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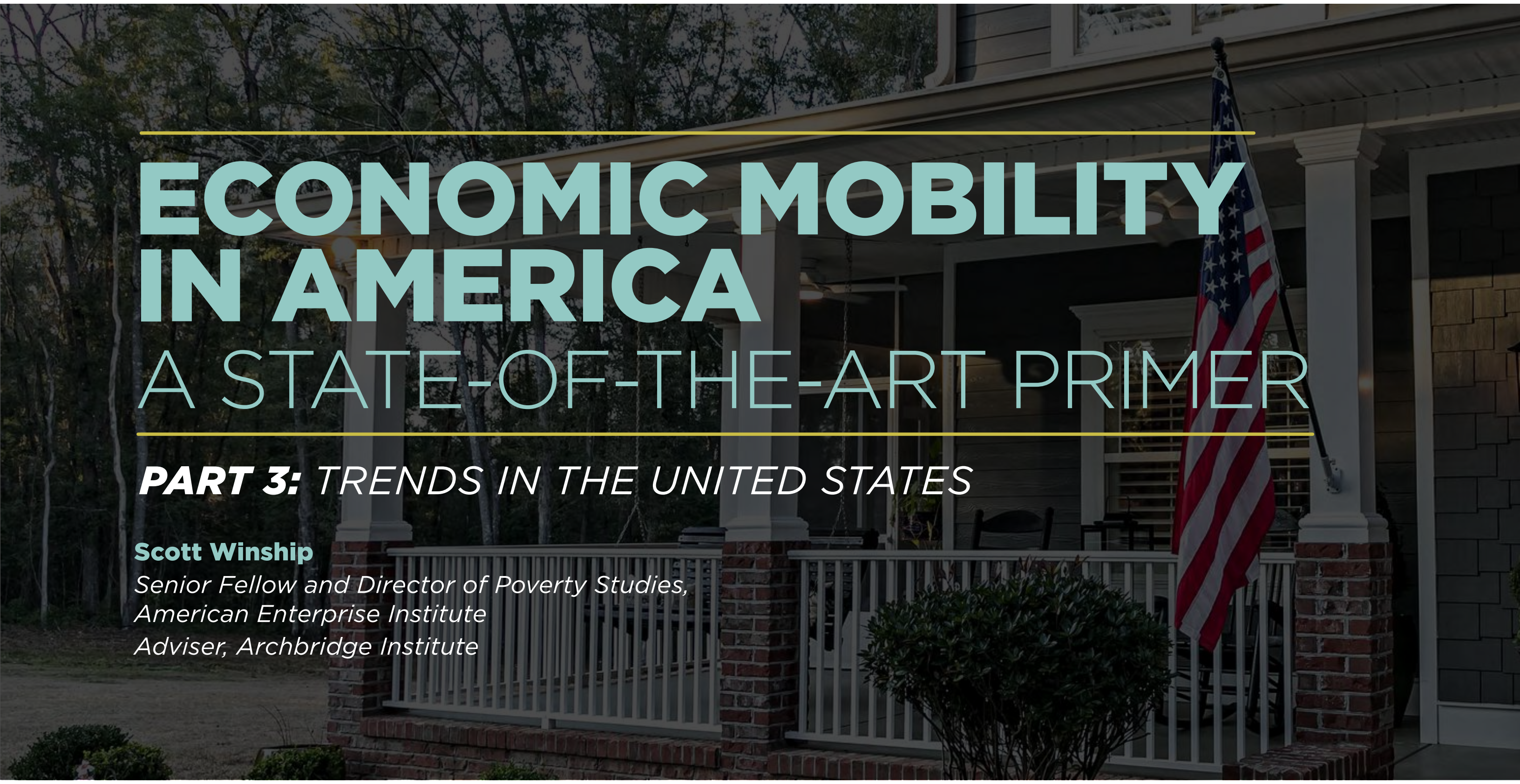

ECONOMIC MOBILITY IN AMERICA

A STATE-OF-THE-ART PRIMER

PART 3: TRENDS IN THE UNITED STATES

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WHAT'S INSIDE

EXECUTIVE SUMMARY

Is the American Dream dying? A number of studies have examined this question by looking at trends in intergenerational economic mobility. However, studies using different datasets have produced different results. Few studies consider both relative and absolute mobility trends for both men and women. Few consider both summary measures of mobility and finer-grained measures. And few compare results from two different data sources.

This report is the third in a series summarizing the state of intergenerational mobility in the United States. While the first considered contemporary levels of mobility by different measures, and the second compared American mobility levels to those in other countries, this installment looks at trends in the US. A number of trends over the past 40 years are presented using four sets of birth cohorts from the Panel Study of Income Dynamics (PSID) and three sets of cohorts from the National Longitudinal Surveys (NLS). These birth cohorts are chosen so as to be comparable to each other to the extent possible.

I compare sons' and daughters' earnings and family income to the earnings and family income of their parents. I also summarize the complete literature on trends in American intergenerational earnings and income mobility, with an extensive critique of one influential study (Aaronson and Mazumder, 2008). The overall picture is of an American Dream that endures to a greater extent than is appreciated, though one that remains insufficiently accessible to some. The findings are as follows:

Relative Mobility Among Men

- Relative mobility is concerned with the extent to which adults transcend their parents' rankings by moving up or down in ranks. If everyone gets richer, but the poorest children still end up as the poorest adults, there is limited relative mobility.
- Relative earnings mobility, comparing fathers and sons, may have increased slightly over time. To the extent that it did, it likely reflected increased downward mobility from the top.
- Relative mobility trends are similar whether parental family income in adolescence is compared with men's subsequent earnings or with their own family income in adulthood. Relative mobility may have declined, but any change was modest. According to the NLS data, for example, rather than the richest and poorest adolescent sons being separated by just under 25 percentiles in adulthood, as in 1981 (for cohorts born around 1950), they were separated by around 30 percentiles in adulthood in 2014 (for cohorts born in the early 1980s).
    - There is evidence that both upward mobility from the bottom and downward mobility from the top declined. When sons' earnings are the outcome, both the PSID and NLS indicate reduced downward mobility, and both indicate reduced upward mobility when considering sons' family income.

Relative Mobility Among Women

- Daughters' relative mobility also likely declined, whether comparing father earnings, mother earnings, or parental income to daughters' earnings or family income.
- Whether fathers' or mothers' earnings are the baseline, the decline in mobility was modest, except comparing mothers' and daughters' earnings when women without any earnings are included.
- The declines using parents' family income as a baseline are modest in the NLS but sizable in the PSID. In the PSID the adulthood gap between the richest and poorest children rose by 14 to 19 percentiles over the long run.
    - Lower mobility for daughters primarily reflects diminished upward mobility from the bottom.

Absolute Mobility Among Men

- Absolute mobility is concerned with changes in real (inflation-adjusted) income between generations. If poor children stay poor as adults but see large income gains over what their parents received, they experience upward absolute mobility.
- Using a summary measure often described incorrectly as an indicator of relative mobility (the "intergenerational elasticity"), mobility comparing fathers' and sons' earnings changed minimally, possibly increasing between the 1952–59 and 1976–83 birth cohorts.
- Using parents' family income as a benchmark, the change in mobility over 30 years is minimal, though the short-term trends in the PSID and NLS look very different.
    - Mobility was the same in 2014 (among men born 1982–84) as in 1981 (among men born 1949–51) in the sense that the absolute gaps between sons in childhood were similarly narrowed by adulthood in both years.

For example, a 10 percentage-point gap between sons in childhood typically narrowed to a 2.0-point gap in adulthood in 1981. In 2014, it only narrowed to a 2.5-point gap. (The elasticity rose from 0.21 to 0.26.) This estimated change in the NLS samples was small enough that we cannot discount the possibility that no change occurred in the real world represented by these samples.

- Turning to the likelihood that sons will do better than their parents, I find that there has been little change in the share of men who have higher earnings than their fathers.
- It appears that there was little change in the share of sons whose family income exceeds that of their parents, though that share falls in the PSID when incomes are adjusted for family size. In the NLS, for instance, the share was 51 percent among sons born 1949–51 and 53 percent among sons born 1982–84. However, the change in the PSID (using size-adjusted incomes) was from 66 percent to 56 percent, comparing cohorts born 1952–59 to those born 1976–83.
    - The finding of relatively flat absolute mobility is in contrast to widely publicized results from Raj Chetty and his colleagues showing a large decline.
    - I argue that an ideal measure, capturing family income over all of childhood rather than in a single year, would show a decline in absolute mobility, though a smaller one than is reported by Chetty's study and others.
    - The difference between my results and those other studies is that I measure parent income in adolescence rather than near birth. Because income growth was so fast during the 1950s and early 1960s, it is more difficult for sons born mid-century to exceed parental income when the bar is set later in childhood rather than earlier in childhood.

Absolute Mobility Among Women

- The PSID and NLS yield conflicting results. In the PSID, the intergenerational elasticity rises over time, sometimes substantially, indicating falling mobility. In the NLS, the intergenerational elasticity is flat over time. For example, in the PSID, comparing parent family income to daughters' earnings, the IGE rose from 0.25 for cohorts born 1952–59 to 0.43 for those born 1976–83. In the NLS, the elasticity was 0.26 for those born 1951–53, 0.23 for those born 1962–64, and 0.26 for those born 1981–83.
- Outpacing one's father's earnings has increased among daughters, but fewer daughters exceed their mothers' earnings or, perhaps, their parents' family income.

01 | Introduction

Is opportunity as widespread in the United States as in the past, or is it harder for Americans to get ahead today? Answering the question requires not only a way to assess "opportunity" or "getting ahead," but data with consistent measures of these concepts over time. Data constraints have impeded researchers' ability to look at long-term trends in opportunity, but ambiguity about what "opportunity" means also has made interpreting the evidence complicated. Consequently, little consensus exists about the course of opportunity in America.

This report is the final one in a three-part primer assessing the state of intergenerational economic mobility in the US. Comparing individuals' economic circumstances to those of their parents is a natural way to assess their opportunities, though, as discussed in Part One of this primer, looking at outcomes to infer opportunities is necessarily an indirect approach. Part One built on research by economist Bhashkar Mazumder to show that by many measures, intergenerational mobility in the US is lower than most researchers have concluded over the years.[1] The exception to this conclusion is that Americans are more likely to exceed the income of their parents than the most-heralded paper on the topic finds.[2]

Whether mobility is higher or lower than indicated in past research is less important than whether mobility is high or low. But how to make such an assessment? One way is to compare mobility levels in the United States to those in our peer countries. Part Two of this primer summarized research on this topic.[3] The conventional wisdom among researchers and many public commentators is that mobility is greater in most other rich nations than in the US. However, Part Two concluded that this consensus is unsupported for some types of mobility, such as the extent to which individuals' ranking in the distribution of male or female earnings is linked to how their fathers ranked against each other.[4]

A second way to assess levels of American mobility is to compare contemporary levels to levels of American mobility in the past. This question is the subject of the current paper, Part Three of the primer. As with so much of the conventional wisdom on economic mobility, our understanding of trends is clouded by imprecision and oversimplification. The problems are well conveyed by examining a recent interview with Harvard University economist Raj Chetty.[5] Chetty described his research as finding "a really dramatic fading of the American Dream." In that instance, Chetty was referring to a study he coauthored with others that found a sizable decline in the share of Americans who exceed their own parents' income (after adjusting for the rise in the cost of living). His team found that by this measure of "absolute mobility," opportunity has declined in the US.

However, almost exactly two years before this paper was released to grave headlines, Chetty and his colleagues released a study finding that "children entering the labor market today have the same chances of moving up in the income distribution (relative to their parents) as children born in the 1970s."[6] That is, by their measure of "relative mobility," opportunity in America has not declined. In fact, the Chetty team's absolute mobility estimates were the result of assuming, based on the earlier paper, that relative mobility was the same for children born in 1940 as in the early 1980s.

Later in the interview, Chetty asserts that

> [i]f you were an immigrant choosing where to go and have the best chances of climbing the income ladder, then statistically you'd have a better shot of achieving "the American Dream" if you're growing up in Canada or in many Scandinavian countries than the United States. That's just a fact.

But that conclusion is only "a fact" using some measures of economic mobility. In particular, Americans are as able to exceed their parents' income as are Canadians, and it is likely that Americans have higher absolute mobility than Danes.[7] Chetty is declaring that the American Dream is "fading" based on a measure of absolute mobility that suggests Americans do as well as Canadians and better than Danes, and he is declaring the American Dream is more attainable in Canada and Denmark using a measure of relative mobility that, according to his own data, has not worsened in the US.

This report assesses trends in intergenerational mobility, both relative and absolute, for men and for women. It looks at grown-child outcomes from the early 1980s to the present for sons and daughters born between the late 1940s and the mid-1980s. The results are not easily summarized but there is little consistent evidence that mobility has declined notably across types of mobility or across data sources, for men or for women. The exception to this ambiguity is that fewer Americans surpass the incomes of their parents than was true 75 years ago, but even this decline has probably been smaller than the Chetty findings suggest. The executive summary—as well as section summaries throughout the paper—describe the findings in more detail.

02 | Concepts and Methods

As discussed in the first installment of this primer, there are a variety of different ways to assess economic mobility. The primer has focused on mobility in terms of income rather than occupation, education, wealth, or other status indicators. And it has focused on intergenerational mobility (how individuals' incomes compare with those of their parents) rather than intra-generational mobility (how individuals' own incomes move up or down as they age). Beyond these distinctions, it is useful to consider two kinds of intergenerational income mobility—relative and absolute. As described in Part Two of the primer,

> Relative mobility refers to movement between income ranks between generations, regardless of how much income grows at different points in the distribution. If all children end up 20 percent richer than their parents, but the poorest children become the poorest adults and the richest children become the richest adults, no relative mobility will have occurred. Absolute mobility … refers to movement in terms of dollars. When everyone ends up 20 percent richer than their parents, they all experience upward absolute mobility, even if no one sees any relative mobility.

While relative mobility is concerned with the position of people within the income distribution of their peers and how that compares with the position of their parents relative to their peers, measures of absolute mobility focus on how income levels compare with parental income levels, without regard to how anyone ranks in their income distribution.

Whether considering relative or absolute mobility, we can further distinguish between distributional measures that may be used to summarize mobility from different parts of the income distribution and summary measures that indicate in a single number how intergenerational mobility mitigates childhood inequality. As with the previous two installments of this primer, this report will analyze all four ways of measuring intergenerational income mobility.

METHODOLOGICAL CONSIDERATIONS

The previous installment of this primer ran through a number of methodological decisions that complicate the measurement of economic mobility—decisions treated in more detail in Part One:

> Some relate to income measurement. These choices include whether to account for noncash government benefits, employer fringe benefits, or taxes; whether to combine the incomes of roommates or unmarried romantic partners living together; how to account for increases in the cost of living; and whether to factor in the different needs of families and households of varying sizes.
>
> A number of other measurement problems make it challenging to estimate mobility. Income may often be poorly measured at the bottom of the income distribution, where underreporting in surveys and administrative data is common and where business owners may be found whose well-being is not well-reflected by their income in a given year. Especially at the top of the income distribution, tax avoidance strategies can affect what shows up as income in administrative data. Measuring self-employment earnings also raises the question of how to allocate self-employment income between labor income (a form of "earnings," based on the work that is put into the job) and capital income (deriving from the investments made in the business).
>
> Beyond measurement issues, other methodological decisions require attention. How should people or households reporting no earnings or income be treated in mobility analyses? When the earnings of non-resident fathers are unavailable, should those sons be dropped from analyses, or should earnings be imputed to absent fathers? What should the researcher do about survey attrition and nonresponse, which affect who shows up in mobility analyses using longitudinal data?

These technical issues are challenging when estimating cross-sectional mobility levels, but they are even more problematic when comparing countries' mobility levels or changes in mobility over time within a country. Considering trends, the share of income coming from noncash government benefits and employer fringe benefits has increased over time, but income measures that serve as the basis for mobility estimates almost always exclude all or most of these sources. Income is also typically measured before taxes are considered, and tax rates have fallen over time, meaning that disposable income has risen more than pre-tax income.

Other changes that may be important especially for trend estimates include:

- Declining family size (which can have different implications for well-being depending on whether, for example, it reflects choices unrelated to hardship that increase income per person, a greater ability to live alone due to increased affluence, or an inability to afford the larger families of the past);[8]
- Rising cohabitation (the income of unmarried partners is often excluded from "family income");
- Rising father absence (which decreases the number of grown children who may be matched in datasets to fathers with earnings);
- Falling labor force participation (which lowers annual incomes for those only in the labor force part of the year and increases the number of adults without earnings);
- Potentially changing income volatility (which means that a single year of income will be a better or worse estimate of someone's "typical" income);
- Lengthier investment periods before people begin their prime working years (which complicates comparisons of adults to their parents at the same age rather than at the same point in their careers); and
- Potentially changing data quality (including survey nonresponse).

When looking at absolute income growth, it is important to take account of the rising cost of living. That makes the accuracy of inflation measures important, since the most commonly used measures overstate the increase in the cost of living and will understate income growth if used to adjust nominal incomes.[9] This issue affects how many adults will be found exceeding their parental incomes, but it may be especially important if our concern is how that number has changed over time.

When looking at trends in mobility, another issue can introduce comparability problems—the specific years in which parent and grown-child incomes are observed. For example, if grown-child incomes are measured during a stronger point in the business cycle than are parent incomes, that should be the case for all of the mobility estimates being compared over time. If a recent mobility estimate compares grown children in a recession year to their parents in a peak year, while an earlier estimate compares grown children and parents both in recession years, the resulting trend estimate may not accurately convey what the underlying mobility trend would look like if measures of lifetime income were available.

One final issue to note is that there simply are not many datasets that allow for direct estimation of intergenerational mobility for Americans born in different years over a long period of time. Researchers are often left to decide whether to use potentially incomparable surveys, to use clever techniques to impute parental income, or to rely on datasets with samples too small to yield precise estimates.

A number of these methodological issues are addressed in this report and its Appendix 1, insofar as they affect the original analyses presented here. This report attempts to convey what we know about trends in American intergenerational income mobility, but it will be apparent that the methodological challenges involved make strong conclusions about trends inadvisable. The main report describes long-term trends while Appendix 2 provides results in more detail. I focus throughout on research comparing the incomes and earnings of grown children to their parents. A related literature on intergenerational mobility attempts to assign parent income to adults when it cannot be observed directly, summarized in Appendices 3 and 4, the latter of which critiques one particularly highly cited study (Aaronson and Mazumder, 2008). I do not consider intergenerational trends in occupational or educational mobility, nor trends in intragenerational mobility.

DATA SOURCES AND METHODS

The analyses below use two data sources: the Panel Study of Income Dynamics (PSID), administered and maintained by the University of Michigan Institute for Social Research's Survey Research Center, and the Bureau of Labor Statistics' National Longitudinal Surveys (NLS).

The PSID began in 1968 with a nationally representative set of households. It continues to follow them today, with the latest wave of available data coming from 2019 interviews. More importantly, it followed children in the original households as they grew up and formed their own households, and it has followed their offspring as well. Part One of this primer relied on the PSID to estimate contemporary mobility estimates (without considering trends). The estimates in Part One are preferred to those shown in this report as depicting Americans' mobility *levels*, but it is not possible to estimate *trends* that incorporate the methodological improvements used in Part One.

I estimate mobility for four sets of birth cohorts in the PSID, born 1952–59, 1960–67, 1968–75, and 1976–83. Sons' and daughters' own incomes are averaged between the ages of 30 and 35 (using up to three observations). Parental incomes are averaged over up to five consecutive years when children are between the ages of 9 and 21.

In the NLS data, I have mobility estimates for three cohorts, taken from independent surveys. The National Longitudinal Survey of Young Men (NLSYM) followed a nationally representative group of men who were ages 14 to 24 as of the end of March 1966. The survey ended in 1981, when the men were 29 to 39 years old. Similarly, the National Longitudinal Survey of Young Women (NLSYW) began with a group of women ages 14 to 24 at the end of 1967. It followed them through 2003. Many of these youth may be linked to their parents who were surveyed in the National Longitudinal Survey of Older Men and the National Longitudinal Survey of Mature Women, which also began in the 1960s and continued for years thereafter.

Estimates for the second cohort of NLS men and women come from the National Longitudinal Survey of Youth 1979 (NLSY79), which has followed a nationally representative group of men and women who were 14 to 21 years old at the end of 1978. Finally, the third cohort estimates are taken from the National Longitudinal Survey of Youth 1997 (NLSY97), which follows men and women who were 12 to 16 years old at the end of 1996. For all three NLS cohorts, I use a single year of parental income during adolescence and a single year of grown-child income when they are in their early 30s. The resulting analyses compare sons born 1949–51, 1961–63, and 1982–84, observing them in 1981, 1993, and 2014 and daughters born 1951–53, 1962–64, and 1981–83, observing them in 1984, 1995, and 2014.

In the PSID, earnings of household heads and their partners are included every year, as well as family income measures. Parental family income measures are available for all of the NLS cohorts, but information on father earnings is unavailable for the middle cohort (NLSY79) and only available for children with older fathers in the earliest cohorts (NLSYM and NLSYW). Parents and grown children with negative income or no income (henceforth, "non-positive income") are included in the main analyses, and I discuss results excluding non-positive incomes when relevant. Additional details on the PSID and NLS datasets and on my methodological decisions are provided in Appendix 1. Table 1 presents some descriptive statistics specific to the family income mobility analyses discussed in the report.

The PSID and NLS data have been used by previous researchers to analyze mobility trends, as will be reviewed below. However, relatively few PSID papers have leveraged recent waves of data that allow researchers to consider 30-year mobility trends without reliance on modeling to overcome small samples, and none have considered the range of mobility estimates presented here. No study has presented as many different kinds of NLS estimates as does this report, and in fact, none have attempted to measure mobility using all three cohorts available.

Table 1 | DESCRIPTIVE STATISTICS, PSID AND NLS ANALYSES

	PSID—Sons				PSID—Daughters			
	Cohort 1	Cohort 2	Cohort 3	Cohort 4	Cohort 1	Cohort 2	Cohort 3	Cohort 4
Birth Years	1952-59	1960-67	1968-75	1976-83	1952-59	1960-67	1968-75	1976-83
Parent Income Years	1968-72	1976-80	1984-88	1992-96	1968-72	1976-80	1984-88	1992-96
Child Income Years	1982-94	1990-02	1998-10	2006-18	1982-94	1990-02	1998-10	2006-18
Sample Size	1,009	868	535	771	1,105	970	607	856
Median Father Age	42	42	42	42	42	43	42	43
10-90th Percentile Father Age	36-53	34-51	36-52	36-50	36-54	34-54	33-52	35-52
Median Mother Age	40	40	39	39	40	40	39	39
10-90th Percentile Mother Age	33-49	32-49	33-49	32-47	33-49	33-50	32-48	31-48
Median Parent Family Inc	68,992	77,450	81,059	82,747	64,352	78,986	76,453	77,120
10th Percentile Parent Inc	30,417	30,417	27,634	26,379	25,347	30,614	25,494	25,470
90th Percentile Parent In	121,474	138,946	153,393	179,589	122,001	147,190	153,393	169,736
Median Child Family Income	68,917	71,057	80,678	78,913	65,241	70,643	76,763	79,781
10th Percentile Child Inc	26,941	28,311	29,023	23,864	21,373	23,510	28,187	25,018
90th Percentile Child Inc	124,302	144,441	173,852	161,875	126,809	142,401	168,868	162,985
Ave. Unemp. Rate, Childhood	4.7	6.8	6.7	6.3	4.7	6.8	6.7	6.3
Ave. Unemp. Rate, Adulthood	7.0	5.6	5.7	6.2	7.0	5.6	5.7	6.2

	NLS—Sons			NLS—Daughters		
	Cohort 1	Cohort 2	Cohort 3	Cohort 1	Cohort 2	Cohort 3
Birth Years	1949-51	1961-63	1982-84	1951-53	1962-64	1981-83
Parent Income Years	1966	1978	1996	1967	1978	1996
Child Income Years	1981	1993	2014	1984	1995	2014
Sample Size	704	1,113	1,352	628	1,048	1,316
Median Father Age	45	45	42	44	44	42
10-90th Percentile Father Age	37-53	38-56	35-50	37-54	37-56	36-50
Median Mother Age	40	42	40	41	40	40
10-90th Percentile Mother Age	34-48	36-53	34-47	35-49	34-51	34-48
Median Parent Family Inc	55,412	56,695	64,051	53,947	59,679	63,608
10th Percentile Parent Inc	16,883	19,049	15,932	19,571	17,904	14,895
90th Percentile Parent In	101,296	116,375	138,529	91,670	113,391	140,020
Median Child Family Income	53,709	57,130	69,039	53,969	60,621	73,353
10th Percentile Child Inc	19,914	17,766	16,181	10,962	14,397	12,945
90th Percentile Child Inc	95,471	122,331	161,821	107,947	127,454	172,598
Ave. Unemp. Rate, Childhood	3.8	6.1	5.4	3.8	6.1	5.4
Ave. Unemp. Rate, Adulthood	7.6	6.9	6.2	7.2	5.6	6.2

Notes: Parent Income Years column shows years over which parent incomes are measured. Child Income Years column shows years over which child incomes are measured (every other year, up to three observations in the seven-year window in the PSID). Sample size, parent age, and family income columns show number of observations for analyses involving parent and child family incomes. Analyses involving earnings involve somewhat different sample sizes and different ages and incomes (not shown). Parent age and family income statistics are weighted in the PSID using the last grown child weight available over the years incomes are averaged and in the NLS using the grown child weight. Parent age columns are as of the first year incomes are averaged in the PSID (1968, 1976, 1984, and 1992). Family income amounts are in 2020 dollars, using the PCE deflator. Unemployment rates are from the Bureau of Labor Statistics. For the PSID, they are averaged over the years in which parent or child incomes are measured (five years for parents, seven for grown children).

03 | Relative Economic Mobility

This report's examination of recent mobility trends begins with relative mobility measures. Until very recently, few studies of mobility trends had focused on relative measures (an assertion that will come as a surprise to those who wrongly believe that one popular measure, the "intergenerational elasticity" is an indicator of relative mobility). This section presents original estimates of the trend in relative mobility and compares these estimates with the sparse results from earlier research.

SONS

Most research on intergenerational mobility trends has looked at the outcomes of men. In large measure, that reflects the worse labor market trends for men over the past fifty years, which has directed attention to male economic "stagnation" and "decline."[10]

Father Earnings vs. Son Earnings

INCOME RANK ASSOCIATION

A first way to assess the intergenerational mobility of men is to compare the earnings of grown sons to those of their fathers. In the earlier installments of this primer, the analyses were split not only between relative and absolute mobility, but also between distributional measures of economic mobility and summary measures of the persistence of childhood economic inequality. The former may be used to describe mobility between different parts of the joint distribution of parental and grown-child income. Distributional measures can tell us whether poor children experience more upward mobility than in the past, whether middle-class children have become more or less likely to move up or down, and whether well-off children have become increasingly likely to remain well off as adults. Summary measures indicate in a single number the extent to which mobility patterns tend to close the gaps between richer and poorer children by the time they become adults.

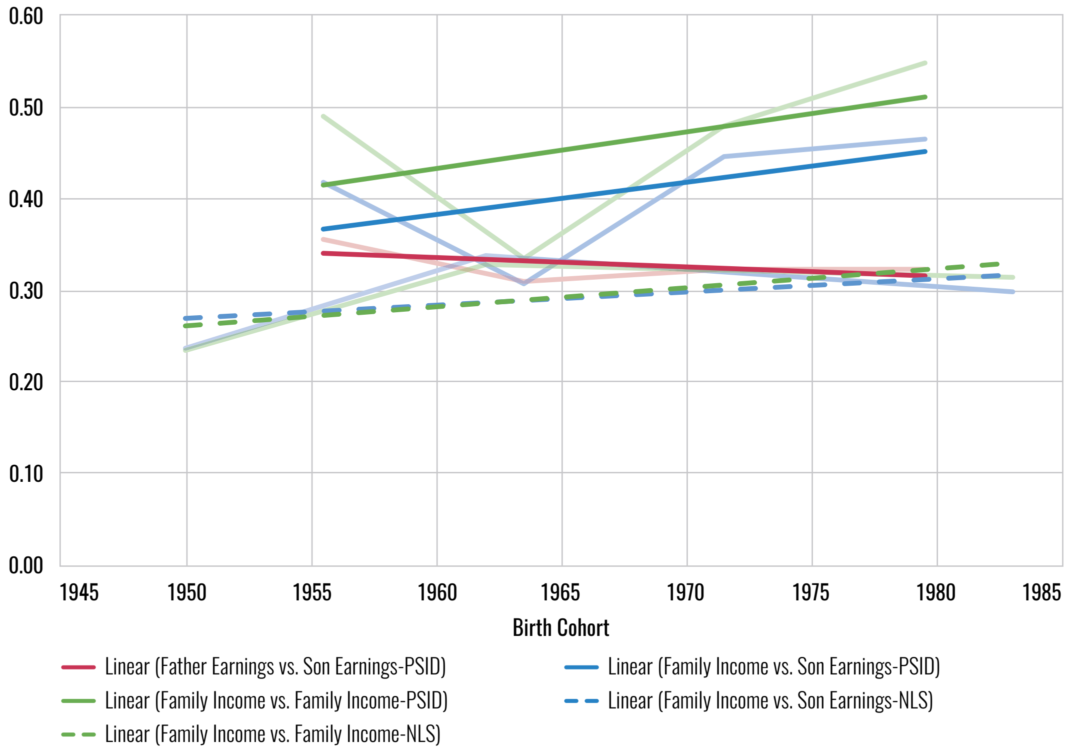
As discussed in Part One of this primer, the most common way to summarize the persistence of childhood *relative* inequality is the income rank association (IRA), also called the "rank-rank slope" or the "Spearman rank correlation." The IRA indicates the extent to which percentile gaps between children narrow when their own income percentiles in adulthood are compared. An IRA of 0.30 implies that between the richest and poorest children (separated by nearly 100 percentiles), the richer one will tend to have an income in adulthood that exceeds that of the poorer by 30 percentiles. Children separated by 10 percentiles in childhood will tend to be separated by only three in adulthood.

The first of this report's mobility trends are shown in Figure 1. I focus on the long-term trends and provide more detailed results in Appendix 2. The intermediate trends sometimes move up or down, often in ways that are inconsistent between the PSID and NLS. In part, this is because there is a fair amount of imprecision in the individual estimates. In part, the differences may reflect the different birth cohorts being examined. (The PSID data points are each for eight cohorts, while the NLS points are for three cohorts each.)

However, it is also true that any short-term change can reflect differences in the specific years in which parental income or grown sons' and daughters' incomes are measured, or changes in survey administration over time (including the way that income questions are asked). As indicated in Table 1, above, for each set of cohorts in each dataset, parent and child incomes are measured in different points of the business cycle. For some mobility estimates, the economy is stronger when parent incomes are measured than when grown-child incomes are measured, while the reverse is true at other times. (However, there are often short-term differences between the PSID and NLS trends even when they reflect similar points in the business cycle.) While the short-term trends in this paper are difficult to interpret, in most cases the long-term trends are similar in both datasets.

In **Figure 1**, the PSID estimates for four cohorts and NLS estimates for three are displayed as lighter lines, while the dark lines display the linear trends through the four (or three) data points. The red lines display the IRA trend

Figure 1 | SONS' INCOME RANK ASSOCIATION (IRA), 1949-84 BIRTH COHORTS



Notes: Estimates are from the Panel Study of Income Dynamics and National Longitudinal Surveys. PSID analyses use four sets of birth cohorts, born 1952–59, 1960–67, 1968–75, and 1976–83. Sons' outcomes are averaged between the ages of 30 and 35 when observed (up to three times) 1982–94, 1990–2002, 1998–2010, or 2006–18. Parental income is averaged over up to five consecutive years, either 1968–1972, 1976–80, 1984–88, or 1992–96. NLS analyses use three sets of birth cohorts, born 1949–51, 1961–63, or 1982–84. Outcomes are measured between the ages of 30 and 32 when observed in 1981, 1993, or 2014. Parental income is measured in 1966, 1978, or 1996. For full methodological details, see Appendix 1. For point estimates and standard errors, see Appendix 2.

for male earnings, from the PSID. The trend shown by the light red line compares sons born 1952–59, 1960–67, 1968–75, and 1976–83, displaying the data points at 1955.5, 1963.5, 1971.5, and 1979.5.

Relative mobility was unchanged over this period. The IRA for the earliest set of cohorts was 0.36, and for the most recent cohorts it was 0.32. (Appendix 2 provides estimates for all cohorts for all trends.) The change is far too imprecise to have confidence an actual decline in the IRA (an increase in mobility) occurred. Given the magnitude of the change and the number of men in the PSID samples, statistical methods allow us to assess how likely it would be to find a change in the data even if there were no true change in the American population. The decline in the IRA fails to achieve "statistical significance" in this sense.[11] The linear trend line closely resembles the trend between the earliest and most recent cohorts.

These estimates are lower than the ideal estimates would be, overstating intergenerational mobility.[12] That is to be expected, since I only average a limited number of earnings observations for fathers and sons and since I observe sons when relatively young. However, what is of interest in this report is the trend, and that will be unaffected unless the bias in my estimates changes over time.

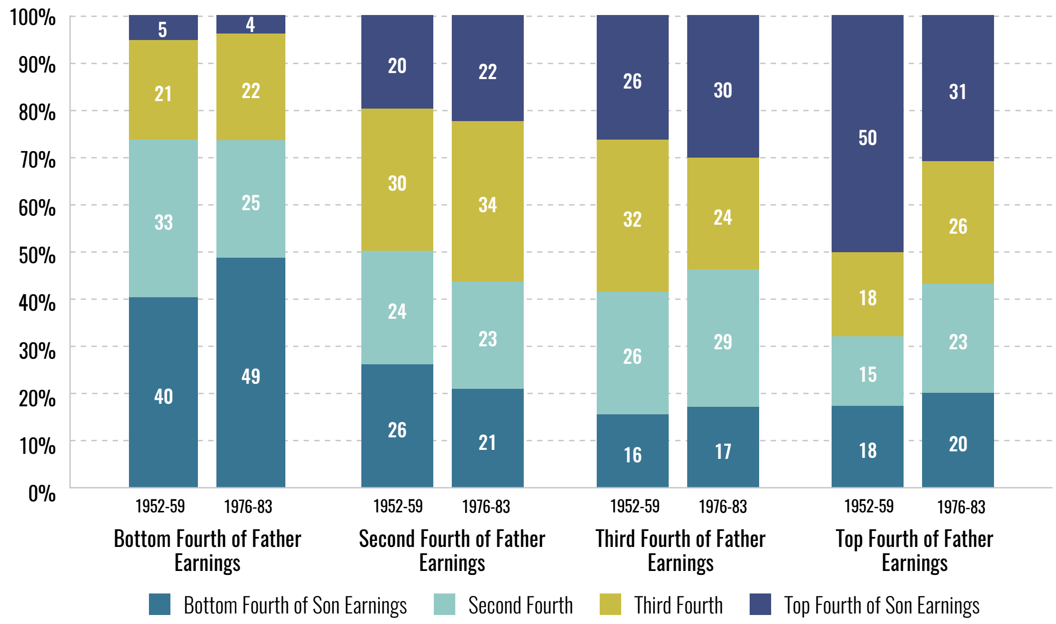
Only one other previous study examines trends in male earnings IRAs in the US. Consistent with my results, Justman and Stiassnie (2021) find very little change between PSID cohorts born 1952–61 and those born 1972–81.

TRANSITION PROBABILITIES

The most common way to analyze distributional measures of relative mobility is to estimate transition probabilities. The distribution of parental income is divided into equal-sized groups, or "quantiles" (typically tenths, fifths, or fourths), as is the distribution of income among adult children. Transition probabilities indicate the share of adult children who began in one quantile and ended up in a given quantile themselves. The following analyses use fourths, or "quartiles," to ensure sufficiently precise estimates given the sample sizes involved.

Figure 2 displays four sets of bars, each of them depicting outcomes for men who grew up with fathers in a different part of the male earnings distribution. The left-most bars show results for the 25 percent of sons with the lowest-earning fathers, the next set does the same for the sons of the next-poorest quarter of fathers, the next bars

Figure 2 | TRANSITION PROBABILITIES BY BIRTH COHORT, FATHER EARNINGS VS. SON EARNINGS



Notes: Estimates are from the Panel Study of Income Dynamics. The analyses compare two sets of birth cohorts, one born 1952–59, the other 1976–83. Sons' outcomes are averaged between the ages of 30 and 35 when observed 1982–94 or 2006–18. Each of the four sets of bars refers to adults whose father earnings during their adolescence was in a given quartile of the male earnings distribution. Each of the two bars in each set refers to a cohort of adults. Each segment within each bar refers to the share of adults ending up in a given quartile of the earnings distribution. For full methodological details, see Appendix 1. For results for 1960–67 and 1968–75 cohorts and standard errors, see Appendix 2.

apply to sons with fathers in the third quartile of earnings, and the right-most bars depict the 25 percent of sons raised by the highest-earning fathers. Within each set are two bars, one for the earliest and most recent set of PSID birth cohorts.

Each bar, in turn, is divided into four portions, showing where in the distribution of male earnings sons from a given cohort and quartile of the father earning distribution end up. In a world where the ranking of fathers on the earnings ladder had no relationship to the ranking of sons, each of the bars in Figure 2 would be divided into four equal sections. For instance, 25 percent of men starting in the bottom fourth would end up in the bottom fourth, 25 percent would end up in the second fourth, 25 percent in the third fourth, and 25 percent at the top. The analyses here are primarily concerned with the trend in mobility, so the important comparisons are within each set of bars in Figure 2.

For instance, the left-most set of bars indicates that 40 percent of sons born 1952–59 and raised in the bottom fourth of father earnings were in the bottom fourth of male earnings themselves when observed as adults. In the subsequent set of birth cohorts, upward mobility out of the bottom fourth became less common over time: 49

percent remained in the bottom fourth. Men who started out in the bottom fourth were also slightly less likely to rise to the top fourth of male earnings over time; that share was 5 percent among sons born 1952–59 and 4 percent among sons born 1976–83.

Due to imprecision in these estimates (which come from samples intended to represent the broader population of men), we cannot conclude that every cohort difference in Figure 2 would be found if we could observe the mobility of the entire population of men in both cohorts. Given some simplifying assumptions, statistical techniques allow us to say that if there were no true difference in mobility between two cohorts of men, we would be very unlikely to find a difference of a given magnitude in the PSID data.

These techniques suggest that only one of the trends in Figure 2 is likely to be "real" in this "statistically significant" sense.[13] Sons raised in the top fourth of father earnings became much less likely to end up in the top fourth themselves (right-most set of bars).[14] Despite this evidence of increasing downward mobility, the IRA in Figure 1 does not reflect an increase, because it appears that upward mobility from the bottom fourth fell (though none of the changes in the left-most set of bars are statistically significant).

No previous research has examined long-term trends in relative mobility in the US looking at transition probabilities that compare fathers' and sons' earnings.[15]

Parental Family Income vs. Son Earnings

INCOME RANK ASSOCIATION

The blue lines in Figure 1 provide IRA estimates comparing parental family income to sons' earnings. The solid dark blue line again displays the linear PSID trend, showing a rise in the IRA (or a fall in mobility). The light blue line that it intersects shows the more detailed estimates. For the 1952–59 cohorts, the IRA was 0.42, but it increased to 0.46 for the 1976–83 ones. This increase, however, fails to achieve statistical significance.[16] When parent income is adjusted for family size, the IRA is 0.43 for both cohorts. Note that the linear trend implies a somewhat larger increase in the IRA over time than the comparison of the first and last data points.

The dashed dark blue line displays the same IRA trend—comparing parental family income to sons' earnings—using the NLS data. There are data points for three sets of birth cohorts, displayed at 1950, 1962, and 1983 and representing men born 1949–51, 1961–63, and 1982–84. I will refer to the NLSYM, NLSY79, and NLSY97 in discussing these three sets of cohorts.

The dashed dark blue line confirms that the increase in the IRA (decline in mobility) found in the PSID is also found in the NLS. The IRA rises between the NLSYM and NLSY79 (from 0.24 to 0.34) and then falls by a smaller amount (to 0.30) in the NLSY97.[17] As in the PSID, however, the overall rise in the IRA across the three sets of cohorts was not large enough, given the sample sizes involved, to be statistically significant.[18] The richest and poorest adolescent sons born around 1950 were still separated by 24 percentiles in their early thirties in 1981. The richest and poorest sons born in the early 1980s were separated by 30 percentiles as adults in 2014.[19]

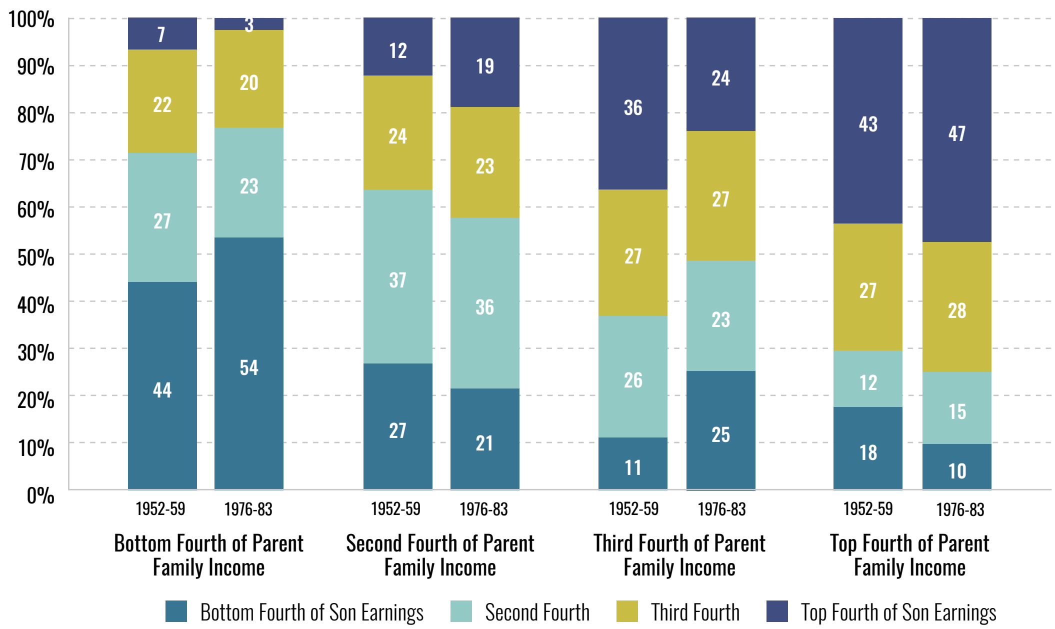
As with the PSID estimates, the NLS levels are lower than the ideal data would indicate. They compare a single year of parental income to a single year of sons' earnings, which means these income measures are noisy estimates of "permanent" or lifetime income. Sons' earnings are measured in their early thirties, which is likely to reflect lifetime earnings worse than would earnings measured near age forty. The IRA levels are lower in the NLS than in the PSID, because the PSID estimates average multiple years of parental income and of sons' earnings, which comes closer to achieving estimates of "permanent" or lifetime income. However, unless the impact of these issues on the mobility estimates changed over the three NLS panels, the trend in mobility would be unaffected.

The few previous IRA trend studies comparing parental income to male earnings in the US have found only modest changes. Using the Panel Study of Income Dynamics (PSID), Justman, Krush, and Millo (2017) found very little change in the IRA (an increase of 0.02) when they compared cohorts born 1952–61 (30 years old between 1982 and 1991) through cohorts born 1970–79 (30 years old between 2000 and 2009). However, they exclude nonwhites from the analysis. Richey and Rosburg (2017) analyze the NLSY79 and NLSY97 and find that sthe IRA fell slightly (not enough to safely rule out a chance result), from 0.265 to 0.277.[20] Bloome, Dyer, and Zhou (2018) report a decline (not statistically significant) of 0.02 comparing men in the NLSY79 and NLSY97 (in their Appendix Table A6).[21]

TRANSITION PROBABILITIES

Figure 3 again displays transition probabilities using the PSID data. There are four changes over time that achieve statistical significance. First, sons raised in the bottom fourth of parental income were less likely to make it to the top fourth as adults (left-most set of bars). This appears primarily to reflect more of them remaining in the bottom fourth, but that change is not statistically significant. Second, sons who started in the top fourth became less likely to end up in the bottom fourth (right-most set of bars). That, too, appears to reflect their being more likely to remain where they started, but like the increased "stickiness" at the bottom, the increased immobility at the top is not statistically significant. Finally two changes among sons raised in the third quartile of parental family income were significant (third set of bars). Such sons became more likely to fall all the way to the bottom fourth as adults, and they became less likely to rise to the top fourth.[22]

Figure 3 | TRANSITION PROBABILITIES BY BIRTH COHORT, PARENTAL FAMILY INCOME VS. SON EARNINGS, PSID

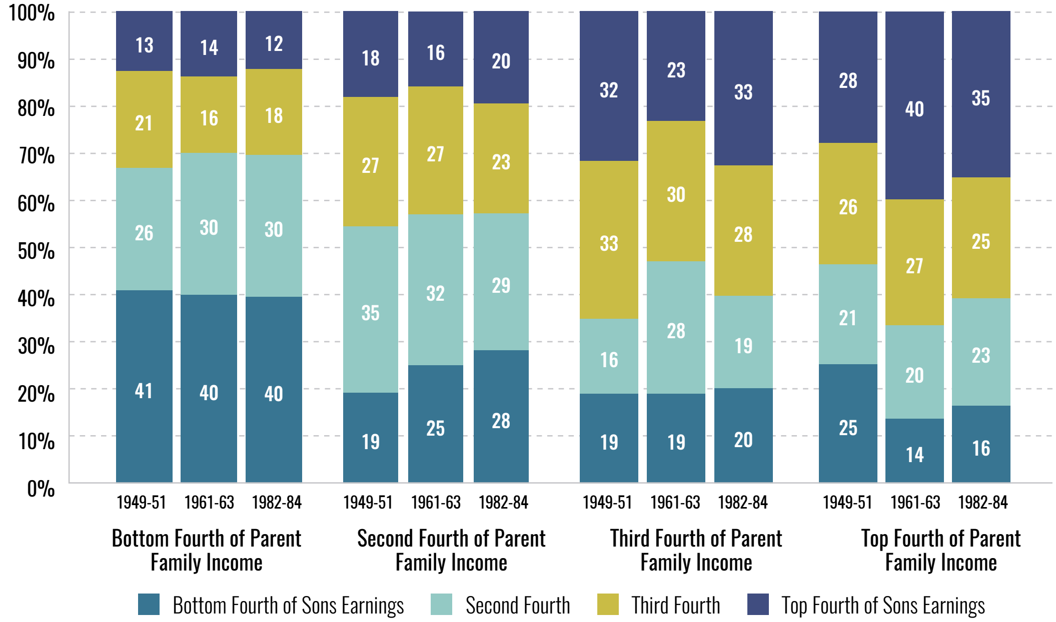


Notes: Estimates are from the Panel Study of Income Dynamics. The analyses compare two sets of birth cohorts, one born 1952–59, the other 1976–83. Sons' outcomes are averaged between the ages of 30 and 35 when observed 1982–94 or 2006–18. Each of the four sets of bars refers to adults whose family income during their adolescence was in a given quartile of the income distribution. Each of the two bars in each set refers to a cohort of adults. Each segment within each bar refers to the share of adults ending up in a given quartile of the earnings distribution. For full methodological details, see Appendix 1. For results for 1960–67 and 1968–75 cohorts and standard errors, see Appendix 2.

Figure 4 presents the change in relative mobility in the NLS data. There are four sets of three bars, with each bar representing an NLS cohort of sons, born 1949–1951, 1961–63, or 1982–84. Only two changes between the NLSYM and NLSY97 are statistically significant.

First, there was a steady increase in the likelihood that men raised in the second fourth of family income fell to the bottom fourth of earnings (second set of bars). Among men in the NLSYM, only 19 percent experienced downward mobility into the bottom fourth, but 28 percent did in the NLSY97. That appears to reflect fewer of them staying in the second fourth or rising to the third fourth (but neither of those trends is sufficiently large to make confident conclusions).

Figure 4 | TRANSITION PROBABILITIES BY BIRTH COHORT, PARENTAL FAMILY INCOME VS. SON EARNINGS, NLS



Notes: Estimates are from the National Longitudinal Surveys. The analyses use three sets of birth cohorts, born 1949–51, 1961–63, or 1982–84. Outcomes are measured between the ages of 30 and 32 when observed in 1981, 1993, or 2014. Each of the four sets of bars refers to adults whose family income during their adolescence was in a given quartile of the income distribution. Each of the three bars in each set refers to a cohort of adults. Each segment within each bar refers to the share of adults ending up in a given quartile of the earnings distribution. For full methodological details, see Appendix 1. For standard errors, see Appendix 2.

Second, the share of sons raised in the top quarter of father earnings who fell all the way to the bottom fourth declined over time (right-most set of bars). In the NLSYM, 25 percent of sons starting in the top fourth fell to the bottom fourth (the same share as would have ended up there if father earnings bore no relationship to son earnings). In the NLSY97, just 16 percent of them fell that far. This decline in downward mobility occurred between the NLSYM and NLSY79.

Between the first two cohorts, the increase in downward mobility from the third quartile of family income to the second quartile of son earnings is statistically significant, reinforcing the PSID finding of rising downward mobility from the third quartile. It appears that the increase in the NLS IRA shown in Figure 1 is mainly the result of falling downward mobility out of the top, primarily at the expense of men raised in the middle half of parental income.

The changes in mobility between the two most recent cohorts are generally very small. Only two seem statistically significant—both of them for sons raised in the third quartile (third set of bars). Such sons were more likely over time to have seen upward mobility into the top fourth of earnings, and they were less likely to fall downward to the second quartile. Otherwise, Figure 4 indicates remarkable stability in mobility over the nearly 20 years between the NLSY79 and NLSY97 cohorts.

No research has examined long-term US trends in relative mobility looking at transition probabilities that compare parental family income to sons' earnings. The estimates in Figures 3 and 4 are consistent with point-in-time estimates from previous studies that use father and son earnings quartiles or parent and son family income quartiles, reported in Part One of this primer.

Parental Family Income vs. Son Family Income

INCOME RANK ASSOCIATION

Returning to Figure 1, the green lines display IRAs comparing parents' and sons' family incomes. The trend results are very similar to the estimates comparing parental income to sons' earnings, whether looking at the PSID or NLS.

In the PSID, the mobility *levels* differ depending on whether parent income is compared to sons' earnings or their family income, but the trends are nearly parallel. As shown by the solid dark green line, the family income IRA increases over time. Between the 1952–59 and 1976–83 cohorts, the IRA increases by 0.06, from 0.49 to 0.55, indicating that mobility fell. Once again, however, the change is not statistically significant.[23] Note that the linear trend increases somewhat more than the change between these two cohorts.

The NLS trend (dashed dark green line) is essentially the same whether sons' earnings or their family income are compared with parent income. The IRA increases from 0.23 to 0.33 and then falls to 0.31. This time, the increase between the first and last cohorts is marginally statistically significant, as is the initial increase between the NLSYM and NLSY79. The drop between the NLSY79 and NLSY97 is not.[24] These figures indicate that the richest and poorest sons born around 1950 were separated by 23 percentiles in 1981, while the gap for sons born in the early 1980s was 31 percentiles. The NLS and PSID results again appear consistent with each other over the long run (but not in the particulars along the way).

Five other studies have examined family income IRA trends for sons. Davis and Mazumder (2020) find a larger increase between the NLSYM and NLSY79 than I report—with the IRA increasing by 0.18 compared to my 0.10. They report a lower IRA than I do for the NLSYM but a higher IRA than I do for the NLSY79. Among the potentially important differences in methods: compared with my analyses, Davis and Mazumder include some younger men in both cohorts and some older men in the NLSYM, they average multiple years of income for parents and sons in both cohorts, and as a consequence of both decisions, they measure sons' incomes at younger ages. Averaging multiple years of income would be expected to increase IRAs, but it should do so in both cohorts, whereas their NLSYM IRA is smaller than mine is. Measuring sons' incomes at younger ages should lower their IRAs, but again in both cohorts.[25] I discuss differences between my results and those of Davis and Mazumder below, in the section on family income absolute mobility summary measures.

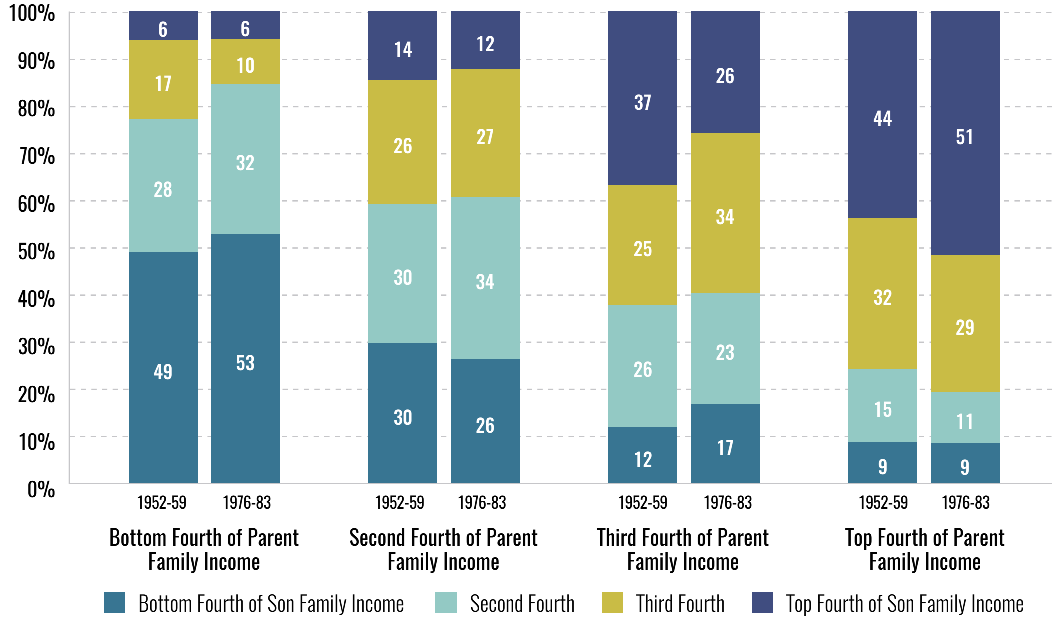
The evidence from other studies and data sources tends to indicate modest to negligible change in the IRA. Using the Panel Study of Income Dynamics (PSID), Justman and Stiassnie (2021) predict family income at age 40 from single-year observations of income and demographics, and then consider mobility in terms of this "lifetime income." They report very little change in the IRA for sons born 1952–61 through cohorts born 1961–70. They then find an increase through the 1963–72 cohorts, followed by a flat trend through 1969–78, and an increase through 1971–80. The IRA increases by 0.07 between the 1952–61 and 1972–81 cohorts, while my NLS estimates rise by 0.08 (comparing the 1949–51 and 1982–84 cohorts) and my PSID increase is 0.06 (between the 1952–59 and 1976–83 cohorts).[26]

Also, consistent with my findings, Bloome, Dyer, and Zhou (2018) find no change between the NLSY79 and NLSY97 cohorts. (See their Appendix Table A4.) Chetty, Hendren, Kline, Saez, and Turner (2014a, 2014b) found no trend for cohorts of sons born between 1971 and 1986 when their mobility was assessed at age 29 or 30 (1971 to 1982 cohorts) or age 26 (1980 to 1986 cohorts).[27]

TRANSITION PROBABILITIES

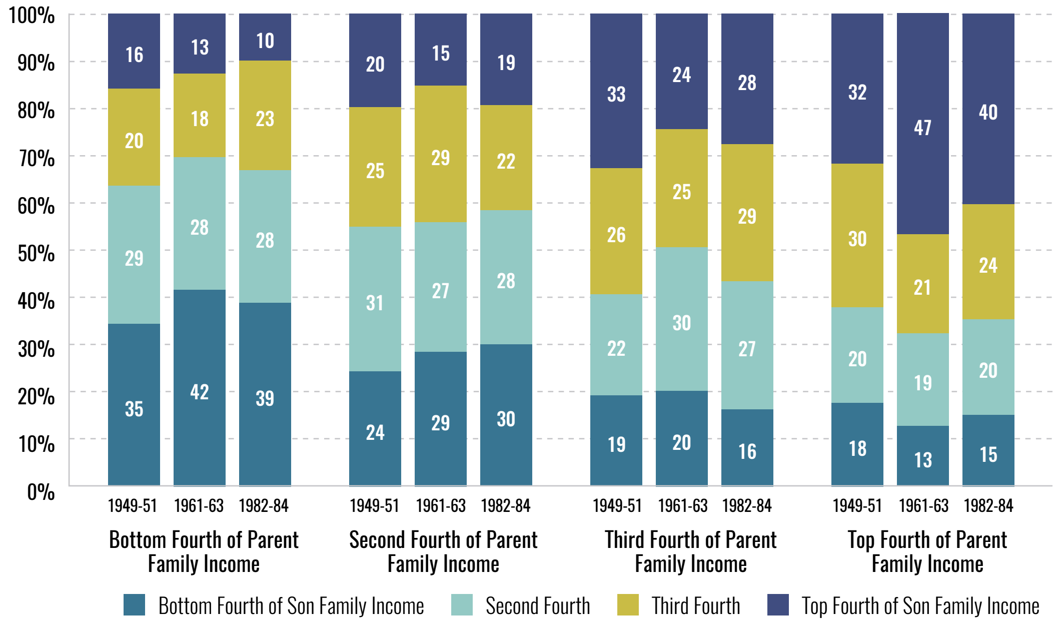
Figure 5 is analogous to Figure 3, showing transition probabilities in the PSID, but this time the outcome is where sons end up in the family income distribution. Two trends are statistically significant. The share of sons raised in the bottom fourth who ended up in the third quartile fell from 17 percent to 10 percent (left-most set of bars). The share of sons raised in the third quartile who ended up in the top quartile fell from 37 percent to 26 percent (third set of bars). Thus, upward mobility fell over time.[28]

Figure 5 | TRANSITION PROBABILITIES BY BIRTH COHORT, PARENTAL FAMILY INCOME VS. SON FAMILY INCOME, PSID



Notes: Estimates are from the Panel Study of Income Dynamics. The analyses compare two sets of birtsh cohorts, one born 1952–59, the other 1976–83. Sons' outcomes are averaged between the ages of 30 and 35 when observed 1982–94 or 2006–18. Each of the four sets of bars refers to adults whose family income during their adolescence was in a given quartile of the income distribution. Each of the two bars in each set refers to a cohort of adults. Each segment within each bar refers to the share of adults ending up in a given quartile of the family income distribution. For full methodological details, see Appendix 1. For results for 1960–67 and 1968–75 cohorts and standard errors, see Appendix 2.

Figure 6 | TRANSITION PROBABILITIES BY BIRTH COHORT, PARENTAL FAMILY INCOME VS. SON FAMILY INCOME, NLS



Notes: Estimates are from the National Longitudinal Surveys. The analyses use three sets of birth cohorts, born 1949–51, 1961–63, or 1982–84. Outcomes are measured between the ages of 30 and 32 when observed in 1981, 1993, or 2014. Each of the four sets of bars refers to adults whose family income during their adolescence was in a given quartile of the income distribution. Each of the three bars in each set refers to a cohort of adults. Each segment within each bar refers to the share of adults ending up in a given quartile of the family income distribution. For full methodological details, see Appendix 1. For standard errors, see Appendix 2.

Transition probabilities in the NLS are shown in **Figure 6**. Over the entire period, there are two trends worth highlighting for their statistical significance. First, over time, the likelihood of rising from the bottom fourth of parental income to the top fourth of family income may have fallen (left-most set of bars). In the NLSYM, 16 percent of sons raised in the bottom fourth made it to the top fourth, but that fell to 13 percent in the NLSY79 and 10 percent in the NLSY97.[29] That is consistent with the fall in upward mobility in the PSID.

Second, downward mobility from the top may have fallen (right-most set of bars). Among sons in the NLSYM raised in the top fourth, only 32 percent remained there in 1981, but 40 percent did in the NLSY97.[30] That change was concentrated between the NLSYM and NLSY79.[31] In the PSID, downward mobility from the top also falls, but the change is not large enough to be statistically significant.

In the NLS, while the decline in mobility when comparing parent family income to sons' earnings seems to have been driven primarily by falling downward mobility from the top, when sons' family income is compared to parent income, both upward and downward mobility appear to have declined.

Almost no changes in Figure 6 between the NLSY79 and NLSY97 were large enough to conclude confidently that they really occurred in the overall population of men. Sons who started in the second-poorest fourth probably were less likely to end up in the second-richest fourth over time (second set of bars). While the likelihood of remaining in the top fourth having started there appears to have fallen between the NLSY79 and NLSY97 (right-most set of bars), the six-point change is imprecisely measured.

No previous study examines trends in family-income transition probabilities for American men. Estimates in Justman and Stiassnie (2021) indicate that the decline in mobility they find using the IRA is due to falling downward mobility from the top (Figure A6, using nonlinear regression).[32] Two studies combine men and women. Bloome, Dyer, and Zhou (2018) report no change between the NLSY79 and NLSY97 in the share of adult children stuck in the bottom fourth or the share staying in the top fourth. Chetty, Hendren, Kline, Saez, and Turner (2014a, 2014b) report results (pooling sons and daughters) showing that the probability of ending in the top fifth of family income, conditional on starting in a given fifth of parental income, was stable for cohorts born between 1971 and 1986. The results from both studies are consistent with the stability Figure 6 shows between the NLSY79 and NLSY97, born in the early 1960s and in the early 1980s.[33]

DAUGHTERS

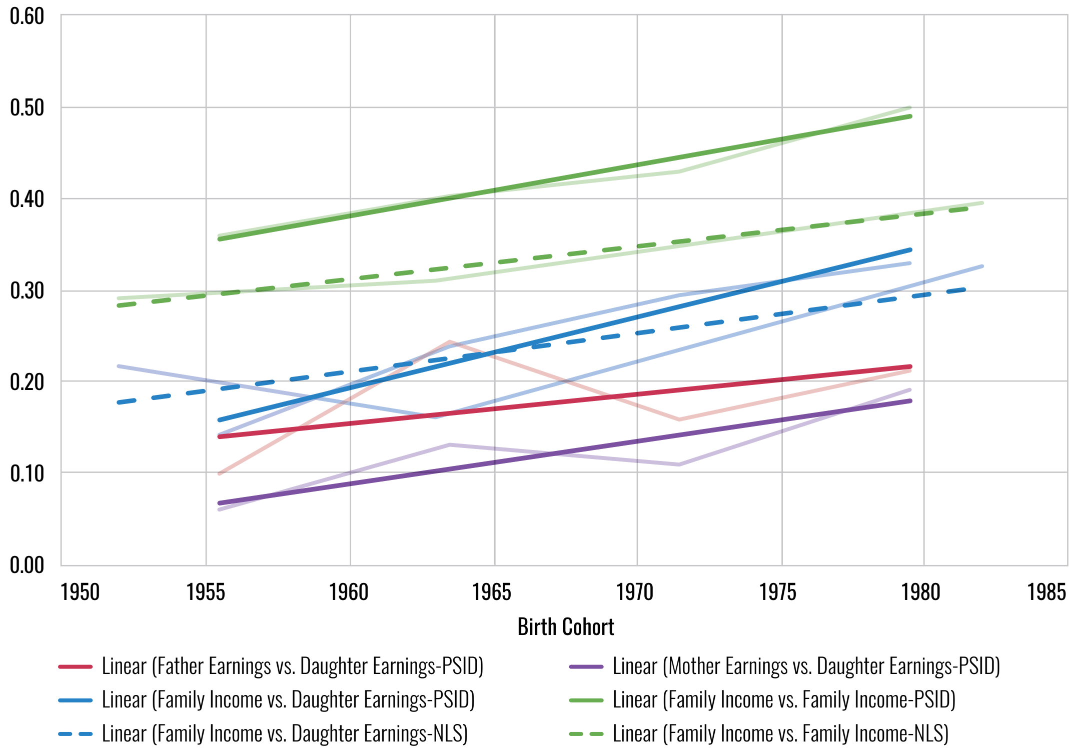
Given the dramatic changes in women's labor force participation, hours in the labor market, and economic opportunities, it is somewhat more difficult to interpret changes in intergenerational mobility for women than it is for men. But given the availability of data from which trends may be estimated, this is a weak justification for the paucity of research on women's mobility to date.

Father Earnings vs. Daughter Earnings

INCOME RANK ASSOCIATION

Figure 7 includes IRA trends for daughters in the PSID and the NLS. Once again, comparisons involving parents' earnings are not possible in the NLS, but the dark red line shows the IRA trend in the PSID comparing daughters' earnings to those of their fathers. The increase in the IRA between the 1952–59 cohorts and 1976–83 cohorts, from 0.10 to 0.21 is not statistically significant, but like the other trends in Figure 7 (and most in Figure 1 for sons), it suggests a decline in mobility. The IRA rises more when fathers and daughters with non-positive earnings are excluded (not shown), increasing from 0.07 to 0.25.[34] The linear trend through the four data points, shown by the dark red line, indicates less change than comparing the estimates for the first and last cohorts.

Figure 7 | DAUGHTERS' INCOME RANK ASSOCIATION (IRA), 1951-83 BIRTH COHORTS



Notes: Estimates are from the Panel Study of Income Dynamics and National Longitudinal Surveys. PSID analyses use four sets of birth cohorts, born 1952–59, 1960–67, 1968–75, and 1976–83. Daughters' outcomes are averaged between the ages of 30 and 35 when observed (up to three times) 1982–94, 1990–2002, 1998–2010, or 2006–18. Parental income is averaged over up to five consecutive years, either 1968–1972, 1976–80, 1984–88, or 1992–96. NLS analyses use three sets of birth cohorts, born 1951–53, 1962–64, or 1981–83. Outcomes are measured between the ages of 31 and 33 when observed in 1984, 1995, or 2014. Parental income is measured in 1967, 1978, or 1996. For full methodological details, see Appendix 1. For point estimates and standard errors, see Appendix 2.

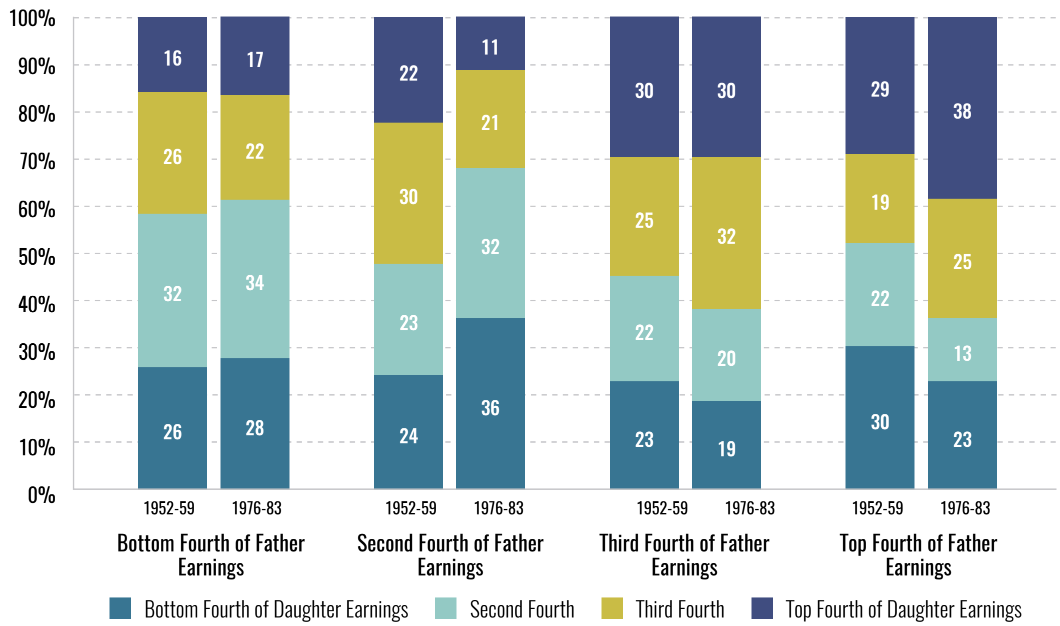
No previous research examines American IRA trends comparing father earnings to daughter earnings, and scarcely any studies estimate even a point-in-time IRA.[35]

TRANSITION PROBABILITIES

Figure 8 displays transition probabilities, comparing the first and last sets of birth cohorts. Only one change achieves statistical significance. Daughters in the second quarter of father earnings became less likely to rise to the top fourth themselves over time (falling from 22 percent to 11 percent, second set of bars).[36]

The only previous study to consider trends in American father-daughter earnings mobility using transition probabilities examined a very short period of time and found a flat trend in the PSID from the late 1980s to the early 1990s (Fertig, 2003).

Figure 8 | TRANSITION PROBABILITIES BY BIRTH COHORT, FATHER EARNINGS VS. DAUGHTER EARNINGS



Notes: Estimates are from the Panel Study of Income Dynamics. The analyses compare two sets of birth cohorts, one born 1952–59, the other 1976–83. Daughters' outcomes are averaged between the ages of 30 and 35 when observed 1982–94 or 2006–18. Each of the four sets of bars refers to adults whose father earnings during their adolescence was in a given quartile of the male earnings distribution. Each of the two bars in each set refers to a cohort of adults. Each segment within each bar refers to the share of adults ending up in a given quartile of the earnings distribution. For full methodological details, see Appendix 1. For results for 1960–67 and 1968–75 cohorts and standard errors, see Appendix 2.

Mother Earnings vs. Daughter Earnings

INCOME RANK ASSOCIATION

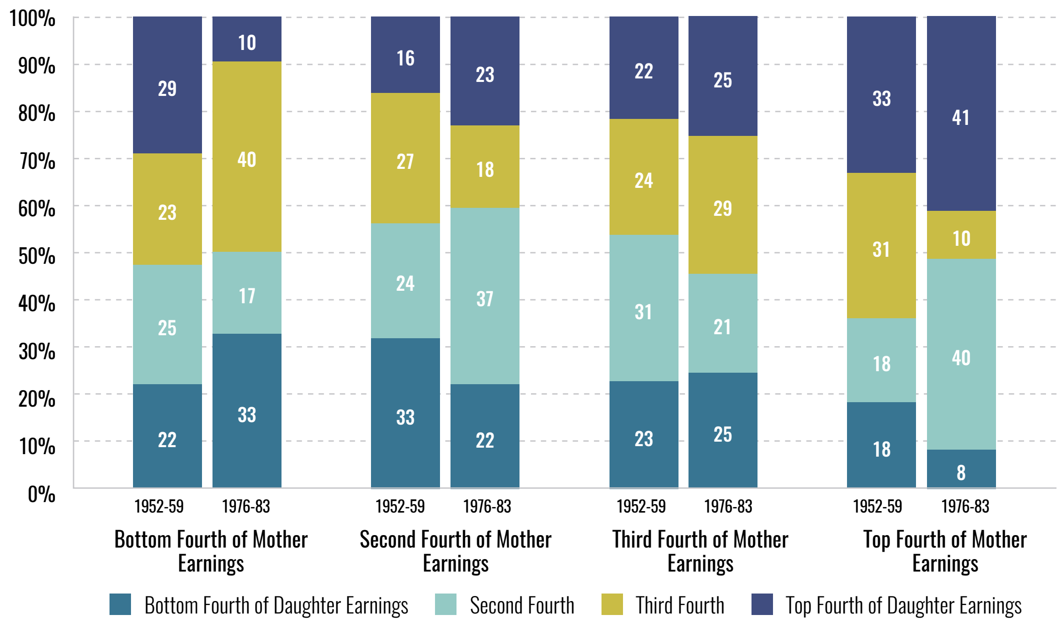
Figure 7, above, includes a PSID trend comparing daughters' earnings to their mothers' (purple line). This IRA increases from 0.06 to 0.19, a rise that is marginally statistically significant. When women with non-positive earnings are excluded, the increase is smaller—from 0.16 to 0.21—and not statistically significant.[37] These results hint that female earnings mobility may have declined in part because there used to be barely any relationship between mother and daughter earnings when so many mothers in the 1950s spent time out of the workforce as homemakers.

To my knowledge, Part One of this primer provided the only previous IRA estimates comparing American mothers' and daughters' earnings, with a range from 0.31 to 0.42. This range more accurately conveys the true IRA level, but estimating a trend requires using less-ideal measures.

TRANSITION PROBABILITIES

Figure 9 reveals that the decline in female earnings mobility is primarily due to falling upward mobility among daughters with the lowest-earning mothers. Two of the trends in the left-most set of bars are statistically significant. The share of daughters starting in the bottom fourth who made it to the top fourth fell, while the share that made it to the third fourth rose.[38] Mobility out of the bottom was high for the 1952–59 birth cohorts, probably because more daughters worked (or worked more) compared with their mothers. This was less true of the 1976–83 cohorts, though the increase in the share raised in the bottom who remained there as adults was not statistically significant. (Not shown in Figure 9, if women with non-positive earnings are excluded, the share of daughters raised in the bottom fourth who remained in the bottom fourth *falls* from 36 percent to 26 percent, a change that also is not statistically significant.) In addition, the share of daughters starting out in the top fourth who fell to the second fourth rose, while the share falling to the bottom fourth or the third fourth fell (right-most set of bars).[39]

Figure 9 | TRANSITION PROBABILITIES BY BIRTH COHORT, MOTHER EARNINGS VS. DAUGHTER EARNINGS



Notes: Estimates are from the Panel Study of Income Dynamics. The analyses compare two sets of birth cohorts, one born 1952–59, the other 1976–83. Daughters' outcomes are averaged between the ages of 30 and 35 when observed 1982–94 or 2006–18. Each of the four sets of bars refers to adults whose mother earnings during their adolescence was in a given quartile of the female earnings distribution. Each of the two bars in each set refers to a cohort of adults. Each segment within each bar refers to the share of adults ending up in a given quartile of the earnings distribution. For full methodological details, see Appendix 1. For results for 1960–67 and 1968–75 cohorts and standard errors, see Appendix 2.

No previous study, to my knowledge, has estimated trends in transition probabilities comparing American mothers' and daughters' earnings, and Part One of this primer is the only one to have estimated a point-in-time transition matrix.

Parental Family Income vs. Daughter Earnings

INCOME RANK ASSOCIATION

Returning to Figure 7, the solid dark blue line shows the IRA trend in the PSID when daughter earnings are compared to parental family income. The IRA more than doubles over time, from 0.14 to 0.33.[40] This increase is statistically significant and nearly the same when parents and daughters with non-positive income are excluded or when parental income is adjusted for family size. By this measure, mobility fell substantially. The linear trend through the four PSID points suggests the same conclusion.

The dashed dark blue line indicates the NLS trend.[41] (I will refer to the earliest set of cohorts using "NLSYW.") The estimates for the NLSYW and NLSY97 are similar to those for sons in the NLSYM and NLSY97, but in between, the IRA falls and then rises, while for sons it rises and then falls. The overall rise from the NLSYW to the NLSY97, from 0.22 to 0.33, is again statistically significant, as is the doubling of the IRA between the NLSY79 and NLSY97. The initial fall in the IRA for daughters, however, from 0.22 to 0.16, is not.[42] The trend is similar if only positive parental incomes and daughter earnings are included, though the overall increase over 30 years is smaller and not statistically significant.[43] The linear trend through the three data points in Figure 7 is of a similar magnitude to the NLSYW-to-NLSY97 change.

The increase in the NLS IRA over the entire three sets of cohorts appears somewhat smaller than the increase in the PSID IRA, but both rise over the period.

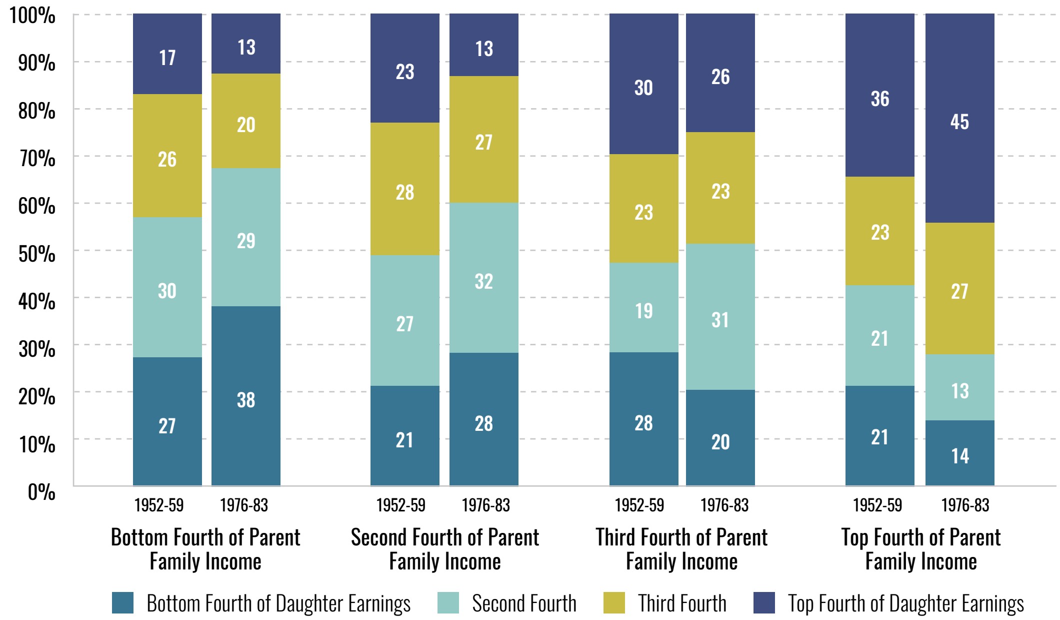
Only two previous studies examine IRA trends in the US comparing daughters' earnings to parental income. Richey and Rosburg (2017) find a more modest decline in mobility than I do—an increase in the IRA from 0.23 in the NLSY79 to 0.27 in the NLSY97—and one that is not statistically significant. These estimates, however, are based on statistical models that control for several demographic variables, which may account for the differences from my results. Bloome, Dyer, and Zhou (2018) also find a small and not statistically significant increase in the IRA using the NLSY79 and NLSY97—0.045 versus my 0.16. Both of these studies average multiple years of income but also measure daughter earnings at younger ages than I do and exclude self-employment earnings.

Chetty, Hendren, Kline, Saez, and Turner (2014a, 2014b) note in passing that when they compare the family income of parents to the individual income of grown children, they find no change in the IRA. However, it appears that this finding pools sons and daughters.[44]

TRANSITION PROBABILITIES

PSID transition probabilities are shown in **Figure 10**. The decline in mobility found in Figure 7 seems to be driven both by reduced upward mobility from the bottom and downward mobility from the top. The 11-point increase in the share of daughters starting in the bottom fourth who remained there as adults was marginally statistically significant. Otherwise, the share moving from the second quartile to the top quartile fell, and the share falling from the third to the second quartile fell.[45]

Figure 10 | TRANSITION PROBABILITIES BY BIRTH COHORT, PARENTAL FAMILY INCOME VS. DAUGHTER EARNINGS, PSID



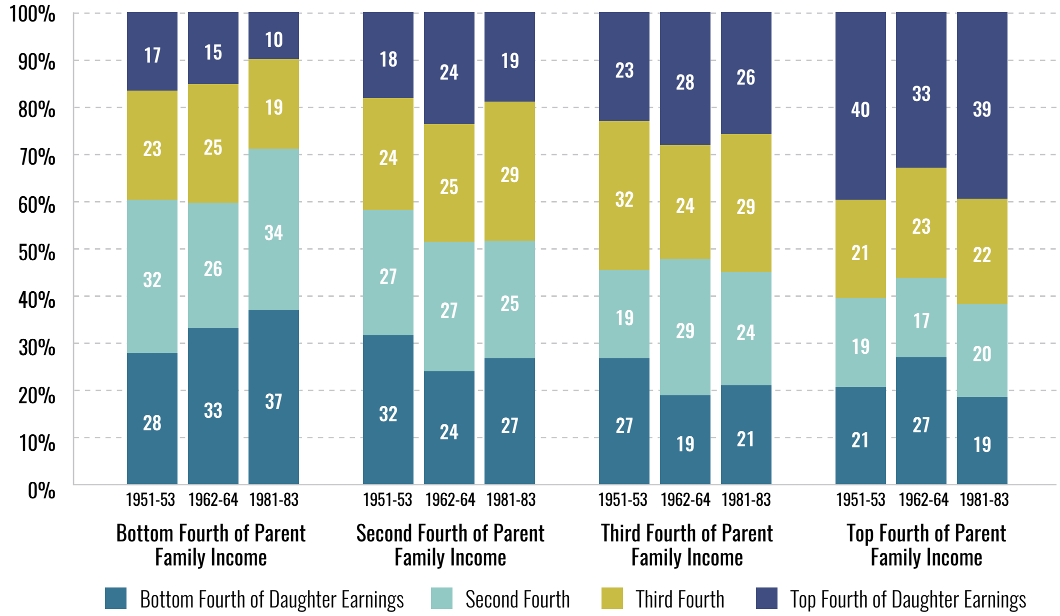
Notes: Estimates are from the Panel Study of Income Dynamics. The analyses compare two sets of birth cohorts, one born 1952–59, the other 1976–83. Daughters' outcomes are averaged between the ages of 30 and 35 when observed 1982–94 or 2006–18. Each of the four sets of bars refers to adults whose family income during their adolescence was in a given quartile of the income distribution. Each of the two bars in each set refers to a cohort of adults. Each segment within each bar refers to the share of adults ending up in a given quartile of the earnings distribution. For full methodological details, see Appendix 1. For results for 1960–67 and 1968–75 cohorts and standard errors, see Appendix 2.

Figure 11 presents the transition probabilities in the NLS. Here the decline in mobility appears to be due to reduced upward mobility. The share of women who started in the bottom fourth and remained there as adults rose substantially over time, from 28 percent in the NLSYW to 33 percent in the NLSY79 and to 37 percent in the NLSY97 (left-most set of bars).[46] Between the NLSY79 and NLSY97, daughters starting in the bottom fourth became more likely to end up in the second fourth (rather than the top half). In addition, in the NLSY97, daughters who were raised in the top quarter became less likely to fall to the bottom fourth (right-most set of bars). Between the NLSYW and

NLSY79, the only change that achieves statistical significance is that daughters raised in the third quartile became more likely to fall to the second quartile (third set of bars).[47]

No research has previously examined trends in transition probabilities that compare daughters to either parental earnings or family income.

Figure 11 | TRANSITION PROBABILITIES BY BIRTH COHORT, PARENTAL FAMILY INCOME VS. DAUGHTER EARNINGS, NLS



Notes: Estimates are from the National Longitudinal Surveys. The analyses use three sets of birth cohorts, born 1951–53, 1962–64, or 1981–83. Outcomes are measured between the ages of 31 and 33 when observed in 1984, 1995, or 2014. Each of the four sets of bars refers to adults whose family income during their adolescence was in a given quartile of the income distribution. Each of the three bars in each set refers to a cohort of adults. Each segment within each bar refers to the share of adults ending up in a given quartile of the earnings distribution. For full methodological details, see Appendix 1. For results for 1960–67 and 1968–75 cohorts and standard errors, see Appendix 2.

Parental Family Income vs. Daughter Family Income

INCOME RANK ASSOCIATION

We can return to Figure 7 a final time to consider the trend in the IRA for daughters when their own family income is compared against that of their parents. These trends are indicated by the dark green lines. Just as the IRA trend for sons was similar whether parental income was compared to their earnings or family income, the IRA trends for daughters are similar for both. In contrast to the NLS estimates for sons, the IRA *levels* are substantially higher for daughters when family income is the outcome than when earnings are the outcome.

In the PSID (solid dark green line), the IRA rises substantially. The increase between the earliest and latest cohorts, from 0.36 to 0.50, is statistically significant.[48] It is very similar whether parents and daughters with non-positive income are excluded or whether incomes are adjusted for family size. The linear trend through the four data points is also very similar to the increase between the first and last set of cohorts.

In the NLS (dashed dark green line), the overall increase in the IRA from the NLSYW to the NLSY97 is statistically significant. (The NLSYW-to-NLSY79 change in mobility, from 0.29 to 0.31, is not statistically significant, but the subsequent increase to 0.39 is.)[49] While family income mobility appears similar for sons and daughters in the NLSY79, it is lower for daughters by the NLSY97. This remains true when excluding daughters and parents with non-positive family income, though none of the year-to-year changes are statistically significant nor large (not shown).[50]

Once again, the increase in the NLS IRA appears smaller than the increase in the PSID IRA, but both rise over the period.

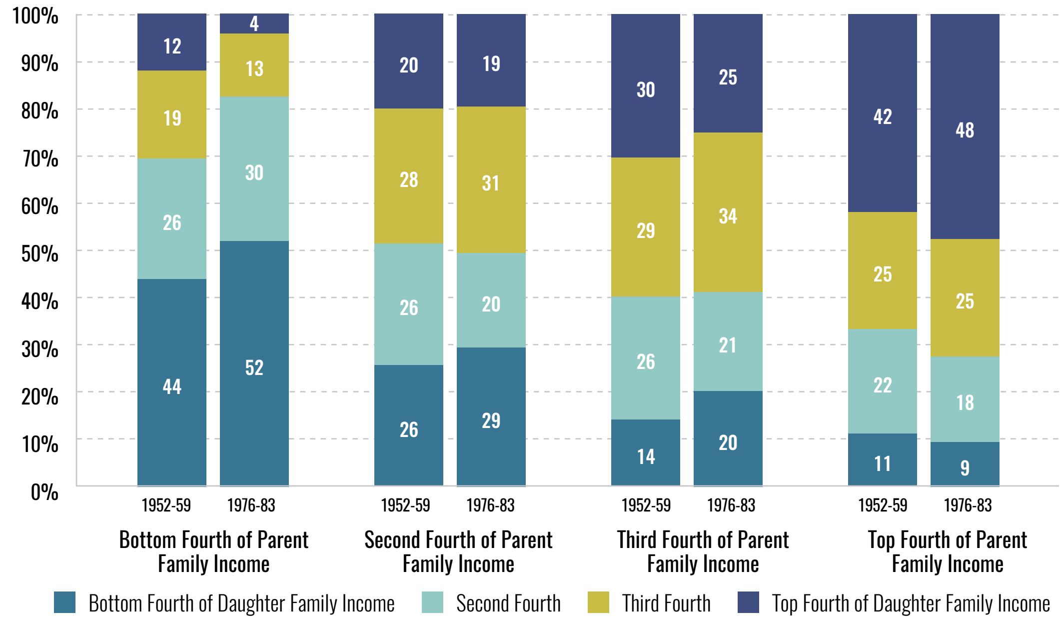
Only three previous studies examine trends in the family income IRAs of American women. Davis and Mazumder (2020) find an increase in the IRA between the first two NLS cohorts, which is inconsistent with my finding no change using the same surveys. As was the case above for sons, they find a lower IRA for the NLSYW cohort than I do and a higher IRA for the NLSY79, producing an increase of 0.08 compared with my decline of 0.01. See the discussion above for differences between the analyses.[51]

Bloome, Dyer, and Zhou (2018) find no change between the NLSY79 and NLSY97 cohorts (increase of 0.015), versus my 0.08 increase. Chetty, Hendren, Kline, Saez, and Turner (2014a, 2014b) found no trend for cohorts of women born between 1971 and 1986, when they were 26 years old.[52]

TRANSITION PROBABILITIES

To complete the relative mobility analyses, Figures 12 and 13 compare parent family income to daughters' family income using transition probabilities. In the PSID (**Figure 12**), there is a single statistically significant change—daughters starting out in the bottom fourth became less likely to make it to the top fourth (left-most set of bars).[53]

Figure 12 | TRANSITION PROBABILITIES BY BIRTH COHORT, PARENTAL FAMILY INCOME VS. DAUGHTER FAMILY INCOME, PSID

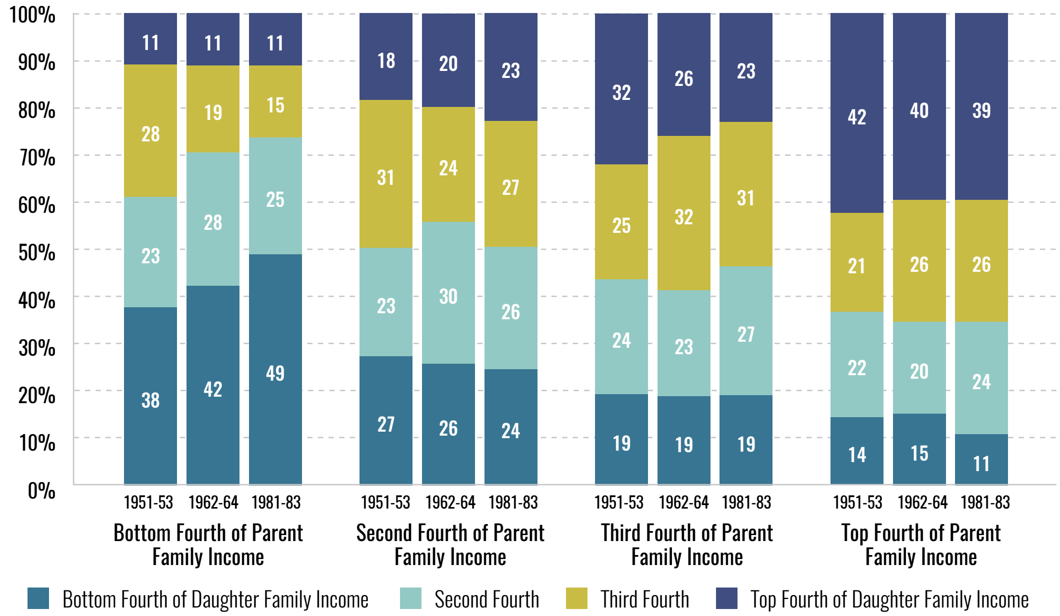


Notes: Estimates are from the Panel Study of Income Dynamics. The analyses compare two sets of birth cohorts, one born 1952–59, the other 1976–83. Daughters' outcomes are averaged between the ages of 30 and 35 when observed 1982–84 or 2006–18. Each of the four sets of bars refers to adults whose family income during their adolescence was in a given quartile of the income distribution. Each of the two bars in each set refers to a cohort of adults. Each segment within each bar refers to the share of adults ending up in a given quartile of the family income distribution. For full methodological details, see Appendix 1. For results for 1960–67 and 1968–75 cohorts and standard errors, see Appendix 2.

There are three trends of note in the NLS (**Figure 13**). First, immobility within the bottom fourth has increased substantially over 30 years. In the NLSYW, 38 percent of daughters with parents in the bottom quarter were still there themselves, compared with 49 percent by the NLSY97 (left-most set of bars). This increase is reflected in the decline in the share who start in the bottom but rise to the third quarter (the second meaningful trend, also in the left-most set of bars). Third, the share of women who start in the third quarter and rise to the top has declined (third set of bars). As when daughters' earnings are the outcome, the increase in the IRA shown in Figure 7 seems primarily to reflect falling upward mobility from the bottom.

Comparing the NLSY79 and NLSY97, the only change that is likely to be statistically meaningful is the increase in the share of women stuck in the bottom fourth, which was 42 percent in the NLSY79 and 49 percent in the NLSY97.[54] When non-positive family incomes are excluded, there is no increase between the two cohorts, however (not shown).

Figure 13 | TRANSITION PROBABILITIES BY BIRTH COHORT, PARENTAL FAMILY INCOME VS. DAUGHTER FAMILY INCOME, NLS



Notes: Estimates are from the National Longitudinal Surveys. The analyses use three sets of birth cohorts, born 1951–53, 1962–64, or 1981–83. Outcomes are measured between the ages of 31 and 33 when observed in 1984, 1995, or 2014. Each of the four sets of bars refers to adults whose family income during their adolescence was in a given quartile of the income distribution. Each of the three bars in each set refers to a cohort of adults. Each segment within each bar refers to the share of adults ending up in a given quartile of the family income distribution. For full methodological details, see Appendix 1. For results standard errors, see Appendix 2.

As noted earlier, two studies look at transition matrices comparing the family incomes of American parents and adult children (pooling sons and daughters). Bloome, Dyer, and Zhou (2018) report no change between the NLSY79 and NLSY97 in the share of adult children stuck in the bottom fourth or the share staying in the top fourth. Chetty, Hendren, Kline, Saez, and Turner (2014a, 2014b) report the probability of ending in the top fifth of family income, conditional on starting in a given fifth of parental income. They find no change for cohorts born between 1971 and 1986.[55]

SUMMARY OF RELATIVE MOBILITY TRENDS

Comparing fathers' and sons' earnings, mobility may have increased slightly between cohorts born in the 1950s and cohorts born in the late 1970s and early 1980s, but any change was likely small. Any increase seems mostly to reflect an increase in downward mobility from the top.

Comparing parental family income to sons' outcomes, relative mobility may have fallen over the long run, but again, any decline was modest. According to the NLS data, rather than the richest and poorest adolescent sons being separated by just under 25 percentiles in adulthood, as in 1981 (for cohorts born around 1950), they were separated by 30 percentiles in adulthood in 2014 (for cohorts born in the early 1980s). In the PSID, the richest and poorest adolescent sons in terms of family income were separated by 49 percentiles in adulthood in the 1980s and early 1990s (for cohorts born in the 1950s) but by 55 percentiles in the late 2000s and 2010s (for cohorts born in the late 1970s and early 1980s). There is evidence that both upward mobility from the bottom and downward mobility from the top declined. When sons' earnings are the outcome, both the PSID and NLS indicate reduced downward mobility, and both indicate reduced upward mobility when considering sons' family income.

Mobility may have fallen more among daughters, but the evidence is mixed. Earnings mobility fell by a large amount when mothers and daughters are compared and non-positive earnings are included, but otherwise the decline was modest.

Using parental income as a benchmark, mobility among daughters also fell. The 30-year decline in the NLS is roughly the same as for men despite different timing. In the PSID, the long-term declines are sizable, with the IRA increasing by 0.14 to 0.19. Lower mobility for daughters primarily reflects diminished upward mobility from the bottom.

04 | Absolute Economic Mobility—Intergenerational Elasticity

This section assesses mobility trends as indicated by the most widely used summary measure of how absolute mobility affects inequality, the intergenerational elasticity (IGE). The next section assesses the share of adults who exceed their parents' income—a distributional measure in the sense that it is possible to estimate this share for children who start out in different parts of the distribution of parental income.

The IRA summarizes the extent to which two people whose parents rank far apart from each other also rank far apart themselves. That is, it summarizes how intergenerational changes in income rankings reduce childhood inequality in income rankings by adulthood.

An analogous measure focused on absolute mobility should summarize the extent to which two people whose parents are far apart in terms of absolute income levels are also far apart themselves. It should indicate how intergenerational changes in income levels reduce childhood inequality in income levels. However, it generally will be unlikely that a given initial dollar difference between two children will translate into the same dollar difference in adulthood, regardless of where children start out. A $50,000 difference in childhood is likely to translate into a larger income difference in adulthood when the children have $10,000 and $60,000 than when they have $500,000 and $550,000. In contrast, a 50 percent difference in childhood income may translate into a similar percentage difference in adulthood income regardless of where children start out. The IGE essentially assesses how early percentage differences translate into later percentage differences.

Rather than telling us, for example, that a child with parental income $50,000 less than another child will tend to have income $20,000 less than the richer child when they are adults, the IGE might tell us that a child with parental income 50 percent lower than another child will tend to have income 20 percent lower than the richer child when they are adults. Technically, the IGE is the regression coefficient in a statistical model predicting grown-child income from parent income, after transforming incomes by computing their natural logarithms. An IGE of 0.40 indicates that if one child has father earnings 10 percent greater than another child, by adulthood he will tend to have earnings higher by 3.9 percent.[56]

Economic mobility researchers often have called the IGE a summary measure of relative mobility rather than absolute mobility. But the fact that the IGE expresses intergenerational income movements and intragenerational income differences in percentage terms does not mean it summarizes relative mobility. Percentage differences in childhood income can be higher or lower in adulthood even if all children have the same income rank as their parents. That can happen because the distribution of income can become more or less unequal; the gap between the lowest and highest incomes can increase without any relative mobility, which will cause absolute income gaps to rise. What matters for the IGE is not how positions change but how income growth rates for poor children compare with income growth rates for rich children, and that comparison is affected not just by who moves up or down in rankings, but by changes in how poor "poor" people are and how rich "rich" people are.

As in the previous section, the current one relies on PSID and NLS data, this time to trace out trends in absolute mobility comparing parental income to the outcomes of grown sons and daughters.

SONS

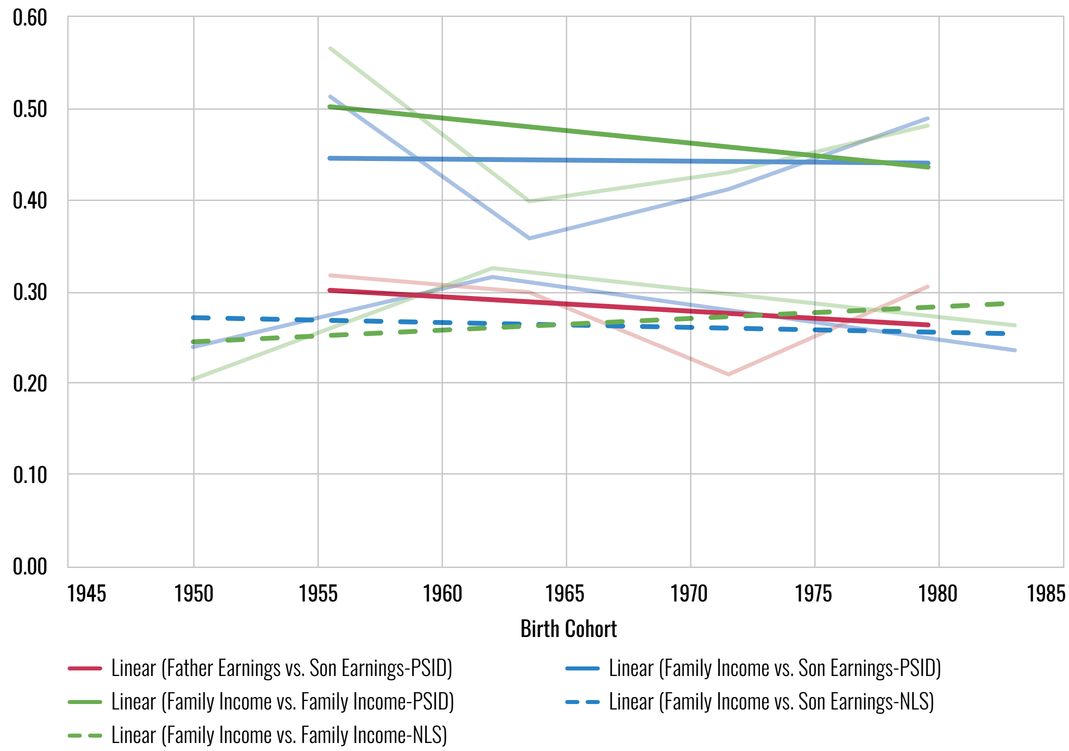
Figure 14 presents IGE trends for sons, when either their individual earnings or their family incomes are compared against father earnings or parental income. As with the IRA estimates, I begin by looking at earnings.

Father Earnings vs. Son Earnings

The dark red line in **Figure 14** shows the IGE trend for male earnings, using the PSID. As in Figure 1, the data points in the light red line are plotted at 1955.5, 1963.5, 1971.5, and 1979.5, representing sons born 1952–59, 1960–67, 1968–1975, and 1976–83, with their outcomes averaged between the ages of 30 and 35. One difference between the IGE and IRA analyses is that parents and sons with no income (or negative income) are omitted this time. This happens because the IGE involves transforming incomes by taking their natural logarithms, and the natural logarithm of non-positive numbers is undefined.

The IGE is the same for the earliest and most recent cohorts (0.32 and 0.31).[57] This change is very similar to the change in the IRA when non-positive earnings are excluded. The linear trend shown by the dark red line suggests a slight decline in the IGE, as was the case for the IRA. None of the inter-cohort changes shown in the light red line are statistically significant.

Figure 14 | SONS' INTERGENERATIONAL ELASTICITY (IGE), 1949-84 BIRTH COHORTS



Notes: Estimates are from the Panel Study of Income Dynamics and National Longitudinal Surveys. PSID analyses use four sets of birth cohorts, born 1952–59, 1960–67, 1968–75, and 1976–83. Sons' outcomes are averaged between the ages of 30 and 35 when observed (up to three times) 1982–94, 1990–2002, 1998–2010, or 2006–18. Parental income is averaged over up to five consecutive years, either 1968–1972, 1976–80, 1984–88, or 1992–96. NLS analyses use three sets of birth cohorts, born 1949–51, 1961–63, or 1982–84. Outcomes are measured between the ages of 30 and 32 when observed in 1981, 1993, or 2014. Parental income is measured in 1966, 1978, or 1996. For full methodological details, see Appendix 1. For point estimates and standard errors, see Appendix 2.

The male earnings IGE levels shown in Figure 14 are lower (indicating more mobility) than the best-measured IGE estimates in the literature indicate.[58] This is probably because I only average five years of father earnings and three years of son earnings, and the latter are measured when sons are in their early thirties. However, unless the bias in the levels changes over time, the trend will be unaffected.

Three papers have considered US trends in the IGE comparing father and son earnings, all of them using the PSID. Justman and Stiassnie (2021) are the only previous researchers to present longer-term trends. They find an increase in the male earnings IGE from the 1952–61 birth cohorts to the 1957–66 cohorts and again from 1969–78 to 1972–81. Reville (1996) found the IGE flat or declining between 1979 and 1988 for men between the ages of 25 and 35 (birth cohorts between 1951 to 1958), while Fertig (2003) reported an implausibly large decline between the late 1980s and the early 1990s (birth cohorts between 1945 and 1972).[59]

Parental Family Income vs. Son Earnings

The blue lines in Figure 14 show IGE trends comparing parental family income to sons' earnings. In the PSID (solid line), the IGE indicates less mobility than when fathers' and sons' earnings were compared. It falls from 0.51 to 0.49 between the earliest and most recent cohorts. As with the decline in the male earnings IGE, this change is also too small to achieve statistical significance.[60] The linear trend through the four data points declines by about the same amount.

The dashed blue line shows NLS estimates. As above, the data points are displayed at 1950, 1962, and 1983, representing men born 1949–51, 1961–63, and 1982–84, whose outcomes were observed in 1981, 1993, and 2014 when they were in their early thirties. The IGE trends look similar to the IRA trends in the NLS from Figure 1. The IGE initially rises from 0.24 to 0.32 (meaning that mobility declines) but then falls back to 0.24. Neither of these changes is large enough to achieve statistical significance, but other ways of performing the analyses show the same basic trend and indicate the rise and fall are statistically significant.[61] The flatness of the overall long-term trend is consistent with the PSID estimates.

While the PSID and NLS long-term trends are similar, as indicated by the linear trend lines in Figure 14, the shorter-term trends differ.

Three earlier PSID studies find a decline in the IGE across cohorts born in the late 1940s or early 1950s and cohorts born in the late 1950s or early 1960s.[62] Blanden (2005), for example, shows a sizable drop from cohorts born 1950–53 to those born 1957–60 (from 0.48 to 0.22), observed 1980–83 versus 1987–90. That is consistent with the PSID evidence in Figure 14. However, Justman, Krush, and Millo (2017) find a large increase in the IGE between a set of PSID cohorts born 1953–62 and a set born 1962–71. (See their Appendix Table B2.) They show little trend thereafter, through the 1970–79 cohorts.

Levine and Mazumder (2002) find an increase in the IGE between the NLSYM and NLSY79 cohorts that is very consistent with my NLS results in Figure 14. They report an increase from 0.235 to 0.33, compared with mine from 0.24 to 0.32.[63]

Finally, Aaronson and Mazumder (2008) estimate a number of trends using pseudo-IGE measures, generally finding steady increases for cohorts born between 1930 and 1965. Davis and Mazumder (2020) extend the results, finding an increase through 1970 cohorts. However, as I discuss in Appendix 4, which includes a replication and reanalysis of the Aaronson-Mazumder methods, these results appear unreliable.[64]

Parental Family Income vs. Son Family Income

Figure 14 also presents IGE trends when parent family income is compared to sons' family income (green lines). The results are similar to those when sons' earnings are the outcome of interest. In the PSID, the apparent decline in the IGE is larger when using sons' family income (from 0.57 to 0.48), but the result falls short of statistical significance.[65] The linear trend indicates a decline of about the same amount. When size-adjusted family incomes are used, the decline is only from 0.55 to 0.52 (not shown).

In the NLS (dashed line), the IGE rises from 0.21 to 0.33 and then falls to 0.26. The change between the NLSYM and NLSY97 is not statistically significant, however.[66]

Turning to the previous research, there have been 10 studies using the PSID to estimate sons' family income IGE trends, which paint a complicated picture. The six studies published before 2010 are more or less consistent. As best as can be discerned, the IGE changed little from the early 1950s birth cohorts to the mid-1950s ones (Blanden, 2005; Mayer and Lopoo, 2005). It then fell substantially between the mid-1950s cohorts and the early 1960s (Blanden, 2005; Mayer and Lopoo, 2005; Hertz, 2007; Lee and Solon, 2009, Table 2). Nam (2004) also found a decline between the mid-1950s and the mid-1960s cohorts. These results are broadly consistent with the PSID trend I estimate.

Also consistent with my results, this earlier research indicates that between the early 1960s and late 1960s cohorts, the IGE rose (Blanden, 2005; Mayer and Lopoo, 2005; Hertz, 2007; Lee and Solon, 2009, Table 2). There was then little change between the mid- to late-1960s and the late 1960s/early 1970s (Lee and Solon, 2009).

The overall picture from the pre-2010 PSID research, then, is one of little long-term change, despite periods where the IGE rose and fell (Harding, Jencks, Lopoo, and Mayer, 2005; Hertz, 2007; Lee and Solon, 2009). More recent PSID research also finds either little change or declines in the IGE. Bloome (2015) found very little change across *any* of the 1954 through 1974 birth cohorts. Palomino, Marrero, and Rodriguez (2018), looking at calendar years, found an initial decline in the IGE followed by a smaller increase. Durlauf, Kourtellos, and Tan (2017) found a decline between a set of cohorts born 1953–62 and a set born 1958–67. After that, different analyses in the paper indicate either a continued decline or an increase that more than reverses the earlier decline.

Contrary to most of these other papers, the most recent PSID study, by Justman and Stiassnie (2021), finds the IGE *rose* between a set of cohorts born 1952–61 and a set born 1972–81. The increases primarily occurred between the mid- to late 1950s cohorts and the late 1950s/early 1960s and between the 1960s cohorts and the late 1960s/early 1970s.[67]

Three studies have looked at sons' family income IGE trends using the NLS data, and their results are qualitatively consistent with mine. Torche (2011) uses the NLSYM and NLSY79, reporting an increase in the IGE of 0.20. Her NLSY79 estimate is substantially larger than mine. Bloome and Western (2011) also use those two datasets and find the IGE rose for both black and white men, though their analyses look at absolute mobility *within* racial groups. Davis and Mazumder (2020) also find an increase between these two cohorts, but again, much larger than the increase I find (0.34 rather than 0.12). None of these studies examines the NLSY97 (which was still young at the time of the Torche and Bloome and Western studies). However, Davis and Mazumder report that within the NLSY79, it appears that the IGE (this time pooling sons and daughters) fell between the 1960 and 1964 birth cohorts (the youngest in the survey).[68]

Finally, Chetty, Hendren, Kline, Saez, and Turner (2014a, 2014b) found no family income IGE trend between the 1971 and 1982 birth cohorts, using tax data from the Internal Revenue Service.

Regarding the discrepancy between the Davis-Mazumder results and my own, I have been able to replicate most of their estimates, and there are several reasons for the difference. The most important is their averaging of up to three years of sons' family income, whereas I use a single year in my NLS analyses. Their averaging of up to three years of parental family income is the next most important difference. For instance, if I make only those changes in estimating the IGE for the NLSY79 sample, my IGE rises from 0.33 to 0.41 (compared with their 0.48). If I make the same choices they make but retain my single-year income measures, the IGE is 0.40.[69] If I make all the choices they made, the IGE is 0.47.

I chose not to average multiple years of income in the NLS analyses for several reasons. I did not want to rely on incomes of sons in their twenties, when incomes are less well correlated with lifetime income. I did not want to introduce selection problems that might arise when more years of parental income are available for sons who live at home for longer durations. And in the data for the first NLS cohort, in particular, the income measures available sometimes seemed of dubious consistency across survey waves.[70] When I re-estimated my NLSYM IGE using multi-year averages of parental family income, as Davis and Mazumder do, the estimate fell rather than (as would be expected) increasing.

I repeated my cross-cohort NLS IGE analyses, but this time used an average of up to three years of sons' family income (without averaging parent family income). I also restricted the sample as Davis and Mazumder did, conditioning on parents' age when sons were born.[71] My results were closer to theirs but showed the same basic trend I show in Figure 14. Whereas my NLSYM and NLSY79 estimates in Figure 14 go from 0.21 to 0.33, and whereas Davis and Mazumder's increase is from 0.14 to 0.48, my alternate estimates rise from 0.25 to 0.41. However, my revised IGE then falls to 0.29, leaving the NLSY97 mobility estimate not statistically different from the NLSYM one. Whereas my NLS estimates in Figure 14 show a 0.06 increase in the IGE, these indicate an increase of just 0.04.

It is worth noting that several papers (including some not focused on trends) have estimated family income IGEs using the NLSY79; the estimates have ranged from 0.37 to 0.45.[72] Only Davis and Mazumder have estimated an IGE as high as 0.48 (their estimate for men) or 0.53 (for women) using the NLSY79.[73] On the other hand, the three previous studies that estimate sons' family income IGEs using the NLSYM find IGEs ranging from 0.21 to 0.31—consistent with my estimates rather than Davis and Mazumder's.[74]

If it were possible to produce ideally measured IGE estimates for the earlier NLS cohort and for the NLSY79, the IGE likely would be higher in both years than reported in either this paper or in Davis and Mazumder. It likely would be higher in the NLSY97 too. There is little reason to think the *trend* would be different from that in Figure 14.[75]

DAUGHTERS

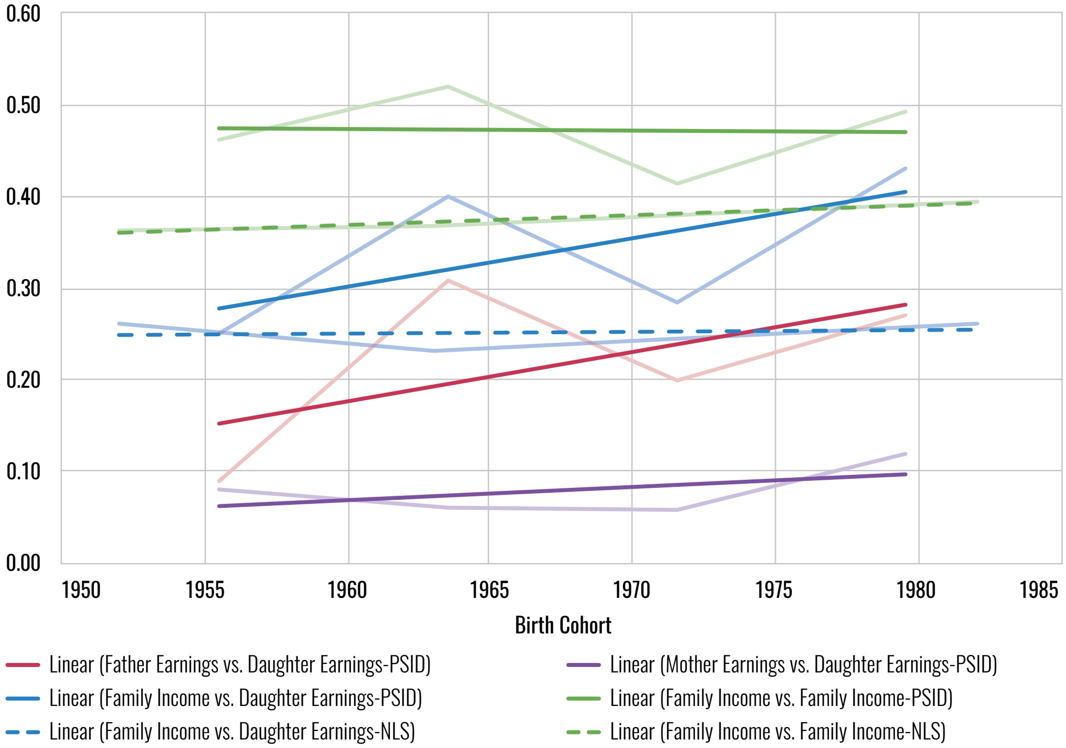
Next, I look at how absolute income changes between generations reduce absolute inequality between daughters and how this has changed over time—at trends in the IGE among women. Note that because the IGE is modeled using logged incomes, the issue of nonworking women becomes simpler in a sense; the log of non-positive income is undefined, and so women without earnings are dropped. The reader should bear in mind, however, that IGEs necessarily exclude nonworking women, which may be more consequential when looking at female trends than it is for male trends.

Father Earnings vs. Daughter Earnings

Figure 15 displays the same trends for daughters shown in Figure 14 for sons. The red lines compare father and daughter earnings in the PSID, using the same birth cohorts as for men. The lighter line shows an increase in the IGE (a fall in mobility) from 0.09 in the earliest cohorts to 0.27 in the most recent ones. This change, while large, again falls short of statistical significance.[76] The linear trend shown in the dark red line indicates a somewhat smaller rise, but nearly doubles over time. The increase in the IGE is larger than that for the IRA in Figure 7.

Only one previous study has estimated trends in the IGE comparing father and daughter earnings. Fertig (2003) found an implausibly large decline in the IGE over a very short period (1985–89 to 1989–1993), and it was not statistically significant.[77]

Figure 15 | DAUGHTERS' INTERGENERATIONAL ELASTICITY (IGE), 1951-83 BIRTH COHORTS



Notes: Estimates are from the Panel Study of Income Dynamics and National Longitudinal Surveys. PSID analyses use four sets of birth cohorts, born 1952–59, 1960–67, 1968–75, and 1976–83. Daughters' outcomes are averaged between the ages of 30 and 35 when observed (up to three times) 1982–94, 1990–2002, 1998–2010, or 2006–18. Parental income is averaged over up to five consecutive years, either 1968–1972, 1976–80, 1984–88, or 1992–96. NLS analyses use three sets of birth cohorts, born 1951–53, 1962–64, or 1981–83. Outcomes are measured between the ages of 31 and 33 when observed in 1984, 1995, or 2014. Parental income is measured in 1967, 1978, or 1996. For full methodological details, see Appendix 1. For point estimates and standard errors, see Appendix 2.

Mother Earnings vs. Daughter Earnings

The purple lines in Figure 15 compare the earnings of PSID mothers and daughters. The IGE rises from 0.08 to 0.12 between the earliest and most recent cohorts, but once again, the increase is not statistically significant. This increase is similar to that for the IRA when excluding women with non-positive earnings (not shown).

Fertig (2003) is also the only study to estimate earnings IGE trends comparing mothers and daughters. She reported no change between 1985–89 and 1989–93.[78]

Parental Family Income vs. Daughter Earnings

The blue lines in Figure 15 show IGE trends comparing daughter earnings to parents' family income. In the PSID, the IGE increases by 0.18 between the first and last sets of cohorts, from 0.25 to 0.43, a change that is marginally statistically significant.[79] According to the PSID, then, both relative and absolute mobility have fallen when daughter earnings are compared to parental family income.

The NLS estimates contradict this finding. As was the case for the male IGE trends, the NLS trends for women indicate smaller changes over time than when the IRA is used. The IGE was basically flat between the three NLS datasets. It fell between the NLSYW and the NLSY79, from 0.26 to 0.23, and then rose back to 0.26, but neither of these changes was statistically significant.

No previous trend estimates are available comparing daughter earnings to parent family income. Davis and Mazumder (2020) pool sons and daughters and find the IGE dramatically increases between the NLSYW and NLSY79, by 0.23. Richey and Rosburg (2017) also pooled sons and daughters and found flat to declining IGEs between the NLSY79 and NLSY97.[80]

Parental Family Income vs. Daughter Family Income

The final set of IGE estimates is shown in the green lines in Figure 15, displaying trends when daughters' family incomes are compared with those of their parents. Both the PSID and NLS suggest modest declines in mobility on this dimension, though neither shows a statistically significant change. The PSID IGE rises from 0.46 to 0.49 between the first and last cohorts. In the NLS, the increase is from 0.36 to 0.39.[81] The linear trend in the PSID is slightly downward.

A number of studies using the PSID have found only small changes in the family income IGE among daughters, generally not statistically significant.[82] However, Hartley et al. (2020) compare daughters' family incomes to those of their mothers in the PSID, pooling a wide range of birth cohorts at any point in time. They find the IGE increased dramatically between 1978 and 2010 but was flat between 2000 and 2010. The estimates of Davis and Mazumder (2020) also show a large increase between the NLSYW and NLSY79, from 0.28 to 0.53. However, Torche (2011), using the same datasets, finds only a small (probably not statistically significant) rise, from 0.34 to 0.37—very similar to my NLS estimates.[83]

SUMMARY OF IGE TRENDS

Comparing fathers' and sons' earnings, mobility may have increased slightly between cohorts born in the 1950s and cohorts born in the late 1970s and early 1980s, but any change was likely small. This is consistent with the IRA trend in the PSID. Comparing parental family income to sons' outcomes, the change in mobility over 30 years is minimal, though the short-term trends in the PSID and NLS look very different.

The trends for daughters are somewhat more ambiguous. In the PSID, most trends indicate falling mobility, except when daughters' and parents' family incomes are compared. The change in the NLS data is minimal over 30 years, consistent with the NLS results for men.

The NLS data suggests that the pattern of absolute income changes between childhood and adulthood are as inequality-reducing today as in the past. Both the PSID and NLS data indicate that is true for sons. The final section of this paper considers whether there has been any change in the likelihood that absolute income changes will make sons and daughters better off than their parents.

05 | Absolute Economic Mobility—Surpassing Parental Income

Raj Chetty and his team generated widespread attention for their finding, noted in the introduction of this report, that the share of adults surpassing their parents' income has fallen dramatically.[84] The vast majority of thirty-year-olds—over 90 percent—born in 1940 exceeded the family income of their own parents at the same age. However, only 50 percent of thirty-year-olds born in 1984 were richer than their parents.

The Chetty team, for the most part, did not rely on true intergenerational data, in which children are observed growing up and then again in adulthood. Instead, using cross-sectional data from the decennial census and the Current Population Survey, the team estimated income distributions for thirty-year-olds from a range of birth cohorts and for parents between the ages of 25 and 35 who had children born in those cohorts. These samples were large enough so that the parental and adult-child incomes for each cohort could be assigned ranks.

The team next mapped the income ranks of adults onto their "parents," leveraging results from two earlier papers. The first, Chetty, Hendren, Kline, Saez, and Turner (2014a, 2014b), found stable relative mobility between the 1971 and 1986 birth cohorts. Based on this result, the team assumed that relative mobility has been unchanged all the way back to the 1940 birth cohort. The second paper (Chetty, Hendren, Kline, and Saez, 2014) used tax data for cohorts born in the early 1980s that allowed the Chetty team to link adults to their own parents years earlier. For these cohorts, the Chetty team had true intergenerational estimates of the likelihood of starting in any one parental family income centile and ending up in any given family income centile themselves.

By assuming this 100-by-100 transition matrix was unchanging over many decades, the team could, for a given cohort, compare the parental income associated with a given parental percentile with the grown-child income associated with a given grown-child percentile and determine if the grown-child income exceeded the parent income. If the tenth centile of parental income for some cohort is $8,000 and the fourth centile of adult-child income for the cohort is $8,001, then knowing the probability of starting in the tenth percentile and ending up in the fourth percentile or higher tells you the share of children starting in the tenth percentile who exceed their parental income. By the assumption of flat relative mobility, the Chetty team could estimate these shares for all parental income centiles for all birth cohorts.

How do the Chetty results hold up against true intergenerational data? The rest of this section presents original estimates of absolute mobility trends using the PSID and NLS samples. The results suggest that the dramatic declines in absolute mobility found by Chetty (and others since) may overstate the drop in absolute mobility that would be revealed if we had access to lifetime income measures.

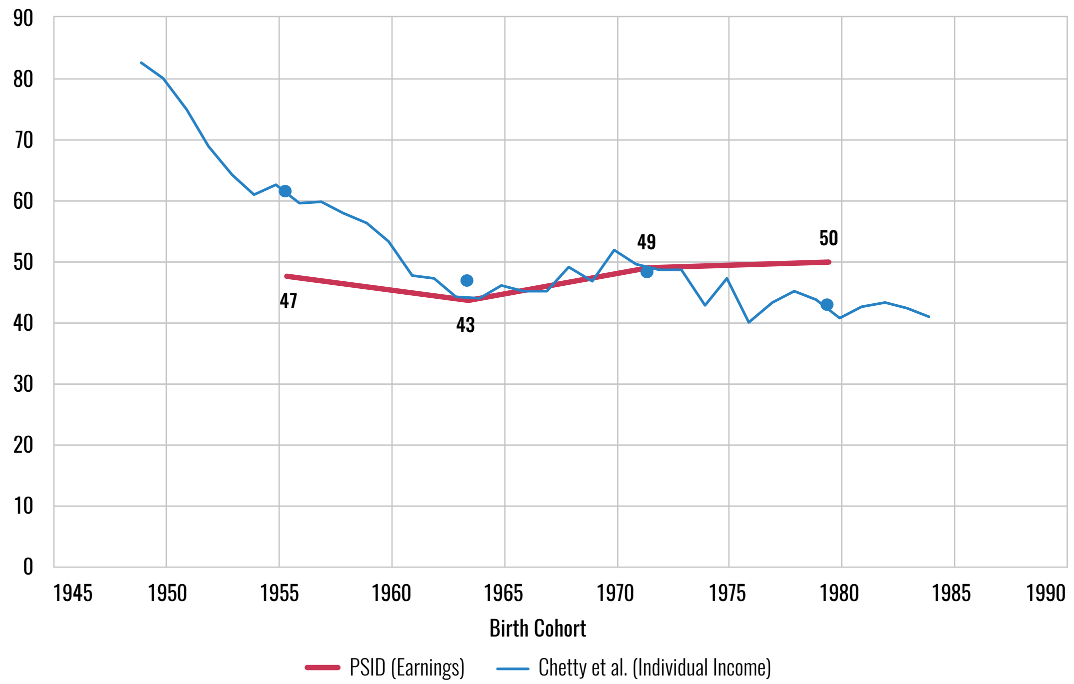
SONS

Father Earnings vs. Son Earnings

The red line in **Figure 16**, below, displays estimates of absolute family income mobility for the four sets of PSID cohorts. For cohorts born 1952–59, 47 percent of sons had higher earnings in their early 30s than their fathers had when the sons were between the ages of 8 and 21. The figure for sons born 1976–83 was 50 percent—no different in the sense of being statistically significant.

In contrast, the blue line in the chart comes from the Chetty et al. data and shows the trend for 1949–84 birth cohorts comparing the individual incomes of fathers and sons. (Individual income includes non-earnings income, so the trend is not strictly comparable to the PSID trend.) The four blue dots plot the eight-year averages for the same sets of birth cohorts that are combined in the PSID. The Chetty estimates indicate that absolute mobility fell dramatically over time. Across the cohorts covered in the PSID, the decline was from 61 percent to 42 percent.

Figure 16 | PERCENT OF SONS EXCEEDING THEIR FATHERS' EARNINGS



Notes: PSID analyses use four sets of birth cohorts, born 1952–59, 1960–67, 1968–75, and 1976–83 (plotted at 1955.5, 1963.5, 1971.5, and 1979.5). Sons' outcomes are averaged between the ages of 30 and 35 when observed (up to three times) 1982–94, 1990–2002, 1998–2010, or 2006–18. Father earnings are averaged over up to five consecutive years, either 1968–1972, 1976–80, 1984–88, or 1992–96. For full methodological details, see Appendix 1. For standard errors, see Appendix 2. Chetty et al. (2016, 2017) estimates are for individual income rather than earnings. Income for sons is measured at age 30 using the Annual Social and Economic Supplement to the Current Population Survey. Income for fathers is measured between the ages of 25 and 35 using decennial census public use samples. The dots are plotted at the same years as the PSID estimates and display equally weighted averages across the same birth cohorts covered by the PSID estimates.

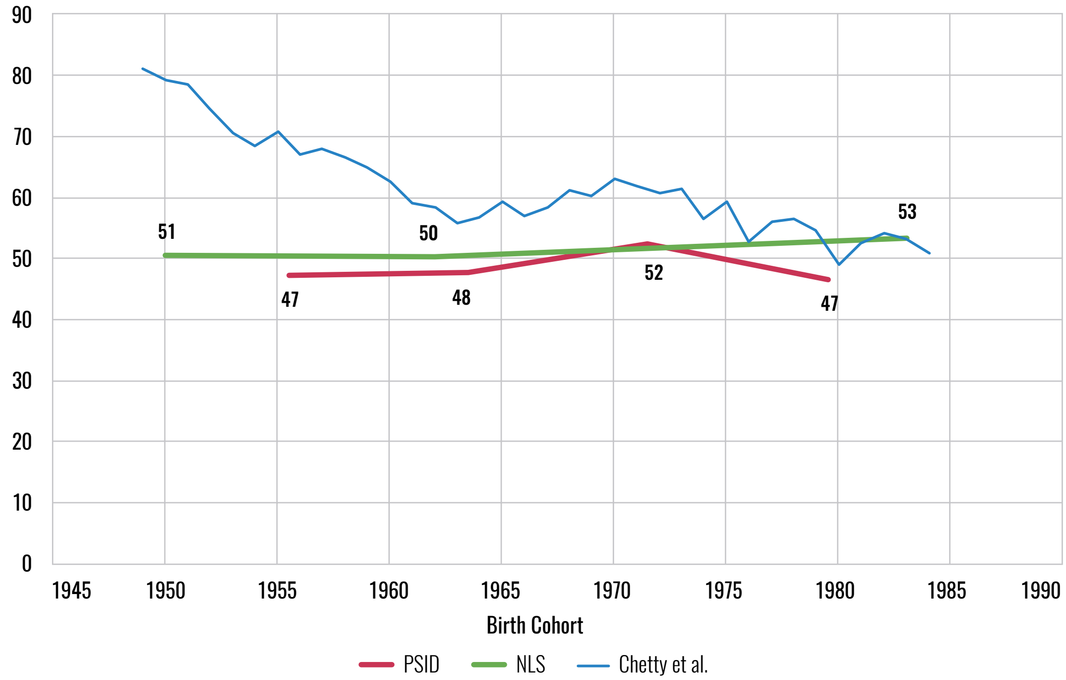
One discrepancy between the two sets of estimates is that the Chetty et al. estimates adjust incomes for inflation using the CPI-U-RS, while I use the PCE deflator. The Chetty team's supplementary estimates looking at family income absolute mobility suggest that their estimates using the PCE deflator would be a bit higher than shown in Figure 16, but the trend would not be much affected.[85] Before further considering the source of the disparity between the Chetty and PSID estimates, I first look at sons' absolute mobility in terms of family income.

In addition to the Chetty et al. paper, four other studies consider trends in absolute earnings or individual income mobility for sons. Hell (2019) and Manduca et al. (2020) report similar results using the same approach as Chetty et al. Stockhausen (2018) finds a decline in absolute mobility using the PSID to compare fathers' and sons' earnings, from 68 percent in 1956–60 to 64 percent in 1961–65, 60 percent in 1966–70, and 48 percent in 1971–75. In contrast, Justman and Stiassnie (2021), also looking at male earnings absolute mobility in the PSID, report an increase in absolute mobility between 1952–61 and 1972–81, from 55 percent to 59 percent.[86]

Parental Family Income vs. Son Family Income

Figure 17 repeats the analysis in Figure 16 but compares sons' family income to that of their parents. The red line shows the PSID trend, with absolute mobility flat over the long run at 47 percent. (The 52 percent rate for the 1968–75 cohorts is higher than the 1952–59 rate.)[87] After adjusting incomes for family size (not shown), absolute mobility is higher for each set of cohorts, because the rise in income over time has occurred while family size has fallen. More importantly, absolute mobility falls from 66 percent to 56 percent, a change that is statistically significant.[88]

Figure 17 | PERCENT OF SONS EXCEEDING THEIR PARENTS' FAMILY INCOME



Notes: PSID analyses use four sets of birth cohorts, born 1952–59, 1960–67, 1968–75, and 1976–83 (plotted at 1955.5, 1963.5, 1971.5, and 1979.5). Sons' outcomes are averaged between the ages of 30 and 35 when observed (up to three times) 1982–94, 1990–2002, 1998–2010, or 2006–18. Parents' family incomes are averaged over up to five consecutive years, either 1968–1972, 1976–80, 1984–88, or 1992–96. NLS analyses use three sets of birth cohorts, born 1949–51, 1961–63, or 1982–84. Outcomes are measured between the ages of 30 and 32 when observed in 1981, 1993, or 2014. Parental income is measured in 1966, 1978, or 1996. For full methodological details, see Appendix 1. For point estimates and standard errors, see Appendix 2. Chetty et al. (2016, 2017) estimates involve the combined income of individuals and their spouses. Income for sons is measured at age 30 using the Annual Social and Economic Supplement to the Current Population Survey. Income for parents is measured between the ages of 25 and 35 using decennial census public use samples.

The green line presents the NLS trend, showing the share of men between the ages of 30 and 32 that had higher family income than their parents did when the sons were ages 15 to 17. It, too, shows a flat trend. None of the three NLS estimates are statistically different from the others. The trend remains flat after adjusting incomes for family size, with about two-thirds of sons exceeding their parents' family income in their early thirties in each cohort.[89]

In contrast, as shown by the blue line in Figure 17, the Chetty estimates comparing sons' and parents' family incomes shows a sharp decline in absolute mobility.[90] As is apparent, for all but the earliest cohorts, the Chetty estimates are relatively similar to those in the PSID and NLS. In particular, the *trends* are similar, especially comparing the trend over the final three PSID cohorts to the Chetty trend. However, for the earliest cohorts, the Chetty estimates show much higher absolute mobility.

What might account for the difference? While the Chetty team lacked true intergenerational data for all but the most recent birth cohorts, they address a variety of potential problems with their analyses using well-conceived sensitivity analyses. In particular, they provide bounds for their absolute mobility estimates for each cohort, which they create by making different assumptions about how relative mobility changes over time. The bounds are fairly wide for most birth cohorts. They include my estimates for all of the PSID and NLS birth cohorts except for the NLS 1949–51 cohorts.[91] However, Berman (2020) shows (combining sons and daughters) that more realistic bounds are much tighter than those shown by Chetty et al. and rule out the most recent cohorts having absolute mobility as high as any of the preceding ones I examine in the PSID and NLS.[92] Manduca et al. (2020) validate the Chetty team's approach by showing that trend and level estimates using the approach are very similar to those obtained using linked intergenerational data for five countries where the data exists.

It appears that the major difference between the Chetty et al. estimates and mine is the timing of when parental income is measured.[93] Chetty and his team estimate parents' income near age 30, regardless of the age of their children or even whether children have been born yet. In contrast, my PSID and NLS results measure parental income when children are teenagers or young adults (and their parents are almost all over thirty years old). Because family incomes grew so much in the 1950s and 1960s, it ends up making a difference which approach is used for children born 1952–59 (in my PSID sample) or 1949–51 (in my NLS sample).

In an appendix table, Chetty and his team display the mean parental incomes in their data by birth cohort. These averages may be compared with averages computed from published Census Bureau tables providing mean family incomes for families with children.[94] It turns out that the Census Bureau averages for years that correspond to birth years in the Chetty data align very well with the Chetty averages. For example, take 1950, the midpoint of the three birth cohorts in my earliest NLS sample. The Census Bureau average for families with children in 1950 (including parents and children with varying ages, not just parents around age 30) was $30,225 (in 2014 dollars, as in Chetty et al.). Meanwhile, the Chetty-reported parental income average for parents ages 25 to 35 (measured in different censuses, not necessarily in 1950) was $32,968 for the 1950 birth cohort.[95]

As these parents and their sons aged during the 1950s and 1960s, they enjoyed rapid income growth. By 1966, when parental incomes are measured for the 1949–51 cohorts in the NLS, the Census Bureau reports that the mean income of families with children was $51,957—a remarkable 72 percent increase in 16 years.[96] Fast-forward to 1981, when I observe those 1949–51 cohorts as adult men, and fewer sons in their early thirties were able to surpass $52,000 (in 2014 dollars) than were able to surpass $30,000 or $33,000.

Similarly, the mean of average parent incomes in the Census Bureau data across 1952–59 (the earliest set of birth cohorts in my PSID data) is $37,522. If I take the mean of the Chetty-reported parental income averages across 1952–59, it is $38,259. The Census Bureau mean across the years 1967–71, when I estimate parental income in the PSID, is $56,548. As in the NLS data, it is easier for the PSID sons to surpass their parents' income when parents were younger in the 1950s than when they were older in the late 1960s.

This is a different issue than the general point that surpassing parental income is more difficult if sons are compared to parents later in their career than earlier in their career. The difference between family income when parents are near age 30 and parental income when children are teenagers is much less dramatic for the later PSID and NLS cohorts. For example, returning to the NLS cohorts, while the Census Bureau shows a 72 percent increase in the mean income of families with children between 1950 and 1966, it shows only a 35 percent increase between 1962 (the midpoint of the middle NLS cohort of sons) and 1978 (when parental income is measured for the cohort). The increase between 1983 and 1996 (the corresponding years for the most recent NLS cohort of sons) was just 13 percent.

Put another way, mean income of families with children grew 81 percent between 1950 and 1983 (corresponding roughly to the birth years of the earliest and most recent NLS cohorts) but by just 19 percent between 1966 and 1996 (corresponding roughly to the years in which NLS sons from these cohorts were observed as adolescents).[97] One would expect to see less absolute mobility when the bar rises 81 percent than when it rises 19 percent.

Another way to convey the same point is to consider Chetty et al. estimates that compare grown-child incomes and parent incomes when both are measured at age 40.[98] Parent income for the 1950 cohort will then be measured in a later calendar year (closer to 1960), thereby missing some of the fast income growth that occurred before 1960. Figure 3C of that study shows the trend in absolute mobility when parental and grown-child incomes are measured around age 40 (combining men and women). For the 1950 birth cohort, the rate of absolute mobility falls by ten points if examined at age 40 rather than age 30—from 79 percent (measured at age 30) to 69 percent. For the 1962 cohort, it rises from 58 percent to 64 percent. Rather than a 21-point drop between the 1950 and 1962 cohorts, the decline is 5 points.[99]

If parent income averaged over the course of an entire childhood were compared with grown sons' incomes, we would see a smaller decline in absolute mobility than Chetty et al. show. But the flat trend I find probably obscures that a real decline occurred, by missing the lower childhood incomes that the earliest PSID and NLS cohorts had when they were very young. Absent average childhood income estimates, researchers must distinguish between what is more relevant—how thirty-year-olds are doing or feel themselves to be doing compared with their parents' situation at age 30, or how they are doing or feeling compared with their living standard as teenagers.

Three other papers estimate trends in the absolute family income mobility of sons. Davis and Mazumder (2020) report a 10-point drop between the NLSYM and NLSY79, from 45 percent to 35 percent, compared with my 1-point drop. Their NLSY79 estimate is much lower than any previously reported estimate for absolute family income mobility.

Hell (2019), one of the coauthors of the Chetty study, used a similar approach to that paper but estimated separate 10-by-10 transition matrices for blacks and whites (from PSID data) rather than using a single 100-by-100 matrix (from tax data) to assign parent and child incomes to ranks. He found a similar decline as Chetty et al. between the 1950 and 1960 cohorts but a larger decline between the 1960 and 1983 cohorts. Over the entire period, absolute mobility falls from 86–88 percent to 40–45 percent. Justman and Stiassnie (2021), using the PSID, find that absolute mobility rose between 1952–61 and 1959–68, from 67 percent to 69–70 percent. It then fell steadily through 1972–81, ending at 62 percent.

Four papers examine trends in absolute family income mobility pooling sons and daughters. Chetty et al. (2016, 2017) show the same decline as for sons.[100] Hell (2019), using a 10-by-10 transition matrix pooling blacks and whites to assign parent and child income to ranks, finds a steeper decline, from around 90 percent during the mid-twentieth century to about 45 percent for mid-1980s cohorts.

Manduca et al. (2020), which includes Hell and another author from the Chetty et al. paper, use the same methods as that paper but use estimates for the distribution of child incomes that include a variety of noncash transfers and refundable tax credits in income. Pooling men and women, they report absolute mobility levels of 66–67 percent for the 1962 cohort and around 55 percent for the 1983 cohort—again a steeper decline than in Chetty et al. They report estimates for earlier cohorts using income as of age 40 rather than age 30. Absolute mobility fell from around 76 percent for the 1950 cohort to around 66 percent for the 1962 cohort, also steeper than in Chetty et al. (where it fell from 69 percent to 64 percent). Both Hell and Manduca et al. report absolute mobility rising after the 1984 birth cohort—the final year Chetty et al. could observe.

Finally, Berman (2020) abstracts from actual two-generation data even further. He shows that Chetty-style absolute mobility estimates are so insensitive to the assumption about underlying relative mobility that using any real-world relative mobility measure will produce similar absolute mobility estimates. Using estimates of pre-tax national income from the World Inequality Database (ultimately from Piketty, Saez, and Zucman, 2016), he finds absolute mobility rates of 80 percent for the 1950 cohort and 55 percent for the 1980 cohort (the most recent available).[101] The trend is very close to that in Chetty et al. Berman's approach allows him to extend his analyses to earlier cohorts. He finds that absolute mobility rose from cohorts born in the late 1910s to those born in the early 1930s (from 77 percent in 1917 to 94 percent in 1933) and declined modestly through the 1930s cohorts (to 91 percent by 1940). The 1950 absolute mobility rate was about the same as in 1920.[102]

A final point worth reiterating is that while not affecting the *trend* in absolute mobility, the PSID and NLS estimates in Figure 17 understate the extent to which sons surpassed their parents' income *in each year* because parental incomes were generally measured when they were older than their early 30s. For example, in the NLSY79 (the middle cohort of my three NLS samples), it is possible to look at absolute mobility when sons are older. Using the same birth cohorts as in the main analyses, but measuring sons' family incomes in 2005 at ages 42 to 44 instead of in 1993 at 30 to 32, 66 percent of men exceeded their parents' family income prior to family-size adjustment, and 79 percent did after.[103]

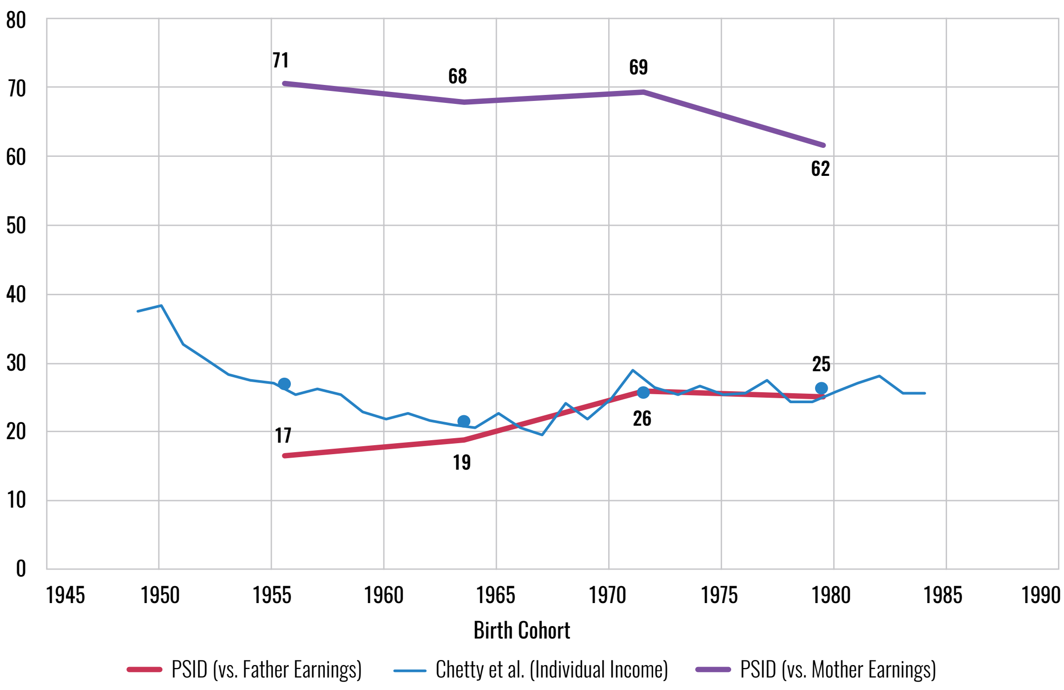
If absolute mobility has, in fact, been flat in recent decades, that would suggest that two-thirds to 80 percent of today's forty-somethings are better off than their parents were at the same age—much higher than the 50 percent suggested by Chetty et al. Similarly, Part One of this primer reported estimates from the PSID, using size-adjusted family income and pooling sons and daughters. The range of those estimates ran from 63 percent to 77 percent for recent cohorts of adults.

DAUGHTERS

Father Earnings vs. Daughter Earnings

Absolute mobility trends comparing PSID daughters' and fathers' earnings are shown in the red line in **Figure 18**. Absolute mobility rises—while 17 percent of daughters born 1952–59 exceeded their fathers' earnings, that was true of 25 percent of daughters born 1976–83.[104] The blue line displays the corresponding estimates from Chetty et al., comparing daughters' and fathers' individual incomes. The estimates are surprisingly close. (Compare the blue dots, which average the Chetty estimates for the same birth cohorts as in the PSID.) The exception is that the earliest PSID cohort has lower absolute mobility than in the Chetty study, so the decline present in the Chetty data is absent in the PSID.

Figure 18 | PERCENT OF DAUGHTERS EXCEEDING THEIR FATHERS' AND MOTHERS' EARNINGS



Notes: PSID analyses use four sets of birth cohorts, born 1952–59, 1960–67, 1968–75, and 1976–83 (plotted at 1955.5, 1963.5, 1971.5, and 1979.5). Daughters' outcomes are averaged between the ages of 30 and 35 when observed (up to three times) 1982–94, 1990–2002, 1998–2010, or 2006–18. Father and mother earnings are averaged over up to five consecutive years, either 1968–1972, 1976–80, 1984–88, or 1992–96. For full methodological details, see Appendix 1. For point estimates and standard errors, see Appendix 2. Chetty et al. (2016, 2017) estimates are for individual income rather than earnings. Income for daughters is measured at age 30 using the Annual Social and Economic Supplement to the Current Population Survey. Income for fathers is measured between the ages of 25 and 35 using decennial census public use samples. The dots are plotted at the same years as the PSID estimates and display equally weighted averages across the same birth cohorts covered by the PSID estimates.

In addition to the Chetty et al. study, Hell (2019) finds that absolute mobility fell for black and white daughters, comparing their own individual incomes to the individual incomes of family heads (who might be fathers or mothers). The upturn in absolute mobility found by Chetty et al. is confined to white daughters. This study relies on the same approach as Chetty et al. (Hell was part of that research team.)

Mother Earnings vs. Daughter Earnings

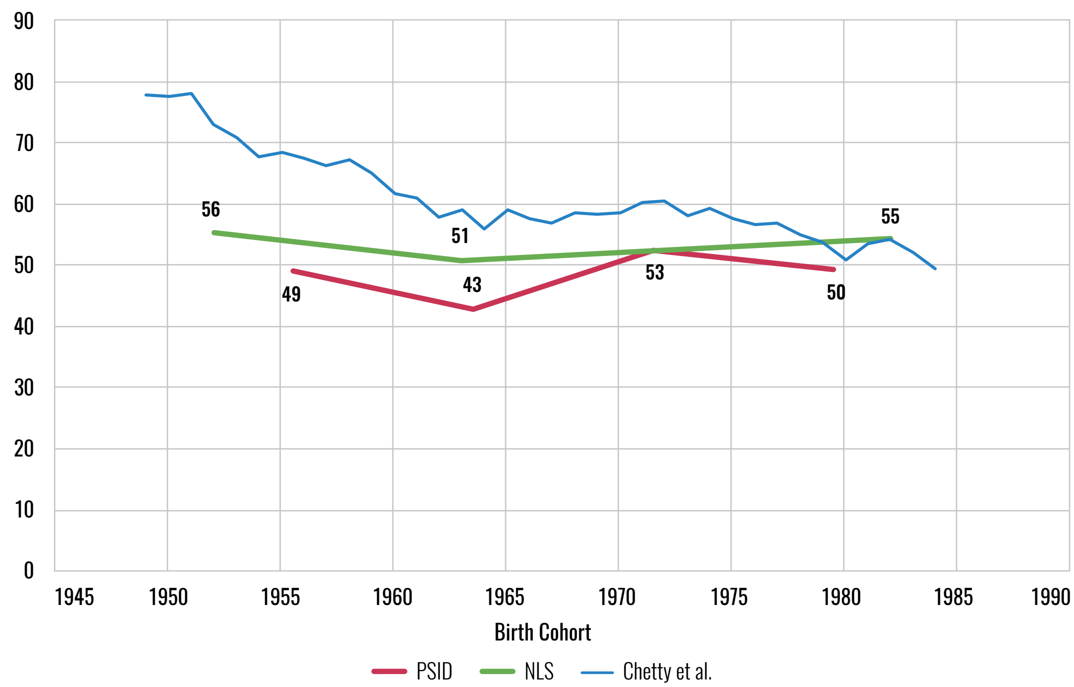
The purple line in Figure 18 indicates the share of daughters with earnings in excess of their mothers. Absolute mobility in this sense declined across the four PSID birth cohorts, from 71 percent for the 1952–59 cohorts to 62 percent for the 1976–83 cohorts.[105]

Hell (2019) compares daughters' individual incomes to those of their mothers. He reports a fall in absolute mobility for both white and black daughters, through at least the 1980 birth cohort.

Parental Family Income vs. Daughter Family Income

The final set of mobility estimates presented in this paper are shown in **Figure 19**. The red line shows the share of PSID daughters whose family income exceeds that of their parents. The change is minimal over time, and not statistically significant.[106] If incomes are adjusted for family size (not shown), the initial drop is larger (from 64 percent to 53 percent) and the subsequent recovery (to 57 percent) is small enough that the decline between the first and last cohorts is statistically significant.

Figure 19 | PERCENT OF DAUGHTERS EXCEEDING THEIR PARENTS' FAMILY INCOME



Notes: PSID analyses use four sets of birth cohorts, born 1952–59, 1960–67, 1968–75, and 1976–83 (plotted at 1955.5, 1963.5, 1971.5, and 1979.5). Daughters' outcomes are averaged between the ages of 30 and 35 when observed (up to three times) 1982–94, 1990–2002, 1998–2010, or 2006–18. Parents' family incomes are averaged over up to five consecutive years, either 1968–1972, 1976–80, 1984–88, or 1992–96. NLS analyses use three sets of birth cohorts, born 1951–53, 1962–64, or 1981–83. Outcomes are measured between the ages of 31 and 33 when observed in 1984, 1995, or 2014. Parental income is measured in 1967, 1978, or 1996. For full methodological details, see Appendix 1. For point estimates and standard errors, see Appendix 2. Chetty et al. (2016, 2017) estimates involve the combined income of individuals and their spouses. Income for daughters is measured at age 30 using the Annual Social and Economic Supplement to the Current Population Survey. Income for parents is measured between the ages of 25 and 35 using decennial census public use samples.

The green line displays the same trend using the NLS data. As in the PSID, absolute mobility initially declines, falling from 56 percent to 51 percent between the 1951–53 and 1962–64 NLS cohorts. Absolute mobility then appears to turn upward, mirroring the PSID trend. The drop in the first period is marginally statistically significant, but the change between the earliest and most recent cohorts is not.[107] After adjusting for family size, the drop is from 67 percent to 62 percent, after which it remains at a very similar 63 percent (not shown). The decline between the first and last cohorts is statistically significant in this case.

As with the results for men, the PSID and NLS estimates disagree with the Chetty team's (blue line), which indicate a larger decline in absolute mobility. The trend in Hell (2019) for daughters is similar to Chetty's. As noted above, in the discussion of sons' absolute mobility, the true decline in absolute mobility is likely to lie somewhere between the Chetty team's results and my own, because they and I rely on a single year of parental income (at different points in childhood) rather than income over all of childhood, and incomes rose rapidly during the childhoods of the earliest PSID and NLS cohorts.

Davis and Mazumder (2020) estimate an absolute mobility trend between the first two NLS cohorts and report results very similar to my own, with absolute mobility falling from 63 percent to 51 percent. For other estimates that pool sons and daughters, see the section above on sons' absolute mobility.

If I estimate absolute mobility for daughters in the NLSY79 when they are ages 41–43 in 2005 (rather than 31–33 in 1995), I find that 61 percent experienced upward absolute mobility (rather than the 51 percent shown in Figure 19). That is lower than the range of estimates I report in Part One of this primer using data not limited by the imperatives of trend analyses, and it is lower than the estimates from other studies cited there. Adjusting for changes in family size brings the estimate to 71 percent—more in line with those other studies.

SUMMARY OF UPWARD ABSOLUTE MOBILITY TRENDS

Upward absolute mobility, in the sense of having a higher income in one's early 30s than parents did in one's adolescence, seems to have changed little among men since the mid-twentieth century. (However, using size-adjusted incomes, family income mobility in the PSID fell by 10 percentage points.) Outpacing one's father's earnings has increased among daughters, but fewer daughters exceed their mothers' earnings or, perhaps, their parents' family income.

My analyses suggest that the declines in absolute mobility found by Chetty et al. and other studies using similar methods may be overstated. They measure parent income around the time children are born, setting a relatively low bar for children born mid-century to clear when compared with parent income over their whole childhood. Because my trends start with parent income measurements that miss the remarkably fast income growth of the 1950s and early 1960s, they miss or understate likely declines in absolute mobility during the mid-twentieth century.

06 | Conclusion

Reports of the death of the American Dream are premature. Just as Part Two of this primer showed that by some measures of mobility, the US compares well against its peer nations, this report has shown that many trends in American income mobility appear less worrisome than is often claimed. For sons, relative earnings mobility may have increased over the long run, while relative mobility against parental family income may have declined. Any changes were modest. Evidence reviewed in Appendix 3 even suggests that men's relative earnings mobility has been more or less flat since 1860 or 1870.

The male IGE trends presented here also paint a picture of stability. There has been little change at all in the ability of intergenerational mobility to narrow absolute gaps in childhood income, despite rising point-in-time inequality.

The evidence is somewhat more ambiguous for women. Most of the evidence indicates that relative mobility fell. However, except for family income mobility in the PSID, the changes are modest, ambiguous, or both. Some of the IGE trends in the PSID, but not in the NLS, also indicate falling mobility.

The clearest evidence of deterioration in intergenerational mobility comes from looking at the share of Americans whose income exceeds that of their parents. Even here, however, the news is not as bad as it may seem. For one, as I argue above, the research from Chetty and others finding large declines in absolute mobility probably overstates the magnitude of the declines. In these studies (and mine) childhood income and adulthood income each are measured in a single year. But the rapid increase in incomes mid-century makes absolute mobility estimates for Americans born in this era sensitive to the point in childhood where parent incomes are measured. Setting the bar at parental income at the time a child was born (say, 1950) will produce lower absolute mobility estimates than setting it at parental income in adolescence (say, 1966). That distinction makes less of a difference for later birth cohorts. Ideally, we would measure parental income over the entire course of childhood, in which case the decline in absolute mobility over time would likely be more moderate.

Even accepting that absolute mobility has declined over time, however, such a result must be put into context. Absolute mobility appears to have declined across many diverse industrialized nations.[108] It declined across all 50 states.[109] This common trend is likely to reflect the fact that as nations grow richer, it becomes more challenging to maintain high rates of economic growth. Rapidly developing countries like China almost surely have higher absolute mobility than the US, but few Americans would trade our higher living standards for China's higher absolute mobility. Similarly, Berman (2020) finds that absolute mobility in the US peaked in 1933, but who among us prefers that they would have been born into the Depression? Finally, absolute mobility is also higher among poor children than among rich children.[110] Yet, it defies reason to think it better to be poor and have a 90 percent chance of upward mobility than to be born in the upper middle class with a 70 percent chance.

Nevertheless, while the American Dream is not dying, it could surely be in better shape. Part One of this primer found that by many measures, intergenerational mobility levels are substantially lower than many mobility researchers believe. Part Two confirmed that relative family income mobility is lower in the US than in most of its peer countries, and absolute growth between childhood and adulthood reduces childhood inequality less than occurs in other countries. Finally, mobility gaps between black and white Americans are vast. Among whites who grow up in the bottom fifth of income, 28 percent of sons and 33 percent of daughters remain in the bottom fifth of family income as adults. Among blacks, those figures are 50 percent and 62 percent.[111] One in one hundred white Americans in their thirties is experiencing the third consecutive generation of being in the bottom fifth; among black Americans, one in five are.[112]

The American Dream is an ideal, and while ideals are unachievable, they provide an aspiration to which we may commit ourselves. Few policy issues are as simple and straightforward as the loudest advocates with the tidiest stories suggest; intergenerational mobility and opportunity are no different. We do ourselves no service oversimplifying matters if we want to help the disadvantaged. My hope is that this primer can usefully complicate narratives of decline and decay while arming those who seek to expand opportunities to children in need of more.

This appendix provides detailed information on the data sources used in the mobility analyses and the methodological choices I made.

PANEL STUDY OF INCOME DYNAMICS

The Panel Study of Income Dynamics (PSID) has been conducted by the University of Michigan Institute for Social Research's Survey Research Center. The PSID began in 1968 with two samples. The first included almost 3,000 households representing the lower 48 states. A second targeted low-income non-elderly households from metropolitan areas and the rural South, adding nearly 2,000 additional households. For the next 29 years, the PSID interviewed these families annually. As children grew up and left to form their own households, the PSID followed them as well, and it has followed *their* children to new homes too. Since 1997, PSID interviews have been conducted only every two years, with the 2019 data the most recent available.

The PSID added samples of immigrants in 1997 and again in 2017 and 2019, so that the data would better reflect the contemporary American population rather than the family trees of the late-1960s population. However, because the parental income of immigrants or children of immigrants is observable in the PSID only if parents are included in the survey, intergenerational analyses will not strictly reflect the experiences of the contemporary American population in all its diversity. (In particular, the 2017 and 2019 immigrant samples are dropped from these analyses, because no one in these samples has been observed both in adulthood and as children.)

I chose the PSID sub-samples to analyze in this paper in order to be consistent with the National Longitudinal Survey analyses described below. I created four sets of birth cohorts and estimated mobility trends across these groups. Rather than strictly attempt to replicate my NLS methods, I leverage the strengths of the PSID to produce improved mobility estimates, by averaging multiple years of income.

Specifically, the earliest group I analyze includes children who were between the ages of 10 and 17 when their parents were interviewed in 1969. Because most interviews occurred by early May, that means that most of these children were born 1952–59. I refer to this set of cohorts by these birth years in the paper, and I refer to the subsequent cohorts similarly. I average parent incomes across the 1969–73 waves of the PSID, measuring income over up to five years between 1968 and 1972. (The PSID income questions ask about income earned the previous calendar year.) For comparison, the earliest cohort of sons in the NLS data (described below) were born 1949–51, and their parental income is observed in 1966, when they were 14–17. The earliest cohort of NLS daughters were born 1951–53, and I observe their parental income in 1967, when they were 13–16.

I then compare the parental income of PSID sons and daughters to their own incomes in adulthood. I average their incomes over the seven years 1982, 1984, 1986, 1988, 1990, 1992, and 1994, when they are between the ages of 30 and 35. I use every other year because in the more recent PSID sub-samples I analyze, grown sons and daughters are observed during the time when the PSID was biennially administered, so I treat the earlier sub-sample as if it were subject to the same limitation. For comparison, in the NLS samples, the sons in the earliest cohort are observed in 1981 when they are 29–32, and the daughters are observed in 1984, when they are 30–33.

Mobility in this early set of PSID cohorts is then compared with mobility in more recent sets. The second set of cohorts was born 1960–67, with parental income measured 1976–80 and grown-child income measured over every other year from 1990 to 2002. The third set of cohorts was born 1968–75, with parental income measured 1984–88 and grown-child income measured over every other year from 1998–2010. Finally, the most recent set of cohorts was born 1976–83, with parental income measured 1992–96 and grown-child income measured over every other year from 2006 to 2018.

Just as the earliest PSID cohort is roughly comparable to the earliest NLS cohort described below, the most recent PSID cohort is roughly comparable to the most recent NLS cohort. In my NLS analyses, the most recent cohort of sons was born 1982–84 and the most recent cohort of daughters was born 1981–83; their parental income is measured in 1996. The NLS adult sons and daughters are observed in 2014 when they are 30–32 (sons) or 31–33 (daughters).

Income Measures

For most years in the PSID, earnings are not available in the PSID data for anyone who is not a family head or a partner of the head (married or cohabiting in a serious relationship). I therefore exclude all earnings from non-heads and non-partners in my earnings mobility analyses.

Earnings in the PSID are probably better measured for men than for women. When a family is headed by a married or cohabiting couple, the PSID has defaulted to designating the male partner as the head. The earnings of heads have been more consistently and completely assessed over the years than have the earnings of heads' partners. While women in couples are sometimes designated the head, this is relatively unusual.

For heads, earnings include income from wages and salaries; bonuses, overtime pay, and commissions; tips (mentioned beginning in 1981); income from a professional practice or trade; the labor part of income from farms, businesses, gardening (through 2013), and (through 1992) taking on roomers and boarders; and (since 1993) "wages from extra jobs" and "other job-related income." It also includes what was previously considered the asset part of income from gardening from 1994 through 2013.

Bonuses, overtime pay, and commissions were combined in one PSID question through 1980. Tips were added to this question from 1981 to 1992, before the four types of income were separated in 1993. Labor income from roomers, boarders, farming, and market gardening were combined in one question in 1968 and 1969. In 1970, farming income split off into its own question, and in 1979, market gardening income split off from rooming and boarding.

For heads' partners, through the 1978 wave, earnings are taken primarily from up to two questions about income sources, in which the respondent indicated both the source and the amount. From 1979 forward, one question has been asked specifically about "earnings from work." Otherwise, in many years, information on ownership of a family farm or business and work for that enterprise is used to allocate some earnings to partners. (The other components of earnings are available specifically for partners beginning in 2015, but for consistency, I chose not to include them.)

The variables in the PSID data apportion income from self-employment between labor and asset income and heads and partners by allocating positive incomes equally between labor and asset income (separately for each partner) and by assigning negative incomes entirely to asset income. (This methodology appears to be somewhat more complicated in practice for some years.)

I create head labor income from farms after 1993 by assigning half of the head's farm income to labor income, setting it at $0 if the head's farm income is negative. (This effectively gives all farm income to the head rather than splitting it between heads and partners, as seems to have been done in the PSID most of the time.) I then add it to the created head labor income variable in the PSID. Labor income from businesses is not included in the PSID-created head and partner labor income variables after 1993 so I add them in (after creating them for immigrants in the 1997 data).

Family income in the PSID includes wage and salary income; bonuses, overtime pay, commissions, and tips; income from businesses and farms; income from a professional practice or trade; income from gardening; income from roomers and boarders; rent; interest; dividends; royalties; income from trust funds; Social Security; other retirement income; government cash welfare programs; unemployment benefits and worker's compensation; alimony and child support payments; help from others outside the household; and other income. This definition has been consistent since very early in the history of the PSID, though the wording of questions, the extent to which specific sources are explicitly mentioned, and the number of questions used to record income amounts have changed over the years. Because family income is bottom-coded at $0 in the PSID data prior to 1994, I bottom code income in every year to $0. (Labor income is also bottom-coded at $0 in the PSID, as losses from small business employment are recorded as asset income.)

Other Details

I use the PSID Family Identification Mapping System utility to match grown children to their parents. Parents may be biological or adoptive.

In all analyses, I weight each observation by the sampling weight included in the most recent PSID wave in which a grown son or daughter is observed. The sampling weights in any survey wave adjust the PSID sample to be representative of the contemporary American population (including immigrants), in part by adjusting for sample attrition from the survey. Using weights from different survey years is not ideal, but the difference is unlikely to matter much for my analyses, and I am primarily interested in avoiding bias from selective attrition.[113] The sample weights also adjust for the impact of changing criteria for sample inclusion of family members, efforts to bring attriters back into the survey, changes in the criteria for who is retained in the sample when people move out of households, and a 1997 reduction of the low-income sub-sample.[114]

I include people from the low-income sub-sample (the “SEO sample”), that was drawn in the 1960s, in order to boost my sample sizes.[115] The original drawing of the SEO sample, unfortunately, involved significant departures from random sampling.[116] However, my past research on volatility and mobility suggests that trends are similar whether or not the SEO sample is included.[117]

In the relative mobility analyses, I rank parental incomes and the incomes of grown sons and daughters within the sub-sample of families that have valid observations on both parent and grown-child income. Within a generation, I assign people with the same income the same rank. I do not control for birth cohort or age, but all incomes are adjusted for inflation using the personal consumption expenditures (PCE) deflator. In the main analyses, I include people with incomes of $0 when I rank incomes.

In the intergenerational elasticity analyses, I bottom-code positive logged incomes at the third percentile of such incomes. This ensures that trends are not affected by changes in the number of people reporting very low incomes in the PSID. During the early 1990s, a variety of methodological changes in the PSID administration increased the number of very low income reports, and when incomes are logged, that can unduly affect the resulting trend estimates.[118] When converting to logs, non-positive incomes are undefined and are therefore not included in IGE analyses.

In sensitivity analyses, I adjusted family incomes for family size by dividing them by the square root of the number of family members. Unless noted in the report, the results were not significantly changed.

For the IGE and IRA analyses, I pooled the data on all four cohorts and included an interaction between parental income and cohort in order to obtain standard errors for the change (relative to the first cohort). In the transition matrix and absolute upward mobility analyses, I estimated results separately by cohort and treat the standard errors as if they were obtained from independent samples. Sensitivity checks examining IGEs indicated that this treatment is innocuous.

Sample sizes and descriptive statistics for family income analyses that include non-positive incomes are shown in Table 1 at the beginning of the report.

NATIONAL LONGITUDINAL SURVEYS

The analyses rely on three studies that are part of the National Longitudinal Surveys, described below. The analyses of men compare the following samples:

- Men 30 to 32 years old as of the end of March 1982, born mostly between 1949 and 1951. Their own earnings and family income over the 12 months preceding their interview in 1981 are assessed. Since interviews were conducted between October and December, that roughly corresponds with 1981 income. Their 1966 parental family income is also assessed. (The men were 15 to 17 years old as of the end of March 1967.)
- Men 30 to 32 years old as of the end of 1993, born between 1961 and 1963. Their own 1993 earnings and family income are assessed, as is their 1978 parental family income. (The men were 15 to 17 years old as of the end of 1978.)
- Men 30 to 32 years old as of the end of 2014, born between 1982 and 1984. Their own 2014 earnings and family income are assessed, as is their 1996 parental household income. (The men were 12 to 14 years old as of the end of 1996.)

The analyses of women involve the following comparisons:

- Women 31 to 33 years old as of the end of 1984, born between 1951 and 1953. Their own earnings and family income are assessed for the 12 months preceding their interview in 1985, and since interviews were conducted between January and March, that roughly corresponds with 1984 income. Their parental family income is assessed as of 1967. (The women were 14 to 16 years old as of the end of 1967.)
- Women 31 to 33 years old as of the end of 1995, born between 1962 and 1964. Their own 1995 earnings and family income are assessed, as is their 1978 parental family income. (The women were 14 to 16 years old as of the end of 1978.)
- Women 31 to 33 years old as of the end of 2014, born between 1981 and 1983. Their own 2014 earnings and family income are assessed, as is their 1996 parental household income. (The women were 13 to 15 years old as of the end of 1996.)

Ideally, the analyses would examine sons and daughters at older ages, near age 40, but the last year in which men were observed in the data for the earliest cohorts was 1981, when the oldest men who were previously observed at home at 17 were 32. For all of these samples, I exclude grown sons and daughters who are enrolled in school. (In my PSID analyses, these sons and daughters are included and up to three years of income are averaged.)

In all analyses, I use a single year of earnings or income rather than averaging, simply because it is not always possible to average incomes consistently across samples (especially parental incomes, particularly given that children selectively move out of their homes as they age). Using single-year measures of income will tend to bias the estimated mobility levels upward (indicating too much mobility). However, that is only a problem for trend analyses if incomes become more or less volatile over time, so that any given single year of income becomes a noisier or less noisy measure of permanent income. Most research suggests that volatility has not changed much since the early 1980s, though it may have increased in the 1970s.[119] One study suggests minimal change in short-term earnings movements between the mid-1950s and the early 2000s.[120]

Earnings include self-employment income along with wage and salary income (and commissions and tips). I top code the highest 3 percent of earnings and incomes for each sample, and I bottom-code the lowest 3 percent of logged earnings and income in the IGE analyses. (I make no attempt to create consistent top codes across the samples, though this is unlikely to be important for any of the analyses.) All incomes are adjusted for inflation using the Personal Consumption Expenditures (PCE) deflator.[121]

Each of the NLS studies includes sampling weights that are intended to correct for attrition and oversampling of subgroups. I use the sampling weights for the survey year in which grown children's incomes are observed. I have estimated all IRAs and IGEs without weights, and the trends are not meaningfully affected.

All IRA and transition matrix analyses include earnings and family incomes of $0 or less, as do the analyses of upward absolute mobility. When converting to logs, non-positive incomes are undefined and are therefore not included in IGE analyses. In IRA and transition matrix analyses, tied incomes are given the same percentile rank. In general, trends are not sensitive to these decisions.

In IRA and transition matrix analyses, ranks are computed for parental income and grown child income without regard to whether income is observed in the other generation (but enrolled students are excluded in computing grown-child ranks). (This methodology differs from that in the PSID, where the ranking occurs only within the sub-samples with both parental and grown child income.)

Sample sizes and descriptive statistics for family income analyses that include non-positive incomes are shown in Table 1 at the beginning of the report.

NLSYM and NLSYW

Estimates for the earliest of the three cohorts came from the "Original Cohorts" of the National Longitudinal Surveys program, sponsored by the Office of Manpower, Automation, and Training within the Department of Labor.[122] The National Longitudinal Survey of Young Men (NLSYM) included 5,225 adolescents and young adults who were between the ages of 14 and 24 as of the end of March 1966. These men were interviewed 12 times between 1966 and 1981. The early-cohort estimates for women come from the National Longitudinal Survey of Young Women. The NLSYW began with 5,159 girls and women who were ages 14 to 24 at the end of 1967. They were interviewed 22 times between 1968 and 2003.

These youth report their family income, which is effectively their parental income while they live at home.[123] There are two problems with these income measures. First, teenagers and young adults cannot necessarily be expected to report their parents' incomes accurately. Second, respondents were only asked to place their total family income into one of eleven ranges.

Information on family income as reported by their parents is obtainable for many NLSYM men and NLSYW women whose parents participated in parallel surveys. The National Longitudinal Survey of Older Men was a study of 5,020 men who were ages 45 to 59 at the end of March 1966. They were interviewed 13 times between 1966 and 1990. The National Longitudinal Survey of Mature Women included 5,083 women between the ages of 30 and 44 at the end of March 1967, who were interviewed 21 times between 1967 and 2003. The respondents in these four "Original Cohorts" were selected from the same initial sample, and the records for sampled household members may be linked in the microdata.

In the 1967 waves of the NLSOM and NLSMW, respondents were asked about multiple types of income received in 1966, providing amounts for wages, salaries, commissions, and tips (separately for themselves, their spouses, and other family members); business, professional practice, or partnership income (separately for themselves and other family members); farm income; rental income; income from interest and dividends; unemployment compensation (separately for themselves and other family members); disability income (separately for themselves and other family members); Social Security income; public assistance; Title X job training benefits; food stamps; public pensions; private pensions; and other income.[124]

Unfortunately, because of the age ranges sampled for the Original Cohorts, the sub-sample of sons and daughters who may be matched with mothers or fathers is not representative.[125] The youngest sons and daughters (the ones used in my sample) who have older mothers cannot be matched to them, while those with younger fathers cannot be matched to them. Furthermore, sons and daughters are more likely to be matched to mothers than to fathers.

This is because the NLSOM sample is relatively old but also because the initial sample of housing units from which the Original Cohorts were drawn was rescreened for eligible NLSYM sample members following the drawing of the NLSOM sample, capturing some sons that could not be linked to fathers in the NLSOM.[126] The NLSMW and NLSYW samples were then drawn incorporating the results of the rescreening. The resulting sub-sample of the youngest NLSYM and NLSYW participants who may be matched to a parent disproportionately excludes children born to older parents.

Using parent-reported incomes tends to result in estimates of mobility that are somewhat lower than when using child-reported incomes (assigning families midpoint of the income range children indicate, or 1.5 times the lower end of the range for the highest category). However, the differences are not large enough to overturn the conclusions in the report. Given the potential for substantial measurement error in the child-reported incomes, the parent-reported continuous measure is the basis for the results in the paper (using the mother-reported amount if available, otherwise the father-reported amount). I also experimented with implementing a reliability correction to the IGE and IRA estimates, using son-reported incomes, as described below in the section on survey differences. For reasons outlined there, I chose not to report these results.

The adult earnings and family income of grown sons are taken from the final NLSYM survey, in 1981. The earnings measure for men in the NLSYM does not include farm income, since the survey does not allocate such income between husbands and wives.

The family income of grown sons in the NLSYM is assessed through a series of recorded amounts for income received the previous 12 months (separately for respondents and for their spouses) including wages, salaries, commissions, and tips; business or professional practice income; unemployment compensation; supplemental unemployment benefits from employers; and disability benefits. The surveys also record combined amounts for husbands and wives received as farm income, rent, interest and dividends, food stamps, Aid to Families with Dependent Children, Supplemental Security Income, alimony and child support payments, financial assistance from relatives, and other income. Aggregate income from other family members is recorded as falling in one of 11 income ranges. I recode this variable to the midpoint of categories, or 1.5 times the lower end of the range for the highest category. Then I add all of the components together, treating missing components as $0.[127]

For grown daughters, earnings and family income come from the 1985 wave of the NLSYW. Earnings do include a woman's individual farm income.

Daughters' own family income comes from aggregating the wage and salary income of the daughter and her spouse, the business and farm income of both, and the unemployment compensation income of both, as well as the combined income of daughters and their husbands from food stamps, public assistance, aid provided by relatives, and other income. Finally, aggregate income from other family members, asked in a single question, is included. Missing components are treated as $0. Comparison with a created variable in the data that treats family income as missing when any component is missing suggests that the results are insensitive to the assumption.

In addition to looking at results including or excluding non-positive incomes and giving ties the same or different ranks, I conducted a number of other sensitivity checks. I ran models with weights adjusted to "weight up" sons with fathers who have relatively older wives so that the weighted number of NLSYM sons matched to NLSOM fathers was equal to the weighted number of sons matched to NLSMW mothers. I used family incomes adjusted for family size. I ran models using the categorical son-reported family income variable (recoded to category midpoints) and using parent-reported income recoded to resemble the son-reported category midpoints. I ran models using only sons with both parent-reported and son-reported family income. I ran models that computed percentile ranks using only the subset of parents with grown-son income available and using only the subset of sons with parental income available (rather than computing ranks independently for parent and son incomes). I also ran models using parental income in 1968, taken from the NLSOM and NLSMW. To give a sense of how sensitive the results are to such decisions, estimated IRAs comparing son earnings to family income ranged from 0.18 to 0.28 across these tests, compared with the 0.24 I report in Figure 1, all lower than the estimates I report for the NLSY79 and NLSY97.

NLSY79

The National Longitudinal Survey of Youth 1979 (NLSY79) was originally sponsored by the Office of Manpower, Automation, and Training and now sits within the Bureau of Labor Statistics in the Department of Labor.[128] The NLSY79 included 12,686 adolescents and young adults who were between the ages of 14 and 21 as of the end of 1978. The survey is ongoing. The most recent data available is from the 2018 wave—the 28th since 1979. Initially, the sample included a nationally representative group of men serving in the armed forces, but all but a few members of that sample were dropped after the 1984 interviews. (Only a few are in the birth cohorts I examine, who were all under 18 at the start of the survey. I drop them from my analyses.) Oversamples of Latinos, African Americans, and poor youth not black or Latino were also drawn at the outset, but the poor non-black, non-Latino sub-sample was dropped after 1990.

I use income information taken from respondents' first interviews, in 1979, which ask about income in 1978. If the youth lived at home, his or her parent provided information about family income, and I restrict my analyses to such youth. Information is unavailable on the earnings of fathers during 1978; parent respondents are asked about whether they received a variety of kinds of income, but they are asked to report only a single total family income amount after these questions.

The adult earnings and family income of sons are taken from the 1994 wave, which captured income received in 1993. For women, earnings and family income are measured in 1995, taken from the 1996 wave. I add military pay to wage and salary and self-employment income for my earnings measure, though this ends up making little difference to the results. Family income is estimated from individual amounts recorded separately for the spouses from wages, salaries, commissions, and tips; military pay; farm income; other business income; unemployment benefits; and education benefits. Combined spousal income is recorded for child support, alimony, Aid to Families with Dependent Children, food stamps, other public assistance, disability and Social Security benefits, and other income. Aggregated welfare and non-welfare income are also recorded for other family members.

In addition to looking at results including or excluding non-positive incomes and giving ties the same or different ranks, I conducted a number of other sensitivity checks. I used family incomes adjusted for family size. I ran models that computed percentile ranks using only the subset of parents with grown-son income available and using only the subset of sons with parental income available (rather than computing ranks independently for parent and son incomes). I used a more limited parental family income measure from the screening interviews done in late 1978 and measures that used either that measure or the measure from the 1979 interview. I used a version of the adult family income variable that included the income of cohabiting partners. To give a sense of how sensitive the results are to such decisions, estimated IRAs comparing son earnings to family income ranged from 0.30 to 0.37 across these tests, compared with the 0.34 I report in Figure 1. These estimates are all higher than the range of estimates in the NLSYM, but some are lower than some of the estimates I report for the NLSY97, reinforcing the conclusion that there was minimal change between these two cohorts.

NLSY97

Sponsored by the Bureau of Labor Statistics, the National Longitudinal Survey of Youth 1997 (NLSY97) has followed adolescents who were between the ages of 12 and 16 at the end of 1996. The most recent survey for which data is available occurred between 2017 and 2018—the 18th wave of the survey. The sample originally was comprised of 8,984 youth.

The parental income information is taken from the first wave of the survey, in 1997. Parents answered questions about the income they received in 1996 from a variety of sources: wages, salaries, commissions, and tips (separately for the respondent and his or her spouse or partner); business, farm, partnership, or professional practice income (separately for respondents and spouses and partners); interest and dividends; Aid to Families with Dependent Children; food stamps; Supplemental Security Income; child support; and other income. They also reported aggregated income for other household members ages 14 and older, for earnings and for other income. Note that the summary measure is for household income rather than family income, as in previous cohorts. The NLSY97 differs from the other cohorts in that respondents were probed further when they did not indicate an amount for some income type and encouraged to report income within a range.

Adult earnings and family income for 2014 are obtained from the 2015–16 survey. Family income amounts are summed from questions about wages, salaries, commissions, and tips; business, farm, partnership, and professional practice income; and worker's compensation (all separately for the respondent and his or her spouse or partner). Also incorporated into family income are aggregated amounts for the respondent and spouse's child support; interest and dividends; income from rent, estates, trusts, inheritances, and gifts; and other income, as well other family members' aggregated income. For each of these individual components, respondents were probed further when they did not indicate an amount for some income type and encouraged to report income within a range. For earnings, I ignored all adults who reported their income in a range. Note that unlike in previous cohorts "family" income includes the income of the youth's cohabiting partner.

In addition to looking at results including or excluding non-positive incomes and giving ties the same or different ranks, I conducted a number of other sensitivity checks. I used family incomes adjusted for family size. I used parental income measures that included only those who reported all income component amounts when initially asked (rather than on a follow up, in which case the income component is reported in categories). In a second measure, I also recoded those follow-up answers myself and combined them with the income amounts people reported directly. I used a measure that excluded cohabiters for better consistency with the previous NLS cohorts. I replaced household income in 1996 with income in 1997 or 1998 if it was missing for 1996. And I used an adult earnings measure that recoded categorical responses for wage and salary income and self-employment income to categorical midpoints. Because of higher rates of missing earnings data in 2015–16, I also ran results for the NLSY97 replacing missing values with estimates of 2012 earnings from the 2013–14 survey. To give a sense of how sensitive the results are to such decisions, estimated IRAs comparing son earnings to family income ranged from 0.28 to 0.34 across these tests, compared with the 0.31 I report in Figure 1. These estimates are all higher than the range of estimates in the NLSYM, but the range overlaps with that for the NLSY79, reinforcing the conclusion that there was minimal change between these two cohorts.

Inconsistencies between the Three NLS Cohorts

There are a variety of differences between the three NLS datasets that could potentially create comparability problems. This section reviews those differences and considers whether any of them are likely to bias the estimated trends in mobility described in the paper.

UNIVERSE COVERED, SAMPLING RULES, AND WEIGHTING

The NLSYM sample initially reflected the national civilian institutionalized population between 14 and 24 as of the end of March 1966.[129] As the survey progressed, men were not interviewed during a wave in which they were institutionalized or in the armed forces or (in later years) lived abroad. They were permanently dropped from the sample if they missed two interviews in a row for reasons other than refusing to participate. Tests I conducted using the NLSY79 indicate that excluding respondents outside the US or in the military and those who have missed two previous interviews does not affect the results meaningfully, so this is probably not a major source of inconsistency.

As of the 1981 wave of the NLSYM—the final one and the wave from which I draw adult earnings information—65 percent of the original sample was interviewed. Sampling weights included in the data are intended to keep the weighted sample representative of the national civilian institutionalized population between the relevant ages as of the time the sample was originally drawn.[130]

The NLSYW sample was chosen to be representative of the national civilian institutional population between 14 and 24 as of the end of 1967.[131] In subsequent years, women were not interviewed if they were institutionalized, in the armed forces, or lived abroad. Beginning in 1969, they were permanently dropped if they previously refused to be interviewed, and starting in 1971 they were permanently dropped if they missed two years in a row for a reason other than refusal. As of the 1985 wave, 61 percent of the original sample was interviewed.

In the NLSY79, the initial sample is representative of the national population between 14 and 21 as of the end of 1978 that is not permanently institutionalized. It therefore includes members of the armed forces, including those living on a military base. The sample also includes youth who are in school and living in a dormitory or who are incarcerated. The sampling weights are intended to make the data representative of the national population (including, it appears, the permanently institutionalized) between the relevant ages as of the time the sample was originally drawn. Since my analyses focus on adolescents who were living at home at the start of the survey, the broader scope of the NLSY79 (including non-civilians) is unlikely to create important inconsistencies in the estimates from the three cohorts.

Unlike in the NLSYM and NLSYW, NLSY79 original sample members were all followed subsequently (except when parts of the sample were dropped en masse).[132] As of the 1994 wave, 89 percent of the original respondents eligible to participate continued to do so.[133]

The NLSY97 is representative of the national population between 12 and 16 years old as of the end of 1996 that is not permanently institutionalized. In this regard, it is like the NLSY79, except that because sample members are not old enough to join the armed forces, the distinction between the civilian population and the national population is irrelevant. The sampling weights make the data representative of the civilian non-institutionalized population living in households between the relevant ages as of the time the sample was drawn.[134] The small number of institutionalized people who were adolescents no older than 16 means the distinction between this universe and that for the NLSY79 (including the institutionalized), is unimportant.

As in the NLSY79, but not the NLSYM or NLSYW, original sample members were all followed subsequently. As of the 2015–16 wave, 79 percent of the original respondents continued to participate.[135]

SAMPLE ATTRITION

The information cited above on continued participation in the NLS surveys applies to the entire sample. When focusing on the sub-samples in my analyses, it does not appear that different attrition rates over time should unduly affect my results. In the NLSYM, 65 percent of the men in my sample were still participating by 1981, as were 72 percent of NLSYW women in 1985. In the NLSY79, the figures were 78 percent for both men in 1994 and women in 1996, and in the NLSY97, 77 percent of men and 82 percent of women were participating in 2015.

Therefore, attrition was somewhat higher for men and women in the earliest of the three cohorts. Research suggests that attrition was somewhat worse for NLSYM respondents from more disadvantaged socioeconomic categories.[136] Similarly, in the NLSYW, poorer, less-educated, and non-white sample members were more likely to fall out of the sample over time.[137] This differential attrition on the part of disadvantaged men and women corresponds with evidence from the PSID. Research has found that such differential sample attrition makes mobility look higher than it really is.[138] However, because the NLSY79 and NLSY97 also likely suffer from differential attrition, and because the differences across the surveys in overall attrition rates are not large, it is unlikely that the conclusions drawn in the paper are unduly affected.

MISSING AND NON-POSITIVE EARNINGS AND INCOME

The completeness of income reporting varies across the samples and potentially could affect the comparability of the estimates. Income data can be missing because parents or children do not answer all of the relevant questions or because they drop out of the survey entirely. Non-positive income can be an issue especially for earnings measurement, because non-workers' earnings are not observed, but they would be non-zero if the person were employed. If the number of non-workers changes over time, that can affect the interpretation of the results. However, these issues do not seem to be important, based on the data and several sensitivity checks.

In the NLSYM, 43 percent of the weighted potential sample (still participating when sons' incomes are assessed in 1981, in the right age range, and not enrolled in school) is missing parental income, while fewer than 1 percent has negative or no income. The large number with missing data stems from the problems matching NLSYM sons to their parents in the NLSOM and NLSMW surveys. After experimenting with a correction for this (see the next section), I concluded that this potential issue does not meaningfully affect my results. The data is more complete for sons' own earnings (7 percent missing, 4 percent non-positive) and family income (less than 1 percent missing, 4 percent non-positive).

In the NLSY79, 19 percent of weighted sons in the potential sample are missing parental family income, while less than 1 percent have non-positive parental income. However, the missing data does not appear to bias the estimates significantly. During the screening of potential sample members for the NLSY79, which occurred primarily between September and December of 1978, civilians were also asked a single question about family income in the past 12 months.[139] When I substituted this family income information where parental income from the 1979 interview was unavailable, just 10 percent of sons had missing parental income, and the IGE estimates were not much affected. Similarly, the family income reported by grown sons was often missing (18 percent, with 1 percent of incomes non-positive). But when I substituted missing values for 1993 income with reported income in 1992 or 1991, just 5 percent of incomes were missing, and the IGE estimates were very similar. Sons' earnings are well-reported, with 4 percent missing and 6 percent non-positive.

The NLSY97 also suffers from missing income problems. Fully 24 percent of sons potentially in the weighted sample are missing parental household income (while less than 1 percent have non-positive income). However, when I substituted for missing 1996 income the income reported for 1997 or 1998, just 3 percent were missing parental income, and the IGE estimate did not change much. Some 10 percent of sons have missing family income as adults (and 4 percent have non-positive income). When I replaced missing 2014 values with reported 2012 income, just 4 percent of sons had missing data, and the IGE estimates were similar. Finally, 10 percent of sons were missing earnings data, and 12 percent had non-positive earnings. When I replace missing 2014 earnings with reported 2012 earnings, and when I replace non-positive 2014 earnings with 2012 earnings (if positive), those percentages change to 4 percent and 8 percent, and the IGE estimates are not affected.

Turning to daughters, 39 percent of the potential sample members in the NLSYW analyses have missing parent income data, due to the necessity of matching them to the NLSOM and NLSMW. Just 1 percent of those who are matched have non-positive parent family income. Only four percent of potential sample members have missing earnings data. However, 21 percent have non-positive earnings data, reflecting the relatively large number of non-workers among the daughters. For daughters' own family income, there are no missing values, and 6 percent are non-positive.

In the NLSY79, 19 percent of daughters have missing parent income data, and less than 1 percent have non-positive parent income; 4 percent have missing earnings, and 19 percent have non-positive earnings; while 18 percent having missing family income, and 1 percent have non-positive family income.

In the NLSY97, 24 percent of daughters have missing parent income but less than 1 percent have non-positive parent income; 11 percent have missing earnings, and 22 percent have non-positive earnings; 10 percent have missing family income, and 4 percent have non-positive family income.

RESTRICTION OF RANGE IN THE EARLY COHORTS

As noted above, in the section on the NLSYM and NLSYW, the parent-reported income measure used for those cohorts, taken from the parallel NLSOM and NLSMW surveys after linking children to parents, suffers from restriction of range. Children with the youngest fathers and the oldest mothers are less likely to be matched to their parents. Only two-thirds of children may be matched, and this restricted range could affect the association of parent-reported income with adult earnings. That would make the NLSYM and NLSYW estimates less consistent with the NLSY79 and NLSY97 estimates.

To address this issue, I experimented with implementing a reliability correction to the IGE and IRA estimates for the NLSYM cohorts, using son-reported incomes.[140] Doing so produced meaningfully lower estimates of mobility for the NLSYM and NLSYW cohorts.[141] However, I chose not to highlight these results, primarily because there is also some degree of measurement error in the parental income measures in the NLSY79 and NLSY97. It is possible to implement a reliability correction for the NLSY79 estimates, and when I did so, the trend between the NLSYM and NLSY79 was not affected.[142] Given the assumptions embedded in the reliability correction, and given that the trend in mobility seems not to be affected, I do not report these estimates. I conclude that the restriction of range in the early cohort measures of income reported by parents does not bias the trends I report in the paper.

INCOME MEASURES

Income is measured similarly across the surveys. All family income measures aggregate incomes from a variety of sources, with one exception: in the NLSY79, parental income is reported as a single amount (after asking about receipt of income from a variety of sources).

In the NLSY97, it is the household income of parents rather than family income that is reported, but when I estimated a family income measure among only those with a spouse or who were living without a cohabiting partner, the results were not much affected. Similarly, when I looked at the association between logged combined earnings of parents (or their cohabiting partners) and grown child income, mobility looked similar after excluding cohabiter earnings. Also, the family income of grown children includes the income of unmarried partners, whereas that income is excluded in previous surveys.

In the NLSYM, farm income cannot be separated between spouses, so I exclude it from my earnings measure. "Earnings" in all other years include business and farm income.

BUSINESS CYCLE CONSIDERATIONS

Ideally, parental income and the incomes of grown children would be measured at the same point in the business cycle for all three cohorts. The concern is that if either generation's income is measured during an unusually strong or weak economy, then that could affect the mobility level. If this is true in one year but not others, the trend in mobility might simply reflect non-comparable years rather than the true underlying change in mobility. This problem is lessened to the extent that multiple years of incomes may be averaged, but that is impractical using the NLS data if consistency across surveys is to be maximized.

In the early cohorts, parental family income is measured in 1966, when the unemployment rate was exceptionally low—3.8 percent. When grown sons' incomes were assessed, in 1981, unemployment was elevated (7.6 percent). In 1985, when grown daughters' incomes were assessed, it was still 7.2 percent. That means that between the two generations, the unemployment rate rose 3.4 to 3.8 points.

That stands in contrast to the NLSY79 and NLSY97 samples. In the former, mobility measures for sons compare incomes in 1978 and 1993, a period during which the unemployment rate rose only from 6.1 percent to 6.9 percent (0.8 points). For daughters, it fell from 6.1 to 5.6 percent between 1978 and 1995 (a fall of 0.5 points). In the NLSY97, the unemployment rate was 5.4 percent in 1996 and 6.2 percent in 2014, increasing 0.8 points for both men and women.

The early cohorts, then, might be less comparable to the NLSY79 and NLSY97 cohorts because parental income is measured in a year with lower unemployment than in the later surveys, because grown child income is measured in a year with higher unemployment than in the later surveys, or for both reasons. However, it is unclear how large we might expect this bias to be or in what direction. The strength of the business cycle affects who has any earnings or income, who enrolls in school, and the wages that workers command. Economic conditions also are likely to affect the children of richer and poorer parents differently—both those experienced as children and those experienced as grown adults. Furthermore, experience with past business cycle highs and lows is likely to have lingering effects on income, so even someone in a strong economy may have seen their mobility affected by living through a weak economy.

In the section on absolute upward mobility, I demonstrate that sons' mobility in the NLSYM is not much different if parents and sons with incomes of $0 or less are excluded. The fact that my IRA trends (including those using earnings as the outcome) are also robust to excluding non-positive incomes is another indication that business cycle effects are unlikely to bias my trend estimates significantly.

INTERVIEW METHODS

Interviews of sample members across all the surveys, in the years analyzed here, were mostly done in person. The exception is that respondents to the NLSYW were interviewed over the phone in 1985.

Detailed Estimates | INCOME RANK ASSOCIATIONS (FIGURES 1 AND 7)

PSID		1952-59	1960-67	1968-75	1976-83
Father vs. Son Earnings n=1,664	Point Estimate	0.355	0.310	0.324	0.323
	Coefficient	–	-0.046	-0.032	-0.032
	Standard Error	(0.046)	(0.067)	(0.078)	(0.089)
Parental Family Income vs. Son Earnings n=2,648	Point Estimate	0.417	0.307	0.446	0.464
	Coefficient	–	-0.110	0.029	0.047
	Standard Error	(0.038)	(0.058)	(0.062)	(0.059)
Parental Family Income vs. Son Family Income n=2,897	Point Estimate	0.489	0.334	0.479	0.547
	Coefficient	–	-0.154	-0.010	0.058
	Standard Error	(0.034)	(0.053)	(0.058)	(0.053)
Father vs. Daughter Earnings n=1,986	Point Estimate	0.099	0.243	0.158	0.212
	Coefficient	–	0.144	0.059	0.113
	Standard Error	(0.044)	(0.065)	(0.083)	(0.082)
Mother vs. Daughter Earnings n=2,491	Point Estimate	0.060	0.131	0.109	0.191
	Coefficient	–	0.071	0.049	0.131
	Standard Error	(0.036)	(0.055)	(0.070)	(0.083)
Parental Family Income vs. Daughter Earnings n=2,899	Point Estimate	0.142	0.238	0.294	0.329
	Coefficient	–	0.096	0.152	0.187
	Standard Error	(0.041)	(0.058)	(0.067)	(0.066)
Parental Family Income vs. Daughter Family Income n=3,027	Point Estimate	0.358	0.402	0.428	0.499
	Coefficient	–	0.044	0.070	0.140
	Standard Error	(0.038)	(0.052)	(0.060)	(0.056)

NLS		1949-51	1961-63	1982-84
Parental Family Income vs. Son Earnings	Point Estimate	0.237	0.338	0.298
	Standard Error	(0.042)	(0.031)	(0.028)
	Observations	669	1,352	1,353
Parental Family Income vs. Son Family Income	Point Estimate	0.235	0.328	0.314
	Standard Error	(0.039)	(0.034)	(0.027)
	Observations	680	1,078	1,352

NLS		1951-53	1962-64	1981-83
Parental Family Income vs. Daughter Earnings	Point Estimate	0.217	0.161	0.326
	Standard Error	(0.045)	(0.038)	(0.033)
	Observations	605	1,239	1,302
Parental Family Income vs. Daughter Family Income	Point Estimate	0.291	0.309	0.394
	Standard Error	(0.039)	(0.035)	(0.027)
	Observations	628	1,048	1,316

Notes: PSID coefficients indicate the change for a cohort relative to Cohort 1. Summing the Cohort 1 point estimate and a subsequent cohort's coefficient produces the subsequent cohort's point estimate. For subsequent cohorts, the standard error applies to the coefficient rather than to the point estimate

Detailed Estimates | TRANSITION PROBABILITIES (FIGURES 2–6, 8–13)—PSID

By cohort; column percentages, standard errors in parentheses, unweighted cell sizes)

	1952-59				1960-67				1968-75				1976-83			
Child Fourth	Parent Fourth				Parent Fourth				Parent Fourth				Parent Fourth			
	1	2	3	4	1	2	3	4	1	2	3	4	1	2	3	4
PSID—Father vs. Son Earnings																
1	40.3 (4.2) 114	26.1 (4) 52	15.7 (3.5) 20	17.5 (4) 17	32.9 (4.2) 94	25.6 (4.2) 36	27.7 (4.6) 31	15.7 (3.9) 16	39.8 (6.7) 29	35.8 (5.9) 29	8.4 (3.1) 10	17.4 (5.1) 10	48.8 (8) 33	21.0 (5.9) 16	17.2 (7.5) 9	20.3 (7.3) 7
2	33.4 (4.1) 66	24.1 (3.9) 41	25.9 (4.2) 33	14.7 (3.6) 15	36.3 (4.6) 60	29.3 (4.3) 45	19.7 (3.9) 23	13.7 (3.7) 13	26.1 (5.7) 22	26.1 (5.4) 23	34.8 (6.2) 24	14.7 (4.9) 10	24.8 (6.5) 17	22.8 (6.1) 16	29.1 (7.3) 14	22.9 (6.8) 10
3	21.0 (3.5) 44	30.1 (4.2) 46	32.1 (4.4) 39	17.7 (3.8) 19	19.7 (3.6) 39	30.5 (4.5) 41	27.3 (4.5) 28	22.8 (4.5) 23	22.2 (5.8) 14	21.1 (5.4) 14	30.3 (6.3) 20	22.9 (5.6) 15	22.5 (7.7) 7	33.9 (7.3) 17	23.5 (6.9) 11	25.9 (7.8) 9
4	5.3 (2) 14	19.7 (3.7) 26	26.4 (4.3) 29	50.2 (5.1) 50	11.1 (3) 15	14.6 (3.5) 17	25.3 (4.3) 27	47.8 (5.4) 41	12.0 (4.4) 8	17.0 (4.7) 13	26.5 (5.8) 19	45.1 (6.9) 27	4.0 (2.7) 3	22.4 (6.9) 11	30.1 (7.9) 11	30.9 (8) 12
PSID—Parental Family Income vs. Son Earnings																
1	44.2 (3.5) 221	26.9 (3.7) 57	11.3 (2.7) 21	17.8 (3.5) 26	34.4 (3.7) 150	24.4 (3.8) 52	22.6 (3.9) 35	22.1 (4) 26	44.0 (5.6) 66	24.9 (4.3) 33	21.9 (4.5) 23	9.9 (3.2) 10	53.5 (5.5) 92	21.5 (4.1) 43	25.4 (4.9) 30	9.8 (3.1) 11
2	27.4 (3.2) 101	36.7 (4) 71	25.6 (3.7) 41	11.9 (2.8) 17	39.6 (4.1) 93	27.6 (4) 48	17.2 (3.4) 30	16.0 (3.5) 19	27.4 (4.6) 45	31.0 (4.7) 39	24.9 (4.5) 32	15.6 (3.9) 17	23.4 (4.5) 43	36.3 (5.2) 49	23.3 (4.1) 33	15.3 (3.8) 19
3	21.8 (3) 72	24.3 (3.5) 44	26.8 (3.8) 42	27.0 (4) 35	13.5 (2.6) 47	34.3 (4.3) 55	28.2 (4.1) 43	22.1 (4) 27	22.2 (4.8) 28	21.6 (4.2) 29	26.3 (5) 24	30.3 (5.3) 27	20.5 (4.6) 26	23.4 (4.2) 29	27.5 (4.6) 30	27.6 (4.8) 27
4	6.6 (1.9) 21	12.1 (2.7) 22	36.3 (4.1) 53	43.3 (4.5) 54	12.5 (2.9) 19	13.8 (3) 24	32.0 (4.4) 39	39.8 (4.8) 43	6.4 (2.6) 8	22.5 (4.2) 27	26.9 (4.8) 26	44.2 (5.6) 40	2.6 (1.4) 6	18.8 (4.3) 20	23.9 (4.4) 25	47.3 (5.3) 43
PSID—Parental Family Income vs. Son Family Income																
1	49.1 (3.5) 239	29.8 (3.7) 76	12.2 (2.6) 27	9.0 (2.6) 15	43.8 (3.9) 189	22.0 (3.4) 56	21.0 (3.7) 36	18.4 (3.6) 24	44.6 (5.4) 84	25.6 (4.3) 36	22.1 (4.4) 26	6.2 (2.5) 7	52.8 (5.1) 105	26.4 (4.5) 53	16.9 (3.6) 32	8.6 (2.8) 11
2	28.1 (3.1) 116	29.5 (3.6) 63	25.8 (3.7) 40	15.4 (3.2) 21	31.8 (3.7) 106	25.9 (3.6) 57	23.7 (3.7) 44	16.4 (3.5) 24	32.2 (5.2) 46	32.0 (4.6) 42	22.2 (4.3) 27	16.2 (4.2) 14	31.8 (4.7) 63	34.2 (4.8) 55	23.5 (4) 32	10.9 (3.1) 15
3	16.8 (2.7) 57	26.2 (3.5) 52	25.2 (3.6) 41	32.0 (4.1) 45	16.3 (2.9) 42	29.4 (3.9) 51	23.2 (3.8) 31	28.1 (4.1) 36	17.2 (3.8) 27	23.0 (4.3) 29	25.4 (4.5) 29	33.9 (5.3) 34	9.8 (3.2) 15	27.3 (4.6) 30	33.9 (4.7) 42	29.0 (4.6) 32
4	6.0 (1.7) 28	14.5 (2.8) 30	36.8 (4) 60	43.6 (4.4) 57	8.1 (2.4) 13	22.7 (3.6) 37	32.1 (4.2) 44	37.1 (4.5) 46	6.0 (2.6) 7	19.4 (4.2) 23	30.4 (5.1) 28	43.7 (5.5) 41	5.7 (2.3) 10	12.1 (3.6) 15	25.6 (4.4) 31	51.5 (5.1) 53
PSID—Father vs. Daughter Earnings																
1	25.9 (3.5) 80	24.5 (3.7) 44	22.9 (3.7) 32	30.2 (4.3) 36	34.5 (4.1) 94	18.9 (3.6) 34	22.4 (3.8) 34	27.4 (4.4) 31	37.3 (6.4) 36	15.9 (4.8) 12	31.6 (6.6) 17	18.0 (5.9) 8	27.8 (5.7) 26	36.3 (6.7) 23	18.9 (5.3) 13	23.0 (5.9) 12
2	30.2 (4.3) 36	23.3 (3.6) 40	22.5 (3.8) 28	21.9 (3.9) 26	27.7 (3.8) 84	34.3 (4.5) 53	24.7 (4) 33	12.8 (3.3) 13	30.2 (6.2) 29	23.1 (5.2) 24	26.2 (6.3) 16	18.4 (5.9) 8	33.7 (6.5) 37	31.8 (6.8) 19	19.6 (5) 15	13.4 (4.9) 7
3	25.8 (3.4) 92	29.9 (3.9) 60	24.9 (3.8) 35	18.9 (3.7) 22	29.2 (4) 64	26.5 (4.3) 41	26.7 (4.1) 37	17.3 (3.8) 18	16.7 (4.9) 23	32.6 (6.5) 19	18.4 (5.1) 15	32.0 (7.2) 15	22.0 (5.3) 18	20.7 (5.1) 22	31.9 (6.5) 20	25.1 (6.1) 15
4	15.8 (2.9) 45	22.4 (3.7) 37	29.8 (4.1) 44	29.0 (4.2) 35	8.6 (2.4) 18	20.4 (4) 28	26.2 (4.1) 37	42.5 (5) 44	15.8 (5.3) 12	28.4 (6) 19	23.8 (6.2) 13	31.7 (7.3) 13	16.6 (5.1) 16	11.2 (4.2) 8	29.7 (6.3) 21	38.5 (7.1) 20

Continued on next page

Child Fourth	1952-59				1960-67				1968-75				1976-83			
	Parent Fourth				Parent Fourth				Parent Fourth				Parent Fourth			
	1	2	3	4	1	2	3	4	1	2	3	4	1	2	3	4
PSID—Mother vs. Daughter Earnings																
1	22.2 (3.1) 67	32.9 (3.9) 72	22.8 (3.3) 65	18.5 (3.2) 39	30.3 (3.6) 92	30.9 (4) 73	22.9 (3.4) 61	17.3 (3.3) 35	28.8 (5.3) 42	23.7 (5.2) 21	34.8 (5.8) 32	17.6 (4.8) 12	33.1 (7.2) 29	22.1 (6.5) 18	24.5 (7.3) 12	8.3 (4.3)
2	25.4 (3.3) 60	23.8 (3.6) 51	31.2 (3.8) 84	17.7 (3.4) 40	22.5 (3.4) 59	27.2 (3.9) 56	26.3 (3.6) 66	19.6 (3.7) 34	24.6 (5.1) 38	32.5 (5.6) 34	20.7 (4.7) 26	17.6 (4.9) 16	17.1 (5.6) 23	37.4 (8.1) 28	20.9 (5.5) 18	40.4 (8.9) 16
3	23.2 (3.2) 59	26.9 (3.7) 63	24.4 (3.4) 75	30.7 (3.9) 75	27.1 (3.7) 61	18.8 (3.4) 47	30.3 (3.9) 65	26.8 (4.1) 48	22.6 (5.1) 27	21.2 (4.7) 26	25.4 (4.9) 34	33.0 (6) 26	40.2 (7.8) 21	17.6 (6.8) 10	29.2 (6.9) 18	10.1 (5.2) 7
4	29.2 (3.5) 60	16.4 (3.1) 35	21.7 (3.3) 57	33.1 (4) 62	20.2 (3.4) 37	23.1 (3.7) 35	20.5 (3.4) 41	36.4 (4.5) 53	24.0 (5.6) 14	22.7 (5.1) 18	19.2 (4.7) 17	31.9 (5.7) 29	9.6 (4.6) 5	23.0 (6.5) 13	25.3 (6.7) 13	41.2 (8.8) 17
PSID—Parental Family Income vs. Daughter Earnings																
1	27.4 (3.1) 120	21.5 (3.3) 47	28.5 (3.8) 45	21.2 (3.6) 28	28.5 (3.4) 129	25.5 (3.7) 53	28.6 (4) 48	19.4 (3.5) 29	38.8 (5.1) 68	25.7 (4.6) 31	18.9 (4.2) 19	21.7 (4.8) 17	38.1 (5.2) 73	28.3 (4.6) 32	20.4 (4) 25	14.3 (3.7) 14
2	29.7 (3.3) 126	27.5 (3.7) 55	18.9 (3.3) 27	20.7 (3.7) 27	30.3 (3.6) 104	26.9 (3.8) 50	20.4 (3.5) 36	17.5 (3.6) 25	29.3 (4.8) 63	37.7 (5.1) 45	20.5 (4.4) 25	9.7 (3.5) 8	29.3 (5.1) 68	31.8 (4.7) 52	30.7 (4.7) 34	13.0 (3.6) 13
3	25.9 (3) 127	28.0 (3.6) 70	23.0 (3.5) 40	22.6 (3.8) 29	31.1 (3.6) 101	32.4 (4.1) 56	21.2 (3.6) 39	17.3 (3.6) 21	20.8 (4.2) 52	24.7 (4.6) 33	24.8 (4.5) 28	32.2 (5.4) 30	19.9 (4.1) 33	26.8 (4.4) 40	23.4 (4.1) 33	27.4 (4.7) 31
4	17.0 (2.6) 69	23.0 (3.5) 51	29.7 (3.9) 48	35.5 (4.2) 48	10.0 (2.2) 36	15.2 (3.1) 30	29.8 (4.2) 43	45.8 (4.6) 58	11.1 (3.4) 16	12.0 (3.5) 14	35.8 (5.2) 39	36.5 (5.7) 29	12.8 (4) 24	13.2 (3.4) 18	25.6 (4.4) 32	45.3 (5.3) 45
PSID—Parental Family Income vs. Daughter Family Income																
1	44.0 (3.4) 243	25.7 (3.5) 64	14.3 (3) 24	11.2 (3.2) 12	40.0 (3.6) 207	28.2 (3.7) 70	15.9 (3) 38	10.1 (2.8) 17	53.9 (5.2) 131	27.2 (4.6) 44	15.3 (4) 19	8.4 (3.3) 7	52.0 (5.3) 121	29.4 (4.4) 56	20.3 (4.2) 26	9.3 (3) 11
2	25.5 (3.1) 111	25.9 (3.5) 67	26.0 (3.6) 44	22.1 (3.6) 32	30.8 (3.6) 98	25.1 (3.6) 54	26.8 (4) 44	16.0 (3.3) 24	20.3 (4) 40	26.5 (4.6) 34	24.1 (4.5) 26	27.0 (5.1) 24	30.5 (5.2) 52	20.0 (4.1) 30	20.9 (3.9) 33	18.2 (4) 19
3	18.6 (2.7) 72	28.3 (3.6) 60	29.3 (3.8) 46	25.0 (3.7) 35	21.5 (3.2) 62	28.7 (3.9) 50	24.9 (3.8) 38	30.2 (4.2) 39	20.3 (4.2) 34	34.2 (5) 38	23.9 (4.4) 29	24.5 (4.8) 24	13.4 (3.4) 28	31.2 (4.6) 39	33.8 (4.7) 39	25.0 (4.5) 27
4	11.9 (2.2) 38	20.1 (3.1) 45	30.4 (3.8) 51	41.8 (4.4) 56	7.7 (2.1) 20	18.0 (3.3) 27	32.3 (4.1) 50	43.7 (4.5) 58	5.5 (2.6) 7	12.1 (3.4) 13	36.8 (5.2) 40	40.1 (5.7) 32	4.2 (1.9) 9	19.5 (4.2) 23	25.0 (4.4) 28	47.5 (5.2) 48

Detailed Estimates | TRANSITION PROBABILITIES (FIGURES 2–6, 8–13)—NLS

By cohort; column percentages, standard errors in parentheses, unweighted cell sizes)

Child Fourth	1949-51 Parent Fourth 1	2	3	4	1961-63 Parent Fourth 1	2	3	4	1982-84 Parent Fourth 1	2	3	4
NLS—Parental Family Income vs. Son Earnings												
1	40.81 (3.9) 101	19.27 (3.5) 34	19.03 (3.4) 29	25.23 (4) 35	39.92 (2.7) 243	25.08 (2.7) 101	19.18 (2.6) 60	13.71 (2.5) 32	39.5 (2.5) 212	28.38 (2.7) 100	20.34 (2.4) 64	16.49 (2.3) 47
2	26.03 (3.4) 63	35.29 (4.3) 5	15.85 (3.1) 25	21.13 (3.8) 28	30.17 (2.6) 143	32.02 (3) 110	27.91 (2.9) 79	19.8 (3) 40	30.19 (2.4) 135	28.88 (2.7) 92	19.4 (2.4) 58	22.9 (2.6) 61
3	20.55 (3.3) 41	27.3 (4) 38	33.36 (4.1) 47	25.91 (4) 36	16 (2.1) 75	27.06 (2.8) 86	29.71 (3) 81	26.72 (3.3) 54	18.2 (2) 79	23.19 (2.6) 69	27.68 (2.7) 82	25.37 (2.8) 65
4	12.61 (2.6) 28	18.14 (3.4) 27	31.77 (4) 47	27.73 (4) 38	13.91 (2.1) 56	15.84 (2.3) 48	23.19 (2.7) 70	39.77 (3.7) 74	12.11 (1.8) 49	19.55 (2.4) 57	32.58 (2.9) 91	35.24 (3) 92
NLS— Parental Family Income vs. Son Family Income												
1	34.55 (3.6) 99	24.35 (3.7) 41	19.22 (3.3) 31	17.59 (3.3) 27	41.51 (3.1) 193	28.51 (3.1) 92	20.34 (2.8) 54	12.82 (2.6) 26	38.85 (2.5) 201	30.09 (2.7) 107	16.35 (2.2) 52	15.07 (2.2) 43
2	29.04 (3.5) 68	30.59 (4.0) 50	21.57 (3.5) 35	20.31 (3.8) 28	28.09 (3.0) 100	27.44 (3.1) 78	30.24 (3.3) 68	19.49 (3.1) 36	28.14 (2.4) 126	28.37 (2.7) 88	26.99 (2.7) 81	20.26 (2.6) 51
3	20.47 (3.2) 44	25.39 (3.8) 38	26.45 (3.7) 41	30.37 (4.2) 45	17.82 (2.5) 67	28.94 (3.1) 78	24.97 (3.0) 61	21.07 (3.2) 40	23.11 (2.3) 96	22.34 (2.5) 75	29.02 (2.7) 89	24.42 (2.7) 67
4	15.95 (2.9) 36	19.68 (3.4) 31	32.76 (4.0) 48	31.74 (4.1) 42	12.58 (2.3) 39	15.11 (2.4) 43	24.45 (3.0) 61	46.62 (3.9) 77	9.9 (1.6) 40	19.21 (2.4) 59	27.64 (2.7) 75	40.24 (3.1) 102

Child Fourth	1951-53 Parent Fourth 1	2	3	4	1962-64 Parent Fourth 1	2	3	4	1981-83 Parent Fourth 1	2	3	4
NLS—Parental Family Income vs. Daughter Earnings												
1	28.04 (3.6) 63	31.7 (4.2) 41	26.8 (4.2) 31	20.93 (3.9) 24	33.34 (2.7) 171	24.09 (2.9) 78	19.13 (2.8) 44	27.24 (3.2) 56	36.95 (2.7) 154	26.79 (2.7) 85	21.24 (2.6) 60	18.92 (2.6) 43
2	32.45 (3.8) 77	26.51 (4.0) 37	18.64 (3.6) 23	18.66 (3.7) 22	26.46 (2.5) 143	27.29 (3.1) 73	28.77 (3.3) 63	16.61 (2.7) 36	34.24 (2.6) 155	24.91 (2.5) 86	23.75 (2.6) 70	19.58 (2.6) 48
3	22.94 (3.5) 47	23.69 (3.9) 33	31.54 (4.3) 42	20.81 (3.8) 27	24.95 (2.6) 113	24.99 (2.9) 79	24 (3.1) 57	23.32 (3.0) 54	18.87 (2.1) 83	29.34 (2.7) 107	29.17 (2.8) 83	22.15 (2.7) 54
4	16.58 (3.0) 36	18.09 (3.5) 24	23.02 (3.8) 32	39.6 (4.7) 46	15.25 (2.2) 69	23.63 (2.9) 73	28.1 (3.3) 62	32.84 (3.5) 68	9.94 (1.7) 41	18.96 (2.4) 60	25.84 (2.7) 73	39.35 (3.2) 100
NLS—Parental Family Income vs. Daughter Family Income												
1	37.69 (3.8) 102	27.33 (4.1) 38	19.2 (3.6) 25	14.47 (3.2) 20	42.32 (3.1) 206	25.69 (3.2) 72	18.89 (3.1) 39	15.23 (2.7) 31	48.98 (2.7) 231	24.49 (2.5) 89	19.09 (2.4) 62	10.71 (2.1) 26
2	23.46 (3.3) 57	23.12 (3.7) 35	24.45 (4.0) 31	22.37 (3.9) 27	28.17 (2.9) 111	30.12 (3.4) 76	22.59 (3.3) 46	19.51 (3.0) 39	24.83 (2.4) 99	26.02 (2.6) 93	27.35 (2.7) 84	23.99 (2.8) 58
3	28.02 (3.7) 52	31.24 (4.1) 44	24.5 (3.9) 34	20.79 (3.8) 25	18.56 (2.7) 54	24.38 (3.1) 68	32.44 (3.7) 59	25.72 (3.4) 50	15.18 (2.0) 64	26.63 (2.7) 85	30.64 (2.8) 89	25.8 (2.9) 63
4	10.83 (2.7) 20	18.31 (3.4) 26	31.85 (4.2) 41	42.37 (4.6) 51	10.95 (2.3) 33	19.81 (3.0) 44	26.09 (3.5) 51	39.55 (3.8) 69	11 (1.7) 45	22.86 (2.5) 70	22.92 (2.6) 63	39.49 (3.2) 95

Detailed Estimates | INTERGENERATIONAL ELASTICITIES (FIGURES 14 AND 15)

PSID		1952-59	1960-67	1968-75	1976-83
Father vs. Son Earnings n=1,734	Point Estimate	0.319	0.300	0.211	0.306
	Coefficient	–	-0.019	-0.108	-0.013
	Standard Error	(0.050)	(0.069)	(0.094)	(0.079)
Parental Family Income vs. Son Earnings n=2,797	Point Estimate	0.513	0.359	0.412	0.489
	Coefficient	–	-0.154	-0.101	-0.024
	Standard Error	-0.024	(0.075)	(0.09)	(0.083)
Parental Family Income vs. Son Family Income n=3,179	Point Estimate	0.565	0.399	0.399	0.482
	Coefficient	–	-0.166	-0.135	-0.084
	Standard Error	(0.040)	(0.061)	(0.074)	(0.057)
Father vs. Daughter Earnings n=1,792	Point Estimate	0.090	0.309	0.199	0.270
	Coefficient	–	0.219	0.109	0.181
	Standard Error	(0.078)	(0.112)	(0.107)	(0.122)
Mother vs. Daughter Earnings n=2,188	Point Estimate	0.080	0.061	0.059	0.119
	Coefficient	–	-0.019	-0.022	0.039
	Standard Error	(0.035)	(0.050)	(0.059)	(0.061)
Parental Family Income vs. Daughter Earnings n=3,052	Point Estimate	0.251	0.400	0.284	0.431
	Coefficient	–	0.149	0.033	0.180
	Standard Error	(0.082)	(0.110)	(0.098)	(0.099)
Parental Family Income vs. Daughter Family Income n=3,542	Point Estimate	0.462	0.520	0.414	0.492
	Coefficient	–	0.058	-0.048	0.030
	Standard Error	(0.050)	(0.066)	(0.064)	(0.062)

NLS		1949-51	1961-63	1982-84
Parental Family Income vs. Son Earnings	Point Estimate	0.241	0.317	0.237
	Standard Error	(0.045)	(0.037)	(0.028)
	Observations	635	1,230	1,125
Parental Family Income vs. Son Family Income	Point Estimate	0.205	0.326	0.265
	Standard Error	(0.037)	(0.040	(0.028)
	Observations	680	1,078	1,260

NLS		1951-53	1962-64	1981-83
Parental Family Income vs. Daughter Earnings	Point Estimate	0.261	0.232	0.262
	Standard Error	(0.083)	(0.050)	(0.037)
	Observations	469	965	966
Parental Family Income vs. Daughter Family Income	Point Estimate	0.363	0.368	0.394
	Standard Error	(0.046)	(0.049)	(0.030)
	Observations	591	1,024	1,241

Notes: PSID coefficients indicate the change for a cohort relative to Cohort 1. Summing the Cohort 1 point estimate and a subsequent cohort's coefficient produces the subsequent cohort's point estimate. For subsequent cohorts, the standard error applies to the coefficient rather than to the point estimate.

Detailed Estimates | ABSOLUTE UPWARD MOBILITY (FIGURES 16–19)

PSID		1952-59	1960-67	1968-75	1976-83
Father vs. Son Earnings	Point Estimate	0.474	0.434	0.487	0.498
	Standard Error	(0.022)	(0.024)	(0.032)	(0.033)
	Observations	662	561	305	297
Parental Family Income vs. Son Family Income	Point Estimate	0.473	0.478	0.524	0.465
	Standard Error	(0.019)	(0.021)	(0.026)	(0.022)
	Observations	1,009	868	535	773
Father vs. Daughter Earnings	Point Estimate	0.165	0.189	0.259	0.252
	Standard Error	(0.016)	(0.018)	(0.030)	(0.028)
	Observations	752	671	294	325
Mother vs. Daughter Earnings	Point Estimate	0.707	0.680	0.694	0.617
	Standard Error	(0.018)	(0.020)	(0.027)	(0.028)
	Observations	1,031	896	471	514
Parental Family Income vs. Daughter Family Income	Point Estimate	0.495	0.432	0.528	0.497
	Standard Error	(0.019)	(0.020)	(0.026)	(0.021)
	Observations	1,105	970	607	861

NLS		1949-51	1961-63	1982-84
Parental Family Income vs. Son Family Income	Point Estimate	0.506	0.504	0.533
	Standard Error	(0.021)	(0.018)	(0.015)
	Observations	704	1,113	1,297
Parental Family Income vs. Daughter Family Income	Point Estimate	0.557	0.510	0.546
	Standard Error	(0.022)	(0.018)	(0.015)
	Observations	628	1,048	1,267

LONGER-TERM TRENDS IN ECONOMIC MOBILITY USING PARENT OCCUPATION TO IMPUTE INCOME AND RANK

Income data for nationally representative samples are not available further back in time than 1940.[143] Indeed, true longitudinal datasets that follow nationally representative groups of sons and daughters from their parental homes into adulthood only date to the mid-1960s (with the grown children then observed only years later). Researchers have gotten around these limitations through various ways of imputing parental income (and sometimes child income). This appendix describes the literature that does so using information on occupations. Appendix 4 provides an extended critique of one widely cited study of mobility trends that imputes parental income from information on birth states (Aaronson and Mazumder, 2008).

To estimate mobility in earlier years, researchers typically rely on decennial census data. Census data becomes public after 72 years, and it is currently possible to identify people by name in the 1850 through 1940 censuses.[144] Therefore, researchers can (in theory) link people between these censuses, observing them at, say, 35 years old in 1940 and 5 years old in 1910.

However, there is substantial error in matching names (originally handwritten by census takers) across censuses, especially since names are not unique and people move. Further, slaves are not identifiable by name in the 1850 and 1860 censuses, so analyses that rely on those censuses must exclude African Americans, relatively few of whom were free. Since women almost always took their husband's last name upon getting married, analyses are possible only for men (or single women). (The absence of family income measures also makes it difficult to analyze women's mobility in earlier years, since women's employment rates were so low.)[145]

Rather than attempt to link the same people in different censuses, a growing body of research leverages the fact that first and last names are correlated with socioeconomic status. Researchers create a measure of status by, for instance, ranking occupations. Then they calculate the average occupational status across all adults in a census with the same last name. Finally, they observe adults in a later census and assign them the average occupational status of parents in the earlier census who shared their last name. This imputation allows researchers to roughly estimate mobility measures that resemble those that would be computed if adults could be linked to their actual parents.[146]

Regardless of how generations are linked, there is the second problem that income measures are unavailable until 1940 (and only for wage and salary income in that year). To get around this issue, researchers have imputed income to parents and grown children using information from the 1950 census on the relationship between income on the one hand and, on the other hand, occupation and other variables that are available in earlier censuses.

Absolute Mobility

For instance, in one influential paper, Olivetti and Paserman (2015) consider mobility from 1870 to 1940. They observe the occupations of men in different censuses and assign them the median income received by people in the same occupation in 1950 ("occupational income"). They then impute father occupational incomes to men between the ages of 20 and 35 with a given first name. They do so by finding children ages 0 to 15 in the census 20 years earlier who have the same first name and averaging *their* fathers' "occupational incomes." That is, everyone who has the same first name in a given census gets the same "father occupational income." Finally, they compare men's occupational income to their imputed father occupational income.

Using a summary measure of absolute mobility (the "intergenerational elasticity," discussed in the paper, above), Olivetti and Paserman find that mobility among white men was relatively flat between 1870 and 1900, then fell substantially from 1900 to 1920 before rising more modestly through 1940.[147] In an online appendix, they look at men when they are 30 to 45 years old instead and find that mobility fell between 1880 and 1900, was unchanged between 1900 and 1910, declined between 1910 and 1930 and then was flat between 1930 and 1940.[148] Other approaches they try consistently find a large drop in mobility between 1900 and 1920 and lower mobility after 1920 than before 1900.

More recent papers have challenged the Olivetti and Paserman study. Choi, Gu, and Shen (2018) argue that the weak relationship between child first names and father occupational incomes results in overstated mobility estimates in the earlier paper. More importantly, they argue that the *varying* weakness of the relationship over time leads Olivetti and Paserman to estimate a biased trend in mobility. After correcting for the technical issues involved, Choi, Gu, and Shen report that mobility among white men fell by much less between 1900 and 1920 than implied by Olivetti and Paserman and rose by much more from 1920 to 1930. Whereas Olivetti and Paserman find mobility lower in 1930 than in the nineteenth century, Choi, Gu, and Shen find it higher.

Both of these studies use the same occupational income measure. Saavedra and Twinam (2020) instead allow imputed incomes to vary by not only by occupation, but by industry, sex, age, race, and state. They rely on a dataset that tries to directly match people in the 1880 census to other censuses. In contrast to Olivetti and Paserman, they find that the mobility of white men increased modestly from 1880 to 1900 and from 1880 to 1910. This difference appears due to the better income imputation in Saavedra and Twinam rather than the different way of producing intergenerational links, since they find mobility declines modestly when they use the Olivetti-Paserman imputation.

Ward (2020) imputes father earnings to men in each census using average 1940 earnings by occupation, race, and region and average 1960 self-employed earnings. Unlike the other studies, he reports his trends by birth cohort rather than by the year in which grown sons are observed. He finds relatively flat mobility among white men for cohorts born between 1840 and 1880, an increase from 1880 to 1900, then a decline from 1900 to 1920. Mobility in 1910 was higher than in the nineteenth century, but mobility in 1920 was lower.

Taken together, these studies suggest that absolute mobility among white men changed little between 1870 and 1910 (in the sense that changes in income between childhood and adulthood were no more likely in 1910 to have reduced childhood inequalities between sons than they had been in 1870). But the Ward estimates and the Choi, Gu, and Shen results suggest that intergenerational income changes were more equalizing around 1930 than around 1910 (mobility rose). Ward's results imply that they were less equalizing than ever before circa 1950. The Ward estimates indicate the fall in mobility was concentrated between the 1910 and 1920 birth cohorts (age 30 in 1940 and 1950).

Ward also provides trend estimates when white and black men are combined, by imputing occupational income to slaves in the earlier censuses. He finds that male mobility increased from the 1840 cohort to the 1860 cohort, fell by nearly as much between the 1860 and 1870 cohorts, changed little between the 1870 and 1910 cohorts, then fell between the 1910 and 1920 cohorts. Mobility for the men born in 1920 was lower than for men born in any cohort between 1840 and 1910, consistent with Ward's results for white men.

It is worth emphasizing that the mobility estimates that impute income to fathers by using average incomes among fathers in the same group (defined by their children's names or by their children's birthplace) should not be thought of as directly estimating the strength of the relationship between fathers' incomes and grown child incomes. When Olivetti and Paserman assign father income based on the first names of children and then look at mobility, they are considering how the income associated with child names is related to grown child income. That relationship is not the same as the relationship between father income and grown-child income.[149]

More importantly for this report, *trends* in these two different kinds of estimated mobility may look similar, but they may not. Olivetti and Paserman find a small decline in mobility from 1880 to 1900 when they impute father income with first names, a small decline when they use last names instead, and a sizable *increase* when they impute father income by computing father income averages within birth states (rather than within child name groupings). The mobility estimates they compute when linking individual people across censuses show no change between 1880 and 1900. If we want to know whether children who grow up poor become poor as adults, and whether that is truer at a certain point in time than in the past, then it is of only limited and indirect benefit to know whether children who have a name commonly chosen in poor families grow up to become poor adults (and whether that is truer than in the past).

The studies that attempt to directly link people to their fathers across censuses do not suffer from this problem (though the accuracy of the linking affects the mobility estimates, and so changes in the accuracy of the linking may also matter). But when parental income or grown-child income is unavailable, even these studies involve using group income averages rather than individual incomes.

Relative Mobility

One potential shortcoming of the studies using pre-1940 data is their reliance on an "occupational income" measure. These studies must assume that the 1950 incomes accruing to different occupations are equally good indicators of the incomes accruing to the same occupations in censuses farther away from and closer to 1950. To see how this issue could affect the validity of trend estimates, consider the sparse research on nineteenth and early twentieth century relative mobility trends. The Olivetti and Paserman paper includes trend estimates using the ranks of fathers and sons in terms of their occupational income. They found that mobility fell substantially between 1870 and 1920 and then rose back to its late-nineteenth century level by 1940.[150]

Rather than using occupational income tied to the 1950 census, Song et al. (2020) created occupational status measures by ranking occupations for each birth cohort across censuses, which allows the status associated with an occupation to change over time. Each census not only includes workers' occupation, but their education level or, prior to 1940, their literacy level. The researchers assign occupational status rankings using the distribution of educational attainment or literacy among workers within each occupation within each birth cohort. Occupations with more workers with higher education or literacy levels receive higher status scores. Song et al. then examine trends in the correlation between fathers' and sons' occupational status scores, after converting them to percentile ranks.

They find that mobility among white men fell from the 1830 birth cohort to the 1880 birth cohort, then was relatively flat through the 1980 birth cohort (possibly falling modestly after 1950 or 1960). Adding 30 years to these birth years to make the trend roughly comparable to the Olivetti and Paserman results (which are reported by the census year in which grown children are ages 20 to 35), mobility fell from 1860 to 1910 and then was flat through 2010. The 1920–1940 increase in mobility found by Olivetti and Paserman is absent in Song et al.

Ward (2021) further refines the Song et al. occupational status measure, giving everyone a ranking based not only on the typical educational attainment or literacy level of their occupation within their birth cohort, but also within their race and region. In other words, southerners and northerners with the same occupation can receive very different occupational status scores if the education levels within that occupation differ by region.

When Ward uses the same kind of measure as Song et al., directly linking men across censuses, he finds similar results—mobility for white men falling during the nineteenth century and then flattening out through the 1920 birth cohort. When he uses his more refined occupational status measure, however, mobility increases between the 1850 and 1880 cohorts, then falls by a smaller amount between the 1880 and 1910 cohorts. Similar estimates he constructs using the Panel Study of Income Dynamics (PSID) survey indicate mobility rates for the 1960 through 1980 cohorts that are higher than for any earlier cohorts except perhaps those born in 1880 or 1890. The Song et al. paper suggests that using linked Census Bureau data rather than the PSID would put post-1950 mobility at all-time highs.

Ward's approach allows him to include black men in his analyses, and the trend for the combined group of white and black men looks similar to the trend for whites. Though the *levels* of mobility across the entire period are lower than when analyzing whites alone, the post-1950 mobility rates are at unprecedented highs when whites and blacks are combined.[151] It appears, then, that sons' relative mobility has increased strongly over 140 years, with the rise concentrated between 1840 and 1880 and between 1910 and 1960.

A CRITIQUE OF AARONSON AND MAZUMDER (2008)

In their 2008 paper, Daniel Aaronson and Bhashkar Mazumder developed an innovative way to examine long-term economic mobility trends despite the problem of limited intergenerational datasets with income measures. Aaronson and Mazumder (AM) observed the wage and salary income of men in various decennial censuses and approximated their parental family income when they were children.

This approximation is necessary because there is no way to link, for example, 25- to 29-year-olds in the 1980 census to themselves or their parents in the 1960 census, when they were 5 to 9 years old.[152] However, the 1980 census does tell us the states in which adults were born, and because we know their ages, we know their birth years (in this example, 1951 to 1955). AM took the family income of boys ages 5 to 9 in the 1960 census and of boys 15 to 19 in the 1970 census (all of whom were born 1951 to 1955) and averaged them within states.[153] They then assigned to 25- to 29-year-olds in the 1980 census the average parental family income corresponding with their birth state.

Similarly, observing men 50 to 54 years old in 1980, AM looked back to boys ages 10 to 14 in the 1940 census. Other censuses were used to approximate parental family income for men between the ages of 30 and 49 in 1980. And parental family incomes were approximated for men 25 to 54 in other census years in the same way. In each instance, an observed adult's "parental family income" in childhood was the mean for observed children in their birth state and birth cohort, across one or more censuses.

AM then computed intergenerational elasticities, or IGEs, by pooling all of the men across census years and estimating models relating "parent" incomes and son earnings. Their models allowed IGEs to vary by the census year in which adult sons were observed, by the five-year cohort into which they were born, or by both. Davis and Mazumder (2020) presented results extended to 2010 by using data from the 2010 American Community Survey (ACS).

Here I offer a reanalysis of their approach to demonstrate what I view as its unreliability. My analyses extend further to 2019 using the ACS from that year. (I follow AM in referring to the year of each dataset, even though the decennial censuses ask about income and earnings received in the previous year—1939, 1949, etc.—while the ACS asks about income over the past 12 months, which encompasses parts of two calendar years for most respondents.)

Table A1 is a reproduction and update of AM's Figure 1, which tries to clarify this approach. In the table, five-year age groups are shown by birth cohort and by the year in which they are observed in the census or ACS data. The shaded cells show which age groups in which years are used to measure grown son earnings, and the non-shaded cells to their left show which birth cohorts in which census years are used to impute parent income for each of those age groups.

In the analyses below, for instance, 1960 grown-son earnings come from 25- to 29-year-olds, 30- to 34-year-olds, and 35- to 39-year-olds. The 25- to 29-year-olds are compared to parents of children observed in 1940 when between the ages of 5 and 9 or in 1950 when between the ages of 15 and 19. The 30- to 34-year-olds are compared to parents of children observed in 1940 when 10 to 14 years old, and the 35- to 39-year-olds are compared to parents of children observed in 1940 when 15 to 19 years old.

Note that this approach relies on incomplete data relative to what would be ideal. The ages of children (and, thus, of parents) when "parent" income is measured varies by census year. Only in 1970 and after are parent incomes imputed using children observed as young as age four or less. (To see this, observe the shaded 1960 cells and note that the cells to their left only include children as young as 5-9.) In earlier censuses, "parent" income is measured only at older child ages (15-19 for the 1950 adults, 5-19 for the 1960 adults), meaning it is assessed at older parent ages as well. And in those earlier censuses, "parent" income is assessed in fewer birth cohorts. Child age varies by birth cohort too. Only 15- to 19-year-olds are observed for the 1921-25 birth cohort, and only 10- to 14-year-olds

Table A1 | CENSUS BUREAU DATA SHOWN BY BIRTH COHORT AND CENSUS/ACS YEAR

Cohort	1940	1950	1960	1970	1980	1990	2000	2010	2019
1921-25	15-19	25-29	35-39	45-49					
1926-30	10-14		30-34	40-44	50-54				
1931-35	5-9	15-19	25-29	35-39	45-49				
1936-40	0-4	10-14		30-34	40-44	50-54			
1941-45		5-9	15-19	25-29	35-39	45-49			
1946-50		0-4	10-14		30-34	40-44	50-54		
1951-55			5-9	15-19	25-29	35-39	45-49		
1956-60			0-4	10-14		30-34	40-44	50-54	
1961-65				5-9	15-19	25-29	35-39	45-49	
1966-70				0-4	10-14		30-34	40-44	49-53
1971-75					5-9	15-19	25-29	35-39	44-48
1976-80					0-4	10-14		30-34	39-43
1981-85						5-9	15-19	25-29	34-38
1986-90						0-4	10-14		29-33
1991-95							5-9	15-19	25-28

Notes: Adults in the 2019 ACS are between the ages of 25 and 28 rather than 24 and 28 and thus reflect the 1991–94 birth cohorts rather than the 1991–95 cohorts. This is to keep the minimum age of adults consistent across samples at age 25. Observing adults at lower ages becomes more complicated due to school enrollment. The children used to impute parental income to these grown adults (observed in 2000 and 2010) do reflect the 1991–95 birth cohorts.

for the 1926-30 cohort. Subsequently, children are observed either from ages 5 to 9 or 15 to 19 for seven cohorts, or from ages 0 to 4 or 10 to 14 for six cohorts.

Only in 1980 and after are grown-son earnings reflected by adults observed as young as 25 and as old as 54. In earlier censuses, the adults were observed at younger ages, and, again, reflecting fewer birth cohorts. Only in 1980 and after are there six separate birth cohorts from which parent income is imputed. Only in 1990 and after are there 12 separate birth-cohort-by-census cells from which parent income is imputed. In 1980 there are 11 cells available, in 1970 there are eight, in 1960 there are four, and in 1950 there is one.

The incomplete data creates a few potential problems for the AM analyses. AM display mobility estimates for 40-year-old men, corresponding to the 1910 birth cohort for adults observed in 1950 and the 1920 cohort for adults observed in 1960. But it is clear from Table A1 that no "parent" data is available for those birth cohorts. This means that the AM estimates for 40-year-olds rely on extrapolation from their data. They essentially treat members of pre-1921 cohorts as if they were born between 1921 and 1925.[154]

A second issue is that "parent" income is measured for younger or older children—and thus, for younger or older parents—depending on the birth cohort and the census year in which adults are observed. For adults in 1950, the parents are all of children between the ages of 15 and 19 years old. For adults in 1990 through 2019, parents are equally likely to be observed regardless of child age. Since IGE estimates are known to be lower when parental income is measured at relatively younger or older ages (versus in the middle of the prime-age working years), this could affect measured mobility trends. The AM models do not control for the age at which parent incomes are measured or allow IGEs to vary depending on the age at which their incomes are measured.[155]

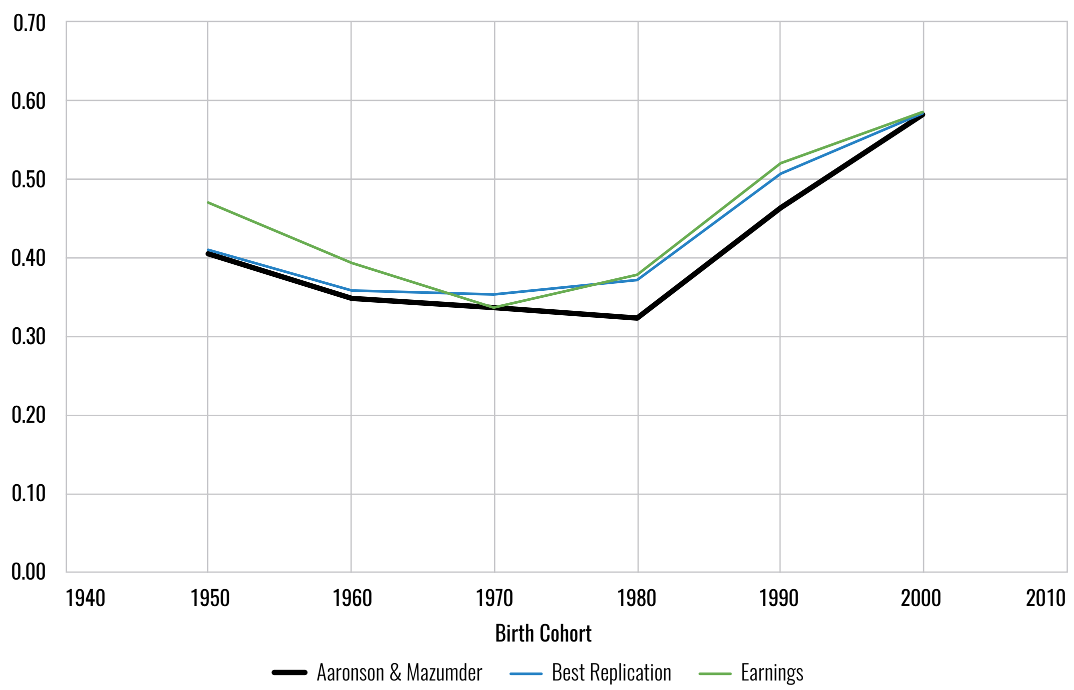
Another problem with the child-age variation is that some sons ages 15 to 19 no longer live at home, and that share is likely to vary over time. The "parent" income of children not living at home isn't parent income at all, especially after subtracting the son's own income, as I and AM do.

A third issue is that "parent" income is not only measured for different birth cohorts and at different child ages, but in different census years. This could introduce variation in mobility trends that is not due to cohort effects or the year in which adults are observed but to the year in which "parents" are observed. These "parent year" effects could reflect real factors common across birth cohorts and parent and child ages within a given census year. But they could also reflect measurement or survey administration differences across censuses that affect trend estimates artificially. In particular, the 1940 "parent" income measure is not a comprehensive measure of family income, but family wage and salary income. A family that relies on self-employment income will appear to have little—or even no—income by this 1940 measure. Indeed, while the fraction of "parents" with no "income" is consistently below 3.5 percent from 1950 to 2010, it is 23.5 percent in 1940. Differences in income measures across other years are smaller but conceivably important.[156]

More generally, the AM results are modeled, and the assumptions embedded in the modeling might be consequential for the estimates. As noted above, Olivetti and Paserman (2015) find a small decline in mobility from 1880 to 1900 when they impute father income with first or last names, but a sizable *increase* when they impute father income by computing father income averages within birth states (rather than within child name groupings). To explore the sensitivity of the AM estimates to these and other issues, I replicated their published results and then conducted a variety of checks to see how robust they were.[157] I conclude that their estimates are too sensitive to draw confident conclusions from them.

As a baseline, the black line in **Figure A1** displays the AM estimates from their model that allows mobility to vary by the census year in which adults are observed and by birth cohort.[158] The blue line indicates my best replication attempt.[159] My estimate is 0.01 higher than theirs in 1950, 1960, and 1970; 0.05 higher in 1980 and 1990; and the same as theirs in 2000. The trend is very similar to theirs, except that I show a small increase in the IGE during the 1970s while AM find a small decline. The green line in the chart switches from men's wage and salary income as the outcome to men's earnings (including self-employment earnings), which I use in subsequent analyses, though the choice of outcome makes little difference.[160]

Figure A1 | INTERGENERATIONAL ELASTICITY COMPARING PARENTAL INCOME TO SONS' EARNINGS, 1950-2000, 40-YEAR-OLD MEN

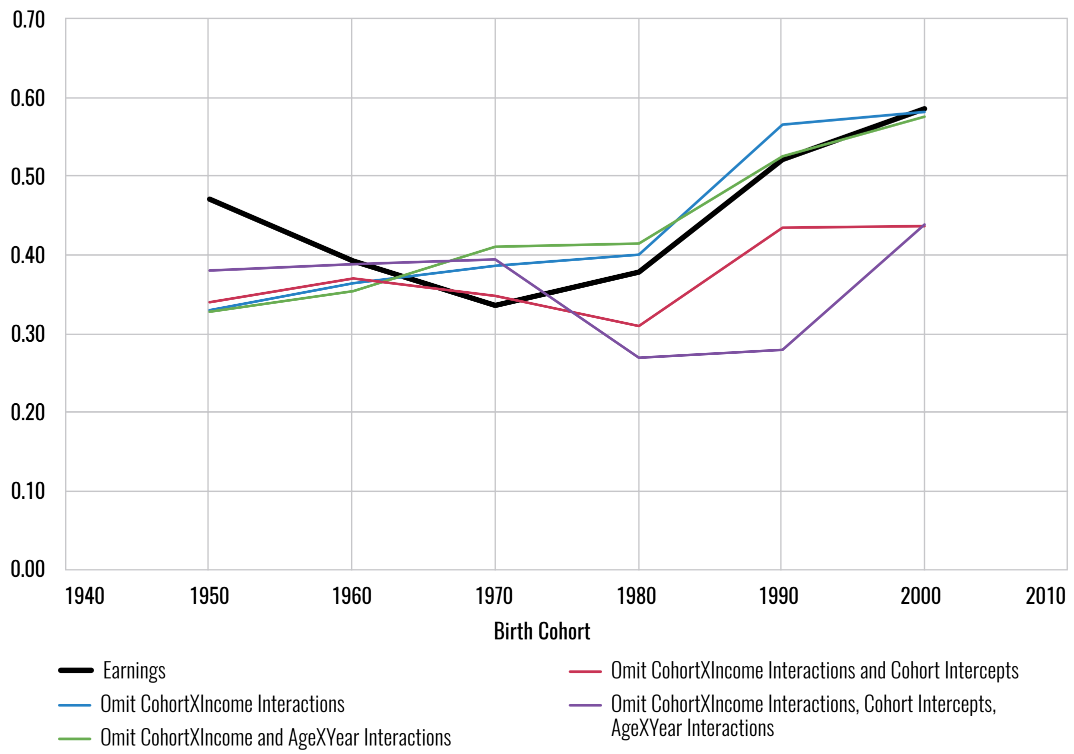


Notes: Adults in the 2019 ACS are between the ages of 25 and 28 rather than 24 and 28 and thus reflect the 1991–94 birth cohorts rather than the 1991–95 cohorts. This is to keep the minimum age of adults consistent across samples at age 25. Observing adults at lower ages becomes more complicated due to school enrollment. The children used to impute parental income to these grown adults (observed in 2000 and 2010) do reflect the 1991–95 birth cohorts.

Different ways of modeling the relationships between parental income, birth cohort, sons' ages, and census year can produce divergent trend estimates. The black line in **Figure A2** carries over the IGE estimates for earnings from Figure A1. Subsequent lines show estimates from models that omit certain terms from the baseline AM model. In this baseline model, adult earnings are predicted from a quartic in age, cohort dummies, year dummies (indicating the census year in which adults are observed), interactions of census year dummies with age, and interactions of parental income with age, cohort dummies, and census year dummies.

The second (blue) line excludes the terms interacting parental income with birth cohort dummies (but leaves the cohort dummies themselves in the model). This model assumes that the IGE can vary depending on the census year in which an adult son is observed and by the age at which an adult is observed, but not by his birth cohort. AM estimate this model themselves, and Davis and Mazumder update the results through 2010.[161]

Figure A2 | INTERGENERATIONAL ELASTICITY COMPARING PARENTAL INCOME TO SONS' EARNINGS, 1950-2000, 40-YEAR-OLD MEN



Using this model changes the 1950–1970 trend so that the IGE increases rather than declining. In the AM paper, this model also shows the 1970–1980 trend increasing, as I find here, despite the estimates declining over the 1970s in their model including the income-by-cohort interactions. The 1980–2000 trend is not much affected by excluding these interactions.

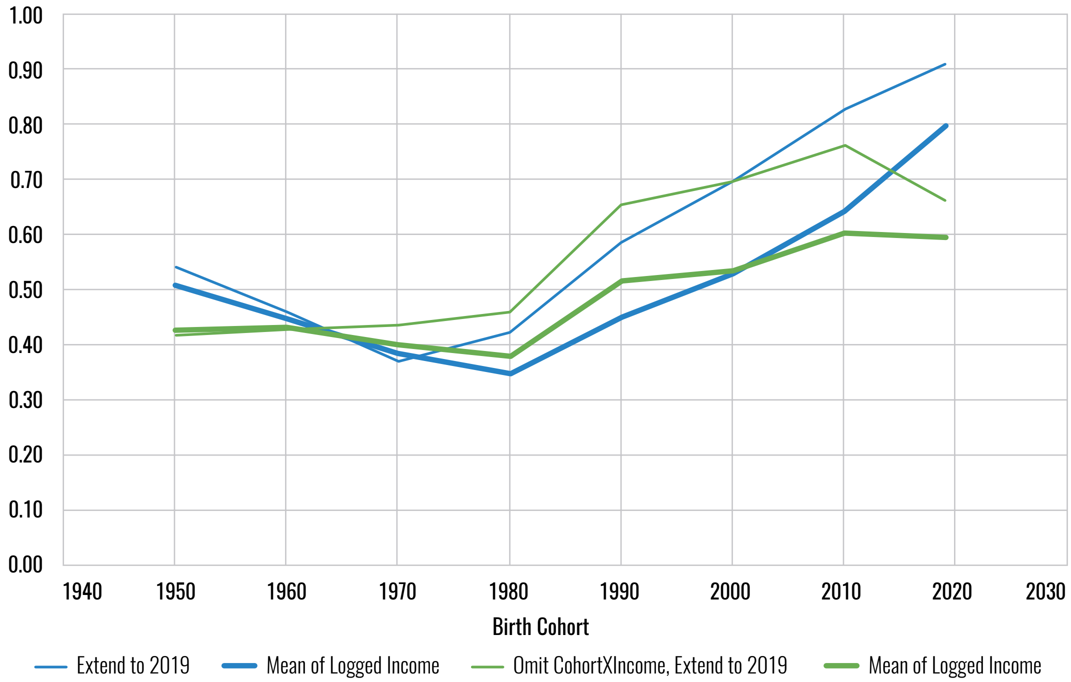
The third (green) line also excludes terms that interact the census year in which adults are observed with a quartic in age (that is, interactions with age, age squared, age cubed, and age to the fourth power).[162] This additional exclusion makes little difference, but that will not be the case below. Note that the model still allows the IGE to vary depending on a son's age in adulthood when his earnings are observed; it just does not allow the relationship between sons' age and their earnings to vary by census year.

Line four (in red) excludes the birth-cohort-by-parental-income interactions and the birth cohort dummy variables. The model still includes the age variables and age-by-income interactions, so the main difference from the baseline model is that all cohort effects are assumed to be zero while the AM model attempts to distinguish between cohort and age effects. Both are likely to be important in reality, but the particular way that AM model these effects need not capture well how they affect earnings and intergenerational mobility. These estimates show the IGE falling from 1960 to 1980, unlike the first three lines in the chart. The only striking instability in this trend line is the same large increase during the 1980s found in the other lines.

Finally, the last line in Figure A2 drops not only the cohort-by-income interactions and the cohort dummies but the age-by-year interactions. This time excluding the age-by-year interactions changes the trend again, with a large decline in the IGE in the 1970s and a large increase in the 1990s, but little change otherwise. Unlike the estimates produced by the other models, there is no large increase in the 1980s.[163]

Figure A3 provides a transition to subsequent analyses. The thinner lines extend the IGE trends to 2019, showing the baseline model with both year-by-income and cohort-by-income interactions and the model that omits cohort-by-income interactions. These differ from the AM results and from the estimates in Figures A1 and A2 in three ways. First, I combine smaller geographically adjacent states together when averaging "parent" income by birth state (and drop Alaska and Hawaii, which are not included in the data until 1960).[164] This has a minimal impact on the trend estimates. Second, when "parent" incomes are averaged within birth cohorts and birth states, children who no longer reside in their birth state when observed in the census are included.[165] This shifts the IGE levels from 1950 to 2000 upward and produces a modestly larger increase in the IGE. Third, the estimates use a larger 1960 sample than was available to AM and expand the 1970 sample by a factor of six, neither of which meaningfully affects the results.[166]

Figure A3 | INTERGENERATIONAL ELASTICITY COMPARING PARENTAL INCOME TO SONS' EARNINGS, 1950-2019, 40-YEAR-OLD MEN



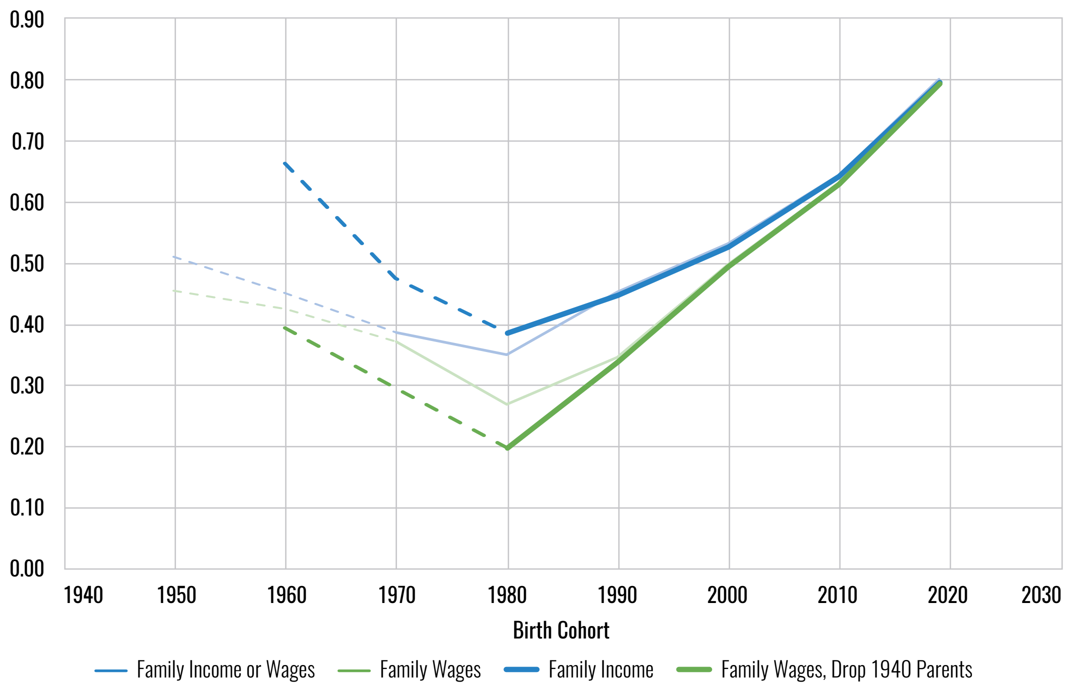
The IGE continues to increase during the 2000s, but the two different models show sharply divergent trends from 2010 to 2019 depending on whether or not cohort-by-income interactions are included. The IGE either continued to soar to new heights in 2019 or it fell to its lowest level in 30 years.

All of the estimates so far have followed AM in estimating "parent" income averages and then regressing logged son earnings on the log of these averaged incomes. However, in regression models the coefficient on the log of cell averages will in general be different than the coefficient on the log of the underlying individually measured incomes.

The coefficient on the cell averages of logged income, however, will equal the coefficient on the log of the individually measured incomes. The thick lines in Figure A3 indicate smaller changes in the IGE over time when it is estimated from the cell means of logged "parent" income rather than from the log of the cell means of "parent" income. The post-2010 divergence is still apparent. I will use the means of logged income from here forward.[167]

Figure A4 explores two other issues affecting the IGE estimates in the earlier years. As noted, the 1940 "parent" income measure includes only the wage and salary income of family members, making it potentially inconsistent with "parent" income in other years. The thin blue line in Figure A4 repeats the baseline trend from Figure A3 (this time partly dashed, to which I will return), which makes no distinction between "parent" family wage and salary income (in the 1940 data) and "parent" family income (in the subsequent data). The thicker blue line above it excludes the 1940 data when estimating cell averages for "parent" income, thus using only a family income concept for all "parent" incomes. While the two series show essentially the same trend from 1990 to 2019, the estimates diverge the further back they go in time before 1990. According to the series that drops the 1940 data, the IGE exceeded its 1960 level only in 2019, and the drop in the IGE from 1960 to 1980 was much steeper than the AM estimates would suggest. If AM had used this methodology in their paper, which ended with estimates from 2000, they would have concluded that mobility remained higher than in 1960.

Figure A4 | INTERGENERATIONAL ELASTICITY COMPARING PARENTAL INCOME TO SONS' EARNINGS, 1950-2019, 40-YEAR-OLD MEN



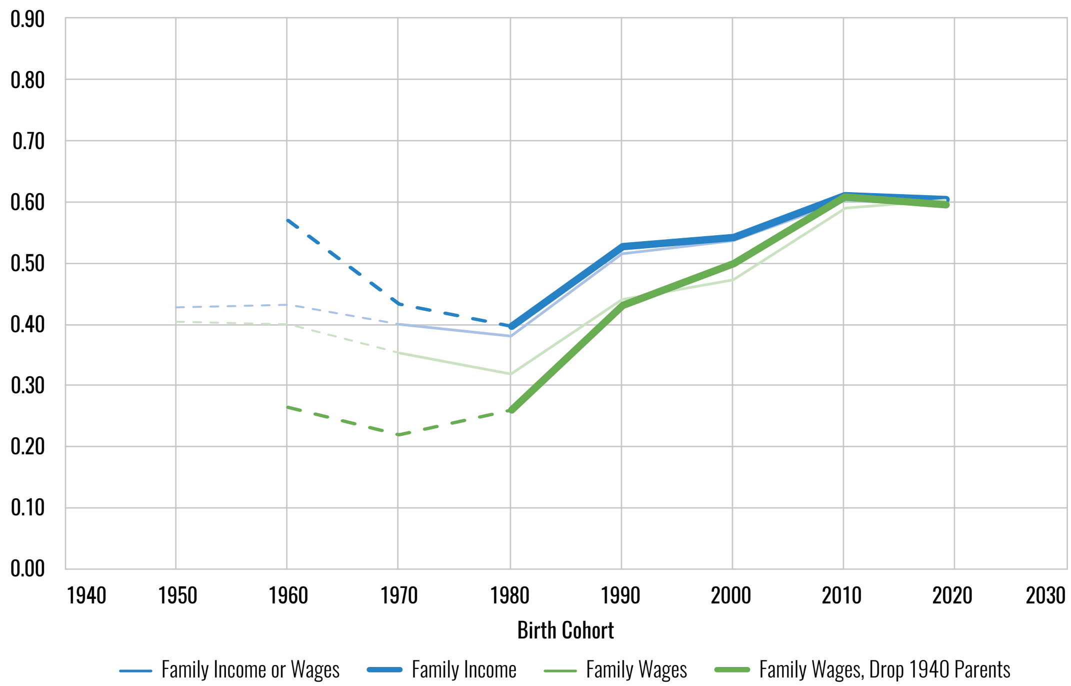
As mentioned above, these estimates for 40-year-olds involve extrapolating from the data for earlier years. For instance, while the 1921 birth cohort is the earliest included in estimating "parent" income, a 40-year-old in 1950 would have been born in 1910. The dashed lines show the part of the IGE trends that rely on extrapolated estimates (involving the 1950 and 1960 estimates when the 1940 data is included in estimating "parent" income, and involving the 1960 and 1970 estimates when the 1940 data is excluded). If we discount the extrapolated estimates, the remaining ones are fairly close, whether the 1940 data is excluded or not.

Another way to assess the longer-term trend is to use family wage and salary income in the years after 1940 for "parent" income. The thin green line shows the resulting IGE trend.[168] It shows a steeper decline from 1950 to 1980 than the baseline trend that mixes family wage and salary income in 1940 with family income thereafter, but a shallower decline from 1960 to 1980 than when family income is used consistently after excluding the 1940 data.

The thicker green line shows the trend when family wage and salary income is used for all years but the 1940 data is dropped. Since this time the 1940 income measure is consistent with the other years, dropping the 1940 data would be expected to affect the results minimally. In fact, doing so lowers the IGEs from 1960 to 1980. Nevertheless, the basic pattern is the same across the four trend lines in Figure A4: a fall in the IGE from 1950 to 1980 followed by an increase. The relative magnitude of the changes for these two periods is unclear, however, though all four trends show the IGE at an all-time high in 2019.

Figure A5 presents the same analyses for models that exclude birth-cohort-by-income interactions. The pre-1980 trend is less clear for these models, though the post-1980 trends (relying on estimates not extrapolated from the data) are consistent with each other. The trend in Figure A5 differs visibly from that in Figure A4, however, showing a deceleration in the increase in the IGE and little change between 2010 and 2019.

Figure A5 | INTERGENERATIONAL ELASTICITY COMPARING PARENTAL INCOME TO SONS' EARNINGS, 1950-2019, 40-YEAR-OLD MEN

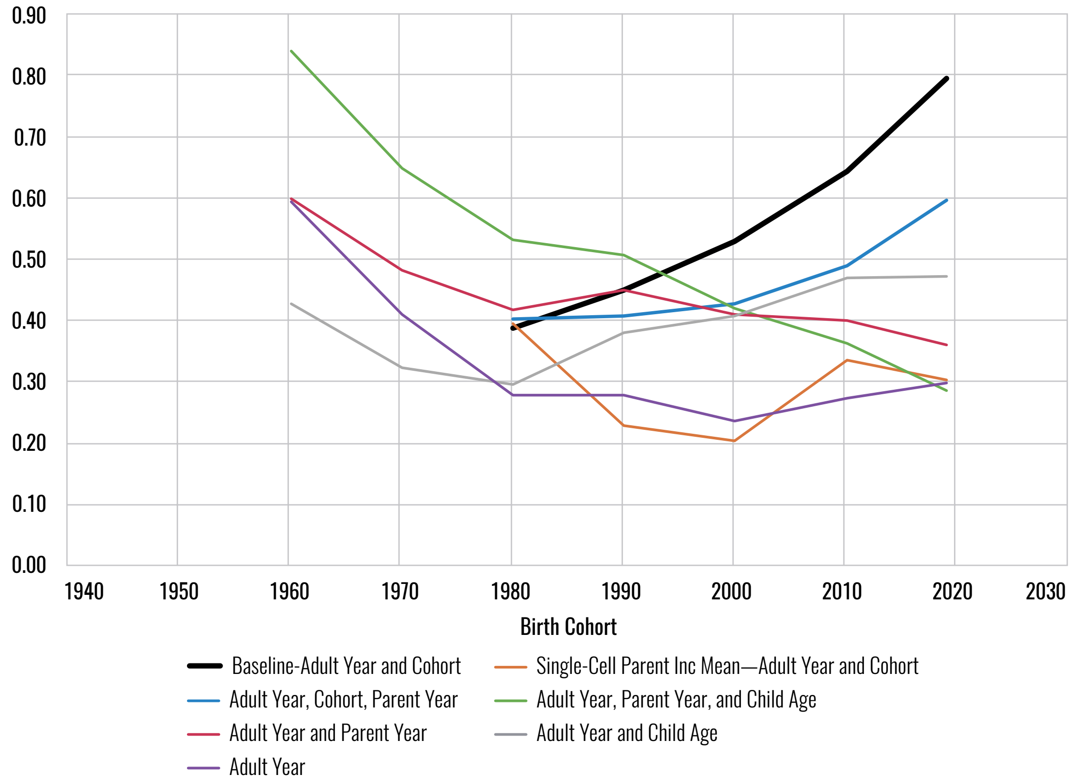


All of these estimates have shown an increase in the IGE after 1980, which might seem to suggest a robust finding. Indeed, AM presented a variety of estimates that were sometimes inconsistent before 1980 but consistently found a large 1980–1990 rise in the IGE.[169] However, as mentioned above, a possible confounding influence on these estimates is the fact that "parent" incomes are measured in different years. Another potentially confounding factor is the ages of parents when "parent" income is measured.

One way to control for the influence of when these incomes are measured—either a true influence on IGEs or an artificial influence due to differences across censuses or ACSs—and for the influence of parent age is to match adult sons to parent income averages from a single census (rather than from averages incorporating multiple censuses). For example, AM assign an adult son between the ages of 30 and 34 in 1980 and born in Florida a "parent" income that is the average of two averages—mean income in the 1950 census among the parents of children ages 0 to 4 born in Florida, and mean income in the 1960 census among the parents of children ages 10 to 14 born in Florida. (Refer to Table A1, above.) Instead, I randomly assign one of these two averages to the adult son—either from the 1950 census (where parents have children under age 5) or from the 1960 census (where parents have children ages 10 to 14). Then I can include dummies for the year in which parent income is drawn and for the age range of the children of those parents, as well as interacting those dummies with "parent" income.[170]

In **Figure A6** I present results from such models.[171] I show only estimates that do not require extrapolating outside the data (in the case of cohort effects). The thick black line carries over the trend shown by the thick blue line from Figure A4—the baseline trend using family income (excluding 1940 data) extended to 2019 but excluding the pre-1980 estimates that involve extrapolating off the support of the model. Again, these estimates are from a model that allows the IGE to vary not only by the year in which sons' earnings are observed, but by birth cohort and age. The orange line presents the same results when sons are matched to a parent income average from a single census year. The 1980 estimate starts at the same level, but rather than increasing, the IGE declines. It ends up higher in 2019 than in 1990 and 2000, but lower than in 1980 and 2010.[172]

Figure A6 | INTERGENERATIONAL ELASTICITY COMPARING PARENTAL INCOME TO SONS' EARNINGS, 1950-2019, 40-YEAR-OLD MEN



The blue line adds to the model dummies for the year in which "parent" income is taken, as well as interactions of these dummies with "parent" income (so that the IGE may vary depending on the year in which "parent" income is measured. The levels of this line are arbitrary—technically they are shown at the predicted level when "parent" income comes from 1960 (the omitted category in the regression). But the trend reinforces the estimates from the baseline AM approach to modeling the IGE; the IGE rises steadily, though this time only after 1990.

The green line drops cohort dummies and interactions with "parent" income but adds dummies for the age range of children when "parent" income is averaged (and interactions with "parent" income). Like the blue line, it also includes "parent year" dummies and interactions. Because the model does not include cohorts, we can extend it back to 1960 without extrapolating off the support of the model. This line shows a steep and steady decline in the IGE from 1960 to 2019. (Again, the levels are arbitrary, shown at the predicted level when parents have children under age five.)

The red line drops the "child age" dummies and interactions and also shows a decline over time, though a smaller one than with child age included in the model. The gray line only allows the IGE to vary by the year in which adult earnings are observed and by child age. It shows a decline in the IGE from 1960 to 1980, followed by a bigger rise through 2019. Finally, the purple line only allows the IGE to vary by the year in which adult earnings are observed. It shows a sharp decline between 1960 and 1980 with little change thereafter.

Which of these trends most accurately reflects what happened to intergenerational mobility over time? There is little basis for concluding one way or another. Perhaps there are other interactions that should be modeled, or perhaps a more complicated model specification would produce an entirely different result.[173] The fact that the results change so much simply moving from the AM approach to the approach using single-cell means of "parent" income rather than averaging multiple cell means suggests that these estimates are very sensitive to the assumptions baked into these models.

While a creative use of non-longitudinal data to deal with the dearth of data for income mobility research, it does not appear that the approach used in AM is robust enough to cite confidently.[174]

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ENDNOTES

1 Winship (2017a). Mazumder (2016).

2 Chetty, Grusky, Hell, Hendren, Manduca, and Narang (2016, 2017).

3 Winship (2018).

4 Part Two also reported that only one study outside the US looked at whether people exceed their parents' income at the same age (Ostrovsky, 2017), and that study found that Canada had the same absolute mobility as the US. Since Part Two was published, evidence has emerged that American absolute mobility may be lower than in Australia, Germany, Norway, Sweden, Finland, the Netherlands, and the United Kingdom, though perhaps not lower than in Canada, France, Japan, or Denmark. See Manduca et al. (2020), Berman (2020), Stockhausen (2018), Bonke, Harnack, and Luthen (2019), and Liss, Korpi, and Wennberg (2019).

5 Klein (2019).

6 Chetty, Hendren, Kline, Saez, and Turner (2014a, 2014b).

7 Manduca et al. (2020) find the US has higher absolute mobility than Denmark and mobility at least as high as in Canada. One coauthor of that paper had previously found the US and Canada have similar absolute mobility (Ostrovsky, 2017). Berman (2020) affirms the Canadian result but finds higher mobility in Denmark. However, as he discusses (comparing his own results to Manduca et al.), the Denmark result is probably an overestimation because he uses a less applicable distribution of income to approximate true parental income.

8 Winship (2015).

9 Winship (2016). Appendix 2.

10 For critiques of these declensionist narratives, see Sheffield and Winship (2020), Winship (2017b).

11 Technically, the p value for the t statistic testing whether the IRA for the 1976–83 cohorts is different from the IRA for the 1952–59 cohorts is 0.72. This p value indicates the likelihood that by virtue of chance, I might get the sample change reported in the event that the true change in the overall population of interest was zero. When the p value is low, that indicates that the reported result is unlikely to be the result of idiosyncrasies related to the sample being analyzed, such as the particular people who were sampled.

The p values indicating whether the 1960–67 or 1968–75 cohorts were different from the 1952–59 cohort are also very large, suggesting little reason to think they differ. Excluding men with non-positive earnings does not change the conclusion—the change between the first and last cohorts is 0.08 but the p value is 0.39.

12 As summarized in Part One of this primer, a reasonable conclusion from past research is that the male earnings IRA, when optimally measured, is around 0.40. My own preferred estimates from Part One, which attempted to improve upon earlier research, ranged between 0.44 and 0.52.

13 Technically, the changes is statistically significant at $p<0.05$. I have implemented no correction for multiple comparisons, and I assume the two sets of birth cohorts come from independent samples. The latter assumption is not strictly true, but my IRA analyses suggested the assumption makes little practical difference.

14 The p value for the change is 0.04. Not shown, there was also an increase in the likelihood of falling from the third quartile to the first quartile between the 1952–59 and 1960–67 cohorts, with a p value of 0.04.

15 Fertig (2003) examined short-term trends in transition probabilities comparing father and son earnings. Between the late 1980s and the early 1990s, she found an increase in upward mobility from the bottom fifth and a small increase in downward mobility from the top fifth. The study also reported a small increase in downward mobility from the middle fifth.

The estimates in Figure 2 generally indicate less upward and downward mobility than point-in-time estimates from previous studies that use father and son earnings quartiles, reported in Part One of this primer, though the 31 percent estimate for sons' persisting at the top in the 1976–83 cohorts is lower than in most earlier studies.

16 The p value for the change over time is 0.43. The initial drop from 0.42 to 0.31 between the 1952–59 and 1960–67 cohorts was marginally statistically significant, with a p value of 0.06. The estimates are little different if non-positive incomes and earnings are dropped.

17 These changes are very similar when ties are given different ranks, when non-positive incomes are excluded, when the regression does not use sample weights, and when parental income is adjusted for family size.

18 The p value for the NLSYM-to-NLSY97 increase is 0.23. For the NLSYM-to-NLSY79 increase, it is 0.06. For the NLSY79–NLSY97 decline, it is 0.35. The year-to-year changes are very similar in the model that does not use sample weights, and the p values are 0.10 for the NLSYM–NLSY97 increase, 0.02 for the NLSYM–NLSY79 increase, and 0.38 for the NLSY79–NLSY97 decline.

19 The NLS levels in Figure 1 are comparable to past research, which generally concludes the IRA comparing parental income to sons' earnings is between 0.30 and 0.40. (See Part One of this primer.) The PSID estimates are higher and closer to my preferred estimates from Part One, which aim for the best possible point-in-time estimates and range from 0.43 to 0.47.

20 The levels estimated in the study are lower than those I find using the same datasets, probably because the Richey and Rosburg estimates are based on a statistical model that controls for several demographic variables, or possibly because they measure income as young as age 26.

21 Pooling sons and daughters, Chetty, Hendren, Kline, Saez, and Turner (2014a, 2014b) compare parent family income to grown-child family income, but they note in passing (2014b, p. 5) that when they used *individual* grown-child income instead, their finding of no change in the IRA for cohorts born 1971–86 held up. Davis and Mazumder (2020) also report results combining sons and daughters and find an increase in the IRA between the original NLS cohorts and the NLSY79 cohorts, from 0.15 to 0.29. (See their Table 8.)

22 The p values for these four changes is 0.09 for Q1 to Q4, 0.09 for Q4Q1, 0.01 for Q3Q1, and 0.04 for Q3Q4. The Q3Q1 and Q4Q1 changes are also significant from 1952-59 to 1968-75, and the Q3Q4 change nearly is (p=0.14).

23 The p value for the change is 0.27. The initial drop of 0.15 between the 1952–59 and 1960–67 cohorts is statistically significant (p value equals 0.004). If size-adjusted family incomes are used, the results are very similar, and they are similar dropping non-positive incomes.

24 The p value for the NLSYM–NLSY97 increase is 0.09, that for the NLSYM–NLSY79 increase is 0.07, and that for the NLSY79–NLSY97 decline is 0.76. Again, it matters very little how ties are treated, whether non-positive incomes are included, whether regressions rely on sample weights, or whether incomes are size-adjusted.

25 It is possible, too, that by including some older sons (and averaging parental incomes of sons that we both include, but incorporating their incomes at older ages into those averages), Davis and Mazumder introduce selection into their analyses. Not all sons live at home at older ages. Sons in the NLSY79 who do not live at home will not have parental income observable in any such year, while parental income continues to be observable for older sons in the NLSYM, even when they leave home. However, when I include older sons in my sample, my IRA rises only from 0.33 to 0.35.

26 See their Appendix Table A5.

27 Most past research on male family income IRAs puts them between 0.30 and 0.40 (where the NLS estimates lie for the two most recent cohorts). (See Parts One and Two of this primer.) My preferred estimates in Part One ranged from 0.47 to 0.60, higher than the Figure 1 NLS estimates for recent years, but consistent with the PSID estimates.

28 The p values for the two changes are 0.09 and 0.06.

29 The p value of the decline was 0.07.

30 The p value of the decline was 0.10.

31 The p value was 0.01.

32 In a working paper that was superseded by their 2021 study, Justman and Stiassnie (2020) found that upward

mobility out of the bottom fourth rose between the 1952–61 and 1962–71 birth cohorts, but then it fell by a bit more in the 1972–81 cohorts. In contrast, the share of sons starting in the top fourth who moved downward fell and then rose by a bit less. There was little change in relative mobility in the middle of the parental income distribution.

33 The NLS estimates in Figure 6 are consistent with the levels of mobility reported in three previous studies that examined family income quartiles, reported in Part One of this primer. Peters (1992) examined sons. Schoeni and Wiemers (2015) and Charles et al. (2014) pooled sons and daughters. The PSID levels indicate lower mobility than those estimates.

34 The p value for the change when non-positive earnings are included is 0.17 but 0.04 when they are excluded. The p value for the 0.14 increase in the IRA between the 1952–59 and 1960–67 was 0.03.

35 Dahl and DeLeire (2008) estimated a father-daughter earnings IRA between 0.08 and 0.17. My own estimates from Part One of the primer ranged from 0.22 to 0.39, with estimates on the higher end of that range when dropping non-positive earnings.

36 The p value is 0.05. When fathers and daughters with non-positive earnings are excluded, no changes between the first and last set of cohorts achieve statistical significance.

37 The p values for the mother-daughter trends are 0.11 including non-positive incomes and 0.55 excluding them.

38 The p values for the change are 0.05 for Q1Q3 and 0.001 for Q1Q4.

39 The p values for the changes are 0.06 for Q4Q1, 0.02 for Q4Q2, and 0.002 for Q4Q3.

40 The change from the 1952–59 cohorts to each of the other cohorts was at least marginally statistically significant. The p value for the increase between the first and last cohorts is 0.004.

41 The NLSYW began after the NLSYM. To keep samples for women comparable across the three NLS cohorts, I use different years than in the analyses for sons. See Appendix 1.

42 The p values are 0.35, 0.001, and 0.05 for the NLSYW–NLSY79 change, NLSY79–NLSY97 change, and NLSYW–NLSY97 change.

43 The trend is from 0.23 to 0.19 (p value for the change is 0.43) and then to 0.30 (p value for the change is 0.03, 0.24 for the NLSYW-NLSY97 change).

44 The estimates in Figure 7 for the most recent cohorts are toward the lower end of the range I report in Part One of this primer (0.29 to 0.44). Chetty, Hendren, Kline, and Saez (2014) report an estimate of 0.25.

45 The p value for Q1Q1 is 0.08, the value for Q2Q4 is 0.04, and the value for Q3Q2 is 0.04. No changes are statistically significant when non-positive incomes are excluded.

46 The p value for the increase in the share stuck in the bottom fourth is 0.05 for the entire period, but only 0.34 between the NLSY79 and NLSY97. The p values for the decline in the shares making it to the third quartile and the top quartile are 0.32 and 0.06 over the entire 30 years but 0.07 and 0.05 between the NLSY79 and NLSY97.

47 When parents and daughters with non-positive income are excluded, the share of women starting in the bottom and remaining there as adults increases from 30 percent in the NLSYW to 33 percent in the NLSY79 and to 44 percent in the NLSY97, and both the inter-cohort changes are statistically significant. In contrast, the changes in downward mobility from the top are never statistically significant (nor large).

48 The p value of the change is 0.01.

49 The p value of the NLSYW–NLSY79 change is 0.06, while the value for the change over 30 years is 0.03.

50 The IRA is flat at 0.32 and then rises to 0.36, but the p values for these changes are never less than 0.39.

51 Unlike in their analyses of sons, Davis and Mazumder measure daughter family incomes when they are older (near age 40).

52 The estimates in Figure 7 are lower than my preferred estimates from Part One of this primer, which ranged from 0.56 to 0.65. However, they are near other family income IRA estimates for daughters from other papers, which range from 0.34 to 0.40, as reported in Part One.

53 The p values for the change is 0.01.

54 This change barely misses achieving statistical significance at $p<0.10$.

55 The estimates in Figures 12 and 13 are consistent with, but occasionally somewhat higher than, the levels of mobility reported in three previous studies that examined family income quartiles, reported in Part One of this primer. Peters (1992) examined daughters separately. Schoeni and Wiemers (2015) and Charles et al. (2014) pooled sons and daughters.

56 The formula is 1.10^0.40.

57 The p value is 0.87.

58 As described in Part One of this primer, recent analyses have increasingly found male earnings IGEs of 0.50 or higher. The preferred range in my own Part One analyses was 0.44 to 0.78.

59 Aaronson and Mazumder (2008, p. 148, note 21) report (without showing) that their results showing falling mobility were not greatly affected when they used father earnings instead of parent family income to compare against sons' earnings. However, as I discuss in Appendix 4, their methodology appears unreliable.

Other papers using the PSID and a different measure of absolute mobility (the intergenerational correlation, or IGC) found no change or increases in male earnings mobility between the late 1970s and the early 2010s. The IGC can be viewed as measuring the extent to which patterns of absolute mobility reduce childhood income gaps in adulthood, but after adjusting for the change in the dispersion of the income distribution. Reville (1996) finds a flat to declining trend, as he does for the IGE. Fertig (2003) finds a decline, but one that is not statistically significant. Corcoran (2001) reports the IGC fell from the late 1970s and early 1980s to the late 1980s and early 1990s (cohorts born 1953-60 versus 1961-68). She finds similar results looking at hourly wages. Brenner (2007) found an increase comparing men born 1945–54 and those born 1965–74, but it was not statistically significant.

Levine and Mazumder (2007) use the NLSYM and NLSY79 to estimate trends in the correlation between brothers' earnings, which is related to the IGE and IGC but captures all of the family background influences shared by brothers (rather than just family income). They find a decline in mobility (an increase in the brother correlation).

60 The p value is 0.78. The p value for the drop in the IGE between the first two cohorts, however, is 0.04.

61 The p values for the NLSYM–NLSY79, NLSY79–NLSY97, and NLSYM–NLSY97 changes are 0.20, 0.09, and 0.94. If the regressions are estimated without using sampling weights, the IGE rises by 0.09 and then falls by 0.09, ending 0.01 higher, and the p values are 0.04, 0.03, and 0.86. If they are weighted but parent incomes are adjusted for family size, the initial rise is 0.11, the subsequent fall is 0.08, and the overall change is 0.03. The p values are 0.06, 0.08, and 0.59.

62 Levine and Mazumder (2002) find a decline of 0.08 that is not statistically significant between 1980 and 1992 (observing cohorts born 1946–54 or 1957–65). Blanden (2005) reports two sets of estimates. In one, she finds a not-statistically-significant increase in the IGE between the 1954–57 and 1958–62 cohorts, no change between the 1958–62 and 1963–70 cohorts, and no statistically significant change across the three groups. In a second set of estimates, the trend is dominated by a decline in the IGE between the 1950–53 and 1957–60 cohorts but also shows an uneven increase between 1957–60 and 1962–65. Mayer and Lopoo (2005) report in passing (p. 179) that their analyses, which focus on sons' family income rather than earnings, were similar when they looked at sons' earnings. That would suggest that they resemble the second set of Blanden results, which were designed to mimic an earlier version of the Mayer-Lopoo paper.

63 Pooling sons and daughters in the NLSYM and NLSY79, Davis and Mazumder (2020) also find an increase between the two cohorts. Richey and Rosburg (2017) analyze the NLSY79 and NLSY97, finding the IGE fell slightly, but not enough to be statistically significant. This study pools sons and daughters, however, as does Richey and Rosburg (2014), which finds a similarly small decline in the IGE.

64 In addition to these PSID and NLS analyses, Levine and Mazumder (2002) and Levine (1999) use relatively crude income measures in the General Social Survey and find, respectively, an implausibly large increase in the IGE from around 1980 to around 1990 and a smaller increase from the 1970s to the mid-1980s and early 1990s.

Two of the PSID studies estimate trends in a different measure of absolute mobility, the IGC. Blanden (2005) reports a decline in the IGC (an increase in mobility) between the 1950–53 and 1957–60 cohorts, followed by an rising IGC (consistent with her IGE results, but less varying). Justman, Krush, and Millo (2017) find a largely flat IGC between the 1952–61 cohorts and the 1964–73 cohorts, followed by a rise through the 1968–77 cohorts and then a smaller decline through the 1970–79 cohorts.

As reviewed in Part One of this primer, the estimates in the previous literature estimating IGEs that compare parental income to sons' earnings (without looking at the trend) find IGEs between roughly 0.30 and 0.60. My own preferred estimates were higher, ranging from 0.64 to 0.87.

65 The p value is 0.14. Both the 1960–67 and 1968–75 cohorts have IGEs lower than the 1952–59 cohort by a statistically significant amount (p=0.01 and 0.07, respectively).

66 Only the increase between the NLSYM and NLSY79 is statistically significant. The p value for the initial rise is 0.03, while the p values for the subsequent fall and for the increase over the entire period are 0.21 and 0.20, respectively. When the analyses are conducted without sample weights, the trend is very similar and the p value for the recent decline is 0.08 while the p value for the NLSYM–NLSY97 increase is 0.09. Using size-adjusted parental income also produces similar results, but again only the initial increase is statistically significant at conventional levels.

67 Several studies report IGCs using the PSID that are consistent with their IGE results. See Mayer and Lopoo (2004), Blanden (2005), and Justman and Krush (2013). Corcoran (2001) finds the IGC declined from the late 1970s and early 1980s to the late 1980s and early 1990s. Justman and Stiassnie (2020) find a rise in the IGC.

68 In addition to these studies, Levine and Mazumder (2007) report a decline in mobility among sons (an increase in the brother income correlation) between the NLSYM and NLSY79.

69 These include using sample weights from the first year of the survey rather than in the ending year of the analyses (thereby ignoring the sample attrition correction in the later weights), conditioning on parental age at birth for consistency with the NLSYM and NLSYW samples, not conditioning on being out of school when grown-child income is observed, and using an expanded set of birth cohorts.

70 The 1968 surveys from which parental income might be drawn (and appear to have been drawn in the Davis-Mazumder study) were mailed to participants and included a single income question where respondents were to choose an income range. In the surrounding years, the interviews were done in person and respondents were asked about income amounts from several sources. Income from family members other than the respondent and spouse was recorded differently in 1978 and 1980 than in 1981, and the 1978 and 1980 interviews were over the telephone rather than in person.

71 I continued to use a single year of parental family income, a narrower range of birth cohorts than Davis and Mazumder, and the sample weight for the final year in which sons are observed, and I conditioned on not being enrolled in school in the last year where income might be measured. But I restricted the NLSY79 and NLSY97 samples to sons who had either a father aged 26–46 when they were born or a mother aged 10–30, and I averaged up to three years of sons' family income (1978, 1980, and 1981 in the NLSYM, 1990, 1991, and 1993 in the NLSY79, and 2010, 2012, and 2014 in the NLSY97). Sons were 27–32 years old when income is measured in the NLSYM and NLSY79, but 26–32 when income is measured in the NLSY97.

72 See Bloome, Dyer, and Zhou (2018), Raaum et al. (2007), Torche (2011), Bratberg et al. (2017), Torche and Corvalan (2018). Bloome and Western (2011) estimate lower IGEs separately for whites and blacks. Richey and Rosburg (2014, 2017) estimate lower IGEs pooling sons and daughters and comparing their individual incomes to their parent family income.

73 One research team has reported IGEs comparing parent family income to sons' earnings that range from 0.48 to 0.56. (See Bratsberg et al. (2007), Raaum et al. (2007), and Jantti et al. (2006).) But these may not be comparable to the IGEs comparing parents' and sons' family incomes. Raaum et al. find the IGE comparing parent family income to combined son-and-spouse wage and salary income is 0.43—lower than the 0.48

estimate for sons' family income. Further, Gregg et al. (2017) and Levine and Mazumder (2002) report IGEs ranging from 0.33 to 0.43 comparing parent family income to sons' earnings. These studies all look at men who are older than in Davis and Mazumder.

74 See Torche (2011), Peters (1992), and Altonji and Dunn (1991). Those studies find even higher family income IGEs looking at daughters. A number of studies have found smaller IGEs comparing father and son earnings using the NLSYM, though there is no reason to think these should be comparable to the family income IGE, and the two studies that include estimates of both find the family income IGE is larger. See Peters (1992) and Altonji and Dunn (1991), as well as Couch and Lillard (1998), Lillard (2001), Couch and Lillard (2004), and Grawe (2004). Furthermore, Groves (2005) finds a male earnings IGE of 0.25, and Levine and Mazumder (2002) report an IGE comparing parental family income to sons' earnings of 0.235. (Zimmerman (1992) finds large male earnings IGEs, but he restricts the sample to full-time, full-year workers.)

75 As reviewed in Part One of this primer, previous research has found family income IGEs ranging between 0.35 and 0.70. My own preferred estimates from that paper ranged between 0.60 and 0.84.

76 The p value is 0.14. The increase in the IGE between the 1952–59 and 1960–67 cohorts, however, is statistically significant (p=0.05).

77 Fertig reported similar results using the IGC rather than the IGE. In Part One of this primer, I reported a preferred range for the father-daughter earnings IGE level (ignoring trends) of 0.44 to 0.46, consistent with other research looking at levels.

78 She reports similar results using IGCs. The mother-daughter IGE level I estimated in Part One of this primer ranged from 0.27 to 0.54.

79 The p value is 0.07. Using size-adjusted parental income, the increase is 0.21 and the p value is 0.03.

80 In terms of levels, the review in Part One of this primer found estimates from previous studies ranging between 0.25 and 0.55, while my own estimates ranged between 0.64 and 0.82.

81 The p values for the changes are 0.63 in the PSID and 0.57 in the NLS.

82 See Hertz (2007), Lee and Solon (2009), Mayer and Lopoo (2004), Page (2004), and (pooling sons and daughters) Bloome (2015) and Chetty, Hendren, Kline, Saez, and Turner (2014a, 2014b). Mayer and Lopoo (2004) also find little change in the IGC.

83 In Part One of this primer, I reviewed studies that assessed family income IGE levels and reported estimates ranging from 0.34 to 0.71. My own preferred estimates reported in Part One ranged between 0.59 and 0.66.

84 Chetty et al. (2016, 2017).

85 See their "Alternative Estimates of Absolute Income Mobility by Birth Cohort" table (Online Data Table 4): https://opportunityinsights.org/data/?geographic_level=0&topic=0&paper_id=546#resource-listing. While their main results show family income mobility for the four sets of cohorts as 0.69, 0.59, 0.60, and 0.54, the results using the PCE deflator are 0.70, 0.60, 0.62, and 0.57. On the superiority of the PCE deflator as an inflation measure, see Winship (2016).

86 Sheffield and Winship (2020) estimate the ratio of median individual income of non-Hispanic men ages 25–29 and the same median 30 years later. They find that it tracks the Chetty et al. results for individual income qualitatively, though it finds a much smaller decline between the 1949 and 1959 cohorts than the Chetty et al. estimates.

87 The p value for the change is 0.06. I assume for simplicity that the estimates for the two sets of cohorts come from independently drawn samples, which is not strictly true.

88 The p value for the change is <0.001.

89 The three estimates are 65 percent, 66 percent, and 63 percent, respectively.

90 The estimates come from the Chetty team's Online Data Table 3, available at https://opportunityinsights.org/data/?geographic_level=0&topic=0&paper_id=546#resource-listing. Size-adjusted results are unavailable

separately for sons. Estimates pooling men and women that do not adjust for family size are very similar to the estimates for men or women alone—see Online Data Table 1 at https://opportunityinsights.org/wp-content/uploads/2018/04/table1_national_absmob_by_cohort_parpctile.xlsx.

91 The Chetty et al. bounds pool men and women, but bounds for each individually would look very similar.

92 See his Figure 1, which shows that had the US moved from contemporary American levels of relative mobility to Scandinavian levels, the absolute mobility trend would have been largely the same. Trends in absolute mobility are driven by changes in the distribution of grown-child and parent incomes rather than by changes in relative mobility.

93 One potential problem with my estimates involves the larger disparity in the earliest PSID and NLS samples between the point in the business cycle at which parental income is measured and the point at which grown-child income is measured. Looking at the NLS, the unemployment rate was twice as high in 1981 as in 1966, the two years in which incomes are compared to estimate mobility for the first NLS cohort of sons (a 3.8-percentage point difference, see Table 1 above). For both of the subsequent NLS cohorts of sons, the unemployment rate was just 0.8 points higher when grown-child income was measured than when parental income was measured. The first and last cohorts of NLS daughters show the same difference. In the PSID, the earliest cohorts of sons and daughters saw an increase in unemployment between childhood and adulthood, while the other cohorts saw declines. The concern is that absolute mobility might have been higher in the earliest cohorts if not for the fact that the mobility estimates for those cohorts compare grown children in high-unemployment years to their parents in low-unemployment years.

One way to check the sensitivity of the trend to this issue is to re-estimate the absolute mobility estimates after excluding parents or sons with incomes of $0 or less. Doing so removes less than 4 percent of observations from the first two NLS cohorts of sons and 7 percent from the most recent one. As expected, the share of sons experiencing absolute mobility in 1981 does go up once the poorest parents and sons are removed, but only from 50.6 to 52.1. Even if I use the lower 1993 estimate when non-positive incomes are included, the 1.8-point drop would not be statistically significant. Furthermore, the Chetty et al. estimates exhibit only a modest cyclical component. In addition, my PSID estimates average parent and son incomes over multiple years, and sons' multi-year averages are taken from different years depending on when sons are born.

Nor does it appear that changes in the age of parents can account for my results. In the PSID, the distribution of father and mother ages changes little over time (Table 1). In the NLS, the median age of fathers fell in the most recent cohorts. All else equal, we might expect that the NLSYM estimate (for cohorts born around 1950) and the NLSY79 estimate (for cohorts born in the early 1960s) are biased downward relative to the NLSY97 estimate, since it will be harder for sons in their thirties to exceed parents' income when the latter is measured in peak earnings years. But if I restrict the NLSYM and NLSY97 samples to sons with fathers between 45 and 59 (the age range of fathers whose income reports may be linked to NLSYM sons, as described in Appendix 1), absolute mobility is 47 percent for both cohorts.

94 See Historical Income Tables: Families, Table F-9. https://www2.census.gov/programs-surveys/cps/tables/time-series/historical-income-families/f09ar.xls. The Chetty team extrapolates the CPI-U-RS price index to extend back from 1977 by using the 1977 ratio of the CPI-U-RS to the CPI-U. I do the same, putting the Census Bureau estimates in 2014 dollars to match the Chetty estimates. For the Chetty estimates, see their Table S1 at https://opportunityinsights.org/wp-content/uploads/2018/03/abs_mobility_paper.pdf.

95 The Chetty estimates are higher in part because in their study, 30-year-old income for the parents of the 1950 birth cohort is measured in 1960 for parents who had their children before they turned 25. Income at 30 is measured for parents of newborns in the 1950 census if they were between the ages of 25 and 34, and it is approximated using the 1940 census by taking the incomes of adults ages 25 to 34, some of whom will become parents in 1950 when over 35, but this last group is weighted down to reflect the small share of births that occurred to older women in 1950.

96 In my NLS data, mean parental income in 1966 for the earliest NLS sample was $63,031. This average is higher than the Census Bureau mean for all parents in 1966 because NLS parents were relatively old (75 percent had a father at least 40 years old). But these older parents were much richer than older parents in 1950 due to strong economic growth.

The fact that NLS parents are older than their grown children when their incomes are measured will make absolute mobility lower than would be the case if NLS parents were in their early thirties when their incomes are measured. But the trend should not be affected unless there is variation between the three NLS samples in typical age of parents. The median age of fathers was 45, 44, and 41 in the three samples (and in the three samples, half of fathers were between the ages of 40 and 48, 39 and 49, and 37 and 45). So while the most recent sample featured somewhat younger fathers, that cannot explain why the NLS estimate for the 1949–51 cohorts is so much lower than in the Chetty study.

97 These estimates of income growth are understated (and more so for the most recent figures) due to various measurement challenges, including the proper measurement of inflation, the omission of noncash government benefits and employer health care from income, and the failure to account for taxes. But improving the income growth measures would also increase absolute mobility. The point is to explain the difference between the Chetty et al. results and my own.

98 See Online Appendix Table 4 for the age-40 estimates.

99 There is no estimate at age 40 for the 1983 cohort, since it will turn 40 in 2023.

100 Katz and Krueger (2017) show that the Chetty et al. trend closely tracks the difference in the median of child income (in the Chetty data) and the median of "parent" income for each cohort. They also report that the share of children with income higher than the median "parent" (again, using the Chetty data) also tracks the Chetty et al. absolute mobility trend.

101 He reports similar results for individual labor income, pooling sons and daughters.

102 Absolute mobility estimates for other countries are presented for Australia, Canada, Denmark, Finland, France, Japan, Norway, Sweden, and the UK in Berman (2020); for Canada, Denmark, Finland, Netherlands, Norway, Sweden, and the UK in Manduca et al. (2020); for Germany in Stockhausen (2018) and Bonke, Harnack, and Luthen (2019); for Canada in Ostrovsky (2017); and for Sweden in Liss, Korpi, and Wennberg (2019). Part Two of this primer, Winship (2018), only was able to include the Ostrovsky study. Taken as a whole, the research indicates that absolute mobility has declined across all of these countries since the mid-twentieth century.

103 Other factors probably bias the trends in absolute mobility estimated by Chetty et al. and by me downward. Noncash government benefits became more generous over time, including food stamps, housing assistance, school breakfasts and lunches, Medicaid, and Medicare. Except for food stamps (in the NLS data), none of these benefits are counted in family income. Including them would raise absolute mobility levels for each cohort but would probably raise them more for the most recent cohort. Similarly, employer-sponsored fringe benefits like health insurance are also excluded from income, and those have risen as a share of employee compensation over time. The income figures are also pre-tax estimates, and since taxes have fallen over the years, post-tax figures would also show more absolute mobility for each cohort and would probably make a greater difference in the most recent cohort. Furthermore, the income estimates in Figure 17 are adjusted for inflation using indices that likely overstate the rise in prices and thereby understates the true increase in real income (PCE deflator for my analyses, CPI-U-RS for Chetty et al.).

104 The p value is 0.002 comparing the two sets of cohorts.

105 The p value for the change is 0.001.

106 The p value for the change is 0.93. However, the initial decline, from 49 percent to 43 percent, is statistically significant (p value < 0.01).

107 The p value is 0.10 for the initial decline, 0.13 for the subsequent rise, and 0.69 for the overall decline. The cohort-to-cohort changes are larger when non-positive incomes are dropped, but the 3-point decline between the first and last cohorts is not statistically significant (p=0.25).

108 Berman (2020); Manduca et al. (2020).

109 Chetty et al. (2016, 2017).

110 Chetty et al. (2016, 2017); Isaacs, Sawhill, and Haskins (2008); Pew Charitable Trusts (2012); Acs, Elliott, and

Kalish (2016); Bengali and Daly (2013).

[111] Winship, Reeves, and Guyot (2018).

[112] Winship, Pulliam, Gelrud Shiro, Reeves, and Deambrosi (2021).

[113] See Winship (2009) for citations to past research on attrition bias in the PSID.

[114] See Winship (2009) for details.

[115] The PSID also includes a "Latino sample," which was surveyed from 1990 to 1995, before being dropped. I do not use this sample.

[116] Brown (1996).

[117] Winship (2009); Winship, Pulliam, Gelrud Shiro, Reeves, and Deambrosi (2021).

[118] See Winship (2009) for an extensive discussion.

[119] See, e.g., Moffitt (2020) and Winship (2009).

[120] Kopczuk, Saez, and Song (2007).

[121] On the superiority of the PCE deflator, see Winship (2016).

[122] See https://nlsinfo.org/ and https://www.bls.gov/nls/.

[123] Son-reported family income (referring to income in the previous 12 months) is available in the same form in the 1966 through 1971 surveys. (Sons were all at least 18 at the start of 1971, so "family income" refers to parental income less frequently over time.) Sons are asked about income over the previous twelve months but are generally interviewed late in the year (October through December in 1966). Daughter-reported family income received in the past 12 months is available in the 1968 through 1972 surveys, which generally occur early in the year. See the questionnaires at https://www.nlsinfo.org/content/cohorts/older-and-young-men/other-documentation/questionnaires/young-men-questionnaires and https://www.nlsinfo.org/content/cohorts/mature-and-young-women/other-documentation/questionnaires/young-women-questionnaires.

[124] See the questionnaires at https://www.nlsinfo.org/content/cohorts/older-and-young-men/other-documentation/questionnaires/older-men-questionnaires and https://www.nlsinfo.org/content/cohorts/mature-and-young-women/other-documentation/questionnaires/mature-women. NLSOM respondents also report family income in the 1966, 1968, 1969, and 1971 waves, and NLSMW respondents do so in the 1968, 1969, and 1971 waves. The 1968 waves were conducted through the mail and asked respondents to place themselves into an income range, making the measure that year less comparable to other years.

[125] Two-thirds of sons in my sample (67 percent) may be matched to a mother, a father, or both. Fewer—57 percent—of sons reporting income are matched to a parent reporting income. Among daughters, those figures are 72 percent and 59 percent. About half of sons match to a mother while only a quarter match to a father. Among daughters, 59 percent match to a mother, but only one-fifth match to a father. Children can be matched to a parent if their mother was 29 to 43 years old as of the end of March 1966 or if their father was 45 to 59 years old.

[126] Center for Human Resource Research (2001).

[127] I experimented with other created family income variables available in the NLSYM data and with a measure based on self-reported income categories. The results were relatively insensitive.

[128] See https://www.bls.gov/nls/NLS-50th-Anniversary-Conference-Horrigan.pdf.

[129] See https://www.nlsinfo.org/content/cohorts/older-and-young-men/intro-to-the-sample/sample-design-and-screening-process.

[130] https://www.nlsinfo.org/content/cohorts/older-and-young-men/using-and-understanding-the-data/sample-weights.

131 https://www.nlsinfo.org/content/cohorts/mature-and-young-women/intro-to-the-sample/sample-design-and-screening-process

132 See https://www.nlsinfo.org/sites/nlsinfo.org/files/attachments/130212/NLSY79%20Tech%20Samp%20Rpt.pdf and https://www.nlsinfo.org/sites/nlsinfo.org/files/attachments/121212/NLSY79HouseholdScreener_IRM_reduced.pdf. A small number of "difficult cases" were dropped beginning in 1982.

133 See https://www.nlsinfo.org/content/cohorts/nlsy79/intro-to-the-sample/retention-reasons-noninterview.

134 See https://www.nlsinfo.org/sites/nlsinfo.org/files/attachments/121221/TechnicalSamplingReport.pdf.

135 See https://nlsinfo.org/content/cohorts/nlsy97/intro-to-the-sample/retention-reasons-non-interview.

136 See https://www.bls.gov/nls/handbook/2005/nlshc6.pdf and https://www.nlsinfo.org/content/cohorts/older-and-young-men/intro-to-the-sample/retention-and-reasons-non-interview/page/0/1/#attrition.

137 See http://www.bls.gov/nls/ywguide/2001/nlsywg0.pdf.

138 Schoeni and Wiemers (2015).

139 See pages 118 to 126 of https://www.nlsinfo.org/sites/nlsinfo.org/files/attachments/121212/NLSY79HouseholdScreener_IRM_reduced.pdf.

140 I regressed logged parent-reported income on logged child-reported income, using sample weights, to obtain a reliability ratio for son-reported income. Then I estimated the IGE by regressing the logged earnings or income of grown sons on logged son-reported childhood income, using the full sample available in the NLSYM, regardless of whether parent-reported childhood income is available. Finally, I divided this IGE by the reliability ratio. This procedure assumes that the error in son-reported income is random, and it also assumes that the reliability ratio for the restricted sample of sons who may be matched to parents is the same as the ratio that would be obtained if the full sample of sons could be matched to parents.

141 For instance, using unweighted data, the IGE comparing sons' earnings to parent-reported income was 0.24, and the IGE using son-reported income was 0.22, but the IGE after the reliability correction was 0.30. The reliability ratio was 0.75.

142 The NLSY79 estimates in the paper use a measure of parent-reported income from the 1979 survey, assessing income received in 1978. As noted in the previous section, potential civilian sample members were asked about family income received the previous 12 months when they were screened for the NLSY79 in late 1978. By regressing the logged screener measure on the logged measure from the 1979 interview (using sampling weights), I obtained a reliability ratio of 0.78 for sons. That raised the estimated IGE comparing grown sons' earnings to parental family income from 0.32 to 0.41. The change over time between the NLSYM and the NLSY79 was, thus, +0.10 without the reliability corrections and +0.11 with the corrections.

143 Abramitzky, Boustan, Eriksson, Feigenbaum and Pérez (2019), Feigenbaum (2018), Feigenbaum (2016), and Parman (2011) use income measurements in the 1915 Iowa census for mobility analyses. Feigenbaum (2015) uses a Bureau of Labor Statistics cost of living survey of urban married parents conducted in 1918 and 1919.

144 A fire in 1921 destroyed practically all of the 1890 census records, and so researchers cannot analyze data from that year. Prior to 1850, census records are at the household level, indicating the number of household members in a limited number of occupational categories but not the occupations of any individual member (nor any demographic information about any individual members).

145 A few studies compare the statuses of women's husbands and fathers to estimate trends in women's mobility. See Olivetti and Paserman (2015); Choi, Gu, and Shen (2018); Craig, Eriksson, and Niemesh (2019)

146 This approach is related to the one discussed in Part Two of this primer to obtain mobility estimates in countries that lack true intergenerational data. In that approach, father earnings are imputed by looking at the average earnings of men in a given occupation in one dataset and then assigning them as "father earnings" to sons in a second dataset who report their father's occupation. Both strategies are a form of "two-sample two-stage least squares" estimation.

[147] Olivetti, Paserman, and Salisbury (2018) find similar results from 1900 to 1940 when they control for grandparent occupational income, except that mobility falls between 1930 and 1940.

[148] https://assets.aeaweb.org/asset-server/articles-attachments/aer/app/10508/20130821_app.pdf.

[149] In particular, they will be different depending on four considerations: how large is the association between child names and father income, how noisy are the father income averages by child name due to observing a limited number of fathers for each child name, the extent to which the relationship between the father income associated with child name and grown-child income reflects factors other than the relationship between father income and grown-child income, and the extent to which the relationship between father income and grown-child income reflects the influence of things unrelated to child name. See the discussion in Olivetti and Paserman related to their Equations 1 and 2.

[150] Collins and Wanamaker (2020) find that relative mobility, measured using 1940-based occupational earnings scores, rose modestly between 1900 and 1930 for both whites and blacks. This increase appears to have more to do with greater upward mobility out of the bottom rather than more downward mobility from the top. (Using surveys that may not be comparable with each other or with the linked census data, they also find mobility rising between 1930 and 1962 and between 1962 and 1973 before falling between 1973 and 1990, ending at 1962 levels.)

Abramitzky, Boustan, Jacome, and Perez (2019) report that relative mobility, using 1940-based occupational income, was flat for native-born white men between 1910 and 1940 but fell modestly for foreign-born white men.

[151] Ward also shows that measurement error overstates mobility levels.

[152] Even if it were possible, the publicly available census data involve only 1, 5, or 10 percent samples of the full census, so only a small proportion of grown children could be matched to parents.

[153] Technically, they average incomes within a state in the 1960 census, then in the 1970 census, then take the average of these two averages.

[154] In their models that allow the IGE to vary by birth cohort, they interact birth cohort dummy variables with "parent" income, while their model allowing the IGE to vary only by census year lacks those interaction terms. However, all of their models include the birth cohort dummies themselves. Aaronson and Mazumder essentially assume that the IGE varies in the same way for pre-1921 birth cohorts as it does for the 1921–25 cohort, and they assume that the marginal increment to son earnings is the same for pre-1921 cohorts as for the 1921–25 cohort.

AM also assume that the relationship between age and sons' earnings is the same in 1950, 1960, and 1970, perhaps for consistency with the assumption that birth cohorts do not differ between 1910, 1920, and 1930, though they offer no explicit justification.

In additional analyses, AM impute parent income using per capita personal income in birth states around the same time that someone is born. This allows them to extend their analyses further back in time, and lets them observe "parent" data for pre-1921 birth cohorts. The trends differ from their main analyses depending on which model they use (with both year and cohort interactions with "parent" income, or with just one or the other interacting with it).

[155] AM do report that their results were similar to their main results when they ran two different sets of analyses confined to parents of children 0 to 9 years old. In one, they used only a single census for each birth cohort to impute parent incomes, rather than averaging incomes from two censuses. In a second, they did the same but confined the censuses to the pre-1980 ones.

[156] In 1950, a one-in-five subsample received questions about individual income, and if they were family heads, they also received three questions about the income of other family members. After 1950, family income is the sum of individual incomes within families, each of which comes from the sum of component variables. Other year-to-year differences are primarily to do with bottom- and top-coding, the number of income component questions asked, the coding of incomes and components from intervals into point estimates, and editing and allocation when there are data errors.

AM report that they conducted their analyses using father earnings rather than family income and obtained similar results.

157 The data used to impute parental income of adults come from the 1940 through 2000 decennial censuses. I use samples from IPUMS USA, at the University of Minnesota. In replicating the AM estimates, I start with 1 percent samples from the 1940, 1950, 1960, and 1970 censuses and 5 percent samples from the 1980, 1990, and 2000 censuses. (The 1970 sample is the "Form 2 Metro Sample".) In my main analyses, I use a 5 percent sample for 1960 that was released after the AM paper was published and I combine six independently drawn 1 percent samples from 1970. Not all of these would have been available to AM because two do not include state of residence. AM require children to be living in their birth state when they are observed in the data.

I look at boys ages 0 to 19, after dropping everyone living in group quarters, born abroad, or born in an unidentified state. I also drop those under 20 if they are household heads or spouses of household heads. I drop those who are not related to the household head, since family income is available in 1950 only for members of families related to a sample line household head. If there are multiple boys in a family in the same five-year birth cohort, I drop all but one. Initially, following AM, sons who are no longer living in their birth state are also dropped, but in my main analyses I include them.

To assign parental income to sons, initially I follow AM. I average family incomes in two steps. First, I average family incomes across sons within cells defined by 11 five-year birth cohorts (1921–25, 1926–30, 1931–35, 1936–40, 1941–45, 1946–50, 1951–55, 1956–60, 1961–65, 1966–70, 1971–75), 50 states (and the District of Columbia), and six census years (1940, 1950, 1960, 1970, 1980, and 1990). Any income of the son is subtracted from family income before averaging. (In later analyses, I extend the birth cohorts to 1991–95 and the census years to 2010. I also combine several birth states and drop Alaska and Hawaii.)

In the second step, I average these within-cell family income means across census years (but within birth cohorts and states) when the same birth cohort is observed in two census years, equally weighting the two sub-means for both census years. This ensures that the parental income assigned to children is not unduly driven by a particular year of census data.

These birth-cohort-by-birth-state averages are then assigned to men in later censuses, where there is information on everyone's age and birth state. Initially, I look at men ages 25 to 54 in the 1950, 1960, 1970, 1980, 1990, and 2000 decennial censuses; later I add the 2010 and 2019 American Community Surveys. The samples are again from IPUMS USA. Initially, I use the same 1 percent samples for 1950, 1960, and 1970 as above and the 5 percent samples for 1980, 1990, and 2000. Later, I use the 5 percent sample from 1960 and the six 1 percent samples for 1970, as well as 1 percent ACS samples for 2010 and 2019. Once again, I drop those living in group quarters or who were born abroad or in an unidentified state.

No attempt is made to make bottom- or top-coding of the income variables consistent over time, as sensitivity checks indicated it made little difference. While AM adjust incomes for inflation using the consumer price index for all urban consumers (CPI-U), I use the personal consumption expenditures (PCE) deflator. Sensitivity checks indicated it makes little difference.

158 Taken from Column 5 of Table 1 in their paper.

159 The results I present that include "cohort effects" are from the following model (from Aaronson and Mazumder):

In this model, *i* indexes individuals, *b* five-year birth cohorts, *s* birth states, and *t* years in which adult children are observed. is the income of grown child *i*, from birth cohort *b* and birth state *s*, observed in year *t*. Similarly, is the (imputed) parental income of the grown child. The next two terms in the equation interact parental income with birth cohort dummies and with year dummies. The terms in the first brackets interact parental income with a quartic in adult age. Age is centered on 40 for computational ease, since the terms in the brackets then sum to zero. The terms in the second brackets interact year dummies with a quartic in adult age. They, too, sum to zero when computing mobility at age 40. The *u* and *v* terms represent year and birth cohort fixed effects.

For identification, the model drops the dummy indicator for the 1921–25 cohort, its interaction with parental income, the dummy indicator for 1960, and its interaction with parental income. Estimating mobility at age 40 for year *t*, then, involves summing the coefficient and the relevant and coefficients. For instance, a 40-year-old in 1990 is in the 1946–50 birth cohort, so *b*=1946-50 and *t*=1990. A 40-year-old in 1960 was born in 1920. The estimate assumes that the "effect" of being born in 1920 is the same as being born in the 1921–25 cohort, so *b*=1921–25 and *t*=1960. Given that these terms are dropped from the model, the estimate for 40-year-olds in 1960 is simply the coefficient .

The model that excludes "cohort effects" is as follows:

In this model, the interaction of the 1960 dummy with parental income is retained, but the dummy indicators for the 1921–25 cohort and for 1960 are still omitted for identification. Mobility at age 40 for year *t* is given by the relevant coefficient.

All the models use sample weights, but sensitivity checks indicate that running unweighted regression models does not appreciably change the results.

160 This line also drops the AM restriction that the relationship between age and sons' earnings be the same in 1950 and 1960 as in 1970, which raises the 1950 and 1960 estimates in particular.

Including self-employment earnings ensures that sons do not have low wage and salary income simply because they are self-employed and receive substantial earnings that way.

161 My estimates, when I restrict the relationship between age and sons' earnings to be the same in 1950 and 1960 as in 1970, are all within 0.02 to 0.04 of the original AM estimates (not shown).

162 The quartic in age is still included in the model.

163 Not shown in Figure A2, omitting age-by-income interactions has little effect on trend estimates, mostly shifting IGE levels by a similar amount across years. AM report that they estimated models excluding these interactions and also found their results were unaffected.

164 Specifically, I combine Maine, New Hampshire, and Vermont; Connecticut and Rhode Island; Delaware, Washington DC, and Maryland; Montana, North Dakota, and South Dakota; Colorado and Wyoming; Idaho and Utah; Arizona, Nevada, and New Mexico; and Oregon and Washington.

165 AM cite Card and Krueger (1992) for excluding these children, though in my reading the paper offers no justification for their decision. By requiring sons to still be in their birth states, there is a potential selection issue that interacts with child age. The sons who remain in their birth state at age one are a much more representative group than the sons who remain in their birth state at age 19.

166 The 5 percent 1960 sample became available on the IPUMS website in 2016. The 1970 samples were available when the AM paper was published. There are six independently drawn 1 percent samples for 1970 that can all be used for the mobility analyses, although only four include current state of birth, which is necessary for defining the AM sample. I divide all of the weights for these samples by six in my analyses, so that the pooled sample is weighted up to the 1970 population.

167 AM prefer using the log of cell means because they say it allows them to include non-positive incomes in their cell averages and because it makes their analyses consistent with additional ones they present that are not based on microdata. They also cite sensitivity checks they conducted that showed the distinction not to be substantively meaningful.

168 I confirmed I could replicate the created family wage and salary income variable included in the IPUMS data in 1940 and then recreated it for subsequent years. Unlike the IPUMS variable, I leave the value missing for people outside the primary family, whereas the IPUMS variable assigns them the value for the primary family. I also subtract the wage and salary income of sons from this amount.

Family earnings—including self-employment income—are unavailable across these years because earnings are not available in 1940.

169 For example, in supplementary analyses, they impute parent income from birth-state-by-ancestry-group means, finding the IGE doubled from 1980 to 1990.

170 AM report that when they estimated their model using only a single census for each birth cohort (in which boys were observed either as 0- to 4-year-olds or 5- to 9-year-olds), their results were not affected. The approach here keeps sons observed between the ages of 10 and 19 but picks a single census to which they are matched

(when sons are 0–4 or 5–-9, or when they are 10–14 or 15–19). This approach allows for controlling the year in which "parent" income is measured.

[171] Specifically, if an adult son can be matched to "parent" income averages from birth-cohort-by-birth-state cells in two different censuses, I randomly match him to the cell average from a single census. I then include in my models dummies for the census from which the mean was taken (omitting 1960) and dummies for the age ranges of children in that cell (omitting 0–4). I also ran models where I treated sons that matched to birth cohorts in two different censuses as two observations, with different values for "parent" income, for the year in which parent income was estimated, and for child age when parent income was estimated but the same values on all other variables. The results were extremely similar to those shown.

Unless otherwise noted, these models include the same variables as in the AM models, including interactions of age with the year in which sons' earnings are observed and interactions of age with "parent" income.

[172] The change in the trend is not due to having an insufficient number of observations when I switch from using mean parent income (by birth cohort and birth state) for one census year rather than two. In no cell do I have fewer than 100 observations, and outside 1950, most cells have 1,000 people or more. There are over 7.3 million sons in the analysis that produced the trend estimates.

[173] When I tried estimating models that included interactions of "parent" income with cohort, year in which grown sons' earnings were observed, year in which "parent" income was measured, age, and child age, I ran into collinearity problems. The same was true when I tried estimating models that included all of these except for the year in which "parent" income was measured.

[174] In response to the argument presented here, Mazumder communicated that he and Aaronson believe that the alternative models I present are misspecified or lack *a priori* justification. He emphasized that the models generally reinforce their finding that mobility fell during the 1980s.

ABOUT THE AUTHOR

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