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Intergenerational Earnings Mobility Revisited: Estimates Based on Lifetime Earnings*

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Abstract

Using Norwegian intergenerational data, which include a substantial part of the life-cycle earnings for children and almost the entire life-cycle earnings for their fathers, we present new estimates of intergenerational mobility. Extending the length of fathers' earnings window from 5 to 25 years increases estimated elasticities. Increasing the age at which fathers' earnings are observed has the opposite effect. Biases in the estimated elasticities are related to both transitory earnings variation and life-cycle measurement error; the former appear to be more important than the latter. Estimation bias stemming from persistence in transitory innovations plays only a minor role. Our findings indicate that intergenerational earnings mobility in Norway might have been strongly overstated in many earlier studies with shorter earnings histories. Some of our new estimates are twice as large as earlier estimates.

Keywords: Intergenerational mobility; measurement error

JEL classification: *J*62; *C*23

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I. Introduction

Equalization of opportunities independent of social background is a widely accepted goal in modern societies. Measures of intergenerational earnings mobility are informative for the evaluation of the extent to which societies are successful in equalizing opportunities, because low intergenerational earnings mobility implies that a person's position in the earnings distribution is highly dependent on their parents' position, while high mobility implies the opposite.

The natural measure of economic success is lifetime earnings. Reliable data on lifetime earnings are, however, difficult to obtain. Instead, researchers have used earnings data for shorter periods as proxies. The resulting measurement errors give rise to several forms of bias, which, as might be suspected, is one reason why the differences in the estimates of intergenerational earnings mobility are remarkably high, across both countries and time. First, transitory earnings shocks have a distorting effect on the permanent income measure. Through the averaging of yearly observations, typically five years or less, this has become internalized in the literature (e.g., Solon, 1992; Zimmerman, 1992). Second, Mazumder (2005) claims that five years is a far too small a window, given the persistence characterizing transitory shocks. For this reason, earlier estimates display a downward bias from the remaining measurement errors.[1] Third, the variance of the transitory innovation might change over the life cycle, typically with the smallest values around prime age and considerably higher before and – particularly – after this period in life. Hence, not only the length of the earnings interval but also the fathers' age at the time of observation determines the bias (e.g., Mazumder, 2001; Baker and Solon, 2003).

Several recent papers, notably Haider and Solon (2006), Grawe (2006), and Böhlmark and Lindquist (2006), discuss another source of measurement error with implications for the estimation of intergenerational mobility.[2] They argue that life-cycle variation in the association between current and permanent earnings represents a form of error where the classical errors-in-variables model is misspecified. The life-cycle bias adds to the standard errors-in-variables bias when the current earnings level is used as a proxy for the earnings of fathers as well as their children's lifetime earnings, with the inconsistency varying across the age of fathers and/or children. Controlling for multiyear averages of current income cannot eliminate this measurement error.

[1] Based on nationally representative social security data, Mazumder (2005) presents intergenerational elasticities for the US that are about 50 percent higher than previous estimates (approximately 0.6 instead of 0.4).

[2] Earlier contributions on the same topic, although employing somewhat different frameworks, are Jenkins (1987) and Björklund (1993).

The first purpose of our paper is to estimate intergenerational earnings mobility using high-quality earnings data for very large parts of the life cycle (in some cases, up to 25 years). The second purpose is to use these estimates as reference points to evaluate the magnitudes of the biases that arise if we use earnings data for shorter periods. The paper then lies at the intersection of two types of measurement problem: biases stemming from transitory earnings shocks, including the possibility of persistence and age-varying volatility, and life-cycle biases. We explore the effect of the persistence in the earnings shocks and its importance for the magnitude of the bias by expanding the earnings window of fathers, similarly to the analysis of US data by Mazumder (2005). At the same time, we incorporate our empirical findings regarding life-cycle bias, in that we attempt to measure children's and fathers' earnings at ages where the life-cycle bias is supposed to be of minor importance. Our procedure also allows for an evaluation of the effect of the transitory earnings variation that also changes with age.

In the existing empirical body of literature on intergenerational mobility, there is a substantial variation in the length of the fathers' earnings window as well as the age of the father at observation. This makes it difficult to compare studies within and/or between countries. We believe that our results regarding the effects of different measurement problems make it easier to undertake a comparison of existing empirical findings in the literature.

The data employed in this study have several advantages. First, they provide us with a very long earnings series. Second, the data sources are administrative registers (e.g., the public tax register), thereby reducing the problems of self-reporting errors, attrition, etc. Third, they are census data and therefore they are highly representative and provide a large number of observations. Fourth, the data do not suffer from the truncation problems present in the data of Mazumder (2005).[3] Finally, unlike most other studies in this field, the data include information about female earnings.

By measuring fathers' earnings at various ages to see the effects of transitory earnings variation and life-cycle location, we find that the intergenerational elasticities fall 1.1 percentage points for men and 0.7 percentage points for women if the income of the father is measured one year later. This appears to be primarily because of transitory earnings variation rather than the life-cycle effects. Extending the length of the fathers' earnings window to see the effect of persistence bias, the intergenerational

[3] The data in Mazumder (2005) are partly imputed (because of top coding), and the number of observations is relatively low. Mazumder (2005) suggests that future research should attempt to verify his findings using long-term measures of permanent earnings from other sources.

elasticities increase 0.5 and 0.3 percentage points for sons and daughters, respectively, for each additional year the fathers' earnings are averaged. Our intergenerational elasticities estimates are much higher, sometimes twice as much, compared with those reported in recent studies for Norway (see Table A1 for references and a comparison). Part of this difference is because the respective studies use different lengths of the fathers' earnings window. It is, however, likely that differences in the age of the father at observation is the more important source of bias. Nevertheless, after carefully correcting for these measurement issues, Norway is still a country characterized by high intergenerational earnings mobility, especially when compared with the US and the UK.

The remainder of the paper is structured as follows. In Section II we describe the various sources of measurement error and their subsequent biases. In Section III we present the data used in the analysis. In Section IV we discuss the empirical results, while we give our concluding remarks in Section V.

II. Sources of Measurement Bias

The standard approach to the measurement of intergenerational mobility is to regress children's earning on their parents' earnings:

$$y_{1i} = \rho y_{0i} + \varepsilon_i. \tag{1}$$

Here, subscripts 1 and 0 indicate the child and parent, respectively, y is a measure of 'lifetime' or 'permanent' income in logs, ρ is the slope coefficient, and ε is the random error term.[4] In addition, quadratic functions of the ages of both generations are commonly included. The estimated coefficient ρ measures the intergenerational earnings elasticity (IGE) between parents and children. The closer ρ is to zero, the higher the intergenerational mobility.

In spite of its striking simplicity, IGE estimates have undergone considerable adjustment during the past few decades, largely because of measurement issues. While Becker and Tomes (1986) base their rather optimistic view of intergenerational mobility on a ρ of just 0.2 or lower, Solon (1992) and Zimmerman (1992) both conclude that the IGE for men in the US is twice as high: 0.4 or even slightly higher. One interpretation of this remarkable discrepancy is that it is because of a classical measurement error. We assume that the parents' earnings in a given year t, y_{0it}, consist of a permanent component, y_{0i}, and a transitory component, w_{0it}:

$$y_{0it} = y_{0i} + w_{0it}. \tag{2}$$

[4] All variables are expressed as deviations from their population mean to suppress the intercept.

If, as in the estimates surveyed in Becker and Tomes (1986), the IGE is based on single-year observations of parental earnings as proxies for permanent earnings, $\hat{\rho}$ will be downward biased by a factor

$$\phi = \frac{Var(y_{0i})}{Var(y_{0i}) + Var(w_{0it})}.$$

Solon (1992) and Zimmerman (1992) apply averages of up to five years of single-year earnings as their proxies. Averaging over T years implies that the inconsistency (attenuation) factor becomes

$$\phi_{\mathrm{T}} = \frac{Var(y_{0i})}{Var(y_{0i}) + [Var(w_{0it})/T]}.$$

Clearly, noise (in the form of the transitory earnings shocks) is higher relative to the signal in the first case compared with the second case, leaving the former estimates highly downward biased.

However, the above attenuation factor rests on the unrealistic assumption of the absence of persistence in the transitory shocks. Mazumder (2005) follows Solon (1992) and introduces persistence in the transitory fluctuations in the form of a first-order autoregressive process.[5] However, even with this relatively simple earnings dynamics, the bias becomes rather complicated. For instance, Mazumder (2005) performs simulations that demonstrate that even with a moderate degree of persistence (an autocorrelation coefficient of 0.5), the attenuation factor becomes 0.69 when using a five-year average, compared with 0.83 in the absence of autocorrelation (see Mazumder, 2005, Table 1, p. 238). This implies that the estimates of Solon (1992) and Zimmerman (1992) of a ρ of 0.4 might be 30 percent downward-biased estimates of a true IGE of 0.6. Furthermore, Mazumder (2005) illustrates that under these earnings assumptions, we require averaging over more than 25 years to obtain a reasonable value (i.e., close to 1) for the attenuation factor (see Mazumder, 2005, Figure 1, p. 239).

Another source of measurement error with implications for the estimation of intergenerational mobility reflects the life-cycle variations in the association between current earnings and permanent earnings for children and their fathers.[6] We might model this association as $y_{1i\tau} = \lambda_{1\tau} y_{1i} + u_{1i\tau}$ and $y_{0it} = \lambda_{0t} y_{0i} + u_{0it}$, where τ and t are the ages of children and fathers,

[5] Solon (1992) illustrates first-order moving average and autoregression (Note 17, p. 237). Baker and Solon (2003) introduce non-stationary (random walk) and stationary components in their earnings dynamics models for Canada.

[6] The typical life-cycle profile of earnings is concave, and more so the higher the lifetime earnings, indicating a more rapid earnings growth through most of the life cycle for high-income earners relative to low-income earners. Thus, early in the career, the gap between high- and low-income workers is understated (and can even have the wrong sign), whereas it tends to become overstated later in the career.

respectively. These life-cycle variations represent a form of error where the classical errors-in-variables model is misspecified. First, in intergenerational earnings regressions, current instead of lifetime earnings for the children (i.e., the left-hand side variable) also yield biased OLS estimates. Assuming that we have an appropriate measure of parents' earnings (i.e., $\lambda_{0t} = 1$, but $y_{1i\tau}$ is used as a proxy for y_{1i}), the IGE estimates will be confounded by the children's own life-cycle variation:

$$y_{1i\tau} = \lambda_{1\tau}(\rho y_{0i} + \varepsilon_i) + u_{1i\tau}. \tag{3}$$

The probability limit of the slope coefficient $\hat{\rho}$ then becomes $\rho \cdot \lambda_{1_\tau}$, implying that a necessary condition for the OLS estimate of ρ to be unbiased is that $\lambda_{1_\tau} = 1$.

Second, the life-cycle bias adds to the standard errors-in-variables bias when current earnings are used as a proxy for the fathers' lifetime earnings. Because we do not observe the permanent earnings of equation (1), and instead have to rely on proxies, we run the regression[7] $y_{1i\tau} = \tilde{\rho} y_{0it} + w_{it}$, where

$$\begin{aligned}\operatorname{plim} \tilde{\rho}_{t,\tau} &= \frac{E(y_{1i\tau} y_{0it})}{E(y_{0it})^2} = \frac{E[(\lambda_{1\tau} y_{1i} + u_{1i\tau})(\lambda_{0t} y_{0i} + u_{0i\tau})]}{E(\lambda_{0t} y_{0i} + u_{0i\tau})^2} \\ &= \frac{E[(\lambda_{1\tau}\rho y_{0i} + \lambda_{1\tau}\varepsilon_i + u_{1i\tau})(\lambda_{0t} y_{0i} + u_{0i\tau})]}{E(\lambda_{0t} y_{0i} + u_{0i\tau})^2} = \lambda_{0\tau}\theta_t\rho\end{aligned} \tag{4}$$

and

$$\theta_t = \frac{\lambda_{0t} Var(y_{0i})}{\lambda_{0t}^2 Var(y_{0i}) + Var(u_{0it})} = \frac{\lambda_{0t}}{\lambda_{0t}^2 + Var(u_{0it})/Var(y_{0i})}. \tag{5}$$

Here θ_t partly contains the classical attenuation bias ϕ_T stemming from the transitory component of the fathers' earnings. In addition, θ_t contains the life-cycle bias stemming from the permanent component, with the inconsistency varying across fathers' ages. The size and direction of the total bias (attenuation plus the life cycle) then becomes quite involved. In fact, as demonstrated by Haider and Solon (2006), it might change character from attenuation (negative) to amplification (positive).

Finally, the fathers' position in the life cycle might also influence the attenuation factor, ϕ_T. For example, Mazumder (2001) and Baker and Solon (2003) both argue that the variance of the transitory innovation, $Var(u_{0it})$, follows a U-shaped pattern over the life cycle, with the smallest values at around 45 years of age. Before, and particularly after, this period in life, the variance typically appears to be considerably higher.

We start with the intention of correcting the potential bias stemming from persistence in the transitory earnings shocks. To isolate this form

[7] We ignore here any time averaging and/or persistence of the type mentioned earlier.

of attenuation bias, we require some sort of control for the potential bias stemming from the life-cycle variation in the permanent earnings. For this task, our estimation procedure is as follows.[8] First, we estimate the periods of the children's and fathers' lives where $\lambda_{\tau,t}$ are closest to 1, following the procedure in Haider and Solon (2006). This implies running the regressions $y_{1i\tau} = \lambda_{1\tau} y_{1i} + u_{1i\tau}$ and $y_{0it} = \lambda_{0t} y_{0i} + u_{0it}$ for children and fathers, respectively. In our benchmark case, we condition on the period in life where our estimates of $\lambda_{\tau,t}$ for both generations are (close to) 1. We then interpret any remaining bias in the IGE as attenuation bias in the case of the fathers, stemming from either persistence or age-varying volatility in the transitory shocks. The inconsistency factor θ_t (also referred to as the reliability ratio and containing both transitory earnings variation and life-cycle effects) is estimated by running the reverse regression $y_{0i} = \theta_t y_{0it} + e_{0it}$. With estimates of λ_{0t} and θ_t in hand, it is then straightforward to calculate the ratio of transitory to permanent income variance from equation (5):

$$\frac{Var(u_{0it})}{Var(y_{0i})} = \frac{\lambda_{0t}(1-\theta_t\lambda_{0t})}{\theta_t}. \tag{6}$$

As in Mazumder (2005), our test procedure for investigating whether attenuation bias arises from persistence in the transitory earnings component implies successively extending the length of the period for which we observe the fathers' earnings. However, unlike Mazumder (2005), we explicitly pay attention to the possible confounding transitory earnings variation and/or life-cycle effect.

III. Dataset and Variables

Our data are collected from different administrative datasets linked together by an individual identity code for the entire Norwegian working-age population. Our data contain the full series of yearly gross earnings from 1967 to 2005 based on mandatory tax reports, in addition to family characteristics and birth year. The earnings series were originally collected for calculating old-age pensions. This implies that they include earnings but exclude interest, capital income, etc. Unemployment benefits, disability benefits, and sick pay are included, but means-tested benefits are not.[9]

We first adjust all of the income variables to real 1995 income, using the consumer price index. We then discount the fathers' and offspring's incomes to the year they were 25 years of age, using a discount factor of

[8] See section 4 in Grawe (2006) for a discussion of alternative procedures.

[9] This means that the fraction of 'true' zero earners in our data is very low and that individuals in the mandatory tax register appear to be registered with zero earnings simply because of some data anomalies.

2 percent.[10] Importantly, unlike the data in Mazumder (2005) and Haider and Solon (2006), the earnings variables are uncensored, at both the top and the bottom of the distribution. This quality, together with the large number of individuals, allows us to use simpler and better estimation methods, which potentially improves the precision of the estimates.

We include children of both sexes from the 1959–1962 birth cohorts. We could have included earlier cohorts. However, this might have confounded our analysis with trends in mobility across time.[11] The chosen cohorts also limit the sample to children whose compulsory schooling is at least nine years long.[12] Later cohorts of children are not included, as we want to follow the individuals at least until the age of 40. In addition, the trend argument also applies for the inclusion of younger cohorts. We also exclude individuals born to parents younger than 16 years or older than 40 years.

We also limited our sample to fathers born between 1927 and 1942. This means that we can observe the earnings of all fathers from when they were at least 40 years old to when they were 60 years of age. This is a key period if we wish to study the effect of the life-cycle bias. Admittedly, this limitation results in a sample of fathers that had become parents somewhat earlier than the overall Norwegian population.[13]

Finally, we excluded individuals born outside Norway and non-Norwegian citizens because of the relatively high frequency of missing information about earnings.

For both fathers and children, earnings are in logs, with the averages over log earnings. Because they were born in the period 1959–1962, the sons and daughters in our sample were between 43 and 46 years of age when the earnings series ended in 2005. We use the fathers' earnings as the only indicator of the family's earnings capacity.[14] For both children and their fathers, the five-year averages are based on at least three

[10] We have also tried with an alternative discount factor of 4 percent; this did not affect our results in measurable ways.

[11] Bratberg *et al.* (2005) report a slight upward trend in mobility when comparing the 1950 cohort with the 1960 cohort.

[12] We thereby avoid the possible confounding effect from a major reform of compulsory schooling that affected the children and not the fathers in our sample. The increase of compulsory schooling from seven to nine years took place during the 1960s and early 1970s, with 1974 as the final year for the minimum of seven years of schooling. See Aakvik *et al.* (2010) for details and analysis of the effect of the compulsory schooling reform on earnings. Members of the 1959 birth cohort ended their compulsory schooling in 1975. Thus, all of the children in our sample had at least nine years of schooling, while all of the fathers had at least seven years.

[13] The average age of fathers at a child's birth was approximately 32 years around 1960, while it is 28 years in our sample (based on the sample presented in Table 1, row 1).

[14] Using fathers' earnings as a proxy for household earnings is not too unrealistic, as fathers were typically the breadwinners for the families included in the cohorts analyzed in this study, while mothers commonly undertook home duties.

years of positive earnings (i.e., individuals with two years or less of earnings are excluded).[15] When we extend the size of the window from 5- to 25-year averages, the corresponding requirements of strictly positive earnings are 5, 8, 10, and 13 years. In our regressions, we age-adjust the fathers' earnings by including fathers' ages and fathers' ages squared. In addition, we control for any potential cohort effects by including cohort dummies for the children.[16]

Finally, observing the individuals at different points in time and for different time spans implies that the compositions of the samples will differ. For instance, when we move the year used as the focal point for fathers' earnings, some individuals drop out because of too few observations of positive earnings in the relevant period. For the same reason, the samples that we observe for, say, 15 years need not be identical to a sample based on only five years of observations. To avoid the influence of sample composition, we balance the samples, meaning they are fixed within each table.

IV. Results

We start by presenting our estimates of the life-cycle bias for sons and daughters in Figure 1. For sons, we can see that λ_τ starts below 1, equals 1 at age 30, and stabilizes at approximately 1.1 from then on. This appears to accord with the values reported by Haider and Solon (2006) in the US and by Böhlmark and Lindquist (2006) for Swedish men born between 1929 and 1933. Accordingly, there is a negative bias if we use annual earnings from early in the career as a proxy for lifetime earnings. Somewhere in the men's thirties, the bias becomes positive but small and remains small as long as we are able to follow the sons (until they are aged 46). Turning to λ_τ for daughters, we find that growth in the estimates is much steeper than for men, and that we reach a maximum at an earlier stage of the life cycle. Böhlmark and Lindquist (2006) also report an inverted U-shape for Swedish women, and they relate this finding to increased earnings and income heterogeneity in the childrearing period. Thus, using current earnings as a proxy for lifetime earnings might be more problematic for

[15] Note again that our analysis draws on information from mandatory tax reports. In addition to earned income, the earnings measure includes unemployment benefits, disability benefits, and sickness pay. This means that the proportion of 'true' zero earners in our data is small, between 2 and 6 percent in a given year. Our experience working with these register data is that individuals registered with zero earnings are mainly because of some data anomaly (see Bratberg *et al*., 2005, 2007, 2008).

[16] Children's ages and ages squared are normally included in the regressions. However, as all offspring's earnings are measured at the same (average) age, this was not necessary in our analysis.

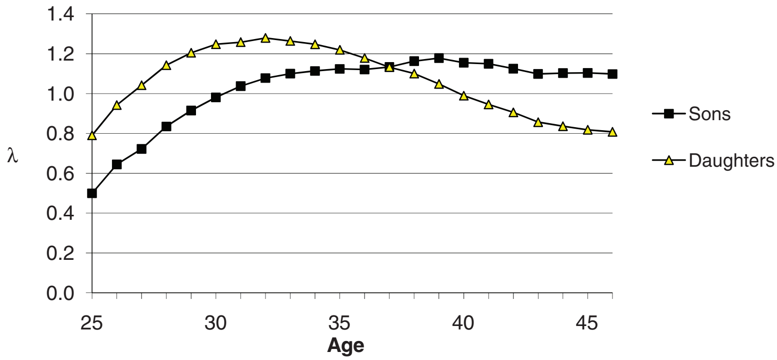

Fig. 1. Estimates of λ_τ for sons and daughters born 1959–1962

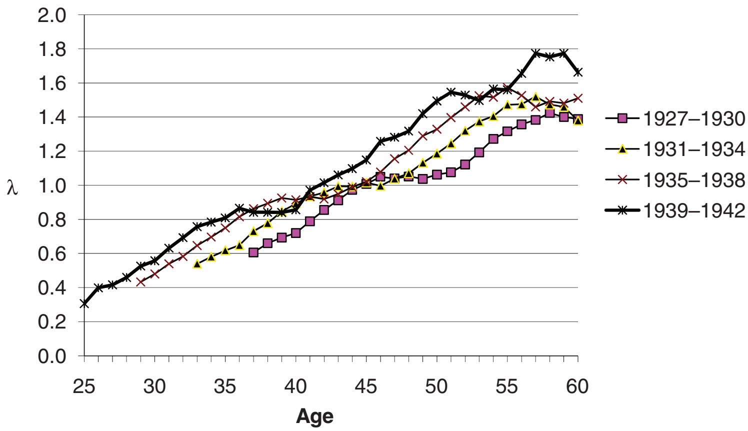

Fig. 2. Estimates of λ_t for fathers

women than for men, but the late thirties and early forties appears to be a relatively stable age. Drawing on our own estimates and through comparison with other studies, we proceed to use earnings at age 36–40 to minimize the influence of life-cycle bias for sons and daughters.

Figure 2 details the estimates of λ_t for the different cohorts of fathers. The 1942 birth cohort is 25 years of age in 1967 and 60 years in 2002; hence, the observations span 35 years. Obviously, we observe the older cohorts later in life and for shorter periods. For the oldest birth cohort, 1927, we only have 20 observations from 40 to 60 years (from 1967 to 1987). Clearly, the estimates of λ_t will be influenced by the time in the life cycle at which the individuals are observed and by the length of the observation period. Starting with the youngest cohorts with the most complete life cycle (1927–1930), Figure 2 indicates that λ_t increases when they have passed their mid-thirties, then it stabilizes for a period, close to but slightly below 1. The patterns of the later cohorts resemble those of the first group, although λ_t flattens out slightly later and is slightly closer to 1.[17] Our estimates are then quite similar to earlier estimates from the US, Sweden, and Germany (see, for instance, Figure 3 in Brenner, 2010). One exception is that after an intermediate period of stability, λ_t continues to increase. The increase in the later part of the life cycle is what we would expect if those with low education have a flat income path and those with high education have continuously rising income and higher lifetime income.[18] Why this process seems to pause for a while in the late thirties and early forties, however, is difficult to explain. Nevertheless, it does show the fathers' earnings period when the life-cycle bias appears lowest.

As discussed in Section II, θ is a compound measure stemming from the life-cycle bias and the bias associated with the transitory shocks. The latter might arise because of persistence along with age variation in the variance. Figure 3 plots the estimated θ_t across the fathers' ages. Accordingly, it contains the sum of the potential life-cycle bias and the bias from the age variation in transitory earnings. As is the case for the estimates of λ_t, θ_t also varies across the cohorts and the length of observation. The main finding is that *within* our (groups of) cohorts, θ_t decreases with age, while *between* (groups of) cohorts, the oldest are those with higher θ_t and, hence, lower measurement biases.

Figure 4 allows us to compare the two different sources of age variation in θ_t. We focus on the estimates from the cohorts with the longer observation period (1939–1942) and we use equation (6) to calculate the variance ratio. Note that the variance of the permanent component, $Var(y_{0i})$, is stable across age, so we can attribute any change in the ratio to the variance of the transitory component, $Var(u_{0it})$. As in Baker and Solon (2003) and Mazumder (2005), we find that the variance increases quite steeply after the fathers have entered their forties. However, our estimates deviate from these other studies, along with Brenner (2010), in that we find a relatively

[17] There could be several reasons for the differences between cohorts: different length of series of annual earnings, annual earnings taken from different parts of the life cycle, and changes in institutional settings over time. After considerable investigation, we conclude that the alleged differences between cohorts are most likely caused by the fact that earnings are measured at different stages of the life cycle – and only part of the lifetime earnings are included in our lifetime earnings measure – and not because of differences between cohorts per se.

[18] See the model described by Haider and Solon (2006) and their equations (2)–(5).

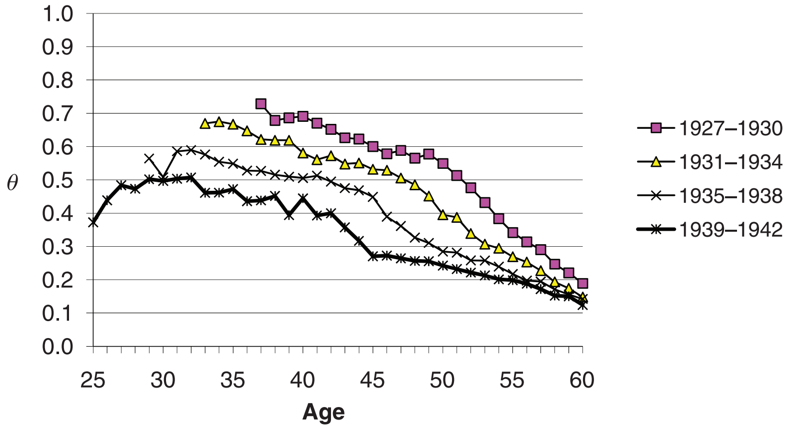

Fig. 3. Estimates of θ_t for fathers

stable variance up to this same age.[19,20] Roughly speaking, our estimates of θ_t fall between 0.5 and 0.4 during the thirties, then fall monotonically from age 40 onward. The decrease in θ_t is then consistent with a rise in λ_t and/or the variance ratio. However, at first impression, the pattern exhibited in the θ_t graph appears to owe more to the pattern in the variance ratio than λ_t. This is particularly so for the late thirties, when our λ_t estimates are remarkably stable, while θ_t varies inversely with the variance of the transitory shock.

Armed with these estimates of the life-cycle bias for offspring and the 'reliability ratio' for fathers, we move on to the main task of the paper: identifying and comparing different sources of biases in the estimation of the IGE.

First, we estimate the IGE where we average the fathers' earnings over a relatively short period (a maximum of five years). This allows comparison with other research. A major challenge, however, is to separate the attenuation bias, on the one hand, from the life-cycle bias on the other. For the latter, we can see from Figures 2 and 4 that our λ_t estimates for fathers flatten out in their mid-thirties and are close to 1 during the first

[19] See Figure A1, where we draw a corresponding picture of $Var(u_{0it})/Var(y_{0i})$ based on estimates of λ and θ by Haider and Solon (2006), Böhlmark and Lindquist (2006), and Brenner (2010).

[20] In fact, the variance increases throughout the entire life cycle, instead of following the U-shape reported by other studies.

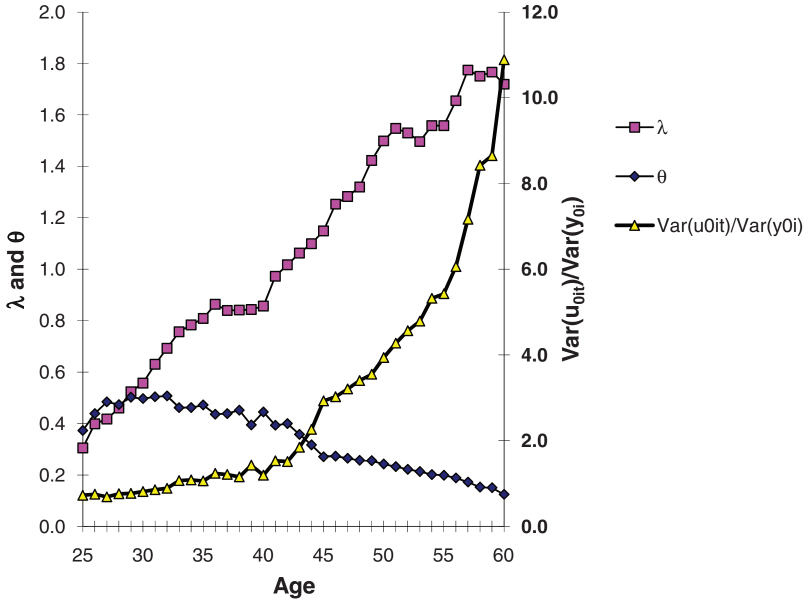

Fig. 4. λ_t, θ_t, and transitory to permanent income variance, $Var(u_{0it})/Var(y_{0i})$ for fathers born in 1939–1942

half of their forties.[21] This is not too far from the results of Böhlmark and Lindquist (2006), who find no significant life-cycle bias from using current income as a proxy for lifetime income, as long as current income is measured after the age of 33. As for the attenuation bias, this might stem from persistence in the transitory earnings fluctuations; alternatively, the variance of transitory earnings in the chosen five-year period might be exceptionally high. Our calculations show that the variance starts rising in the early forties; at earlier ages, it is rather stable, even with a slightly negative trend down to age 25. Taken together, this implies that for fathers, the age should be set to minimize the age-related and life-cycle bias, and the earnings series should be sufficiently long to deal with the persistence bias. Our benchmark case will be a five-year average for earnings between 1967 and 1971, during which time the fathers' average age is around 36. In the next step, we construct earnings measures where we average progressively up to 25 years (1967–1991).

[21] This generally applies for the majority of the cohorts. The exception is the oldest cohorts, observed later in life and for a shorter period, say, five years later in the life cycle, where λ is closer to 1.

Table 1. *Intergenerational earnings mobility estimates for sons and daughters; length of time span constant, fathers' age increasing (earnings from 1967–1971 to 1982–1986)*

	Elasticities of sons–fathers			Elasticities of daughters–fathers		
	Coeff.	Std err.	Fathers' average age	Coeff.	Std err.	Fathers' average age
Fathers' earnings 1967–1971	0.3383	0.0074	36.1	0.2300	0.0089	36.1
Fathers' earnings 1972–1976	0.2817	0.0062	41.1	0.1864	0.0076	41.1
Fathers' earnings 1977–1981	0.2534	0.0060	46.1	0.1640	0.0073	46.1
Fathers' earnings 1982–1986	0.1632	0.0043	51.1	0.1173	0.0052	51.0
Cohorts of fathers		1927–1942			1927–1942	
Number of observations		57,510			53,481	

Notes: The dependent variable is the average of children's log earnings for age 36–40 (1959–1962 birth cohorts). Fathers' log earnings measure five-year average at increasing ages. Children's log earnings regressed on log fathers' earnings, fathers' age and age squared, and dummies for children's birth cohorts. The sample is balanced (i.e., the same individuals are observed in all time periods within the table). Only years with earnings > 0 are included. Five-year averages for fathers are based on at least three years with earnings > 0.

As pointed out earlier, measurement error from life-cycle variation also represents a source of inconsistency if present in the dependent variable (i.e., in the proxies for sons' and daughters' lifetime earnings). For sons, Figure 1 reveals that the life-cycle bias is only slightly positive and is quite stable in the early thirties. This is quite in line with Böhlmark and Lindquist (2006), where the age appears to be between 35 and 40 for comparable cohorts. For the female cohorts, the bias is much more volatile, but the late thirties and early forties appear to be a period where λ is not too far from 1. As a compromise, and to facilitate comparison between the sexes and between offspring and fathers, we have used earnings at ages 36–40 for both sons and daughters.

Table 1 reports the estimated IGEs when we average over (a maximum of) five years only. The first row reports the IGEs when the fathers are, on average, 36 years of age. For sons, we find the elasticity to be 0.338, while for daughters it is 0.230. In the remaining three rows in Table 1, we test the effect of measuring the fathers' earnings at later stages of their life cycles. Hence, the average age is 41 in row 2, 46 in row 3, and 51 in row 4. The time span over which we are averaging is fixed (maximum five years), so any changes in the estimated IGE are likely to be attributed to the age and/or life-cycle effects. Our results indicate a substantial effect from varying the fathers' earnings age. In our estimates, the variance of the transitory earnings component, as well as the life-cycle component, increases. Hence, we expect the IGE to be smaller as the fathers get older. Indeed, moving the five-year earnings window to 1972–1976 (when the fathers are, on average, 41) reduces the IGE to 0.282 and 0.186 for sons and daughters, respectively, and the reduction continues to 0.163 and 0.117, respectively, when their fathers are, on average, 51 years of age.

Our next step is to measure the effects on the IGE of progressively increasing the number of years used for construction of the proxy of fathers' permanent income. The intention is to illustrate the effect on the IGE of reducing the influence of persistence in the transitory components. Note that when we simply expand the window of the fathers' earnings forward, both the length of the fathers' earnings window and the fathers' average age increase. The expansion might therefore have a positive impact on the estimated elasticities, which could be counteracted by the negative age or life-cycle effect. As a means of separating persistence and age as possible sources of error, we hold the average age of the fathers constant by calculating earnings when all the averages center on 1974, when the fathers are, on average, 41 years of age. Now any changes in the estimated IGE are likely to be attributed to the length of the observed earnings window. The estimates of this exercise are reported in Table 2.

In the upper panel of Table 2, we center the earnings on 1974 (the average of 1972–1976). As long as we expand symmetrically,[22] the data limit us to 15-year averages at the most, but the pattern none the less appears relatively clear, with the elasticity increasing by approximately 30 percent, for sons as well as daughters. According to the upper panel of Table 2, the window extension appears to have a significant effect on the estimates, indicating that persistence in the transitory innovations does indeed appear to be a source of bias in former analyses of IGE.

In the lower panel of Table 2, we again symmetrically expand with average earnings for the period 1977–1981 as the center, implying that the fathers are, on average, about five years older than in the upper panel.[23] As expected, the increased average age has a negative effect on the elasticities. As for the effect of increasing the time span of the observations, we reveal the same pattern as in the upper panel: a proportionate increase in the IGE estimates for each five-year expansion.

[22] As 1967 is our first year of observation, it was impossible to expand symmetrically around the period 1967–1971. Instead, we started out with the next age group in Table 1 (i.e., 1972–1976). The IGE is slightly different in Table 2 compared with Table 1 (0.263 versus 0.282 for sons, and 0.175 versus 0.186 for daughters). This is because of the balancing of the samples, resulting in different sample sizes.

[23] In the lower panel of Table 2, our subsample of fathers has earnings observations for five more years (20 years at the most, compared with 15 years in the upper panel).

Table 2. *Intergenerational earnings mobility estimates for sons and daughters; length of window increased, age constant (earnings centered on 1974 and 1979)*

	Elasticities of sons–fathers			Elasticities of daughters–fathers		
	Coeff.	Std err.	Fathers' average age	Coeff.	Std err.	Fathers' average age
Fathers' earnings 1972–1976	0.2631	0.0056	41.1	0.1746	0.0074	41.4
Fathers' earnings 1970–1979	0.3040	0.0061	41.6	0.2019	0.0077	41.6
Fathers' earnings 1967–1981	0.3429	0.0066	41.1	0.2271	0.0080	41.1
Cohorts of fathers		1927–1942			1927–1942	
Number of observations		60,867			56,567	
Fathers' earnings 1977–1981	0.2454	0.0057	46.1	0.1581	0.0069	46.1
Fathers' earnings 1975–1984	0.2648	0.0057	46.6	0.1733	0.0069	46.5
Fathers' earnings 1972–1986	0.2887	0.0059	46.0	0.1918	0.0071	46.0
Fathers' earnings 1970–1989	0.2933	0.0059	46.3	0.1973	0.0071	46.3
Cohorts of fathers		1927–1942			1927–1942	
Number of observations		60,072			55,837	

Notes: The dependent variable is the average of children's log earnings for age 36–40 (1959–1962 birth cohorts). Fathers' log earnings measure increasing averages at constant ages. Children's log earnings regressed on log fathers' earnings, fathers' age and age squared, and dummies for children's birth cohorts. The sample is balanced (i.e., the same individuals in all time periods within each panel), but separately for upper and lower panels. Only years with earnings > 0 are included. Five-year averages for fathers are based on at least three years with earnings > 0. Corresponding requirements of possible earnings when expanding to 10, 15, and 20 observations are 5, 8, and 10 years.

In Table 3, we exploit the fact that for a subsample of the fathers' we have earnings observations for many more years than indicated in Tables 1 and 2. In fact, as we have earnings for the entire population from 1967–2002, there could be some fathers for whom we have 35 years of observations. The trade-off is the length of observations versus the number of cohorts included. In the upper panel, we report the IGEs for those in the 1935–1942 cohorts that we observe for 25 years (1967–1991). Of course, this is only a small fraction (about one-third) of that in Table 2, so we do not expect identical estimates for the comparable periods.

In both panels of Table 3, we keep the fathers' average age constant, while we expand the windows symmetrically, paying attention to the effect on the estimated IGEs. In the upper panel, we demonstrate that the increasing effect first revealed in Table 2 appears to continue, for

Table 3. *Intergenerational earnings mobility estimates for sons and daughters; length of window increased, age constant (earnings centered on 1979 and 1984)*

	Elasticities of sons–fathers			Elasticities of daughters–fathers		
	Coeff.	Std err.	Fathers' average age	Coeff.	Std err.	Fathers' average age
Fathers' earnings 1977–1981	0.2106	0.0101	41.7	0.1462	0.0121	41.7
Fathers' earnings 1975–1984	0.2393	0.0104	42.2	0.1719	0.0125	42.2
Fathers' earnings 1972–1986	0.2685	0.0109	41.6	0.1914	0.0131	41.6
Fathers' earnings 1970–1989	0.2725	0.0109	42.1	0.1982	0.0132	42.1
Fathers' earnings 1967–1991	0.2917	0.0113	41.5	0.2088	0.0138	41.5
Cohorts of fathers		1935–1942			1936–1942	
Number of observations		20,866			19,469	
Fathers' earnings 1982–1986	0.1593	0.0081	46.7	0.1184	0.0094	46.7
Fathers' earnings 1980–1989	0.1828	0.0087	47.2	0.1333	0.0103	47.2
Fathers' earnings 1977–1991	0.2118	0.0095	46.6	0.1488	0.0111	46.6
Fathers' earnings 1975–1994	0.2161	0.0095	46.9	0.1549	0.0113	46.9
Cohorts of fathers		1935–1942			1935–1942	
Number of observations		20,157			18,858	

Notes: The dependent variable is the average of children's log earnings for age 36–40 (1959–1962 birth cohorts). Fathers' log earnings measure increasing averages at constant ages. Children's log earnings regressed on log fathers' earnings, fathers' age and age squared, and dummies for children's birth cohorts. The sample is balanced (i.e., the same individuals in all time periods within each panel), but separately for upper and lower panels. Only years with earnings > 0 are included. Five-year averages for fathers are based on at least three years with earnings > 0. Corresponding requirements of possible earnings when expanding to 10, 15, 20, and 25 observations are 5, 8, 10, and 13 years.

sons as well as daughters, when we expand to both 20 and 25 years of observation.

A second point to note when comparing the lower panel of Table 2 with the upper panel of Table 3 is that for earnings measured in 1977–1981, the IGE in Table 3 is lower than in Table 2. This is the case for the three other comparable windows (1975–1984, 1972–1986, and 1970–1989). There could be several reasons for this pattern. Obviously, the average age is different. More specifically, the 1935–1942 cohorts in the upper panel of Table 3 are, on average, 41 years of age, which is five years below the average age of the 1927–1942 cohorts in the lower panel of Table 2. However, if the higher age of the fathers is associated with lower IGEs, the elasticities in the upper panel of Table 3 ought to be higher than the comparable figures in the lower panel of Table 2. Our findings indicate the opposite.[24] In the lower panel of Table 3, we add five years of age to the individuals constituting the 1935–1942 samples. The result is equivalent to what we found for the 1927–1942 samples in Table 2. When we increase the age of the comparable samples, the estimated IGEs decrease as expected if age and/or life-cycle bias is the driving force.[25]

In total, our results lend support to the existing findings in the literature. That is, extending the length of the fathers' earnings window increases the estimated elasticities. Furthermore, there is a rather strong negative effect on the estimated intergenerational elasticities from the age of fathers when their earnings are measured. This is consistent with biases caused by the association between permanent earnings and annual earnings over the life cycle; alternatively, it could be evidence that the variance of the earnings shocks might have increased throughout the life cycle. Both appear to be the case according to the estimates presented earlier in this section, with (weak) evidence pointing to the increasing variation in transitory earnings shocks as the stronger source.

We conclude this section with a brief comparison with some previous findings. In Table A1, we report results from other Norwegian studies analyzing intergenerational earnings mobility. If we focus on studies comparable to ours, in that they are based on a linear regression model with average log earnings of sons or daughters regressed on fathers' average

[24] These cohort differences might arise from our sample construction. Because the oldest cohorts are removed from Table 3, the fathers are systematically selected based on family formation early in their lives. Comparing the fathers in Table 3 with the overall average of the 1935–1942 cohorts in our dataset, we find that educational attainment and average earnings over the period 1976–1980 are slightly lower. Concerning earnings, we might have expected the opposite, as the selected sample has a relatively long and stable history in the labor market (see Section III for details). Alternatively, the lower averages for earnings and the length of education for the selected sample are possibly because of their reduced opportunity to take part in the educational expansion that took place in Norway in the 1960s and 1970s. In any case, the relatively low elasticity for the lower part of the earnings distribution is consistent with both Bratberg *et al.* (2005) and Bratsberg *et al.* (2007).

[25] We also checked whether differences in the exclusion criteria affect the level of the elasticity. In the upper part of Table 3, we required that at least 13 out of 25 years had positive earnings observations. When we applied a less restrictive exclusion criterion (8 out of 25 years with positive earnings observations, instead of 13 out of 25) we found no significant differences in the level of the elasticity.

log earnings,[26] we find estimates that vary between 0.129 and 0.159 for sons, and 0.121 and 0.126 for daughters. We also see that the age of the offspring and fathers, together with the number of years fathers' earnings are averaged over, vary from one study to another. These elasticities are comparable to findings for the other Scandinavian countries, but at the lower end.

Bratberg *et al.* (2005) – see their Tables 2 and 4 – find elasticities of 0.129 and 0.126, for men and women, respectively, for children born in 1960. The earnings of the children are measured when they are, on average, 33 years of age, while the earnings of the fathers are measured in 1977–1981 when they are, on average, 47 years old. Bratsberg *et al.* (2007) report an elasticity of 0.159 for men born in 1958, who are, on average, 37 years of age at observation. The earnings are averages over two years for both sons and fathers (1992 and 1999 for sons, and 1971 and 1976 for fathers). If we assume that the age of the fathers is, on average, 32 years when the sons are born (similar to the average around 1960), fathers' earnings are measured when they are, on average, 48 years old. Jäntti *et al.* (2006) report elasticities of 0.150 and 0.121 for sons and daughters who are, on average, 37 years old. The average age of fathers is also 48 years here, which is older than in our study. Furthermore, an average based on one observation only might suffer from the attenuation bias already discussed. Thus, when comparing the results in our study and those reported in Bratberg *et al.* (2005), Bratsberg *et al.* (2007), and Jänti (2006), we should take into consideration both the age of the fathers (and age of the sons/daughters) when their earnings are measured and the number of years over which the earnings averages are calculated. Doing so, the reported elasticities in the three studies *op. cit.* become closer to ours.

In other work, Grawe (2006) concluded that the effect of increasing the age of the father at observation was that "... the average estimated earnings persistence drops ... a little more than one percentage point per year" in the US Panel Study of Income Dynamics and National Longitudinal Survey (NLS), the Canadian Intergenerational Income Data, and the German Socioeconomic Panel. We find the corresponding figures to be 1.1 percentage points for men and 0.7 percentage points for women, based on our Table 1.[27] In the US, Mazumder (2005), using the findings of the

[26] This excludes Hansen (2010), who uses the log of average family earnings, whereas we use the average of log fathers' earnings, and Raaum *et al.* (2007) uses a third-order polynomial of log fathers' earnings.

[27] We find this by simply running OLS with the coefficient estimates in Table 1 as the dependent variable and the averages of the fathers' ages as the explanatory variable. Note, however, that we do not make any extrapolation outside the age range used in the table.

Survey of Income and Program Participation matched to the Social Security Administration's Summary Earnings Records, reported in his Table 8, that the estimated elasticity increases by 2.1 and 0.9 percentage points for men and women, respectively, for each additional year the fathers' earnings are averaged.[28] Based on the findings in our Tables 2 and 3, our corresponding numbers are 0.5 and 0.3 percentage points for men and women, respectively. Hence, our findings are in line with Mazumder (2005), even though the magnitudes are, in percentage points, somewhat different.[29]

V. Concluding Remarks

The extraordinary length of our generational data allows us to observe a substantial part of the life-cycle earnings of four birth cohorts born around 1960, and almost the entire life-cycle earnings of their fathers. We find two factors that influence the estimated intergenerational elasticities that we interpret as sources of measurement error.

First, there is a strong, negative age dependency. Based on fixed (five-year) averages, we find that the IGE for the youngest group (fathers are, on average, 36 years old) is approximately twice the size of the IGE for oldest group (fathers are, on average, 51 years old) for both sons and daughters. Second, the estimated elasticities also depend on the length of the fathers' earnings window. Contrary to the age effect, lengthening the window positively affects the IGEs. The estimates based on 15-year averages are 25–30 percent higher than our benchmark case with five-year averages, for both sons and daughters.[30]

Like Mazumder (2005), we interpret the influence of the length of the fathers' earnings window as an indication of bias stemming from persistence in the transitory earnings component. However, the age of the fathers appears to be a more important cause of bias. The age dependence might be attributed to life-cycle variation in the permanent earnings component and/or changes in the variance of the transitory component. As illustrated

[28] Here we have used the same type of simple regression described above.

[29] Based on the point estimates reported in Tables 2 and 3, we have tested whether the effect of extending the window length is linear. For this, we run a regression where the estimated coefficients are regressed on window length and window length squared together with one dummy for each of the four panels. We have run regressions for sons–fathers and daughters–fathers separately. At a window length of five years, the marginal effect of a one-year window extension is 0.7 and 0.5 percentage points for sons and daughters, respectively. This window extension has an increasing effect until 26 years for sons and over 28 years for daughters.

[30] This effect persists even with a period of 15 years. There is an additional positive effect extending the period to 25 years, although the age of the fathers in the latter case is higher than recommended.

in Figure 4, we find that both sources are present. However, unlike Haider and Solon (2006), Böhlmark and Lindquist (2006), and Grawe (2006), we consider the latter of these sources to be the more important, although this is not formally tested in the present paper.

The IGE estimates in this paper are higher than those previously reported for Norway (e.g., Bratberg *et al.*, 2005; Bratsberg *et al.*, 2007). For fathers with earnings measured in their early forties and offspring in their late thirties (the period in life when, according to our findings, the age and/or life-cycle bias appears to be least problematic), we find estimated IGEs of 0.282 and 0.186 for sons and daughters, respectively, when the earnings are based on five-year averages. The estimated intergenerational elasticities increase to 0.343 and 0.227 for sons and daughters, respectively, when we use 15-year averages for the fathers instead of five-year averages. The upward correction in intergenerational earnings persistence is also a tendency found in recent analysis of US data. Hence, in relative terms, Norway remains a country characterized by high intergenerational earnings mobility.

Appendix A

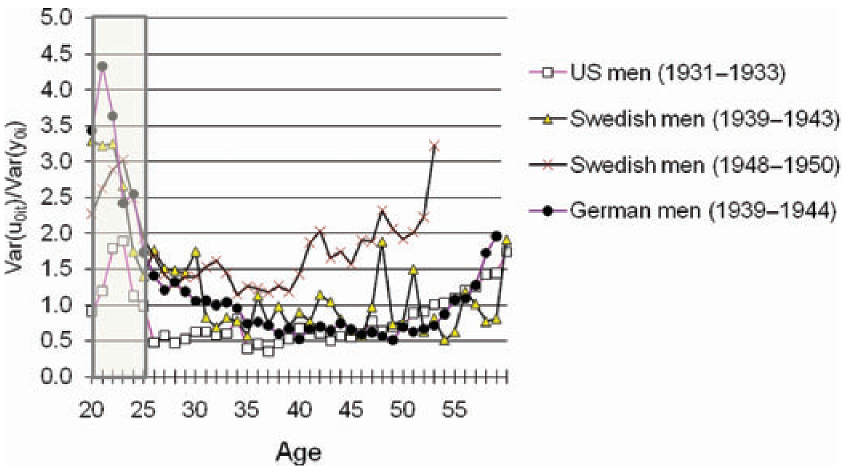

Fig. A1. Transitory to permanent income variance, $Var(u_{0it})/Var(y_{0i})$, for US, Sweden, and Germany
Notes: The shaded area (age 20–25 years) corresponds to an interval not covered by our Norwegian data and therefore not shown in Figure 4.

Table A1. *Intergenerational earnings elasticity estimates for Norway*

	Coeff. sons	Coeff. daughters	Sons' age	Daughters' age	Fathers' age	Fathers' window length (years)	Source
Bratberg *et al*. (2005)[a]	0.129	0.126	33	33	47	5	Table 2/Table 4
Bratsberg *et al*. (2007)[b]	0.159	–	37	–	48	2	Figure 6/Table 1
Hansen (2010)[c]	0.192	0.158	34	34	44	5	Table 2
Jäntti *et al*. (2006)	0.150	0.121	37	37	48	1	Table 5
Raaum *et al*. (2007)[d]	(0.269)	(0.186)	37	41	48	2	Table 2

Notes: The results are based on linear regression models; one exception is Raaum *et al*. (2007). Age (sons', daughters', or fathers') refers to the average age at which earnings are measured. Window length refers to the number of years that fathers' earnings are averaged over. Source refers to the tables/figures in the given papers in which the elasticity estimates are found. (a) See Bratberg *et al*. (2007) for quantile regression results based on the same data. (b) Bratsberg *et al*. (2009) also include a third-order polynomial of log fathers' earnings. At the 50 percentile, the slope is 0.281. (c) Different from the others is Hansen (2010), who uses the log of the average earnings instead of the average of the logs. (d) These estimates are based on a third-order polynomial of log fathers' earnings at the median. The elasticity is measured at the median.

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