Heterogeneous Income Profiles and Life-Cycle Bias in Intergenerational Mobility Estimation

Martin Nybom[*], Jan Stuhler[†][‡]

March 10, 2013

Abstract

Using snapshots of income over shorter periods in intergenerational mobility estimation causes a so-called life-cycle bias if the snapshots cannot mimic lifetime outcomes. We use uniquely long series of Swedish income data and find that current empirical strategies do not eliminate such bias. Application of a widely adopted generalization of the classical errors-in-variables model improves OLS estimates of the intergenerational elasticity, but substantial bias remains. IV estimates show even stronger life-cycle effects and cannot provide reliable parameter bounds. Remaining inconsistencies stem from within-family correlation of income profile heterogeneity, with implications for other literatures that depend on measurement of long-run income.

[*]Stockholm University, Swedish Institute for Social Research (email: martin.nybom@sofi.su.se)

[†]University College London, Department of Economics and Centre for Research and Analysis of Migration (email: j.stuhler@ucl.ac.uk)

[‡]Financial support from the Swedish Council of Working Life (FAS) and the German National Academic Foundation is gratefully acknowledged. We thank Anders Björklund and Markus Jäntti for advice and encouragement. We are further grateful for comments from the editor, the referees, Christian Dustmann, Michael Amior, Anders Böhlmark, Thomas Cornelissen, Flavio Cunha, Nathan Grawe, Steven Haider, Stephen Jenkins, Kristian Koerselmann, Mikael Lindahl, Matthew Lindquist, Steve Machin, Magne Mogstad, Marieke Schnabel, Gary Solon, Uta Schönberg, Yoram Weiss, and seminar participants at the 2010 ENTER conference in Toulouse, the 2011 ENTER conference in Tilburg, the 2011 ESPE conference in Hangzhou, the 2011 EALE conference in Cyprus, and the Swedish Institute for Social Research, Stockholm University.

Introduction

Transmission of economic status within families is often measured by the intergenerational elasticity between parents' and children's lifetime income. A large and growing literature has estimated this parameter in order to analyze the extent of intergenerational mobility across countries, groups and time.[1] Unfortunately, the estimates in the early literature suffered greatly from measurement error in lifetime income, and successive methodological improvements led to large-scale corrections.[2]

While the early estimates were severely attenuated from approximation of lifetime values by noisy single-year income data for parents, Jenkins (1987) identifies systematic deviations of current from lifetime values over the life cycle as an additional source of inconsistency. Haider and Solon (2006) and Grawe (2006) show that the latter is empirically of great importance. Various refined methods to address such *life-cycle bias* have recently been presented. In particular, Haider and Solon proposed a tractable generalization of the classical errors-in-variables model that, while applicable also in other contexts, has come to be widely adopted in the intergenerational mobility literature.

In this paper we make use of Swedish income data to evaluate these refined methods and to quantify the importance of life-cycle effects empirically. Our data contain nearly complete lifetime income histories of both fathers and sons, allowing us to derive a benchmark estimate and thus to directly expose the bias in both ordinary least squares (OLS) and instrumental variable (IV) estimates that results from approximation of lifetime by annual incomes. We confirm that Haider and Solon's generalization provides a useful improvement over the classical errors-in-variables model, but also show theoretically and empirically that it cannot eliminate life-cycle bias in intergenerational elasticity estimates. IV estimates show even stronger life-cycle effects and do not provide reliable parameter bounds. Finally, we examine if minor modifications of current standard procedures can reduce life-cycle bias further, and discuss our results in the more general context of income dynamics over the life cycle.

The main part of our analysis centers on Haider and Solon's generalization of the classical errors-in-variables model. This generalization adds an age-dependent slope coefficient to true lifetime incomes but maintains the assumption that the remaining error is uncorrelated with true values. The intergenerational elasticity can under these assumptions be consistently estimated if lifetime incomes are approximated by current incomes at a certain age. We find that this procedure improves estimates, but also that the life-cycle bias is substantially larger than the generalized model predicts. The model reduces this bias only partially since it disregards some of the heterogeneity in income profiles. We find that the remaining bias from left-side measurement error alone amounts to about 20 percent of the true elasticity (0.21 vs. 0.27) under favorable conditions.[3] These results extend to a variant of the model proposed in Lee and Solon (2009),

[1]See Solon (1999) for a comprehensive evaluation of the early empirical literature. Recent surveys include Björklund and Jäntti (2009) and Black and Devereux (2011).

[2]For example, the intergenerational elasticity of earnings for fathers and sons in the U.S. was estimated to be less than 0.2 among early studies (surveyed in Becker and Tomes, 1986), ranged between about 0.3 and 0.5 in the studies surveyed in Solon (1999), and is estimated to be around 0.6 or above in more recent studies like Mazumder (2005) and Gouskova et al. (2010).

[3]Assuming that central parameters of the generalized errors-in-variables model are perfectly observed, so that current income is measured at the exact proposed age. Right-side measurement error aggravates the life-cycle

which is often used to analyze mobility trends. Our results therefore suggest that the practice of measuring annual income at a certain age as surrogate for unobserved lifetime income, which is widespread not only in the intergenerational mobility literature, is still subject to life-cycle bias.

We also analyze two other methods that are used to address incomplete income data. First, we illustrate why the consideration of differential income growth across subgroups will not yield consistent estimates. Second, we show that IV estimates based on a typical instrument suffer from even greater life-cycle effects than OLS estimates. Contrary to previous findings, IV estimators should thus not be expected to provide an upper bound of the true parameter, even after application of the generalized errors-in-variables model.

Our results are hence rather pessimistic. They imply that current methods to compensate for incomplete income data are less successful and mobility estimates less accurate than commonly assumed. Well-established findings from the literature, such that income mobility is much lower in the U.S. than in the Nordic countries, are not put into doubt. But attempts to detect more gradual differences in mobility between populations, as in recent studies on mobility trends, may be compromised by remaining life-cycle biases.

However, observing a benchmark elasticity also allows us to describe the direction and magnitude of the bias at different stages in the life cycle, and thus to provide some recommendations for practitioners. We confirm that measuring incomes around midlife, as proposed by Haider and Solon (2006), is a good rule of thumb. Attempts to find an exact "right age" at which incomes ought to be measured are less promising. We find instead that averaging over multiple income observations also on the left-hand side (i.e. for the offspring) reduces life-cycle bias further, and that the treatment of missing and zero income observations has important consequences.

Life-cycle bias stems from the interaction of two factors: heterogeneity in income profiles cannot be fully accounted for, and unobserved idiosyncratic deviations from average profiles correlate with individual and family characteristics. For example, the offspring from poorer families may have higher initial incomes but flatter slopes if credit constraints affect human capital accumulation and job-search behavior in their early career. Such mechanisms are also of importance for other literatures that depend on measurement of long-run income and income dynamics. Examples include studies on the returns to schooling and the extensive literature that relates measures of stochastic income shocks to consumption or other outcomes.

The next section describes the general methodology and identifying assumptions employed in the early literature. We then examine the generalized errors-in-variables model theoretically in section 2 and empirically in section 3, and IV estimation and the consideration of differential income growth across subgroups in section 4. Section 5 concludes.

1 The Intergenerational Mobility Literature

The most common regression model in intergenerational mobility research is

$$y^*_{s,i} = \beta y^*_{f,i} + \epsilon_i, \tag{1}$$

bias further if fathers' and sons' incomes are measured at similar ages.

where $y^*_{s,i}$ denotes log lifetime income of the son in family i, $y^*_{f,i}$ log lifetime income of his father, ϵ_i is an error term that is orthogonal to $y^*_{f,i}$, and variables are expressed as deviations from their generational means.[4] The coefficient β is interpreted as the intergenerational income elasticity.

Equations akin to (1) appear in two distinctive forms in the literature. First, as a *statistical relationship* to measure the *outcome* of interest, i.e. the degree of intergenerational mobility. Second, as a *structural relationship* to study *causal mechanisms* of intergenerational transmission, derived from an economic model as in Becker and Tomes (1979). The statistical relationship is typically based on broad ex-post measures of long-run economic status such as lifetime income. The structural relationship instead relates to the ex-ante concept "permanent income", since expectations on long-run status determine individual behavior.[5] For simplicity, our analysis relates to the statistical relationship, but incomplete measurement of long-run status impedes identification of both types.

Approximation of Lifetime Income

As commonly available data sets do not contain complete income histories for two generations, a major challenge is how to approximate lifetime income.[6] Let y_i be some observed proxy for unobserved log lifetime income of an individual in family i, e.g. a single-year observation, an average of multiple annual income observations, or a more complex estimate based on such annual incomes. Observed values are related to true values by

$$y_{s,i} = y^*_{s,i} + u_{s,i},$$

where $y^*_{s,i}$ is the unobserved true log lifetime income of the son in family i and $u_{s,i}$ is measurement error. Similarly, for the father we observe

$$y_{f,i} = y^*_{f,i} + u_{f,i}.$$

The probability limit of the OLS estimator from a linear regression of y_s on y_f can be decomposed into

$$plim\ \hat{\beta}_{approx} = \frac{Cov(y_f, y_s)}{Var(y_f)} = \frac{\beta\, Var(y^*_f) + Cov(y^*_f, u_s) + Cov(y^*_s, u_f) + Cov(u_s, u_f)}{Var(y^*_f) + Var(u_f) + 2\, Cov(y^*_f, u_f)}, \qquad (2)$$

where we used eq. (1) to substitute for $y^*_{s,i}$ and applied the covariance restriction $Cov(y^*_{f,i}, \epsilon_i) =$

[4] We use the terms earnings and income interchangeably (since the issues that arise are similar), and examine fathers and sons since this has been the baseline case in the literature. A growing literature exists on intergenerational mobility in other family dimensions (e.g mothers, daughters or siblings) and in other income concepts (such as household income), for which our conceptual arguments are likewise relevant.

[5] For various reasons these concepts are not always clearly distinguished. First, simple economic models assign one time period to each generation, so that the concept of permanent and lifetime income coincide. Second, permanent income is difficult to measure. Empirical analysis of the structural relationship is still based on ex-post measures of (current) income, and is then often similar to the statistical relationship. Third, some of the empirical work in the literature has lately adopted the term "permanent income" even while focusing on the measurement of outcomes.

[6] Note that the availability of better data would not generally solve the identification problem, since data sets cannot contain complete income histories for contemporary populations.

0. It follows that the estimator can be down- or upward biased and that the covariances between measurement errors and lifetime incomes impact on consistency. The empirical strategies employed in the literature in the last decades can be broadly categorized in terms of changes in identifying assumptions about these covariances.

First Two Waves of Studies

The *first wave* of studies, surveyed in Becker and Tomes (1986), neglected the problem of measurement error in lifetime status. Often just single-year income measures were used as proxies for lifetime income, thereby implicitly assuming that

$$Cov(y_f^*, u_s) = Cov(y_s^*, u_f) = Cov(u_s, u_f) = Cov(y_f^*, u_f) = 0,$$

and

$$Var(u_f) = 0.$$

Classical measurement error in lifetime income violates the latter assumption, so that estimates suffered from large attenuation bias. Estimates of the intergenerational elasticity were therefore too low. This problem was recognized in Atkinson (1980) and then frequently addressed in the *second wave* of studies (surveyed in Solon 1999). But the assumption remained that measurement errors are random noise, independent of each other and of true lifetime income. That life-cycle variation had to be accounted for was recognized, but it was generally assumed that including age controls in the regression equation would suffice. The assumptions were therefore

$$Cov(y_f^*, u_s) = Cov(y_s^*, u_f) = Cov(u_s, u_f) = Cov(y_f^*, u_f) = 0,$$

and

$$Var(u_f) \neq 0.$$

If these hold, then the probability limit in eq. (2) reduces to

$$plim\ \hat{\beta}_{approx} = \beta \frac{Var(y_f^*)}{Var(y_f^*) + Var(u_f)}.$$

This is the classical errors-in-variables model; inconsistencies are limited to attenuation bias caused by measurement error in lifetime income of *fathers*. In contrast, measurement error in sons' lifetime income is not a source of inconsistency in this model. Researchers typically used averages of multiple income observations for fathers to increase the signal-to-noise ratio, but gave less attention to the measurement of sons' income.

Recent Literature

Recently the focus has shifted towards the importance of non-classical measurement error. An early theoretical discussion can be found in Jenkins (1987). Analyzing a simple model of income formation, he finds that usage of current incomes in eq. (1) will bias $\hat{\beta}$ as income growth over the life cycle varies across individuals. He concludes that the direction of this *life-cycle bias* is ambiguous, that it can be large, and that it will not necessarily be smaller if fathers' and sons' incomes are measured at the same age.

Haider and Solon (2006) demonstrate that life-cycle bias can explain the previously noted pattern that intergenerational elasticity estimates increase with the age of sampled sons.[7] They show that the association between current and lifetime income varies systematically over the life cycle, contrary to a classical errors-in-variables model with measurement error independent of true values. Böhlmark and Lindquist (2006) find strikingly similar patterns in a replication study with Swedish data.

Haider and Solon also note that controlling for the central tendency of income growth in the population by including age controls in eq. (1) will not suffice, as variation around the average growth rate will bias estimates. Vogel (2006) provides an illustration based on the insight that highly educated workers experience steeper-than-average income growth. Since available data tend to cover annual incomes of young sons and old fathers, lifetime incomes of highly educated sons (fathers) will be understated (overstated), which is likely to bias $\hat{\beta}_{approx}$ substantially downwards if educational achievement is correlated within families. Indeed, the probability limit of $\hat{\beta}_{approx}$ can be negative in extreme cases, as our data will confirm. Various refined estimation procedures have been proposed to address such life-cycle bias. We proceed to examine the most popular one in detail.

2 Measuring Income at a Certain Age

Haider and Solon (HS) generalize the classical errors-in-variables model to allow for variation in the association between annual and lifetime income over the life cycle, which they document to be substantial. Their underlying intuition is that for two individuals with different income trajectories there will nevertheless exist an age t^* where the difference between individuals' log annual incomes equals the difference between their log (annuitized) lifetime incomes. The generalized model coincides with a classical errors-in-variables model at t^*, suggesting that lifetime incomes should be approximated by annual incomes around this age.

The model is applicable to any analysis that relies on approximation of lifetime income by short-term measures, but we describe it here in the context of the intergenerational mobility literature. As HS we first focus on left-side measurement error and assume that $y^*_{s,i}$ is unobserved and proxied by $y_{s,it}$, log annual income of sons at age t. Their generalization of the classical errors-in-variables model is given by

$$y_{s,it} = \lambda_{s,t} y^*_{s,i} + u_{s,it}, \tag{3}$$

where $\lambda_{s,t}$ (the slope coefficient in the linear projection of $y_{s,it}$ on $y^*_{s,i}$) is allowed to vary by age and $u_{s,it}$ is orthogonal to $y^*_{s,i}$ by construction. Regressing $y_{s,it}$ on $y^*_{f,i}$ by OLS, and using eqs. (3) and (1) to substitute for $y_{s,it}$ and $y^*_{s,i}$, yields

[7] For a summary, see Solon (1999). Age-dependency of elasticity estimates could also arise if the dispersion in transitory income and thus the attenuation bias vary over the life cycle. Such variation has been documented in Björklund (1993) for Sweden, but Grawe (2006) finds that the observed age-dependency can be better explained by the existence of life-cycle bias.

$$plim\ \hat{\beta}_t = \frac{Cov(y_{s,t}, y_f^*)}{Var(y_f^*)} = \beta\lambda_{s,t} + \frac{Corr(y_f^*, u_{s,t})\,\sigma_{u_{s,t}}}{\sigma_{y_f^*}}. \tag{4}$$

HS make the assumption that

$$Corr(y_f^*, u_{s,t}) = 0, \tag{5}$$

which implies that left-side measurement error would be innocuous for consistency of intergenerational elasticity estimates if lifetime incomes of sons were proxied by annual incomes at an age t^* where $\lambda_{s,t}$ is close to one. Their empirical analysis reveals that for an American cohort born in the early 1930s $\lambda_{s,t}$ is below one for young ages, but close to one around midlife.

The model, often referred to as the *generalized errors-in-variables (GEiV) model*, thus illustrates how life-cycle bias should be expected to vary with age. Apart from providing conceptual insight, this knowledge is potentially also of great usefulness in applications. Researchers often face the problem that long-run outcomes like lifetime income are of theoretical interest, but that available data only contain short snapshots of income. The GEiV model offers a potential remedy since it implies that measurement of income at a certain age might suffice if long-run outcomes are not directly observed. Possible applications are for example the returns to schooling or, as emphasized by HS, the intergenerational mobility literature.

The model has indeed been widely adopted in the latter, where the implied procedure to measure income around midlife has become standard practice.[8] A variation of the model that relies on the same intuition has been presented in Lee and Solon (2009).

But as the classical errors-in-variables model, the GEiV model depends critically on assumption (5), as also noted by HS. The validity of this assumption has not been examined and the current literature tends to assume that the model can eliminate or nearly eliminate life-cycle bias in applications.[9] But this is unlikely to be the case since there are reasons why assumption (5) or similar assumptions should not be expected to hold.

To understand the intuition, first note that for more than two workers we will generally *not* find an age t^* where annual income is an undistorted approximation of lifetime income. Figure 1 illustrates this argument by plotting log income trajectories for workers 1, 2 (as in Figure 1 in HS) and an additional worker 3. The horizontal lines depict log annuitized lifetime income, and differences in workers' log lifetime income are given by the vertical distances between these lines. At age t_1^* the distance between the annual income trajectories equals the distance between the horizontal lines for workers 1 and 2, and at age t_2^* for workers 1 and 3. There exists no age where these distances are equal for all three workers at once.[10] This example illustrates that the parameter $\lambda_{s,t}$ only captures how differences in annual income and differences in lifetime income relate *on average* among *all* workers. Individuals, and groups of individuals, will nevertheless deviate from this average relationship, so that their annual incomes systematically over- or understate their lifetime incomes compared to the rest of the population. A typical example is

[8]Among others, in Gouskova et al. (2010) for the US; Björklund et al. (2006, 2009) for Sweden; Nilsen et al. (2012) for Norway; Raaum et al. (2007) for Denmark, Finland, Norway, the UK and the US; Nicoletti and Ermisch (2007) for the UK; Piraino (2007) and Mocetti (2007) for Italy. More examples are covered in the surveys of Björklund and Jäntti (2009) and Black and Devereux (2011).

[9]See for example Gouskova et al. (2010), Grawe (2006), and Lee and Solon (2009).

[10]This result does not depend on a high degree of complexity in income growth processes, but holds for example also for a simple log-linear income formation model as analyzed in HS (see Appendix A1).

that highly educated individuals tend to experience steeper income growth over the life cycle, such that their annual incomes understate (overstate) lifetime incomes at young (old) ages relative to individuals with less education.

For intergenerational mobility studies it is crucial that such idiosyncratic deviations might correlate within families or with parental income. For example, sons from poorer families may have higher initial incomes and flatter slopes if credit constraints affect human capital accumulation and job-search behavior in their early career. More generally, there are several reasons to suspect dependency within families: parents can transmit abilities and preferences, influence their offspring's educational, or their occupational choices; all of which are factors that may affect the shape of income profiles over the life cycle. The individual association between annual and lifetime income is thus likely to exhibit an intergenerational correlation itself and cannot be sufficiently captured by a single population parameter like $\lambda_{s,t}$.

Assumption (5) is then unlikely to hold, the probability limit of $\hat{\beta}_t$ does not equal $\lambda_{s,t}\beta$, and knowledge of the exact life-cycle pattern of $\lambda_{s,t}$ cannot eliminate life-cycle bias.[11] The illustrative usefulness of the GEiV model is not impaired by these arguments. Furthermore, it may represent a large improvement over the assumptions of the classical errors-in-variables model in applications, which we will examine empirically. Our arguments however imply that life-cycle bias is harder to address than has been hoped in the subsequent applied literature, and that the search for a "right age" to measure income at might not be an entirely satisfying path to follow.

There are various ways to probe our theoretical arguments. One can examine the validity of assumption (5) formally by deriving the elements of $u_{s,it}$ for a given income formation model and analyzing its relation to the regressor $y^*_{f,i}$. While it can be shown that $u_{s,it}$ is correlated with $y^*_{f,i}$ even for a simple log-linear income formation model (see Nybom and Stuhler, 2011), such exercises will not be informative on the magnitude of life-cycle bias that should be expected in practice. In the next section we will thus provide empirical evidence on its size. In section 4 we will also present brief evidence on parent-related heterogeneity in the shape of income profiles, which is the source of this bias.

3 Empirical Evidence on Life-Cycle Bias

We use Swedish panel data containing nearly life-long income histories to provide direct evidence on the life-cycle bias in estimates of the intergenerational elasticity that are based on annual incomes. The size of the bias that remains after application of the GEiV model depends on two factors. First, the complexity of income profiles in the population.[12] Second, if the dispersion in income profiles is caused by heterogeneity or stochastic shocks. The former more than the latter would cause idiosyncratic deviations from average income profiles to be correlated within families.

[11] Corresponding biases arise in the case of right-side measurement error in which unobserved lifetime income of fathers is approximated by annual income (see Appendix A.2) and if approximations are made for both fathers and sons (Appendix A.3).

[12] For example, if individuals merely differ in linear income growth then differences in log lifetime income are well approximated by differences in log current income around midlife for the whole population and the GEiV model would perform relatively well.

3.1 Data Sources and Sample Selection

To the best of our knowledge, Swedish tax registry data offer the longest panel of income data, covering annual incomes across 48 years for a large and representative share of the population. Moreover, a multi-generational register matches children to parents, and census data provide information on schooling and other individual characteristics. All merged together, the data provide a unique possibility to examine life-cycle bias in intergenerational mobility estimation using actual income histories.

To select our sample, we apply a number of necessary restrictions. As we mainly aim to make a methodological point, we follow the majority of the literature and limit our sample to sons and their biological fathers. To these we merge income data for the years 1960-2007.[13] Since most other income measures are available only from 1968, we use total (pre-tax) income, which is the sum of an individual's labor (and labor-related) earnings, early-age pensions, and net income from business and capital realizations.

Our main analysis is based on sons born 1955-1957. Earlier cohorts could be used, but then we would observe fewer early-career incomes for fathers. Conversely, later cohorts are not included since we want to follow the sons for as long as possible. Moreover, to avoid large differences in the birth year of fathers, we exclude pairs where the father was older than 28 years at the son's birth. Young fathers and first-born sons are thus over-represented in our sample. Although this is a limitation, we expect any detected bias for this particular sample to understate the bias in the population.[14] On other sampling issues we adopt the restrictions applied by HS and Böhlmark and Lindquist (2006).[15]

Our data come with a couple of drawbacks. To maximize the length of the income histories we use the measure total income, whereas e.g. HS use labor earnings. However, total income is a highly relevant measure of economic status, approximation of lifetime status gives rise to the same methodological challenges, and Böhlmark and Lindquist find that total income and earnings yield similar estimates of life-cycle bias. Further, the use of tax-based data could raise concerns about missing data in the low end of the distribution if individuals have no income to declare. The Swedish system however provides strong incentives to declare some taxable income since doing so is a requirement for eligibility to most social insurance programs. Hence, this concern most likely only applies to a very small share of the population.

Our data also have many advantages. First, they are almost entirely free from attrition. Second, they pertain to all jobs. Third, in contrast to many other studies, our data are not right-censored. Fourth, we use registry data, which is believed to suffer less from reporting errors than survey data. Fifth, and most important, we have annual data from 1960 to 2007, giving us nearly career-long series of income for both sons and their fathers. Overall, we believe that the data are the best available for the purpose of this study.

Our main sample consists of 3504 father-son pairs, with sons' income measured from age 22

[13] Income data for the year 1967 are missing in the registry.

[14] Reduced sample heterogeneity will tend to decrease heterogeneity in income profiles, which in turn diminishes the idiosyncratic deviations from sample average relationships between annual and lifetime income that cause life-cycle bias.

[15] We restrict the sample to fathers and sons who report positive income in at least 10 years. We exclude those who died before age 50, and sons who immigrated to Sweden after age 16 or migrated from Sweden on a long-term basis (at least 10 years).

to age 50 and fathers' income measured from age 33 to age 65, irrespective of birth years. We express all incomes in 2005 prices, apply an annual discount rate of 2 percent, and divide the sums by the number of non-missing income observations to construct our measures of annuitized lifetime income. Table 1 reports descriptive statistics. Rows (2) and (3) show that dispersions in lifetime income are of similar magnitudes for fathers and sons. Rows (4) and (5) provide information on the number of positive income observations. On average there are more than 28 observations for sons, and more than 30 for fathers, with relatively low dispersion in both cases.

3.2 Empirical Strategy

To assess the size of life-cycle bias we compare estimates based on annual incomes with a benchmark estimate that is based on lifetime incomes. As in the theoretical discussion we focus on left-side measurement error (i.e., for sons), although we provide brief evidence on life-cycle bias due to right-side (i.e., for fathers) and measurement error on both sides in a later subsection. We do this for two reasons. First, left-side measurement error has until recently been neglected in the literature. Second, life-cycle bias is not confounded by attenuation bias from classical measurement error on the left-hand side, which simplifies the analysis.

We use our measures of log lifetime incomes $y^*_{f,i}$ and $y^*_{s,i}$ to estimate eq. (1) by OLS, which yields our benchmark estimate $\hat{\beta}$.[16] We then approximate log lifetime income of sons $y^*_{s,i}$ by log annual income $y_{s,it}$ (left-side measurement error) to estimate

$$y_{s,it} = \beta_t y^*_{f,i} + \epsilon_i,$$

separately for each age t, to obtain a set of estimates $\hat{\beta}_t$. Finally, we estimate eq. (3), which provides us with estimates of $\lambda_{s,t}$. Note that none of these estimations include any additional controls.

Under the assumptions of the GEiV model, the probability limit of $\hat{\beta}_t$ equals $\lambda_{s,t}\beta$, and using annual income of sons at age t^* where $\lambda_{s,t} = 1$ consistently estimates β.[17] As discussed in the previous section, we anticipate $\hat{\beta}_t$ to be biased even after adjustment by $\hat{\lambda}_{s,t}$. The *remaining* life-cycle bias after adjustment by the GEiV model, denoted by $\hat{b(t)} = \hat{\beta}_t/\hat{\lambda}_{s,t} - \hat{\beta}$, is thus of central interest.[18] Note that we assume that $\hat{\lambda}_{s,t}$ is known in order to evaluate the model's theoretical capability to adjust for life-cycle bias under favorable conditions. A second (known) source of inconsistency can arise in that the age profile of $\lambda_{s,t}$ will typically not be directly estimable by the researcher.

[16] Of course, this estimate is not exactly *true* since we still lack some years of income. This does however not affect of our approach to use the estimate as a benchmark. The GEiV model is not restricted to any specific population, and should therefore be applicable to our variant of the Swedish population in which we truncate income profiles at some age. It is nevertheless advantageous that we have long income histories. First, our benchmark estimate will be closer to the true value. Second, since the income profiles contain most of the idiosyncratic heterogeneity that leads to life-cycle bias, we expect our estimate of the bias to be representative for a typical application. We provide evidence that our main findings are not sensitive to the exact length of observed income histories in section 3.4.

[17] Since $\lambda_{s,t}$ will not necessarily equal exactly one at t^* we adjust $\hat{\beta}_t$ by $\hat{\lambda}^{-1}_{s,t}$ at all ages, including t^*.

[18] The arguments of HS relate to the probability limit. In a finite sample we need to consider the distribution of $\hat{b(t)}$. Reported standard errors for $\hat{b(t)}$ are based on a Taylor approximation and take the covariance structure of $\hat{\beta}$, $\hat{\beta}_t$, and $\hat{\lambda}_{s,t}$ into account.

3.3 Empirical Results

We first present estimates of $\lambda_{s,t}$. Figure 2 shows that $\hat{\lambda}_{s,t}$ rises over age and crosses one at around age $t^* = 33$. Largely consistent with others, we find that income differences at young (old) age substantially understate (overstate) differences in lifetime income. We note that $\hat{\lambda}_{s,t}$ is close to one only for a short time around age 33, in contrast to the pattern found for older American and Swedish cohorts in HS and Böhlmark and Lindquist (2006) in which $\hat{\lambda}_t$ remains close to one for an extended period through midlife. A general concern is thus that measuring annual income only a few years earlier or later can cause large differences in elasticity estimates.

Our central estimates are presented in Figure 3, which plots $\hat{\beta}$ (the benchmark elasticity), $\hat{\beta}_t$ (estimates based on annual income of sons at age t), and $\hat{\beta}_t/\hat{\lambda}_{s,t}$ (estimates at age t adjusted by the GEiV model). Table 2 provides additional statistics in the most central age range around t^*. Note that the sample is balanced within (but not across) each age. Zero or missing income observations that are not considered for estimation of $\lambda_{s,t}$ and β_t are not used to estimate β, which is reestimated for each age. The benchmark elasticity thus varies slightly over age. We list our key findings.

First. Our benchmark estimate of the intergenerational elasticity of lifetime income for our Swedish cohort is about 0.27 (see also Table 2). This is marginally higher than what most previous studies have found for Sweden, and should be closer to the population parameter due to our nearly complete income profiles.[19]

Second. We confirm that the variation of $\hat{\beta}_t$ over age resembles the pattern in $\hat{\lambda}_{s,t}$, as predicted by the GEiV model. We therefore find that $\hat{\beta}_t$ increases with age and that the life-cycle bias is negative for young and positive for old ages of sons. One of the central predictions of the GEiV model, that current income around midlife is a better proxy for lifetime income than income in young or old ages, is thus confirmed.

Third. The magnitude of life-cycle bias stemming from left-side measurement error alone can be striking. For example, analysis based on annual income of sons only two years below age t^* yields $\hat{\beta}_t = 0.191$, in contrast to a benchmark estimate that is almost 40 percent larger. Moreover, analysis based on income below age 26 yields a *negative* elasticity. We therefore find direct evidence on the importance of life-cycle bias in intergenerational mobility estimates that has been discussed in the recent literature.

Fourth. The life-cycle bias is larger than implied by the GEiV model. While the adjustment of estimates according to this model leads on average to sizable improvements, it cannot fully eliminate the bias. This holds true even under the assumption that the central parameters $\lambda_{s,t}$ are directly estimable. The remaining bias is overall substantial, and especially large for young ages. Intergenerational elasticity estimates based on income at very young ages are still negative.[20]

Fifth. The life-cycle bias is not minimized at age t^*, the age at which the current empirical

[19] The benchmark elasticity is nevertheless still likely to understate the true intergenerational elasticity. We lack some early observations of fathers and late observations of sons, which reduces $\sigma_{y_s^*}$ and increases $\sigma_{y_f^*}$, thereby reducing the numerator and increasing the denominator of the OLS estimator.

[20] The results further imply that even exact knowledge of the pattern of $\lambda_{s,t}$ over age is not much more useful than the rule of thumb that income should be measured around midlife instead of young or old ages. In fact, for our cohort the "correction" of elasticity estimates by $\hat{\lambda}_{s,t}$ *worsens* elasticity estimates around midlife (age 33 to age 40), but improves estimates at older ages.

literature aims to measure income, but at an age $t > t^*$. We report a similar pattern for other cohorts in section 3.4.

Sixth. The remaining life-cycle bias $\hat{b(t)}$ around age t^* is substantial and significantly different from zero. Table 2 shows that $\hat{b(t)}$ is on average around 0.05 over ages 31-35, which corresponds to about 20 percent of our benchmark. Knowledge of age t^* will thus not eliminate life-cycle bias. Furthermore, the large deviation from this average at age 32 indicates that mobility estimates based on single annual incomes on the left-hand side may not only suffer from systematic age-dependent bias but also less predictable year-to-year variability.

We briefly compare these empirical results with our theoretical discussion of the determinants of $\hat{b(t)}$. Table 3 shows the components of $\hat{b(t)}$ according to eq. (4). Variation of $\hat{b(t)}$ over age stems mostly from variation in the residual correlation $Corr(y_f^*, u_{s,t})$, while the ratio $\sigma_{u_{s,t}}/\lambda_{s,t}\sigma_{y_f^*}$ is close to one over most of the life cycle.[21] Seemingly small residual correlations can thus translate into substantive biases. For example, a residual correlation of 0.03 translates into a life-cycle bias of more than 10 percent of the benchmark elasticity.

We provided intuition why the residuals from eq. (3) correlate with parental income in the previous section. For further evidence we examine if the residuals correlate also with various other characteristics, specifically: (i) father's age at birth of his son, (ii) father's education, (iii) son's education, (iv) son's cognitive ability, and (v) son's country of birth. Table 4 describes how each variable is measured and presents the results. Most estimates are significantly different from zero. The residuals correlate particularly strongly with education, implying that the GEiV model cannot capture some of the heterogeneity in income profiles that arises from human capital investment. But the residuals correlate also with other variables, such as ethnic background.[22] The GEiV model should thus not be expected to eliminate life-cycle bias in other literatures, in which interest lies on different explanatory variables. It captures changes in the average association between annual and lifetime income in the population over age, but applications are typically based on comparisons of specific subgroups of the population. The model can then not fully eliminate life-cycle bias since the association between annual and lifetime income varies not only over age, but also over groups defined by parental income, years of schooling, gender, or other characteristics.[23]

These results provide guidance for applied research, but some remarks about generalizability are warranted. Life-cycle bias will differ quantitatively across populations. The bias is determined by the degree of systematic differences in income profiles between sons from poor and sons from rich families. This mechanism is likely to vary across cohorts and countries. The question is if observed qualitative patterns over age can nevertheless be generalized. Figure 3 demonstrates that annual income at old age provides a more reliable base for application of the GEiV model in intergenerational studies than income at young age. The remaining life-cycle bias is large and

[21] The previously documented increase in $\lambda_{s,t}$ over age is offset by an increase in $\sigma_{u_{s,t}}$.

[22] The observation that annual incomes in early age tend to understate lifetime incomes for sons born outside Sweden may for example relate to earnings assimilation, the tendency of immigrants to experience lower initial earnings but faster growth than native workers.

[23] The observation that the residuals correlate most strongly with education indicates that the GEiV model may perform worse in applications in which education plays a central role. Bhuller et al. (2011) examine life-cycle bias in returns to schooling estimates, and also analyze the applicability of the GEiV model in this context.

negative up until the early forties, but then small for most older ages.[24] Thus, the relationship between current and lifetime income differs with respect to family background particularly at the beginning of the life cycle. This result is intuitive if one considers potential causal mechanisms of intergenerational transmission. Sons from rich families might acquire more education or face different conditions that particularly affect initial job search (e.g. regarding credit-constraints, family networks, or ex-ante information on labor market characteristics). Such mechanisms are likely to apply to some degree to most populations. Although the size of the life-cycle bias is bound to differ across populations, its pattern over age is thus likely to hold more generally. This conclusion is supported by results for other Swedish cohorts, as we will discuss later on.

3.4 Extensions

We proceed to examine alterations of the estimation procedure to reduce the bias and to test the sensitivity of our results concerning the GEiV model.

Multi-Year Averages of Current Income

The importance of dealing with transitory noise in short-run income measures on the right-hand side, for example by using multi-year averages, is well recognized in the literature (see Mazumder, 2005). But some recent studies that reference to the GEiV model (see footnote 8) average also over multiple income observations on the left-hand side (e.g. for sons), although without clear theoretical motivation. One rationale could be that researchers do not know the exact age at which $\lambda_{s,t}$ equals one. Our finding that life-cycle bias is substantial even if this age would be known raises the question if and how such practice can help to reduce the bias.

We therefore estimate β_t using three-, five- and seven-year averages of son's income centered around age t^*. These averages are also used to estimate $\lambda_{s,t}$, and the remaining life-cycle bias after adjustment by $\hat{\lambda}_{s,t}$. The results are summarized in Table 5. The remaining life-cycle bias falls in the number of income observations but is not eliminated. For the seven-year average, the estimated bias (in absolute value) is on average slightly below 0.03 at ages 31-35 compared to about 0.05 using one-year measures. The standard deviation of the residuals $\hat{\sigma}_{u_{s,t}}$, which is a central component of the bias, decreases by about a third when moving from one- to seven-year measures, and diminishes the estimated bias proportionally. The residual correlation falls only slightly and estimates of $\lambda_{s,t}$ remain stable. These improvements are moderate, but they are generalizable since they are simply driven by the fact that the residual variance decreases when more income observations are used. Our results thus provide a rationale for averaging over multiple income observations also on the left-hand side when possible.

Treatment of Outliers in the Income Data

Intergenerational elasticity estimates can be sensitive to how one treats outliers in general, and observations of zero or missing income in particular (Couch and Lillard, 1998; Dahl and DeLeire, 2008). We test the robustness of our results along this dimension by (i) balancing the sample

[24] The latter result cannot easily be exploited. Adjustment of $\hat{\beta}_t$ by $\hat{\lambda}_{s,t}$ can rarely be done in practice due to lack of information on the latter. Importing estimates of $\lambda_{s,t}$ from other sources can be misleading since its pattern over age could differ across populations.

across ages such that only sons with positive income in all ages 31-35 are included, (ii) bottom-coding very low non-missing incomes, and (iii) top-coding very high incomes.[25] We compare the life-cycle bias for ages 31-35 for each of these samples (summarized in Table 6) with the results for our main sample in Table 2.

Estimates of the remaining life-cycle bias are on average about a third lower for the balanced sample than for our main sample (at ages 31-35), but still correspond to more than 10 percent of the benchmark elasticity. Decreases in both the residual correlation and residual variance contribute to this drop.[26] Bottom-coding has the opposite effect and increases the bias slightly since observations with zero income are now always included. Finally, results for a sample with top-coded incomes are very similar to those for the main sample, implying low sensitivity to the exact measurement of high incomes. While we thus find that zero and missing incomes are influential for the size of life-cycle bias, it is not obvious what the right sampling choice would be. To derive a general measure of mobility one would like to include all individuals, but our analysis shows that doing so comes with the cost of increased life-cycle bias.

Length of Observed Income Profiles

Although our data are to our knowledge the best available for our purpose, it might be a concern that our measures of lifetime income are still based on incomplete income histories. We thus perform a number of robustness tests. We consider a younger cohort — sons born 1958-60 — to study the influence of early-age income data of fathers, and an older cohort — born 1952-54 — to study the influence of late-age data of sons.

Age profiles of the life-cycle bias before and after adjustment by $\hat{\lambda}_{s,t}$ are shown in Figures 4 (main sample), 5 (cohort 1958-60), and 6 (cohort 1952-54) for variations of the age spans. Abstracting from general cohort differences, we find that changes in the fathers' age span have little effect on the life-cycle bias, probably due to our focus on left-side measurement error. In contrast, changes in the sons' age span cause noticeable shifts. This is not unexpected since changes in the age span on which our measures of lifetime income are based are likely to alter both $\sigma_{y_s^*}$ and $\lambda_{s,t}$ slightly. While the exact relation between the size of the life-cycle bias and age therefore depends on the definition of the age span, the major facts remain stable: the remaining life-cycle bias after adjustment by $\hat{\lambda}_{s,t}$ can be large and tends to be negative for young ages and around t^*.

Cohort and Population Differences

We use the same three cohort groups to briefly assess if the magnitude of life-cycle bias can be expected to vary across populations. To separate true cohort differences from differences due to age span definitions, we limit the income profiles of both fathers and sons to the longest age

[25] As of the log-specification we do not expect high extremes to have as large influence as low extremes. Top-coding has however been suggested to test the sensitivity to some changes of administrative routines and tax levels across our time period (see Böhlmark and Lindquist, 2006).

[26] Excluding those with occasional zeros or missings reduces the number of extreme values and thereby the variation in $u_{s,it}$. The residual correlation decreases since individuals with frequent zero and missing income observations are likely to experience quite different income profiles than the average population, and therefore amplify the heterogeneity in income profiles that causes the residual correlation.

span observed in all three samples. We thus use incomes of sons for ages 22-47, and incomes of fathers for ages 36-65.[27]

Table 7 presents the most central results around age t^* for each sample.[28] The 1958-60 cohort has an estimated benchmark elasticity $\hat{\beta}$ that is similar to our main cohort but a slightly larger remaining life-cycle bias $\hat{b(t)}$. For the 1952-54 cohort both $\hat{\beta}$ and $\hat{b(t)}$ are substantially lower.

Figure 7 plots estimates of β_t for all three samples over the full age range. While the overall pattern over age are relatively similar, the differences between elasticity estimates at each age are quite volatile. These differences — substantial even for large random samples and a fixed sampling procedure across cohorts within Sweden — confirm that life-cycle bias should be expected to differ across studies and populations even if incomes are measured at the same age.

Variants in the Intergenerational Mobility Literature

Lee and Solon (2009) present an extension of the GEiV model that allows researchers to use income observations over multiple years. They exploit that the results in Haider and Solon (2006) imply that life-cycle bias is a function of sons' age at measurement, an implication that is confirmed by our data. This functional relation can thus be explicitly captured in a regression equation such as

$$y_{s,it} = \alpha' D + \beta y^*_{f,i} + \delta_1(t-t^*) + \ldots + \delta_4(t-t^*)^4 + \theta_1 y^*_{f,i}(t-t^*) + \ldots + \theta_4 y^*_{f,i}(t-t^*)^4 + \epsilon_i. \quad (6)$$

This equation contains a vector of year dummies D, a quartic in child's age (normalized to zero at age t^*), and an interaction of the child's normalized age quartic with father's log lifetime income.[29] Intuitively, the latter approximates the life-cycle pattern of β_t as evident in Figure 3. The choice of t^* reflects at which age elasticity estimates are expected to be unbiased based on the predictions of the GEiV model.

This specification provides two important advantages. First, the usage of additional income observations for each cohort can potentially improve statistical efficiency. Second, intergenerational elasticities can be estimated for cohorts for which income is not observed at age t^*.[30] These two properties are especially useful in analyses of trends in income mobility, as these typically rely on sparse income data and measurement of income at young or old ages for some cohorts (see Lee and Solon, 2009). Hence this specification has been used in most of the recent

[27] Restricting the age intervals reduces the benchmark estimate. Dropping income observations for sons at old age and fathers at young age decreases $\sigma_{y^*_s}$ and increases $\sigma_{y^*_f}$, reducing the numerator and increasing the denominator of the OLS estimator.

[28] More detailed evidence on cohort differences is also provided in Figures 11 and 12 in Nybom and Stuhler (2011).

[29] Our specification is a simplified version of the specification used in Lee and Solon (2009). First, we do not control for father's age since we observe comparable measures of lifetime income for all fathers. Second, we only estimate one elasticity parameter β instead of elasticity parameters for each cohort or year since we are testing for life-cycle bias instead of estimating mobility trends.

[30] Both these advantages however hinge on assumptions, namely (i) that the pattern of life-cycle bias over the age of sons can be well approximated by a fourth-order polynomial, and (ii) that this age pattern is stable across cohorts. The latter seems problematic given the results presented in our previous subsection. From Figure 3 we however expect that the first assumption is indeed valid.

research on mobility trends, as for example in Hertz (2007), Pekkala and Lucas (2007), Nicoletti and Ermisch (2007) and Aaronson and Mazumder (2008).

However, specification (6) requires that elasticity estimates are unbiased at age t^*, and thus relies on the same assumption as the GEiV model. We therefore expect it to be subject to a similar degree of life-cycle bias. To probe this conjecture we estimate (6) by OLS, separately for 10-year intervals of income observations for sons around each age from age 28 (interval 24-33) to 37 (interval 33-42).[31] The resulting estimates of β are all in the range 0.205-0.218, close to our previously reported estimate $\hat{\beta}_t = 0.203$ that is based on sons' annual income at age $t^* = 33$. Statistical precision does however indeed rise, as standard errors of the elasticity estimates shrink by almost a half. Both findings also hold when using a randomly selected number of income observations for each son in a given age range. We conclude that Lee and Solon's objective to improve statistical efficiency by pooling income observations over multiple years has been fulfilled, but that estimates suffer from a similar level of life-cycle bias as other estimates that are based on the GEiV model. Estimates still differ by almost 20 percent from our benchmark elasticity based on lifetime incomes, even when based on a large number of income observations per son. This remaining life-cycle bias can differ by cohort (see previous subsection) and may thus mask gradual changes of mobility over time or generate a false impression of such trends. These results may partly explain why the recent literature on intergenerational mobility trends has produced wildly diverging estimates (see Lee and Solon, 2009).

3.5 Measurement Error on the Right-Hand Side or Both Sides

Although our findings on left-side measurement error are conceptually interesting, evidence on the combined effects of life-cycle bias from both sides is more relevant for practitioners. The questions arise whether we find similar life-cycle effects from the right-hand side, and whether these tend to cancel out or aggravate the effects from left-side measurement error. Our data allow us to directly examine these questions. We now base estimates of β_t on lifetime income of sons and approximation of lifetime income by annual income for fathers (right-side measurement error) or approximation for both fathers and sons (measurement error on both sides). The probability limit of $\hat{\beta}_t$ is then affected by attenuation and life-cycle bias. We adjust for both according to the GEiV model. Results are shown in Figures 8 and 9.[32]

Figure 8 demonstrates the additional large attenuating effects from right-side measurement error. The remaining life-cycle bias after adjustment by the GEiV model follows a similar qualitative pattern over age as for the case of left-side measurement error. Figure 9 shows the remaining life-cycle bias in the case of measurement error on both sides with fathers' and sons'

[31] We do not report estimates that are based on income spans at very young or very old ages of sons since estimates of β become very erratic if not at least some observations around age t^* are included in the regression. This result is due to the fourth-order polynomial approximation of life-cycle patterns in (6). We find that usage of a quadratic instead of a quartic in age provides more reliable results in such cases.

[32] Adjustment is based on separate estimates of λ_t for both fathers and sons, denoted $\hat{\lambda}_{f,t}$ and $\hat{\lambda}_{s,t}$. According to the GEiV model the probability limit of $\hat{\beta}_t$ equals $\theta_{f,t}\beta = (\lambda_{f,t}\sigma^2_{y_f^*}/(\lambda^2_{f,t}\sigma^2_{y_f^*} + \sigma^2_{u_{f,t}}))\beta$ for right-side and $\lambda_{s,t}\theta_{f,t}\beta$ for both-side measurement error (assuming $Corr(u_{s,it}, u_{f,it}) = 0$ in addition to assumption 5). Therefore the remaining life-cycle biases equal $\hat{b(t)} = \hat{\beta}_t/\hat{\theta}_{f,t} - \hat{\beta}$ and $\hat{b(t)} = \hat{\beta}_t/\hat{\lambda}_{s,t}\hat{\theta}_{f,t} - \hat{\beta}$, respectively. See Appendix A.2 and A.3 for a detailed derivation of the components of these biases. For presentational purpose we use only one age subscript t and display combinations of annual income for sons and fathers with equal distances to their respective t^* in Figure 9.

incomes measured at similar ages. It is overall larger than for left-side measurement error alone, thus indicating aggravating effects of measurement error on both sides.[33] Importantly, this is also the case when fathers' and sons' incomes are measured at their respective t^*. We again find that the GEiV model is less successful in reducing the bias for early ages and around t^* than for later ages. Moreover, the estimates suffer from strong year-to-year variability. Reducing this variability is an additional motive for averaging over multiple income observations on both sides, apart from our previous finding that it reduces the size of the bias.

4 Other Methods to Address Incomplete Income Data

We briefly examine two other methods that are employed in the intergenerational mobility literature to address incomplete income data. We examine life-cycle effects in instrumental variable (IV) estimators, and discuss why the consideration of differential income growth across subgroups will not suffice to eliminate life-cycle bias.

4.1 Instrumental Variable Estimates

IV methods have been proposed as a way to tackle *attenuation* bias that stems from right-side measurement error (Zimmerman, 1992; Solon, 1992). Furthermore, in the form of two-sample IV (TSIV) they are heavily relied on for countries with less rich data.[34] Under the classical errors-in-variables model, IV estimates are typically expected to provide an upper bound to the true intergenerational elasticity, since instruments such as parental education tend to have a positive independent effect on offspring's income (conditional on father's income). However, little is known about life-cycle effects from the usage of annual instead of lifetime incomes in an IV setting.

Haider and Solon (2006) discuss why the size and direction of bias in both OLS and IV estimates depends on the ages of sampled fathers and sons. They note that given a valid instrument, life-cycle bias from right-hand side measurement error could be addressed also in IV estimates if annual incomes were measured at an age where the slope coefficient of a regression of annual on lifetime incomes is one. Grawe (2006) discusses IV estimates in more detail and makes similar arguments. In practice, when using imperfect instruments such as those typically applied in the literature, one might thus expect that IV estimators bound the true elasticity from above if incomes were measured at the suggested age (see for example Lefranc et al., 2012).

Our argument that such strategies cannot eliminate life-cycle bias in OLS estimates extends however also to IV estimates. For example, father's education may affect the shape of income profiles over the life cycle, and thus correlate with the residual from the GEiV model (e.g., $u_{s,t}$ from eq. 3).[35] We thus use our Swedish data to provide a first examination of life-cycle bias in

[33] This holds true if estimates are only adjusted for attenuation bias but not for life-cycle effects according to the GEiV model (see Figure 13 in our working paper Nybom and Stuhler, 2011). These results confirm and substantiate the theoretical predictions of Jenkins (1987) that measuring fathers' and sons' income at similar ages might not necessarily reduce life-cycle bias, and contradict arguments in the recent literature that such "life course matching" generally leads to smaller biases than asymmetric age combinations.

[34] TSIV was first applied to the intergenerational mobility literature by Björklund and Jäntti (1997).

[35] Our working paper Nybom and Stuhler (2011) contains a formal derivation of the probability limit of the IV estimator given that the assumptions of the classical or the generalized errors-in-variables model do not hold.

two-stage least-squares IV estimates and compare them with our findings from OLS estimation. As most of the literature, we instrument for father's income by his years of education.[36] Note that our empirical results relate therefore to this particular (but frequently used) instrument.

We first derive a benchmark estimate $\hat{\beta}^{IV}$ using our measures of lifetime income y_f^* and y_s^*, to assess if IV methods provide an upper bound if lifetime incomes would be truly observed. The IV benchmark estimate ($\hat{\beta}^{IV} = 0.309, \hat{\sigma}_{\hat{\beta}^{IV}} = 0.037$) is larger than our OLS benchmark estimate ($\hat{\beta} = 0.273, \hat{\sigma}_{\hat{\beta}} = 0.017$), indicating that father's education has a positive independent relation with son's income, and that the IV estimator could indeed provide an upper bound if it were based on lifetime incomes. However, the difference $\hat{\beta}^{IV} - \hat{\beta}$ is statistically insignificant.

We again focus on left-side measurement error (using son's annual income at age t) in order to abstract from attenuation bias and to directly compare life-cycle bias in IV estimates with our results for OLS estimates. Figure 10 plots these estimates together with the OLS benchmark $\hat{\beta}$ and reveals two important results.

First, life-cycle effects from left-side measurement error are substantially larger in IV than in OLS estimates.[37] Adjustment by $\lambda_{s,t}$ would thus improve IV estimates only modestly. Usage of education as an instrument aggravates the life-cycle bias since income profiles differ strongly with education — the correlations between parental education and measurement errors in sons' and fathers' incomes are thus relatively large (c.f. Table 4). Second, IV estimates are well below the benchmark also at $t*$ at age 33 ($\hat{\beta}_{t*}^{IV} = 0.183, \hat{\sigma}_{\hat{\beta}_{t*}^{IV}} = 0.056$, whereas $\hat{\beta} = 0.270, \hat{\sigma}_{\hat{\beta}} = 0.017$).

We therefore conclude that *absent life-cycle effects*, IV estimates bound the true parameter from above. But since applications are typically based on current income, IV estimates do not bound β in practice. Given the large sensitivity of IV estimates to the age at which sons' incomes are measured (ranging between 0.08 and 0.53 over ages 30-45), we argue that such estimates need to be interpreted with caution, and that comparisons across populations may not be reliable if based on short spans of income data.

4.2 Heterogeneity in Income Profiles across Subgroups

An alternative method to address life-cycle bias in intergenerational mobility estimation is to model income processes across subgroups, instead of assuming a uniform growth rate in the population. Income growth over the life-cycle can be predicted based on a set of observable characteristics, as proposed by Vogel (2006) and Hertz (2007).

Distinguishing income growth rates across subgroups defined by education, as in Vogel (2006), can reduce measurement error in lifetime income that arises from idiosyncratic deviations from population average growth. However, after accounting for differential income growth across educational groups, other determinants of income will lead to deviations from the mean growth rate within any given group. Since such determinants might again be shared by members of the same family, measurement errors in income growth rates are still likely to be correlated within families. For example, if a father holds an occupation that typically leads to steeper than average

[36] We impute years of education from data on level of educational attainment as recorded in the 1970 census, i.e. when the fathers were around 40 years old. Using level dummies yields similar results.

[37] In contrast, life-cycle effects from right-side measurement error are not particularly strong in IV estimates (figure available from the authors upon request).

income growth, then his son's income growth might also be steeper because he is relatively more likely to enter the same occupation.[38]

Estimates could be improved by considering additional individual characteristics for the estimation of growth rates in more specific subgroups, as in Hertz (2007). But we will not be able to sufficiently project life-cycle trajectories of income if individual growth rates are determined by both observable and unobservable characteristics that correlate within families. Unexplained dispersion in income growth is large: for example, Jenkins (2009) finds substantial deviations of individual from average income trajectories in groups defined by education, sex, and birth cohort in British data. The remaining life-cycle bias caused by within-family correlation of the unexplained part of individual income growth should therefore not be expected to be negligible.

Our data allow us to provide empirical evidence in support of this argument. We derive average growth in log income for various groups of sons by regressing current log income on a polynomial in age. Figure 11 depicts such income trajectories for four groups of sons defined by education (non-college/college) and their father's lifetime income (below/above median). While income trajectories are simply shifted for the two groups without college education, the difference in income growth over the life cycle is substantial for the other two groups: college-educated sons of richer fathers have much stronger income growth than college-educated sons of poorer fathers. We thus find evidence for parent-related heterogeneity in income profiles even after controlling for a range of observable characteristics (sex, cohort, age, country of birth and education). We further find that college-educated sons of richer fathers have lower initial incomes and steeper income growth over the life-cycle than sons of poorer fathers also for a given level of sons' lifetime incomes. The generalized model does not consider this type of heterogeneity, and thus underestimates the intergenerational elasticity when sons' incomes are observed at younger ages.

These observations are of interest beyond the intergenerational mobility literature, in particular for the extensive literature on income dynamics and stochastic shocks. A lively debate is ongoing in this field on whether idiosyncratic differences in income profiles of otherwise observationally equivalent individuals are mainly due to deterministic heterogeneity or persistent stochastic shocks.[39] Life-cycle patterns in income growth that relate systematically to parental background are unlikely to stem from stochastic shocks that arrive unexpectedly to an individual. The observation of long series of income for two generations, as in Figure 11, thus provides simple evidence on the existence of a parent-related component of income growth.

5 Conclusions

Using snapshots of income over shorter periods in the estimation of intergenerational elasticities causes a so-called life-cycle bias if the snapshots cannot mimic lifetime outcomes (Jenkins, 1987). We use nearly career-long income data of fathers and theirs sons to expose the large magnitude of this bias in practice. We confirm that Haider and Solon's (2006) generalization of the classical

[38]Examples for within-family correlation in the choice of profession or employer can be easily found, for instance in the list of presidents of the United States. More comprehensive evidence on the intergenerational transmission of employers is given in Corak and Piraino (2010).

[39]See Meghir and Pistaferri (2011) for a recent summary of contributions to this debate. An intergenerational dimension provides a novel perspective, as has recently been argued by Mayer (2010).

errors-in-variables model and their widely adopted suggestion to measure incomes around mid-age provide clear improvements. However, we also show analytically and empirically that the failure of other errors-in-variables assumptions prevents full elimination of life-cycle bias in applications. The bias that persists in our Swedish data even after application of the generalized model is strongly negative when using annual income below age thirty and remains negative up until the early forties. Estimates substantially understate the true elasticity also when income is measured around the preferred age as predicted by the generalized model. Since the recent literature has aimed to measure incomes at this age we may expect that the resulting estimates tend to (still) understate the intergenerational elasticity.

Comparisons of intergenerational mobility estimates across countries, groups or cohorts may thus be of limited reliability if based on short-run income data.[40] Still, some of the major conclusions from cross-country studies are not put into question. For example, the findings that income mobility is much lower than found by the early literature, and that mobility differs strongly across countries (e.g. being lower in the U.S. than in the Nordic countries and Canada), are robust even to sizable revisions in the underlying estimates. It might however be necessary to revisit those conclusions that are based on more marginal differences between estimates. Studies on mobility trends are potentially affected since even moderate life-cycle biases may be sufficient to mask gradual changes of mobility over time. It is thus noteworthy that we find similar levels of life-cycle bias in estimates from an extension of the GEiV model presented by Lee and Solon (2009), which has been applied in much of the recent research on mobility trends. Comparisons across subgroups of a population can be compromised when the age-pattern in income profiles differs, which may for example be the case when groups are classified by education, sex or immigration status.

Finally, our findings seem most consequential for studies based on instrumental variable estimators. We find IV estimates based on sparse income data to be very sensitive to the exact age at which offspring income is measured. In particular, they do not provide upper bounds. Recent OLS estimates of the intergenerational elasticity in the U.S. that are close to or in excess of 0.6. (e.g. Mazumder, 2005, and Gouskova et al., 2010) are thus not necessarily at odds with lower IV estimates from the earlier literature (Solon, 1992; Zimmerman, 1992).

While these results are mostly negative, our analysis does provide some guidance for applied research. We find evidence that incomes at later ages (e.g. age 40-50) provide a more reliable base for application of the GEiV model. The extension presented in Lee and Solon (2009) does improve statistical efficiency compared to the original GEiV model, but the size of the life-cycle bias is largely unaffected. The bias can instead be reduced by averaging over multiple income observations from midlife (if available) for *both* fathers and sons. Finally, the treatment of zero and missing income observations has important consequences. To derive a general measure of mobility one would like to include such observations, but doing so comes with the cost of increased vulnerability to life-cycle bias.

[40] One might hope that the bias is of similar magnitude across populations, such that the validity of comparative studies is not affected. Cross-country comparisons would for example be reliable if both the dispersion and the intergenerational correlation in the shape of income profiles is of the same magnitude in each country. But since the intergenerational correlation in income levels varies across countries we suspect that it also differs in other dimensions of income profiles. Our finding that the life-cycle bias varies even across Swedish cohorts born in the same decade supports this conclusion.

Further refinements of empirical practice with restricted use of income observations around a specific age can thus improve upon previous estimates, but will not eliminate life-cycle bias. Development of a more structured approach that aims to capitalize on all available income data seems desirable. Future research could in particular benefit from a more comprehensive exploitation of partially observed income growth patterns. Intergenerational mobility estimates are often based on multiple income observations per individual, but researchers typically disregard the idiosyncratic income growth across these observations. Such partially observed growth patterns are determined by both observable and unobservable characteristics of the individual and hence contain more information on lifetime income than what current income levels and observable characteristics can provide.

Our results add to a general conclusion that can be drawn from the intergenerational mobility literature: addressing heterogeneity in income profiles is an important, difficult and recurrently underestimated task. The central problem is that idiosyncratic deviations from average income profiles correlate with a wide range of individual and family characteristics. The widespread practice of measuring annual income at a certain age as a surrogate for unobserved lifetime income is still prone to life-cycle bias, since the most appropriate age for measurement is unpredictable and since estimates can be quite sensitive to small age changes. These issues are potentially important for other literatures that rely on measurement of long-run income or income dynamics.

6 Tables and Figures

Table 1: Summary Statistics by Birth Year of Sons

	All	1955	1956	1957
Father's age at birth of son	24.68 (2.53)	24.66 (2.51)	24.77 (2.50)	24.62 (2.58)
log lifetime income (sons)	11.97 (0.43)	11.98 (0.42)	11.98 (0.42)	11.95 (0.44)
log lifetime income (fathers)	11.72 (0.42)	11.73 (0.44)	11.72 (0.43)	11.72 (0.40)
# of pos. income obs. (sons)	28.52 (1.86)	28.57 (1.71)	28.56 (1.74)	28.43 (2.11)
# of pos. income obs. (fathers)	30.32 (3.76)	29.99 (4.13)	30.36 (3.62)	30.59 (3.48)
Father-son pairs (N)	3504	1167	1173	1164

Notes: The table reports means with standard deviations within parentheses.

Table 2: OLS Estimates of Elasticities and Life-Cycle Bias

t=Age	$\hat{\lambda}_{s,t}$	$\hat{\beta}$	$\hat{\beta}_t$	$\hat{\beta}_t/\hat{\lambda}_{s,t}$	$\hat{b(t)}$	$\hat{b(t)}$ in %	N
31	0.897	0.266	0.191	0.213	-0.053	19.8	3478
	(0.019)	(0.016)	(0.023)	(0.029)	(0.021)		
32	0.909	0.267	0.246	0.271	0.003	1.3	3476
	(0.018)	(0.016)	(0.023)	(0.028)	(0.021)		
33	0.982	0.267	0.203	0.207	-0.061	22.7	3479
	(0.020)	(0.016)	(0.025)	(0.031)	(0.024)		
34	1.039	0.256	0.212	0.204	-0.051	20.1	3469
	(0.019)	(0.016)	(0.025)	(0.031)	(0.023)		
35	1.114	0.261	0.234	0.210	-0.052	19.7	3460
	(0.021)	(0.016)	(0.027)	(0.029)	(0.022)		

Notes: Cohort group 1955-1957, left-side measurement error only. The sample and thus the benchmark estimate $\hat{\beta}$ are allowed to vary by age due to partially missing data. Standard errors in parentheses, which for $\hat{\beta}_t/\hat{\lambda}_{s,t}$ and $\hat{b(t)}$ are based on Taylor approximations that take the covariance structure of $\hat{\lambda}_{s,t}$, $\hat{\beta}$, and $\hat{\beta}_t$ into account. Column (7) displays $\hat{b(t)}$ in percent of the benchmark estimate $\hat{\beta}$.

Table 3: Decomposition of Life-Cycle Bias

t=Age	$\hat{b(t)}$	$Corr(y_f^*, \hat{u}_{s,t})$	$\hat{\sigma}_{u_{s,t}}$	$\hat{\sigma}_{y_f^*}$	$\hat{\sigma}_{u_{s,t}}/\hat{\lambda}_{s,t}\hat{\sigma}_{y_f^*}$
31	-0.053	-0.044	0.455	0.424	1.198
32	0.003	0.003	0.431	0.423	1.123
33	-0.061	-0.052	0.485	0.422	1.169
34	-0.051	-0.050	0.452	0.422	1.031
35	-0.052	-0.049	0.494	0.422	1.050

Notes: The table displays the remaining bias, $\hat{b(t)}$, together with its associated components. Results are for cohort group 1955-1957, left-side measurement error only.

Table 4: Correlations Between Residuals and Characteristics

	Age Interval of Sons				
	26-30	31-35	36-40	41-45	46-50
Father's log lifetime income	-0.057*	-0.050*	-0.063*	-0.020	-0.007
Father's age at birth of son	-0.054*	0.014	0.045*	0.017	-0.006
Father's education	-0.158*	-0.061*	-0.045*	0.035	0.028
Son's education	-0.278*	-0.112*	-0.002	0.085*	0.088*
Son's cognitive ability	-0.108*	-0.073*	-0.050*	0.022	-0.004
Son's country of birth	-0.040*	-0.026	-0.002	-0.032	0.028

Table reports correlations between characteristics listed in the first column and sons' income residuals (as average in each five-year year age interval) from eq. (3) for cohort group 1955-1957. The education variables are years of education measured at about age 35, "Son's country of birth" is an indicator for being born outside Sweden, and "Son's cognitive ability" is a standardized cognitive ability measure from the military enlistment cognitive test at age 18. Star superscripts indicate correlations with p-value<0.05.

Table 5: OLS Estimates with Multi-Year Averages of Son's Income

	Three-Year Average			Five-Year Average			Seven-Year Average		
t=Age	$\hat{\beta}$	$\hat{\beta}_t$	$\widehat{b(t)}$	$\hat{\beta}$	$\hat{\beta}_t$	$\widehat{b(t)}$	$\hat{\beta}$	$\hat{\beta}_t$	$\widehat{b(t)}$
31	0.268	0.218	-0.015	0.268	0.213	-0.017	0.270	0.221	-0.015
	(0.016)	(0.020)	(0.017)	(0.016)	(0.018)	(0.014)	(0.016)	(0.018)	(0.013)
32	0.267	0.214	-0.041	0.268	0.229	-0.020	0.268	0.227	-0.018
	(0.016)	(0.021)	(0.017)	(0.016)	(0.020)	(0.014)	(0.016)	(0.018)	(0.013)
33	0.267	0.229	-0.041	0.267	0.226	-0.043	0.268	0.238	-0.023
	(0.016)	(0.022)	(0.022)	(0.016)	(0.021)	(0.016)	(0.016)	(0.020)	(0.013)
34	0.268	0.229	-0.056	0.267	0.235	-0.044	0.268	0.245	-0.038
	(0.016)	(0.024)	(0.020)	(0.016)	(0.022)	(0.018)	(0.016)	(0.022)	(0.015)
35	0.262	0.232	-0.059	0.268	0.247	-0.052	0.267	0.251	-0.044
	(0.016)	(0.025)	(0.020)	(0.016)	(0.024)	(0.019)	(0.016)	(0.023)	(0.017)

Notes: Cohort group 1955-1957, left-side measurement error only. Standard errors in parentheses.

Table 6: Summary of Robustness Tests

	Balanced Sample			Bottom-Coded Incomes			Top-Coded Incomes		
t=Age	$\hat{\beta}$	$\hat{\beta}_t$	$\hat{b(t)}$	$\hat{\beta}$	$\hat{\beta}_t$	$\hat{b(t)}$	$\hat{\beta}$	$\hat{\beta}_t$	$\hat{b(t)}$
31	0.257	0.184	-0.033	0.271	0.205	-0.046	0.251	0.191	-0.050
	(0.015)	(0.021)	(0.021)	(0.016)	(0.023)	(0.020)	(0.016)	(0.024)	(0.020)
32	0.257	0.227	0.014	0.271	0.245	-0.012	0.252	0.246	0.005
	(0.015)	(0.020)	(0.021)	(0.016)	(0.023)	(0.020)	(0.016)	(0.023)	(0.019)
33	0.257	0.185	-0.053	0.270	0.201	-0.069	0.252	0.203	-0.056
	(0.015)	(0.023)	(0.025)	(0.016)	(0.024)	(0.022)	(0.016)	(0.025)	(0.023)
34	0.257	0.219	-0.029	0.270	0.247	-0.044	0.240	0.213	-0.047
	(0.015)	(0.022)	(0.021)	(0.016)	(0.025)	(0.020)	(0.015)	(0.025)	(0.022)
35	0.257	0.239	-0.027	0.270	0.250	-0.053	0.246	0.234	-0.048
	(0.015)	(0.024)	(0.020)	(0.016)	(0.026)	(0.019)	(0.015)	(0.027)	(0.020)

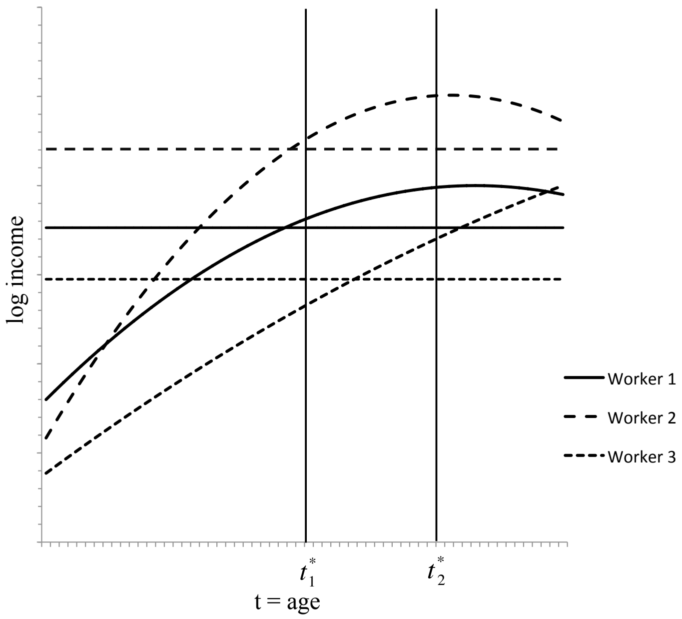
Notes: Cohort group 1955-1957, left-side measurement error only. Standard errors in parentheses. The sample in columns (1)-(3) is balanced across ages, hence excluding individuals who have zero or missing incomes at any age 31-35. The sample in columns (4)-(6) is with low non-missing incomes bottom-coded as 10 000 SEK. The sample in columns (7)-(9) is with high incomes top-coded as 2 000 000 SEK.

Table 7: Summary of Cohort Differences, Averages over Ages 31-35

Cohort Group	$\hat{\lambda}_{s,t}$	$\hat{\beta}$	$\hat{\beta}_t$	$\hat{\beta}_t/\hat{\lambda}_{s,t}$	$\hat{b(t)}$	$\hat{b(t)}$ in %	N
1958-60	1.071	0.274	0.235	0.220	-0.054	19.9	3427
	(0.022)	(0.016)	(0.028)	(0.032)	(0.026)		
1955-57	1.066	0.246	0.216	0.204	-0.042	17.2	3444
	(0.020)	(0.015)	(0.024)	(0.028)	(0.020)		
1952-54	1.059	0.206	0.190	0.179	-0.027	12.8	3160
	(0.021)	(0.015)	(0.024)	(0.027)	(0.019)		

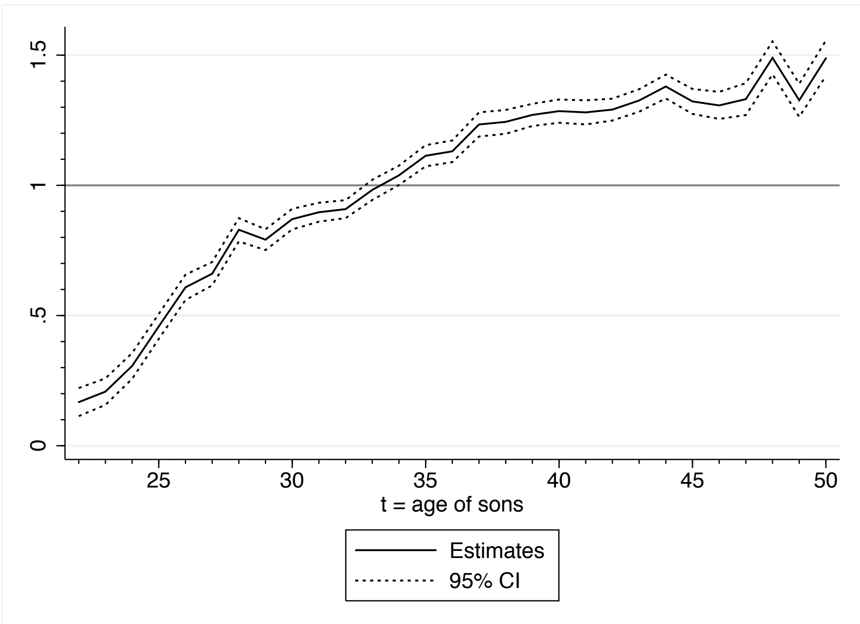
Notes: Left-side measurement error only. Table displays averages of estimates and standard errors (in parentheses) across ages 31-35. $\hat{b(t)}$ is significantly different from zero (p-value$<$0.05) at three ages (out of five) for 1958-60, at four ages for 1955-57, and at two ages for 1952-54. For all cohort groups, lifetime income is restricted to be measured over identical ages: 22-47 for sons, and 36-65 for fathers. Column (7) displays $\hat{b(t)}$ in percent of our benchmark estimate $\hat{\beta}$ (as average over the age interval).

Figure 1: Illustrative Example of Log Annual Income Trajectories



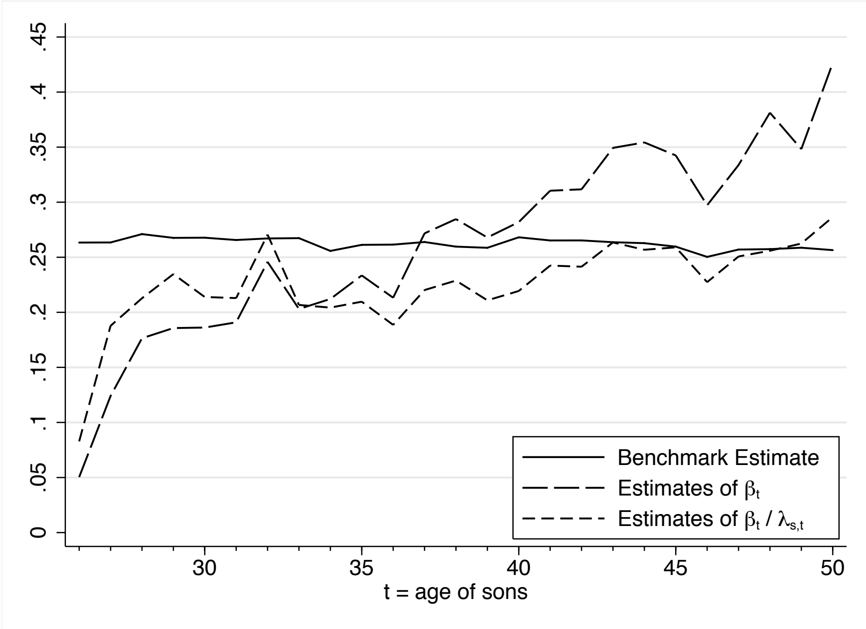
Notes: Illustrative Example. For each worker, the upward-sloping line depicts log annual income by age, the horizontal line depicts log annuitized lifetime income.

Figure 2: OLS Estimates of $\lambda_{s,t}$



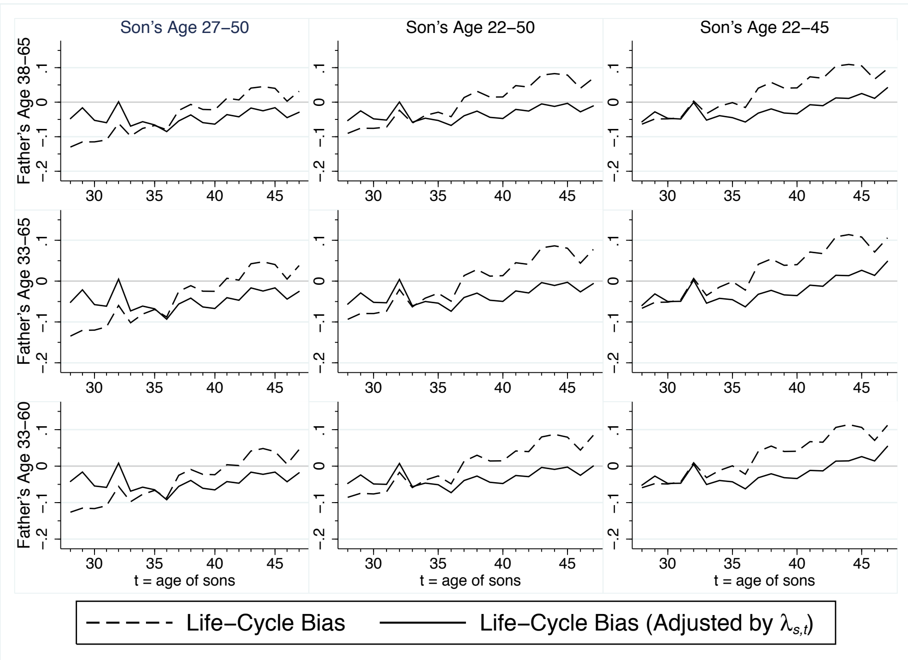
Notes: The figure shows estimates of $\lambda_{s,t}$ by sons' age for cohorts 1955-57. $\lambda_{s,t}$ is the regression coefficient in a regression of son's log annual income on son's log lifetime income, see eq. (3).

Figure 3: OLS Estimates of Elasticities and Life-Cycle Bias



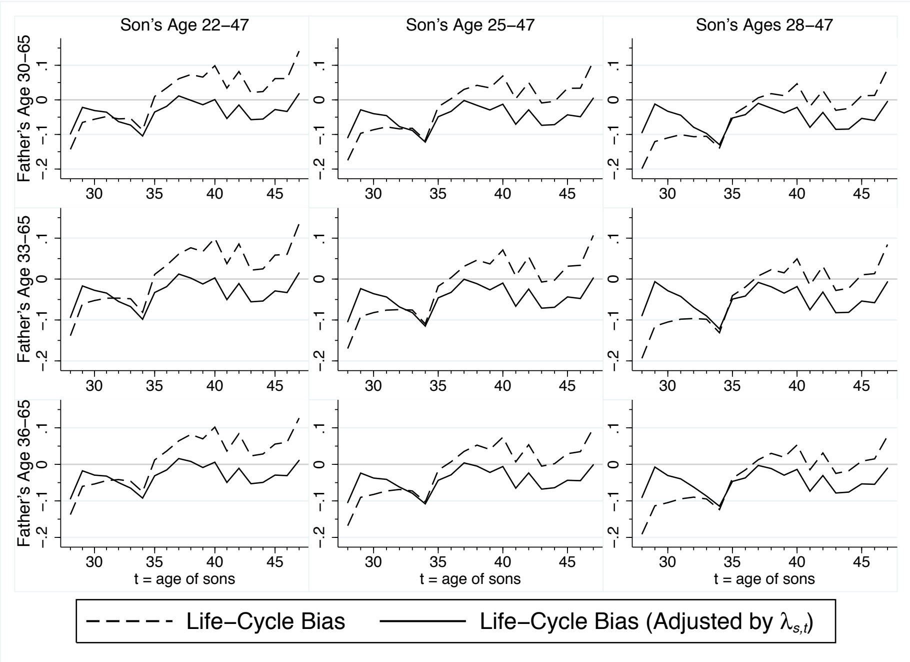
Notes: The figure shows the benchmark estimate of the intergenerational elasticity together with the unadjusted and adjusted (by the GEiV model) estimates based on sons' annual income. The estimates are for cohort 1955-57, left-side measurement error only.

Figure 4: Estimates of Life-Cycle Bias for Different Age Spans (Cohort 1955-57)



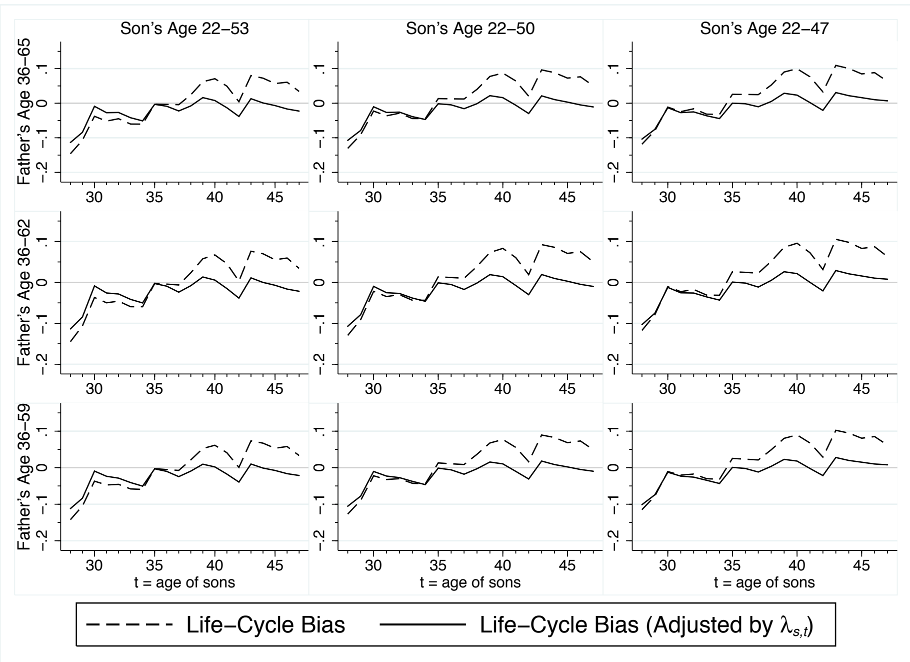
Notes: Left-side measurement error only. The age span of observed incomes of sons (fathers) varies along the horizontal (vertical) dimension.

Figure 5: Estimates of Life-Cycle Bias for Different Age Spans (Cohort 1958-60)



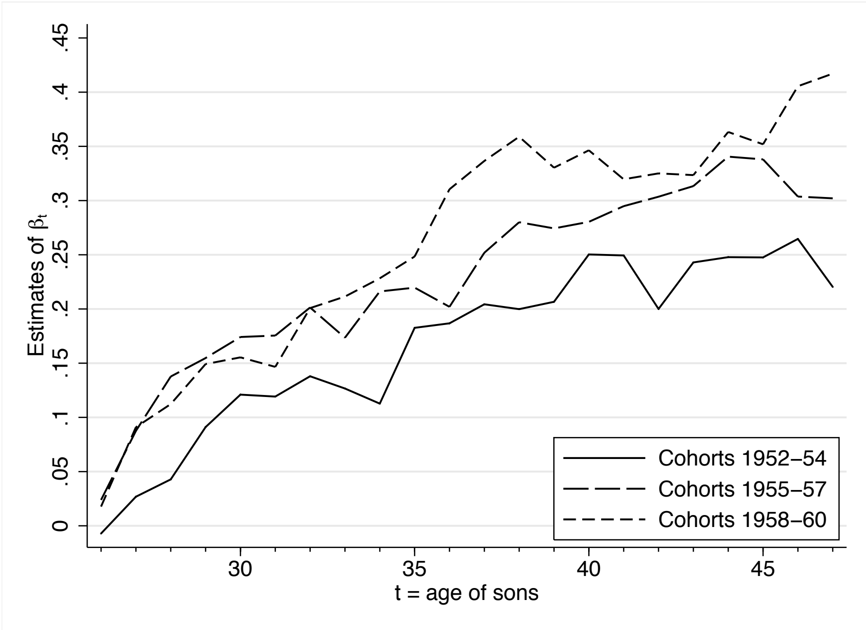
Notes: Left-side measurement error only. The age span of observed incomes of sons (fathers) varies along the horizontal (vertical) dimension.

Figure 6: Estimates of Life-Cycle Bias for Different Age Spans (Cohort 1952-54)



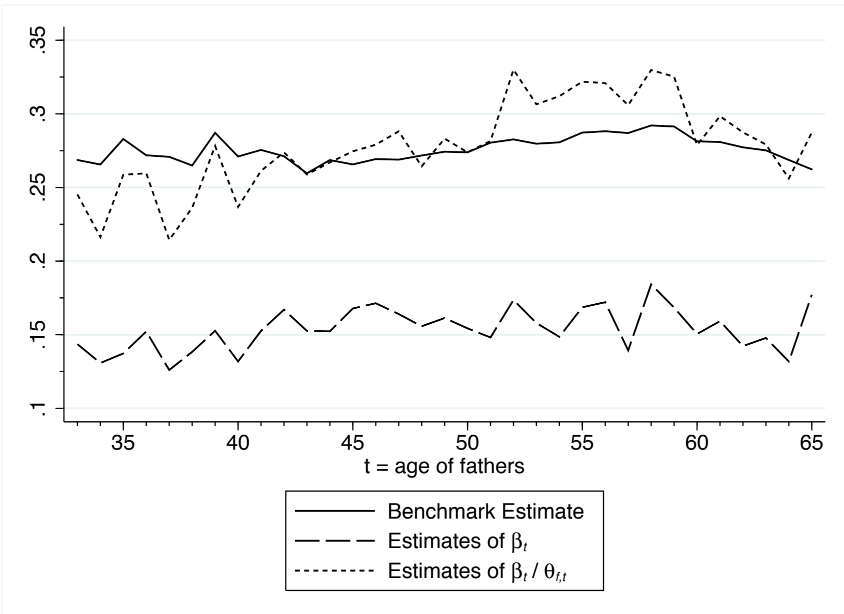
Notes: Left-side measurement error only. The age span of observed incomes of sons (fathers) varies along the horizontal (vertical) dimension.

Figure 7: OLS Estimates of Elasticities for Various Cohorts



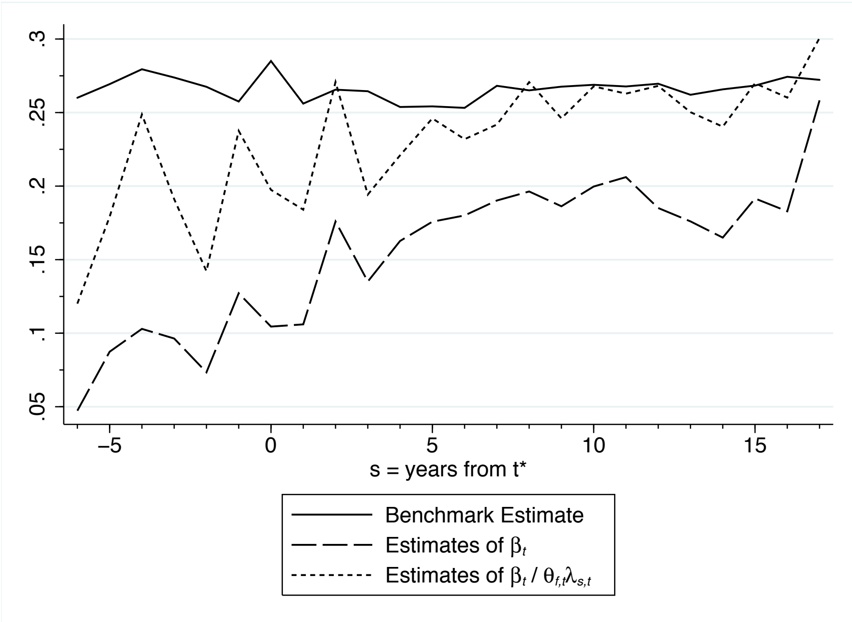
Notes: Cohort 1955-57, left-side measurement error only.

Figure 8: OLS Estimates of Elasticities with Right-Side Measurement Error



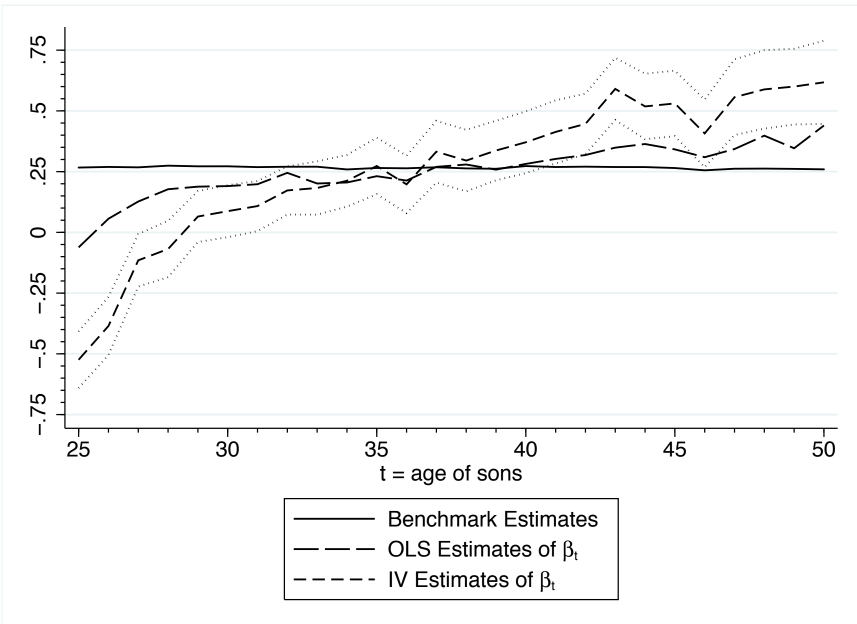
Notes: Cohort 1955-57, right-side measurement error only.

Figure 9: OLS Estimates of Elasticities with Both-Side Measurement Error



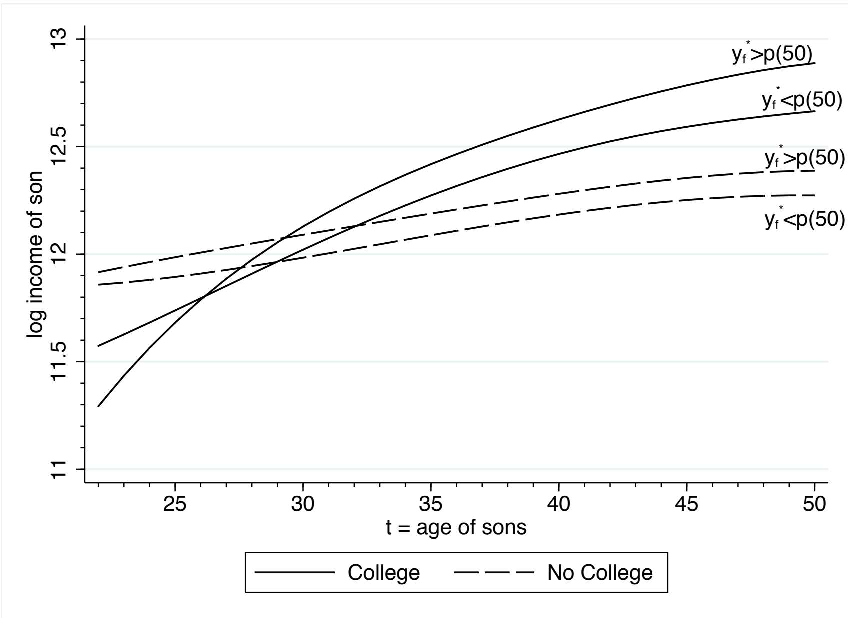
Notes: Cohort 1955-57, measurement error on both sides. To keep the analysis in two dimensions, we only display results for annual incomes at the same distance from t* for sons and fathers. At s=0 both are measured at their respective t*, at s=5 both are measured five years after t*, etc.

Figure 10: IV Estimates Compared with OLS and Benchmark



Notes: Cohort 1955-57, left-side measurement error only.

Figure 11: Life-Cycle Patterns in Income Across Subgroups



Notes: The trajectories depict average growth in log income over the life cycle for sons born in 1955-57, separately for sons with fathers above and below median lifetime income.

7 Appendix

A.1 Annual and Lifetime Values Over the Life Cycle

As in Haider and Solon (2006), suppose that log annual income of worker i at age t is given by

$$y_{it} = \eta_i + \gamma_i t \tag{7}$$

For simplicity assume infinite lifetimes and a constant real interest rate $r > \gamma_i$.

Proposition. *(i) For all age t, the difference between log annual income y_{it} and the log of the annuitized value of the present discounted value of lifetime income varies with respect to the individual's income growth rate γ_i. (ii) For any given age t, the difference will be equal for at most two different realizations of γ_i.*

Proof. The annuitized value of the present discounted value of lifetime income, denoted B_i, is

$$\sum_{s=0}^{\infty} exp(\eta_i + \gamma_i s)(1+r)^{-s} = \sum_{s=0}^{\infty} B_i (1+r)^{-s} = \frac{1+r}{r} B_i$$

Hence the log of the annuitized value equals

$$\begin{aligned} log\, B_i &= log\left(\frac{r}{1+r}\sum_{s=0}^{\infty} exp(\eta_i + \gamma_i s)(1+r)^{-s}\right) \\ &\cong log\, r + \eta_i - log(r - \gamma_i) \end{aligned}$$

The difference D_{it} between log annual income y_{it} and the log of the annuitized value of the present discounted value of lifetime income $log\, B_i$ is thus

$$D_{it} = \gamma_i t - log\, r + log(r - \gamma_i)$$

Depending on t, D_{it} decreases or increases in individuals' income growth rates γ_i,

$$\frac{\partial D_{it}}{\partial \gamma_i} = t - \frac{1}{r - \gamma_i}$$

The second derivative with respect to γ_i is negative,

$$\frac{\partial^2 D_{it}}{\partial^2 \gamma_i} = -(r - \gamma_i)^{-2} < 0$$

D_{it} is therefore a strictly concave function of γ_i conditional on t given, and a specific value of D_{it} can stem from at most two different values of γ_i. □

A.2 Life-Cycle Bias: Right-Side Measurement Error

Assume that we wish to estimate the regression model (1), but that log lifetime income of fathers $y^*_{f,i}$ is approximated by $y_{f,it}$, log annual income at age t. Sons' log lifetime income $y^*_{s,i}$

is observed. We express the linear projection of $y_{f,it}$ on $y^*_{f,i}$ as

$$y_{f,it} = \lambda_{f,t} y^*_{f,i} + u_{f,it}$$

The probability limit of the OLS estimator of a linear regression of $y^*_{s,i}$ on $y_{f,it}$ is then

$$plim\,\hat{\beta}_t = \frac{Cov(y_{f,it}, y^*_{s,i})}{Var(y_{f,it})} = \theta_{f,t}\beta + \theta_{f,t}\frac{Cov(u_{f,it}, y^*_{s,i})}{\lambda_{f,t} Var(y^*_{f,i})}$$

where $\theta_t = \lambda_{f,t} Var(y^*_{f,i}) / \left(\lambda^2_{f,t} Var(y^*_{f,i}) + Var(u_{f,it})\right)$ is the slope coefficient in the reverse regression of $y^*_{f,i}$ on $y_{f,it}$. This "reliability ratio" reduces to the familiar attenuation bias if $y_{f,it}$ is measured at age t^* such that $\lambda_{f,t} = 1$. The GEiV model is based on the assumption that $u_{f,it}$ is uncorrelated to $y^*_{s,i}$. It can thus account for the reliability ratio, but not for the remaining life-cycle bias that stems from correlation in the shape of income profiles within families.

A.3 Life-Cycle Bias: Left- and Right-Side Measurement Error

Assume that we wish to estimate the regression model (1), but that log lifetime incomes of fathers $y^*_{f,i}$ and sons $y^*_{f,i}$ are not observed and thus approximated by $y_{f,it}$ and $y_{s,it}$, log annual incomes at age t.[41] We express the linear projection of $y_{f,it}$ on $y^*_{f,i}$ as

$$y_{f,it} = \lambda_{f,t} y^*_{f,i} + u_{f,it}$$

and the linear projection of $y_{s,it}$ on $y^*_{s,i}$ as

$$y_{s,it} = \lambda_{s,t} y^*_{s,i} + u_{s,it}$$

The probability limit of the OLS estimator of a linear regression of $y_{s,it}$ on $y_{f,it}$ is then

$$\begin{aligned} plim\,\hat{\beta}_t &= \frac{Cov(y_{s,it}, y_{f,it})}{Var(y_{f,it})} \\ &= \frac{\beta\lambda_{s,t}\lambda_{f,t} Var(y^*_{f,i}) + \lambda_{f,t} Cov(u_{s,it}, y^*_{f,i}) + \lambda_{s,t} Cov(y^*_{s,i}, u_{f,it}) + Cov(u_{s,it}, u_{f,it})}{\lambda^2_{f,t} Var(y^*_{f,i}) + Var(u_{f,it})} \end{aligned}$$

If incomes are measured at ages such that $\lambda_{s,t} = \lambda_{f,t} = 1$ the probability limit reduces to

$$plim\,\hat{\beta}_t = \frac{\beta Var(y^*_{f,i}) + Cov(u_{s,it}, y^*_{f,i}) + Cov(y^*_{s,i}, u_{f,it}) + Cov(u_{s,it}, u_{f,it})}{Var(y^*_{f,i}) + Var(u_{f,it})}$$

an expression akin (except for the subscript t) to the general eq. (2).

[41] Note that for notational simplicity we here do not distinguish the age subscripts for fathers and sons.

References

Aaronson, D., and B. Mazumder (2008): "Intergenerational Economic Mobility in the United States, 1940 to 2000," *Journal of Human Resources*, 43(1).

Atkinson, A. (1980): "On Intergenerational Income Mobility in Britain," *Journal of Post Keynesian Economics*, 3(2), 194–218.

Becker, G., and N. Tomes (1979): "An Equilibrium Theory of the Distribution of Income and Intergenerational Mobility," *The Journal of Political Economy*, 87(6), 1153–1189.

——— (1986): "Human Capital and the Rise and Fall of Families," *Journal of Labor Economics*, 4(3), 1–39.

Bhuller, M., M. Mogstad, and K. G. Salvanes (2011): "Life-Cycle Bias and the Returns to Schooling in Current and Lifetime Earnings," IZA Discussion Papers 5788, Institute for the Study of Labor (IZA).

Björklund, A. (1993): "A Comparison between Actual Distributions of Annual and Lifetime Income: Sweden 1951-89," *Review of Income and Wealth*, 39(4), 377–386.

Björklund, A., and M. Jäntti (1997): "Intergenerational Income Mobility in Sweden Compared to the United States," *American Economic Review*, 87(5), 1009–18.

Björklund, A., and M. Jäntti (2009): "Intergenerational Income Mobility and the Role of Family Background," in *Oxford Handbook of Economic Inequality*, ed. by B. N. Salverda Wiemer, and T. M. Smeeding. Oxford University Press.

Björklund, A., M. Jäntti, and M. J. Lindquist (2009): "Family Background and Income During the Rise of the Welfare State: Brother Correlations in Income for Swedish Men born 1932-1968," *Journal of Public Economics*, 93(5-6), 671 – 680.

Björklund, A., M. Lindahl, and E. Plug (2006): "The Origins of Intergenerational Associations: Lessons from Swedish Adoption Data," *Quarterly Journal of Economics*, 121(3), 999–1029.

Black, S. E., and P. Devereux (2011): "Recent Developments in Intergenerational Mobility," in *Handbook of Labor Economics*, ed. by O. Ashenfelter, and D. Card, vol. 4A. Elsevier.

Böhlmark, A., and M. Lindquist (2006): "Life-Cycle Variations in the Association between Current and Lifetime Income: Replication and Extension for Sweden," *Journal of Labor Economics*, 24(4), 879–896.

Corak, M., and P. Piraino (2010): "The Intergenerational Transmission of Employers," *Journal of Labor Economics*, 29(1), 37–68.

Couch, K., and D. Lillard (1998): "Sample Selection Rules and the Intergenerational Correlation of Earnings," *Labour Economics*, 5(3), 313–329.

Dahl, M., and T. DeLeire (2008): "The Association Between Children's Earnings And Fathers' Lifetime Earnings: Estimates Using Administrative Data," Discussion Paper No. 1342-08, Institute for Research on Poverty, University of Wisconsin-Madison.

Gouskova, E., N. Chiteji, and F. Stafford (2010): "Estimating the Intergenerational Persistence of Lifetime Earnings with Life Course Matching: Evidence from the PSID," *Labour Economics*, 17(3), 592–597.

Grawe, N. D. (2006): "Lifecycle Bias in Estimates of Intergenerational Earnings Persistence," *Labour Economics*, 13(5), 551–570.

Haider, S., and G. Solon (2006): "Life-Cycle Variation in the Association between Current and Lifetime Earnings," *American Economic Review*, 96(4), 1308–1320.

Hertz, T. (2007): "Trends in the Intergenerational Elasticity of Family Income in the United States," *Industrial Relations*, 46(1), 22–50.

Jenkins, S. (1987): "Snapshots versus Movies: 'Lifecycle biases' and the Estimation of Intergenerational Earnings Inheritance," *European Economic Review*, 31(5), 1149–1158.

——— (2009): "Spaghetti Unravelled: A Model-Based Description of Differences in Income-Age Trajectories," ISER Working Paper Series 2009-30, Institute for Social and Economic Research, University of Essex.

Lee, C.-I., and G. Solon (2009): "Trends in Intergenerational Income Mobility," *The Review of Economics and Statistics*, 91(4), 766–772.

Lefranc, A., F. Ojima, and T. Yoshida (2012): "Intergenerational Earnings Mobility in Japan among Sons and Daughters: Levels and Trends," IZA Discussion Papers 7045, Institute for the Study of Labor (IZA).

Mayer, A. (2010): "The Evolution of Wages Over the Lifecycle: Insights from Intergenerational Connections," *Applied Economics*, 42(22), 2817–2833.

Mazumder, B. (2005): "Fortunate Sons: New Estimates of Intergenerational Mobility in the United States Using Social Security Earnings Data," *Review of Economics and Statistics*, 87(2), 235–255.

Meghir, C., and L. Pistaferri (2011): "Earnings, Consumption and Life Cycle Choices," in *Handbook of Labor Economics*, ed. by O. Ashenfelter, and D. Card, vol. 4B. Elsevier.

Mocetti, S. (2007): "Intergenerational Earnings Mobility in Italy," *The B.E. Journal of Economic Analysis & Policy*, 7(2), Article 5.

Nicoletti, C., and J. Ermisch (2007): "Intergenerational Earnings Mobility: Changes across Cohorts in Britain," *The B.E. Journal of Economic Analysis & Policy*, 7(2), Article 9.

Nilsen, Ø., K. Vaage, A. Aakvik, and K. Jacobsen (2012): "Intergenerational Earnings Mobility Revisited: Estimates Based on Lifetime Earnings," *Scandinavian Journal of Economics*, 114(1), 1–23.

Nybom, M., and J. L. Stuhler (2011): "Heterogeneous Income Profiles and Life-Cycle Bias in Intergenerational Mobility Estimation," IZA Discussion Papers 5697, Institute for the Study of Labor (IZA).

Pekkala, S., and R. E. B. Lucas (2007): "Differences across Cohorts in Finnish Intergenerational Income Mobility," *Industrial Relations: A Journal of Economy and Society*, 46(1), 81–111.

Piraino, P. (2007): "Comparable Estimates of Intergenerational Income Mobility in Italy," *The B.E. Journal of Economic Analysis & Policy*, 7(2), Article 1.

Raaum, O., B. Bratsberg, K. Røed, E. Österbacka, T. Eriksson, M. Jäntti, and R. Naylor (2007): "Marital Sorting, Household Labor Supply, and Intergenerational Earnings Mobility across Countries," *The B.E. Journal of Economic Analysis & Policy*, 7(2), Article 7.

Solon, G. (1992): "Intergenerational Income Mobility in the United States," *American Economic Review*, 82(3), 393–408.

Solon, G. (1999): "Intergenerational Mobility in the Labor Market," in *Handbook of Labor Economics*, ed. by O. Ashenfelter, and D. Card, vol. 3A, chap. 29, pp. 1761–1800. Elsevier.

Vogel, T. (2006): "Reassessing Intergenerational Mobility in Germany and the United States: The Impact of Differences in Lifecycle Earnings Patterns," SFB 649 Discussion Papers 2006-055, Sonderforschungsbereich 649, Humboldt University, Berlin, Germany.

Zimmerman, D. J. (1992): "Regression toward Mediocrity in Economic Stature," *American Economic Review*, 82(3), 409–29.