveys to those of actual fathers in the Census in the years when the respondents were children. Ward (2023) warns that Census-takers made errors in recording the occupation variable, but we would still be worried if our respondents' recollection of their fathers' occupations differed dramatically from fathers' occupations in the Census during the years in which the respondents were growing up. In all of the exercises in this section, we consider both the earlier and later corresponding Censuses, when respondents were ages 0–10 and 11–20, respectively.

We begin by comparing the predicted family income of fathers in the surveys with the predicted family income of fathers in the Census. Appendix Figure C.4 regresses the estimates of logged parental income on a dummy for whether the father's income measure came from the surveys.[45] We note that the predicted family income of fathers in the surveys is slightly lower than that of fathers in the Census, but the point estimates are small. More importantly, there does not seem to be any pattern in how the estimates are changing, suggesting that recall bias is not improving or deteriorating across cohorts. This lack of a consistent pattern, especially in the first half of cohorts, suggests that the rise in mobility that we find is not driven by respondents' remembering their fathers' occupations differently across cohorts (or in other words, it does not seem to be the case that the rise in mobility is driven by measurement error changing monotonically over time).

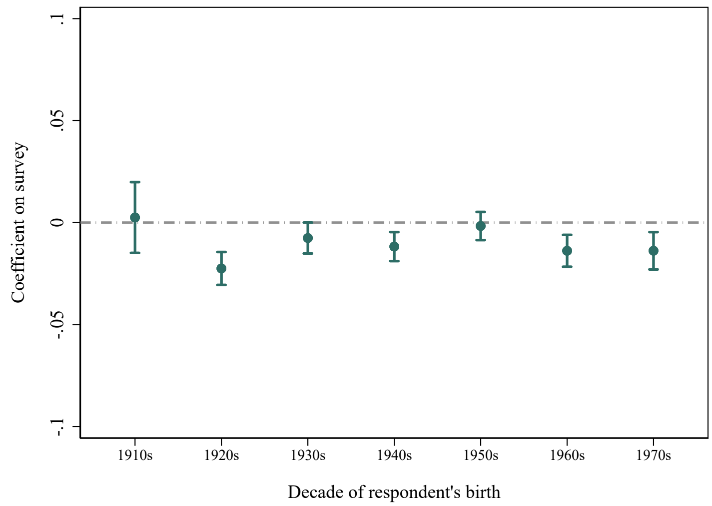
Appendix Tables C.2 and C.3 compare the mix of coarsened occupations that our respondents report their fathers as having to that of fathers in the Census. In both of these tables, we find that the share of fathers with each occupation are comparable to the corresponding shares in the Census.[46]

[45] We do not include an analogous exercise using ranked father's income. When calculating ranks for fathers in our main analysis, we rank a survey respondent's father relative to all fathers with children born in the same birth cohort. Because we are comparing these men to fathers in the decennial Census (most of whom have multiple children), it is not obvious which child's year of birth should be used in the ranking. Similarly, because we do not know the exact age of survey respondents' fathers, we cannot rank survey and Census fathers using their age.

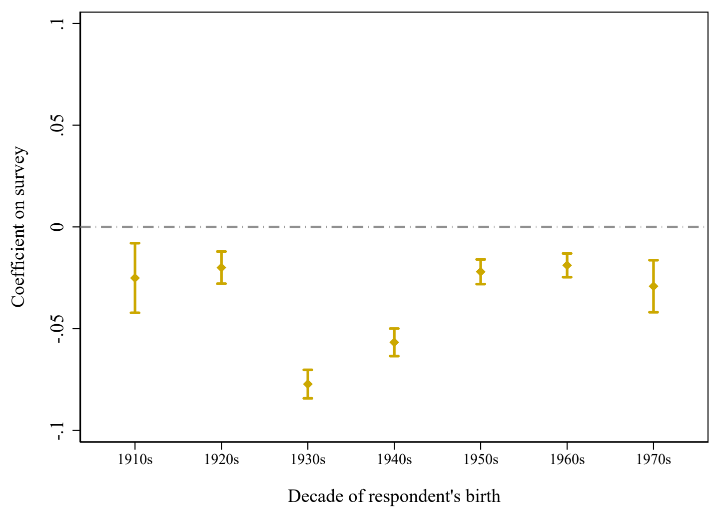
[46] The share of survey fathers who are farmers exceeds the analogous Census share in Appendix Table C.3. However, the decline of agriculture as a dominant occupation was occurring during this time period, so we would expect the Census shares to be lower than the survey shares when considering the later Census.

Figure C.4: Differences in logged family income prediction between Census fathers and survey respondents' fathers

(a) Using earlier Census



(b) Using later Census



Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

Notes: This figure uses our baseline sample of respondents ages 30–50 and Census fathers ages 30–50. In the top panel, the survey respondents' fathers are compared to the fathers in the Census when the respondents would have been between the ages of 1 and 10 (e.g., the fathers of survey respondents born in 1910–1919 are compared to 1920 Census fathers). In the bottom panel, the survey respondents' fathers are compared to fathers in the Census when the respondents would have been between the ages of 11 and 20. Survey respondents in the top panel are assigned a parental income prediction from the decade closest in time to when they fell in the 1–10 and 11–20 age range, respectively (if there is no nearest source, the respondent is assigned a weighted average of the two data sources closest to the desired age range). To predict parental income for Census fathers, we assign fathers a measure of predicted income using the nearest available data source (with the exception of fathers in the 1920, 1930, and 1950 Censuses who receive a weighted average of the two nearest data sources). All income predictions are conditional on father's occupation, race, and region (South vs. elsewhere).

Table C.2: Occupations of Survey Respondents' Fathers and Census Fathers (Using Earlier Census), by Birth Cohort

	1910–1919		1920–1929		1930–1939		1940–1949		1950–1959		1960–1969	
	Census (1920)	Survey	Census (1930)	Survey	Census (1940)	Survey	Census (1950)	Survey	Census (1960)	Survey	Census (1970)	Survey
Coarsened Occupations												
Accountants and auditors	0.34	0.55	0.52	0.48	0.73	0.58	0.88	0.63	0.99	1.16	1.12	0.97
Clergymen	0.46	0.62	0.41	0.72	0.46	0.62	0.40	0.60	0.44	0.77	0.49	0.57
Public-school teachers	0.34	0.62	0.48	0.46	1.05	0.52	0.85	0.89	1.14	1.38	2.03	2.13
Dentists	0.19	0.31	0.24	0.17	0.11	0.23	0.21	0.13	0.21	0.24	0.25	0.19
Physicians and surgeons	0.51	0.69	0.40	0.27	0.23	0.43	0.59	0.48	0.62	0.90	0.67	0.55
Engineers	0.53	1.10	0.72	0.90	0.84	1.09	1.56	2.12	2.58	3.41	3.48	3.83
Lawyers and judges	0.44	0.31	0.45	0.43	0.31	0.49	0.51	0.48	0.51	0.64	0.67	0.72
Social and welfare workers	0.04	0.02	0.03	0.03	0.06	0.08	0.08	0.08	0.12	0.12	0.18	0.21
Nurses (trained or student)	0.00	0.01	0.01	0.05	0.02	0.04	0.01	0.08	0.03	0.06	0.16	0.05
Other professional and technical	0.58	0.41	0.74	0.67	1.11	0.97	1.61	1.77	2.43	3.33	4.63	3.86
Semi-professional	0.69	0.85	0.88	0.66	0.91	1.18	1.49	1.76	2.35	2.21	3.01	2.65
Businessmen (self-employed)	6.44	6.43	6.35	4.29	1.09	3.85	6.52	3.19	4.29	2.82	3.28	2.97
Businessmen (not self-employed)	3.90	4.73	5.24	7.41	6.70	8.08	6.18	11.43	8.09	12.93	9.61	13.55
Bookkeeper	0.48	0.19	0.38	0.34	0.52	0.33	0.30	0.26	0.25	0.19	0.45	0.15
Stenographers	0.08	0.28	0.14	0.18	0.14	0.09	0.16	0.15	0.12	0.11	0.19	0.05
Other clerical workers	3.17	1.65	3.41	2.92	5.47	2.98	4.83	3.71	5.28	3.54	5.03	3.12
Sales: higher-status	0.96	1.33	1.41	1.09	1.01	1.01	1.11	1.25	1.52	1.72	2.05	2.08
Sales: inside sales	2.93	1.90	4.33	2.19	7.99	2.69	4.85	3.57	5.09	3.71	4.96	3.83
Sales: lower-status	0.17	0.39	0.19	0.18	0.08	0.20	0.05	0.07	0.05	0.06	0.06	0.06
Foremen	1.96	1.78	2.14	2.24	2.47	3.15	2.62	3.30	3.30	3.77	4.00	3.74
Craftsmen (skilled)	17.10	15.81	17.17	16.36	18.23	18.15	18.16	19.76	19.03	19.36	18.83	19.89
Craftsmen (semi-skilled)	13.46	13.41	15.07	17.80	22.01	18.36	20.41	18.04	20.46	16.70	18.74	17.13
Protective service officers	0.96	1.18	1.32	2.09	1.87	2.17	2.35	3.54	3.72	4.48	4.20	4.86
Private household workers	0.10	0.03	0.09	0.63	0.27	0.34	0.04	0.25	0.03	0.08	0.02	—
Other service workers	1.84	1.83	2.44	2.69	3.29	2.85	2.54	2.77	2.41	2.84	3.19	2.63
Farm laborers	3.20	1.84	3.37	2.81	4.38	3.32	2.13	2.67	1.37	1.68	1.04	1.18
Unskilled non-farm laborers	10.71	7.55	10.95	7.43	14.98	7.20	6.27	6.29	5.47	5.72	4.60	5.40
Farm operators	27.22	34.18	20.18	24.50	3.65	18.99	10.73	10.72	5.01	6.07	2.61	3.63

Notes: For survey estimates, we use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares. Census shares are weighted using provided weights. Census samples include all Black and white fathers ages 30–50. The fathers in the Census are compared to survey respondents' fathers when the respondents would have been between the ages of 1 and 10 at the time of the Census (e.g., survey respondents born in 1910–1919 are compared to 1920 Census respondents.)

Table C.3: Occupations of Survey Respondents' Fathers and Census Fathers (Using Later Census), by Birth Cohort

	1910–1919		1920–1929		1930–1939		1940–1949		1950–1959		1960–1969	
	Census (1930)	Survey	Census (1940)	Survey	Census (1950)	Survey	Census (1960)	Survey	Census (1970)	Survey	Census (1980)	Survey
Coarsened Occupations												
Accountants and auditors	0.52	0.55	0.73	0.48	0.88	0.58	0.99	0.63	1.12	1.16	1.05	0.97
Clergymen	0.41	0.62	0.46	0.72	0.40	0.62	0.44	0.60	0.49	0.77	0.57	0.57
Public-school teachers	0.48	0.62	1.05	0.46	0.85	0.52	1.14	0.89	2.03	1.38	2.83	2.13
Dentists	0.24	0.31	0.11	0.17	0.21	0.23	0.21	0.13	0.25	0.24	0.28	0.19
Physicians and surgeons	0.40	0.69	0.23	0.27	0.59	0.43	0.62	0.48	0.67	0.90	0.79	0.55
Engineers	0.72	1.10	0.84	0.90	1.56	1.09	2.58	2.12	3.48	3.41	2.96	3.83
Lawyers and judges	0.45	0.31	0.31	0.43	0.51	0.49	0.51	0.48	0.67	0.64	0.92	0.72
Social and welfare workers	0.03	0.02	0.06	0.03	0.08	0.08	0.12	0.08	0.18	0.12	0.29	0.21
Nurses (trained or student)	0.01	0.01	0.02	0.05	0.01	0.04	0.03	0.08	0.16	0.06	0.24	0.05
Other professional and technical	0.74	0.41	1.11	0.67	1.61	0.97	2.43	1.77	4.63	3.33	4.39	3.86
Semi-professional	0.88	0.85	0.91	0.66	1.49	1.18	2.35	1.76	3.01	2.21	3.55	2.65
Businessmen (self-employed)	6.35	6.43	1.09	4.29	6.52	3.85	4.29	3.19	3.28	2.82	3.73	2.97
Businessmen (not self-employed)	5.24	4.73	6.70	7.41	6.18	8.08	8.09	11.43	9.61	12.93	12.31	13.55
Bookkeeper	0.38	0.19	0.52	0.34	0.30	0.33	0.25	0.26	0.45	0.19	0.20	0.15
Stenographers	0.14	0.28	0.14	0.18	0.16	0.09	0.12	0.15	0.19	0.11	0.08	0.05
Other clerical workers	3.41	1.65	5.47	2.92	4.83	2.98	5.28	3.71	5.03	3.54	5.08	3.12
Sales: higher-status	1.41	1.33	1.01	1.09	1.11	1.01	1.52	1.25	2.05	1.72	2.01	2.08
Sales: inside sales	4.33	1.90	7.99	2.19	4.85	2.69	5.09	3.57	4.96	3.71	4.06	3.83
Sales: lower-status	0.19	0.39	0.08	0.18	0.05	0.20	0.05	0.07	0.06	0.06	0.10	0.06
Foremen	2.14	1.78	2.47	2.24	2.62	3.15	3.30	3.30	4.00	3.77	4.55	3.74
Craftsmen (skilled)	17.17	15.81	18.23	16.36	18.16	18.15	19.03	19.76	18.83	19.36	17.13	19.89
Craftsmen (semi-skilled)	15.07	13.41	22.01	17.80	20.41	18.36	20.46	18.04	18.74	16.70	16.97	17.13
Protective service officers	1.32	1.18	1.87	2.09	2.35	2.17	3.72	3.54	4.20	4.48	4.46	4.86
Private household workers	0.09	0.03	0.27	0.63	0.04	0.34	0.03	0.25	0.02	0.08	0.01	—
Other service workers	2.44	1.83	3.29	2.69	2.54	2.85	2.41	2.77	3.19	2.84	3.08	2.63
Farm laborers	3.37	1.84	4.38	2.81	2.13	3.32	1.37	2.67	1.04	1.68	0.79	1.18
Unskilled non-farm laborers	10.95	7.55	14.98	7.43	6.27	7.20	5.47	6.29	4.60	5.72	4.44	5.40
Farm operators	20.18	34.18	3.65	24.50	10.73	18.99	5.01	10.72	2.61	6.07	1.97	3.63

Notes: For survey estimates, we use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares. Census shares are weighted using provided weights. Census samples include all Black and white fathers ages 30–50. The fathers in the Census are compared to survey respondents' fathers when the respondents would have been between the ages of 11 and 20 at the time of the Census (e.g., survey respondents born in 1910–1919 are compared to 1930 Census respondents.)

C.3 Assessing recall bias using the PSID

In this section, we utilize the unique nature of the PSID to consider the extent to which retrospective questions convey accurate information about a father's income level. In particular, the PSID has both retrospective questions about a respondent's father's occupation as well as self-reported father's occupations and income when the respondent was growing up (i.e., in earlier waves of the survey). We focus on the 1997–2015 waves of the PSID because 1997 is the first year in which the retrospective questions are asked with sufficient detail (i.e., 3-digit occupation codes), so that they can be mapped to our coarsened occupations.

The way that we verify the retrospective answers is by looking at individuals who were household heads at some point between 1997–2015 and who were thus asked about their father's occupation while they were growing up. Then, using the Family Identification Mapping System (FIMS) provided by the PSID, we can find these individuals' fathers in earlier waves of the survey and see the fathers' self-reported (coarsened) occupations between the ages of 25–50 (i.e., when the respondents were growing up). We can then see whether the retrospective answers in 1997–2015 matched any of the self-reported occupations in earlier survey years. Note that we often see multiple observations of father's self-reported occupation, as household heads were asked about their current occupation during each survey wave.

We find that for 81% of adult children, their retrospective answers coincided with one of the self-reported occupations of their fathers during their childhood.[47] We can also then see what the most common mistakes were in identifying occupation (in other words, conditional on a respondent mis-reporting his/her father's occupation, what did the adult child typically report versus what did the father typically report). The four most common mistakes—which account for roughly 20% of all mistakes—are the respondents reporting that their fathers were skilled craftsmen, semi-skilled craftsmen, or unskilled non-farm laborers, when instead the father reported one of the other occupations on this same list.[48]

Even if one-in-five respondents are mis-reporting their fathers' occupations, it might still be the case that the retrospective answers convey accurate information about a father's income level. Appendix Figure C.5 plots the predicted income of fathers using the retrospective answers against the predicted income of fathers using self-reported occupations when they were around 40 years old. Both panels of this figure confirm that respondents' retrospective answers are highly correlated with fathers' self-reported answers, and thus convey similar information about the respondents' income level during their upbringing. Importantly, it also does not appear to be the case that respondents with poorer or richer fathers tend to differentially provide inaccurate

[47] There are some instances (roughly 10% of respondents) in which the adult children's retrospective answers change across waves (for example, as a result of re-interviews due to changing family composition), so we consider all of the retrospective answers provided.

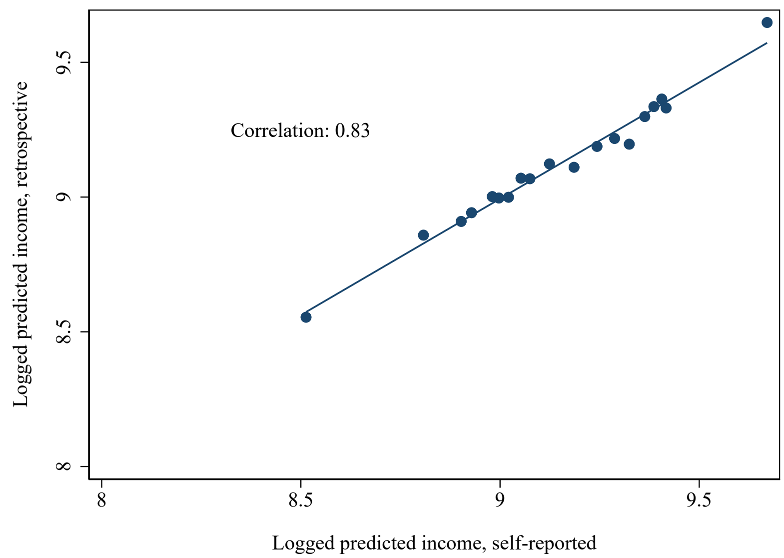
[48] To find the most common mistakes, we first select the 20% of respondents who were unable to accurately report any of their fathers' occupations. We then compare the modal retrospective answer in the data to the modal self-reported occupation of fathers between the ages of 30–50.

information. Appendix Table C.4 regresses the five-year average of a father's self-reported family income on alternative ways of measuring that father's income level. The coefficient of 1 in column 2, which uses the retrospective answer provided by the adult child, highlights that the retrospective answers seem to be reliable measures of a father's permanent income.

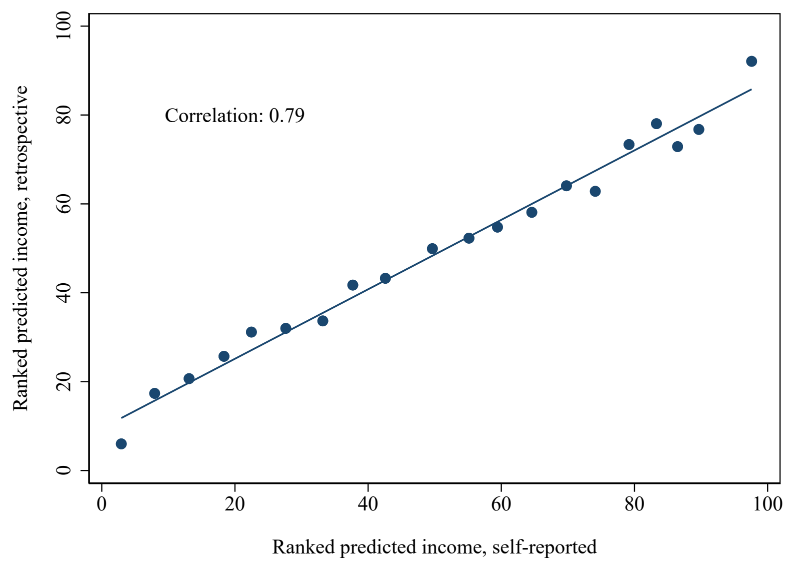
Moreover, most of the estimates in the historical intergenerational mobility literature come from linked Census data (20 or 30 years apart) and use one year of a father's occupation to predict their income. To get a better sense of how estimates that use our retrospective approach differ from those that use the typical approach in the literature, Appendix Table C.5 shows the IGE and rank-rank coefficient from using the adult children's retrospective answers in 1997 (column 1) versus using one year of father's self-reported occupation roughly thirty years earlier (column 2). The mobility estimates are similar using the two approaches. Ward (2023) notes the measurement error that can arise from using one year of father's occupation, so the last column of this table also reports the mobility estimates from averaging the predicted income using three self-reported father's occupations around 1970.

Figure C.5: Comparison of father's family income prediction using adult children's retrospective answers and father's self-reported occupations

(a) Logged predicted income



(b) Ranked predicted income



Sources: This figure uses the 1968–2015 Panel Study of Income Dynamics.

Notes: These figures are bin-scatter depictions of the predicted family income of fathers using the retrospective answers (y-axis) of adult children versus using fathers' self-reported answers in earlier survey waves (x-axis). The y-axis assigns fathers an income prediction using the modal retrospective occupation reported by respondents. The x-axis assigns fathers an income prediction using the five self-reported occupations closest to age 40. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from the 1970 Census.

Table C.4: Relationship between 5-year average of father's self-reported family income and various other ways to measure father's family income, 1997 PSID

	Predicted Income			Actual Income
	(1) Self-reported, in 1970	(2) Retrospective	(3) Self-reported, 1 year, age 40	(4) Self-reported, 1 year, age 40
Logged income	1.045	1.016	1.011	0.572
	[0.113]	[0.118]	[0.119]	[0.161]
Observations	898	898	898	898
R-squared	0.35	0.33	0.33	0.65

Notes: This table regresses the 5-year average of father's self-reported logged family income on four alternative ways to measure father's family income level (denoted in the column headers). The sample used is the fathers of household heads ages 30–50 who provided a retrospective answer in 1997. We include fathers who can be located in an earlier wave of the survey and who had at least five years of available income and occupation information between the ages of 30–50. The dependent variable is the average of five years of father's logged family income closest to age 40. Column 1 uses the income prediction associated with father's self-reported occupation around 1970 as the independent variable. Column 2 uses the income prediction corresponding to the retrospective answer provided by the household head about their father. Column 3 uses the income prediction corresponding to the father's self-reported occupation closest to age 40. Column 4 uses the family income of the father in one year closest to age 40. To predict income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from the 1970 Census. All estimates are weighted using 1997 cross-sectional weights.

Table C.5: IGE and rank coefficient using various ways of measuring parental income, 1997 PSID

(a) Logged income

	(1) Retrospective	(2) Self-reported, 1 year	(3) Self-reported, 3 years
IGE	0.586	0.638	0.650
	[0.106]	[0.100]	[0.101]
Observations	1,061	1,061	1,061

(b) Ranked income

	(1) Retrospective	(2) Self-reported, 1 year	(3) Self-reported, 3 years
Rank-rank correlation	0.224	0.230	0.223
	[0.039]	[0.039]	[0.039]
Observations	1,061	1,061	1,061

Notes: This table reports estimates of the IGE and rank-rank coefficients from specifications that use respondents' retrospective answers about their fathers' occupations (column 1) and fathers' self-reported answers about their occupations in earlier survey waves (columns 2 and 3). The sample used is household heads ages 30–50 who provided a retrospective answer in 1997 and whose father can be located in an earlier wave of the survey. Column 1 uses the provided retrospective answers. Column 2 uses the self-reported occupation of fathers ages 30–50 roughly thirty years earlier (around 1970). The last column uses three years of self-reported occupations (between 1968–1972) and takes an average of the three corresponding income predictions. To predict income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from the 1970 Census. All estimates are weighted using 1997 cross-sectional weights.

D Two-Sample Estimates

In this Appendix, we discuss the econometrics of our methodology. We begin by showing formally the sources of differences between OLS estimates (which are infeasible in the historical period, as we do not observe parental income) and the two-sample two-stage least squares (TS2SLS) estimates. The first is a bias term due to prediction error and the second is a bias term due to an exclusion-restriction violation. We then provide a variety of evidence that these bias terms are unlikely to vary over time in such a manner as to produce our main result—the decline in persistence from the 1910s to 1940s—as a mere artifact. Next, we show that our imputation-based OLS strategy is equivalent to TS2SLS estimates when the intergenerational mobility parameter is estimated using a specification in levels. Finally, we show that we can recover approximations to the IGE and rank-rank measures by combining these levels estimates with sample moments, and that these approximations are very similar to our main results.

D.1 Sources of bias from two-sample imputation approach

In this section, we discuss the potential biases that may arise from the two-sample imputation approach that we and other historical mobility papers tend to follow. We then discuss robustness tests with these biases in mind.

D.1.1 Deriving bias terms from the two-sample approach

Following Solon (1992), consider the standard OLS estimation of intergenerational persistence:

$$y_i = \alpha + \beta^{\text{OLS}} y_i^p + e_i, \tag{7}$$

where i denotes the individual child; y_i is her adult income; and y_i^p is the *permanent* income of the parents of child i. The coefficient β captures the covariance between the two income variables and does not typically take on a causal interpretation. As correlation and not causality is the goal, $\mathrm{E}[y_i^p e_i] = 0$ by definition.[49]

The main challenge in our context—common in the historical U.S. mobility literature—is that we do not observe parental permanent income, y_i^p, for our children i. Instead, we have information on parental attributes Z_i that the children i report their parents to have had. We thus rely on a "first-stage" estimation of the relationship between parental income and attributes Z from an auxiliary dataset of parents j. Given the two samples involved in this procedure, this approach is a two-sample two-stage least squares approach (see, e.g., Angrist and Krueger, 1992; Inoue and Solon, 2010).

[49] While all of our main specifications omit covariates, note that we can include covariates that are unavailable in the first-stage in the second-stage *so long* as they are uncorrelated with the instruments and the error term in the first-stage; i.e., if a covariate would only improve precision in the first stage, it can be included in the second stage alone.

One population. We begin by considering the simplest scenario with only *one* population. Here, a researcher can do an analogous estimation, using half of the sample (henceforth group j) to estimate the relationship between parental income and attributes Z and then construct income values for the other half of the sample (henceforth group i) using the estimated first-stage parameters (i.e., a split-sample instrumental variables approach; Angrist and Krueger, 1995).

In other words, we can estimate income for parent j as a function of attributes Z_j:

$$y_j^p = Z_j\Gamma + V_j, \tag{8}$$

where $\mathrm{E}[V_j] = 0$, $\mathrm{Var}(V_j) < \infty$, and $\mathrm{Cov}(V_j, Z_j) = 0$. With an estimated $\hat{\Gamma}$ we can now return to children i and write the predicted income of child i's parental income as $\tilde{y}_i^p = Z_i\hat{\Gamma}$.

To clarify the resulting biases from this two-step approach, we can write the linear projection of y_i^p on $\tilde{y}_i^p$ as:

$$y_i^p = \theta\tilde{y}_i^p + w_i. \tag{9}$$

In this one-population scenario, the coefficient θ equals one.[50]

Finally, we can write the coefficient from this two-step procedure β^{TS2SLS} as a function of β^{OLS} as follows:

$$\beta^{\text{TS2SLS}} = \theta\beta^{\text{OLS}} + \underbrace{\frac{\mathrm{Cov}(e_i, \tilde{y}_i^p)}{\mathrm{Var}(\tilde{y}_i^p)}}_{\omega} \tag{10}$$

In this one-population scenario, $\beta^{\text{TS2SLS}} = \beta^{\text{OLS}} + \omega$. The ω term arises from violations of the exclusion restriction, whereby attributes Z_i are correlated with unobserved determinants of y_{ic} that do not enter through y_i^p.[51]

To provide some intuition for the ω bias in our setting, consider the variable "born in Mississippi," which is largely unobserved to us, as we do not always have information on place of birth. Assume that growing up in Mississippi predicts lower income during childhood even conditional on race, father's occupation, and South (our set of instruments). Assume further that growing up in Mississippi also predicts lower adult income even conditional on parental income. Under this scenario, $\mathrm{Cov}(e_i, \tilde{y}_i^p) > 0$ and thus $\omega > 0$. While in principle it is possible that this covariance is negative, the ease of coming up with examples such as our Mississippi example suggests that the ω bias is likely positive (Zimmerman, 1992).

[50] Bloise *et al.* (2021) also studies the two biases that result from two-stage techniques. In their derivation, they write the linear projection of $\tilde{y}_i^p$ on y_i^p as $\tilde{y}_i^p = \gamma y_i^p + v_i$ so that their version of equation (10) is a function of γ and v_i.

[51] Note that $\beta^{\text{TS2SLS}} = \beta^{\text{OLS}} + \omega$ also applies to any two-stage least squares coefficient, with one sample or two, and is not specific to the split-sample IV case.

Two populations. Now consider a different scenario, in which we have *two* populations: we use population j to estimate the relationship between parental income and attributes Z and then construct income values for population i. Here, we also have $\beta^{\text{TS2SLS}} = \beta^{\text{OLS}} + \omega$ as long as the parents of children i and the parents j are drawn from the same underlying population and the relationship between attributes Z and parental income are equivalent in populations i and j.

Nevertheless, if the two populations have different data-generating processes, then the functional form of the relationship between parental income and attributes Z will be mis-specified in the first stage. As an example, if respondents imperfectly recall their parental attributes Z, then the regression of y_i^p on $\tilde{y}_i^p$ would not necessarily have a coefficient equal to one. In short, if for *any* reason, the first-stage structural equation is different in the two populations, then β^{TS2SLS} could be biased not just via failures of the exclusion restriction (ω), but also through the projection of the dependent variable onto its prediction not having a coefficient $\theta = 1$ (Zhao *et al.*, 2019).

Finite samples. Finally, we now consider additional bias that may enter the estimation from using finite samples, as we do in this paper.

Consider again the simplest case where we know the data-generating process is the same in the two samples (e.g., a split-sample instrumental variables approach). Here, even though the individuals i and j belong to the same population and have the same data-generating process for income, there is sampling error in these finite samples and $\hat{\theta}$ does not need to mechanically equal one when two samples are involved.[52] Even in this simplified case, sampling error means that β^{TS2SLS} could be biased from two sources of error, θ and ω.

In sum, the utilization of two finite samples in this paper means that we must consider both θ and ω biases. The primary concern with using β^{TS2SLS} to recover time-varying patterns in intergenerational mobility is that the θ and ω terms could vary over birth cohorts in a manner that causes β^{TS2SLS} to decline between the 1910s and 1940s birth cohorts whereas the true β^{OLS} trends differently. In the next subsection, we present a variety of evidence that this concern is unlikely to hold.

D.1.2 Is the 1910–1940 decline in persistence a mere artifact of changing bias terms?

The decline of the IGE and rank-rank correlation between the 1910s and 1940s birth cohorts is our main novel result, and there are several factors that we think will push against this finding being driven by changes in θ and ω across birth cohorts.

[52] Angrist and Krueger (1995) write of the same θ coefficient from SSIV estimation that it is the "matrix of coefficients from a regression of [true values] on [predicted values]" and "represents a kind of attenuation bias arising from the use of reduced-form coefficients from a separate sample. Corollary 1.1 explicitly provides a formula for $\mathrm{E}[\widehat{\theta}]$ in the SSIV case. They note that this property of SSIV "contrasts sharply with the tendency of conventional IV estimates to be biased toward OLS."

Consistent relationship between first- and second-stage datasets. As noted above, a key challenge that could arise in our setting is differences in the underlying populations of the two samples. We provide evidence that the families in the surveys are drawn from roughly the same underlying population as the families in the Census and that these patterns do not seem to be changing over time (Figure C.4, Tables C.2 and C.3). Related, we show that our measures of parental income track known trends in inequality as well as the Black-white income gap over the 20th century (Figure A.1), providing reassurance that the predictions convey useful information about the distribution of parental income.

Another challenge present in our approach is imperfect recall of parental attributes. Appendix C extensively considers the accuracy of recall by comparing answers across respondent sex (Figures C.1 and C.2, Table C.1); between siblings (Figure C.3); as well as by comparing answers between parents and children in the PSID, in which we see both self-reported parental answers and retrospective children's answers (Figure C.5, Tables C.4 and C.5). In particular, the exercises comparing answers across respondent sex over time also indicate that the accuracy of recall is not changing over time in a way that would drive the 1910s–1940s persistence decline. We summarize these results in Section 3.2 of the paper.

Robustness to changing sets of IVs. Our preferred set of instrumental variables is the *father occupation* $\times$ *race* $\times$ *South* triplets. They allow us the maximum predictive power for parental income among variables that are available in all of our surveys. But we can show robustness of our main result to adding more covariates (which necessitates using a subsample of the data) or to reducing covariates.

Why is this robustness test important? Each of these sets of IVs will have different prediction error from the first-stage estimation (the θ term) and a different exclusion-restriction violation (the ω term). We have no *ex ante* intuition on the relative sizes of these bias terms over time as the set of instruments changes. As $\beta^{\text{TS2SLS}} = \theta\beta^{\text{OLS}} + \omega$, then continuing to find a decline in β^{TS2SLS} as θ and ω are allowed to vary suggests that the decline is driven by a true decline in β^{OLS} from the 1910s to 1940s cohorts.

We have already shown in the paper that when we add *more* predictors in the first stage we continue to find a robust decline in β^{TS2SLS}. To recap, we use father's education as an additional instrument in Figures B.2 and B.3, and we use more detailed childhood regions (instead of merely a *South* dummy) in Figure B.4. In both cases, the results are very similar to our baseline figures and show a significant, monotonic decline in intergenerational persistence over the 1910s to 1940s birth cohorts.

We can show similar results when we *reduce* the set of IVs. Figure D.1 shows that we can recover the decline in the estimated IGE from 1910s to 1940s using any subset of our *father occupation* $\times$ *race* $\times$ *South* triplets. In almost all cases, not only do the results recover the 1910s–1940s decline, but the monotonicity of the decline is also replicated. Moreover, once occupation is included as a predictor of parental income, all of the estimates are very close to each other. In the spirit of an over-identification test, the fact that we continue to recover the decline given that each of these alternative estimates has different θ and ω values over time suggests again that β^{OLS} is likely driving the decline in our estimated β^{TS2SLS}. In particular the quantitative similarity of the estimates when occupation is included as an instrument, but not without, is consistent with the intuition that using race and region cells on their own are likely to fail the exclusion restriction, but interacting them with occupation yields valid instruments.

Comparison to results using direct measures of parental income. Panel data sources such as the NLS are too modern to allow us to examine persistence in the 1910s–1930s cohorts, but we can compare our two-sample results with OLS results from the NLS for the late 1940s and 1950s cohorts, like those in Davis and Mazumder (2022).

The NLS surveys interview both parents and children, thereby providing measures of observed parental income and predicted parental income (based on the recollection of their children). We can thus use these surveys to directly compare β^{TS2SLS} and β^{OLS} (and thus to directly examine the net effects of the θ and ω terms).

We show results from this exercise in Appendix Figure D.2 utilizing our usual sample of respondents for whom we have all necessary information to construct our predicted childhood income. In panel (a), the first series shows the estimated IGE when we directly use the average of observed parental household income. The second series shows results using our usual two-step imputation method. The second series always sits above the first, though they are not statistically distinguishable.[53]

But the important point we take from this figure is that the two series move together *in changes*. Our claim in the paper is not that our two-step estimates of mobility are the same *in levels* as ones that we could hypothetically estimate if we observed actual parental income. We instead make the claim that the decline in our two-step estimates indicates a decline in actual persistence. That these two estimates using data from the 1940s and 1950s largely move together in changes is reassuring.

We can repeat this exercise for the late 1940s and 1950s cohorts with the PSID, shown in Appendix Figure D.3, and again find that the mobility measures estimated using predicted vs. actual income are not statistically different from each other and mirror each other in changes.

While not a direct test of the full 1910s–1940s decline, it is heartening to see that our method provides very similar results to those using direct measures of parental income for the late 1940s and 1950s birth cohorts, the earliest cohorts for which we can perform this exercise in US data.

Varying prediction error. A potential concern is that the decline in β^{TS2SLS} could be driven by a decline in the prediction-error term θ while β^{OLS} and ω terms remain unchanged (or are even increasing).

[53] It is possible that using only a few years of observed parental income contains more measurement error than our two-step process. If parental income is very noisy from year to year, then our triplet may better capture its permanent component than an average based on only a few years.

Our first argument against this idea is that the quality of the data sources is improving over time. This improvement would cause the θ term to *increase* over the 1910s to 1940s birth cohorts, rather than decline, thus pushing *against* our basic result that persistence declined.

Our second argument is based on the multiple imputation results in Figure B.5. Recall that this exercise maps each IV triplet to a randomly chosen income value in the corresponding cell (and repeats this procedure 100 times), using microdata from the Census and the 1936 Expenditure Survey. This exercise attenuates the estimated IGE in every year. Cortes-Orihuela *et al.* (2022) conduct a similar exercise in administrative Chilean data and show that this produces a lower bound on the IGE. In our data, we also see a substantially attenuated IGE in the multiple imputations exercise, but we still see qualitatively similar results on the *trend* of the decline, suggesting that changing amounts of prediction error are not driving our results. While these estimates put a lower bound on the trend in θ, we note that they are still vulnerable to omitted variables bias from changing ω given that the choice of instruments is being held constant. But the robustness of the qualitative pattern to assuming that every deviation of father's income from the cell-average is prediction error is reassuring.

Mobility measures that do not depend on IV estimation. As noted, the key challenge for historical income mobility estimation is the absence of parental income and the need to model it as the first step in a two-step procedure. We thus show two measures of mobility that do *not* depend on IV estimation to assuage concerns that our main results are merely artifacts of changing bias terms across the 1910s to 1940s cohorts.

First, as we show in Section 6, part of the overall IGE and rank-rank measures come strictly from *between-group differences* and thus are free from the biases introduced from two-step estimation. We show in Figure A.12 that the between-group terms of both the IGE and the rank-rank follow the same pattern across cohorts as our baseline figures, despite our baseline estimates being susceptible to biases.[54] We thus conclude that these biases are not changing appreciably across time, as mobility measures stripped of this bias follow the same pattern as the baseline results across cohorts (that is, roughly a u-shape for the IGE and an L-shape for the rank-rank).

Another mobility measure we can estimate across our cohorts that does not depend on first-stage prediction is *educational* mobility, because in many of our surveys respondents are asked their fathers' level of education. We show these results in Figure A.7, which depicts a similar decline between the 1910 and 1940 period as our main figures (in this case, the decline continues through the 1950s cohorts), but does not rely on any characteristics Z_i to predict parental income.

[54] While the third term in the decomposition uses our predicted parental income variable, only aggregate, not individual, measures enter into the expression. Note that we show in Figure A.1 that our predicted Black-white gaps for parental income follow the aggregate time series in Margo (2016).

D.2 Imputation-TS2SLS Equivalence in Levels Specification

In this section, we further illustrate the similarity between our primary imputation-based OLS estimator and the TS2SLS estimator. We re-write the standard OLS estimation from equation (7) using levels of income and predicted parental income as the right-hand-side variable for a given birth cohort c:

$$Y_{ic} = \alpha + \beta_c^{\text{levels}} \tilde{Y}_{ic}^p + e_i, \tag{11}$$

In a levels specification, our imputation approach—using the mean household income for each combination of father's occupation, region, and race—is numerically identical to the TS2SLS estimates where the instruments are fully interacted. If P_Z is the projection matrix onto the vector of race-region-occupation cells Z, then the imputations are given by $\tilde{Y}^p = P_Z Y^p$. For illustrative purposes and to reduce notation, assume both samples have the same size.[55] Then we also have $\beta^{\text{TSIV}} = (P_Z Y^p)^{-1} P_Z Y_c$ as the TSIV estimator[56], which is numerically identical to the OLS estimate using imputed data $\beta^{\text{OLS}} = \frac{\text{Cov}(\tilde{Y}^p, Y_c)}{\text{Var}(\tilde{Y}^p)} = ((P_Z Y^p)'(P_Z Y^p))^{-1}(P_Z Y^p)' Y_c = (P_Z Y^p)^{-1} P_Z Y_c = \beta^{\text{TSIV}}$ by the usual idempotency and self-adjointness of P_Z.

While in theory, the standard errors could be larger due to error in the first-stage regression, in practice the standards errors between the two estimates are quite close, owing to the large sample sizes in the Census being used for the first stage. We further adjust the TS2SLS for heteroskedascity following Pacini and Windmeijer (2016).

Panel (a) of Figure D.4 shows the numerical equivalence between TS2SLS and OLS for $\hat{\beta}^{\text{levels}}$ in our data.[57] We present a table of coefficients from this specification in Appendix Table D.1.[58]

A natural concern with the levels specification is that it misses non-linearities in the underlying structural relationship, which would be implied, for example, by credit constraints (Loury, 1981). Consistent with the literature, we have a concave relationship in the levels-on-levels regression, and this appears stable over time. Table D.2 shows the non-linearity with a quadratic specification, following Løken *et al.* (2012). The quadratic term (the square of predicted parental income) is generally significant in every year and a similar order of magnitude over the 20th century, and the resulting effect at the 25$^{\text{th}}$ percentile also shows a u-shape over time.

The numerical equivalence between OLS and TS2SLS will not hold for the log-log

[55] We also suppress the i subscripts for notational simplicity.

[56] Inoue and Solon (2010) show that this is dominated in efficiency terms by the TS2SLS estimator that adjusts for finite-sample issues in the empirical covariance matrix, but for illustration and because both of our samples are large, we focus on the TSIV estimator in the text, but conduct all estimates with the TS2SLS estimator.

[57] For completeness, Appendix Figures D.5 and D.6 also present the results by gender and by subgroup using equation (11).

[58] Note that the estimates in this table will not be identical to those plotted in Figure D.4 because of slightly different methodologies. The figure implements two-stage least squares using the nearest source of microdata relative to the respondent's childhood (thus only considering 1920s–1970s cohorts). In contrast, the table uses levels of parental income based on the baseline (interpolated) measure of parental income.

specification estimated in the main text because we impute the log of average income (as we only have mean incomes for the early cohorts in our sample) and the usual differences between $\log(\mathrm{E}[x])$ and $\mathrm{E}[\log(x)]$. Nonetheless, the difference is quantitatively small, and the basic "u-shape" pattern in the IGE under the TS2SLS approach is shown in panel (b) of Appendix Figure D.4.

While the TS2SLS and imputation approaches agree exactly in levels and approximately in IGE space, the primary limitation is that we have no microdata for the 1910s cohort of farmer fathers, owing to the lack of any agricultural microdata from this early cohort. Given the importance of farmers in this period, and the importance of this cohort in showing the trend of increasing mobility in the early part of the 20th century, we present the imputation-based estimates in the main text.

D.3 Connecting level-, log-, and rank-based estimates

The levels-on-levels specification is not completely unfamiliar to the mobility literature, being used in Dahl and Lochner (2012) and Løken *et al.* (2012). It is, however, not regularly used in estimates of U.S. mobility, which have traditionally focused on the log-log specification as in Becker and Tomes (1979) or, more recently, the rank-rank specification as in Chetty *et al.* (2014a). But these latter two measures can be approximated using β^{levels}. For example, the intergenerational elasticity (at the population mean) can be approximated by $\beta^{\text{IGE}} = \frac{\mathrm{E}[Y_i^p]}{\mathrm{E}[Y_{ic}]}\beta^{\text{levels}}$ using the levels-based specification in equation (11) and first-order Taylor approximations of $\log(Y_i^p)$ and $\log(Y_{ic})$ around their means (i.e., $\log(x) - \log(\mathrm{E}[x]) \approx \frac{x}{\mathrm{E}[x]} - 1$ for both generations). As shown in Figure D.7, the estimated IGEs using this approximation are generally lower than those from the main text, but they retain the visible and stark u-shape over the 20th century.

To approximate the rank-rank, note that if the income distribution is normal or lognormal then the rank-rank correlation is exactly equal to $\beta^{\text{RR}} = \frac{6}{\pi}\arcsin(\frac{\beta^{\text{IGC}}}{2})$, where β^{IGC} is the intergenerational correlation, which can also be obtained from β^{levels} by multiplying it with the ratio of the standard deviations (i.e., $\beta^{\text{IGC}} = \sqrt{\frac{\mathrm{Var}[Y_i^p]}{\mathrm{Var}[Y_{ic}]}}\beta^{\text{levels}}$). The lognormal assumption is likely close to true in the large populations we are sampling from, even if it may not hold for the within-county income distributions of interest to the recent literature. Figure D.7 shows the approximated $\hat{\beta}^{\text{RR}} \approx \frac{6}{\pi}\arcsin\left(\frac{1}{2}\sqrt{\frac{\mathrm{Var}[\hat{Y}_i^p]}{\mathrm{Var}[\hat{Y}_{ic}]}}\hat{\beta}^{\text{levels}}\right)$ approximation (standard errors calculated using the delta-method) and that, in practice, the levels-based approximation of the IGC and rank-rank estimates using our baseline approach are very similar both quantitatively and qualitatively.[59]

To summarize, translating these population relationships to estimates, the TS2SLS and imputation-based OLS yield point estimates of $\hat{\beta}^{\text{levels}}$ that are quantitatively iden-

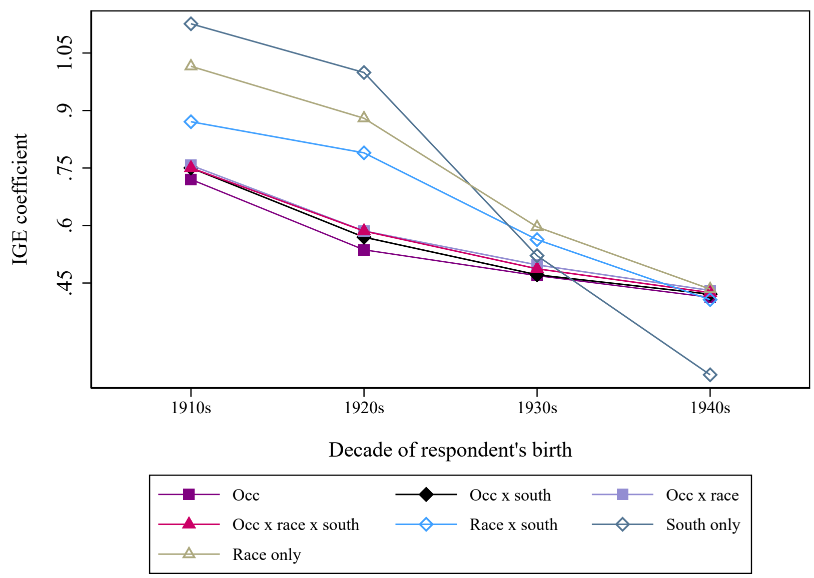
[59] A coarser approximation is to simply set $\beta^{\text{RR}} \approx \beta^{\text{IGC}}$, since note that the first two non-zero terms of the Taylor expansion of arcsin gives $\frac{6}{\pi}\arcsin(\frac{\beta^{\text{IGC}}}{2}) \approx \frac{3}{\pi}\beta^{\text{IGC}} + \frac{1}{4\pi}(\beta^{\text{IGC}})^3$. Thus the approximation $\beta^{\text{RR}} \approx \beta^{\text{IGC}}$ is likely to be a good approximation given the coarseness of the historical data we are working with. As the IGC can be calculated using a simple linear regression rather than non-linear transformations it is a good measure for capturing "pure" mobility, independent of inequality, in data-constrained historical contexts.

tical. Further, armed only with TS2SLS estimates of $\hat{\beta}^{\text{levels}}$, we can recover approximations to the primary measures used in our paper (and in the literature), without using any non-linear transformations of predicted parental income. These approximated estimates exhibit qualitatively very similar patterns to their analogues in the main text. We conclude that none of our results depend on the use of imputed incomes in OLS versus TS2SLS regressions.

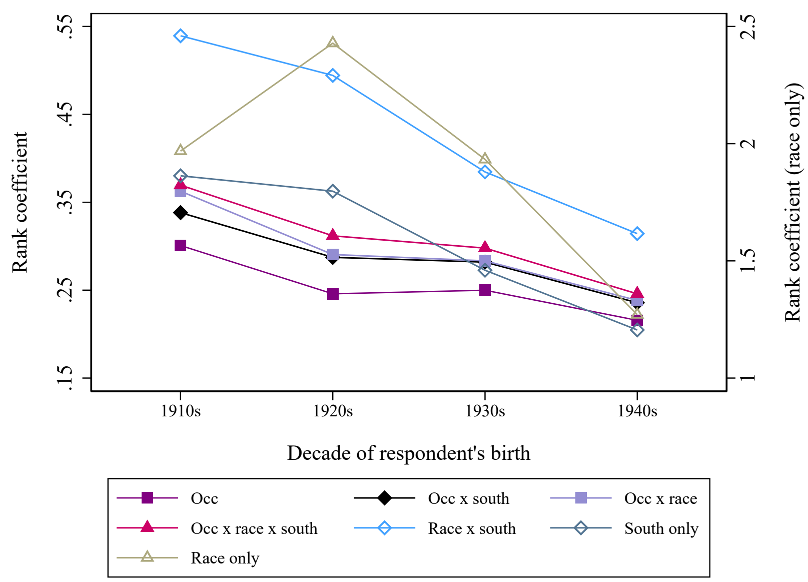
Finally, we note that one key difference between the logs-based and levels-based estimates is that the 1940s cohort is the most mobile when considering the IGE, whereas the 1950s cohort appears more mobile when considering levels of income, even as the rank-rank correlation stays constant. This difference can be explained by the standardizations of income discussed above. In short, holding the intergenerational correlation fixed (which appears to be approximately the case starting in the 1940s; see Figure 1), the IGE is equal to the IGC multiplied by the standard deviation of logged children's income over logged parent's income $\left(\text{i.e., } \frac{\hat{\sigma}_{y_{ic}}}{\hat{\sigma}_{y_i^p}}\right)$. The 1950s cohort had relatively lower parental inequality and higher adult children inequality, thereby making the IGE rise (the ratio of standard deviations increased from 1.92 to 2.36 between 1940 and 1950). Next, the levels coefficient is approximately equal to the IGE multiplied by the mean of children's income over parent's income using levels of income. Given immense growth of parental income in this time period, this ratio of $\frac{\mathrm{E}[\hat{Y}_{ic}]}{\mathrm{E}[\hat{Y}_i^p]}$ fell from 1.51 to 1.03, so that the levels coefficient declined between 1940 and 1950 (despite the increase in the IGE). In sum, the rapid rate of growth in parental income during a period of relatively lower inequality makes it so that the IGE rises, while the levels coefficient continues to decline (see Appendix Figure D.8).

Figure D.1: 1910–1940 IGE and rank-rank correlation varying sets of instruments

(a) IGE



(b) Rank-rank

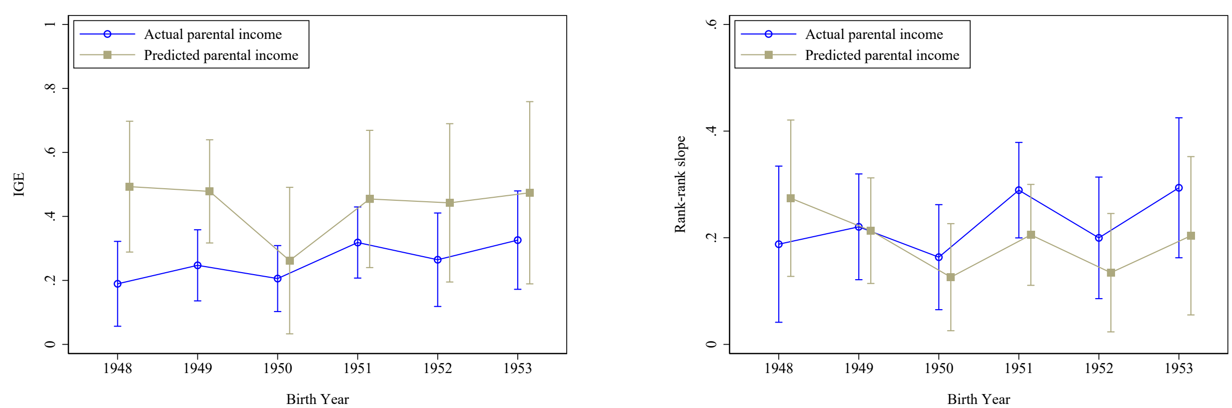


Sources: Data come from 15 different surveys, described in Section 2 and in further detail in Appendix E.

Notes: The IGE and rank-rank estimates are based on the baseline sample of respondents ages 30–50 using equations (2) and (3) for the 1910s–1940s cohorts. Each series uses different characteristics to predict parental income. To predict parental income, we use family income from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares. In panel (b), the estimates that only use race as a predictor of parental income are plotted on the secondary y-axis.

Figure D.2: IGE and rank-rank correlation using actual vs. predicted income in NLS sample

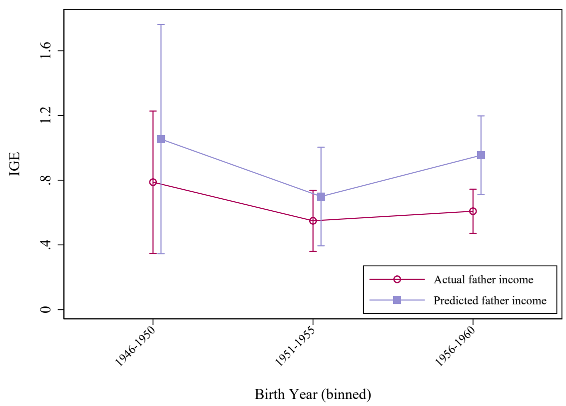
(a) IGE **(b)** Rank-rank



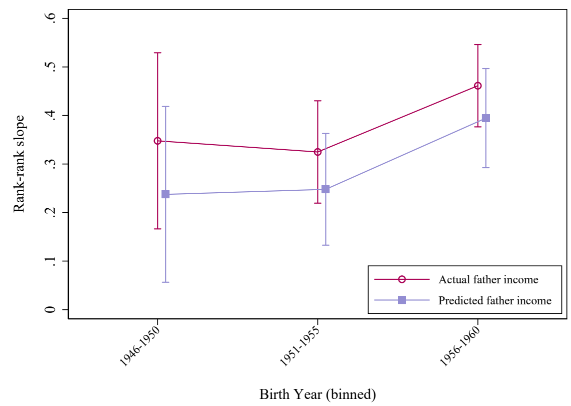
Notes: These figures plot the intergenerational elasticity and rank-rank correlation based on the sample from Davis and Mazumder (2022) using the National Longitudinal Surveys. The figures restrict the sample to individuals who reported a father's occupation retrospectively in the first wave of the survey. Actual income refers to the measure from Davis and Mazumder (2022) (i.e., calculated as an average of all total family income reports in the first three survey waves where income data was reported). Predicted parental income refers to utilizing our baseline income predictions (which vary by *occupation* × *race* × *South*). In both panels, we follow Davis and Mazumder (2022) and use the weights corresponding to the adult children in the first round of the survey.

Figure D.3: IGE and rank-rank correlation using actual vs. predicted income in PSID sample

(a) IGE



(b) Rank-rank

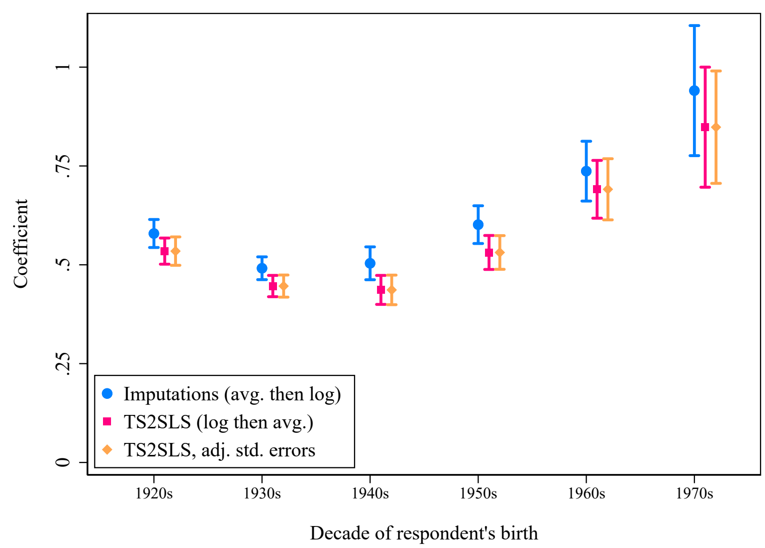


Notes: These figures plot the intergenerational elasticity and rank-rank correlation using the Panel Study of Income Dynamics. The first panel plots the IGE using actual and predicted parental income. The second panel plots the analogous rank-rank correlations. We create 5-year birth cohort bins, and respondents are ranked relative to other respondents in the same bin. Actual income is calculated as an average of total family income reports in the first three survey waves around when the adult child respondent turns 40. Actual parental income is calculated in the same manner. Predicted parental income refers to utilizing our baseline income predictions (which vary by *occupation* $\times$ *race* $\times$ *South*). In all panels, we use the 1997 cross-sectional, individual weights for adult children. The figures restrict the sample to adult child respondents with available actual income (3 years), working father actual income (3 years), and predicted working father income (i.e., individuals who reported a father's occupation retrospectively). All income measures are in 2015 dollars.

Figure D.4: Intergenerational mobility by birth decade using imputed average incomes and two-sample two-stage least squares

(a) Levels of income

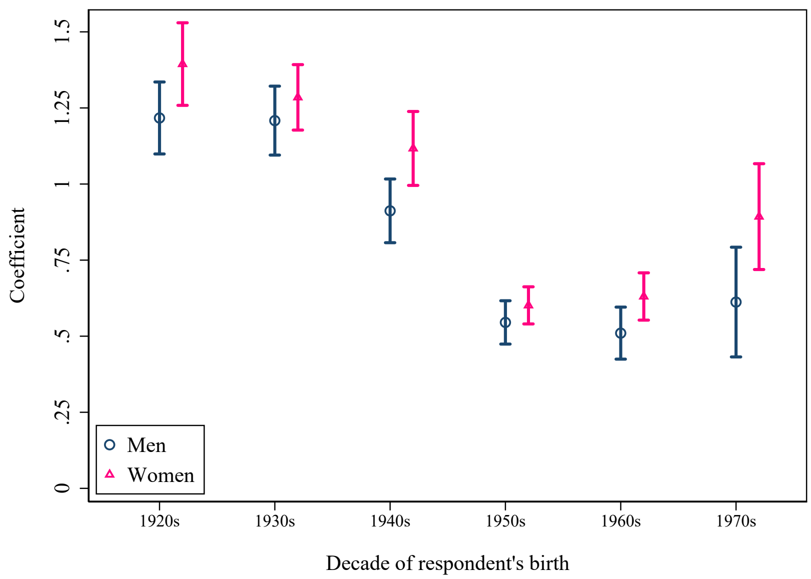
(b) Logged income



Sources: Data come from 15 different surveys, described in Section 2 and in further detail in Appendix E.

Notes: The estimates are based on the baseline sample of respondents ages 30–50. The top panel considers levels of income and the bottom panel logged household income. The first series in both panels uses the baseline approach for predicting parental family income (i.e., calculating average household incomes at the *occupation* × *race* × *South* level and in the bottom panel, subsequently applying the log transformation). The second series in the top panel uses a two-sample two-stage least squares regression, using household income in the Census to predict parental family income in the surveys. The third series adjusts robust standard errors using Pacini and Windmeijer (2016). Panel (b) uses an identical approach for the last two series, except that it uses the Census to predict logged household income. To predict parental income in each cohort, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from the nearest Census to the respondent's childhood. Specifically, for the 1920s–1930s birth cohorts, we use the 1940 Census with farmer and self-employed income from the 1936 Expenditure Survey. For the 1940s–1970s birth cohorts, we use the 1950–1980 Censuses, respectively.

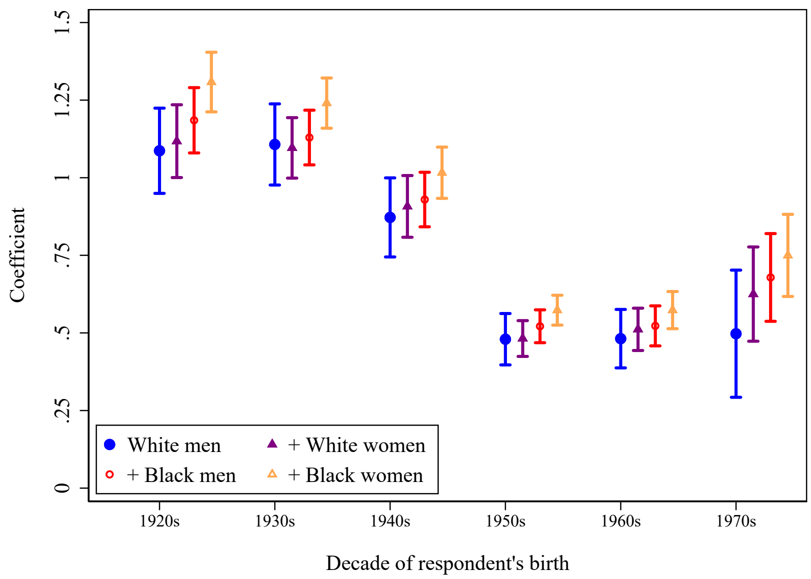
Figure D.5: Intergenerational mobility using levels of family income, by sex



Sources: This figure combines 15 surveys, described in Section 2 and in further detail in Appendix E.

Notes: This figure uses a two-sample two-stage least squares regression, using household income in the Census to predict parental family income in the surveys. All estimates report robust standard errors using Pacini and Windmeijer (2016). To predict parental income in each cohort, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from the nearest Census to the respondent's childhood. Specifically, for the 1920–1930s birth cohorts, we use the 1940 Census with farmer and self-employed income from the 1936 Expenditure Survey. For the 1940s–1970s birth cohorts, we use the 1950–1980 Censuses, respectively. We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* $\times$ *sex* shares.

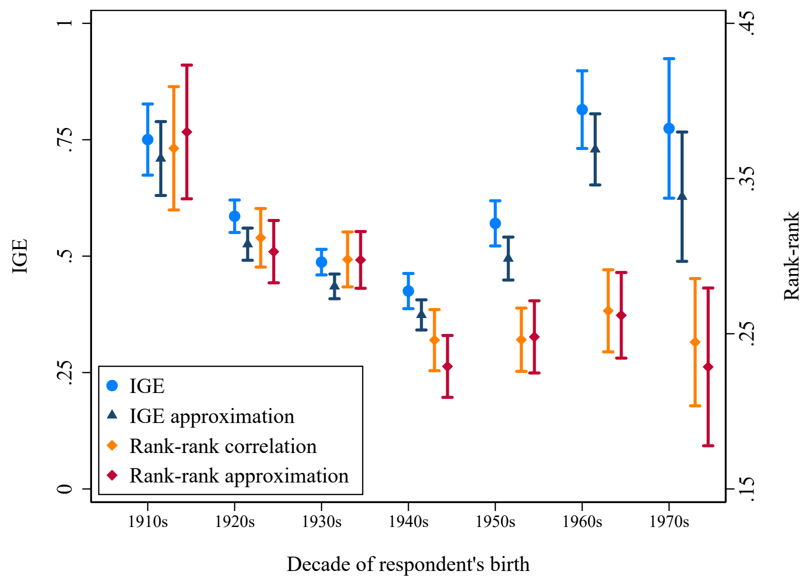
Figure D.6: Intergenerational mobility using levels of family income and including under-represented groups



Sources: This figure combines 15 surveys, described in Section 2 and in further detail in Appendix E.

Notes: This figure uses a two-sample two-stage least squares regression, using household income in the Census to predict parental family income in the surveys. All estimates report robust standard errors using Pacini and Windmeijer (2016). To predict parental income in each cohort, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from the nearest Census to the respondent's childhood. Specifically, for the 1920–1930s birth cohorts, we use the 1940 Census with farmer and self-employed income from the 1936 Expenditure Survey. For the 1940s–1970s birth cohorts, we use the 1950–1980 Censuses, respectively. We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares.

Figure D.7: Mobility by birth cohort, approximating IGE and rank-rank correlation with levels specification

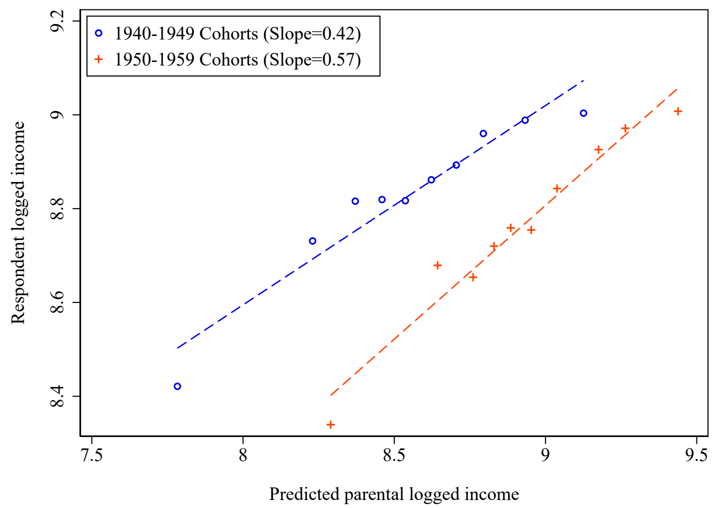


Sources: This figure combines 15 different surveys, which are described in Section 2 and in further detail in Appendix E.

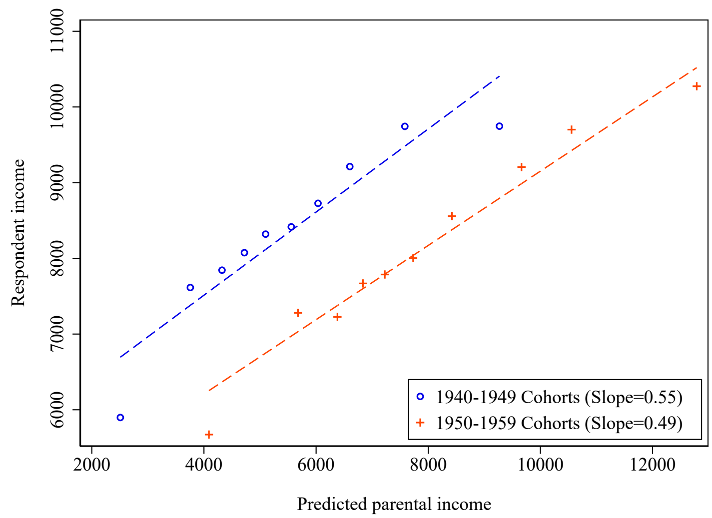
Notes: All estimates are based on the baseline sample of respondents ages 30–50. The first and third series reproduce the baseline IGE and rank-rank estimates from Figure 1. The second and fourth series come from linear specifications using levels of income for both generations (reported in Appendix Table D.1). The second series multiplies the levels-based estimate with the ratio of average parental income to average adult children's income. The fourth series transforms the levels-based estimate using $\beta^{\text{RR}} = \frac{6}{\pi}\arcsin(\frac{\beta^{\text{IGC}}}{2})$, where $\beta^{\text{IGC}} = \sqrt{\frac{\text{Var}[Y_i^p]}{\text{Var}[Y_{ic}]}}\widehat{\beta^{\text{levels}}}$. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares.

Figure D.8: Bin-scatter depictions of the change in intergenerational persistence between 1940s and 1950s cohorts

(a) Intergenerational elasticities



(b) Levels-on-levels estimates



Sources: Data come from 15 different surveys, described in Section 2 and in further detail in Appendix E.

Notes: The estimates are based on the baseline sample of respondents ages 30–50 in the 1940s and 1950s cohorts. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares.

Table D.1: Intergenerational mobility using levels of family income, by birth cohort

	(1) 1910	(2) 1920	(3) 1930	(4) 1940	(5) 1950	(6) 1960	(7) 1970
Level coefficient	1.669	1.389	0.964	0.549	0.490	0.632	0.562
	[0.095]	[0.047]	[0.030]	[0.024]	[0.023]	[0.034]	[0.063]
Observations	5,207	13,328	12,446	11,580	10,964	6,605	3,132

Notes: The estimates are based on the baseline sample of respondents ages 30–50 and come from an analogous specification to equation (2), but using income levels for both generations. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares.

Table D.2: Intergenerational mobility using levels of family income and quadratic specification, by birth cohort

	(1) 1910	(2) 1920	(3) 1930	(4) 1940	(5) 1950	(6) 1960	(7) 1970
Level coefficient, linear	2.071	2.311	1.484	1.082	0.961	1.173	1.593
	[0.348]	[0.174]	[0.111]	[0.083]	[0.082]	[0.145]	[0.213]
Level coeff., quadratic (×100)	-0.008	-0.017	-0.007	-0.004	-0.003	-0.003	-0.004
	[0.008]	[0.003]	[0.002]	[0.001]	[0.000]	[0.001]	[0.001]
Derivative at 25th percentile	1.780	1.617	1.113	0.711	0.642	0.774	0.898
Observations	5,207	13,328	12,446	11,580	10,964	6,605	3,132

Notes: The estimates are based on the baseline sample of respondents ages 30–50 and come from an analogous specification to equation (2), but using income levels for both generations and allowing for a quadratic term in parental income. For ease of exposition, the quadratic term is divided by 100 in the specification. "Derivative at 25th percentile" refers to the marginal effect evaluated at the income level that corresponds to the 25$^{\text{th}}$ percentile of the predicted parental income distribution for the corresponding cohort. To predict parental income, we use family income conditional on father's occupation, race, and region (South vs. elsewhere) from auxiliary data (often the Census) as close as possible to the respondents' tenth birthday (see Section 3.2 for more details). We use sample weights where provided and further re-weight each birth cohort (i.e., decade) so that they have representative *race* × *sex* shares.

Table D.3: Intergenerational elasticity using quadratic specification, by birth cohort

	(1) 1910s	(2) 1920s	(3) 1930s	(4) 1940s	(5) 1950s	(6) 1960s	(7) 1970s
Linear coefficient	-0.167	0.672	0.393	1.912	2.751	2.503	8.680
	[0.930]	[0.427]	[0.336]	[0.483]	[0.699]	[1.648]	[5.157]
Quadratic coefficient	0.061	-0.006	0.006	-0.089	-0.123	-0.093	-0.430
	[0.061]	[0.028]	[0.021]	[0.029]	[0.040]	[0.091]	[0.279]
Coeff., linear only	0.750	0.586	0.487	0.425	0.570	0.815	0.774
Slope at mean	0.770	0.583	0.489	0.395	0.549	0.808	0.801
	(0.69, 0.85)	(0.54, 0.62)	(0.46, 0.52)	(0.35, 0.44)	(0.50, 0.60)	(0.72, 0.89)	(0.63, 0.97)
Slope, 10th perc.	0.705	0.589	0.483	0.472	0.639	0.860	1.028
	(0.58, 0.83)	(0.54, 0.64)	(0.44, 0.52)	(0.42, 0.52)	(0.58, 0.70)	(0.73, 0.99)	(0.63, 1.43)
Slope, 90th perc.	0.834	0.578	0.495	0.315	0.454	0.750	0.541
	(0.66, 1.01)	(0.50, 0.66)	(0.44, 0.55)	(0.24, 0.39)	(0.36, 0.54)	(0.61, 0.89)	(0.25, 0.83)
R-sq., linear only	0.163	0.117	0.101	0.049	0.058	0.066	0.054
R-sq., quadratic	0.164	0.117	0.101	0.050	0.059	0.066	0.057
Observations	5,207	13,328	12,446	11,580	10,964	6,605	3,132

Notes: The estimates are based on the baseline sample of respondents ages 30–50 and come from an analogous specification to equation (2) but including a quadratic term in logged parental income. "Coeff., linear only" refers to the intergenerational elasticity from the linear specification. "Slope at mean" refers to the marginal effect evaluated at the mean of the predicted parental income distribution for the corresponding cohort. "Slope, 10th perc." and "Slope, 90th perc." refer to the marginal effect evaluated at the 10th and 90th percentile of the predicted parental income distribution, respectively, for each cohort. The 95 percent confidence intervals of these slopes are all reported in parentheses underneath their corresponding estimate. "R-sq." refer to the R^2 from the linear and quadratic specifications.

Table D.4: Rank-rank correlation using quadratic specification, by birth cohort

	(1) 1910s	(2) 1920s	(3) 1930s	(4) 1940s	(5) 1950s	(6) 1960s	(7) 1970s
Linear coefficient	0.499	0.512	0.362	0.423	0.404	0.373	0.327
	[0.074]	[0.037]	[0.035]	[0.040]	[0.041]	[0.055]	[0.083]
Quadratic coefficient	-0.132	-0.203	-0.065	-0.180	-0.160	-0.111	-0.084
	[0.075]	[0.037]	[0.034]	[0.040]	[0.041]	[0.054]	[0.084]
Coeff., linear only	0.369	0.312	0.298	0.246	0.246	0.265	0.245
Slope at mean	0.376	0.320	0.300	0.251	0.250	0.267	0.246
	(0.34, 0.42)	(0.30, 0.34)	(0.28, 0.32)	(0.23, 0.27)	(0.23, 0.27)	(0.24, 0.29)	(0.20, 0.29)
Slope, 10th perc.	0.472	0.472	0.349	0.387	0.372	0.353	0.312
	(0.35, 0.59)	(0.41, 0.53)	(0.29, 0.40)	(0.32, 0.45)	(0.31, 0.44)	(0.26, 0.44)	(0.18, 0.45)
Slope, 90th perc.	0.263	0.150	0.245	0.106	0.121	0.183	0.182
	(0.14, 0.39)	(0.09, 0.21)	(0.19, 0.30)	(0.04, 0.17)	(0.05, 0.19)	(0.10, 0.27)	(0.05, 0.31)
R-sq., linear only	0.139	0.100	0.091	0.061	0.060	0.069	0.061
R-sq., quadratic	0.140	0.103	0.091	0.063	0.062	0.070	0.061
Observations	5,207	13,328	12,446	11,580	10,964	6,605	3,132

Notes: The estimates are based on the baseline sample of respondents ages 30–50 and come from an analogous specification to equation (2) but including a quadratic term in logged parental income. For ease of exposition, the quadratic term is divided by 100 in the specification. "Coeff., linear only" refers to the intergenerational elasticity from the linear specification. "Slope at mean" refers to the marginal effect evaluated at the mean of the predicted parental income distribution for the corresponding cohort. "Slope, 10th perc." and "Slope, 90th perc." refer to the marginal effect evaluated at the 10th and 90th percentile of the predicted parental income distribution, respectively, for each cohort. The 95 percent confidence intervals of these slopes are all reported in parentheses underneath their corresponding estimate. "R-sq." refer to the R^2 from the linear and quadratic specifications.

E Additional detail on data sources

E.1 Harmonizing surveys

We typically include a survey in the analysis if it meets two main conditions: First, it must ask survey respondents about their family income. And second, it must ask respondents about their fathers' occupation while they were growing up, and the available occupation codes must be able to be mapped to our coarsened occupations (discussed below). The surveys that meet these conditions usually also include other useful information, including demographic characteristics of the respondent as well as those of the father and mother.

In the end, we have fifteen harmonized surveys:

- American National Election Studies (ANES),1956–1970
- Americans View Their Mental Health (AVTMH), 1957 & 1976
- General Social Survey (GSS), 1972-2018
- National Fertility Survey (NFS), 1970
- NLS Mature Women (NLSMW), 1967
- NLS Older Men (NLSOM), 1966
- NLS of Youth, 2002 [60]
- NLS Young Men (NLSYM), 1981 [60]
- NLS Young Women (NLSYW), 1988 [60]
- National Survey of Black Americans (NSBA), 1979–1980
- National Survey of Families and Households (NSFH), 1987–1988
- Occupational Changes in a Generation (OCG), 1962 & 1973
- Panel Study of Income Dynamics (PSID), 1997 & 2017 [61]

The cohorts present in each survey and a description of each sample are displayed in Table E.1.

To minimize life-cycle bias, we restrict the sample to U.S.-born respondents aged 30–50. We also include respondents in this age range for whom we do not know where

[60] Note that the National Longitudinal Surveys can also be used as repeated cross-sections. For these three surveys, we select the cross-section to use by first observing the median age in the earliest cross-section of the survey. We then calculate the year in which the median age of respondents would be around 40. If the survey was not conducted in this year, we take the nearest survey year. Nevertheless, in these three NLS surveys (similarly to in the NLSMW and NLSOM) we typically use the first wave to collect demographic information about the respondent (e.g., sex, race, birth year, birthplace) as well as retrospective information about the parents' occupations and educational attainment. The one exception is that in the NLSYW survey, we collect information about the mother's occupation when the question was re-asked in 1978.

[61] Note that the PSID can be used as repeated cross-sections. We rely on the 1997 wave because this was the first year in which retrospective questions about parents' occupations were asked with sufficient detail and in which cross-sectional individual weights were available. We also then use the 2017 survey to bring in a new cohort of individuals ages 30–50. We intentionally exclude any 1997 respondents who appear again in 2017. Retrospective questions were only asked to household heads and their wives; whenever we have two respondents within a family with all of the available information for our analysis, we select a member at random.

they were born.[62] Finally, we restrict the sample to individuals whose race is recorded as white or Black.[63]

Once we identify and clean these surveys, we pool them together for the analysis. An individual is in our baseline sample if he/she has an available family income, recorded race, region of birthplace/childhood (South vs. non-South), and father's occupation. Together, these four components allow us to measure the respondent's income level and predict parental family income. Table E.2 shows how the sample size changes as we sequentially impose each sample restriction. The baseline cohorts and samples in each survey are summarized in Figure E.1 as well as Tables E.3 and E.4.

E.2 Respondent family income

In all of our harmonized surveys, respondents are asked about their family income in that year. Some surveys provide the information in categories, while others provide exact numerical values. To be consistent in our coding, we rely on the bin structure of the surveys and assign respondents the midpoint of that category. The exception to this step is that for individuals who make the least (i.e., whose income falls in the bottom bin, including those with zero income), we assign them 0.75 × the upper boundry of the category. For respondents who make the most (i.e., whose income falls in the top bin), we assign them 1.25 × the lower boundry of the category.

For surveys that report exact income values, we replicate the bin structure for assigning respondents a family income value. In particular, we first find a survey that took place around the same time period and use that survey's bin structure as a template. We then assign individuals the midpoint of their corresponding bin.[64] Ultimately, we want to observe a roughly equal proportion of respondents in each bin. When the outlined procedure does not yield this result, we consider alternative bin structures (namely, the bin structure in other surveys) until we find a template that results in a relatively equal distribution.

Finally, for consistency, we ensure that each survey has roughly 10–12 bins for respondent family income. For surveys that have significantly more bins, we combine bins and assign respondents the midpoint of the new category (while simultaneously ensuring that each bin has roughly the same share of respondents). Table E.5 summarizes that share of the baseline sample that is top and bottom coded in each survey as well as the number of income bins utilized.

[62] We exclude foreign-born respondents because we cannot know with certainty whether they grew up in or outside of the United States. Because we predict parental family income using U.S.-based data and because the average income for the same occupation can differ across countries, we refrain from predicting parental income for the fathers of these respondents and thus exclude these father-children pairs.

[63] Respondents who are classified as Hispanic in surveys are re-classified as white unless there is additional information available on race. Respondents of other races, who comprise a tiny share of survey samples, are excluded from the analysis.

[64] For instance, because NSFH interviews took place in 1987 and 1988, we use the 1988 bins from the GSS as a template for the bin structure of family income for NSFH respondents.

E.3 Predicting parental family income

E.3.1 Coarsened occupations

We obtain father occupation from the respondent, who typically reports his/her father's occupation when the respondent was growing up or around 14–16 years old. In many of the surveys, we are also able to obtain analogous information for the mother's occupation.

Across all surveys, we harmonize occupations into 28 categories, corresponding to the main occupations in the American National Election Survey. The ANES occupations we use are:

- Accountants and auditors
- Clergymen
- Teachers
- Dentists
- Physicians and surgeons
- Engineers
- Lawyers and judges
- Social and welfare workers
- Nurses
- Other professional and technical occupations
- Semi-professional occupations
- Self-employed businessmen, managers, and officials
- Businessmen, managers, and officials
- Bookkeepers
- Stenographers, typists, and secretaries
- Other clerical workers
- Higher-status sales workers in "outside" sales
- Inside sales workers (e.g., salesmen, clerks)
- Lower-status sales workers in "outside" sales (e.g., peddlers, newsboys)
- Foremen
- Skilled craftsmen and kindred workers
- Semi-skilled operatives and kindred workers
- Protective service workers
- Private household workers
- Other service workers
- Farm laborers
- Non-farm laborers
- Farm operators

E.3.2 Constructing baseline measures of parental income

In order to approximate parental family income, we use various datasets from throughout the 20th century. In particular, we rely on data from the 1901 Cost of Living Survey and the 1900 Census of Agriculture (which we refer to as our "1900-based" income predictions), the full-count 1940 Census and the 1936 Expenditure Survey (henceforth our

"1940-based" income predictions), and the 1960–1990 Censuses (Ruggles *et al.* 2021). We provide more details on the 1936 Expenditure Survey in Section E.3.4.

Because we want our baseline measures of parental income to approximate the income of the fathers' generation, we restrict these datasets whenever possible to individuals who resemble the survey respondents' fathers (Ruggles *et al.*, 2021). In particular, we restrict the samples to men who are between the ages of 30–50, whose race was recorded as either white or Black, and who had a child younger than 18 present in the household. We then build and use crosswalks that map the Census occupations into our 28 coarsened occupations.

Next, we calculate the mean income in each occupation for individuals with certain characteristics. In our baseline specification, we calculate the mean income by *occupation* × *race* × *South*.[65] Our preferred measure of income is mean household income, which sums the income of all family members within a household. To ensure that these measures are comparable throughout the analysis, all predicted income is reported in 1950 dollars.

The two exceptions to this straightforward approach are our 1900- and 1940-based income predictions. First, to construct 1900-based predicted income, we use information on average earnings by occupation from the 1901 Cost of Living Survey (Preston and Haines 1991) and collapse this information to our coarsened occupations. We then use the income of fathers ages 30–50 in the 1940 full-count Census to adjust these income values by race and Southern residence. For fathers who are farmers, we calculate predicted income using the 1900 Census of Agriculture. In particular, we use information on farm output and expenses from Merriam (1902) and follow the approach in Goldenweiser (1916) and Abramitzky *et al.* (2012) to calculate farmers' income by race and Southern residence. We additionally adjust these values by the share of farmers in that race and region that were owners (assuming that non-owners earn 50% of the estimated farm income).

Second, the 1940 income variable (i.e., wage and salary income) excludes income from self-employment and farming. We thus use an alternative data source from this time period — the 1936 Expenditure Survey — to calculate the average family income of farmers and of self-employed fathers separately by race and Southern residence.

After we construct these six versions of predicted income, we assign fathers an estimate using the data sources that are closest in time to when the respondent was 10 years old. In particular, we assign cohorts born between 1910 and 1930 a weighted average of the 1900- and 1940-based logged income predictions, with the weights reflecting the number of years between when the respondent was 10 years old and 1940. For cohorts born between 1930 and 1950, we assign a weighted average of the 1940-

[65] For any *occupation* × *race* × *South* cells with no available Black fathers, we impute the average income for that cell using the average racial income gap in that same region and the income of white fathers in that same occupation. In particular, we first calculate the white-Black income gap within an occupation and region, and then average these gaps across the occupations in that region, allowing occupations with more Black fathers to get greater weight in the calculation. Finally, we use the income of white fathers in the desired occupation and region in conjunction with the estimated average racial income gap in that region to impute the income of the missing cell.

and 1960-based logged income predictions, with the weights once again reflecting the number of years between when the respondent was 10 years old and 1960. For cohorts born between 1950 and 1960, we assign a weighted average of the 1960- and 1970-based logged income predictions, with the weights again reflecting the number of years between when the respondent was 10 years old and 1970. We continue this process for all respondents born in the 1960s–1970s cohorts using the income predictions constructed with the 1970–1990 Censuses.

A couple of times throughout the paper, we calculate and use predicted income for survey respondents, rather than using their total family income at the time of the survey. Similar to the method utilized to predict parental income, we use Census data as well as the 1936 Expenditure Survey to approximate respondents' income by their occupation, race, and Southern residence (as observed at the time of the survey). We assign respondents a predicted income value using the data sources that are closest in time to when the respondent is 40 years old. In practice, cohorts born between 1910 and 1920 are assigned a weighted average of the 1940- and 1960-based income predictions. All subsequent birth cohorts are similarly assigned predicted income values that are weighted averages constructed using the 1960–2000 Censuses as well as the 2010 and 2019 American Community Surveys.

E.3.3 Alternative ways to predict parental income

Our baseline 1940-based income predictions use the 1936 Expenditure Survey to estimate the average family income of farmer and self-employed fathers. However, we also consider in the robustness checks a different approach for predicting farmer and self-employed income for 1940. Following the approach in Collins and Wanamaker (2022), we use fathers ages 30–50 in the 1960 Census to calculate the ratio of farmer income to farm laborer income. We then use farm laborers' income in 1940 as well as these ratios to impute the 1940 income of farmers.[66] Second, we adjust the income of self-employed non-farm workers using a similar approach: we consider fathers ages 30–50 in the 1960 Census and compute ratios of mean earnings for self-employed workers relative to wage-and-salary workers. We then impute the earnings of self-employed non-farm workers in 1940 using these ratios. Throughout the analysis, we use the same level of granularity to compute ratios as we do when predicting parental income. Our preferred ratios therefore vary at the *race* $\times$ *South* level.[67] We then use this measure of farmer and self-employed income in the 1940-based income predictions, and blend these measures with earlier and later data sources similarly to in the baseline approach.

Throughout the robustness checks, we sometimes estimate parental income using fewer predictors of income than our baseline income predictions; sometimes the income predictions only use information about a father's occupation and other times they

[66] Throughout these calculations, we also follow Collins and Wanamaker (2022) and adjust farmer and farm laborer income measures upward to reflect the value of in-kind income.

[67] If there were fewer than 20 individuals in the 1960 Census cell (e.g., *occupation* $\times$ *race* $\times$ *South* $\times$ *education*), we rely on the mean income of individuals in the broader group (*occupation* $\times$ *race* $\times$ *South*) to construct ratios.

incorporate information on both occupation and race. For a sub-sample of respondents, we also predict parental family income using more predictors relative to our baseline predictions (namely, using information about the father's educational level as well as more-detailed information about the region of residence). For education variations, we use five levels of education—less than 8th grade, 8th grade, some high school, completed high school, and at least some college—to calculate income at the *occupation* × *race* × *South* ×*education* level.[68] For *occupation* × *race* × *region* variations, the four Census regions are used: Northeast, Midwest, South, and West. For both of these more granular versions of predicted income, we use the Collins and Wanamaker (2022) adjustment for farmers and self-employed fathers given the small sample size of the 1936 Expenditure Survey. Finally, we also calculate the mean of the 1950 *occscore* variable—which reflects the median total income of all persons with that particular three-digit occupation in that Census—for the 28 coarsened occupations (i.e., with no additional variation at the race or region level).

Next, we predict parental family income for working mothers and for non-working fathers. For individuals who provided information about their mothers' occupations, we predict income in an analogous way to our baseline approach, but utilizing the income of mothers who were household heads and ages 30–50 in the 1940–1990 Censuses.[69] Moreover, certain survey respondents had a missing father occupation not because the respondent did not know what the occupation was, but because the respondent reported that their father was not working (e.g., unemployed, retired). We assign a predicted income value to these non-working fathers using information on the average incomes of non-working fathers in the 1901 Cost of Living Survey and the 1940–1990 Censuses.

Throughout the robustness checks, we also consider other variations of the baseline income prediction. One variation only considers the income of fathers rather than household income. In another variation, we allow fathers with more children to get greater weight when calculating the average income in an *occupation* × *race* × *South* cell (i.e., weighting each father by the number of children younger than 18 present in the household). Last, we construct a version that uses the closest source of microdata relative to the survey respondents' childhood. In practice, this means excluding the 1900-based income measures and refraining from blending the predictions. Instead, respondents born in the 1910–1930s cohorts are assigned 1940-based income predictions (including the 1936 adjustments); those born in the 1940s are assigned 1950-based income predictions (incorporating the 1950 Census in this instance given that there is no blending); and those born in the 1950s, 1960s, and 1970s cohorts are assigned 1960-,

[68] Similar to the baseline income predictions, we use the full-count 1940 Census to adjust 1900-based income predictions by race, Southern residence, and education. Specifically, we calculate the ratio of income within an *occupation* × *race* × *South* ×*education* cell relative to the income in the analogous *occupation* × *race* × *South* cell. We then average these ratios across occupations (weighting by population) and use those averages as constant factors for scaling income in an occupation for a particular education level.

[69] Given that our 1900-based data sources do not include information about mothers, survey respondents born before 1930 with working mothers are assigned 1940-based income predictions with adjustments following Collins and Wanamaker (2022).

1970- and 1980-based predicted income, respectively.

A final variation adjusts our baseline income predictions for household size in both generations. We are able to retrieve respondent household size in thirteen of our fifteen surveys. This information is provided in one of two ways: 1) a pre-constructed survey variable for household size, or 2) a variable that lists the number of adults living in the respondent's household and another variable that lists the number of children aged 0–17 living in the household. If information on household size is given in the latter manner, we combine the two variables to obtain household size, on the condition that both variables are available for the respondent. If not, household size is recorded as missing. To adjust predicted parental income for household size, we use the 1910–1990 Censuses to calculate the median family size in that *occupation* × *race* × *South* cell when the respondent is 10 years old and take weighted averages of the median sizes in the two nearest Censuses.

E.3.4 1936 Expenditure Survey

The 1935–36 Study of Consumer Purchases, or "1936 Expenditure Survey", is one survey in the Bureau of Labor Statistics' (BLS) Cost of Living series, a recurring effort to estimate the cost of living for a "typical" American family (United States Department of Labor *et al.*, 2009). The survey consists of two data sources, an urban expenditure study conducted by the BLS and a concurrent study of small cities, villages, and farms by the Bureau of Home Economics (BHS). The purpose of this novel combination of urban and rural data on income, expenditures, and demographics was to "learn how families of different incomes, occupations, and family types apportion[ed] their expenditures among specific goods and services, in different parts of the country" (United States Department of Labor and Bureau of Labor Statistics, 1941). Income-related survey questions asked respondents to list all income received by each employed member of the household, as well as income from other sources such as gifts, interests and dividends, and pensions. Expenditure-related questions covered a wide range and include, but were not limited to, utility costs, medical care, educational expenses, automobile expenses, personal care costs, clothing expenses, furnishing expenses, and quantity of food items consumed in the week prior to the interview. Standard demographic characteristics such as relationship to the household head, age, sex, and occupation were recorded.

Primary sampling units were not chosen at random; rather, respondents were selected from 257 cities, villages, and rural counties within six geographic regions. Several subsequent rounds of random sampling within these geographic areas resulted in a sample of roughly 6,000 native-born families providing information on both income and expenditures. All families resided in the U.S. at the time of the survey.

In the urban component of the survey, the sample was further limited to families in which both husband and wife were present. The sample was almost exclusively white, with data from Black native-born families obtained only in New York, Columbus, and the Southeast. Families receiving welfare at any point during the year and families with an income level below the "customary" level (United States Department of Labor and Bureau of Labor Statistics, 1941) of non-recipient families were also excluded from the urban sample. Other restrictions related to recent housing changes and long-term guests or boarders were applied.

Similar to the urban study, husband and wife, both native-born, had to be present in the household for inclusion in the rural component of the survey. White-only families were interviewed in all regions other than the Southeast, where a separate study of Black families occurred. Families could not have moved during the survey year and had to have operated their farms, whether owned or rented. An exception was made in the Southeast, where sharecropper families were included. Selected families could not have received welfare during the 12-month period of the survey. As observed in a 1941 report from the United States Department of Agriculture (USDA), these eligibility requirements resulted in the omission of mostly less-advantaged groups from the rural study (i.e., foreign-born families, non-two parent families, large families, Black families, welfare recipients, and farm laborers) (Stiebeling *et al.*, 1941).

Like all of our other data sources, occupations in the 1936 Expenditure survey are mapped to our coarsened occupations. Table E.7 compares characteristics of 1936 respondents with those of fathers in the 1930 and 1940 Census, focusing on the five occupation $\times$ race $\times$ South groups that comprise most farmer and self-employed fathers in the early cohorts of our surveys.

As is clear from the above description, the 1936 data will not be strictly representative. However, some of its biases match the target sample (fathers with children) for our prediction exercise. As is clear from Appendix Table E.7, the 1936 sample is almost all married (as is the "target sample" from the 1940 Census that we use to calculate childhood income for our earliest cohorts). In general the Census families seem to have a somewhat greater number of children living with them than do our 1936 families.

Given its exclusion of those on welfare, we might expect the 1936 data to be somewhat positively selected.[70] However, we do not see any evidence of systematic positive selection with respect to education (which is asked in the 1936 rural, but not urban, sample), the most important socio-economic status marker we can compare. For white, non-Southern farmers, 83 (76) percent of our 1936 (1940 Census) sample finished 8th grade and 15 (13) percent finished high school. The educational attainment for white Southern farmers in the 1936 Expenditure and 1940 Census are nearly identical: 43 percent of the 1936 sample finished 8th grade, compared to 42 in the Census, and in both datasets seven percent finished high school. Black Southern farmers report very low levels of education in both datasets: two (seven) percent finished eighth grade in 1936 (1940) and two (one) percent finished high school.

[70] Conversations with Bob Margo suggest that the 1936 data might be missing both the top and the bottom parts of the distribution. As we are using the 1936 data mostly to estimate cell *means*, this concern is hopefully second-order, but still worth keeping in mind.

E.3.5 Merging predicted parental income to survey respondents' parents

As previously mentioned, we harmonize fathers' occupations (and mothers' occupation whenever available) into 28 coarsened categories. To do so, we construct crosswalks between the 1950 Census occupations and our coarsened occupations, as well as analogous crosswalks for the 1960, 1970, 1980, 2000, and 2010 Census occupations. If the occupations in a survey did not match the Census list of occupations, we created survey-specific crosswalks between the available occupation codes and our coarsened occupations.

Once we finish coarsening occupations, we merge our predicted parental income measures by father occupation, race, and whether the *respondent* grew up in the South. While our surveys provide father occupation, they do not report information on his race. We thus proxy father race with respondent race. Moreover, our surveys do not report the state or region in which the respondent's father worked when the respondent was growing up. We can, however, observe the region in which the respondent was born or grew up. We therefore use respondent residence in childhood/adolescence to proxy for father residence. Whenever we have information on both birthplace and childhood region, we use the latter to proxy for father residence.

Finally, whenever a father's occupation is unavailable but the occupation of the mother is provided, we merge in the corresponding measures of predicted income for mothers, again by mother's occupation, race, and whether the respondent grew up in the South.

E.4 Educational attainment

Our constructed measures of educational attainment always reflect years of schooling *completed*. In some surveys, respondent and father education are binned (i.e., "less than grade school," "grade school," "less than high school," etc.), while in other surveys they are categorical (i.e., 0-20+ years of schooling). To harmonize across surveys, we create two education variables.

The first binned variable assigns consecutive, ascending values as follows:

(0) no education (0 years)
(1) less than grade school (1-7 years)
(2) grade school (8 years)
(3) less than high school (9-11 years)
(4) high school (12 years)
(5) some college (13-15 years)
(6) college+ (16+ years)

In contrast, the second binned variable assigns *years of schooling* in the following manner:

(0) no education
(6) less than grade school
(8) grade school

(10) less than high school
(12) high school
(14) some college
(16) college+

We create these two variables for the respondent and for the respondent's father. Whenever available, we make similar variables for the respondents' mothers. Finally, we create indicator variables denoting high school and college completion for the respondent, for the father, and for the mother if possible.

E.5 Weights

We begin by taking the provided weight in each survey and dividing it by its mean so that the weight has an average of 1 within a survey. For surveys that consist of repeated cross-sections (i.e., the ANES and GSS), we re-center the weight in each survey year. If a survey does not have a weight, we create a weight with all values set to one. We then combine these re-centered weights into one variable, and this weight acts as our "survey weight" measure.

The main weight we use in the analysis builds on this centered weight, but adjusts it further for population characteristics. In particular, because some of our surveys are not representative by race or sex, certain cohorts in the pooled dataset will not be nationally representative. We therefore adjust the centered weights so that the share of white men, white women, Black men, and Black women in each cohort (i.e., decade) coincides with the corresponding share in the Census when these respondents were roughly 40 years old (e.g., for the 1920 cohort, we use the 1960 Census to calculate these shares). Table E.6 shows the relative weight of each survey in each birth cohort using this baseline, population-adjusted weight.

Throughout the analysis, we sometimes restrict the sample to certain respondents (e.g., individuals whose fathers are not farmers, individuals with available information on father's education). For these secondary samples, we also adjust the centered weight so that the share of white men, white women, Black men, and Black women in each cohort of that sub-sample correspond to the analogous share in the Census.

The final weight that we construct goes a step further and adjusts the survey weights not only using race and sex shares, but also the share of individuals who have graduated high school in that cohort as well as the shares of the population present in five-year age bins between ages 30–50. Similar to our baseline weight, we adjust the survey weights to match the corresponding $race \times sex \times education \times age$ shares in the Census when these respondents were roughly 40 years old.

E.6 Ranking respondents and their fathers

We rank respondents and their fathers based on family income and predicted parental income, respectively. In particular, we rank respondents relative to other survey respondents born in the same birth year. Similarly, we rank fathers relative to all other fathers with children born in the same year. Notably, we rank respondents and their fathers on the condition that we have a minimum of 100 observations in a given birth year for the relevant sample. Our baseline analysis sample ends up including individuals born in every year between 1911 and 1979. In our baseline approach, we use the population-adjusted weights when creating ranks.

Whenever we consider secondary samples of individuals, we re-rank respondents (and their fathers) so that individuals are compared to the other individuals in that sub-sample. We use the population-adjusted weights that correspond to that sub-sample when ranking.

E.7 Benchmarking two-sample two-stage least squares approach to OLS using the NLS & PSID surveys

In Appendix D, we compare our estimates of mobility (using retrospective information about the parents) to estimates of mobility that use actual (self-reported) parental income. For the NLS, we follow Davis and Mazumder (2022), using their preferred sample as well as their measures of child actual income and parental actual income. Non-missing predicted parental income indicates that the respondent provided information on retrospective parental occupation, race, and childhood region. To keep the sample consistent across specifications, we restrict the sample to respondents with available child income, parental actual income, and predicted parental income. We use two samples: one that only includes respondents with information about the father's occupation, and one that incorporates respondents with non-working fathers or with working mothers.

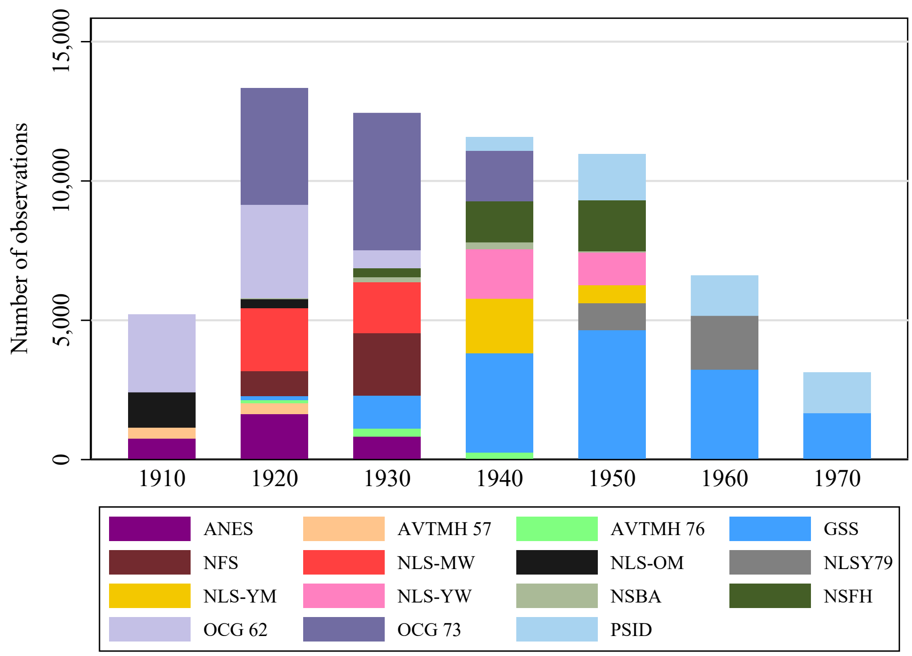
Appendix figure D.3 explores the differences in OLS estimates using actual vs. predicted income in the PSID. The usual restrictions are made (i.e., respondent is aged 30–50 and US-born). Respondents are further required to: (1) link to at least one parent present in an earlier PSID wave (using the PSID's Family Identification Mapping System), (2) to have participated in the survey at least once from 1997–2015 (when retrospective questions begin to be asked), and (3) to identify as the head/reference person or wife/spouse/cohabitating partner at least once during the same time frame (retrospective questions are only asked of these members). Finally, like with the NLS, the analysis sample consists of respondents with available child actual income and **both** kinds of parental income (i.e., actual and predicted) for their father or their mother.

E.8 Comparison to modern data

In Section 7 of the paper, we compare the mobility patterns of white men, white women, Black men, and Black women to the mobility patterns of these groups today. To make this comparison, we use publicly available data from Chetty *et al.* (2020) at https://opportunityinsights.org/data/. These data indicate the average household income rank of children growing up at the 25^{th} percentile of the income distribution by race and gender.

Next, we consider the within- and between-group components of the modern intergenerational mobility based on equations (4) and (5). For the IGE, we combine population shares as well as the mean and median incomes by race from Chetty *et al.* (2020). Specifically, we use Online Appendix Tables VI and IX to retrieve sample sizes and to parametrize the lognormal distributions of household income for the parents and children in the full population and for each subgroup. We do an analogous exercise for rank-rank correlations using publicly available data from Opportunity Insights on income distributions and population shares for the six racial and ethnic subgroups for which there are data available. We calculate that for the U.S. IGE and rank-rank correlation to fall to the Danish or Canadian level of 0.2 without between-group racial convergence in family income, the within-group IGE slopes (rank-rank correlations) would have to fall to around 0.04 (0.1). To simplify the calculation, we assume that all groups would have the same within-group slopes (Chetty *et al.* (2020) show that within-group rank-rank slopes are quite similar across groups, ranging from the mid-twenties to the low thirties, with the higher within-group mobility of Asian individuals being the exception).

Figure E.1: Survey data per birth cohort



Sources: This figure combines the 15 surveys, showing the number of respondents in each birth cohort in our baseline sample.

Table E.1: Survey cohorts and samples

Survey	Cohorts	Sample
American National Election Survey, 1956–1970	1910–1930	Representative
Americans View Their Mental Health, 1957	1910–1920	Representative
Americans View Their Mental Health, 1976	1920–1940	Representative
General Social Surveys, 1977–2018	1920–1970	Representative
Occupational Changes in a Generation, 1962	1910–1930	Representative & male
Occupational Changes in a Generation, 1973	1920–1940	Representative & male
National Fertility Survey, 1970	1920–1930	Ever-married women ages 30–44
National Longitudinal Survey of Mature Women, 1967	1920–1930	Representative & female, ages 30–44
National Longitudinal Survey of Older Men, 1966	1910–1920	Representative & male, ages 45–50
National Longitudinal Survey of Young Women, 1988	1940–1950	Representative & female, ages 34–46
National Longitudinal Survey of Young Men, 1981	1940–1950	Representative & male, ages 30–40
National Longitudinal Survey of Youth, 2002	1950–1960	Representative, ages 37–45
National Survey of Black Americans 1979–1980	1920–1950	Representative & Black Americans
National Survey of Families and Households 1987–1988	1930–1950	Representative
Panel Study of Income Dynamics, 1997 & 2017	1940–1970	Representative

Notes: This table reports the cohorts and sample for each of the 15 surveys in our baseline sample. "Representative & male" and "Representative & female" refers to having representative samples by race within an all-male or all-female survey, respectively. "Representative & Black Americans" refers to representative samples (e.g., in terms of age groups) within the Black-American population.

Table E.2: Sample size across successive restrictions, by survey

Surveys	(1) Ages 30–50 & U.S.-born	(2) +Available weight & relevant cohort	(3) +Non-missing income & father occupation
American National Election Survey, 1956–1970	3,781	3,625	3,218
Americans View Their Mental Health, 1957	1,049	930	782
Americans View Their Mental Health, 1976	804	799	665
General Social Surveys, 1977–2018	20,909	19,338	14,432
National Fertility Study, 1970	3,502	3,449	3,137
National Longitudinal Survey of Mature Women, 1967	4,846	4,782	4,090
National Longitudinal Survey of Older Men, 1966	2,115	2,088	1,630
National Longitudinal Survey of Youth, 2002	4,604	4,298	2,880
National Longitudinal Survey of Young Men, 1981	3,162	3,144	2,622
National Longitudinal Survey of Young Women, 1988	3,451	3,421	2,964
National Survey of Black Americans, 1979–1980	732	732	439
National Survey of Families & Households, 1987–1988	5,103	5,062	3,639
Occupational Changes in a Generation, 1962	10,341	10,260	6,780
Occupational Changes in a Generation, 1973	14,975	14,858	10,913
Panel Study of Income Dynamics, 1997 & 2017	7,866	5,755	5,071

Notes: The first column lists the original number of respondents ages 30–50 and U.S.-born in each survey. For the PSID, this column also imposes the restriction of keeping one member per household. Columns 2–3 show the change in sample size as we implement several restrictions. Column 2 restricts the sample to those born in the 1910–1979 birth cohorts as well as those who had a non-missing survey weight. Column 3 shows the number of respondents that meet the restriction of being in the baseline sample (i.e., having non-missing family income, race, region, and father occupation).

Table E.3: Number of observations, by cohort and survey

Surveys	1910	1920	1930	1940	1950	1960	1970
American National Election Survey, 1956–1970	754	1,632	832	—	—	—	—
Americans View Their Mental Health, 1957	392	390	—	—	—	—	—
Americans View Their Mental Health, 1976	—	117	288	260	—	—	—
General Social Surveys, 1977–2018	—	140	1,184	3,554	4,659	3,231	1,664
National Fertility Study, 1970	—	904	2,233	—	—	—	—
National Longitudinal Survey of Mature Women, 1967	—	2,249	1,841	—	—	—	—
National Longitudinal Survey of Older Men, 1966	1,267	363	—	—	—	—	—
National Longitudinal Survey of Youth, 2002	—	—	—	—	952	1,928	—
National Longitudinal Survey of Young Men, 1981	—	—	—	1,967	655	—	—
National Longitudinal Survey of Young Women, 1988	—	—	—	1,773	1,191	—	—
National Survey of Black Americans, 1979–1980	—	10	166	241	22	—	—
National Survey of Families & Households, 1987–1988	—	—	326	1,485	1,828	—	—
Occupational Changes in a Generation, 1962	2,794	3,338	648	—	—	—	—
Occupational Changes in a Generation, 1973	—	4,185	4,928	1,800	—	—	—
Panel Study of Income Dynamics, 1997 & 2017	—	—	—	500	1,657	1,446	1,468

Notes: This table lists the number of respondents in each survey and birth cohort in our baseline sample of respondents (analogous to Appendix Figure E.1). A dash indicates that zero survey respondents were born in that decade.

Table E.4: Additional details about sampling, weights, & available variables

Surveys	Sampling level	Racial oversampling	Weights available	Father edu. available	Childhood region available	Household size available
American National Election Survey, 1956–1970	HH	✓	✓		✓	✓
Americans View Their Mental Health, 1957	HH				✓	✓
Americans View Their Mental Health, 1976	HH			✓	✓	✓
General Social Surveys, 1977–2018	HH	✓	✓	✓	✓	✓
National Fertility Study, 1970	HH	✓	✓			
National Longitudinal Survey of Mature Women, 1967	Ind.	✓	✓	✓		✓
National Longitudinal Survey of Older Men, 1966	Ind.	✓	✓	✓		✓
National Longitudinal Survey of Youth, 2002	Ind.	✓	✓	✓		✓
National Longitudinal Survey of Young Men, 1981	Ind.	✓	✓	✓		✓
National Longitudinal Survey of Young Women, 1988	Ind.	✓	✓	✓		✓
National Survey of Black Americans, 1979–1980	HH	✓		✓	✓	✓
National Survey of Families & Households, 1987–1988	Ind.	✓	✓	✓	✓	✓
Occupational Changes in a Generation, 1962	HH		✓	✓	✓	
Occupational Changes in a Generation, 1973	HH	✓	✓	✓	✓	✓
Panel Study of Income Dynamics, 1997 & 2017	Ind.	✓	✓	✓	✓	✓

Notes: In the column denoting sampling level,“HH” signifies the selection of one person per household and “Ind.” indicates that multiple household members were eligible for selection into the survey. In the case of the NLS surveys, it is also possible that several household members were interviewed *across* surveys (e.g., a mother and daughter were interviewed for the NLSMW and NLSYW surveys, respectively). In the third column, “racial sampling” refers to the oversampling of Black respondents. Oversampling occurs in 3 ANES cross sections and in 2 GSS cross sections. Weights are only available in five out of seven ANES cross sections. A survey receives a checkmark in the “childhood region available” column if it asks respondents where they were born and/or where they grew up with sufficient detail to identify the four Census regions. A survey receives a checkmark in the “household size available” column if it includes sufficient information to calculate the number of individuals living in the respondent’s household.

Table E.5: Summary statistics of respondent family income, by survey

Surveys	(1) Share bottom coded	(2) Share top coded	(3) Number of bins, family income
American National Election Survey, 1956–1970	0.02	0.06	10
Americans View Their Mental Health, 1957	0.04	0.03	11
Americans View Their Mental Health, 1976	0.04	0.09	11
General Social Surveys, 1977–2018	0.03	0.11	10
National Fertility Study, 1970	0.02	0.23	12
National Longitudinal Survey of Mature Women, 1967	0.09	0.02	11
National Longitudinal Survey of Older Men, 1966	0.04	0.12	11
National Longitudinal Survey of Youth, 2002	0.04	0.09	12
National Longitudinal Survey of Young Men, 1981	0.03	0.08	11
National Longitudinal Survey of Young Women, 1988	0.03	0.27	11
National Survey of Black Americans, 1979–1980	0.06	0.10	10
National Survey of Families & Households, 1987–1988	0.05	0.16	10
Occupational Changes in a Generation, 1962	0.10	0.04	10
Occupational Changes in a Generation, 1973	0.01	0.06	10
Panel Study of Income Dynamics, 1997	0.07	0.09	11
Panel Study of Income Dynamics, 2017	0.07	0.09	12

Notes: Columns 1 and 2 list the share of respondents whose total family income is in the bottom and top bin, respectively, in that survey. All shares are based on the baseline sample and are unweighted. Column 3 lists the number of bins for respondent family income in each survey. For the ANES and GSS, we report the median number of bins across survey years.

Table E.6: Relative weight of each survey, by birth cohort

Surveys	1910	1920	1930	1940	1950	1960	1970
American National Election Survey, 1956–1970	0.38	0.14	0.07	—	—	—	—
Americans View Their Mental Health, 1957	0.18	0.04	—	—	—	—	—
Americans View Their Mental Health, 1976	—	0.01	0.02	0.02	—	—	—
General Social Surveys, 1977–2018	—	0.01	0.10	0.31	0.43	0.49	0.53
National Fertility Study, 1970	—	0.11	0.20	—	—	—	—
National Longitudinal Survey of Mature Women, 1967	—	0.26	0.17	—	—	—	—
National Longitudinal Survey of Older Men, 1966	0.14	0.02	—	—	—	—	—
National Longitudinal Survey of Youth, 2002	—	—	—	—	0.10	0.27	—
National Longitudinal Survey of Young Men, 1981	—	—	—	0.15	0.06	—	—
National Longitudinal Survey of Young Women, 1988	—	—	—	0.18	0.10	—	—
National Survey of Black Americans, 1979–1980	—	0.00	0.01	0.02	0.00	—	—
National Survey of Families & Households, 1987–1988	—	—	0.03	0.13	0.16	—	—
Occupational Changes in a Generation, 1962	0.31	0.19	0.05	—	—	—	—
Occupational Changes in a Generation, 1973	—	0.22	0.34	0.13	—	—	—
Panel Study of Income Dynamics, 1997 & 2017	—	—	—	0.06	0.15	0.24	0.47

Notes: This table shows the relative weight of each survey in each birth cohort using the baseline sample of respondents and population-adjusted weights. Each cell of the table divides total weight for a *survey* in a given birth decade by total weight for the entire birth decade. A dash indicates that 0 survey respondents were born in that decade. “0.00” corresponds to observations that are given extremely low weights in that cohort (i.e., due to small numbers of respondents born in that decade).

Table E.7: Summary statistics, comparing 1936 survey fathers to Census fathers

	1930 Census	1936 Survey	1940 Census
Self-employed × white × non-South			
Age	40.31	40.95	40.68
Lives in Northeast	0.46	0.13	0.44
Lives in Midwest	0.39	0.57	0.41
Lives in West	0.14	0.31	0.16
Married	0.99	1.00	0.99
# of kids <18 in household	2.29	2.04	1.94
Owns his home	0.59	0.48	0.52
Family income, 1936$	—	2,369	—
Share in Census	0.05	—	0.04
Observations	5,161	166	4,439
Self-employed × white × South			
Age	40.19	42.14	40.06
Married	0.98	1.00	0.99
# of kids <18 in household	2.44	1.66	2.03
Owns his home	0.64	0.66	0.65
Family income, 1936$	—	2,298	—
Share in Census	0.01	—	0.01
Observations	1,419	35	1,539
Farmer × white × non-South			
Age	40.31	41.44	40.59
Completed 8th grade	—	0.83	0.76
Completed HS	—	0.15	0.13
Lives in Northeast	0.13	0.26	0.11
Lives in Midwest	0.72	0.54	0.75
Lives in West	0.15	0.20	0.14
Married	0.98	1.00	0.98
# of kids <18 in household	2.99	2.22	2.70
Owns his home	0.57	0.65	0.52
Family income, 1936$	—	1,365	—
Share in Census	0.10	—	0.07
Observations	10,186	316	7,791
Farmer × white × South			
Age	40.03	40.46	40.02
Completed 8th grade	—	0.43	0.42
Completed HS	—	0.07	0.07
Married	0.98	1.00	0.99
# of kids <18 in household	3.54	2.80	3.03
Owns his home	0.46	0.53	0.47
Family income, 1936$	—	1,118	—
Share in Census	0.08	—	0.06
Observations	8,266	205	6,904
Farmer × Black × South			
Age	40.52	41.44	39.75
Completed 8th grade	—	0.02	0.07
Completed HS	—	0.02	0.01
Married	0.96	1.00	0.98
# of kids <18 in household	3.89	3.02	3.85
Owns his home	0.19	0.06	0.18
Family income, 1936$	—	474	—
Share in Census	0.03	—	0.02
Observations	2,626	57	2,187

Notes: The sample in the table consists of white and Black men ages 30–50 with at least one child living in the household. All estimates are weighted. Census fathers come from 1% Census samples from Ruggles *et al.* (2021). In the 1936 survey, "Owns his home" refers to owning his own dwelling for self-employed fathers and owning any amount of acreage for farmer fathers. For all fathers in the Census, "owns his home" refers to owning his own dwelling. "Share in Census" refers to the share of fathers ages 30–50 with that occupation, race, and region.