Estimates of Intergenerational Elasticities

Based on Lifetime Earnings

by

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Abstract

Using Norwegian intergenerational data with a substantial part of the life-cycle earnings of children and almost the entire life-cycle earnings for their fathers, we present new estimates of intergenerational mobility. Extending the length of the fathers' earnings windows from 5 to 30 years increases the estimated elasticities. Increasing the age of father at observation has the opposite effect. Our findings indicate that intergenerational earnings mobility may have been strongly overstated in many earlier studies with shorter earnings histories. Biases in the estimated elasticities appear to be related to age and/or life-cycle measurement errors more than persistency in the transitory innovations.

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1. Introduction

The degree of intergenerational earnings mobility reflects the rigidity of the society. Low mobility implies that a person's position in the earnings distribution is highly dependent of the parents'position, while high mobility implies the opposite. Hence, measures of intergenerational earnings mobility are informative for the evaluation of the extent to which societies offer fair outcomes for individuals, and the extent to which policy action towards injustice are called for.

The differences in the estimates of intergenerational earnings mobility are remarkably high, across country and across time. As researchers have gained access to better data, they have tried to explain these differences. The distorting effect of transitory earnings shocks on the permanent income measure has been internalized in the literature by averaging several yearly observations, typically over 5 years or less (see for instance Solon (1992) and Zimmerman (1992)). Mazumder (2005) claims that 5 years is a far too small window, given the persistence characterizing the transitory shocks. He argues that the earlier estimates are downward biased due to measurement errors in the form of omitted dynamics in the fathers' earnings variable. Based on nationally representative social security data, he presents intergenerational elasticities (IGE) for the US that are approximately 50 percent higher than earlier believed (approximately 0.6 instead of 0.4). Mazumder's data are partly imputed due to top coding, and the number of observations is relatively low. Therefore he suggests that future research should attempt to verify his results using long-term measures of permanent earnings from other sources.

Another source of measurement error with implications for the estimation of intergenerational mobility is discussed in several recent papers, notably Haider and Solon

(2006), Grawe (2006), and Böhlmark and Lindquist (2006)[1]. Here it is argued 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 current earnings is used as proxy for the fathers' as well as the children's lifetime earnings, with the inconsistency varying across fathers' and/or children's age. Controlling for multi-year average of current income cannot eliminate this measurement error.

The present paper sits at the intersection of two types of measurement problems associated with estimates of intergenerational earnings elasticities; persistency in the transitory earnings shocks and life-cycle biases. Our focus is on the effect of persistency in the earnings shocks and its importance for the attenuation bias We explore this by expanding the fathers' earnings window, similar to Mazumder's analysis based on US data. At the same time we pay attention to the empirical findings regarding lifecycle 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. In the existing empirical literature on intergenerational mobility there is a substantial variation in the length of fathers' earnings window as well as the age of the father at observation. This makes it hard to compare studies within and/or between countries. We believe that our results as regards the effects of different measurement problems make it easier to do comparisons of the existing empirical findings in the literature.

Our data have several advantages: (1) they provide us with very long earnings series; (2) the sources are administrative registers, e.g., the public tax register, reducing the problems of self reporting errors, attrition, etc.; (3) they are census data and therefore highly representative and give a high number of observations; (4) they do not suffer from the

[1] Earlier contributions on the same topic, but within somewhat different frameworks, are Jenkins (1987) and Björklund (1993).

truncation problems that are present in Mazumder's data; and (5) unlike most other studies within this field, they include information about female earnings.

Measuring fathers' earnings at various age to see the effects of life-cycle bias, we find that the intergenerational elasticities drops 1.1 percentage point for men and 0.7 percentage points for women per year the older the fathers get. Extending the length of the father's earnings window to see the effect of attenuation bias, the intergenerational elasticities increases 0.5 and 0.3 percentage points, sons and daughters respectively, for each additional year the fathers' earnings are averaged over. Our intergenerational elasticities estimates are much higher than those reported in recent Norwegian research, see for instance Bratberg et al. (2005) and Bratsberg et al. (2007). Part of this difference is due differences in both the length of fathers' earnings window and the age of the father at observation. Nevertheless, after carefully correcting for these measurement issues, Norway is still a country characterized by high intergenerational earnings mobility, especially compared to the United States and the United Kingdom.

The paper is structured as follows. Section 2 describes various sources of measurement errors and biases therefrom. Section 3 presents the data used in the analysis. Empirical results are discussed in Section 4, while concluding remarks are given in Section 5.

2. Sources of Measurement Bias

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

(1) $$y_{1i} = \rho y_{0i} + \varepsilon_i ,$$

where subscripts 1 and 0 are child and parent, respectively, y is a measure of "lifetime" or "permanent" income in logs, ρ is the slope coefficient, and ε is a random error term.[2] In addition, quadratic functions of both generations' age are commonly added. ρ measures the intergenerational earnings elasticity (IGE) between parents and children. The closer to zero ρ is, the higher is intergenerational mobility.

In spite of this striking simplicity, the IGE estimates have undergone considerable adjustments during the last couple of decades. This is basically due to measurement issues. While Becker and Tomes (1986) base their quite optimistic views on intergenerational mobility on a ρ of 0.2 or less, Solon (1992) as well as Zimmerman (1992) conclude that the IGE for men in the US is twice as high: 0.4 or a bit higher. This tremendous discrepancy is mainly due to a classical measurement error. Assume that the parents' earnings in a given year t, y_{0it}, consist of the permanent component, y_{0i}, and a transitory component, w_{0it}:

$$(2) \qquad y_{0it} = y_{0i} + w_{0it}.$$

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 their permanent earnings, $\hat{\rho}$ will be downward biased by the factor $\phi = \sigma^2_{y0} / (\sigma^2_{y0} + \sigma^2_{w0})$, where σ^2_{y0} and σ^2_{w0} are the respective variances of y_{0i} and w_{0it}. Solon (1992) and Zimmerman (1992) apply up to 5-year averages of single-year earnings as their proxies. Averaging over T years implies that the factor of inconsistency (the attenuation factor) becomes $\phi_T = \sigma^2_{y0} / (\sigma^2_{y0} + \frac{\sigma^2_{w0}}{T})$. Clearly, the transitory earnings shocks contributes more to the noise relative to the signal in the first compared with the second case, leaving the former estimates highly downward biased.

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

However, the attenuation factor above rests on the fairly unrealistic assumption of 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:[3]

(3) $$w_{0it} = \delta w_{0it-1} + \nu_{0it},$$

where δ is the autocorrelation coefficient of the transitory component and ν_{0it} is white noise. The attenuation factor, still in the case of averaging over T years, becomes:

(4) $$\phi_T = \sigma^2_{y0} / (\sigma^2_{y0} + \frac{\sigma^2_{w0}}{T}\alpha),$$

where $\alpha = 1 + 2\delta \frac{T - \frac{(1-\delta^T)}{(1-\delta)}}{T \cdot (1-\delta)}$.[4] Even with these relatively simple earnings dynamics, the bias now becomes rather complicated. Mazumder (2005) performs simulations that demonstrate that even with quite a low degree of persistence (δ = 0.5) the attenuation factor becomes 0.69 when using a 5-year average, as compared with 0.83 in the absence of autocorrelation (δ = 0); see Table 1 p. 238. This implies that Solon's and Zimmerman's estimates of ρ of 0.4 may be a 30% downward biased estimate of a true IGE of 0.6. Furthermore, Mazumder (2005) illustrates that under these earnings assumptions one needs averaging over more than 25 years to get a reasonable value (i.e., close to one) for the attenuation factor, see Figure 1, p. 239.

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[3] Solon (1992) illustrates the case of first-order moving average and autoregression, respectively (note 17, p. 237). Baker and Solon (2003) introduce non-stationary (random walk) as well as stationary components in their earnings dynamics models for Canada.

[4] See Solon (1984) for the derivations.

Another source of measurement error with implications for the estimation of intergenerational mobility is the life-cycle variations in the association between current earnings, y_{it}, and permanent earnings, y_i, for children as well as for fathers.[5] This association may be modeled as $y_{1it} = \lambda_{1t} y_{1i} + u_{1it}$ and $y_{0it} = \lambda_{0t} y_{0i} + u_{0it}$, for children and fathers, respectively. These life-cycle variations represent a form of error where the classical errors-in-variables model is misspecified. Firstly, in intergenerational earnings regression current instead of lifetime earnings for the *children* (i.e., the left hand side variable) also yields biased OLS estimates. Assuming that we have an appropriate measure of parents' earnings, i.e., λ_{0t} = 1, but y_{1it} is used as a proxy for y_{1i}, the IGE estimates will be confounded by the children's own life-cycle variation:

$$(5) \qquad y_{1it} = \lambda_{1t}(\rho y_{0i} + \varepsilon_i) + u_{1it} .$$

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

Secondly, the life-cycle bias adds to the standard errors-in-variables bias when current earnings are used as proxy for the *fathers'* lifetime earnings. Assuming that we have an appropriate measure of children's earnings, i.e., λ_{1t} = 1, the inconsistency when estimated by OLS now becomes:[6]

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[5] 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 the high relative to the low 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 at higher ages.

[6] Here any time averaging and/or persistence of the type in eq. (4) are ignored.

(6) $$\operatorname{plim}\hat{\rho} = \rho \frac{\lambda_{0t}\sigma_{0y}^2}{\lambda_{0t}^2\sigma_{0y}^2 + \sigma_{0w}^2} = \rho\theta_t .$$

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

Finally, the fathers' position in the life cycle may also influence the attenuation factor, ϕ_T. Mazumder (2001) and Baker and Solon (2003) both argue that the variance of the transitory innovation, σ_{w0}^2, follows a U-shaped pattern over the life cycle, with smallest values around the age of 45. Before and—particularly—after this period in life, the variance typically appears to be considerably higher. However, Grawe (2006) concludes that the evidence appears to support the hypothesis that life-cycle bias and not growing attenuation bias causes the relationship between fathers' age and estimated IGE.

In this study we start with the intention of correcting the potential bias stemming from persistence in the transitory earnings shocks. To isolate this form of attenuation bias, we need 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.[7] First, we follow the estimates of Haider and Solon's (2006) and Böhlmark and Lindquist's (2006) regarding the periods of children's and fathers' lives where the λ_t s are closest to one.[8] In our benchmark

[7] See Section 4 in Grawe (2006) for a discussion of alternative procedure(s).

[8] In this matter we are mainly guided by the results in Böhlmark and Lindquist (2006), which are quite in line with Haider and Solon (2006), and – more importantly – are based on register data that

case we condition on the period in life where λ_t allegedly is (close to) one. Any remaining bias in the IGE is then interpreted as attenuation bias. Like Mazumder (2005), the test procedure for investigating whether attenuation bias arises due to persistence in the transitory earnings component implies successively extending the length of the period for which the fathers' earnings are observed. But unlike Mazumder we explicitly pay attention to the possible confounding life-cycle effect.

3. Data Set and Variables

Our data are collected from different administrative data sets 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 2002 based on mandatory tax reports, in addition to family characteristics and birth year. The incomes series were originally collected for the purpose of calculating old-age pensions. This implies that they basically include earnings, but exclude interest, capital income, etc. Unemployment benefits, disability benefits and sick pay are included, but not means-tested benefits. All the income variables were first adjusted to real 1999 income using the consumer price index. In addition, we discounted fathers' income down to the year the child was born using a discount factor of 2 percent. Opposite to the data in Mazumder (2005) and Haider and Solon (2006), the earnings variables are uncensored, at the top as well as at 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 on the precision of the estimates.

We include children of both genders from the 1959–1962 birth cohorts. Earlier cohorts might have been included, but we refrain from it to avoid the possible confounding effect

resemble the ones used in our study. It is also advantageous that the results are based on data from Sweden; a welfare state very much of the same kind as Norway.

from trends in mobility across time.[9] We limit our study to individuals whose compulsory schooling is at least 9 years.[10] Later cohorts of children are not included, since we want to follow the individuals at least until the age of 40. In addition, the trend argument applies also in this direction. We also exclude individuals born by parents younger than 16 or older than 40.[11]

We also limited our sample to fathers born between 1927 and 1942. This means that we can observe the earnings of all the fathers at least from when they were 40 to when they were 60 years of age; a key period if we want to study the effect of life-cycle bias. Admittedly, this limitation results in a sample of fathers that had become parents somewhat earlier than in the overall Norwegian population.[12]

For fathers as well as children, earnings are measured in logs; the averages are over log earnings. Being born in 1959–1962, our sons and daughters are between 40 and 43 years of age when our earnings series ends in 2002. We choose the common age of 36–40 for males and females as the years over which the earnings are averaged. We use fathers' earnings as the only indicator of the family's earnings capacity.[13] For both sons/daughters and their

[9] Bratberg, Nilsen, and Vaage (2005) report a slight upward trend in mobility when they compare the 1950 with the 1960 cohorts.

[10] The increase of compulsory schooling from seven to nine years took place during the 1960s and early 1970s, with 1974 as the last year; see Aakvik, Salvanes, and Vaage (2003) for details and analysis of the effect of the compulsory schooling reform on earnings. The 1959 birth cohort ended their compulsory schooling in 1975. Thus all the children in our sample had nine years and all the fathers seven years of compulsory schooling.

[11] We also excluded individuals born outside Norway and non-Norwegian citizens because of the high frequency of missing earnings information.

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

[13] Using fathers' earnings as a proxy of household earnings is not too unrealistic since the fathers typically were the breadwinners of the families, while mothers commonly stayed home for the cohorts analyzed in this study.

fathers, the 5-year averages are based on at least 3 years of positive earnings, i.e., individuals with only two or less years of earnings are excluded. When we extend the size of the window from 5-year to 30-year averages, the corresponding requirements of strictly positive earnings are 5, 8, 10, 13, and 15 years. In our regressions, we age-adjust the fathers' earnings by including fathers' age and age squared. In addition, we control for potential cohort effects by including cohort dummies for the children.[14]

Finally, observing the individuals at different points in time and for different time spans implies that the composition of the samples will differ. For instance, when one move the year used as focal point for fathers' earnings, some individuals drop out due to too few positive earnings observations 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 5 years of observation. To avoid influence from the composition, the samples are balanced, which means that they are fixed within each table.

4. Results

We start out with estimates where the fathers' earnings are averaged over a relatively short period (maximum 5 years). This allows comparison with other research. A major challenge, however, is to separate attenuation bias due to short earnings windows on the one hand from life-cycle bias on the other. As for the latter, Böhlmark and Lindquist (2006) 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 it may stem from persistence in the transitory earnings fluctuations or, alternatively, the variance of the transitory earnings in the chosen 5-year period may be exceptionally high. Mazumder (2005)

[14] One normally includes sons'/daughters' age and age squared in the regressions. But since all offspring's earnings are measured at the same (average) age this was not necessary in our analysis.

and Baker and Solon (2003) find that the variance of the transitory innovation is lowest when the fathers are around age 40.[15] Taken together, this implies that for fathers the age should be set to minimize age-related and life-cycle bias, and the earnings series should be long enough to deal with the persistence bias. Our benchmark case will be a 5-year average for earnings between 1967 and 1971, in which year the fathers' average age is around 36. In the next step we construct earnings measures where we average progressively up to 30 years (1967–1996).

As pointed out earlier, measurement error due to 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. We note that for the male cohorts in Böhlmark and Lindquist (2006) closest to the cohorts in our study, the life-cycle bias is only slightly positive and quite stable between the ages of 35 and 40. For the female cohorts the bias is much more volatile, but the late 30s and early 40s appears to be a relatively stable age. Hence, for the sons and daughters in our sample we use their earnings at age 36–40.

Table 1 reports the estimated intergenerational earnings elasticities when we average over (maximum) 5 years only.

[Table 1 about here.]

The first row reports 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 other three rows of Table 1, we test the effect of measuring the fathers' earnings at later stages of their life cycles. Hence, in row 2 the average age is 41, 46 in row 3, and 51 in row 4. The time span over which we are averaging is fixed (maximum 5 years), so any changes in the estimated IGE are likely to be attributed to age and/or life cycle effects. Our results indicate a substantial effect from varying the fathers' earnings age. We expect the variance of the transitory earnings component to be

[15] Grawe (2006) suggests that both fathers and sons should be measured near midlife when analyzing intergenerational mobility.

larger and, hence, the IGE to be smaller as the fathers get older. Moving the 5-year earnings window to 1972–1976 (when the fathers were 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 for sons and daughters when their fathers are on average 51 years of age. If, as argued by Mazumder (2001) and Baker and Solon (2003), the variance of the transitory innovations—and, hence, the attenuation bias—is smallest around the age of 40, we would expect a decline in the IGE also when we move to a lower average age. However, to the degree that an average age of 36 is sufficiently low compared with 40, this does not seem to be the case in our sample, compare row 1 with row 2 of Table 1. Life-cycle bias through the permanent earnings component is the alternative explanation to the strongly negative age effect.

[Table 2 about here]

Table 2 reports the effects on the IGE of progressively increasing the number of years used for construction of the proxy of fathers' permanent income. The four measures in the upper panel are based on fathers' earnings averaged over 5 years (benchmark) and thereafter expanded to 10, 15, and 20 years, respectively.[16] The intention is to illustrate the effect on the IGE of reducing the influence of persistence in the transitory components (see eq. (4) in Section 2). Moving from the first 5-year average[17] to the 10-year average in row 2, there is actually a small movement in the opposite direction, for sons as well as for daughters. Extending the length of the window to 15 and 20 years, respectively, only gives small and insignificant decrease in the estimated IGEs. Hence, there is hardly any sign of bias stemming from persistence in the transitory earnings fluctuations. This is strongly at odds with Mazumder (2005), where the elasticities—somewhat depending on the sampling rules—often increase 50 percent or so when the period is expanded from 4 to 16 years.

[16] The corresponding requirements of strictly positive earnings are 3, 5, 8, and 10 years.

[17] This IGE is comparable with the first row of Table 1. It is not identical because the different balancing in the two tables results in slightly different samples.

In the lower panel of Table 2 we exploit the fact that for a sub-sample of the fathers we have earnings observations for many more years than the 20 used in the upper panel. In fact, since we observe earnings for the entire population from 1967–2002, there might be some fathers for whome there exist 35 years of observation. The trade-off is length of observations vs number of cohorts included. In the lower panel we report the IGEs for those of the 1936–1942 cohorts that are observed for 30 years (1967–1996). Of course, this is only a small fraction (about one-fourth) of the one in the upper panel, so we do not expect identical estimates for the comparable periods. There is a tendency in the estimated IGEs of an inverted U-shape for sons as well as for daughters, with start (5-year averages) and end (30-year averages) below the start and end in the upper panel. However, there are still no signs of increases of the type reported in Mazumder (2005).

Note that when we expand the window of fathers' earnings forward, as in Table 2, both the length of the fathers' earnings window *and* the fathers' average age increase. It might therefore be that the expansion has a positive impact on the estimated elasticities, which is counteracted by a negative age or life cycle effect. As a way of separating persistence and age as the source of error, we hold the average age of the fathers constant by calculating earnings where all the averages are centered on 1974, when 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 3.

[Table 3 about here]

In the upper panel of Table 3 the earnings are centered on 1974 (average of 1972–1976). As long as we expand *symmetrically*[18] the data limit us to 15-year averages at the most,

[18] Since 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: 1972–1976. The IGE is slightly different in Table 3 relative to Table 1 (0.263 vs 0.282 for sons and 0.175 vs 0.186 for

but the pattern nevertheless appears to be relatively clear. The elasticity increases by approximately 30 percent, for sons as well as for daughters. This contradicts the findings in Table 2, where the extension of the window had more or less no effect of the estimates, but where no attempts were made to control for aging of the sample. According to the upper panel of Table 3, however, window extension appears to have a significant effect on the estimates, indicating that persistency in the transitory innovations does seem to be a source of bias in former analyses of the IGE.

In the lower panel of Table 3 we expand symmetrically with average earnings for the period 1977–1981 as the center, implying that the fathers are on average about 5 years older than in the upper panel. 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, the same pattern as in the upper panel is revealed: a fairly proportionate increase in the IGE estimates for each 5-year expansion.

[Table 4 about here]

In Table 4 we once again exploit the length of our series to test the effects on a sub-sample of fewer individuals with longer earnings histories.[19] In both panels we keep the average age of the fathers 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 that is revealed in Table 3 appears to continue, for sons as well as for daughters, both when we expand to 20 and to 25 years of observation.

A second point to note when comparing the lower panel of Table 3 with the upper panel of Table 4 is that for earnings measured 1977–1981 the IGE in Table 4 is lower than in

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daughters). This is once again due to the balancing of the samples that resulted in different sample sizes.

[19] As in the lower panel of Table 2 this implies that we have to limit our sample to fathers born in the period 1936-1942, leaving us with about one-fourth of the sample in Table 3.

Table 3, and this is also the case for the two other comparable windows (1975–1984 and 1972–1986). There may be several reasons for this pattern. Obviously, the average age is different. More specifically, the 1936–1942 cohorts in the upper panel of Table 4 are on average 41 years of age, which is 5 years below the average age of the 1927–1942 cohorts in the lower panel of Table 3. But if higher age of the fathers is associated with lower IGEs, the elasticities in the upper panel of Table 4 ought to be *higher* than the comparable ones in the lower panel of Table 3. Our findings indicate the opposite.[20] In the lower panel of Table 4 we add 5 years of age to the individuals constituting the 1936–1942 sample. The result is equivalent to what we found for the 1927–1942 sample in Table 3. When we increase the age of comparable samples, the estimated IGEs *decrease* as expected if life-cycle bias is the driving force.[21]

In total, our results lend support to existing findings in the literature; that there is a rather strong negative effect on the estimated intergenerational elasticities from the age of fathers when their earnings are measured, and that extending the length of the fathers' earnings windows increase the estimated elasticities. These findings are consistent with biases

[20] These cohort differences might be due to our sample construction. Since the oldest cohorts are dropped in Table 4, the fathers were systematically selected based on family formation early in their lives. Comparing the fathers in Table 4 with the overall average of the 1936–1942 cohorts in our data set, we found that their educational attainment and average earnings over the period 1976-1980 is slightly lower. As for earnings, one might have expected the opposite, since the selected sample has a relatively long and stable history in the labour market (see Section 3 for details). On the other hand, the lower averages of earnings and length of education for the selected sample possibly are due to their reduced opportunity of taking part in the educational expansion that took place in Norway in the sixties and the seventies. In any case, the relatively low elasticity for the lower part of the earnings distribution is consistent with Bratberg et al. (2005) as well as Bratsberg et al. (2007).

[21] We also checked whether differences in exclusion criteria affect the level of the elasticity. In the upper part of Table 4 we required at least 13 out of 25 years with positive earnings observations. When we applied the exclusion criterion of Table 3 (at least 8 out of 15 years with positive earnings observations) we found no significant differences in the level of the elasticity.

caused by association between permanent earnings and annual earnings over the life-cycle, and persistency in the transitory earnings shocks, respectively. An alternative explanation for the observed life-cycle pattern in our study is that the variance of the earnings shocks might have increased over the life-cycle. No relevant studies exist based on Norwegian data to confirm or reject this. Note, however, that most studies find that the transitory earnings variance decreases over the life-cycle until the mid forties and then increases again (see for instance Mazumder (2001) and Baker and Solon (2003)). In most of our tables[22] the oldest average age of the fathers is 46 or less. Thus, the attenuation caused by the variance of the transitory innovation over the life-cycle cannot explain the observed pattern in our study, given that the variance of the earnings in Norway follow the same pattern as in other countries.

We conclude this section with a brief comparison with some previous findings. Our comparisons start with two Norwegian studies. Bratberg et al. (2005), Tables 2 and 4, find elasticities of 0.129 and 0.126, men and women respectively for children born 1960. The average log earnings of the children are measured when they are 31–35 years of age, while log average earnings of the fathers are measured in 1977–1981 when they are on average 47 years old. We know from the results of Haider and Solon (2006) and Böhlmark and Lindquist (2006) that life-cycle bias gives the effect that measuring children’s earnings too early will bias the results downwards. The findings reported in Bratberg et al. (2005), Table 3, also shows a pattern consistent with life-cycle bias.[23] Bratsberg et al. (2007) report an elasticity of 0.159 for men born 1958. The earnings are averages over 2 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

[22] Except for Table 1, where the highest average age is 51.

[23] The results reported in Bratberg et al. (2005) are opposite for men and women when it comes to elasticities based on varying ages of the children. This is, however, consistent with the differences between men and women in life-cycle biases reported in Böhlmark and Lindquist (2006).

fathers on average was 32 years when the sons were born (similar to the average around 1960), fathers' earnings are measured when they are on average 48 years old. This is older than in our study. Furthermore, an average based on two observations only may suffer from the attenuation bias already discussed. Thus, when comparing the results in our study and the ones reported in Bratberg et al. (2005) and Bratsberg et al. (2007), one should take into consideration both the age of the sons/daughters and fathers when their earnings are measured, in addition to the number of years over which the earnings averages are calculated. Doing so, the reported elasticities in the two studies *op. cit.* get closer to ours.

Grawe (2006), inspecting the effect of increasing the age of the father at observation, finds that "the average estimated earnings persistence drops [...] a little more than one percentage point per year" in the American PSID and NLS, the Canadian Intergenerational Income Data (IID), and the German Socioeconomic Panel (GSOEP). We find the corresponding numbers to be 1.1 percentage point for men and 0.7 percentage point for women based on the findings in our Table 1.[24] Mazumder (2005), using the Survey of Income and Program Participation matched to Social Security Administration's Summary Earnings Records (US) finds, based on the numbers reported in his Table 8, that the estimated elasticity increases by 2.1 percentage points and 0.9 percentage points for men and women respectively, for each additional year the fathers' earnings are averaged over.[25] Based on the findings in our Tables 3 and 4, our corresponding numbers are 0.5 percentage points for men and 0.3 percentage points for women. Hence, our findings are in line with Mazumder, even though the magnitude is somewhat different.

[24] This number is found simply by running OLS with the coefficient estimates in Table 1 as the dependent variable and the averages of the fathers' ages as the explanatory variable.

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

5. 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, and which we interpret as sources of measurement errors.

Firstly, there is a strong, negative age dependency. Based on fixed (5-year) averages we find that the IGE for the youngest group (fathers on average 36 years of age) is approximately twice the size of the oldest group (fathers on average 51 years of age), for sons as well as daughters. Secondly, the estimated elasticities also depend on the length of the fathers' earnings window. Contrary to the age effect, lengthening of the window affects the IGEs positively. The estimates based on 15-year averages are 25–30 percent higher than our benchmark case with 5-year averages, for sons as well as for daughters.[26]

The age dependence may be attributed to life-cycle variation in the permanent earnings component and/or variation in the variance of the transitory component. Like Haider and Solon (2006), Böhlmark and Lindquist (2006), and Grawe (2006), we consider the first of these sources to be the most likely, although this was not formally tested in the present paper. Like Mazumder (2005) we interpret the influence from the length of the fathers' earnings window as an indication of bias stemming from persistence in the transitory earnings component. Hence, there are (at least) two sources of bias to take into account in the estimation of the IGE, of which life-cycle bias appears to be the more important in our case.

Our IGE estimates are higher than those reported in recent Norwegian research, e.g., Bratberg et al. (2005) and Bratsberg et al. (2007). For fathers with earnings measured in their early forties and offspring in their late thirties—the period in life where, according to recent

[26] Not even with a period of 15 years this effect seems to be exhausted: there is an additional positive effect of extending the period to 25 years, although the age of the fathers in the latter case is higher than recommended.

research, the age and/or life-cycle bias appears to be least of a problem—we find estimated IGEs of 0.282 and 0.186 for sons and daughters, respectively, when the earnings are based on 5-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 5-year averages. The upward correction of the intergenerational earnings persistence is also the tendency in recent analysis based on US data. Hence, in relative terms Norway is still a country characterized by high intergenerational earnings mobility.

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Table 1: Intergenerational earnings mobility estimates for sons and daughters
Length of time span constant, increasing fathers' age (earnings from 1967-71 to 1982-86)

	Sons-fathers' elasticities			Daughters-fathers' elasticities		
	Coef.	Std. err.	Fathers' av. age	Coef.	Std. err.	Fathers' av. age
Fath earn 1967-71	0.3383	0.0074	36.1	0.2300	0.0089	36.1
Fath earn 1972-76	0.2817	0.0062	41.1	0.1864	0.0076	41.1
Fath earn 1977-81	0.2534	0.0060	46.1	0.1640	0.0073	46.1
Fath earn 1982-86	0.1632	0.0043	51.1	0.1173	0.0052	51.0
Cohorts of fathers	1927-1942			1927-1942		
Nbr of observations	57510			53481		

Notes: Dependent variable is average of children's log earnings for age 36-40. 1959-62 birth cohorts
Fathers' log earnings measure: 5-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. same individuals observed in all time period within the table.
Only years with earnings > 0 are included. Five-year averages for fathers are based on at least 3 years with earnings > 0.

Table 2: Intergenerational earnings mobility estimates for sons and daughters
Expanding earnings window, and age (earnings starting in 1967)

Upper panel	Sons-fathers' elasticities			Daughters-fathers' elasticities		
	Coef.	Std. err.	Fathers' av. age	Coef.	Std. err.	Fathers' av. age
Fath earn 1967-71	0.3356	0.0070	36.1	0.2277	0.0085	36.1
Fath earn 1967-76	0.3412	0.0068	38.6	0.2304	0.0082	38.6
Fath earn 1967-81	0.3406	0.0067	41.1	0.2280	0.0081	41.1
Fath earn 1967-86	0.3203	0.0062	43.5	0.2171	0.0075	43.4
Cohorts of fathers	1927-1942			1927-1942		
Nbr of observations	60408			56193		

Lower panel	Sons-fathers' elasticities			Daughters-fathers' elasticities		
	Coef.	Std. err.	Fathers' av. age	Coef.	Std. err.	Fathers' av. age
Fath earn 1967-71	0.3223	0.0138	31.1	0.1787	0.0187	31.1
Fath earn 1967-76	0.3399	0.0156	33.6	0.2061	0.0189	33.6
Fath earn 1967-81	0.3568	0.0158	36.1	0.2159	0.0190	36.1
Fath earn 1967-86	0.3520	0.0150	38.5	0.2099	0.0179	38.5
Fath earn 1967-91	0.3223	0.0138	40.9	0.1893	0.0165	40.9
Fath earn 1967-96	0.2878	0.0129	43.1	0.1798	0.0154	43.1
Cohorts of fathers	1936-1942			1936-1942		
Nbr of observations	15977			14878		

Notes: Dependent variable is average of children's log earnings for age 36-40. 1959-62 birth cohorts.
Fathers' log earnings measure: increasing averages 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. same individuals in all time period within each panel, but separately for upper and lower panel, respectively.
Only years with earnings > 0 are included. Five-year averages for fathers are based on at least 3 years with earnings > 0. Corresponding requirements of positive earnings when expanding to 10, 15, 20, 25, and 30 observations are 5, 8, 10, 13, and 15 years.

Table 3: Intergenerational earnings mobility estimates for sons and daughters
Increase length of window and keep age constant (earnings centered in 1974, and 1979)

Upper panel	Sons-fathers' elasticities			Daughters-fathers' elasticities		
	Coef.	Std. err.	Fathers' av. age	Coef.	Std. err.	Fathers' av. age
Fath earn 1972-76	0.2631	0.0056	41.1	0.1746	0.0074	41.4
Fath earn 1970-79	0.3040	0.0061	41.6	0.2019	0.0077	41.6
Fath earn 1967-81	0.3429	0.0066	41.1	0.2271	0.0080	41.1
Cohorts of fathers	1927-1942			1927-1942		
Nbr of observations	60867			56567		

Lower panel	Sons-fathers' elasticities			Daughters-fathers' elasticities		
	Coef.	Std. err.	Fathers' av. age	Coef.	Std. err.	Fathers' av. age
Fath earn 1977-81	0.2348	0.0054	46.1	0.1488	0.0066	46.1
Fath earn 1975-84	0.2565	0.0055	46.6	0.1658	0.0067	46.5
Fath earn 1972-86	0.2803	0.0057	46.0	0.1852	0.0069	46.0
Cohorts of fathers	1927-1942			1927-1942		
Nbr of observations	60522			56232		

Notes: Dependent variable is average of children's log earnings for age 36-40. 1959-62 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. same individuals in all time period within each panel, but separately for upper and lower panel, respectively.
Only years with earnings > 0 are included. Five-year averages for fathers are based on at least 3 years with earnings > 0. Corresponding requirements of pos. earnings when expanding to 10 and 15 observations are 5 and 8 years.

Table 4: Intergenerational earnings mobility estimates for sons and daughters
Increase length of window and keep age constant (earnings centered in 1979, and 1984)

Upper panel	Sons-fathers' elasticities			Daughters-fathers' elasticities		
	Coef.	Std. err.	Fathers' av. age	Coef.	Std. err.	Fathers' av. age
Fath earn 1977-81	0.2101	0.0115	41.1	0.1319	0.0141	41.1
Fath earn 1975-84	0.2382	0.0119	41.6	0.1552	0.0066	41.6
Fath earn 1972-86	0.2693	0.0125	41.0	0.1730	0.0149	41.0
Fath earn 1970-89	0.2752	0.0125	41.5	0.1791	0.0150	41.4
Fath earn 1967-91	0.2952	0.0131	40.9	0.1879	0.0157	40.9
Cohorts of fathers	1936-1942			1936-1942		
Nbr of observations	16470			15320		

Lower panel	Sons-fathers' elasticities			Daughters-fathers' elasticities		
	Coef.	Std. err.	Fathers' av. age	Coef.	Std. err.	Fathers' av. age
Fath earn 1982-86	0.1616	0.0092	46.1	0.1063	0.0106	46.1
Fath earn 1980-89	0.1872	0.0100	46.5	0.1203	0.0115	46.5
Fath earn 1977-91	0.2169	0.0108	46.0	0.1350	0.0126	46.0
Cohorts of fathers	1936-1942			1936-1942		
Nbr of observations	15931			14821		

Notes: Dependent variable is average of children's log earnings for age 36-40. 1959-62 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. same individuals in all time period within each panel, but separately for upper and lower panel, respectively.
Only years with earnings > 0 are included. Five-year averages for fathers are based on at least 3 years with earnings > 0. Corresponding requirements of pos. earnings when expanding to 10, 15, 20, and 25 observations are 5, 8, 10, and 13 years.