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The Decline in Intergenerational Mobility After 1980

Jonathan Davis and Bhashkar Mazumder

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Jonathan Davis
Department of Economics, University of Oregon
516 PLC, 1285 University of Oregon, Eugene, OR 97403
Jdavis5@uoregon.edu

Bhashkar Mazumder
Federal Reserve Bank of Chicago and University of Bergen
230 S LaSalle St, Chicago, IL 60604
bhash.mazumder@gmail.com

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Abstract

We demonstrate that intergenerational mobility declined sharply for cohorts born in the early 1960s compared to those born around 1950. The former entered the labor market largely after the large rise in inequality that occurred around 1980 while the latter entered the labor market well before this inflection point. We show that the rank-rank slope rose from 0.22 to 0.37. We document concurrent increases in the education-parent income gradient, in the returns to education, and in the marriage-parent income gradient and provide suggestive evidence that the increase in the education-parent income gradient explains much of the increase in persistence.

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I. Introduction

One of the most notable changes in the U.S. economy in recent decades has been the rise in inequality. A key inflection point appears to be around 1980. During the early 1980s there was a pronounced increase in the 90-10 income gap and a sharp rise in the income share of the top 1 percent (see Figure 1). At the same time, consumption inequality (Meyer and Sullivan, 2013), the returns to education (Goldin and Katz, 1999), and income segregation (Reardon et al, 2018) all grew markedly. With the advent of a more unequal society, concerns about a possible decline in inequality of opportunity have risen to the forefront of policy discussion in the U.S.

To better understand inequality of opportunity, researchers have increasingly focused attention on studies of intergenerational mobility. These studies estimate the association between parent income and the income of their offspring as adults. If the strength of the association is high, it suggests that there may be a low degree of intergenerational mobility as a family's position in the income distribution is largely replicated from one generation to the next. In contrast, if associations are relatively small, then we might infer that there is a high degree of mobility as families are more likely to move up and down the income distribution. The consensus view is that intergenerational income mobility in the U.S. is low compared to other advanced economies (Black and Devereaux, 2011). Further, high inequality countries tend to have lower rates of intergenerational mobility –a pattern that has been referred to as the "Great Gatsby curve" (e.g. Corak, 2013).

Has it always been the case that intergenerational mobility has been low in the U.S.? Between 1948 and 1973, for example, the U.S. economy experienced a long period of relatively rapid economic growth and much lower inequality than in the period since. One might wonder whether intergenerational mobility might have been much more rapid for individuals who entered the labor market during this so-called "golden age". Interestingly, there is very little evidence on this point.[1] Due to data limitations, few studies have followed a large sample of individuals who entered the labor market during this golden age. Three studies that have used data from the Decennial Censuses have found evidence of a decline in intergenerational mobility after 1980 but these studies have relied on more indirect methods.[2]

We present the first evidence to fully utilize the longitudinal income data reported by both parents and children in the National Longitudinal Surveys (NLS) to *directly* measure changes in intergenerational mobility for large samples of relevant cohorts.[3] We document that intergenerational mobility was substantially lower for cohorts born in the early 1960s who entered the labor market after the rise in inequality compared to those born around 1950.[4]

The NLS are uniquely suited to address this question because they utilize cohort-based sampling frames providing large samples of both the "pre" and "post" cohorts. In contrast, several previous studies on trends in intergenerational mobility

[1] Studies of earlier historical periods (Long and Ferrie, 2013; Olivetti and Paserman, 2015) suggest that intergenerational mobility fell early in the 20th century but those studies do not examine trends in more recent decades.

[2] Aaronson and Mazumder (2008), which we discuss in more detail later in the paper, use a synthetic cohort approach linking groups of parents and children across Decennial Censuses by state and year of birth of the child. Hilger (2017) uses historical cross-sectional Census data to estimate long-run trends in a different concept of intergenerational mobility, educational mobility. Chetty et al. (2017) study cohorts born as far back as 1940 but only with respect to absolute mobility which they define as the share of children whose income exceeds that of their parents. They do not study *relative* mobility which has been the focus of the literature since it better captures equality of opportunity. We discuss their findings in more detail in Section IVB.

[3] Levine and Mazumder (2002) and Bloome and Western (2011) do not use the self-reported family income in the NLS66's "Older Men" and "Mature Women" samples and instead use a categorical family income measure in the young men's survey (which could also reflect some of the child's own earnings). Because they only use the NLS66's young mens sample (discussed in more detail in the next section), they only measure children's income until they are in their late 20s and early 30s. Levine and Mazumder (2007) only look at changes in the brother correlation in income. In particular, they do not look at changes in the association between parent and child income measures.

[4] We restrict attention to cohorts in the NLS for which children were 18 or younger in the year of the first interview.

have relied on the Panel Study of Income Dynamics (PSID).[5] While the PSID is a valuable resource for intergenerational analysis, it has limitations that make it less suitable for studying longer term trends (Mazumder, 2018). Since it does not use a cohort-based sampling frame, there are only about half as many individuals in the cohorts of interest in the PSID providing much less power to detect statistically significant changes.[6] Nevertheless, as we discuss later, our results are consistent with several recent PSID papers that have found striking increases in intergenerational persistence.

The NLS are also superior to currently available administrative records for studying *trends* in intergenerational mobility. For example, the population-wide tax records used by Chetty et al. (2014A) only cover 17 years (1996 to 2012) and cannot be used to estimate time trends covering cohorts born in the 1940s.[7] In order to establish the basic facts on trends in intergenerational mobility, researchers must either use the best available survey data or utilize indirect approaches. The NLS are the best suited survey data since they contain large samples of the cohorts needed for studying trends around the rise in inequality. Therefore, going forward the NLS estimates should form the baseline view on trends in intergenerational mobility.

For our main analysis we measure intergenerational mobility using the rank-rank slope. We show qualitatively similar results using the intergenerational elasticity (IGE) in Appendix V. The rank-rank slope describes the rate of intergenerational persistence in ranks providing a measure of *positional* mobility. The IGE provides a measure of intergenerational income persistence and can be

[5] See Hertz (2007), Lee and Solon (2009), Hartley et al. (2017), Justman and Krush (2013), and Justman et al. (2017).
[6] The PSID began with only about 5000 families in 1968. Since intergenerational mobility studies typically require that children are below a certain age (e.g. 17 or 18) in order to avoid selecting families where children are living at home at late ages, only cohorts born starting around 1950 are usually included.
[7] We also show that our findings are consistent with Chetty et al (2014B) who use tax records to estimate very short time trends in income rank mobility from 2000 to 2012.

used to infer how long gaps in log income across families are likely to persist. The IGE is also inclusive of changes in inequality over generations. In both cases, a higher figure indicates greater persistence and less mobility. We show that by both measures persistence increased markedly between these two cohort groups. The rank-rank slope rose from 0.22 to 0.37 while the IGE rose from 0.28 to 0.52.

To put this in context, the time variation in US rank mobility is of a similar magnitude to the geographic variation in rank mobility documented by Chetty et al. (2014A) for the 1980-82 cohorts. The cross-cohort decline in mobility is the equivalent of moving from the 2nd percentile city (MSA) to the 67th percentile city.[8]

We document three notable cross cohort changes that could explain the increase in intergenerational income persistence.[9] These include: the steep rise in the association between parent income and children's education; the increase in the returns to post-secondary education; and the increase in the gradient between parent income and whether a child ever became married. While each of these patterns warrants further investigation, we find that the association between parent income and children's education most consistently accounts for the increase in persistence. However, we urge caution as even this evidence is somewhat sensitive to how we measure parent income.

II. Data

We use the National Longitudinal Surveys of Older Men and Young Men and Mature Women and Young Women (NLS66) and the National Longitudinal

[8] See Chetty et al.'s (2014A) online table 4 that presents estimates for 381 MSAs.
[9] We explore whether concurrent changes in the economy can explain the observed decline in intergenerational mobility across cohorts. Nybom and Stuhler (2016) show that changes in intergenerational mobility may not even necessarily reflect contemporaneous events and in principle, could be due to changes in policy or the economic environment that occurred well in the past.

Survey of Youth 1979 (NLSY79). We construct our samples to maximize comparability across these surveys.

A. National Longitudinal Surveys of Older Men and Young Men and Mature Women and Young Women (NLS66)

The NLS66 separately sampled young men who were 14-24 years old on March 31, 1966, young women who were 14-24 on December 31, 1967, older men who were 45-59 on March 31, 1966, and older women who were 30-44 as of March 31, 1967. The surveys frequently include respondents from common households. In each of the four surveys we are also able to gather information on the spouses of individuals as well as the total family income of a household. Therefore, even if the linkage is from say, a son in the young men's sample to a mother in the older women's sample, we are also able to potentially gather information on the father when he is present, and total family income will include both parents' income.

To avoid over-representing children who live with their parents in adulthood, we restrict our sample to parent-child households where the child was 18 or younger in the first survey year.[10] This implies that our NLS66 sample includes daughters born between 1949 and 1953 and sons born between 1948 and 1952.

Our NLS66 sample consists of 1,802 parent-son pairs and 1,735 parent-daughter pairs.[11] For children linked to a father in the Older Men survey (about a

[10] We show that our results are robust to changing this sample restriction in Appendix III.
[11] An earlier working paper of this paper only used father-child pairs. Because of the NLS66's cohort-based sampling, fathers in the Older Men survey who were linked to a son or daughter in the NLS66 were all 27 to 46 years old at the time of that child's birth. Mothers in the Mature Women survey who were linked to a child in the NLS66, on the other hand, were all 12 to 31 years old at the time of their child's birth. As a result, adding the NLS66's mother-child pairs to our analysis made our sample more representative, by including younger parents, and more than doubled our sample size.

third of the sample), we measure total family income using income reports from 1966, 1967, and 1969.[12] Fathers ranged from 44 to 64 years old[13] and were 50.4 years old, on average. Mothers, who were present and had positive earnings in at least one year in 55% of cases, ranged from 26 to 62 years old and were 45.6 on average. The age range is wider for mothers than fathers because father's ages were restricted by the NLS66 sample frame. Daughters were 12 to 19 and sons were 13 to 20.[14]

For the remaining children who were only matched to a mother in the Mature Women survey, we measure total family income using reports from 1967, 1969, and 1971 when mothers were 28 to 46 years old (38.9 on average). Fathers, who were present with positive earnings in 82% of cases, were 22 to 73 years old and 41.7 on average. Mirroring the father-child linkages, fathers' ages are more dispersed here because mothers' ages are restricted by the NLS66 sample frame. Daughters were 13 to 21 and sons were 14 to 22.

We measure daughters' adult family income using the average of all available total family income from 1991, 1993, 1995, 1997, 1999, and 2001 from the Young Women surveys when the daughters were 37 to 51 years old. Finally, we measure sons' adult family income using all available total family income reports from the 1978, 1980, and 1981 Young Men surveys when sons were 25 to 32 years old. Income for both generations is non-missing for 1,269 parent-son pairs and for 1,252 parent-daughter pairs.[15]

Unlike the Young Women's survey which continued through 2003, the Young Men's survey was discontinued more than 20 years earlier in 1981.

[12] Income is always reported for the prior year.
[13] Daughters in our parent-daughter sample were linked to two fathers in the older men sample who turned 62 and 63 in 1966.
[14] We limit parents' income to three years to make the analysis parallel with the NLSY79 where we observe three years of family income as described later.
[15] Appendix I discusses missingness of income in both surveys.

Therefore, for the NLS66, we only observe sons relatively early in their career at an average age of around 29. In contrast, daughters are followed into the prime of their careers at an average age of around 43. Due to lifecycle bias (Haider and Solon, 2006), IGE estimates and to a lesser extent, rank-rank slopes, are typically lower when children are measured early in their career rather than during the prime of their life cycle (Nybom and Stuhler, 2017; Mazumder, 2016). Our preferred analysis uses parent-daughter pairs since we observe income during prime earning years for both cohort groups. However, we also show estimates for the parent-son pairs for the NLS66 sample where we expect life cycle bias to attenuate the estimates.

We weight our analysis using the child's survey weight in the first round of the survey.[16]

B. National Longitudinal Survey of Youth 1979 (NLSY79)

For the NLSY79, we combine a nationally representative sample of 6,111 individuals and an oversample of 5,295 Hispanic, Black, and economically disadvantaged non-Black, non-Hispanic individuals. The sample is representative of the 1979 population living in the U.S. born between 1957 and 1964.

We construct our NLSY79 sample to mirror the NLS66 samples as closely as possible. We mimic the restriction in the NLS66 that children must be matched to a father in the Older Men survey or a mother in the Mature Women's survey by requiring that children have either a father or mother in the age ranges implied by the NLS66's sample frames. For example, to be included in our sample, daughters

[16] These weights are necessary to make the survey samples representative of the national samples they target because both surveys used stratified sampling designs with heterogeneous probabilities of being sampled (Solon et al. 2015). Using weights is especially important given our use of the rank-rank slope with ranks defined by parents' and children's rank in their respective marginal income distributions.

in the NLSY79 either had to have a father who was between 28 and 46 years old at the time of their birth, mimicking the NLS66 daughter to father linkage, or a mother who was between 14 and 31 at the time of their birth, mirroring the NLS66 daughter to mother linkages. Likewise, we require sons be matched to fathers who were between 27 and 44 or whose mothers were between 12 and 29 at the time of their birth. We restrict our sample to parent-child households where the child was 18 or younger in the first survey year. Consequently, our NLSY79 samples include children from the 1961 to 1964 cohorts.

We use the same parent income measures in both the parent-son and parent-daughter samples. Parents were asked to report total family income from the previous year in 1979, 1980, and 1981 when their children were still living at home. Our parent income measure averages all non-missing family income for up to three years, less any income of the youth. For the subsample of youth with a father in the required age range, fathers were 42 to 59 years old and 49.7 on average. Mothers, who were present in the household and had positive earnings for 70% of youth, were 30 to 59 years old and 45.8 on average. For the subsample of youth with a mother (but not a father) in the required age range, mothers were 29 to 50 years old and 38.5 on average during these years. This group of youths' fathers, who were present in the household with positive earnings in at least one year for 76% of youth, were 29 to 46 years old and 40.1 on average. Both sons and daughters were 14 to 19 years old during these years.

Our adult income measures differ across the NLSY79 parent-daughter and parent-son samples. For the parent-daughter sample, we use the average of all non-missing measures of total family income in the previous year from the 2002, 2004, 2006, 2008, 2010 and 2012 surveys when the women were 37 to 50 years old. For the parent-son sample, we use the average of all non-missing measures of total family income from the 1991, 1992, and 1994 surveys when the men were 26 to 32 years old in order to mimic the data restriction in our NLS66 sample. We also produce estimates using sons at their prime age to show what we would find if we used the same measurement approach that we use for daughters in the NLSY79.

As with the NLS66, we weight our analysis using the child's survey weight in the first round of the survey.

III. Methods

We estimate summary measures of intergenerational mobility in the NLS66 and NLSY79 using the following regression:

$$M_{1is} = \alpha + \beta \times I_{is} + \gamma^{NLS66} M_{0i} \times (1 - I_{is}) + \gamma^{NLSY79} M_{0i} \times I_{is} + \varepsilon_{is}, (1)$$

where i indexes parent-child pairs and s denotes pair i's survey. M_0 and M_1 are income measures for the parent and child generations, respectively. I_s is an indicator for being in the NLSY79 sample. γ^{NLS66} and γ^{NLSY79} are intergenerational mobility measures for the NLS66 and NLSY79, respectively.[17]

We measure income using parent and child rank in their respective generation's income distribution. Here, the coefficients γ^{NLS66} and γ^{NLSY79} are interpretable as rank-rank slopes for the NLS66 and NLSY79 cohorts, respectively. Analogous estimates using log income are shown in Appendix V. In that case, γ^{NLS66} and γ^{NLSY79} represent the intergenerational elasticity (IGE) for each cohort. When using log income, the small number of parent-child pairs with negative or zero total family income are dropped from the analysis.

[17] While we do not include controls for parent age at birth or children's birth year in our main specification, we show in Appendix III that our results are highly robust to their inclusion.

Appendix II shows binned scatter plots for both of our income measures and for our parent-daughter and parent-son samples. The figures demonstrate that the relationships are all approximately linear, justifying our linear specifications.

IV. Results

Estimates of γ^{NLS66} and γ^{NLSY79} from our rank-rank regressions are shown in the first column of Table 1.[18] Panel A shows our preferred estimates for matched parent-daughter pairs since they include income during the prime earning years.[19] $\hat{\gamma}^{NLS66}$, which is interpretable as the rank-rank slope among parent-daughter pairs in the NLS66, is 0.22. In contrast, the rank-rank slope among NLSY79 parent-daughter pairs, $\hat{\gamma}^{NLSY79}$, is 0.37, which indicates a 71 percent increase in rank persistence. As a benchmark, we can compare these rank-rank slopes to estimates by city (MSA) reported in Chetty et al (2014A). The rank persistence for cohorts born between 1949-1953, 0.22, corresponds to the 8th most mobile city out of the 381 cities (about the 2nd percentile) in Chetty et al (2014A)'s data. Among the NLSY79 cohorts, born just over a decade later, rank persistence corresponds to the 256th most mobile city (about the 67h percentile). The difference between rank persistence across these two cohorts is statistically significant at conventional levels ($p < 0.01$).[20]

[18] Analogous estimates measuring income using parent and child's log incomes are shown in appendix Table A2 and described in Appendix V. While the results are substantively quite similar, the differences are highlighted in the main text.

[19] Recall that the NLS66's young men cohort was only followed until 1981 before their prime earning years whereas the cohort of young women was followed until 2003.

[20] Appendix III shows that our estimates of the decline in intergenerational mobility are robust to the inclusion of birth year fixed effects or birth year fixed effects and a quartic polynomial in parent age at birth. Appendix III also shows our results are robust to changing our sample restriction that children be 18 or younger at their first interview. We estimate similar declines if we restrict our sample to children who were 16, 17,18, 19, 20, 21, 22, or any age at their first interview.

Panel B shows estimates using the parent-son samples from the two surveys. These estimates suggest that the rank-rank slope increased from 0.17 to 0.36, or 110 percent, for cohorts born around 1963 compared to cohorts born around 1951. This difference is also statistically significant at conventional levels ($p < 0.01$).

We construct our NLSY79 parent-son sample in order to best match the features of the NLS66's parent-son pairs. To demonstrate the impact of these features on the estimates, we show a second version of estimates for the NLSY79 parent-son pairs using the income measures used for the parent-daughter analysis. Using income during prime earning years instead of early career income increases the estimate by 7 percent.[21]

The remaining columns of Table 1 consider potential explanations for why persistence increased. Columns 2 and 3 explore changes in education and columns 5 and 6 consider changes in marital patterns.

In column 2 we find a large and statistically significant increase in the association between parent rank and children's years of completed education. For daughters, the estimates rise by 38 percent, from 0.021 to 0.029, and for sons by 28 percent (from 0.025 to 0.032). Both changes are statistically significant ($p = 0.03$ for both daughters and sons). These estimates suggest that the expected education gap between children whose parents were at the 75th percentile compared to those who were at the 25th percentile increased by about 0.4 years across these cohorts.[22]

How much of the increase in persistence between the two cohorts can be explained by this increase in the parent rank-education gradient? To address this,

[21] This is consistent with the findings in Mazumder (2016).
[22] Hendricks et al. (2018) find that the relationship between parent income and college attendance has declined over time as college attendance has become more strongly correlated with student ability. However, they show that this reversal had largely occurred by the 1960 cohort of high school graduates who were slightly older than our NLS66 cohorts.

we use the Current Population Survey's Annual Social and Economic Supplement (CPS-ASEC) to calculate average family income by years of education for men born between 1948 and 1952 and for women born between 1949 and 1953 (the NLS66 cohorts) during the same years as in our sample. We then generate a simulated rank for each observation in our NLS66 and NLSY79 samples using the average income for their level of education among NLS66 cohorts, rather than their actual income. The idea is to hold the returns to education constant at their NLS66 levels and to see how the change in the association between parent rank and education translates into changes in rank persistence. If the parent rank-education gradient was unchanged, we would find no difference in the simulated rank-rank slopes across the two cohorts. In fact, we find that the increase in the gradient increases the rank-rank slope by 0.07 for women and by 0.06 for men. Simultaneously estimating this counterfactual model and the true rank-rank regression, we can only reject that the observed 0.15 increase in the rank-rank slope for women is different than this simulated 0.07 difference at the 10 percent level ($p = 0.06$). For men, on the other hand, we can reject that the observed 0.19 increase in the education-parent rank slope is different than the 0.06 increase in this counterfactual rank-persistence at conventional levels ($p = 0.01$).[23]

This increase in the association between parent rank and education coincided with a similarly large increase in the returns to education. As shown in Figure 2, Goldin and Katz (2009) find that the returns to college increased by nearly 50 percent for prime-aged white men between the 1980 and 1990 census. We can also directly measure the returns to education for the NLS66 and NLSY79 samples by similarly estimating average income by cohort, education, and gender using the

[23] The education-parent rank results are stronger than the education-parent log income results in Appendix V. This difference is driven by the inclusion of parents with non-positive income. Given the sensitivity of these results, we are inclined to view the rank-rank results as suggestive of a potentially interesting mechanism that warrants further investigation.

CPS-ASEC. We define the returns to each level of education as the difference between average income at that level and the average income for individuals in the same cohort with exactly 12 years of education. Figure 3 plots our estimates of the returns to education for women born in the NLS66 cohorts (the solid blue line) and NLSY79 cohorts (the dashed red line). Analogous estimates for men are shown in Appendix IV. The estimates use family income measures in the same years as our NLS income averages.[24] Because we are using family income, this return is interpretable as a total effect of education, including, for example, the impact of education on marital partner and their income.

Similar to Goldin and Katz (2009), we find that the returns to having a college education increased. On average, women with 16 years of education's family income was \$50,817 higher than that of women with 12 years of education for the NLS66 cohorts. This income gain increased by a third to \$67,614 for women in the NLSY79 cohorts. There was little change in the returns to having fewer years of education. If anything, the penalty for having fewer than 10 years of education was lower for the NLSY79 cohort.

We use these estimates to assess how much of the increase in persistence is due to the increase in the returns to education. We generate a counterfactual income measure for each NLS66 observation by subtracting our estimated return to their level of education among the NLS66 cohort and adding in in the estimated return in the NLSY79.[25] Column 3 shows rank-rank estimates that are analogous to those in Column 1 only using this counterfactual income measure to generate child rank for the NLS66 sample. With this adjustment, the differences in the NLS66 and

[24] Women with 8 years or less of education are all grouped in the 8 years of education category. Similarly, women with 16 years or more of education are grouped in the 16 years of education category.
[25]For example, this counterfactual income would be \$16,797 higher for an NLS66 daughter with 16 years of education since the return increased by this amount.

NLSY79 cohorts' rank-rank slopes for daughters and sons decline by 17 percent and 25 percent, respectively, but continue to show large and statistically significant increases in persistence over time. This suggests that increases in the returns to education can only explain some of the cross-cohort increase in rank persistence.

Column 4 documents another striking difference between the two cohorts: the gradient between parent rank and the probability of being married in any of the years (times 100) was nearly flat for the NLS66 cohorts but became much steeper for the NLSY79 cohorts. The estimate for daughters in the NLSY79, 0.24, implies that a child with parents at the 75th percentile is 11.85pp more likely to be married than a child with parents at the 25th percentile. In contrast, the estimate for daughters in the NLS66, 0.06, implies a difference of only 3pp in the probability of being married for children with this same difference in parent ranks. The difference between these estimates is statistically significant ($p < 0.01$). While the estimates using the parent-son sample have the same implication that the gradient between marriage and income became much more positive, increasing from -0.08 to 0.14, the coefficient for the NLSY79 son sample is about 40 percent smaller than the coefficient for the daughter sample. This is because marriage, like income, is measured at younger ages in the sons sample compared to the daughters sample.

Since we are using family income, this change in the gradient between marriage and income could be an important factor in explaining the increase in the persistence between the two cohorts. To address this, Column 5 shows estimates of the rank-rank slope estimated using the child's rank in the own income distribution instead of the family income distribution (family income is still used for the parent generation). In contrast to the other rank-rank estimates, these estimates suggest that persistence may not have changed for daughters. The point estimates suggest persistence increased by 19 percent between the two cohorts, but this difference is not statistically significant ($p = 0.51$). For sons, however, the estimates suggest an even larger increase in persistence, from 0.10 to 0.36, a difference which is statistically significant ($p < 0.01$). This suggests that greater persistence in the economic fortunes of men across generations may be driving the increased persistence in family income observed for women.

A. Discussion

We document that cohorts who entered the labor market well before the rise in inequality that took place around 1980 experienced significantly higher rates of intergenerational mobility than those who entered the labor market afterwards. While new, this finding can be reconciled with the literature relatively easily.

Aaronson and Mazumder (2008) provide a useful framework for considering our results and those of the existing literature. Figure 2 plots a replication of their estimates of the intergenerational elasticity using Census data from 1940 to 2010.[26] Their estimates use a group-based estimation strategy where the average income of groups of individuals defined by state and year of birth is linked to the average income of a synthetic group of parents in a prior Census who had children in the same state and year.[27] The estimates in Aaronson and Mazumder are plotted by the year of income of the child and not by their birth

[26] While we follow Aaronson and Mazumder (2008) and label the results by the year of the Census, the estimates are based on income measured in the year prior to the Census.
[27] One advantage of our analysis is that we do not need to resort to an indirect method, like Aaronson and Mazumder's synthetic cohort approach. Aaronson and Mazumder (2008) show that compared to the traditional IGE, their approach will give more weight to state-level geographic effects which could bias estimates of trends if these effects change over time. In a supplemental analysis they show that their results are robust to using state fixed effects suggesting that trends in geographic effects are not driving their results.

year.[28] They document an increase in intergenerational mobility after 1940 and a decline after 1980 that closely tracks changes in the return to college.

Our earlier cohorts, born between 1948 and 1953 entered the labor market during the 1960s and 1970s, well before the increase in inequality around 1980. The latter group of cohorts, born between 1961 and 1964 in contrast, largely entered the labor market after the pronounced rise in inequality.[29] While our study is the first to fully utilize the longitudinal parent and child income data available in the NLS, it is worth noting that Bloome and Western (2011) also document a significant increase in the IGE across these same cohort groups. However, they used a categorical family income measure included in the Young Men's survey rather than the self-reported income of parents. Similarly, Levine and Mazumder (2007) show that the *sibling* correlation in log wages, log annual earnings and log family income rose by a similar amount between the NLS66 and NLSY79. The sibling correlation is a broader measure of family background that encompasses other shared factors besides parent income.

Seemingly at odds with our findings are the results of Hertz (2007) and Lee and Solon (2009) who show relative stability in IGE trends using the PSID. However, both the significantly smaller sample size and the more limited range of birth cohorts makes the PSID much less suited to picking up changes in the IGE around the inflection point in inequality in 1980. The PSID includes about half as many children in the NLS66 cohorts than the NLS. These small samples in the PSID produce much noisier cohort by cohort estimates making it harder to rule out a change in trend for the specific cohort groups we examine. For example, the point

[28] Their preferred estimates utilize controls for birth cohort bins, a quadratic in age (minus 40) and Census year effects and most flexibly control for these factors. Their estimates can be interpreted as the IGE for a 40-year-old in a given year accounting for birth cohort and year effects.

[29] If most individuals enter the labor market between the ages of 18 and 25, this would imply that the 1948 to 1953 cohorts entered the labor market between 1966 and 1978 and that the 1961 to 1964 cohorts entered the labor market between 1979 and 1989.

estimates of the IGE in Lee and Solon (2009) for women observed in the years 1977 through 1979 range from 0.05 to 0.20 with standard errors between 0.12 and 0.17. The point estimates for women observed 10 years later, range from 0.50 to 0.56 with standard errors between 0.08 to 0.09. While the increase in point estimates is suggestive of a fairly large increase in the IGE comparable to what we find with the NLS in Appendix V, the large standard errors for the oldest cohorts makes it difficult to distinguish an increase from noise.[30]

Moreover, several more recent papers using the PSID (Hartley et al., 2017; Justman and Krush, 2013; and Justman et al., 2017) have documented sharp increases in the IGE over time making it less clear that Hertz (2007) and Lee and Solon (2009) should be viewed as the final word on trends when using the PSID.[31]

The empirical results on income mobility from Chetty et al. (2014B) show that the rank-rank slope stayed roughly constant over the 2001 to 2012 period using cohorts born from 1971 to 1982 and observed at age 30. These results are easily reconciled with our findings as these individuals would have entered the labor market starting in the late 1980s at the earliest and in the late 1990s on average. Our replication of Aaronson and Mazumder (2008) also shows relative stability in intergenerational mobility between 2000 and 2010 (Figure 2). Furthermore, if we replicate Goldin and Katz's estimates of the return to college we similarly find relative stability from 2000 to 2010.[32] Together, this all suggests our estimates are

[30] Further, since the PSID begins in 1968, one cannot observe a representative group of children born in the 1940s living at home with their parents. The PSID can be used to observe cohorts born starting around 1950 who would have been 18 at the time of the very first survey but would miss the earlier birth cohorts available in the NLS.
[31] For example, Figure 5 in Hartley et al. (2017) shows that the IGE in log family income for daughters rose from around 0.25 in 1980 to around 0.55 by the early 2000s which is very similar to the magnitude of the both levels and change in the IGE that we find with the NLS. Recall, that we find that the IGE grew from 0.28 to 0.52 in Appendix V.
[32] Autor (2014) similarly finds relative stability in the returns to college over the same period using annual CPS data.

consistent with the available estimates on trends based on administrative tax records.

Our results are complementary to Chetty et al. (2017) who show that absolute mobility, defined as the share of children whose income exceeds that of their parents, fell from about 90% for children born in 1940 to 50% for children born in the 1980s.[33] Since they do not observe panel data on parent and child income for cohorts born before 1980, they estimate these results by assuming the copula between the marginal distributions of parent and child income is stable for cohorts born before 1980. However, they show that there is evidence of a decline in absolute mobility for any plausible copula. While both of our analyses suggest a decline in mobility, absolute mobility is a fundamentally different measure than relative mobility and the latter better captures equality of opportunity.[34]

We explore several possible mechanisms and suggest that the sharp rise in the gradient between parent income rank and children's education is the most compelling and can explain nearly half of the increase in rank persistence we observe for women and a third of the increase we see for men. However, this result is sensitive to whether observations with non-positive parent income are included so caution is warranted. We believe that further research is needed to more definitively understand the mechanisms behind the decline in intergenerational mobility which we document here.

[33] We estimate absolute mobility fell from 71% to 57% in our preferred parent-daughter sample ($p < 0.01$). In an earlier draft of this paper we found little change in absolute mobility but discovered we were not fully utilizing the available NLS data. See footnote 11 for a more detailed discussion of our sample changes.

[34] For example, if all children earned more than their parents but stayed at the exact same rank, then absolute mobility would be 100% but our relative measures would indicate that there is no relative mobility.

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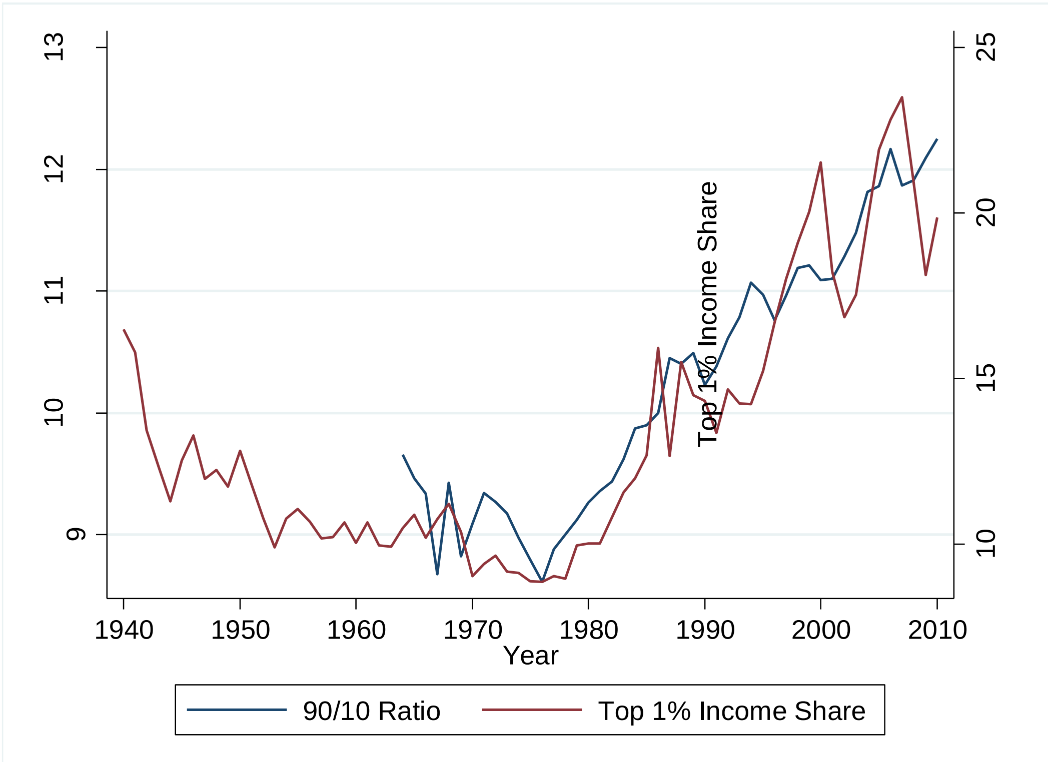
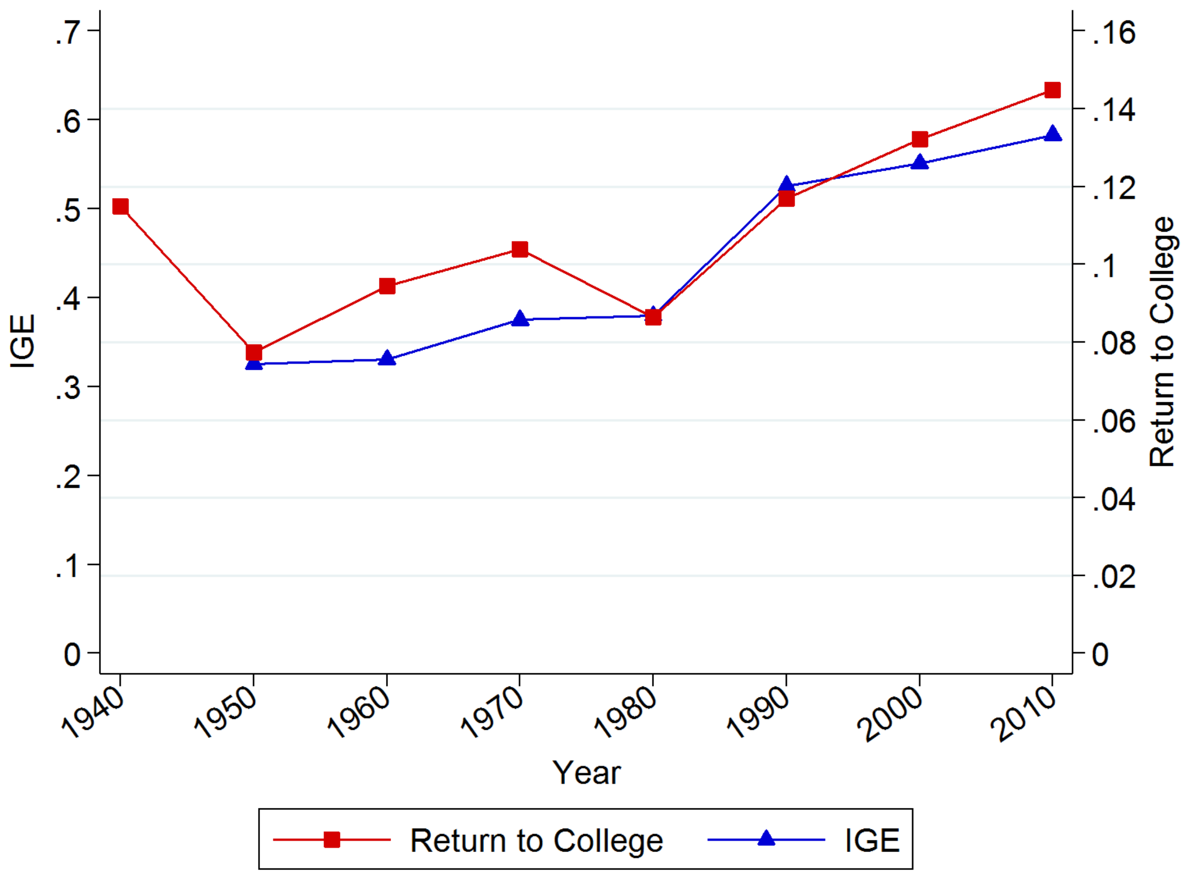
Figure 1. 90/10 Ratio and Top 1% Income Shares, 1940-2010



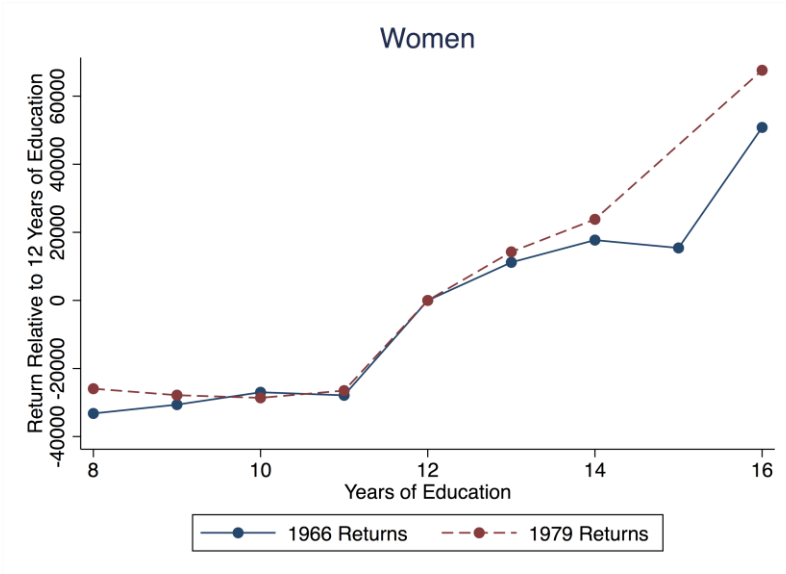
Notes. 90/10 ratio based on authors' calculations using Current Population Survey Annual Social and Economic Supplement data from 1964 to 2010. Estimate are based on average pre-tax family income among the sample of household heads weighted by the supplement weights. Top 1% income shares based on estimates reported in Piketty and Saez (2003). The updated series was downloaded from The World Wealth and Income Database (Alvaredo et al) on December 20th, 2016.

Figure 2. Trends in the IGE and Returns to College



Notes. Authors' replication of Aaronson and Mazumder (2008), Figure 4.C extended to include 2010. Return to college estimated using the methodology of Goldin and Katz (2009), also extended to 2010. All calculations use decennial census and ACS data.

Figure 3. Returns to Education



Notes. This figure plots the returns to education for women born in the NLS66 cohorts (solid blue line) and NLSY79 cohorts (dashed red line). Estimates use data on the same cohorts in the Current Population Survey's March Supplement taking family income measures in the same years as our NLS income averages. Women with 8 years or less of education are all grouped in the 8 years of education category. Similarly, women with 16 years or more of education are grouped in the 16 years of education category.

Table 1. Rank Mobility among NLS66 and NLSY79 Parent-Daughter and Parent-Son Pairs

	Rank-Rank	Years of Education	Rank-Rank with 1979 Ed Returns	Married during any income years	Rank-Rank using Child's Own Income
		Panel A. Parent-Daughter Pairs			
1949-1953 Cohorts, Prime Income	0.22	0.021	0.24	0.06	0.15
(NLS66)	(0.03)	(0.002)	(0.03)	(0.05)	(0.03)
1961-1964 Cohorts, Prime Income	0.37	0.029	0.37	0.24	0.18
(NLSY79)	(0.03)	(0.002)	(0.03)	(0.04)	(0.03)
H_0: Measures Equal, p=	0.00	0.03	0.00	0.00	0.51
NLS66 Pairs	1252	1252	1181	1252	1252
NLSY79 Pairs	1294	1276	1294	1294	1191
		Panel B. Parent-Son Pairs			
1948-1952 Cohorts, Early Career Income	0.17	0.025	0.22	-0.08	0.10
(NLS66)	(0.03)	(0.002)	(0.03)	(0.05)	(0.03)
1961-1964 Cohorts, Early Career Income	0.36	0.032	0.36	0.14	0.36
(NLSY79)	(0.03)	(0.003)	(0.03)	(0.05)	(0.03)
H_0: Measures Equal, p=	0.00	0.03	0.00	0.00	0.00
NLS66 Pairs	1269	1269	1269	1269	1262
NLSY79 Pairs	1187	1155	1187	1187	1132
1961-1964 Cohorts, Prime Income	0.38	0.033	0.38	0.15	0.39
(NLSY79 - Daughter Sampling)	(0.03)	(0.003)	(0.03)	(0.05)	(0.03)

Notes. Panel A shows estimates for the NLS66 and NLSY79 parent-daughter samples. Panel B shows estimates for the parent-son samples. The bottom row of panel B shows estimates from the NLSY79's parent-son sample using the same sample construction as the parent-daughter sample. The first column shows estimates of the rank-rank slope. The second column shows estimates of the education-parent rank gradient. Years of education is topcoded at 18 to make the range consistent across surveys. The third column shows how the estimates from the first column change if we assume the NLS66 cohorts had the same returns to education as the NLSY79 cohorts (see text for more details). The fourth column shows the marriage-parent rank gradient. The final column shows the rank-rank correlation using the child's own income instead of family income. Robust standard errors in parentheses.

Appendices

For Online Publication Only

Appendix I. Missingness of Income

Table A1 documents the share of observations for which each of our income measures is missing. The main difference between our NLS66 and NLSY79 samples is that parent income is never missing in the NLS66 but is missing for a small share of parent-child pairs in the NLSY79.

For observations where parent income is observed but child income is missing, missingness is negatively related to log family income in the parent generation. The relationship between missingness and income is similar in both the NLS66 and NLSY79 parent-daughter samples and parent-son samples. The p-value of the null hypothesis that the relationship is identical across cohorts is 0.34 and 0.68 for parent-daughter and parent-son samples, respectively.

Table A1. Missingness of Income by Cohort

	Daughter Income	Daughter's Parent's Income	Son's Income	Son's Parent's Income
1948-1953 Cohorts *(NLS66)*	25.6%	0.0%	25.7%	0.0%
1961-1964 Cohorts *(NLSY79)*	14.9%	8.5%	9.9%	6.8%

Notes. This table shows the rates at which parent and child income is missing in each cohort. Means are weighted by the child's survey weight in the first survey round.

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Appendix II. Binned Scatter Plots

Figure A1. Rank-Rank Binned Scatter Plot, Women

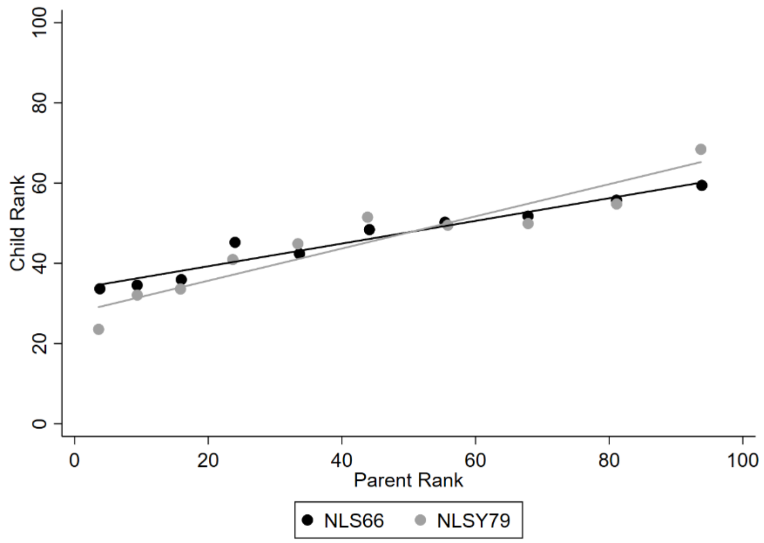
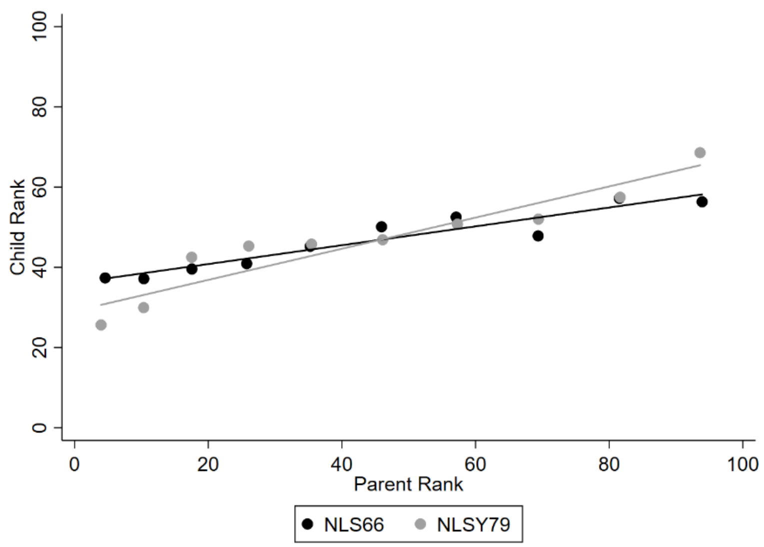


Figure A2. Rank-Rank Binned Scatter Plot, Men



Appendix III. Robustness Checks

Figure A3. Robustness of rank-rank change to additional controls, Women

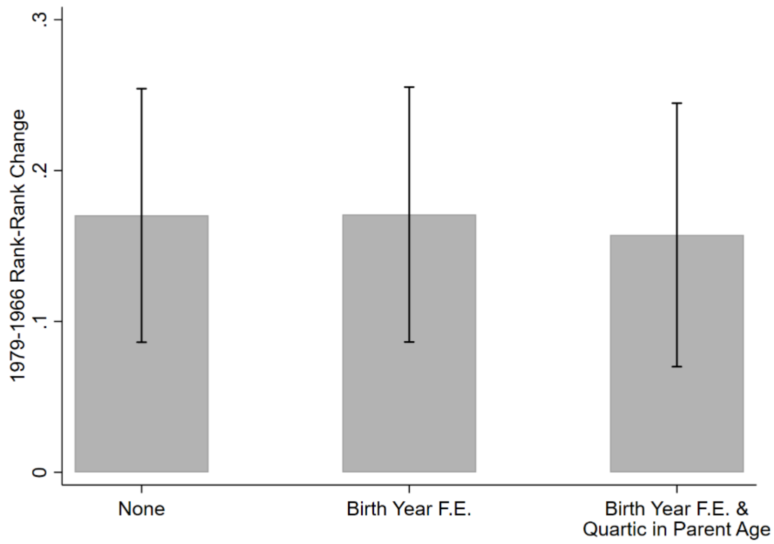


Figure A4. Robustness of rank-rank change to additional controls, Men

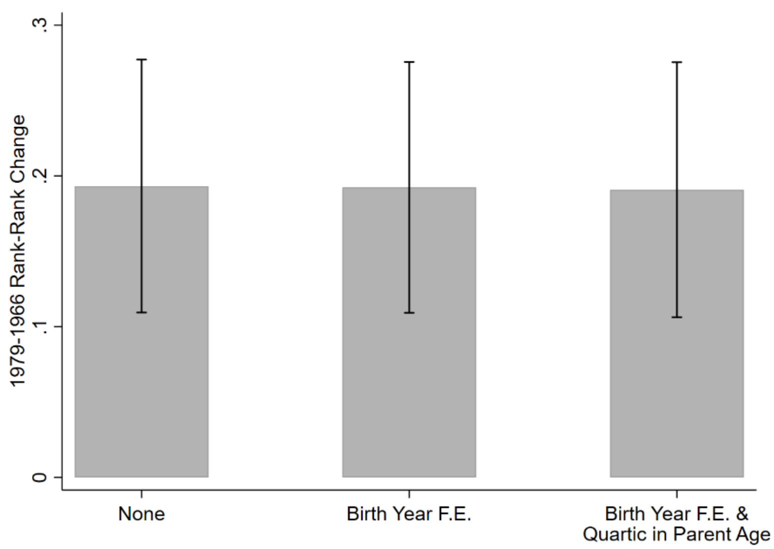
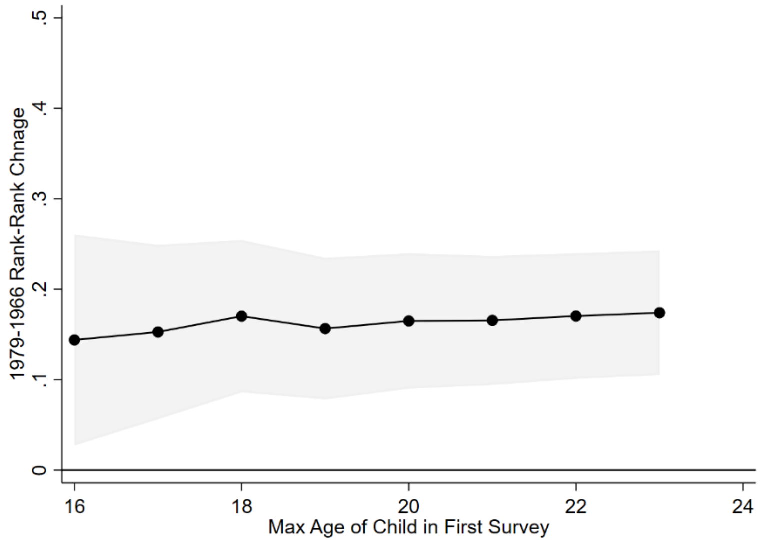
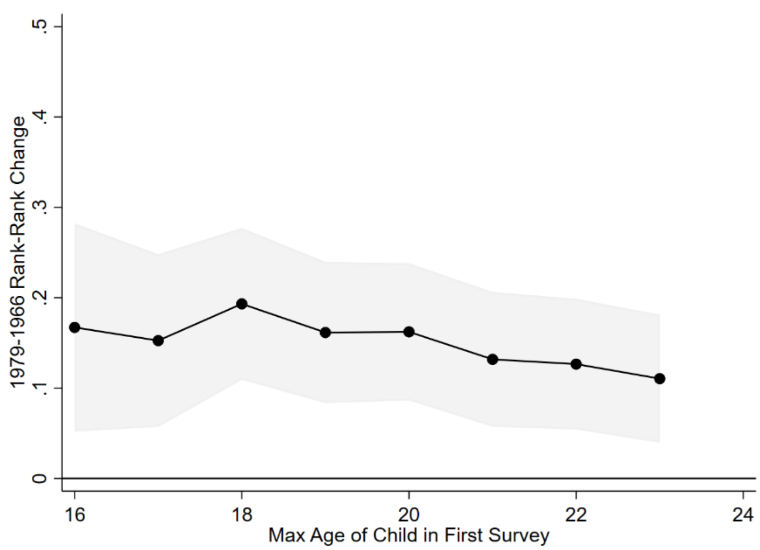


Figure A5. Robustness of rank-rank change to max age of children, Women



Notes. Ranks defined using all children. Ranks in the main analysis are defined using children who were 18 or younger.

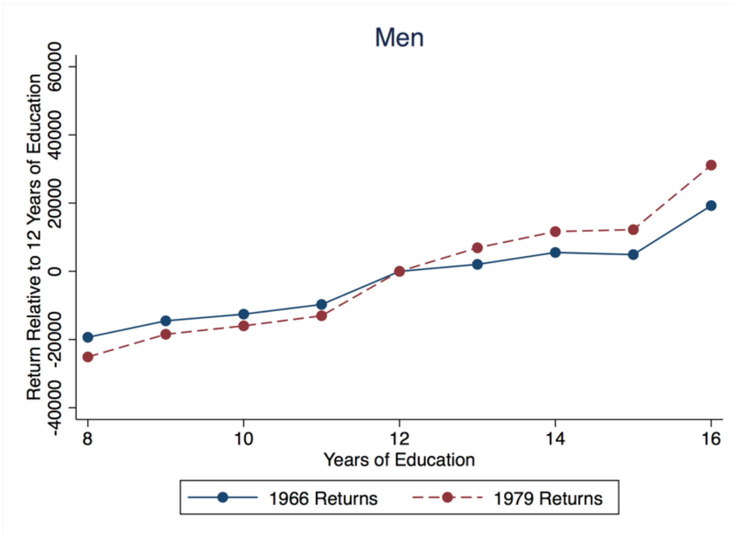
Figure A6. Robustness of rank-rank change to max age of children, Men



Notes. Ranks defined using all children. Ranks in the main analysis are defined using children who were 18 or younger.

Appendix IV. Returns to Education

Figure A7. Returns to Education



Notes. This figure plots the returns to education for men born in the NLS66 cohorts (solid blue line) and NLSY79 cohorts (dashed red line). Estimates use data on the same cohorts in the Current Population Survey's March Supplement taking family income measures in the same years as our NLS income averages. Individuals with 8 years or less of education are all grouped in the 8 years of education category. Similarly, all individuals with 16 years or more of education are grouped in the 16 years of education category.

Appendix V. IGE Results

Table A2 shows analogous results to those in Table 1 using log family income as our measure of income. In this case, the estimates $\hat{\gamma}^{NLS66}$ and $\hat{\gamma}^{NLSY79}$ are interpretable as intergenerational income elasticities (IGEs). Figures A8 through A13 demonstrate that these results are similarly robust to changing the set of controls or the maximum age of children included in our sample.

The estimated IGE for the NLS66 parent-daughter sample, $\hat{\gamma}^{NLS66}$, is 0.28. In contrast, the IGE among NLSY79 parent-daughter pairs, $\hat{\gamma}^{NLSY79}$, is 0.52. Consistent with the rank-rank results, these point estimates suggest that persistence in log family income increased across the two cohorts by 85 percent. This difference is statistically significant at conventional levels ($p < 0.01$). Panel B shows estimates using the parent-son samples from the two surveys. These estimates suggest that the IGE more than tripled for cohorts born around 1963 compared to cohorts born around 1951. This difference is statistically significant at conventional levels ($p < 0.01$). Like the rank-rank slope, when we construct the NLSY79 parent-son sample similarly to the parent-daughter sample, the magnitude of the estimated IGE increases, here, by 14 percent.

Mirroring Table 1, the remaining columns of Table A2 explore two potential explanations for why persistence increased but using log income instead of rank as our income measure.

In contrast to the rank-rank results, changes in education do not explain nearly as much of the increase in the IGEs. We find weaker evidence of a change in the education-log parent income gradient. The point estimates for daughters and sons suggest increases of 17 percent and 25 percent, respectively. But these differences are, at most, marginally significant (with $p = 0.25$ for women and $p = 0.10$ for men). Similarly, the IGE estimates barely change for women if we use our counterfactual NLS66 income using the higher returns to education in the NLSY79. For men, if we adjust NLS66 incomes to have the same returns to education as the NLSY79 cohorts the increase in persistence declines 23 percent but remains large and statistically significant.

What explains these seemingly contradictory findings? It turns out that the difference is due to the observations that are dropped because they have non-positive parent income. If we re-estimate the education-parent rank gradient using the IGE samples, the evidence that the education-parent rank gradient increased is also much weaker. The point estimates suggest that the gradient increased by 21 percent for women and by 19 percent for men, but these differences are much less precisely estimated. The p-values are 0.18 for women and 0.12 for men. On the one hand, the fact that estimates using log income exclude these observations could be viewed as a limitation of the methodology. On the other hand, one might worry that these non-positive income measures are suspect, in which case the log income results may be more credible. Given the sensitivity of these results, we are inclined to view the rank-rank results as suggestive of a potentially interesting mechanism that warrants further investigation.

The results in columns 4 and 5, on the relationship between marriage and parent log income and the IGE estimated using the child's own income rather than family income, are much more consistent with the rank-rank results. We find that the association between marriage and log parent income became much more positive between the two cohorts. Considering this result, we calculate the IGE using own income instead of family income. Echoing the rank-rank results find that this fully explains the increase in the IGE for women, but not for men.

Table A2. The Intergenerational Elasticity among NLS66 and NLSY79 Parent-Daughter and Parent-Son Pairs

	IGE	Years of Education	IGE with 1979 Ed Returns	Married during any income years	IGE using Child's Own Income
			Panel A. Parent-Daughter Pairs		
1949-1953 Cohorts, Prime Income	0.28	1.01	0.29	3.17	0.27
(NLS66)	(0.03)	(0.10)	(0.03)	(1.92)	(0.06)
1961-1964 Cohorts, Prime Income	0.52	1.18	0.52	10.83	0.25
(NLSY79)	(0.04)	(0.11)	(0.04)	(1.80)	(0.05)
H_0: Measures Equal, p=	0.00	0.25	0.00	0.00	0.81
NLS66 Pairs	1195	1212	1138	1212	1105
NLSY79 Pairs	1288	1276	1288	1294	1191
			Panel B. Parent-Son Pairs		
1948-1952 Cohorts, Early Career Income	0.13	1.05	0.20	-3.17	0.04
(NLS66)	(0.03)	(0.09)	(0.03)	(1.77)	(0.04)
1961-1964 Cohorts, Early Career Income	0.43	1.30	0.43	5.54	0.35
(NLSY79)	(0.05)	(0.13)	(0.05)	(2.33)	(0.04)
H_0: Measures Equal, p=	0.00	0.10	0.00	0.00	0.00
NLS66 Pairs	1219	1235	1225	1235	1157
NLSY79 Pairs	1170	1148	1170	1180	1126
1961-1964 Cohorts, Prime Income	0.49	1.31	0.49	6.95	0.47
(NLSY79 - Daughter Sampling)	(0.05)	(0.13)	(0.05)	(2.08)	(0.05)

Notes. Panel A shows estimates for the NLS66 and NLSY79 parent-daughter samples. Panel B shows estimates for the parent-son samples. The bottom row of panel B shows estimates from the NLSY79's parent-son sample using the same sample construction as the parent-daughter sample. The first column shows estimates of the IGE. The second column shows estimates of the education-parent log family income gradient. The third column shows how the estimates from the first column change if we assume the NLS66 cohorts had the same returns to education as the NLSY79 cohorts (see text for more details). The fourth column shows the marriage-parent log family income gradient. The final column shows the IGE using the logarithm of the child's own income instead of family income. Robust standard errors in parentheses.

Figure A8. IGE Binned Scatter Plot, Women

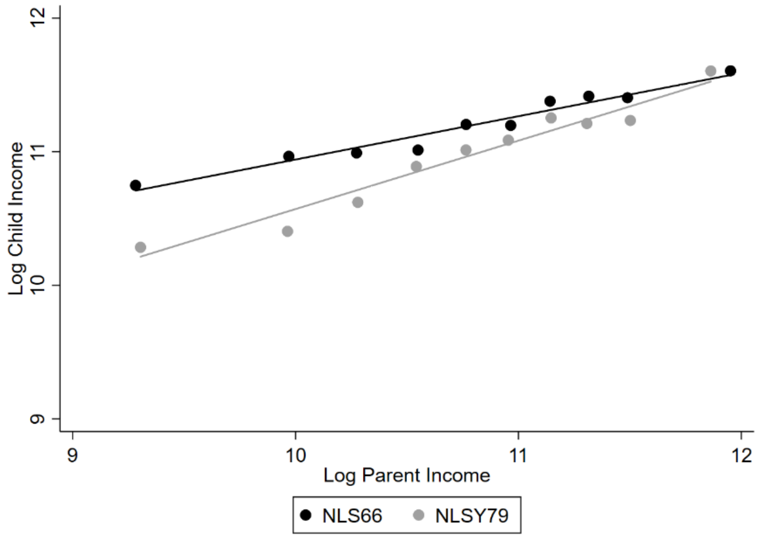


Figure A9. IGE Binned Scatter Plot, Men

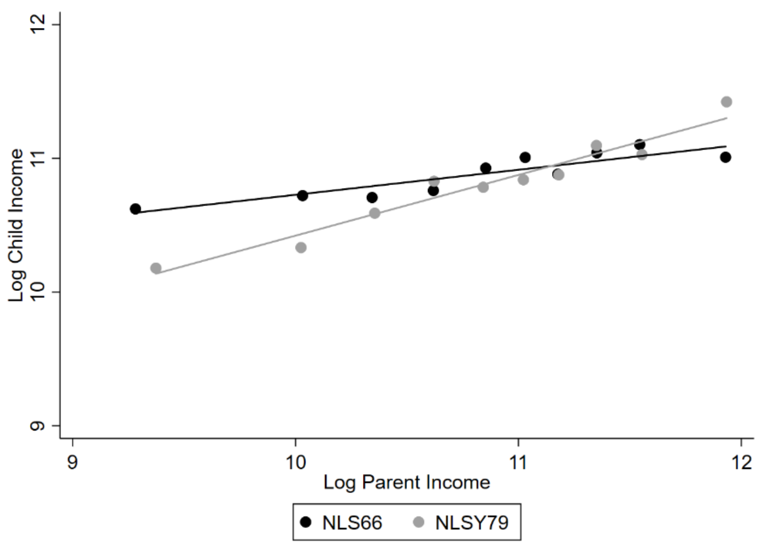


Figure A10. Robustness of IGE change to additional controls, Women

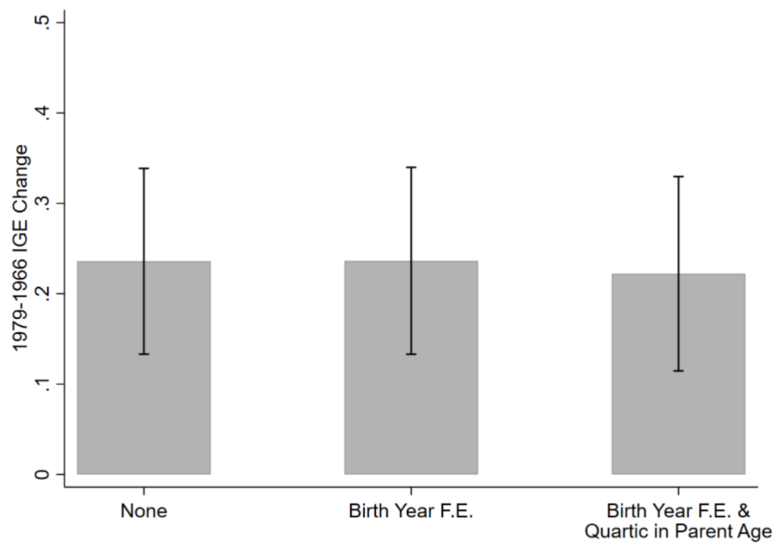


Figure A11. Robustness of IGE change to additional controls, Men

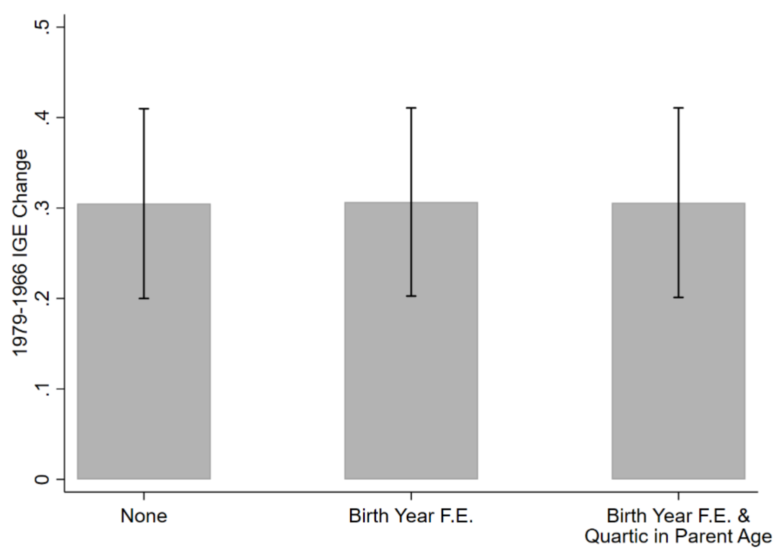


Figure A12. Robustness of IGE change to max age of children, Women

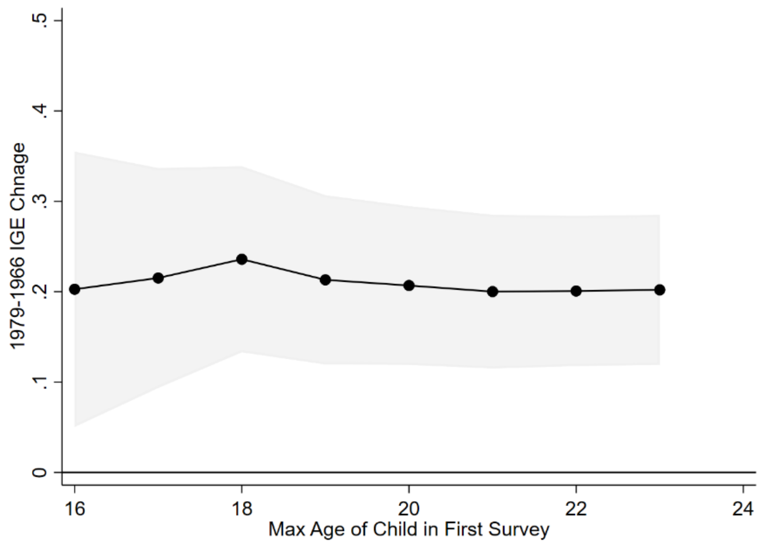
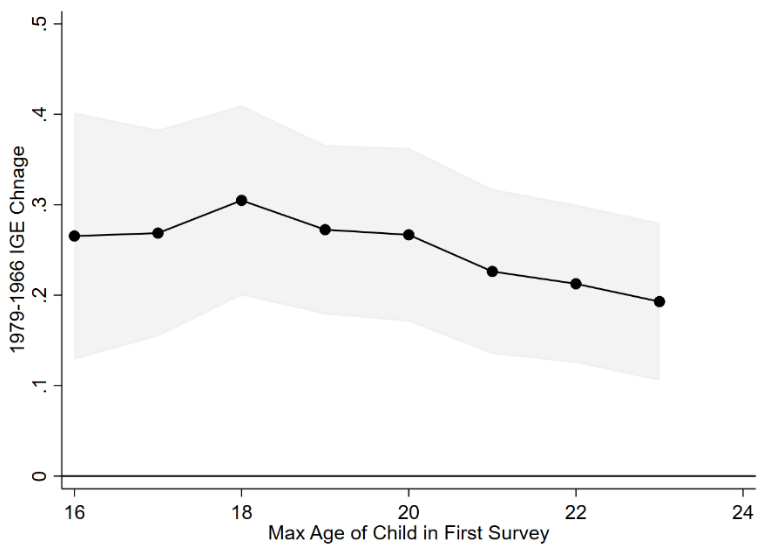


Figure A13. Robustness of IGE change to max age of children, Men



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