A Lifecycle Estimator of Intergenerational Income Mobility*

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Abstract

The estimation of intergenerational mobility ideally requires full income histories to determine lifetime incomes. However, as empirical applications are typically based on shorter snapshots, the resulting estimates are subject to *lifecycle bias*. While the literature has followed different strategies to address this problem, we use long income series from Sweden and the US to illustrate that existing methods struggle to account for one important property of income processes: children from more affluent families tend to experience faster income growth, even conditional on their own observable characteristics. We propose a lifecycle estimator that captures this pattern and show that it performs well across different settings. We then apply this estimator to study mobility trends in Sweden and in the US, including for more recent cohorts that could not be considered in prior work. Our results show that despite rising income inequality, intergenerational income mobility was largely stable over cohorts born 1950-1989 in both Sweden and the US.

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1 Introduction

A key statistic to characterize inequality is the intergenerational mobility in income. But while ideally based on complete income profiles for two generations, most studies have to rely on short snapshots at some particular age. The main methodological challenge is therefore to account for the estimations errors that arise from the use of such snapshots. While the literature has made improvements on this front, concerns remain as to whether the available estimates are sufficiently robust, and whether comparisons across place or time are reliable (Mogstad and Torsvik 2021). For example, recent US estimates of the *intergenerational elasticity of income* (IGE) vary between 0.35 and 0.65, despite building on similar methodological insights (for example, Chetty et al. 2014 and Mazumder 2016).

We therefore propose a new *lifecycle* estimator of the IGE in incomplete income data that is less sensitive to the age at which child income is measured. Our estimator models income profiles as a function of an age and education, but also allows income growth to vary with parental background conditional on own characteristics. We use long income series from Swedish registers and the Panel Study of Income Dynamics (PSID) to illustrate that these family effects on income growth are sizable, and to verify that the estimator performs well in different data settings. We then apply this lifecycle estimator to study time trends in intergenerational mobility for cohorts born between between the 1950s and 1980s, in both the US and Sweden.

We start by analyzing the key components of the income process that affect intergenerational estimators: (i) income growth explained by an individual's own characteristics, (ii) transitory noise, and (iii) income growth unexplained by own characteristics. Crucially, we find that this unexplained income growth nevertheless correlates within families: within educational or occupational groups, children from high-income families have lower initial incomes but steeper growth. This difference in growth is sizable. For example, college-educated sons with fathers in the top quartile of the Swedish income distribution tend to have lower income in their mid 20s, but around 40 percent higher incomes around age 40, compared to college-educated sons from bottom-quartile families. These findings matter for the estimation of income mobility, but also for the wider debate on the properties of income processes: in particular, they lend support to the argument that income grows at an individual-specific and deterministic rate (Guvenen 2009).

We then analyze whether existing methods account for these properties of the income process. Two strategies can be distinguished. One option is to formalize the relation between (observed) annual and (unobserved) lifetime income in an errors-in-variables model, as in Mazumder (2005) or Haider and Solon (2006). An alternative option is to estimate the shape of income profiles over the lifecycle, using partially observed profiles and education or other observable characteristics, as in Hertz (2007). Most applications follow the first option and base their empirical strategy on the generalized errors-in-variables model (Haider and Solon 2006), which suggests that lifecycle bias can be reduced by measuring incomes around midlife. While this is a useful rule-of-thumb, the exact age at which the bias is minimized is typically not known, and data availability and the selected age range vary considerably across applications (see Table A.1). Sometimes, mid-life incomes are inherently unobserved, such as when studying mobility trends for recent birth cohorts.

Based on these observations, we propose a new *lifecycle* estimator for estimating the IGE in incomplete data, which exploits the available income information more fully. We first estimate income profiles based on standard observables, such as age and education (as in Vogel 2007 and Hertz 2007), to predict expected (individual) income profiles in the unobserved age range. However, we also account for the fact that children from high-income families have steeper growth even conditional on those observables, which reduces the sensitivity of mobility estimates to the age at which income is observed. Rather than directly extrapolating individual slopes over the lifecycle, which results in noisy estimates, our preferred specification allows for slopes to vary with parental characteristics. As an alternative for when parent and child incomes are not jointly observed we allow income growth to vary with income levels, motivated by the insight of Creedy (1988) that the "fanning out" of incomes over age can bias mobility estimates. We then construct lifetime income based on the predicted profiles, to estimate the IGE in lifetime income.

In other words, before estimating the association of parent to child income (the intergenerational regression), we first aim to understand its relation to income *growth*. We show that the lifecycle estimator performs well in Swedish and US data, closely tracking a benchmark estimate based on long-run incomes. Moreover, the estimator is not very sensitive to the exact age range at which a given individual is observed, or to the number of income observations available per individual. It promises, therefore, to be applicable in a wide range of settings. In contrast to current practice, it exploits all available income information in the data, and can be used to estimate mobility for recent cohorts, for which mid-life incomes are unobserved. Indeed, to account for differences in income growth is particularly useful when the child generation is observed only at young age. However, the estimator is more difficult to implement than the simple rule-of-thumb used in the current literature, and we discuss how the inclusion of different fixed effects or parental interactions affects the estimates.

Finally, we apply the lifecycle estimator to study mobility trends in Sweden and the US. Our objective is two-fold. First, we examine whether existing evidence may be systematically distorted due to estimation biases. Second, we estimate mobility trends for younger, more recent birth cohorts – which are arguably of prime interest from a policy perspective – exploiting that our method works well even if incomes for the child generation are observed only at early age. For Sweden, accounting for lifecycle effects leads to different conclusions on how income mobility has developed over time. Estimates based on a fixed age suggest that mobility decreased rapidly between the 1950s and 1970s cohorts. Accounting for lifecycle effects, however, yields remarkably stable mobility for cohorts born between the 1950s and 1970s, but a slight increase for those born in the 1980s. Thus, Sweden's comparatively high level of income mobility appears to have been a persistent feature of the second half of the 20th century.

We then estimate mobility trends for the US using the PSID. As with Sweden, failing to account for lifecycle dynamics leads to attenuated IGE estimates for recent cohorts born in the 1980s, who are only observed at young age, thus falsely portraying a sharp increase in mobility. Our lifecycle estimators generally yields larger and more stable estimates, suggesting that the IGE has remained remarkably constant in the US. While some specifications indicate a pattern of marginally increasing mobility in recent cohorts, the long-run change across cohorts born 1951-1989 is always quantitatively small and statistically insignificant. Our findings are therefore not supportive of the expectation that mobility must have plunged dramatically for children born in the 1980s (Putnam et al. 2012), despite growing socioeconomic gaps in parents' monetary and time investments (Ramey and Ramey 2010, Corak 2013). An interesting question for future work is why mobility in an *outcome* such as income has remained so stable despite increasing gaps in inputs.

Our paper adds to an extensive literature measuring the levels and trends of the IGE in different countries (see Solon 1999, Black and Devereux 2011 and Jäntti and Jenkins 2015) and contributes to the literature on measurement error in intergenerational estimates. Early work on this topic focused on classical errors from incomplete income data for fathers (Atkinson 1980, Solon 1999). This wave of studies recognized that lifecycle variation should be accounted for, but assumed that the inclusion of age controls in the intergenerational regression would solve the issue. More recently, the literature has focused on non-classical measurement error and lifecycle bias from incomplete data for the child generation. First discussed in Jenkins (1987), the problem gained attention with the generalized errors-in-variables model proposed by Haider and Solon (2006), and applications in different contexts by Grawe (2006), Böhlmark and Lindquist (2006), and Nilsen et al. (2012), among others.[1] In light of these measurement problems, recent work often abandons the canonical IGE in favor of rank-based measures that are less sensitive to measurement problems (e.g., Chetty et al. 2014).[2] Such measures isolate the dependence structure across generations, but abstract from variation in income inequality across time or place. For this reason, the IGE continues to play an important role in the analysis of income mobility.

We link our arguments also to the large literature on income processes (e.g., Meghir and Pistaferri 2011). This link is interesting in both directions. On the one hand, a better understanding

[1]In parallel, Creedy (1988), Vogel (2007) and Hertz (2007) suggested alternative approaches based on modelling how the income profile varies with observable characteristics, and Chau (2012) and Jäntti and Lindahl (2012) consider lifecycle models with heterogeneous intercepts and slopes.

[2]See also Nybom and Stuhler (2017). Kitagawa et al. (2019), who propose a correction method for measurement error in ranks.

of income processes allows for a more comprehensive understanding of the advantages and disadvantages of existing methods in the intergenerational literature, and informs the direction of future methodological work. On the other hand, the intergenerational perspective contributes to the ongoing debate on the role of unobserved heterogeneity in the income process literature. A controversial question is whether (residual) income grows at an individual-specific and deterministic rate or follows a random walk. These models are difficult to distinguish, and standard tests of the covariance structure of income growth may not be very informative (Guvenen, 2009). We argue that intergenerational data can be informative about this question, and provide transparent evidence in favor of the first model – within educational or occupational groups, children from affluent families have substantially steeper income growth than those from low-income families, in particular in the early stages of the career.[3]

Our method could be particularly useful for work on mobility variation across countries or over time. The observation that income inequality correlates positively with the IGE across countries (Blanden 2011, Corak 2013) and across regions within countries (Chetty et al. 2014) has received much attention. However, it is difficult to estimate the IGE in comparable ways in these settings. If income profiles differ across countries, a simple rule-of-thumb prescription to measure incomes around a specific age is likely to introduce biases of different magnitudes and signs. This measurement problem is crucial for the estimation of mobility *trends*. Existing work is often conflicting. Earlier work found no evidence of shifts in mobility in the US for cohorts born 1952-75 (e.g. Hertz 2007, Lee and Solon 2009) or later (Chetty et al. 2014), a finding that is surprising given the large increase in income inequality over this period, the theoretical link between inequality and mobility, and the observation that gaps in parental inputs have increased (Blanden et al. 2022). However, Davis and Mazumder (2019) show that mobility declined sharply for cohorts born 1957-64, in comparison with those born 1942-53, and Justman and Stiassnie (2021) find declining mobility over roughly the same cohorts considered by Hertz (2007) and Lee and Solon (2009). Our analysis suggests that the treatment of lifecycle bias is one potentially important reason for the conflicting evidence, and the explicit consideration of the income process may help to produce more comparable estimates.

The paper's remaining sections are divided as follows. In Section 2, we describe the Swedish and US data and our sampling. Section 3 provides a discussion of the properties of the income process and evidence on its key components. In Section 4, we analyze existing correction methods for IGE estimates in light of the income process properties. In Section 5, we present and test our new estimator, which we use in Section 6 to study mobility trends in Sweden and the US. Section 7 concludes.

[3]While we do note attempt to identify the precise reasons for this pattern, it is consistent with recent arguments by Lochner and Park (2022) and Halvorsen et al. (2021).

2 Data

We use data from Sweden and the US. For Sweden, we use administrative registers that contain the universe of Swedish citizens aged 16-64 at any point between the years 1960-2018 (born 1896-2002) and gross labor earnings from tax records (reported by employers) for the period 1968-2018.[4] Using multigenerational registers, we link children born 1932 or later (who have been residents of Sweden at some point since 1961) to their biological parents. As we observe individual rather than household income we focus on father-son pairs to abstract from female labor market participation, which also improves comparability with the previous literature. Other administrative registers provide information on education, occupation and other individual characteristics.[5]

We use these data to construct two different samples: a *benchmark sample* for studying the performance of different estimators and a *trends sample* for studying cohort trends in intergenerational mobility. We construct the benchmark sample such that it contains nearly complete income trajectories, allowing us to compare estimates from partial data to a "true" benchmark estimate based on lifetime incomes. Specifically, we consider cohorts born 1952-1960, for which we observe incomes between ages 25 and 58.[6] We restrict this sample further to only include individuals with fathers born from 1927 onwards, such that we can measure fathers' income over a longer period (age 41-58). While our benchmark sample is therefore not representative, this is not a concern for our purposes, as long as we observe a sufficiently heterogeneous sample to study lifecycle patterns in income. We use all the available income data to create measures of lifetime income, using data from ages 25-58 for sons and data from ages 41-58 for fathers.

In addition, we construct a trends sample covering cohorts born 1950-1989, which we analyze by decade of birth. To ensure that the quality of the parental income measure is comparable across cohorts we construct the measure in two steps. First, we randomly select up to five annual income observations for each father (between age 40 and 55), such that the number of observations per father varies little across cohorts. Second, we use these annual observations in a Mincer-type equation to predict income at age 50 for each father.[7] We therefore balance both the number of income observations and the age at measurement across cohorts.

[4]We observe labor earnings (including income from self-employment) for all residents in the years 1968, 1970, 1971, 1973, 1975, 1976, 1979, 1980, 1982 and annually for 1985-2018. We impute data for the gap years that occur between 1968 and 1985 with neighboring observations, bottom code incomes in the first percentile of each cohort and adjust incomes for inflation using the CPI.

[5]The Education Register contains data on highest educational attainment and field of education for practically the entire population alive in 1970 or later. Occupational information comes from different registers and is available bidecennially 1960-1990 and, for a large subsample, annually from 1996 and onwards.

[6]We do not consider income at older ages to abstract from early retirement decisions and to keep the observed ages balanced across cohorts.

[7]Specifically, we run a regression of parental income on a polynomial of parental age interacted with parental education and parental birth cohort.

Table 1: Descriptive Statistics

	Benchmark	1950-59	1960-69	1970-79	1980-89
	Panel A: Swedish Register Data				
Father-son pairs	201,066	525,813	572,811	532,835	530,312
Father's Age at Birth of Son	25.7	31.6	30.1	29.6	31.1
Log Lifetime Income of Sons	12.4	12.4	12.5	12.5	12.3
Percent of Zero Income Obs of Sons	8.6	8.2	8.7	8.7	11.2
Mean Age Son at Earnings' Obs	40.9	42.4	39.1	34.5	29.6
% Sons with College Degree	14.1	16.2	17.0	27.1	26.2
% Sons with Managerial Position	17.8	17.9	17.9	13.1	5.1
Log Lifetime Income of Fathers	12.3	12.3	12.3	12.3	12.4
Percent of Zero Income Obs of Fathers	6.0	5.4	6.2	6.9	8.6
% Fathers with a College Degree	7.7	7.3	10.6	14.9	16.8
% Fathers with Managerial Degree	11.8	11.6	14.7	16.3	12.3
	Panel B: PSID				
Parent-child pairs	1,286	1,283	1,153	1,212	1,419
Share Female	51.2	50.9	51.3	48.9	53.4
Log Lifetime Income Child	9.2	9.2	9.2	9.2	8.9
Mean Age Child at Earnings' Obs	36.1	36.2	34.2	31.6	27.7
Log Income Parent when Child was 15-17	9.3	9.3	9.4	9.3	9.4
Mean Age Parent when Child was 15-17	45.2	45.5	44.7	42.5	44.2
% Child with Some College	51.9	52.6	55.9	69.1	72.4
% Parent with Some College	26.9	26.8	35.7	50.0	56.4

Notes: The benchmark samples (column 1) contain the cohorts born 1952-1960 and are used for testing the performance of the intergenerational elasticity estimators. The other columns present descriptive statistics from the samples used to study mobility trends, separately by decade of birth of the child.

We also use data from the Panel Study of Income Dynamics (PSID), which began in 1968 with a nationally representative sample of over 18,000 individuals living in 5,000 US families. The survey is useful for intergenerational research since it follows children from the original sample as they grow older and form their own households, and contains data on employment, income, education and numerous other topics. We use data from all PSID waves released between 1968 and 2016. The survey was annual up until 1997 and has been biannual thereafter. Apart from a few exceptions, we follow the sampling and variable definitions used by Lee and Solon (2009). As such, we use only the PSID core sample, i.e. the Survey Research Center component. We focus on family rather than individual income and consider both sons and daughters to enlarge our sample.

Our benchmark sample covers children born between 1952 and 1960, which similar to the Swedish case enables us to observe almost complete income histories. To measure parental income, we average log annual family income in the childhood home over the three years when the child was 15-17 years old, similar to the measures in Lee and Solon (2009) and Chetty et al. (2014). We measure the children's adult income by the (log) annual family income in the household in which they were the household head or head's spouse and exclude outlier observations (using the same thresholds as Lee and Solon 2009). As for Sweden, we also construct a trends sample covering US cohorts born 1950-1989, using the same sampling and variable definitions as for the benchmark sample.

To improve the comparability to previous studies, we use sampling and variable definitions that accord with what has been the dominant approach for each data source in previous work. As such, those definitions differ between the two countries. First, we use family income and consider both sons and daughters for the US, while for Sweden we consider labor earnings and consider only father-son pairs. Second, we measure parental income at a given age of the child for the US, but at a given age of the parent for Sweden. Third, the parental income measure is based on up to 18 years of income for Sweden but a three-year average for the US, such that the US estimates are more strongly attenuated by measurement error (see Mazumder 2005). For these reasons, we cannot directly compare differences in mobility levels between countries. However, our results are comparable to prior work for each country as well as across cohorts within countries.

Table 1 reports descriptive statistics for each sample. Our benchmark samples contain 201,066 and 1,286 individuals, for Sweden and the US respectively. The trend sample for Sweden covers roughly 2,150,000 sons, while the corresponding sample for the US includes approximately 5,000 sons and daughters. Columns 2 to 5 in Table 1 show summary statistics, separately by decade of birth. Unsurprisingly, individuals belonging to more recent cohorts are, on average, more educated, have more educated fathers and occupy higher-level positions in the labor market.[8] Comparing our benchmark and trends samples (columns 1 and 2) for Sweden illustrates that the former is slightly negatively selected in terms of income and education, due to its restriction to younger parents for whom we observe sufficiently complete income series. As a consequence, estimates of the IGE will differ between the benchmark sample and the more representative trends sample.

3 An Intergenerational Perspective on Income Processes

Income fluctuations over the life cycle are the primary source of bias in intergenerational estimates. We therefore start by illustrating those properties of the income process that appear particularly important for intergenerational research, using long income series from Sweden and the US (see Section 2). This evidence will then allow us to characterize the advantages and limitations of existing correction methods (Section 4) and to motivate a new lifecycle estimator (Section 5)

[8]Since the age composition changes across cohorts such that the incomes of later cohorts are measured at earlier ages (on average), this does not translate into higher log average income over cohorts.

that addresses our key observation – that income growth varies with parental background even conditional on individuals' own characteristics.

A large literature on income processes has studied the shape of income profiles over the life cycle.[9] While many properties are well established, two contrasting viewpoints exist about the idiosyncratic components of income growth. The *restricted income profile* (RIP) model views income as the sum of a mean-reverting component reflecting *transitory shocks* and a (approximately) random-walk component reflecting *permanent shocks* (MaCurdy 1982). In contrast, the *heterogeneous income profile* (HIP) model assumes that individual income grows at an individual-specific and deterministic rate (Guvenen 2009). The RIP and HIP models are difficult to distinguish in standard data sets. However, intergenerational data can be used to show that income growth does indeed differ systematically between individuals. As a reference point, consider the HIP model by Guvenen (2009), which assumes that log income for individual i with experience h at time t is given by

$$y^i_{h,t} = g(\theta^0_t, X^i_{h,t}) + f(\alpha^i, \beta^i, X^i_{h,t}) + z^i_{h,t} + \phi_t \varepsilon^i_{h,t}. \tag{1}$$

The function g captures the income variation that is common to all individuals and that is explained by observable characteristics $X^i_{h,t}$.[10] In our analysis, we consider as "observed" characteristics that are typically observed by the researcher, such as education or occupation. The second function, f, captures the component of life-cycle earnings that is individual or group-specific and that is unexplained by those characteristics.[11] By "unexplained" we refer to determinants that are *typically* unobserved (and therefore instead picked up by f), such as an individual's ability or parental lifetime income. In the data we use, however, we observe proxies for those characteristics and can therefore test whether they predict lifecycle profiles.[12] Finally, the dynamic component of income is modeled as an AR(1) process, $z^i_{h,t} = \rho z^i_{h,t-1} + \pi_t \eta^i_{h,t}$, with $z^i_{0,t} = 0$ and with π_t capturing possible time-variation in the innovation variance, plus a purely mean-reverting transitory shock, $\varepsilon^i_{h,t}$ scaled by ϕ_t to account for possible non-stationarity in that component.[13]

We show evidence on each of these components in the Swedish data (Figure 1) and the PSID (Figure 2). Panel A of Figure 1 illustrates that, unsurprisingly, income profiles vary systematically

[9]Insights from this literature have been used to study the causal effect of parental income (e.g., Carneiro et al. (2021), but used only for motivational purposes in descriptive studies (an exception is Heidrich 2016).

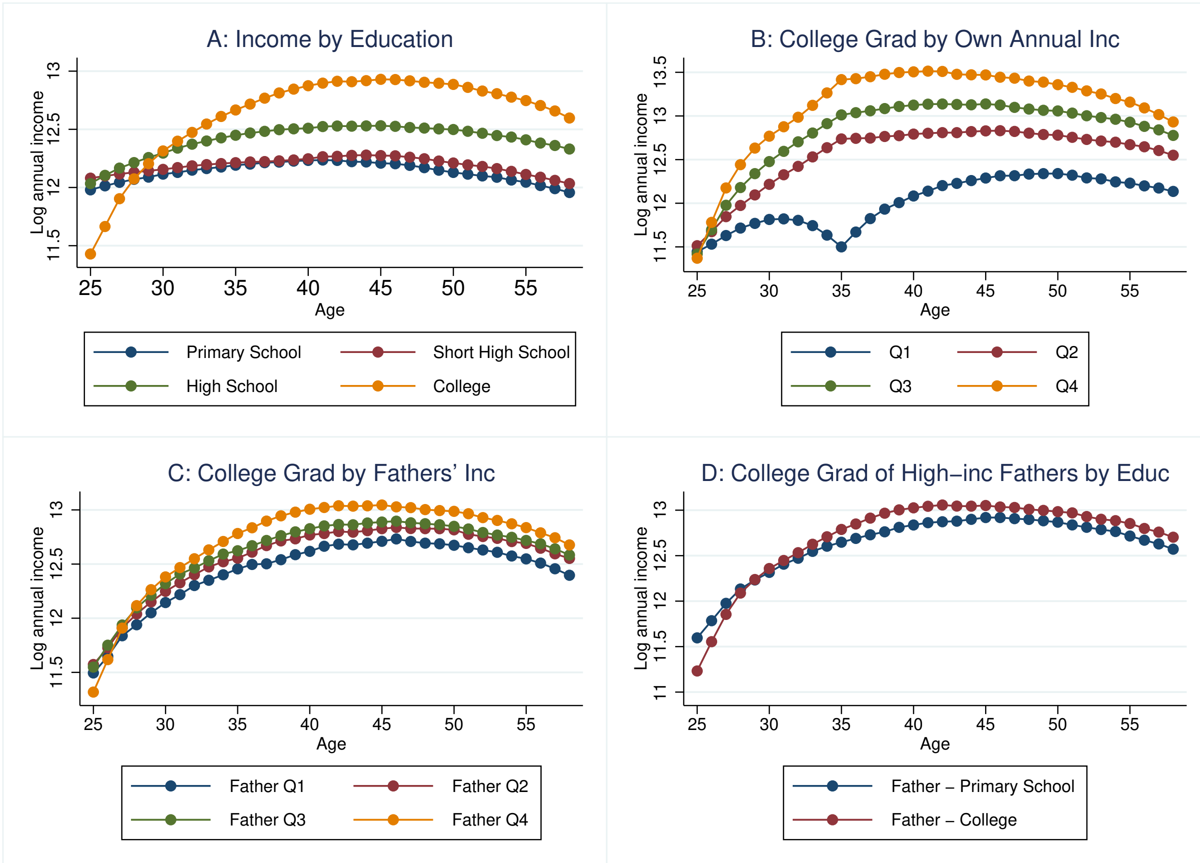
[10]Guvenen (2009) considers a cubic polynomial in experience h. Yet, more generally, we could think of X^i_t as observables that could include education, gender, age, etc. The coefficients θ^0_t are common to all individuals.

[11]Guvenen (2009) assumes that the function is linear in experience, so that $f(\alpha^i, \beta^i, X^i_{h,t}) = \alpha^i + \beta^i h$.

[12]While the distinction between observables and unobservables is ultimately governed by data availability, we here choose a definition that is in line with the literature on income processes, in which researchers have often use a limited set of standard observables such as education or occupation.

[13]The innovations ε and η are assumed to be independent of each other and over time while the random vector (α^i, β^i) is distributed across individuals with zero mean, variances of σ^2_α and σ^2_β, and covariance $\sigma_{\alpha\beta}$. Persistent and transitory shock components are scaled by time-specific coefficients, as they may change over time (see Moffitt and Gottschalk 1995).

Figure 1: Components of the Income Process in the Swedish Data

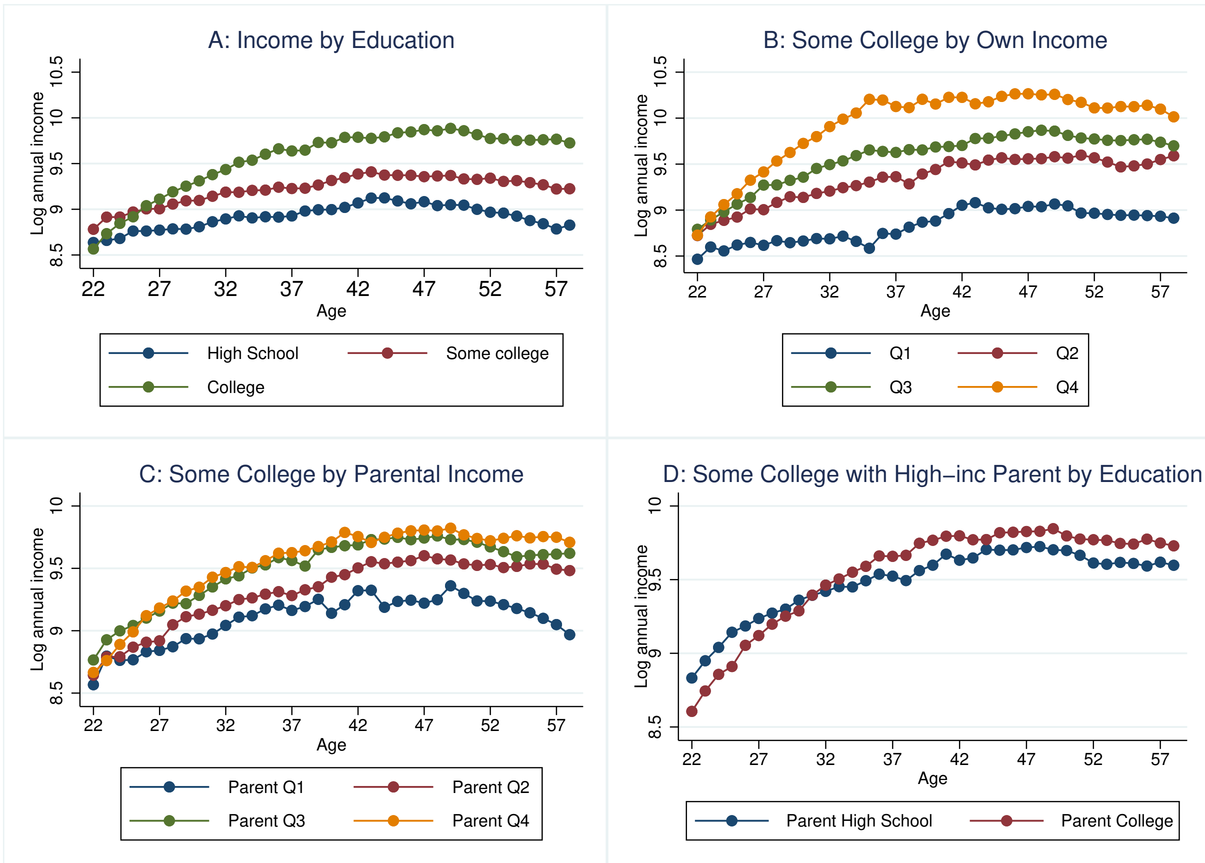


Notes: Panel A shows income trajectories by education category. Panel B focuses only on college-educated sons, who are split in four groups according to their annual income at age 35. Category Q1 refers to the bottom quartile and Q4 to the top. In Panel C, college-educated sons are divided in four groups, according to fathers' lifetime income. In Panel D, college-level sons whose fathers belong to the top half of lifetime income are divided in two additional groups: college-educated fathers and fathers with only primary schooling. We remove time effects from the lifecycle profiles to abstract from business cycle effects.

with education. Similar heterogeneity by occupation groups is documented in Appendix Figure A.1. Accounting for observable heterogeneity is, therefore, important. Panel B illustrates that the level of annual income and subsequent income growth are negatively correlated. Specifically, we split individuals within a given education group into four quartiles of their annual income at age 35. Individuals in the bottom quartile have substantially stronger income growth in the following years, while those in the top quartile experience the lowest growth. Transitory shocks are one explanation for this regression to the mean. Apart from attenuating intergenerational estimates via its effect on parental income (Atkinson, 1980), it may also complicate corrections for lifecycle dynamics in child income (see Section 4).[14]

[14]Transitory shocks also complicate the detection of a HIP component in income growth (Guvenen, 2009).

Figure 2: Components of the Income Process in the PSID



Notes: Panel A shows income trajectories by education category. Panel B focus only on individuals with at least some college, who are split in four groups according to their annual income at age 35. Category Q1 refers to the bottom quartile and Q4 to the top. In Panel C, individuals with at least some college are divided in four groups, according to parental lifetime income. In Panel D, individuals with at least some college whose parental income belong to the top median of the distribution are divided in two groups: parents with at least some college and parents with only primary schooling.

Panel C provides evidence on a more controversial question, namely if residual income $y^i_{h,t}$ grows at an individual-specific and deterministic rate or follows a random walk. The figure plots the average income profiles of college graduates by the quartile of their *fathers'* lifetime income. We find that even conditional on education, sons with fathers in the top income quartile have substantially higher income growth. College-educated sons with high-earning fathers tend to have lower income in their mid 20s, but around 40 percent higher incomes around age 40, compared to college-educated sons from low earning families (Figure 1, Panel C). We find similar evidence in the PSID (Figure 2, Panel C) and conditional on occupation (Figure A.1, Panel C). We also find similar patterns when considering additional dimensions of family background. For example, in Panel D we restrict attention to college graduates whose fathers' income is above the median, comparing their mean income trajectory by fathers' education. We find that those with more educated fathers have steeper income profiles than those with less educated fathers, a pattern that also replicates in the PSID (Figure 2 Panel D).

These findings can be rationalized by standard arguments from human capital theory. For example, the Ben-Porath model of human capital investments implies that differences in the return to human capital investments affect the slope of age-income profiles (Ben-Porath 1967). The pattern shown in Panel C of Figures 1 and 2 could therefore indicate that the rate of human capital accumulation varies systematically with parental income, even conditional on an individual's formal education. Alternatively, the level of human capital investments might depend on parental income, either directly because of liquidity or credit constraints, or indirectly, because of the effect of wealth on risk aversion (Blanden et al., 2022).

However, one might ask whether flexibly controlling for a wider set of observable characteristics could capture this "unobserved" heterogeneity. Table 2 therefore reports a more systematic analysis in the Swedish data, regressing income growth on father's lifetime income and different sets of background characteristics, all interacted with six different age groups. Column (1) reports the raw differences, showing that a log-unit increase in father's lifetime income is associated with a 9.1 log point higher income growth between age 25 and 30. This difference in growth rates diminishes over age, and eventually turns negative. The pattern still holds when controlling for differences in observable characteristics, although the magnitudes decline. For example, the estimates conditional on education in column (2) imply that the incomes of children from fathers with a log-unit higher lifetime income grow more than 4.7 log points faster between age 25 and age 40 but about 1.2 log points slower between age 40 and age 55. The pattern remains very similar when adding controls for cognitive and non-cognitive skill scores from the military draft in column (3). In columns (4) and (5) we introduce controls for occupations (two-digit modal occupation in age 25-30). The parental-income gradient remains large when controlling for occupation only, but becomes comparatively small when conditioning jointly on both education and occupation. In column (6) we introduce all controls simultaneously and add demographic characteristics (birth order, family size, immigrant status). While these controls capture much of the heterogeneity, income growth at younger ages still increases in parental income.

The role of unobserved heterogeneity in income processes has remained controversial, as it is difficult to distinguish from stochastic processes with high persistence. By combining long income series with information on family background, one can however provide direct evidence on this question: income growth varies systematically with parental characteristics, even after controlling for individuals' own observable characteristics. Because parental characteristics are predetermined with respect to, and potentially observed by the child, this pattern is more readily interpreted as a deterministic factor (HIP, in line with arguments by Guvenen 2009) instead of a stochastic

Table 2: Heterogeneity in Income Growth by Parental Income (Swedish data)

	(1)	(2)	(3)	(4)	(5)	(6)
Log (Father's Income)/100						
x Age 25-30	9.119***	2.946***	2.782***	3.656***	1.361***	1.458***
	(0.229)	(0.214)	(0.247)	(0.219)	(0.213)	(0.247)
x Age 30-35	4.799***	1.819***	1.254***	2.689***	1.500***	1.048***
	(0.194)	(0.198)	(0.224)	(0.197)	(0.198)	(0.227)
x Age 35-40	1.276***	-0.022	-0.126	0.154	-0.297	-0.299
	(0.189)	(0.194)	(0.223)	(0.196)	(0.197)	(0.227)
x Age 40-45	0.123	-0.470**	-0.342	0.034	-0.291	-0.212
	(0.177)	(0.183)	(0.209)	(0.184)	(0.186)	(0.214)
x Age 45-50	-0.223	0.044	-0.078	0.154	0.189	-0.020
	(0.173)	(0.178)	(0.207)	(0.180)	(0.182)	(0.211)
x Age 50-55	-1.276***	-0.778***	-0.601**	-0.663***	-0.489**	-0.320
	(0.171)	(0.176)	(0.203)	(0.178)	(0.180)	(0.208)
Education x Age		X	X		X	X
Occupation x Age				X	X	X
Skill scores x Age			X			X
Demographics x Age						X
N	946,534	946,399	741,378	916,201	916,077	717,500
R-sq	0.072	0.110	0.114	0.102	0.122	0.127

Notes: Robust standard errors in parentheses. The dependent variable in each of the columns is the change in log annual income over the indicated age range. Education distinguishes seven levels of highest educational attainment. Occupation is at the two-digit level (66 groups). Skill scores are cognitive and non-cognitive skill from the military draft. Demographic variables are birth order, family size, and an immigrant dummy. All these variables, as well as father's log lifetime income/100, are interacted with the indicators for the six age groups. We remove time effects from annual income observations to abstract from business cycle effects. Annual incomes below 20% of the yearly in-sample median are excluded. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

shock that would come as a surprise to the individual (RIP).[15] But while Guvenen assumes that the individual-specific component is linear in experience, our evidence suggests that family background matters primarily at young age, and that the sign of its relation to income growth may flip at older ages. Such non-linear pattern would be difficult to detect in the higher-order autocovariances of earnings that are often used to identify HIP components (Guvenen 2009, Hoffmann 2019).

The observation that children from high-income parents tend to have steeper income profiles matters for many distributional questions. In particular, the failure to account for this difference in income growth can lead to substantial bias in estimates of income mobility, as we show next.

[15]While our evidence supports the HIP model, many of our arguments relate to properties of the income process that are common to both models.

4 Bias Corrections in the Intergenerational Literature

In many settings, the outcome of interest is an individual's lifetime (or long-run) income, but only short snapshots of income are available. To address this issue the literature has considered two alternative approaches, which vary in the specificity in which the income process is being considered: (i) errors-in-variables models that formalize the relation between (observed) annual and (unobserved) lifetime income, and (ii) models of the income process that determine the relation between annual and lifetime incomes.

4.1 Errors-in-Variables Models

Errors-in-variables models have a long tradition in intergenerational research. The income process is not explicitly modeled, but its assumed properties inform the errors-in-variables assumptions. Many applications rely on a generalization of the classical errors-in-variables model, which allows for the relation between annual and lifetime incomes to vary systematically over the lifecycle (Haider and Solon 2006). Its key implication is that lifecycle bias can be reduced by measuring incomes around midlife, a simple rule-of-thumb that has been widely adopted in the literature. While this strategy can greatly reduce lifecycle bias, it is subject to some limitations (discussed further in Appendix B). First, income growth varies with parental background even conditional on a child's own lifetime income, such that lifecycle bias may not be fully eliminated at the "optimal" age as prescribed by the generalized errors-in-variables model (Nybom and Stuhler, 2016a). More importantly, this optimal age will vary across countries or time and is typically unknown in applications. Researchers therefore measure incomes at *some* age in midlife, subject to data limitations, resulting in substantial age differences across studies (see Table A.1). As even slight age variations can have large effects on the IGE (Table B.1), existing estimates are difficult to compare. Finally, income around midlife may simply not be observable for the population of interest, such as when interest centers on recent birth cohorts.

4.2 Modelling the Income Process

The alternative is to model the income process directly. In a first step, the shape of age-income profiles is estimated based on a set of individual characteristics. In a second step, those estimates are used to predict lifetime income for each person. Typically only short income spans are available for each person, so the challenge is to extrapolate those snapshots to the complete life cycle without introducing biases that co-vary with the explanatory variable of interest (e.g., parental income). We propose such a "lifecycle estimator" in Section 5.1. However, to motivate our approach it is instructive to first review existing work on this problem.

Accounting for Individual Characteristics. One potential strategy is to model the income process as a function of an individual's own characteristics. In a first step, we estimate

$$y_{ict} = \alpha_i + g(A_{ict}, Z_{ic}, \gamma) + \varepsilon_{ict}, \tag{2}$$

where y_{ict} is log income of individual i from birth cohort c in period t, α_i are individual fixed effects, and $g(A_{ict}, Z_{ic})$ a flexible interaction of age with a vector of individual predictors of the age-income profile Z_{ic} (such as education). The estimates can then be used to predict a measure of long-run income. Of course, the population of interest might only be observed over part of their lifecycle; often, children are only observed at young and their parents at older age. Different studies follow different strategies to address this issue. Hertz (2007) predicts incomes at one particular age rather than over the entire lifecycle. Vogel (2007) predicts the entire lifecycle based on the assumption that parents and children have similar age-income profiles (conditional on individual fixed effects and observables). And Justman and Stiassnie (2021) pool many cohorts to estimate lifecycle profiles, a strategy that we follow in our trends analysis in Section 6.

As noted by Hertz (2007) and Justman and Stiassnie (2021), this strategy has advantages compared to the errors-in-variables approach. Individual fixed effects capture heterogeneity in levels, and by allowing slopes to differ by education, an important source of heterogeneity in income growth is accounted for (Panel A in Figures 1 and 2). However, our finding that income growth varies with parental background even within education or occupation groups (Panel C in Figures 1 and A.1) suggests that the procedure may remain sensitive to lifecycle effects. Table 3 probes this hypothesis based on our Swedish benchmark sample, comparing estimates from partial profiles against the "true" IGE based on lifetime incomes. For Panel A, we estimate equation (2) using incomes from a given age range (see left column) and flexible age-education interactions to predict an individual's income at a given age. This reduces lifecycle bias compared to directly using annual income at that same age (bottom panel), but the corrected estimates still increase with (i) the age at which individual incomes are predicted, and (ii) the age range for which equation (2) is estimated. As shown in Panel B, this last pattern still holds if we aggregate predictions over the entire lifecycle.[16]

Why are estimates based on equation (2) so volatile? Because children from high-income families tend to experience higher income growth even after conditioning on their own education or occupation (Section 3), estimates of the fixed effects α_i – and therefore lifetime incomes – depend on the age range included in the first-step estimation. For example, when observing only early (late)

[16]Specifically, we split each income profile into two copies, with income for the "younger" copy assumed to be observed in each of the age ranges of Table 3, and the "older" copy being observed thereafter. This allows us to focus on the problem of missing income information for a given person, while abstracting from the issue that certain age ranges are missing for the entire population of interest.

Table 3: IGE Estimates Accounting for Age-Education Profiles

Observed Age Range	Panel A Prediction at Age 25	30	35	Panel B Prediction of Complete Profiles
25-30	0.041	0.162	-	0.209
	(0.003)	(0.003)		(0.004)
25-35	0.077	0.154	0.219	0.231
	(0.003)	(0.003)	(0.003)	(0.004)
25-40	0.116	0.167	0.216	0.249
	(0.003)	(0.003)	(0.003)	(0.004)
25-45	0.140	0.183	0.217	0.259
	(0.003)	(0.003)	(0.003)	(0.005)
25-58	0.182	0.224	0.248	0.277
	(0.003)	(0.003)	(0.003)	(0.004)
Annual	0.001	0.172	0.253	-
	(0.003)	(0.004)	(0.004)	
True	0.253	0.253	0.253	0.253
	(0.003)	(0.003)	(0.003)	(0.003)

Notes: Benchmark sample from Swedish registers, cohorts 1952-60, N =197,242 observations. The top rows report estimates of the IGE based on the first-step estimation of equation (2), which includes a quartic in age interacted with four education groups. In Panel A, we predict child income at age 25, 30 or 35 (within the observable range). In Panel B, we predict child income over the entire lifecycle (by randomly assigning each observation of the banchmark generation into a "young" or an "old" copy, as explained in Section 5.1).

ages, we understate (overstate) the lifetime income of those with low initial incomes but stronger income growth. Thus, the earlier child incomes are observed, the more the IGE is understated. The argument is illustrated further in Appendix C. The same issue also affects the estimation of mobility *trends*. Many studies (including Hertz 2007 and Justman and Stiassnie 2021) keep the age at which incomes are predicted fixed across cohorts, thereby eliminating the variability of estimates across the columns in Panel A of Table 3. However, estimates of the IGE also vary with respect to the age composition of the sample, across the rows of the same table. For example, predicting incomes at age 25, the estimates increase from 0.056 to 0.162 when the sampling range grows from age 25-30 to age 25-45. This age composition is typically not held constant when estimating mobility trends.[17] In Appendix C we confirm that trend estimates based on (2) and rolling age windows are indeed susceptible to lifecycle effects.

[17]For example, the younger cohorts in both Hertz (2007) and Justman and Stiassnie (2021) are observed over a shorter and earlier age range than older cohorts.

Accounting for the Covariance Between Income Levels and Slopes. An alternative approach allows for income growth to vary with income *levels*.[18] This approach is not commonly used in applications, but its potential advantages have been demonstrated by Creedy (1988). Creedy first demonstrates how changes in the dispersion of income over age affect estimates of the IGE.[19] To account for these changes, it is assumed that individuals retain a constant *relative position* in the income distribution (implying that income growth and levels are correlated). Standardized incomes can then be constructed for each age by rescaling an observed income observation according to

$$z_t = (y_t - \mu_t)/\sigma_t, \tag{3}$$

where μ_t and σ_t are the mean and standard deviation of log income at age t (which could be further distinguished by occupation or other characteristics). Having predicted complete profiles then enables the construction of a measure of lifetime income for each person. In Appendix D, we show that extrapolations based on expression (3) can indeed reduce lifecycle bias. But the resulting estimates tend to overstate the IGE. The source for this upward bias becomes clear from Panel C of Figure 1: due to the influence of transitory shocks, income growth tends to be negatively correlated to initial levels. This mean reversion is not accounted for by expression (3), which therefore overstates lifetime inequality and the IGE.[20] One way to address this issue is to allow for a systematic relation between income levels and growth, as we consider below.

5 A Lifecycle Estimator for the Intergenerational Elasticity

In this section, we propose a lifecycle estimator of lifetime income and intergenerational mobility that accounts for differences in income growth by family background in the child generation. Using long income series from Swedish registers and the PSID, we illustrate that this estimator can be applied in a wide range of data scenarios and that it provides more robust estimates of the IGE than estimators based on simple income averages.

[18]Such heterogeneity could be captured either by estimating individual-specific slopes (as in Jäntti and Lindahl 2012) or by estimating how slopes vary with parental background. We do not pursue the first option here because individual profiles are "wiggly" Jenkins (2009), such that direct extrapolation from partially observed slopes would produce unstable predictions of lifetime income if only few income observations are available per person (a common scenario in intergenerational research).

[19]The study contains therefore one of the first systematic discussions of "lifecycle bias" in intergenerational estimates (see also Jenkins 1987).

[20]Overstating inequality in the child generation leads to an upward bias in estimates of the IGE as the latter is equal to the intergenerational correlation scaled by the ratio of the standard deviations of income in the child and parent generations.

5.1 The Lifecycle Estimator

The estimation consists of two steps. In a first step, we estimate and predict each person's complete lifecycle income profile based on partial income snapshots and individual and parental characteristics. In a second step, we estimate the IGE based on lifetime incomes constructed from the predicted income profiles. This lifecycle estimator uses the available income information more fully than the rule-of-thumb implementations based on income averages that are prevalent in the current literature. Our two-step approach is similar in spirit to earlier contributions, such as Creedy (1988), Hertz (2007) or Justman and Stiassnie (2021), but explicitly accounts for the observation that income growth varies with parental background even conditional on an individual's own characteristics (see Section 3).

Specifically, in a first step we use OLS to estimate variants of

$$y_{ict} = \alpha_i + g(A_{ict}, Z_{ic}, \gamma) + f(A_{ict}, Z_{ic}, P_{ic}, \delta) + \varepsilon_{ict}, \tag{4}$$

where y_{ict} is log income of individual i from cohort c in period t, α_i are individual fixed effects, $g(A_{ict}, Z_{ic}, \gamma)$ represents interactions between age and a vector Z_{ic} of the individual's own characteristics, such as education; $f(A_{ict}, Z_{ic}, P_{ic}, \delta)$ represents interactions between age, education, and parental characteristics P_{ic}; and γ and δ are vectors of coefficients to be estimated. In our application, P_{ic} contains log parental income and indicators for parental education. Our preferred specification allows for a quadratic in age in $f(\cdot)$, as income growth varies more strongly with parental background in the early career than at later ages (see Figures 1 and 2).

We estimate variants of equation (4) with or without individual fixed effects α_i. While allowing for individual intercepts might seem like an obvious improvement, we found that the flexibility of a full set of fixed effects comes at a cost, making it harder to capture the heterogeneity in income *slopes* – in particular when only short snapshots of incomes are observed, or when the functional form of $f(A_{ict}, Z_{ic}, P_{ic}, \delta)$ is misspecified. In these settings, "no-FE" estimators that allow for intercepts to only vary with the regressor of interest (i.e., by replacing α_i with a function of parental income) can show superior performance, as we demonstrate below.

However, information on parental background may not always be observed. As an alternative specification, we therefore allow income slopes to vary with the level of an individual's *own* income. This *slope-level estimator* is motivated by the observation that individuals from high-income families have both higher levels and steeper slopes than those from poorer families (see Figures 1 and 2, Panel C) and that income profiles tend to fan out over age (Creedy 1988), implying a positive correlation between income levels and growth. We allow for income growth to vary with the individual fixed effect rather than current income, to address the strong mean reversion in the latter due to transitory noise (see Section 3). Specifically, we estimate

$$y_{ict} = \mu_i + g(A_{ict}, Z_{ic}, \eta) + f(A_{ict}, Z_{ic}, \mu_i, \theta) + \nu_{ict}, \tag{5}$$

where individual characteristics Z_{ic} are interacted with age and the individual fixed effect μ_i. This model can be estimated recursively (as for example in de la Roca and Puga 2017).[21] Our preferred implementation interacts the individual fixed effects with a quadratic in age, since income growth varies more strongly at early age.

Estimations based on equation (4) or (5) are subject to three conceptual issues. First, the estimation consists of multiple steps, which affects statistical inference. As our benchmark sample for Sweden is large we initially ignore sampling error in the first-step estimation, but later return to this issue to study how sensitive the estimators are to sample size. Second, the dependent variable in equation (4) is the *logarithm* of annual income, and conversion to absolute incomes for the construction of lifetime incomes gives rise to a well-known re-transformation problem: while the fitted values from the estimation of equation (4) have mean zero by construction ($E[\varepsilon_{ict}] = 0$), their mean will be positive after transformation ($E[exp(\varepsilon_{ict})] > 0$).[22] We address this issue using the solution proposed by Wooldridge (2006).[23] A third and conceptually more central issue is that in many applications the population of interest is only observed at young age, so their income profiles need to be extrapolated over the non-observed age range. We initially abstract from this issue by exploiting the fact that our benchmark samples include long income series for each person. Specifically, we randomly split each income profile into a "young" or an "old" group. If assigned to the "young" group, the income profile is assumed to be observed only up to some age threshold. In contrast, if assigned to the "old" group, the income profile is assumed to be observed only thereafter. This allows us to focus on the problem of missing income information for a given person, while abstracting from the issue that certain age ranges are missing for the entire population of interest. We return to this extrapolation issue in a robustness analysis in the next subsection, as well as in Section 6 when estimating mobility trends for recent birth cohorts.

[21] We first derive estimates of the individual fixed effect μ_i by estimating equation (5) while omitting $f(\cdot)$. We then estimate the complete equation (5) with $\widehat{\mu}_i$ included in $f(\cdot)$. This second step can be iterated until estimates of the individual fixed effects converge. However, as further iterations have only negligible effects on our estimates we report estimates from a single iteration below.

[22] If this expectation were constant across individuals and linearly separable in log lifetime income, it would only affect the intercept of the intergenerational regression, not the elasticity of interest. But $E[exp(\varepsilon_{ict})]$ will tend to be larger for individuals with low lifetime income if their income tends to be more variable around the mean tendency over the lifecycle.

[23] Specifically, we estimate complete lifecycle profiles of each individual in the child generation, based on a quartic in age interacted with education dummies and individual fixed effects, to construct $SM_{ic} = \sum_{t=25}^{58} exp(\varepsilon_{ict})$ to adjust the predicted lifetime income accordingly.

5.2 Performance of the Lifecycle Estimator in Swedish Registers

Table 4 presents evidence on the performance of our proposed lifecycle estimator in terms of estimating the intergenerational elasticity of income. We consider different age thresholds, assuming that child income is observed only over age 25-27 (first row), age 25-30 (second row), and so on. For comparison, column (1) reports benchmark estimates based on "true" lifetime incomes, which are about $\widehat{\beta} = 0.26$.[24] In column (2), we report estimates based on pooled annual incomes from age 25 to the indicated upper age bound (e.g. age 25-27 in the first row). As expected, the estimated IGE is very sensitive to the age at measurement, being as low as 0.05 when child incomes are measured only until age 27 but increasing monotonically when increasing that age range.

Table 4: The Lifecycle Estimator (Swedish Registers)

	Direct estimator		Lifecycle estimator				
	Lifetime	Annual	Baseline – FE	Parental Linear FE	Parental Quadratic FE	Parental Quadratic no FE	Slope-level Quadratic FE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Age 25-27	0.259	0.051	0.151	0.199	0.236	0.265	0.196
	(0.003)	(0.002)	(0.004)	(0.004)	(0.004)	(0.002)	(0.006)
R^2	0.052	0.002	0.017	0.030	0.042	0.202	0.013
Age 25-30	0.259	0.106	0.189	0.225	0.265	0.264	0.249
	(0.003)	(0.001)	(0.003)	(0.003)	(0.003)	(0.002)	(0.005)
R^2	0.052	0.007	0.030	0.043	0.059	0.201	0.026
Age 25-35	0.259	0.163	0.206	0.231	0.263	0.263	0.254
	(0.003)	(0.001)	(0.003)	(0.003)	(0.003)	(0.002)	(0.004)
R^2	0.052	0.014	0.040	0.050	0.064	0.199	0.036
Age 25-40	0.259	0.209	0.225	0.270	0.277	0.262	0.269
	(0.003)	(0.001)	(0.003)	(0.003)	(0.003)	(0.002)	(0.004)
R^2	0.052	0.019	0.047	0.066	0.069	0.198	0.042
Age 25-45	0.259	0.239	0.240	0.286	0.274	0.260	0.273
	(0.003)	(0.001)	(0.003)	(0.003)	(0.003)	(0.002)	(0.004)
R^2	0.052	0.022	0.051	0.071	0.066	0.197	0.047

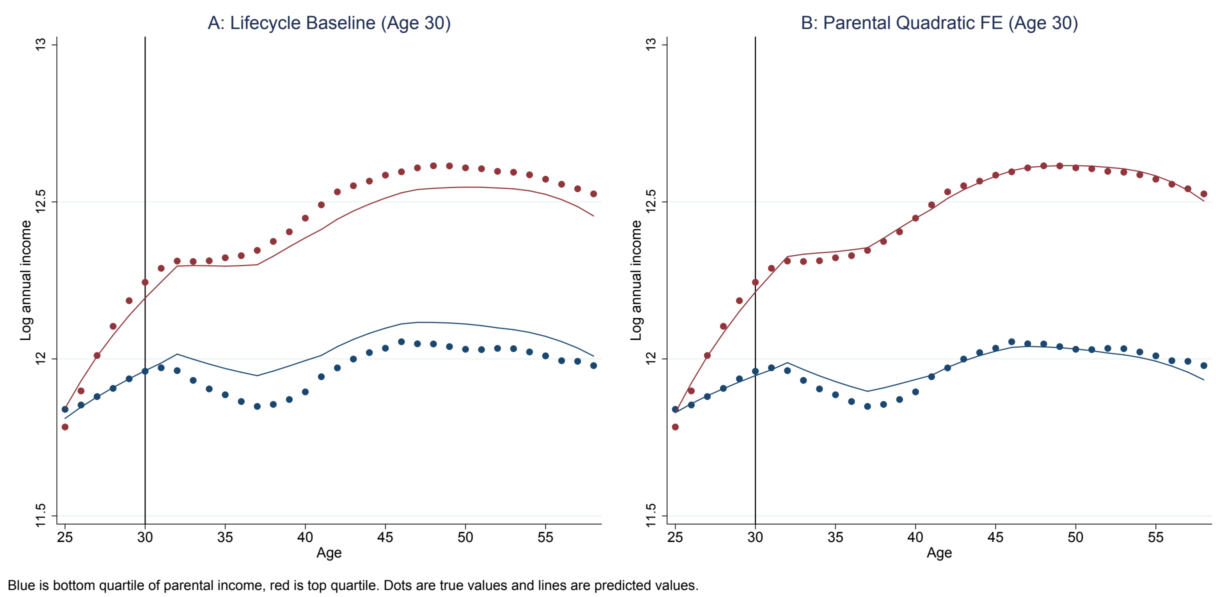
Notes: The table reports the slope coefficient from a regression of son's income on father's lifetime income with $N = 189,251$ observations. The measure for son's income is lifetime income in column (1), the pooled annual incomes from age 25 to the indicated upper age bound in column (2), or the predicted lifetime income from a first-step estimation in the indicated age range of equation (4) in columns (3)-(6) or equation (5) in column (7). See text for detailed definitions of each estimator. Robust standard errors in parentheses.

In columns (3) to (7) we implement different variants of the lifecycle estimator. Column (3) reports estimates from a baseline lifecycle estimator based equation (4) that distinguishes four

[24] Note that the number of observations and benchmark IGE is slightly different from the ones in Table 3. Here we keep in the sample only observations with parental education observed.

education groups (as defined in Figure A.1), but does not include parental characteristics P_{ic}. This baseline lifecycle estimator is similar in spirit to the estimators used in prior studies (see Section 4), and performs better than a direct estimator using annual incomes. However, it still varies with the age at which income snapshots are measured and understates the IGE by nearly one third when incomes are measured at age 25-30. As discussed in Section 4, this estimator remains sensitive to age as it does not account for differences in income growth by parental background. This issue is also illustrated in Panel A of Figure 3, which plots the mean actual (dots) and predicted (solid lines) lifecycle profiles of children from the top (red) or bottom quarter (blue) of the parental income distribution when child income is observed at age 25-30. The baseline lifecycle estimator understates the true income growth at the top and overstates growth at the bottom of the parental income distribution, leading to downward-biased estimates of the IGE.

Figure 3: Comparison between Actual and Predicted Profiles



Notes: The figure plots the actual log income profiles (dots) and predicted profiles (solid lines) separately for the top (red) and the bottom (blue) quartiles of parental income. To predict complete profiles based on income observations until age 30 we implement a lifecycle estimator without parental interactions (baseline, Panel A) or with quadratic interactions between child age and parental income (parental quadratic, Panel B).

Column (4) therefore reports estimates from the "parental" lifecycle estimator, in which the first-step estimation of equation (4) includes linear interactions between child age and parental income and education. This estimator performs better than the baseline lifecycle estimator, in particular at young ages, as it captures some of the heterogeneity in income growth by parental background. However, the estimated IGE still increases systematically with the age at which incomes are measured, and understates the true IGE when incomes are measured at very early age.

The reason follows from Figure 3: the association between income growth and parental background is more pronounced at early than late ages, so linear extrapolations from early age work poorly.

We therefore consider a "parental quadratic" lifecycle estimator in column (5), which uses a quadratic rather than linear polynomial in child age interacted with parental income. The estimates are now quite close to the benchmark for all age ranges. The reason for this good performance is that quadratic interactions capture the heterogeneity in income slopes by parental background quite well, as is illustrated in Figure 3B. Still, even with quadratic interactions, some bias remains when measuring incomes at very young age (e.g., age 25-27). Intuitively, if the functional form of $f(A_{ict}, Z_{ic}, P_{ic}, \delta)$ in equation (4) does not correspond to the true functional form, some of the heterogeneity in income growth will instead be captured by the individual fixed effects α_i.[25]

In column (6), we therefore present estimates from a lifecycle estimator without individual fixed effects ("no-FE"), in which intercepts are allowed to vary systematically with parental income but we do not try to estimate individual variation around that mean tendency. This variant of the lifecycle estimator is insensitive to the age at which income snapshots are observed, and is always close to the benchmark. It captures the heterogeneity in income slopes even if individuals' income is only observed at very young ages (age 25-27). These results illustrate that if the object of interest is the variation in incomes by parental background, there is a potential advantage in modelling only that specific form of heterogeneity rather than attempting to capture individual-level variation in the intercepts.

Finally, in column (7) we report estimates from an alternative variant of the lifecycle estimator that directly accounts for the correlation between income levels and slopes (see Section 4.2). Specifically, this "slope-level" lifecycle estimator is based on equation (5) and includes an interaction between the individuals own estimated fixed effect and a quadratic in age. The estimator performs largely similarly to the corresponding parental lifecycle estimator, suggesting that systematic variation in income growth by parental background could potentially be addressed indirectly, without observing parental characteristics, by accounting for the covariance between income level and growth. As an exception, this estimator performs less well when incomes are observed only at age 25-27, presumably because income levels are not very predictive about long-run income at such early age.

Robustness. While these results are promising, the estimates in Table 4 are all based on large samples with many income observations.[26] In many applications, researchers observe fewer

[25]Specifically, when splitting the income profiles of children into a "young" and "old" copy to estimate equation (4), we estimate larger fixed effects for the "old" than the "young" part.

[26]Moreover, recall that we pooled individuals across all age ranges for the estimation of equation (4). If the cohorts of interest are not observed over their entire lifecycle, researchers need to approximate the overall shape of their lifecycle profiles via other cohorts or other means. We return to this issue when estimating mobility trends for recent birth cohorts in Sweden and the US in Section 6.

individuals, or fewer income observations per individual. We therefore also explore the performance of our preferred variants of the lifecycle estimator in such settings. First, we study how their performance varies with the number of annual observations available per child. Specifically, we randomly select six annual income observations for each person within the indicated age range and successively drop further observations until only two observations remain per person. As shown in Table A.2, this increases the noise in the estimation of lifetime incomes and reduces the R^2 in the intergenerational regression, but estimates of the IGE remain stable in terms of their mean.

Second, we vary the number of sampled individuals. A particular concern is that the shape of lifecycle profiles cannot be precisely estimated in smaller samples. To probe this concern, Table A.3 reports estimates from differently sized samples. We draw fractions $1/k$ of our original sample (as indicated in the top row) and then implement the benchmark estimator based on lifetime incomes, as well as the *parental* (with individual fixed effects) and *slope-level* variants of the lifecycle estimator. The table reports the mean coefficient estimate and the standard deviation of those point estimates across repeated draws from the main sample. The mean of the lifecycle estimator appears robust to sample size, and while its precision decreases in smaller samples, it is not substantially more noisy than the corresponding benchmark estimates based on complete lifetime incomes.

Another concern one might have is that our estimator works well because we use the same cohorts and income years in the first- and second-step estimations, but that it would work more poorly in less ideal situations. We thus explore the out-of-sample performance of the estimators by varying the cohorts and income years used in the first-step prediction, and then using these predictions to estimate the IGE for our baseline cohorts born 1952-1960.[27] Table A.4 shows in column (2) that the lifecycle estimators are largely unaffected when including a wider set of cohorts in the first step. However, columns (3) and (4) indicate that the estimators tend to overstate the IGE somewhat when we in addition only consider more recent income years, which implies that we observe the baseline cohorts only at a relatively old age. However, we also see that this upward bias is considerably smaller compared to when simply using all observed incomes for the same cohorts and years (see row 2).

5.3 Performance of the Lifecycle Estimator in the PSID

We next study how the lifecycle estimator performs using PSID data. We focus on our benchmark sample of individuals born 1952-1960. For these cohorts, we observe nearly complete income profiles and can therefore perform an exercise analogous to the one conducted for Sweden above. We again consider different age thresholds, assuming that child income is observed only at age 22-27, age 22-30, and so on.

[27]Note that we here use the trends sample (see next section), which implies that the corresponding benchmark estimate is slightly lower.

Column (1) of Table 5 shows that our benchmark estimate based on "true" lifetime incomes for the child generation is around 0.43. This estimate is similar to other estimates reported in the literature, but is still downward biased from the use of short income snapshots in the *parent* generation: indeed, Mazumder (2016) argues that the true elasticity in the US is closer to 0.6. Column (2) reports estimates based on annual incomes for the child generation, pooling observations between age 22 and the upper bound indicated in each row. The qualitative pattern is similar, but the estimates are less sensitive to the observed age range than our estimates for Sweden (where individuals tend to enter the labor market at a later age). Yet, the estimates are lowest at early age, and remain substantially below the benchmark for all considered age ranges.

Table 5: The Lifecycle Estimator (PSID)

	Direct estimator		Lifecycle estimator				
	Lifetime	Annual	Baseline – FE	Parental Linear FE	Parental Quadratic FE	Parental Quadratic no FE	Slope-level Quadratic FE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Age 22-27	0.426	0.242	0.319	0.373	0.375	0.428	0.380
	(0.035)	(0.016)	(0.034)	(0.034)	(0.034)	(0.014)	(0.045)
R^2	0.142	0.040	0.090	0.121	0.122	0.520	0.074
Age 22-30	0.426	0.280	0.362	0.416	0.432	0.428	0.428
	(0.035)	(0.013)	(0.033)	(0.033)	(0.033)	(0.014)	(0.042)
R^2	0.142	0.053	0.116	0.151	0.162	0.520	0.105
Age 22-35	0.426	0.321	0.400	0.449	0.456	0.428	0.465
	(0.035)	(0.011)	(0.034)	(0.034)	(0.034)	(0.014)	(0.042)
R^2	0.142	0.064	0.131	0.163	0.167	0.520	0.121
Age 22-40	0.426	0.354	0.412	0.465	0.465	0.428	0.465
	(0.035)	(0.010)	(0.034)	(0.034)	(0.034)	(0.014)	(0.040)
R^2	0.142	0.070	0.139	0.172	0.172	0.520	0.131
Age 22-45	0.426	0.373	0.397	0.426	0.421	0.428	0.425
	(0.035)	(0.009)	(0.034)	(0.034)	(0.034)	(0.014)	(0.037)
R^2	0.142	0.073	0.135	0.153	0.150	0.520	0.129

Notes: The table reports the slope coefficient from a regression of offspring income on parental lifetime income with $N = 892$ observations. The measure for offspring income is lifetime income in column (1), the pooled annual incomes from age 22 to the indicated upper age bound in column (2), or the predicted lifetime income from a first-step estimation in the indicated age range of equation (4) in columns (3)-(6) or equation (5) in column (7). See text for detailed definitions of each estimator. Robust standard errors in parentheses.

In columns (3) to (7) we report different variants of the lifecycle estimator. We keep the discussion brief as the patterns are very similar as in the Swedish registers (although the point estimates are noisier, due to the smaller sample size of the PSID). Column (3) reports estimates from equation (4) without parental characteristics P_{ic}, distinguishing three education groups. As in the Swedish data, this "baseline" lifecycle estimator tends to understate the IGE, in particular when incomes are observed only at early age. Columns (4) and (5) reports the parental lifecycle estimator as defined in equation (4) with either linear or quadratic interactions between child age and parent income. These variants perform better than the baseline estimator, in particular at early age. However, they still understate the IGE if incomes are measured at a very young age, for the same reasons as illustrated in the Swedish data. Column (6) reports estimates from the parental lifecycle estimator without individual fixed effects. As in the Swedish case, this estimator is more stable over age, and always close to the benchmark estimate. Finally, column (7) reports estimates from the slope-level lifecycle estimator based on equation (5). It performs better than the baseline estimator, but still varies with the age at which child incomes are measured.

Overall, the lifecycle estimator interacting a quadratic in child age with parental income, with or without fixed effects (columns 5 and 6), performs well in both the Swedish and US data. It nearly eliminates lifecycle bias in both samples, with estimates fluctuating closely around the benchmark. We showed that the mean estimates are quite stable with respect to (i) the *age range* in which the child generation is observed, (ii) the *number of income observations* available for each person, and (iii) the *number of individuals* in the sample. This stability makes the estimator attractive for comparative purposes, such as mobility comparisons across countries or over time.

6 Recent Trends in Income Mobility in Sweden and the US

After testing its performance, we apply the lifecycle estimator to study mobility trends in Sweden and the US. The estimator's key advantage in this context is its stability with respect to the age range over which the child generation is observed (see Section 5), which makes it also suitable for studying recent cohorts who can only be observed at young age. Our objective is two-fold. First, we examine whether earlier estimates may have been distorted because of lifecycle biases. In particular, the evidence in Section 4 suggests that earlier studies using varying age windows across cohorts may have understated the time trend in the IGE (i.e., hiding a potential decrease in income mobility). Second, we can estimate mobility trends for younger, more recent birth cohorts not considered in prior studies, who are particularly interesting from a policy perspective.

6.1 A Lifecycle Estimator for Mobility Trends

Because recent cohorts can only be observed at a young age, their income profiles need to be extrapolated over the unobserved age range. For example, incomes of cohorts born in 1989 are only observed up to age 29 in the Swedish data and 27 in the PSID. One way to address this issue is to pool individuals of different cohorts (as in Table A.4), and to assume that conditional on education or other observables, the shape of age-income profiles remains constant across cohorts (Vogel 2007, Haider and Solon 2006). However, age-income profiles might change across cohorts (see also Eshaghnia et al. 2022). To illustrate this point, Figure A.2 plots income profiles by education for four different Swedish cohort groups, showing that college-educated workers born in the 1980s have steeper income growth than older cohorts.[28]

To capture those changes in the shape of age-income profiles, we can pool individuals of different cohorts and allow for income profiles to vary across cohorts depending on individuals' own and/or their parents' characteristics. Specifically, we extend equation (4) to estimate

$$y_{ict} = \alpha_i + g(A_{ict}, Z_{ic}, \gamma_c) + f_c(A_{ict}, Z_{ic}, P_{ic}, \delta_c) + \varepsilon_{ict}, \tag{6}$$

where $g(A_{ict}, Z_{ic}, \gamma_c)$ represents age interactions with a vector of the individual's own characteristics Z_{ic} (such as education); $f_c(A_{ict}, Z_{ic}, P_{ic}, \delta_c)$ represents interactions of age, own characteristics, and parental characteristics P_{ic} (such as parental income); and the c subscripts indicate that the slope coefficients are potentially allowed to vary across cohorts. As linear interactions generally perform poorly (see Section 5) and quadratic interactions might be unstable if extrapolating over wide age intervals, we interact dummies for decade of birth with parental income and a "standardized age profile" that corresponds to the average growth in income in the entire population (corresponding to a concave pattern).

6.2 Mobility Trends in Sweden

Table 6 reports estimates of the IGE in Sweden, distinguishing four groups of cohorts born between 1950 and 1989.[29] The first two columns report direct estimates based on annual incomes. For column (1), we pool all available income observations in a regression of (log) annual income of children on the log income of their father. The resulting estimates *decrease* monotonically across cohorts, from 0.23 for those born in the 1950s to 0.16 for recent cohorts born in the 1980s. In column (2), we instead consider incomes at a fixed age range available for all cohorts, age 25-30. The resulting estimates *increase* across cohorts, by nearly 80 percent. Neither specification seems plausible. The estimates in column (1) are based on different age windows for different cohorts, and direct estimates of the IGE tend to increase with the age at which child incomes are observed – explaining why the estimates are largest for earlier cohorts. The estimates in column (2) promise

[28] Appendix Figure A.3 provides the corresponding evidence by occupation. Differences in the shape of age-income profiles between cohorts may represent cohort or time effects, but the distinction is not crucial for our purposes, as both affect lifetime incomes and therefore the IGE.

[29] The estimated IGE for the 1950s cohorts is slightly lower than in our benchmark sample because of differences in how the samples were constructed. To keep quality constant across cohorts, parental income is measured as a shorter average in the trends sample (see Section 2), introducing attenuation bias. Moreover, our benchmark sample is restricted to fathers who were relatively young at the birth of the son, for whom parental income is better observed.

Table 6: Trends in Income Mobility in Sweden (Register data)

	Direct Estimator		Lifecycle estimator			
	Annual All ages	Annual Age 25-30	Baseline FE –	Parental FE –	Parental FE Cohort interaction	Parental no FE Cohort interaction
	(1)	(2)	(3)	(4)	(5)	(6)
Cohorts 1950-59	0.229 (0.002)	0.087 (0.002)	0.196 (0.002)	0.196 (0.002)	0.195 (0.002)	0.181 (0.001)
Cohorts 1960-69	0.223 (0.002)	0.136 (0.002)	0.206 (0.002)	0.211 (0.002)	0.212 (0.002)	0.204 (0.000)
Cohorts 1970-79	0.199 (0.002)	0.162 (0.002)	0.199 (0.002)	0.209 (0.002)	0.199 (0.002)	0.191 (0.000)
Cohorts 1980-89	0.162 (0.002)	0.154 (0.002)	0.181 (0.002)	0.197 (0.002)	0.165 (0.002)	0.162 (0.000)
R^2	0.025	0.017	0.057	0.061	0.058	0.456
Obs.	39,148,343	9,921,334	1,844,829	1,844,829	1,844,829	1,845,362
Individuals	1,844,829	1,842,203	1,844,829	1,844,829	1,844,829	1,845,362

Notes: Columns (1) and (2) are based on direct regressions in which we regress son's log annual income on father's lifetime income, pooling all available income observations at age 25-58 (column 1) or in a fixed age range 25-30 (column 2). Columns (3) to (6) report different variants of the lifecycle estimator based on all available income observations. Column (3) includes individual FEs and a quartic in age interacted with dummies for four educational groups. Column (4) adds a quadratic interaction between son's age and fathers' income and a linear interaction between son's age and father's education. We next add interactions between age x cohort dummies and a standardized profile in column (5) (see main text). Finally, column (6) follows the specification of column (5), but intercepts are a function of parental income rather than individual-specific (no fixed effects).

to address this issue by holding the age window fixed. However, lifecycle profiles can differ across cohorts, partly because increases in formal education translate into lower initial incomes and steeper income growth for later cohorts (Heckman and Landersø 2021), partly because even the education-specific profiles differ between cohorts (see Figure A.2). Trend estimates based on fixed age windows can therefore be misleading (see also Section 4.2).

Columns (3)-(6) report different versions of the lifecycle estimator based on equation (6). Column (3) shows the baseline specification that allows for age-income profiles to vary across education groups but not across cohorts (apart from shifts in their intercept via the fixed effects α_i). The estimates are stable over earlier cohorts but fall slightly for the most recent cohort group: the estimated IGE for children born in the 1980s is around 10 percent lower than for earlier cohorts, and that difference is statistically significant.[30] In column (4) we add interactions between parent (log) income or education and a quadratic of child age to account for variation in income growth by parental background. This addition has little effect on IGE estimates for the early cohorts, but increases the estimates for the more recent cohorts. Intuitively, the prediction of growth trajectories is particularly consequential for recent cohorts for which only early-age incomes are observed.[31] Finally, we allow for the education-specific income profiles to vary across cohort groups. In column (5) we interact child education with cohort-group dummies and a standardized age profile.[32] In column (6) we include the same interaction to account for cohort-variation in levels and slopes, but exclude the individual fixed effects α_i. These variants of the parental lifecycle estimator indicate that the IGE increased mildly between the 1950 and 1970s cohorts before dropping more substantially in the most recent cohorts.

In sum, while estimates based on a fixed age window suggest that mobility *decreased* substantially after the 1950s cohorts, accounting for lifecycle effects yields estimates that vary less across cohorts. Indeed, the IGE has remained remarkably stable for Swedish cohorts born in the 1950s, 1960s and 1970s, and mobility has *increased* for more recent cohorts.

6.3 Mobility Trends in the United States

Previous work found that income mobility has remained approximately stable in the US over the last few decades (Hertz 2007, Lee and Solon 2009, Chetty et al. 2014).[33] This stability is puzzling, given the concurrent increase in income inequality (Katz and Autor 1999) and a negative relation between inequality and mobility predicted by standard models (Solon, 2014) and in fact observed across countries (Blanden 2011; Corak 2013) and across regions within countries (Chetty et al. 2014; Nybom and Stuhler 2021). Some studies discuss mechanisms that might help to explain why such a plunge has not been observed.[34] Others argue that it is yet to happen. For example, Putnam et al. (2012) note that the "*adolescents of the 1990s and 2000s are yet to show up in standard studies of intergenerational mobility but [other evidence suggests] that mobility is poised*

[30]The estimates in column (1) and column (3) are quite similar for early cohorts for whom we observe nearly complete profiles, such that the first-step prediction of lifecycle profiles is less consequential.

[31]Specifically, children from high-income parents tend to have steeper income profiles even conditional on own education, in particular at early age (see Table 4).

[32]As explained earlier, rather than interacting the education-by-cohort-group dummies with a polynomial in age we interact them with a "standardized" age profile defined as the average age-income profile in the sample, thereby capturing the concavity of age-income profiles without including higher-order polynomial interactions.

[33]In addition, Justman and Stiassnie (2021) based on more recent data find that mobility *did* decrease for cohorts born between 1952 and 1981.

[34]Recent studies discuss mechanisms that might help to explain why such a plunge has not been observed. In particular, Davis and Mazumder (2019) note that studies based on the PSID may have missed a reduction in mobility that occurred already for cohorts born in the early 1950s, who entered the labor market when inequality was rising during the 1980s. Moreover, Nybom and Stuhler (2016b) argue that changes in the joint distribution of income and education in the *parent* generation may have counteracted the effect of rising income inequality on more recent cohorts.

Table 7: Trends in Income Mobility in the US (PSID)

	Direct Estimator		Lifecycle estimator			
	Annual All ages	Annual Age 25-30	Baseline FE –	Parental FE –	Parental FE Cohort interaction	Parental no FE Cohort interaction
	(1)	(2)	(3)	(4)	(5)	(6)
Cohorts 1950-59	0.380 (0.034)	0.309 (0.039)	0.417 (0.040)	0.430 (0.040)	0.437 (0.040)	0.434 (0.017)
Cohorts 1960-69	0.391 (0.034)	0.361 (0.037)	0.429 (0.036)	0.447 (0.035)	0.438 (0.036)	0.449 (0.013)
Cohorts 1970-79	0.406 (0.029)	0.349 (0.033)	0.450 (0.033)	0.468 (0.033)	0.470 (0.033)	0.475 (0.009)
Cohorts 1980-89	0.308 (0.025)	0.311 (0.027)	0.363 (0.025)	0.394 (0.025)	0.432 (0.026)	0.443 (0.008)
R^2	0.087	0.082	0.147	0.159	0.165	0.581
Obs	59,458	17,616	4,937	4,937	4,937	4,938
Individuals	4,939	4,565	4,937	4,937	4,937	4,938

Notes: Columns (1) and (2) are based on direct regressions in which we regress offspring's log annual income on parental lifetime income. For column (1) we pool all available income observations at age 22-58. In column (2) we only consider age 25-30. Columns (3) to (6) report different variants of the lifecycle estimator based on all available income observations. Column (3) includes individual FEs and a quartic in age interacted with dummies for three educational groups. Column (4) adds a quadratic interaction between son's age and fathers' income and between son's age and father's education. We next add interactions between age x cohort dummies and a standardized profile in column (5) (see main text). Finally, column (6) follows the specification of column (5), but intercepts are a function of parental income rather than individual-specific (no fixed effects).

to plunge dramatically." We can study these cohorts as our proposed lifecycle estimator performs comparatively well at young ages (see Section 5.1). Specifically, we use the PSID to estimate the IGE for four different cohort groups born in the 1950s, 1960s, 1970s or 1980s, constructing a sample including both sons and daughters as described in Section 2.

Table 7 reports the results, following the same structure as the corresponding Table 6 for Sweden. The first two columns present "naive" direct regressions in which we regress offspring's log annual income on parental income. If pooling all available income observations (column 1) we find lower IGE estimates for more recent cohorts, which are observed only at a young age (generating a downward bias). Holding instead the age window fixed (column 2) suggests that mobility decreased for the 1960s and 70s cohorts, but rebounded in recent cohorts. However, as already discussed, neither of these estimators is sufficiently reliable. Switching to a lifecycle estimator generally yields larger estimates (columns 3-6). In column (3), the baseline lifecycle estimator that does not account for differential income growth by parental background indicates an increase in the IGE between the 1950s and 1970s cohorts (in line with Justman and Stiassnie 2021) but a strong drop for those born in the 1980s (who were not yet examined in other studies, apart from forecasts derived from income at age 26 in Chetty et al. 2014). Allowing for income growth to vary with parental income or education (column 4) has little effect on the earlier cohorts, but increases the IGE estimate for cohorts born in the 1980s. This upward correction is consistent with the evidence provided in Section 3: allowing for differential lifecycle growth is particularly important if individuals are only observed at young ages. Nevertheless, this estimator still suggests that mobility has increased in the most recent cohorts.

However, the lifecycle pattern might not have remained stable across cohorts. Indeed, we observe that income profiles have become steeper for more recent birth cohorts, and particularly so for children from high-income parents. In column (5) we account for such shifts by interacting indicators of the four cohort groups with parental income and a concave function of age.[35] This modification further increases the estimated IGE for recent cohorts, from 0.394 to 0.432. Finally, column (6) tests whether these results are sensitive to the inclusion of individual fixed effects. As discussed in Section 5.1, the high flexibility built into an estimator by such large set of fixed effects can make it harder to capture systematic variation in income growth. However, the results remain similar to those in column (5).

In sum, all variants of the lifecycle estimator suggest that mobility decreased slightly between the 1950s and 1970s cohorts, but the pattern for the more recent cohorts depends critically on whether we account for changes in lifecycle income growth across cohorts. Naive estimators suggest that mobility increased markedly for cohorts born in the 1980s. However, estimators that allow for differential lifecycle growth suggest that mobility did not improve compared to earlier cohorts. The reason is that structural changes on the labor market have not only affected the distribution of income at a given age, but also the distribution of income *growth*: the offspring from richer parents experience faster income growth today than in the past. Of course, these findings are only a snapshot based on early labor market experiences, and it remains to be seen whether those pattern hold up when the 1980s cohorts reach later stages of their careers. However, based on current data and reasonable extrapolations from current patterns, we can reject any meaningful change in US income mobility over the past four decades.

[35]See previous section for further details on the construction of a concave "standardized age profile". We do not extrapolate linearly from early to later ages as differences in income growth tend to much more pronounced at early than at later ages (see Table 2).

7 Concluding Remarks

It is difficult to measure intergenerational mobility in income, and methodological improvements have led to major revisions in mobility estimates over the past two decades (Solon 1999; Mazumder 2016). But despite a better understanding of the source of attenuation and lifecycle biases, the literature still struggles to address those biases effectively. As noted by Mogstad and Torsvik (2021), "*there is considerable uncertainty associated with the IGE estimates, and especially with their comparison across time and place*". A commonly applied "rule of thumb" to measure income at some point around midlife only helps partially, as it cannot be followed for recent cohorts, and mobility estimates remain sensitive to the exact age at measurement.

Instead, we proposed that researchers should make more systematic use of the available income information over the lifecycle. We first illustrated three properties of income processes that complicate this objective: (i) income growth explained by observable characteristics, (ii) transitory noise, and (iii) unexplained income growth that nevertheless correlates within families. This last property is of more general interest, as the literature on income processes has long debated whether (residual) income grows at an individual-specific and deterministic rate or follows a random walk. Using long income series from Sweden and the US, we showed that income growth has indeed a systematic component: children from high-income parents tend to experience faster income growth, even conditional on their education.

The estimation of intergenerational mobility is therefore closely intertwined with the dynamics of income profiles itself. Building on earlier contributions such as Hertz (2007), we proposed a "lifecycle" estimator of income mobility that addresses these dynamics explicitly. In the first step, we estimate income profiles based on age and other observable characteristics. Differently from previous work we however allow income growth to also vary with parental background. Comparing this lifecycle estimator to benchmark estimates in both Swedish and US data, we illustrated that accounting for this heterogeneity greatly reduces lifecycle bias. We showed further that the proposed estimator works well in different data settings, and that it is less sensitive to the age at which incomes are observed than other methods. These properties are attractive for comparative purposes, such as mobility comparisons across place or time. Moreover, a lifecycle estimator can provide credible estimates for individuals observed only at young age, which opens up the possibility to study income mobility for more recent birth cohorts.

We concluded by applying our lifecycle estimator to study mobility trends in Sweden and in the US. This analysis serves two purposes. First, it illustrates how estimates of mobility trends can be distorted by lifecycle effects. Second, we can estimate mobility trends for younger, more recent birth cohorts, which are particularly interesting from a policy perspective. For Sweden, estimates based on a fixed age window suggest that mobility decreased substantially after the 1950s cohorts.

Accounting for lifecycle effects instead suggests that the IGE has remained fairly stable for cohorts born in the 1950s, 1960s and 1970s, while mobility has increased for recent cohorts.

As for Sweden, our US estimates show that accounting for lifecycle effects is particularly important for more recent cohorts. While a naive estimator based on fixed age windows implies a U-shaped pattern across cohorts, our lifecycle estimator yields larger and more stable estimates: the intergenerational elasticity of income is remarkably constant in the US. However, income growth has increasingly diverged in the most recent cohorts, with children from more advantaged backgrounds experiencing faster income growth today than in the past. Our aim here was to account for such patterns, but an interesting question for future work is which mechanisms contribute to family background effects over the lifecycle.

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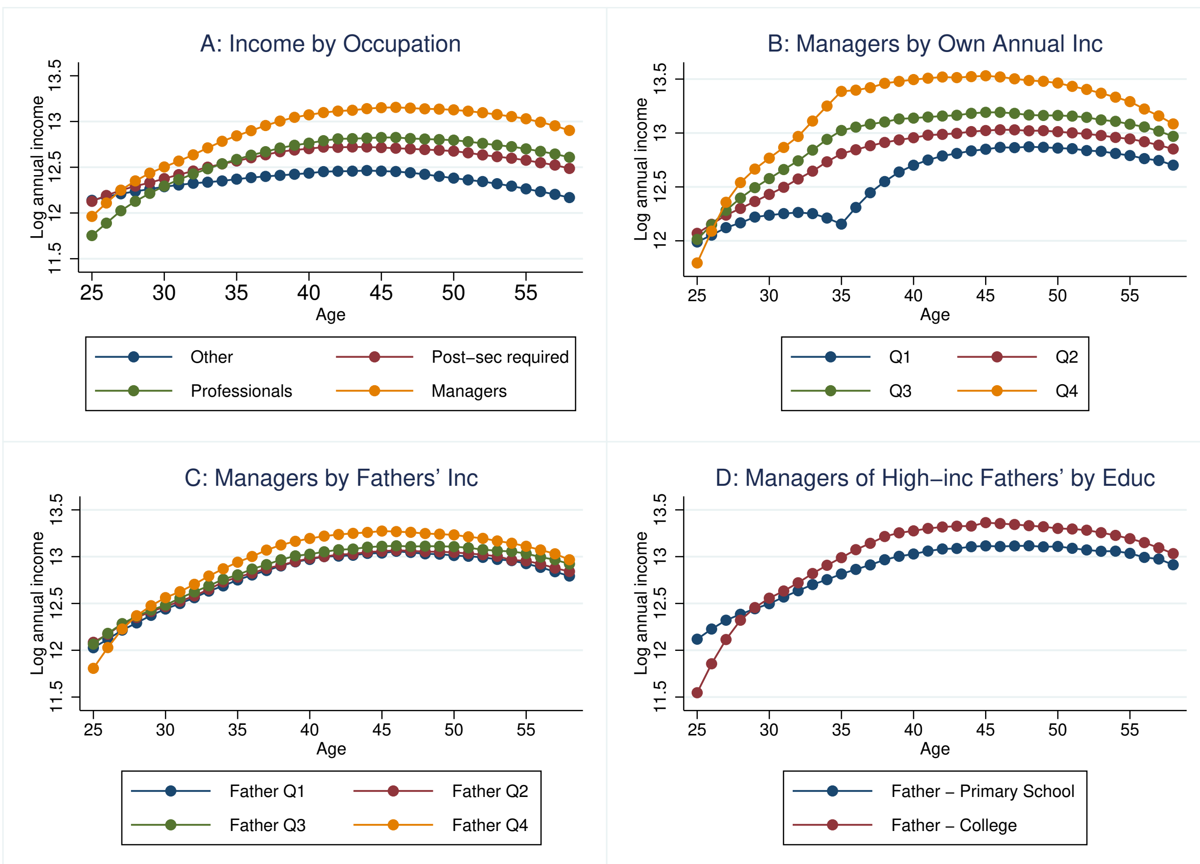
Online Appendix

A Lifecycle Estimator of
Intergenerational Income Mobility

Ursula Mello, Martin Nybom, Jan Stuhler

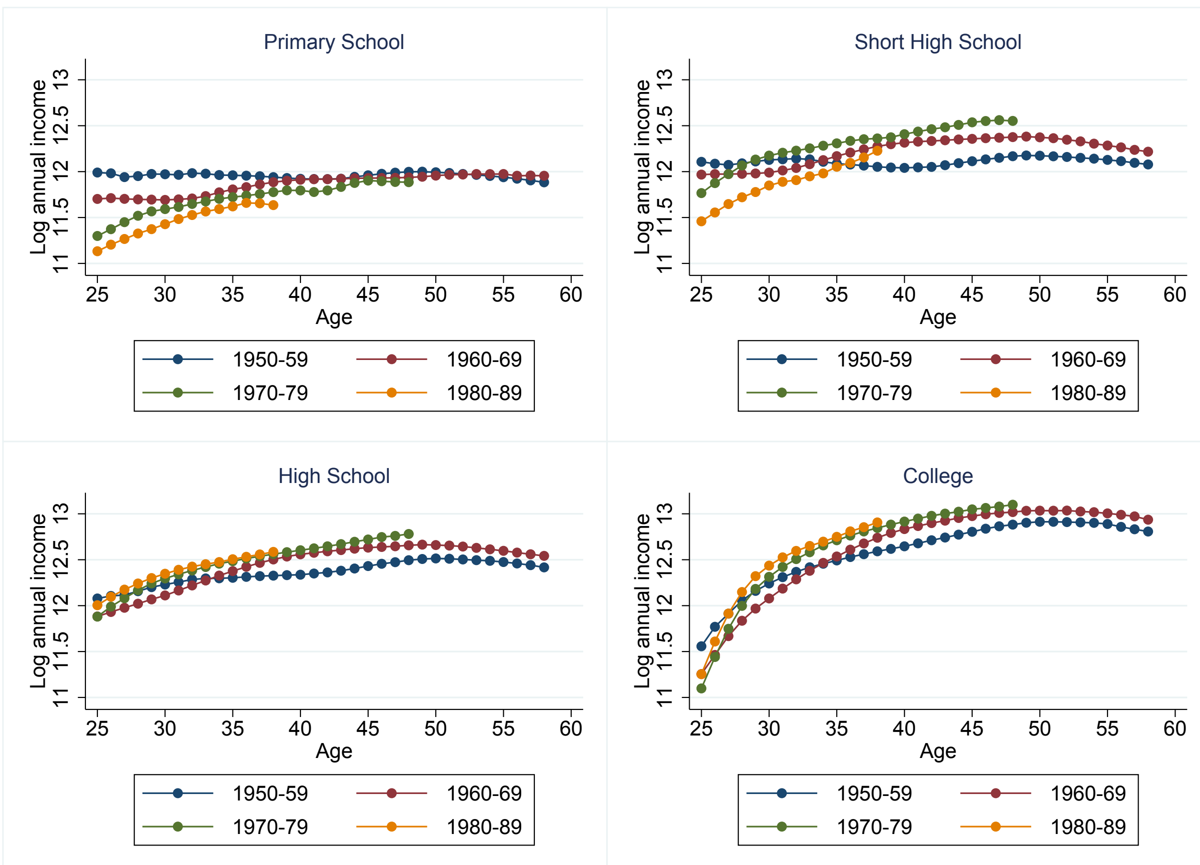
A Additional Figures and Tables

Figure A.1: Components of the Income Process



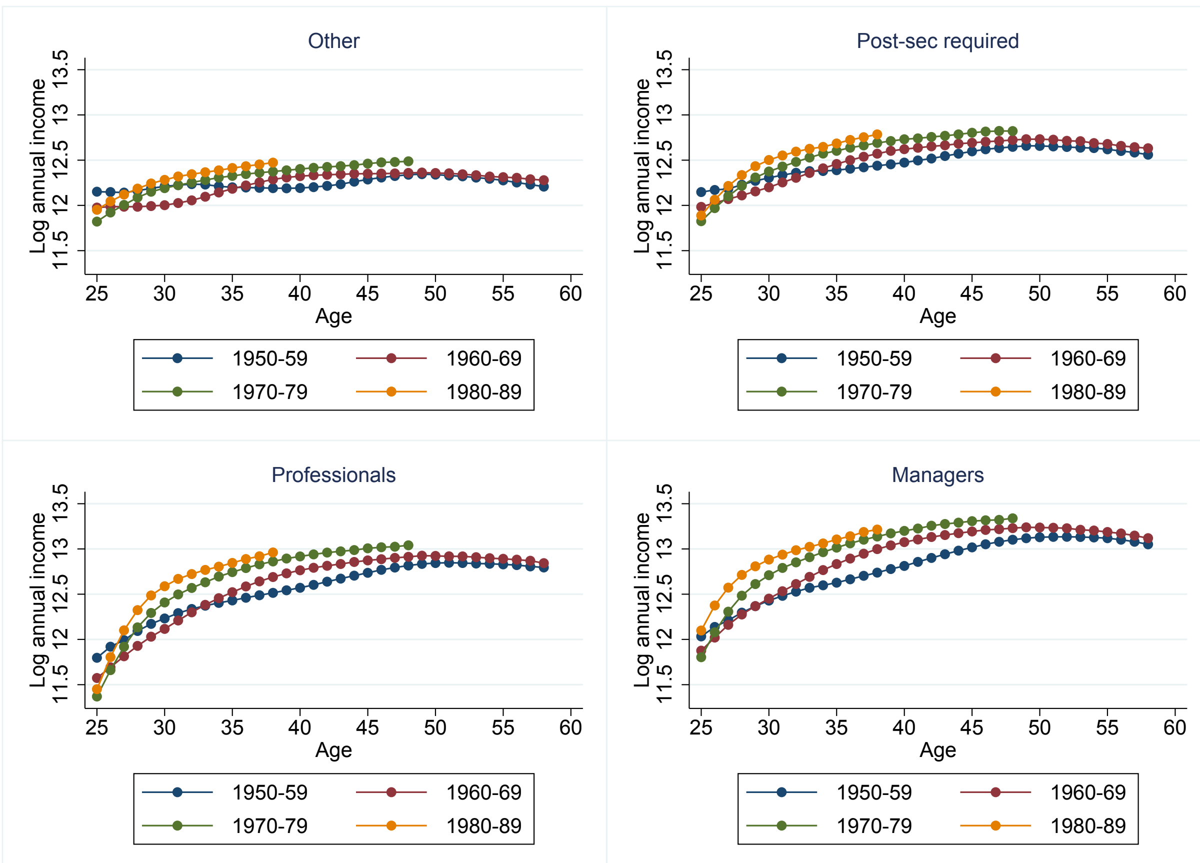
Notes: Panel A shows income trajectories by type of occupation. Panel B focuses only on managers, who are split in four groups, according to their annual income at age 35. Category Q1 refers to the bottom quartile and Q4 to the top. In Panel C, managers are divided in four groups, according to fathers' lifetime income. Finally, in Panel D, managers whose fathers belong to the top half of lifetime income are divided in two additional groups: college-educated fathers and fathers with only primary school. We remove time effects from annual income observations to abstract from business cycle effects. Source: Swedish Register Data.

Figure A.2: Income Profiles by Education Group and Cohort



Notes: The figure plots the observed log income profiles by education group and cohort using Swedish registers.

Figure A.3: Income Profiles by Occupation Group and Cohort



Notes: The figure plots the actual log income profiles by occupation group and cohort using the Swedish data.

Table A.1: Intergenerational Elasticity Literature

Authors	Journal	Estimate (US)	Method	Addressing lifecycle bias in offspring generation
IGE in Levels				
Solon (1992)	AER	0.41	Averaging	Single year annual earnings, average age 29.6
Zimmerman (1992)	AER	0.54	Averaging	Single year of son's annual earnings; average age 33.8
Mazumder (2005)	Restat	0.61	Averaging	Average 4 years of income, age 30-35
Hertz (2006)	Working Paper	0.58	Averaging	Average 4.1 income observations at mean-age 37
Bratsberg et al. (2007)	Economic Journal	0.54	Averaging	Annual income in 1995 and 2001, cohorts 1957-1964.
Gouskova et al. (2010)	Labour Economics	0.63	Averaging	Single year from ages 35-44
Chau (2012)	Economic Letters	0.66	Model Income	At least 3 observations of annual earnings between the ages of 25-60. Use earnings dynamics model.
Jäntti and Lindahl (2012)	Economic Letters	Sweden	Model Income	Formulate simple model with heterogeneous income
Chetty et al. (2014)	QJE	0.34	Averaging	2-year average around ages 29-32 (2011 and 2012)
Mitnik et al. (2015)	Working Paper	0.56	Averaging	Single year around ages 35-38
Mazumder (2016)	Research in Labor Economics	0.66	Averaging	Average between 1 and 11 years around age 40
Borisov and Pissarides (2016)	Working Paper	Russia	Model Income	Predicted value of permanent earnigns based on monthly earnings. Controls for hours worked, age, year of birth, education.
Landersø and Heckman (2017)	Scandinavian Journal	0.29 to 0.45	Averaging	Average between ages 34-41 for older cohorts down to 30-35 for younger cohorts
Deutscher and Mazumder (2020)	Labour Economics	Australia	Averaging	Average over five years around ages 29-37
Connolly et al. (2021)	NBER WP	Canada	Averaging	Average over ages 30 to 36
IGE in Trends				
Mayer and Lopoo (2005)	Journal of Human Resources	Non-linear	Averaging	Son's family income at age 30
Hertz (2007)	Industrial Relations	No trend	Model Income	Estimation of income profiles
Aaronson and Mazumder (2008)	Journal of Human Resources	Non-linear	–	
Lee and Solon (2009)	Restat	No trend	Averaging	Average of all available years, changing across cohorts
Justman and Krush (2013)	Working Paper	Upward	Model Income	Predicted income at age 40, controls for age, education, race, marital status and individual FE
Hartley et al. (2017)	Working Paper	Upward	Averaging	Multi-year average, Lee & Solon age adjustment (mother-daughters)
Davis and Mazumder (2019)	Working Paper	Upward	Averaging	Average of 3 year of son's family income
Lifecycle Bias (Methodological)				
Haider and Solon (2006)	AER	–	–	Generalized errors-in-variables (GEiV) model
Grawe (2006)	Labour Economics	–	–	Discussion of lifecycle bias
Böhlmark and Lindquist (2006)	Journal of Labor Economics	–	–	Discussion of lifecycle biase and GEiV model
Nilsen et al. (2012)	Scandinavian Journal	–	–	Discussion of lifecycle bias
Nybom and Stuhler (2016a)	Journal of Human Resources	–	–	Testing lifecycle bias and GeiV model
Chen et al. (2017)	Labour Economics	–	–	Discussion of lifecycle bias
Gregg et al. (2017)	Oxford Bulletin	–	–	Discussion of lifecycle bias and application for UK

Notes: This table presents recent papers that attempt to measure the intergenerational elasticity in levels or in trends with a brief description of the main approach used to adress the lifecycle bias in the measurement of offspring income. It also contains some methodological papers that discuss and test the lifecycle bias in intergenerational mobility estimates.

Table A.2: The Lifecycle Estimator with Fewer Income Observations (Swedish data)

		Lifecycle estimator (Parental, Quadratic)				
Son's Age	N	≤ 6 obs.	≤ 5 obs.	≤ 4 obs.	≤ 3 obs.	≤ 2 obs.
Age ≤ 30	94,194	0.274	0.28	0.279	0.286	0.296
		(0.003)	(0.003)	(0.004)	(0.004)	(0.004)
R^2		0.063	0.065	0.062	0.062	0.061
Age ≤ 35	94,264	0.237	0.239	0.236	0.237	0.231
		(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
R^2		0.026	0.026	0.026	0.025	0.023
		Lifecycle estimator (Slope-level, Quadratic)				
Son's Age	N	≤ 6 obs.	≤ 5 obs.	≤ 4 obs.	≤ 3 obs.	≤ 2 obs.
Age ≤ 30	94,194	0.266	0.266	0.268	0.265	0.257
		(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
R^2		0.062	0.06	0.06	0.054	0.047
Age ≤ 35	94,264	0.246	0.246	0.247	0.244	0.233
		(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
R^2		0.033	0.032	0.032	0.03	0.025

Notes: The table reports the slope coefficient from a regression of son's income on father's lifetime income. The measure for son's income is the predicted lifetime income from a lifecycle estimator applied to the indicated age range. The top row indicates the maximum number of income observations used for each person in the child generation. The observations are selected randomly from all the observations available for each person below the indicated age threshold. Robust standard errors in parentheses.

Table A.3: The Lifecycle Estimator in Smaller Samples (Swedish data)

Sample Size	k=1/4	k=1/16	k=1/64	k=1/256	k=1/1024
	Son's Age 25-30				
Benchmark	0.261	0.257	0.264	0.257	0.269
std. dev.	(0.003)	(0.011)	(0.027)	(0.054)	(0.103)
Lifecycle (Parental Quadratic)	0.261	0.260	0.265	0.256	0.265
std. dev.	(0.005)	(0.019)	(0.060)	(0.121)	(0.316)
Lifecycle (Slope-level Quadratic)	0.250	0.251	0.243	0.256	0.266
std. dev.	(0.003)	(0.020)	(0.056)	(0.113)	(0.220)
N	1,713,014	428,151	106,979	26,732	6,678
	Son's Age 25-35				
Benchmark	0.255	0.258	0.257	0.256	0.264
std. dev.	(0.004)	(0.011)	(0.021)	(0.051)	(0.107)
Lifecycle (Parental Quadratic)	0.267	0.264	0.263	0.252	0.253
std. dev.	(0.008)	(0.025)	(0.056)	(0.098)	(0.261)
Lifecycle (Slope-level Quadratic)	0.257	0.260	0.261	0.254	0.269
std. dev.	(0.007)	(0.015)	(0.049)	(0.085)	(0.199)
N	1,712,725	428,751	106,886	26,724	6,673

Notes: The table reports the slope coefficient from a regression of son's income on father's lifetime income, comparing the Parental and the Slope-level lifecycle estimators with the benchmark. Each column reports coefficients estimated from multiple draws with replacement of differently sized sub-samples, as indicated in the top row. For each sample size, we report the mean and standard deviation (in parentheses) of the point estimates, computed across the random draws from the main sample. Thus, for $k = \{1/4, 1/16, 1/64, 1/256, , 1/1024\}$ we draw $1/k$ samples of size $N_k = N * k$ from the whole sample of size N. Sons incomes observed from age 25 to 30 in Panel A from age 25 to 35 in Panel B.

Table A.4: Robustness to Cohort and Year Effects (Swedish data)

	(1)	(2)	(3)	(4)
Benchmark	0.215	0.215	0.215	0.216
	(0.002)	(0.002)	(0.002)	(0.002)
Annual, all observed ages	0.237	0.237	0.279	0.272
	(0.002)	(0.002)	(0.003)	(0.003)
Lifecycle (Parental Quadratic)	0.208	0.203	0.235	0.243
	(0.003)	(0.003)	(0.003)	(0.003)
Lifecycle (slope-level Quadratic)	0.201	0.199	0.203	0.245
	(0.002)	(0.002)	(0.003)	(0.003)
First-step sample				
Cohorts:	1952-1960	1950-1989	1950-1989	1950-1989
Income years:	1977-2018	1977-2018	1998-2018	2014-2018
Individuals (second step)	293,333	293,333	284,806	283,767

Notes: The table reports the slope coefficient from a regression of son's income on father's lifetime income, comparing the parental and the slope-level lifecycle estimators with the benchmark and an estimate based on all observed annual earnings. Each column reports estimates for cohorts born 1952-1960 using data from different time periods and (for the first-step estimation) different cohorts. We use the Swedish trends sample (see Section 2), which results in slightly lower benchmark estimates than for the baseline sample. Column (1) uses the benchmark cohorts (born 1952-1960) and all earnings years (when aged 25-58) in steps 1 and 2. Column (2) uses all cohorts (1950-1989) and all earnings years (when aged 25-58) in step 1. Column (3) uses all cohorts (when aged 25-58) during the years 1998-2018 in step 1. Column (4) uses all cohorts (when aged 25-58) during the years 2014-2018 in step 1. Robust standard errors are in parentheses and the final row shows the number of unique individuals used in each column (among the 1952-1960 cohorts).

B Modelling Errors-in-Variables

In the classical errors-in-variables model, inconsistencies in the IGE are limited to attenuation bias caused by the imprecise measurement of the lifetime income of parents (e.g., Atkinson 1980).[36] However, the association between current and lifetime income varies systematically over the life cycle, contrary to a classical errors-in-variables model in which the errors are independent of true values. As a consequence, the use of short income snapshots for the child generation introduces a *lifecycle bias* in mobility estimates (Jenkins, 1987). Grawe (2006) and Haider and Solon (2006) demonstrate that this bias tends to be large, such that mobility estimates are quite sensitive to the age at which child income is being measured.[37]

Recent applications adopt therefore a *generalized errors-in-variables* (GEiV) model proposed by Haider and Solon (2006), which accounts for the systematic relation between annual and lifetime income over the lifecycle.[38] Focusing on left-hand side measurement error, it corresponds to the linear projection

$$y_{sit} = \lambda_{st} y^*_{si} + u_{sit}, \tag{7}$$

where y_{sit} is the annual log income of the child of family i at age t, y^*_{si} is his or her log lifetime income, and y^*_{si} and u_{sit} are uncorrelated by construction. Under the assumption that $Cov(y^*_{fi}, u_{sit}) = 0$, with y^*_{fi} denoting parental log lifetime income, the probability limit of a regression of y_{sit} on y^*_{fi} is

$$plim\ \beta_t = \frac{Cov(y_{sit}, y^*_{fi})}{Var(y^*_{fi})} = \beta\lambda_{st}, \tag{8}$$

where β is the true IGE from regressing y^*_{si} on y^*_{fi}. The use of short income spans would therefore not introduce bias if child income were measured at an age at which λ_{st} is close to one, which tends to be around midlife.[39] The key implication is that researchers can reduce lifecycle bias by measuring income at mid-age.

As shown in Table B.1, this generalization of the classical error-in-variables model captures the relation between annual and lifetime incomes remarkably well. The insight that λ_{st} increases over age and approximates one around mid-age holds in simulated income data calibrated to the US labor market (based on Guvenen 2009, details available upon request), as well as in actual income series

[36] While this bias can be reduced by averaging over longer income snapshots, Mazumder (2005) demonstrates that even 10-year averages are not sufficient because the transitory component of income is highly serially correlated.

[37] This observation also motivates the recent interest in mobility in income *ranks*, as rank correlations suffer less from attenuation and lifecycle bias (Chetty et al. 2014; Nybom and Stuhler 2017).

[38] The GEiV model has been extended in subsequent work. Lee and Solon (2009) adapt it for the study of mobility trends. An et al. (2017) implement it within a non-parametric framework that allows for the IGE to be heterogeneous.

[39] Böhlmark and Lindquist 2006 confirm this prediction in Swedish data. As noted by Haider and Solon (2006), for individuals with different income growth there will nevertheless exist an age t^* around midlife at which the expected difference between individuals' log annual incomes equals the expected difference between their lifetime incomes.

from Sweden and the US. However, the approach is subject to some limitations. First, lifecycle bias may not be fully eliminated at the age at which $\lambda_{st} = 1$ because the assumption $Cov(y^*_{fi}, u_{sit}) = 0$ tends to be violated if income growth varies with parental background even conditional on a child's own lifetime income (as indicated by Figure 1 and shown formally in Nybom and Stuhler 2016a).

Second, the optimal age t^* at which $\lambda_{st} = 1$ is rarely known, as its estimation requires data on lifetime incomes. In practice, applications follow instead a simple rule-of-thumb to measure income at *some* point in midlife. Yet Haider and Solon (2006) warn that t^* is likely to vary across countries, and Table B.1 shows that even slight deviations from this optimal age yield substantially different estimates. The rule-of-thumb estimates prevalent in the literature may therefore contain large biases, in particular given the extent to which the age at measurement varies across studies (see Table A.1).

Table B.1: Lifecycle Bias and the Generalized-Errors-in-Variables Model

Swedish Register Data			US Simulated Data		
Son's Age	λ_{st}	β_t	Son's Age	λ_{st}	β_t
33	0.858	0.221	41	0.896	0.461
34	0.913	0.237	42	0.958	0.470
35	0.969	0.253	43	0.997	0.506
36	1.024	0.270	44	1.036	0.518
37	1.080	0.285	45	1.047	0.525
True		0.253			0.497

Notes: Estimates of λ_{st} are based on equation (7). Estimates of β_t are based on a regression of parental lifetime income on offspring annual income at age t. Source: Swedish register data and simulated income data for the US, based on Guvenen (2009).

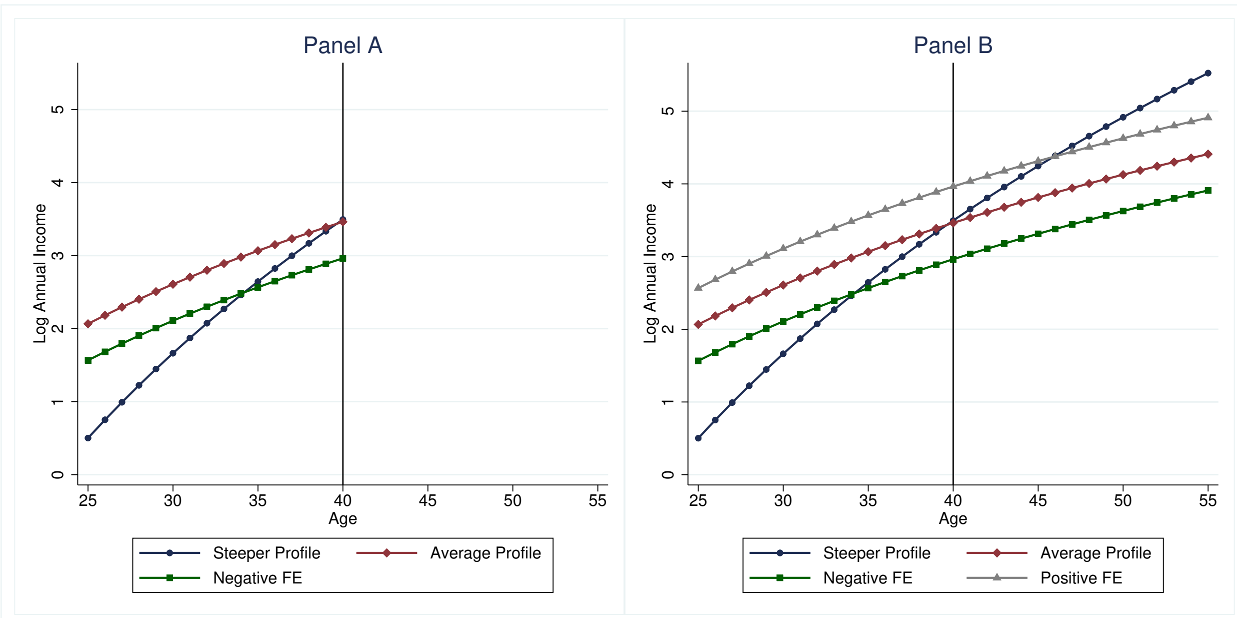
A third problem is that income around midlife is often not observed in the sample of interest. By definition, it will not be available if our interest centers on recent cohorts, who are still in their 20s or early 30s. Lee and Solon (2009) provide an extension of the GEiV model for the estimation of mobility trends, which allows for the inclusion of observations at younger age by accounting for the age difference to a reference age. Lifecycle bias would not affect the estimated mobility trend if that bias remained sufficiently stable (i.e., if λ_{st} and $Cov(y^*_{fi}, u_{sit})$ remain constant) over cohorts. However, the structure of income profiles does change over time (e.g., Guvenen 2009), and the age profile of λ varies over the cohorts in our benchmark sample: at age 35, estimates of λ vary between 1 and 1.2 between cohorts born in the early vs. late 1950s, scaling estimates of the IGE accordingly.[40] As a result, the IGE appears to increase twice as much when using incomes at age

[40]This observation may reflect that income distributions, and therefore the value of λ, can change substantially with macroeconomic conditions – such as the recession that Sweden experienced in the early 1990s.

35 rather than lifetime incomes (see Figure C.2). These observations suggest that estimates based on a fixed age window or fixed reference age, while useful for identify sudden or large shifts in mobility, might not provide a good approximation for more gradual mobility trends over time.

C Modelling the Income Process: Fixed Effects

Figure C.1: Illustration of Potential Problems with Fixed Effect Estimators

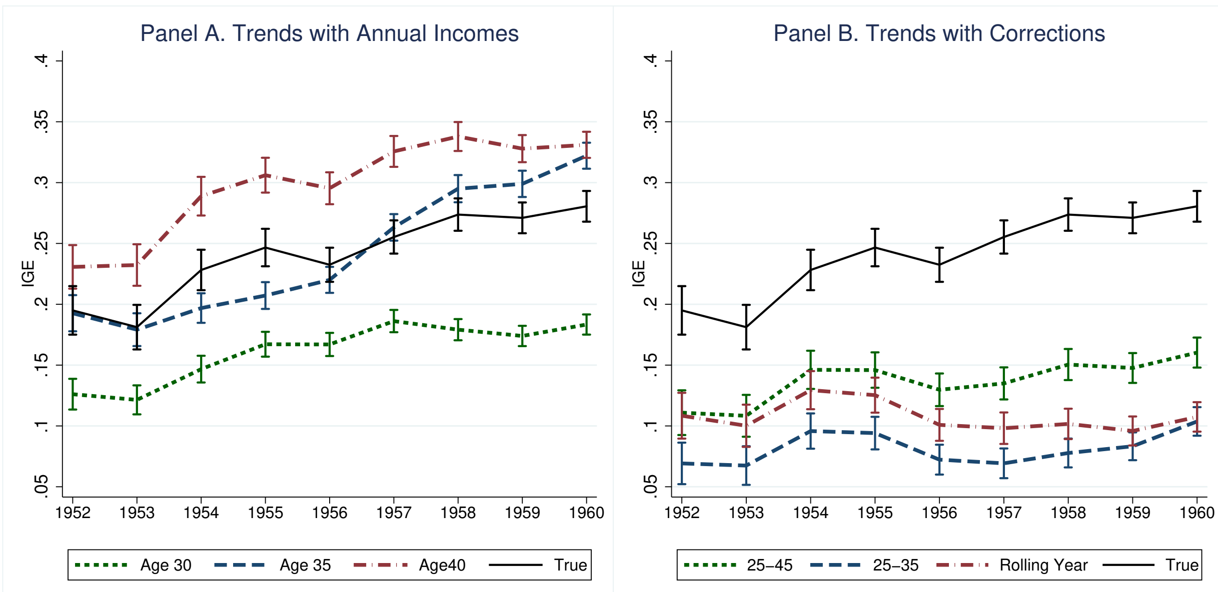


Notes: In the Figure, the red line represents the average income profile in the population while the blue line represents the income trajectory of an individual with steeper than average profile. The gray and green lines correspond to the red line shifted by a positive or negative fixed effect, respectively.

Figure C.1 provides intuition for why mobility estimates based on equation (2) remain sensitive to the age at which incomes are measured. Suppose the blue line (round dots) is the true income trajectory of individual i with a steeper than average profile, while the red line (diamonds) is the average income profile in the population. Now, suppose we only observe incomes at age 25-40, as in Panel A of the figure. In this case, the predicted income profile for individual i is given by the green profile (squares), corresponding to the red line plus a negative individual fixed effect. We would therefore understate the lifetime income of those with steeper profiles. Because income growth increases systematically with parental income even after conditioning on own education or occupation (see Section 3), the intergenerational elasticity is understated as well. The shorter and earlier the age range, the more we are understating the elasticity, as illustrated in Table 3.

The problem will be compounded when using equation (2) to predict lifetime incomes for both the child and the parent generation. Panel B of Figure C.1 illustrates why the the approach understates the income of children (observed early in life) with steeper than average profiles, and overstate the lifetime income of parents with steeper than average profiles (observed late in life). Suppose that the income profile of both parent and child is given by the red line, but that we observe the child earlier in life (e.g., ages 25-40) and the parent later in life (ages 40+). As individual heterogeneity can only be captured by the fixed effects, the father will have a positive fixed effect and the child a negative fixed effect. As a consequence, we would be understating the lifetime income of sons who have steeper than average profiles (green line), overstating the lifetime income of their fathers (gray line), and therefore, understating the intergenerational elasticity.

Figure C.2: Estimation of Trends in the IGE



Notes: In this Figure, we plot trends in the IGE using the Swedish register data. In Panel A, we plot true trends using son's lifetime income (in black) and trends using annual incomes at ages 30, 35 or 40. In Panel B, we plot estimates of the IGE based on the first-step equation (2) using either ages 25-45 or 25-35, to then predict income at age 25 for estimation of the IGE. For the line red in Panel B ("Rolling Year") we instead use a rolling age window that reduces as the cohorts become younger (similar to Hertz 2007). For the 1952 cohort the age range for estimation is 25-43, for the 1953 cohort the range is 25-42, and so on.

The same problem also affects the estimation of mobility trends, as is illustrated in Figure C.2 based on our Swedish benchmark sample. The "true" cohort trend (based on lifetime incomes, black line) increases for cohorts born in the 1950s. Panel A compares this benchmark to estimates based on income at a fixed age. While these estimators agree on the direction of the trend, the magnitudes differ. Panel B compares the benchmark to the two-step estimator as described in Section 4.2. The trend is relatively well captured if the estimator is based on fixed age windows (blue and green lines). However, using a rolling age window – considering age 25-43 for the 1952 cohort but reducing the age range for more recent cohorts – we fail to capture the increase in the

IGE (red line). Our findings therefore suggest that trend estimates based on rolling age windows are susceptible to lifecycle effects.

D Modelling the Income Process: Growth vs. Levels

Creedy (1988) proposes a correction method based on the insight that the dispersion of earnings tends to increase over age, even conditional on education or occupation. To account for this pattern, he assumes that income growth varies with the income *rank* of the individual in the income distribution. An important advantage of this method is that it can be implemented in cross-sectional data sources. In a first step, we estimate how the mean and the variance of log income vary over age within each occupational or educational group. Following Creedy (1988), we estimate

$$y_{ij} = \beta_0 + \beta_1 age_{ij} + \beta_2 age_{ij}^2 + u_{ij}, \tag{9}$$

separately by each occupational or educational group j, where y_{ij} is the log income of individual i and group j. Then, we predict μ_{tj}, which is the average income by each occupational group j and age group t. The variance of log income σ_{tj}^2 is also computed within each group. Next, we estimate:

$$\sigma_{tj}^2 = \beta_0 + \beta_1 age_{tj} + \epsilon_{tj}, \tag{10}$$

and obtain predicted values for σ_{tj}^2. Alternatively, one can obtain these measures from external sources.

In a second step, these predicted values are used to rescale individual incomes to a common base year. First, compute the *standardized* value of an individual's log-earnings,

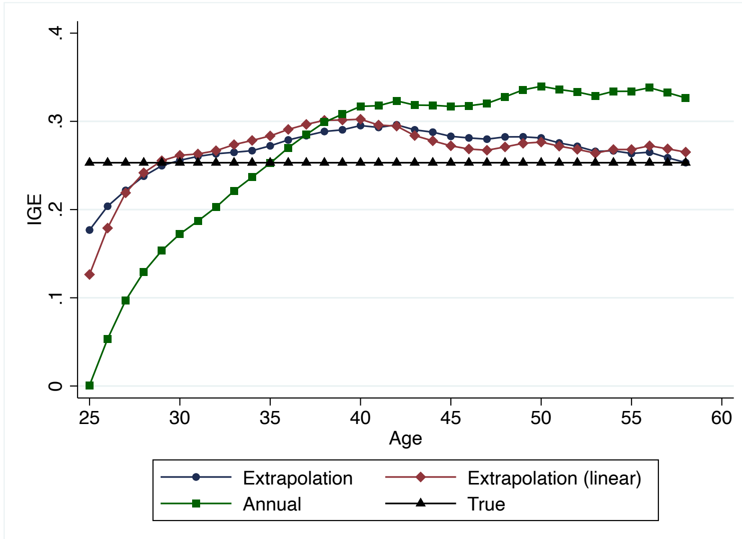
$$z_t = y_t - \mu_{tj}/\sigma_{tj}. \tag{11}$$

Then, rescale these standardized incomes according to the occupation or education-specific age-earning profile to compute adjusted log earnings at a common age $t*$:

$$y_{t^*} = \mu_{t^*j} + z\sigma_{t^*j}. \tag{12}$$

Those adjusted earnings depend on a single observable income at age t and on the values of μ_t and σ_t that were predicted within the educational and/or occupational group. Finally, we have adjusted income observations for different ages, computed based on a single cross-section observation and scaling factors. Creedy (1988) proposes to either use adjusted earnings directly or to compute an aggregated discounted lifetime earnings measure for the estimation of the IGE.

Figure D.1: Extrapolating from Observable Profiles



Notes: We compare IGE estimates based on the "true" lifetime income of sons (black line), estimates based on annual incomes (green line), and two versions of Creedy's proposed estimator. In the first, we approximate the profiles of μ_{tj} and σ^2_{tj} with a linear function in age (red line). In the second, we use their non-parametric age profile as observed in the sample (blue line).

We implement this method in the Swedish data. We combine the first-step estimates of μ_{tj} and σ^2_{tj} with an individual's earning at a certain age, to obtain his predicted income from ages 25-53 (which are then used to construct lifetime incomes). We therefore obtain a different measure of lifetime income, and a different estimate for the IGE, depending on the age at which we measure sons' income. We plot the resulting estimates of the IGE in Panel A of Figure D.1. We compare estimates based on the "true" lifetime income of sons (black line), estimates based on annual incomes (green line), and two versions of Creedy's proposed estimator. In the first, we approximate the profiles of μ_{tj} and σ^2_{tj} with a linear function in age (red line). In the second, we use their non-parametric age profile as observed in the sample (blue line).

The comparison demonstrates that estimates of the IGE can be significantly improved by taking the dispersion of income growth over age into account. The corrected estimates are within 20 percent of the benchmark over the age range 30 to 50, even if the age profiles of μ_{tj} and σ^2_{tj} are approximated linearly. The correction works less well than a correction based on the generalized errors-in-variables model proposed by Haider and Solon (2006), but it is also based on less stringent requirements – only the age pattern of the variances and means is required. As Creedy (1988) discusses, the statistics that are necessary for the correction can potentially be estimated from a single cross-section. However, Figure D.1 also shows that the correction method works only imperfectly, and tends to overstate the IGE over most of the age range.

A key limitation is that equation (12) rescales incomes based on the assumption that individual's rank in the widening income distribution remains stable over age: that is, individuals with high annual rank are assumed to have higher income growth in the future. This is not the case in practice, as is illustrated in Panel B of Figure 1. Because of short-term variability, annual incomes are instead mean-reverting – individuals with high income rank at age t tend to have lower income growth in the next few years. By not accounting for this mean-reverting influence, the imputation in equation (12) tends to overstate the variance of lifetime incomes and therefore the IGE. The method performs better the more important the heterogeneous growth rates are compared to the transitory shock component. For example, in the HIP process proposed by Guvenen (2009) , incomes at early ages are dominated by transitory shocks from an AR(1) process (and intercepts), while incomes at later ages are dominated by idiosyncratic growth rates, and the extrapolation from observed ranks would work better at later ages.