Reassessing intergenerational mobility in Germany and the United States: the impact of differences in lifecycle earnings patterns

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Using longitudinal data on fathers and their children, this study compares the extent of intergenerational mobility in Germany and the United States and introduces an estimation strategy that corrects estimates of intergenerational earnings elasticities for a possible lifecycle bias. In contrast to previous studies, we find that the extent of intergenerational mobility is more limited in the U.S. than in Germany. Furthermore, while the errors-in-variables problems have been dealt with extensively in the literature, the inconsistencies in standard mobility measures due to lifecycle effects have attracted much less attention. The present paper proposes an estimation method that corrects for such inconsistencies. The extent of this lifecycle bias is found to be strong in Germany but only modest in the U.S. **Keywords:** Intergenerational mobility, lifecycle bias, comparison of Germany and the U.S.
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1 Introduction

At least since the times of the French Revolution it has become a widely accepted belief in Western and most other countries that advancements within a society's social hierarchy should not, or only to a minor degree, depend on descent but on personal attitudes and capabilities. In economics the question whether a society is "open" or whether its class boundaries are rather "tight" is studied using the capacity to earn a high income as proxy for an individual's social ranking. From this perspective the intergenerational elasticity (IGE) of earnings, that is, the correlation of log lifetime earnings between, say, fathers and sons gives valuable insights about the openness of a society and also allows for a comparison of the functioning of societies over time and space.[1] IGE estimates are usually found to be around 0.4 or somewhat higher in the U.S., between $0.4 - 0.5$ in Britain and, at the lower end of the distribution, around 0.2 or somewhat lower in Scandinavian countries (Solon 2002, Mazumder 2005, Dearden, Machin and Reed 1997, Bratsberg, Røed, Raaum, Naylor, Jäntti, Eriksson and Österbacka 2007).

When attempting to estimate correlations of lifetime earnings, however, the researcher faces the problem that earnings are usually observed only over relatively short time periods. In fact, the large number of different estimates for the intergenerational elasticity of earnings, even when using the same data, can be traced back to the problem that there is no silver bullet to obtain lifetime earnings from an incomplete history of annual earnings. Using such "snapshots" as proxies for lifetime earnings has its pitfalls of which there are at least three. First, some assumptions need to be made concerning the lifecycle earnings profiles to impute missing earnings observations. The standard assumption in the literature, following Solon (1992) and Zimmerman (1992), is that age effects are captured by a polynomial of age where this functional form is identical for all persons in the sample. The present paper argues that such models are likely to be misspecified and that the estimates based on them are downward biased. The reason for this is simply that there are good reasons to presume that the steepness of the age-earnings profiles is strongly linked to lifetime earnings. University graduates, for instance, exhibit strong earnings growth in early stages of their careers while receiving in total relatively high earnings. This point was already raised in Jenkins (1987) and more recently further elaborated by Haider and Solon (2006) and Grawe (2006). It should be stressed that the downward inconsistency caused by the described misspecification of the model does not go away as

[1] The distinction between income and earnings is crucial in this context because, it appears again to be common belief, the bequest of wealth does not by itself oppose the general notion of openness; however, when going along with unequal chances to earn a good (labour) income, it does. So the central question is how strongly lifetime *earnings* of family members are correlated.

more and more observations of fathers' and sons' earnings become available. It would even persist if the process generating annual earnings was deterministic—in contrast to the famous attenuation bias in this literature which is caused by the stochastic nature of the income generating process (Solon 1989).

The second problem with the usage of "snapshots" is that these snapshots are of very different quality over the course of the lifecycle. For instance, earnings are particularly low during periods when human capital accumulation is the strongest. Students in particular report extremely low earnings while still attending university (possibly because they only work between terms), but we know that on average they will earn relatively high incomes in the years to come. Thus, for the same reason sketched earlier, studies that use a large number of very young sons in their sample can be expected to report downward biased estimates of the intergenerational correlation of earnings. Although related to the first mentioned problem, it is worth listing it separately because, first, the inclusion of very young sons in the sample seems to be the cause for the comparably small estimates of the intergenerational earnings elasticity in some studies (in particular Couch and Dunn 1997) and, second, because strategies to cope with both problems are different.

The third problem with short earnings spells, finally, is that the variability of earnings may change over the lifecycle. Baker and Solon (2003) report a U-shaped relationship between age and the variance of earnings. This would imply that the prevalent and in stochastic models unavoidable attenuation bias of intergenerational earnings elasticities would be more severe when observing individuals at very early or very late stages of their lifecycle because the precision of lifetime earnings estimates goes down as the variability of observed annual earnings increases. This could explain why studies using older samples of fathers find lower intergenerational earnings persistence (Grawe 2006). Still, the bias induced by using relatively old fathers would in principle fade out as more and more observations per father become available, which is not true if the model is misspecified, as argued earlier.

The present paper adds to the literature on intergenerational earnings elasticity in two ways. First, we estimate intergenerational earnings elasticities while explicitly allowing different skill groups to have different wage growth over the lifecycle, thus eradicating a possible lifecycle bias.[2] These estimates are then compared with estimates of a standard

[2]In a complementary effort to purge IGE estimates of a possible lifecycle bias, Dahl and DeLeire (2006) use and confirm findings in Böhlmark and Lindquist (2006) and Haider and Solon (2006) that individuals' annual earnings best proxy for lifetime earnings when they are observed during their mid 30s. The present paper, in contrast, presumes that the lifecycle bias is caused by the positive correlation of the steepness of earnings growth over the lifecycle and the individual permanent

Solon (1992) model to gauge the magnitude of the lifecycle bias. Second, as do others (for example Couch and Dunn (1997) and more recently Bratsberg et al. (2007)), we apply the same estimation method on data on several countries, here Germany and the U.S. This allows us to compare the openness of both societies and at the same time to gain some insights into the sensitivity of our estimation approach.

The data we use in this study comes from two widely-used data sets, the German Socio-Economic Panel (GSOEP) and the Panel Study of Income Dynamics (PSID). While still ignoring lifecycle effects and following also otherwise closely Solon (1992), we obtain estimates of the IGE of earnings of 0.25 in Germany and 0.43 in the U.S. These estimates are considerably higher than the estimates of 0.11 and 0.13 for Germany and, respectively, the U.S. reported in the cross-country study of Couch and Dunn (1997). This difference most likely stems from the fact that our sample only contains employed, prime-aged men. Our estimates thus suggest a significant difference in the degree of openness of both societies. We then estimate lifetime earnings of both fathers and sons while still constraining earnings profiles of all skill groups to be identical. Using these estimates to compute the correlation between lifetime earnings of fathers and sons we obtain estimates of 0.27 for Germany and 0.37 for the U.S., the latter being only slightly below the "reasonable guess" of 0.4 found in Solon (1992) and Zimmerman (1992). Allowing wage growth to be different for different skill groups we estimate earning elasticities of 0.33 in Germany and 0.38 in the U.S. We thus find that due to the strong wage increase during the first ten years of the typical university graduate in Germany, the obtained estimate of intergenerational mobility is downward biased by about one-fifth, whereas in the U.S. the lifecycle bias is found to be negligibly small.

In contrast to earlier studies using GSOEP and PSID data, these data sets have matured considerably. This allows to be more restrictive with respect to the father-son sample used in the estimations. We therefore also re-estimate intergenerational earnings elasticities while demanding that earnings of fathers and sons of each father-son match are observed a fairly large number of at least ten times. This should reduce the attenuation bias. For this subsample we compute intergenerational earnings elasticities of 0.34 in Germany and 0.39 in the U.S. when pooling all skill groups, and of 0.36 in Germany and 0.39 in the U.S. when allowing for different age-earnings profiles. These results could be interpreted as suggesting that prior to this confinement the attenuation bias was still strong in the German sample, where the average number of observations per person is much lower than in the PSID sample, whereas in the U.S. sample it had

component. Being more specific about the cause of the lifecycle bias, our approach also allows to derive testable predictions about the direction of the bias.

already been fairly small in the larger, less restrictive sample.

However, imposing restrictions of that kind on the father-son sample runs the risk of inducing a selection bias (due to non-random sample attrition) which needs to be traded-off against the attenuation bias. The so restricted father-son sample is possibly not representative any more of all father-son pairs in the population because it seems plausible that fathers and sons with strong family ties remain in the sample over long time periods while those with weak ties leave the sample (see also Couch and Lillard (1998) on a discussion of sample selection rules in this literature). To learn about presence and possibly the magnitude of both selection and attenuation bias, we conduct a series of experiments by drawing at random exactly five observations from all available observations on fathers and sons in the sample and then for each draw compute the intergenerational earnings elasticity. Comparing the so obtained estimates with those reported earlier, we find evidence for a positive sample selection bias because the computed lifetime earnings correlations of fathers and sons remain on average larger in the smaller, more restrictive sample than in the larger, less restrictive sample. This finding should make us sceptical whether measures of intergenerational mobility can be expected to increase in precision as the underlying data sets mature further.

The structure of the paper is as follows. Section 2 describes the main estimation strategy of this paper. In this section we also discuss the interpretation of the standard log-linear relationship between lifetime earnings of fathers and sons because we believe the interpretation suggested for example in Solon (1999) misses some important features of human capital and should be modified. This section also discusses the expected direction of the lifecycle bias induced by the misspecification of the age effects in standard models. Section 3 describes the data. Section 4 briefly describes how the estimation strategy is implemented and thereby prepares for section 5 which presents the estimation results and discusses their interpretation. The main results of this paper can be found in Table 1. We check for robustness of these results and conduct the aforementioned experiments in section 6. Section 7 concludes.

2 Econometric Model and Direction of the Bias

Because of the strong link of income with consumption and welfare, measuring the intergenerational mobility in income is of direct interest to economists. Concentrating on father-son relationships, a popular way to link both the lifetime incomes of fathers

(Y_i^{father}) and sons (Y_i^{son}) is

$$\log Y_i^{\text{son}} = \alpha + \beta \log Y_i^{\text{father}} + \varepsilon_i \tag{1}$$

where ε_i is a white-noise error term and the index i denotes family or dynasty i. In this specification the coefficient β measures the elasticity of a son's lifetime income with respect to his father's lifetime income.[3]

A positive correlation of total incomes within families is suggested by the famous Ramsey-Cass-Koopmans model which assumes perfectly altruistic agents. Variants of the stochastic version of this model can be found in Becker and Tomes (1979, 1986) where it is stressed that parents usually invest into human capital of their children rather than bequeathing other forms of assets. Nonetheless, this strand of the literature presumes that parents can invest *any amount* into the future of their offspring, thus abstracting from the inherent finiteness of opportunities to invest into skill and education.[4] Therefore, a relation between *incomes* of fathers and sons, as suggested by equation (1), is more plausible when interpreting incomes very broadly, including asset incomes.

In the theoretical literature on intergenerational mobility, however, both Y_i^{father} and Y_i^{son} are usually more narrowly interpreted as *labour earnings*. Explanations for a positive correlation of within-family labour earnings that explicitly assume a finite number of professions usually draw on the finding that capital markets are imperfect or even completely missing (e.g., Galor and Zeira 1993, Freeman 1996, Ljungqvist 1993, Mookherjee and Ray 2003, Mookherjee and Ray 2002). Imperfect capital markets imply that training may be more costly (in utility terms) for the poor than for the rich which can result in imperfect equalisation of lifetime labour earnings. More recently it has been shown that similar results can be obtained even with perfect capital markets. For example, poor families can have a relatively low incentive to invest into training of their children if during schooling a minimal standard of living needs to be attained (Funk and Vogel 2003) or if some goods (e.g., consumption goods or prestige of occupations) are only imperfectly divisible (Funk and Vogel 2006).

In this paper we follow most of the literature (cited for example in Becker and Tomes 1986, Solon 1999, Solon 2002, Björklund and Jäntti 1997, Grawe 2006) and estimate

[3] Most of the literature assumes that β is constant (as we do), but there have been attempts to allow for a non-linear relationship between Y_i^{son} and Y_i^{father}. See for instance Bratsberg et al. (2007) and Dahl and DeLeire (2006).

[4] In Becker and Tomes (1986) for instance, acknowledging that investment opportunities into human capital of the offspring are finite, all parents invest identical amounts into human capital if capital markets are perfect and for small investments return on investment in human capital exceeds return on investment in physical capital.

the correlation between lifetime *labour earnings* of fathers and sons. Lifetime labour earnings of a member of family i born in period b (be it a father or a son) who enters and leaves the labour market at age T^{entry} and respectively T^{exit} can be expressed as

$$Y_{ib} = \int_{b+T^{\text{entry}}}^{b+T^{\text{exit}}} e^{-r(t-b-25)} Y_{ibt} dt \tag{2}$$

where r is the (constant) discount rate and Y_{ibt} denotes this person's period t earnings. We discount to the age 25 because this will be the earliest age for which we use the available observations. For notational convenience write Y_{ib}^{25} for annual earnings of member b of family i when he is 25 years old. Period t earnings can always be written as

$$Y_{ibt} = Y_{ib}^{25} \times e^{g_{ibt}(t-b-25)}$$

where g_{ibt} denotes the average growth rate of earnings over the interval $(b + 25, t)$. Inserting this expression into (2) and taking logs yields

$$\log Y_{ib} = \log Y_{ib}^{25} + \log \int_{b+T^{\text{entry}}}^{b+T^{\text{exit}}} e^{(g_{ibt}-r)(t-b-25)} dt = \log Y_{ib}^{25} + \phi_{ib} \tag{3}$$

By definition, the variable ϕ_{ib} depends on g_{ibt} and thus on the overall as well as on skill group specific wage growth in period t.

In the literature income at the reference age (here 25) comes under many different names, for example "adjusted current status" (Zimmerman 1992), "permanent component" reflecting the "true long-term earnings capacity" (Mazumder 2005), or "'permanent' component of log annual earnings" (Solon 1992), just to mention a few. The important point to stress here is that when using income at the reference age ($\log Y_{ib}^{25}$) as a proxy for lifetime income ($\log Y_{ib}$), in general the obtained estimate $\hat{\beta}$ is inconsistent. In fact, consistency is in general obtained only as long as ϕ_{ib} is identical within the group of fathers and within the group of sons. If in contrast different skill groups exhibit different age-earnings profiles, then the terms ϕ_{ib} are not identical and regressions of the obtained permanent components of sons on that of their fathers will yield inconsistent estimates for β because ϕ_{ib} and $\log Y_{ib}^{25}$ are expected to be correlated. Furthermore, because of the latter, ignoring differences in skill specific age-earnings profiles will also result in inconsistent estimates of individual permanent components (technically speaking, of the personal fixed effects, $\log Y_{ib}^{25}$).

To be more specific, consider the standard practise to estimate the permanent component (see for instance Zimmerman 1992). The income generating process is usually modelled as

$$\log Y_{ibt} = \alpha_0 + \log Y_{ib}^{25} + \mathbf{X}_{ibt}\alpha + \nu_{ibt} \tag{4}$$

where the errors ν_{ibt} are mean independent of both the permanent components $\log Y_{ib}^{25}$ and the other covariates $\mathbf{X}$ (usually a polynomial in age).[5] Using unbiased estimates for the αs, $\hat{\alpha}$ and $\hat{\alpha}_0$, unbiased estimates of permanent components are obtained via taking averages:

$$\widehat{\log Y_{ib}^{25}} = \overline{\log Y_{ibt}} - \hat{\alpha}_0 - \overline{\mathbf{X}_{ibt}}\hat{\alpha}$$

When only few observations are available per person or if there is strong autocorrelation of the error terms ν_{ibt}, the obtained estimates of the permanent component may be quite imprecise, resulting in downward inconsistent estimates of β when using these estimates of the permanent components (see, e.g., Solon 1989, Solon 1992, Björklund and Jäntti 1997).

Now suppose that model (4) is misspecified because it falsely constrains α to be identical for all skill groups. Then, firstly, estimates of the permanent components are quite likely inconsistent and secondly, and *a fortiori,* the ϕ-terms are falsely assumed to be identical. The second argument shows that it is not sufficient to simply adjust model (4) by lifting the constraint that α is identical for all skill groups.[6] One also needs to properly adjust lifetime earnings. In fact, without the latter things may well become worse, not better.

Direction of the bias If equation (4) is misspecified in that α is (falsely) assumed to be identical for all person while in the true data generating process age-earnings profiles are steeper for persons from skill groups with on average high lifetime earnings, then this will induce a bias on the obtained IGE estimate. Consider two representative individuals born in period 0, one of which is high-skilled and the other is low-skilled. Both enter the labour market at age 25. Panel (a) of Figure 1 depicts the lifecycle earnings profiles of these two persons where wage growth is assumed to be constant but not identical. Instead, we let earnings growth be steeper for the skilled than for the unskilled person. Knowledge of both the income in the base period ("permanent status") and the growth rate of wages allows us to compute lifetime earnings of both persons.

Figure 1 about here

[5] Notice that this kind of model does not allow to identify age or experience effects if age or experience interacts with the skill level. So in these instances the model is in fact a stripped-down version of a Mincer wage equation.

[6] To my knowledge Minicozzi (2003) is the only study that allows for group specific earnings profiles.

Notice that it is always possible to construct an earnings profile that yields identical lifetime earnings for the skilled person but with the relatively low wage growth of the unskilled person if we suitably adjust the skilled person's annual earnings at the beginning of his lifecycle. In the figure such a hypothetical earnings profile is indicated by the dashed line. The distance between both parallel wage curves reflects the difference in lifetime earnings between the two persons. The figure shows that this distance is understated (overstated) when using annual earnings of very young (old) individuals.

Panel (b) of Figure 1 shows the resulting direction of the bias of $\hat{\beta}$ when ignoring these differences in earnings growth of both (groups of) persons. In the graph it is assumed that sons are only observed shortly after entering the labour market and fathers only shortly before leaving it. The dashed line depicts the regression line when not correcting for lifecycle differences in earnings while the solid line shows the true relationship between lifetime earnings of fathers and sons. Since the difference in lifetime earnings of skilled and unskilled fathers (sons) is over(under)estimated, the slope of the dashed regression line unambiguously understates the true correlation between fathers' and sons' lifetime earnings. Adding the correct ϕ to the permanent earnings of each individual (as indicated by the arrow) corrects for this bias.

3 Data

We use two different, fairly standard original data sets, the German Socio-Economic Panel (GSOEP) for Germany and the Panel Study of Income Dynamics (PSID) for the U.S. (as does Couch and Dunn 1997). The PSID began in 1968. Until 1997 interviews were conducted annually, since then biannually. The GSOEP started to interview individuals of selected households in 1984. Here, individuals are interviewed on an annual basis. The important feature of both data sets is that children of original households are followed when moving out from their parents' home and forming their own household. Both data sources include variables that allow to easily establish links between family members, thus making it possible to relate earnings variables of fathers and sons. A detailed description of the PSID can be found in Hill (1992) and of the GSOEP in SOEP Group (2001).

As for the U.S., we only use observations from the Survey Research Center (SRC) component of the PSID. With respect to Germany, we refrain from using data from individuals who used to live in East Germany prior to the fall of the Berlin wall in November 1989.[7] To limit measurement error of reported earnings, which may be severe

[7] In this study we are concerned quite generally with the openness of the German society. With the fall

in the early and late stages of the lifecycle, we only use observations on men who are between 25 and 60 years old.[8] For the same reason we also use only observations of men who report to be employed and not in education any more. The exclusion of young men and men out of the labour force (such as students enrolled in university) from the analysis seems to be the crucial difference in the construction of our sample and that used in Couch and Dunn (1997).

The U.S. earnings variable we use covers non-imputed wage and salary earnings of the head of the household which is reported in the PSID in all waves 1970-2005. For Germany our income measure comes from the monthly calendar information on wage and salary payments of employed workers. Earnings are aggregated into yearly earnings to which we add reported bonus payments. This measure of annual labour earnings can be computed in all currently available waves 1984-2005. Following Couch and Dunn (1997) we drop observations with earnings less than 100 real dollars, respectively, Euros. In the PSID data we also drop observations that are reported to be censored, but at extremely large censoring bounds (1 million dollars between 1988 and 1993 or, as from 1994, 10 million dollars).

Education qualification in both Germany and the U.S. are aggregated into four groups. In the U.S. we group individuals into high school drop-outs, high school graduates, people with some college, and college graduates. In Germany the grouping follows naturally from the German education system: men without vocational training, with vocational training, with further higher education[9], and with a degree from university or technical college. For the computation of the ϕ-terms we make the assumption that men in the U.S. enter the labour market (T^{entry}) at the age of 20 and men in Germany at the age of 21. In both countries all men are assumed to leave the labour market (T^{exit}) at the age of 60.

All earnings data in this study are deflated to year 2000 prices using the Consumer Price Index for each country. To discount annual earnings we use the average inflation-adjusted Treasury Bill Rate of the years 1984-2005 for r which is 2.1 per cent in the U.S. and 2.6 per cent in Germany.

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of the iron curtain chances to rise in the income ladder increased dramatically for people from the former East Germany (especially for the young migrating to the West) such that this single event is expected to seriously confound our estimates. We therefore use data exclusively from West Germans.

[8] Notice that this last restriction does not render it impossible to gauge difference in lifetime earnings that are due to entering the labour market early. The specification of the income-generating function still allows to infer incomes of men below 25.

[9] Many of this group are civil servants who have flatter earnings profiles than university graduates. So it does not seem adequate to merge this group with the group of university graduates (see Figure 2).

4 Estimation Strategy

Insertion of (3) and (4) into (1) yields

$$\log Y_{it}^{\text{son}} = \alpha - \phi_i^{\text{son}} + \alpha_0^{\text{son}} + \beta \log Y_i^{\text{father}} + \mathbf{X}_{it}^{\text{son}} \alpha^{\text{son}} + \varepsilon_i + \nu_{it}^{\text{son}} \tag{5}$$

$$= \left(\alpha - \phi_i^{\text{son}} + \alpha_0^{\text{son}} - \beta \alpha_0^{\text{father}} + \beta \phi_i^{\text{father}}\right) + \beta \log Y_{it}^{\text{father}} + \mathbf{X}_{it}^{\text{son}} \alpha^{\text{son}} \tag{6}$$

$$- \beta \mathbf{X}_{it}^{\text{father}} \alpha^{\text{father}} + \varepsilon_i + \nu_{it}^{\text{son}} - \beta \nu_{it}^{\text{father}}$$

Specification (6) of the model thus differs from standard specifications (for instance Solon 1992) only in that it includes two ϕ-terms. If age-earnings profiles are identical however, they enter the constant and do not affect estimation of β. Therefore, estimates of (6) serve as our benchmark since they allow easy comparison of our results with those reported in the literature.[10] We then estimate equation (5) and (1) which requires as a first step to estimate lifetime earnings of fathers and sons (the latter only to estimate (1)). Having obtained estimates of lifetime earnings in step-1 it is straightforward to estimate (5) and (1) as step-2.

To obtain estimates of lifetime earnings we need to estimate both individual earnings at the reference age and the ϕ-terms. Estimating the ϕ-terms in turn requires estimation of the complete lifecycle earnings profile. Since this is not observed in the data we make the identifying assumption that earnings profiles of both sons and fathers are identical. Presuming this is true, this allows us to infer from the observed earnings profiles of the sons (fathers) when they are young (old) on the unobserved earnings of the fathers (sons) when they were (will be) young (old). To correct for cohort effects, we include a linear time trend.[11] The earnings or income generating function can then be written as

$$\log Y_{ibt} = \alpha_0 + \log Y_{ib}^{25} + \alpha_1 A_{ibt} + \alpha_2 A_{ibt}^2 + \alpha_3 A_{ibt}^3 + \alpha_4 A_{ibt}^4 + \gamma t + \nu_{ibt} \tag{7}$$

where, as assumed earlier, the error term is uncorrelated with the covariates.

Making this assumption on the functional form of the earnings function, observations from *all* men in the data, not only of man for which a father-son match could be established, can actually be used in the estimation of (7). That is to say, the fact that

[10] Notice that we model age effects as a fourth-, not a second-order polynomial because of the evidence presented in Murphy and Welch (1990).

[11] This functional form assumption might appear extremely restrictive, especially in the light of empirical studies that find large shifts in the remuneration of younger cohort (Card and Lemieux 2001). However, using five-year intervals to aggregate cohorts and using dummy variables to indicate these groups does not affect our first step estimations by very much. We therefore use the simple linear form.

for most men in the data a father-son link cannot be established does not invalidate the assumption that also for these men equation (7) describes the statistical process generating their annual earnings. Using data of all men who are comparable to persons for which such a father-son link *can* be established, estimates $\hat{\alpha}_1$, $\hat{\alpha}_2$, $\hat{\gamma}$ can be expected to be measured more precisely and, hence, this should also raise the precision of our estimates of lifetime earnings of fathers and sons. It should however be borne in mind that our estimate of the permanent component $\log Y_{ib}^{25}$, though unbiased, might still be quite imprecise, which is unavoidable and due to the limited length of the panel.

Finally, notice that we effectively use *estimates* of lifetime earnings to obtain $\hat{\beta}$ in step-2 when estimating (1) and (5). It is well understood that such two-step estimators of the coefficients are consistent, but that the reported, uncorrected step-2 standard errors are not (see Pagan 1986, Newey and McFadden 1994). We therefore use the bootstrap (with 400 replications) to compute standard errors of all two-step estimates.

5 Empirical Results

Table 1 reports the main estimates of this paper. The upper panel of the table presents the results obtained from GSOEP data, whereas the lower panel contains PSID estimates. In each row we report OLS estimates of different model specifications and of different samples of fathers and sons. The first row of each panel (labelled 'pooled') shows estimates when earnings growth over the lifecycle is constrained to be identical for all skill groups. The second row of each panel ('unconstrained') shows the respective estimates when relaxing the latter assumption and allowing instead different skill groups to exhibit different earnings growth over their lifecycle. So, the main insights to be gained from inspection of Table 1 is by comparing estimates in the first and second row of each panel ('pooled' vs 'unconstrained') as well as by comparing estimates for both countries.

Table 1 and Table 2 about here

Pooled estimates We begin with a discussion of the estimation results of the standard or benchmark model displayed in equation (6). The estimates are reported in column (1) of Table 1. Here we follow the literature and use observations of sons only in one given year, while for fathers we use averages over a five-year period. For sons we use observations in the year 2003 and for fathers observations over the period 1984-1988, because the difference between both periods amounts to roughly one generation difference and because 1984 is the first year for which German data are available. For this set of years we obtain an estimate of 0.43 for the U.S. data, which is extremely close to the "reasonable guess" of 0.4. For Germany we compute $\hat{\beta}$ to be 0.25. These estimates suggest that opportunities to rise and fall in the German society are significantly greater than they are in the U.S.

Comparing these numbers with those reported in Couch and Dunn (1997), which are 0.12 in both the GSOEP and the PSID sample, the difference is striking. There seem to be two causes that drive these results. The first is that the year 1984 is used by Couch and Dunn also to observe the earnings of sons, while we use sons' earnings in the year 2003 so as to put a one-generation difference between the observations of fathers and sons. The second reason is that we exclude from the analysis men below 25 and men still in education. These differences in the selection of the sample become apparent when comparing the age distribution in our sample with that reported in Couch and Dunn. Columns superscribed (1) in Table 2 summarise the GSOEP and PSID samples used in our estimation of this benchmark model (column (1) of Table 1). While in our GSOEP sample fathers (sons) are on average 43 (34) years old, in Couch and Dunn (1997) they are on average 51 (23) years old. In the PSID sample the differences are similar. Fathers (sons) in the U.S. in our sample are on average 42 (35) years old, while in Couch and Dunn (1997) they are on average 53 (25) years old. Particularly because of the very young sample of sons, our arguments laid out earlier suggest that their estimates might be strongly downward biased and our estimates may be closer to the truth.

The next two columns of Table 1, (2a) and (3a), report $\hat{\beta}$ when using model specification (5) and, respectively, (1) while maintaining the same father-son sample used to obtain the results displayed in column (1). Both specifications (5) and (1) use step-1 estimates of lifetime earnings of fathers (equation (5)) or of fathers and sons (equation (1)) in the estimation of β. Estimated coefficients of our step-1 estimations are not reported here but Figure 2 plots the predicted age-earnings profile for a typical worker of each of the four skill groups. As argued earlier, we use a much larger data set in our step-1 estimations.[12] First, we do not confine ourselves at that stage to men for which father-son matches could be established but use all males which meet our selection criteria (employed, between 25 and 60 and not in education any more). Second, for each

[12]The number of observation used to compute lifecycle earnings patterns shown in Figure 2 are as follows: In the GSOEP data the number of men with lower secondary education, vocational training, higher vocational training and those with a university degree are 976, 2,441, 458 and 967, respectively. So in total we use 50,065 observations of 4,842 men to estimate the plots. Differences in the base year (25) come from differences in estimated person individual effects. In the PSID data the number of men used with less than a high school education, of high school graduates, men with some college education and men with a university degree are 1,423, 2,238, 1,465 and 1,451, respectively. In total, 83,329 observations of 6,577 men are used to generate the U.S. plots.

person we use all available observations (while he is between 25-60, employed and not in education), not just a maximum of five observations. Still, the number of father-son matches in column (3a) used to estimate (1) is smaller because for a number of sons (who we observe in 2003) we have less than five observations.

Figure 2 about here

However, for those fathers and sons for which at least five observations are available, we actually have many more than five so that, when used to estimate permanent earnings, the attenuation bias should be reduced. Columns superscribed as (4a) in Table 2 give an idea of the number of observations per father and son used to obtain the estimates reported in columns (2a) and (3a)—though strictly speaking, they refer to the sample used to compute the estimates reported in column (4a) of Table 1. As we can see from Table 2 is the average available number of observations on fathers in the PSID about 17, while we have on average 14 per son. In contrast, in the GSOEP the average number of observations on fathers is around 12 while it is around 10 for sons. These numbers suggest that we should observe a significant reduction of the attenuation bias when using the full set of observations on fathers in the U.S. estimate of β, while the reduction of the attenuation bias in the German data should be less pronounced. Comparing estimates reported in columns (1) and (2a) this is actually what we find in the U.S. data. In the PSID sample our estimate is 0.54 and, thus, the estimate in column (2a) is about 0.11 points larger than that reported in column (1). In contrast, in the German data both estimates are almost indistinguishable with a value of around 0.25. Using also step-1 estimates of lifetime earnings for sons, the computed $\hat{\beta}$ reported in column (3a) of Table 1 is greater than that reported in column (2a) for both the U.S. and the German sample.

Based again on the model specification (1), we report in column (4a) our estimates of β when using a larger sample of fathers and sons. Here we use step-1 estimates of lifetime earnings of all fathers and sons which are observed at least five times—though not necessarily in 2003 (sons) or over the full period 1984-1988 (fathers). Lifting these latter restrictions boosts considerably the size of our (step-2) father-son sample. In this extended sample we obtain an estimate of β of 0.27 in Germany and of 0.37 in the U.S. Thus, using a larger set of father-son matches, in particular using fathers' and sons' observations which may be less than twenty years apart, the computed $\hat{\beta}$ in column (4a) are strikingly lower than in the smaller, more restrictive sample used for column (3a). Yet, one of the main conclusions of this paper is still sustained: The degree of openness is considerably smaller in the U.S. than in the German society.

Unconstrained estimates Let us turn next to a description of the results when age-earnings profiles are not any more constrained to be identical for all skill groups, which are reported in the rows labelled 'unconstrained' in Table 1. Comparing pooled and unconstrained results for the German sample, reported in columns (2a)-(4a), we observe a considerable increase of the estimated $\hat{\beta}$ when lifting the restriction that all skill groups exhibit identical age-earnings patterns over the lifecycle. For instance, in the large sample used for column (4a) the estimate from the 'pooled' specification was 0.27. But once we abandon the restriction that age-earnings profiles are identical, we obtain an estimate of 0.33 which is about one-fifth higher. Also for the other two specification (columns (2a) and (3a)) we find increases of approximately similar magnitude. In contrast, in the U.S. sample the increase is basically negligible in all three specifications. For instance, in the large sample (column (4a)) $\hat{\beta}$ increases from 0.37 to only 0.38 and, thus, this increase is hardly detectable and certainly not statistically significant.

The reason for this dramatic difference between the effect of differences in the lifecycle earnings patterns on $\hat{\beta}$ is obvious from inspection of Figure 2. As noted, this figure depicts typical earnings patterns over the lifecycle of the four different skill groups in both countries. Thin lines show estimated age-earnings patterns when pooling observations of all four skill groups in each country, whereas thick lines report age-earnings profiles when allowing for different skill groups to exhibit different growth rates of earnings over the lifecycle. Thin lines in Figure 2 are hence parallel by construction. The figure shows two striking differences between the structure of earnings in the U.S. and Germany. The first is that earnings seem to be more compressed in Germany than in the U.S. In Germany estimated lifecycle earnings profiles are relatively close to each other, whereas in the U.S. they are comparatively far apart, suggesting that rewards to education are smaller in Germany than in the U.S. The second and for the present study more important observation from Figure 2 is that wage increases over the first ten years of university graduates are much stronger in Germany than in the U.S. In both countries university graduates exhibit a steeper rise in earnings at the beginning of their career than do men of the other three skill groups. But this rise in earnings between 25 and 35 is more extreme in Germany than it is in the U.S. Hence, we can expect the bias of our β estimates that is due to falsely pooling skill groups, to be more pronounced in the German than in the U.S. data. This is exactly what we find. While the bias of $\hat{\beta}$ when using PSID data is small, it is sizeable in the GSOEP data. One conclusion to be drawn from this finding is that it seems innocuous to ignore a possible lifecycle bias when estimating β to study the general "openness" of the U.S. society. For inference about the German society, however, it seems recommended to correct for the potential lifecycle bias.

Age dependence of intergenerational earnings elasticities The strategy in this paper to correct for lifecycle biases is to add skill-specific correction factors (the ϕ-terms, see equation (3)) to the estimated annual earnings at the reference age (here 25) in order to obtain unbiased estimates of lifetime earnings. A different way to eliminate the lifecycle bias would be to take out skill-specific age effects by resorting to the concept of a skill-specific reference age. Here the idea is to determine a skill-specific reference age (instead of choosing ad-hoc 25 as the reference age) such that adding the correction factors becomes superfluous—though we would still need to correctly condition on skill-specific earnings growth over the lifecycle.[13] Moreover, if we only used observations of men *of this reference age* we could even dispense with all age adjustments without inducing a lifecycle bias.[14] In this spirit, though not specifically dealing with the problem of skill-specific lifecycle earnings patterns, Böhlmark and Lindquist (2006) and Haider and Solon (2006) have recently presented evidence that annual earnings in the mid 30s of men proxy best for their lifetime earnings. Using also U.S. administrative data but a different estimation procedure than Haider and Solon (2006), Dahl and DeLeire (2006) confirm this finding. As it turns out, our data and our identifying assumptions allow us to reach similar conclusions. Going back to Figure 2, we see that in both countries and in all four skill groups thick and thin lines intersect when individuals are in their early 30s.

For high wage earners, who are predominantly high skilled, above the age of, say, 35 their annual earnings in general exaggerate their lifetime earnings while the opposite is true for low skilled persons who mostly earn low wages. Therefore, with fathers being almost always above 35 when their wages are observed in the survey, Figure 2 leads us to conclude that the estimated $\hat{\beta}$ should be the smaller, the higher the average age of the fathers in the sample. This is exactly what Grawe (2006) finds.[15] However while

[13]This approach would however not be more efficient (in the statistical or the computational sense) than the estimation strategy we pursue in this paper because it also requires estimation of lifetime earnings patterns to determine the correct reference age.

[14]This procedure thus would require to use only observations of men whose age is fairly close to the skill-specific reference age. Applying this estimation procedure hence forces the researcher to trade-off a possible lifecycle bias (due to observations further away from the reference age) against the attenuation bias (due to the limited number of observations then available to estimate the permanent component). If age-earnings effects are correctly modelled (or at least correctly approximated locally), in contrast, the attenuation bias can be reduced without inducing a lifecycle bias.

[15]Dahl and DeLeire (2006) report in their Table 6 that $\hat{\beta}$ actually *de*creases from 0.430 to 0.352 when using an on average younger sample of fathers. However, in the former estimation earnings of fathers are used while they are between age 30 and 50, whereas in the latter fathers' age is between 30 and 40. So, it is not clear how much of this drop is due to the attenuation bias since in the second estimation only half of the number of observations of fathers' earnings are used to form the estimates of fathers' lifetime earnings; in particular, since the other numbers in the same table show that the attenuation bias is of significant magnitude.

the explanation in Grawe is centred around the assumption that wage growth of sons exceeds that of their fathers, we base our argument on the finding that high-skilled persons have high lifetime earnings *and* high wage growth. Put differently, wage growth of sons exceeds wage growth of fathers *because* sons are observed early in the lifecycle while fathers are observed late in their lifecycle.

Small father-son sample A major improvement in the estimation of intergenerational earnings elasticities in the early 1990s (Solon 1992, Zimmerman 1992) was to reduce the downward inconsistency of $\hat{\beta}$ by averaging fathers' earnings (usually over five years). Compared with these studies many more waves of data have become available now and so it seems natural to average earnings over longer time intervals in order to reduce the attenuation bias. Moreover, the procedure employed in this paper to infer on permanent earnings of fathers and sons actually allows us to use *all* observations of individual earnings which is available in the data—if one is willing to entertain our identifying assumptions implied by equation (7).

We therefore re-estimate the model (step-1 and step-2) using only men for which we have at least *ten* valid earnings observations. The obtained estimates of β of this subsample are presented in columns (2b)-(4b) of Table 1. Comparing estimates in columns (4a) and (4b), we find a substantial increase in the estimated earnings elasticities in both the PSID and the GSOEP data when using this more restrictive subsample of fathers and sons. Pooling all four skill groups, in the German data $\hat{\beta}$ increases from 0.27 in the larger, less restrictive sample to 0.34 in the smaller, more restrictive sample. In the U.S. data the increase is less drastic, though still noteworthy. The IGE estimate goes up from 0.37 to 0.39. In our preferred specification of the model in which age-earnings profiles are skill group specific, $\hat{\beta}$ increases from 0.33 to 0.36 in the German data and from 0.38 to 0.39 in the U.S. data. Comparing estimates in both specifications of the model, this shows that also in this smaller (restrictive) father-son sample differences in earnings growth between skill groups do bias our estimates of β downwards, though only slightly so in the U.S.

6 Robustness

The present section explores the robustness of our results presented in column (4a) of Table 1. We first check the sensitivity of the estimates with respect to changes in the presumed interest rate. Second, we conduct some experiments to gauge the magnitude of the error-in-variable bias and so to disentangle errors-in-variables bias from a possible sample selection bias (due to non-random sample attrition). Third, checking for outliers we compare the OLS estimates with the results from median regressions.

Interest rates The estimates in Table 1 turn out to be robust against reasonable changes in the interest rate. If the assumed interest rate is greater than the true one, the relatively high earnings of the low skilled while being young are exaggerated, whereas their relatively low earnings are understated; the opposite is true for the high skilled. Both results in a downward bias of the estimate of β. This is exactly what we find in the data, though the magnitude of the changes is extremely small.

In the U.S. the real Treasury Bill Rate over the period 1984-2005 (r) was on average 2.1 percent, so we re-estimate the model presuming real interest rates were between 1.5 and 2.5 percent. Since the interest rate only enters the ϕ-terms and these terms enter only the constant when pooling all fathers and sons, the 'pooled' estimate of 0.372 does not vary with r. However, when allowing for different lifecycle earnings patterns, our choice of r has the predicted effect on the 'unconstrained' estimate. For $r = 0.015$ we compute a $\hat{\beta}$ of 0.381 and for $r = 0.025$ a $\hat{\beta}$ of 0.376, to be compared with $\hat{\beta}$ of 0.378 when $r = 0.021$. So the overall size of the effect of mismeasurement of the interest rate is modest. With respect to the German data, we used a real rate of interest of 2.6 percent to obtain the estimate of β of 0.326 (reported in Table 1) when not pooling fathers and sons of different skill groups. When we set instead r equal to 2 percent, we obtain a $\hat{\beta}$ of 0.331. By contrast, $\hat{\beta}$ is 0.323 when r is set to 3 percent. The results thus appear again robust against misspecification of interest rates of reasonable magnitude.

Errors-in-variables bias vs non-random sample selection Due to our admittedly strong identifying assumptions in equation (7), we can use all available earnings observations in the data that meet our selection criteria to estimate lifetime earnings of fathers and sons. Table 2 shows that in the GSOEP sample used for the estimates in column (4a) of Table 1 there are on average 10 observations per son and almost 12 per father. In the PSID sample the numbers are even higher (14 observations per son on average and around 17 per father) because the PSID is a more mature sample. This fairly large number of observations per father and son allows to conduct a set of experiments that attempt to gauge the magnitude of the attenuation bias which is expected to downward-bias all of the β-estimates. The idea behind these experiments is to randomly select five observations per person from the available data and then to re-estimate the model. This procedure is repeated 500 times. Mean and standard deviations (not to be confused with estimated standard errors) of the distribution of the obtained estimates are reported in

Table 3.

Table 3 about here

Applying the standard argument, we expect to obtain lower estimates for $\hat{\beta}$ and thus a measure for the reduction of the attenuation bias when decreasing the number of (annual) observations per father.[16] We actually compare the estimates reported in column (4a) of Table 1 with averages from two different experiments. In the first experiment earnings from five different waves are randomly selected (without replacement). In the second experiment five *consecutive* observations are randomly selected. If transitory fluctuations of individual earnings are autocorrelated, averaging over consecutive observations leads to a smaller reduction of the errors-in-variable bias than would be expected with white noise error terms (Zimmerman 1992, Mazumder 2005). With the number of father-son pairs sufficiently large, the difference between the estimated earnings elasticities of both experiments should be the greater, the stronger the autocorrelation of transitory fluctuations. Moreover, such differences become more and more visible, the greater the number of observations per person such that the samples drawn in the two experiments are actually reasonably different from each other. We therefore conduct the two experiments in both samples, the one with a minimum of five and the other with a minimum of ten observations per person.

Table 3 reports the results of these experiments. Comparing these with the estimates in column (4a) of Table 1 we find that in the U.S. sample estimates of $\hat{\beta}$ would be biased downwards by at least 5-10 percent if using only five observations per person (and discarding the remaining observations). For instance, while we find a $\hat{\beta}$ of 0.372 in the U.S. when pooling all observations, we obtain an average $\hat{\beta}$ of 0.354 in the first experiment (random selection without replacement from all observations) and of 0.340 in the second experiment (random selection of five consecutive observations). The results also point into the expected direction when allowing for skill-specific earnings growth over the lifecycle: While the estimate in the full sample then is 0.378 it is 0.359 in the first and 0.345 in the second experiment.

The last two columns of Table 3 report the result of the experiments when using the smaller father-son sample in which both fathers and sons are observed at least ten times. In the PSID sample the results for the small father-son sample are similar to those of the large father-son sample. Average estimates are lower in both experiments than the $\hat{\beta}$ of 0.385 ('pooled') and, respectively, 0.392 ('unconstrained') reported in column (4b)

[16] Notice that we also randomly select five observations for each son but that under the standard assumptions in the literature about independence of error terms and covariates this should not affect the estimate of β.

of Table 1.

Noteworthy, however, in the German data the change in the average $\hat{\beta}$ goes into the wrong direction in both experiments and in both samples. Instead of falling, the results shown in Table 3 are bigger than their counterparts in column (4a) of Table 1. Since the GSOEP is a shorter panel than the PSID we would have expected to observe a smaller drop of the average $\hat{\beta}$ in both experiments but the increase remains a puzzle.

Interpreting this drop in $\hat{\beta}$ in the two experiments when using PSID samples of fathers and sons, one should keep in mind that this 5-10 percent decrease only measures the decreases in the attenuation bias when reducing the average number of observations from around 15 to 5. Our estimates in Table 1 are still subject to the errors-in-variables bias because individual fixed effects are still estimated over only a relatively short time period. Hence, these results do not suggest that estimates from short panels with exactly five observations per father should be corrected simply by increasing them by 5-10 percent. This only gives a lower bound but, still, this bound is significantly lower than the correction factor of $1/0.69 \approx 1.45$ suggested by Mazumder (2005) to correct results obtained from short panels.[17]

Since the attenuation bias becomes less important as individual permanent earnings are measured more precisely, ideally one would like to average individual observations over as many years as possible. This is however associated with a cost. First, it reduces the degrees of freedom (we observe less fathers over a period of, say, ten years than over a period of five years) and, second, the sample's representativeness decreases due to non-random sample attrition. A comparison of the results of the two experiments reported in the first two columns of Table 3 with those in the last two columns sheds some light on the second issue.

When drawing at random five observations from either the large or the small father-son sample, estimates are in principle expected to be identical. That is, if the difference in estimates reported in columns (4a) and (4b) of Table 1 was largely due to the attenuation bias, then in both experiments this difference should vanish. However, as we see from Table 3, they do not, neither in the GSOEP nor in the PSID sample. Consider for instance $\hat{\beta}$ in the large 'pooled' GSOEP sample in Table 1, which is 0.276, and compare it with $\hat{\beta}$ in the respective small sample, which is 0.342. Now for the same samples, the average $\hat{\beta}$ in experiment 1 is again 0.276 in the large sample and 0.346 in the small sample. We interpret this finding as evidence that both samples, the father-son sample

[17] Although we agree in principle about the usefulness of such corrections, our results cast some doubt on Mazumder's preferred calibration exercise because $0.86/0.69 \approx 1.24 > 1.1$ (see his Table 1) suggest a significantly bigger attenuation bias in the short panel than we find in the U.S. data.

with a minimal number of five and ten earnings observations per person, are subject to different sample selection procedures. Lifetime earnings of fathers and sons who continue to report their earnings year after year seem to be higher correlated than lifetime earnings of fathers and sons of which a sizeable fraction is going to soon leave the sample.

In this sense the findings of tables 1 and 3 suggest that there might be a trade-off between the precision with which we can hope to estimate individual earnings and the representativeness of the sample. In view of this trade-off and the fact that other "better" (such as administrative) data usually does not allow to link family members (see, however, Mazumder 2005, Dahl and DeLeire 2006), corrections of inconsistent estimates, as for instance proposed by Mazumder (2005), might be the best way out of the dilemma that more data is not always a good thing—if the available data then loses in representativeness.

Median regression Second-step estimates are also computed using median regression (MR) because quantile regressions are less sensitive to possible outliers. The reason why they are not used in the literature on intergenerational mobility is probably that, so far at least, it is not well understood how errors-in-variables affect MR estimates. We therefore use MR only to check for robustness of the earlier described OLS estimates. Table 2 shows huge variation in earnings of both fathers and sons which may, to some extent, come from measurement error of the true annual earnings of these persons. If this occurs, the estimated person fixed effects are mismeasured because of the standard error term and *in addition* because of the misreporting of the true annual earnings. MR is one way to do reduce the effects on $\hat{\beta}$ of the most severe outliers that may well be due to the latter reason.

We find that the MR results are in general comparable to those reported in Table 1, both in magnitude and in relation to each other. When pooling data of all skill groups in the first step, in the GSOEP sample the MR estimate for β in the large father-son sample is 0.249 (standard error 0.044) which should be compared with the OLS estimate of 0.267 (see columns (4a) in Table 1). In the PSID data the respective MR estimate is 0.403 (SE 0.042). Allowing for differences in lifecycle earnings patterns, the MR estimate in Germany is 0.286 (SE 0.056) and in the U.S. 0.394 (SE 0.045). Thus, although differences between 'pooled' and 'unconstrained' estimates are attenuated when applying MR instead of OLS, we again find that allowing for differences in earnings over the lifecycle does have an effect on $\hat{\beta}$ in the German data but not in the U.S. data.

Finally, in the small father-son sample (minimum of ten observations per person) $\hat{\beta}$ is 0.314 (SE 0.075) in Germany when pooling all men in the first step and using MR in the second; to be compared with the OLS estimate of 0.342 reported in column (4b) in Table 1. When not pooling observations in the first step, the MR estimate is 0.388 (SE 0.083) in Germany and 0.420 (SE 0.047) in the U.S. Summarising, the previous finding that $\hat{\beta}$ is greater in the U.S. than in Germany seems to be robust against changes of the step-2 estimators. Moreover, the lifecycle bias again appears to have an effect on $\hat{\beta}$ in the German, but not in the U.S. data.

7 Conclusion

This study compares intergenerational mobility in Germany and the U.S. and introduces an estimation strategy that corrects estimates of the intergenerational elasticity (IGE) of earnings for a possible lifecycle bias. In contrast to a previous study (Couch and Dunn 1997), we do find evidence for American exceptionalism—in the sense that the U.S. society is comparatively rigid.

As a benchmark we re-estimate a standard Solon or Zimmerman model and obtain an estimate of the intergenerational earnings elasticity of 0.25 in Germany and of 0.43 in the U.S. Still following this literature in constraining lifecycle earning patterns to be identical for all men and using estimates of real lifetime earnings of all fathers and sons for which appropriate matches could be established, we find an IGE estimate of 0.27 in Germany and of 0.37 in the U.S. The U.S. estimates of this study thus seem close to the "reasonable guess" (Solon 1992) of around 0.4.

The lifecycle bias affects the estimates of both countries very differently. We find differences in earnings growth between skill groups in both countries but the variation in wage growth we find is much stronger in Germany than in the U.S. More specifically, the typical university graduate is relatively well-paid and his wage profile is much steeper at the beginning of his career, though these wage increases are significantly larger in Germany. This translates into a much more pronounced increase in the earnings elasticity in Germany once we take account of these differences in growth rates of earnings in the estimation of β. While the German estimate increases by 0.06 log points to 0.33, the increase of the U.S. estimate is a modest 0.01 log points.

With the estimates of average lifetime earnings of each skill group at hand, it is straightforward to determine the reference age for which differences in observed annual earnings most closely reflect the differences in lifetime earnings. We find this age to be somewhere between 30 and 35. This fits remarkable well with the results of other studies (Böhlmark and Lindquist 2006, Dahl and DeLeire 2006, Haider and Solon 2006, Mazumder 2005) that also find that, when used as a proxy for lifetime earnings, the predictive power of annual earnings is the greatest at around the mid 30s.

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8 Appendix

Figure 1: Lifecycle bias due to differences in wage growth

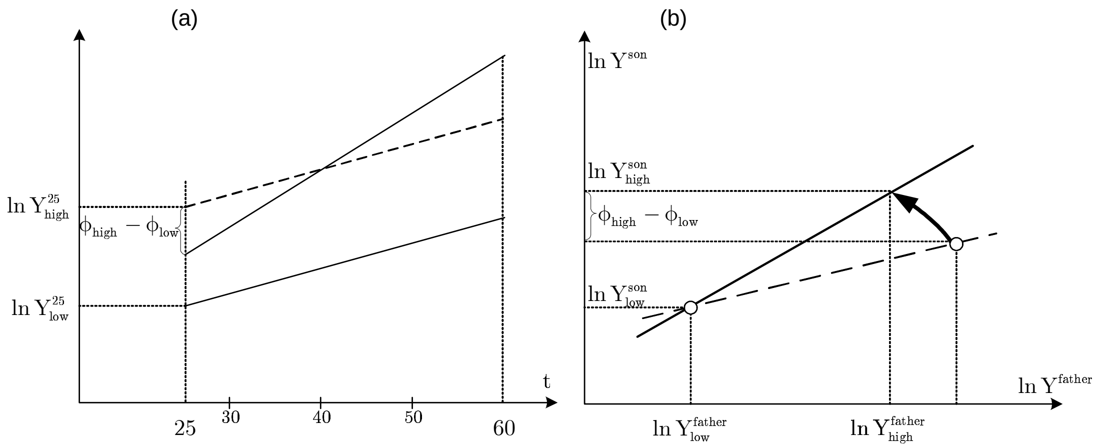


Table 1: OLS estimates of β

		minimal number of observations: 5			minimal number of observations: 10		
	(1)	(2a)	(3a)	(4a)	(2b)	(3b)	(4b)
GSOEP							
pooled	.246	.244	.255	0.267	.209	.377	0.342
	(.084)	(.098)	(.059)	(.040)	(.083)	(.084)	(.055)
unconstrained		.333	.311	.326	.260	.425	.358
		(.110)	(.065)	(.056)	(.096)	(.070)	(.088)
#fathers/#sons	259/314	259/314	219/270	411/515	197/236	92/106	122/142
PSID							
pooled	.426	.535	.628	.372	.538	.578	.385
	(.071)	(.078)	(.067)	(.045)	(.078)	(.067)	(.041)
unconstrained		.531	.633	.378	.532	.580	.392
		(.059)	(.069)	(.048)	(.059)	(.069)	(.042)
#fathers/#sons	290/368	290/368	161/339	564/874	289/367	100/138	309/458

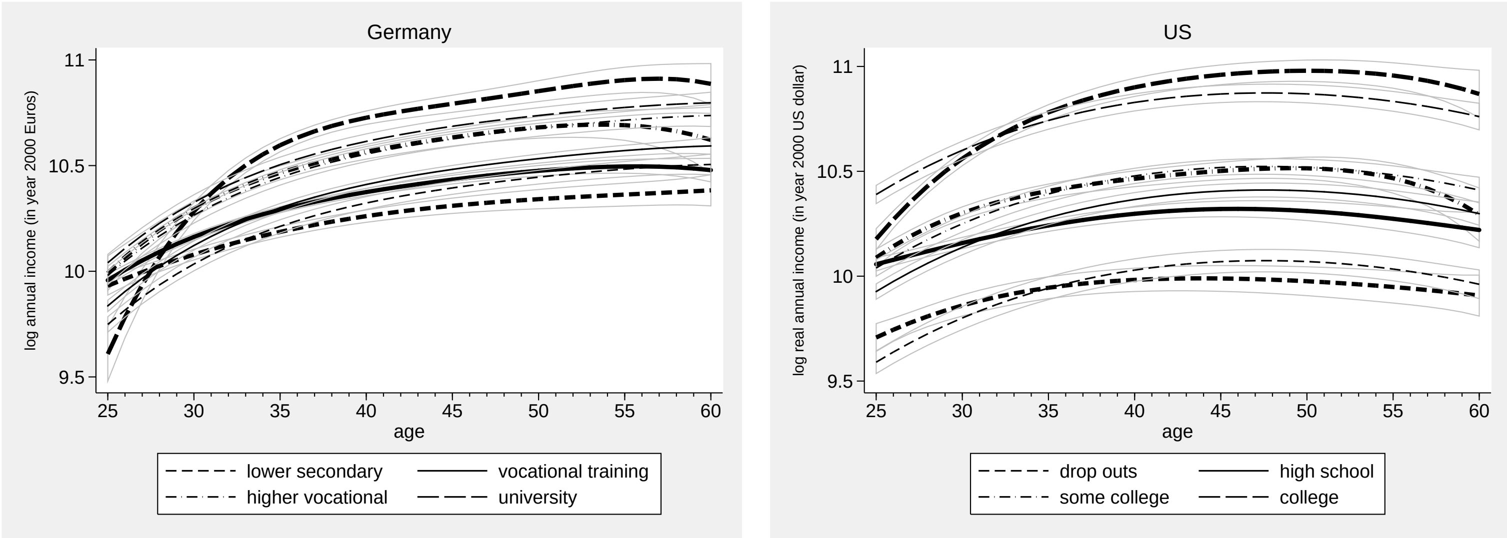
Note: Bootstrapped standard errors in parenthesis. (1) Benchmark: Estimation procedure adapted from Solon (1992). (2a),(2b) Terms of fathers replaced by estimates of fathers' log lifetime earnings. (3a),(3b) Estimates of log lifetime earnings used for both fathers and sons. (4a),(4b) Similar to (3a) and (3b), but observations of fathers and sons that were not used in (1) are used as well.

Table 2: Summary statistics

	GSOEP			PSID		
	(1)	(4a)	(4b)	(1)	(4a)	(4b)
Son's average age	34.4	30.8	31.9	34.9	33.6	34.5
	(5.1)	(2.6)	(1.9)	(7.1)	(3.7)	(2.6)
	[25-50]	[27-43]	[30-40]	[25-53]	[27-54]	[30-45]
Son's real earnings	36,234	30,229	32,866	54,643	44,451	48,090
	(23,244)	(11,490)	(10,816)	(55,481)	(31,668)	(35,490)
	[1,976-296,769]	[2,446-132,932]	[16,467-83,285]	[280-673,980]	[2,203-440,333]	[11,270-440,333]
Son's log real earnings	10.38	10.22	10.32	10.62	10.45	10.56
	(0.50)	(0.36)	(0.29)	(0.80)	(0.55)	(0.47)
	[7.6-12.6]	[7.7-11.6]	[9.6-11.2]	[5.6-13.4]	[7.5-12.2]	[9.2-12.2]
# obs. per son	1	10.3	13.1	1	14.0	16.8
		(4.1)	(2.5)		(6.7)	(4.9)
		[5-22]	[10-22]		[5-31]	[10-31]
# sons	314	515	142	368	874	458
Father's average age	43.4	50.9	50.7	41.9	47.5	47.7
	(6.1)	(4.5)	(3.2)	(8.0)	(6.1)	(6.1)
	[27-56]	[29-58]	[42-56]	[26-56]	[28-58]	[28-58]
Father's real earnings	32,419	33,518	34,450	48,434	49,148	49,396
	(24,732)	(16,563)	(14,259)	(36,790)	(32,521)	(32,972)
	[14,362-366,488]	[15,822-210,603]	[16,726-120,831]	[828-425,942]	[4,512-401,209]	[4,512-401,209]
Father's log real earnings	10.29	10.33	10.37	10.57	10.59	10.69
	(0.37)	(0.35)	(0.33)	(0.75)	(0.59)	(0.56)
	[9.6-12.8]	[9.5-12.2]	[9.7-11.7]	[6.7-12.9]	[7.9-12.7]	[7.9-12.7]
# obs. per father	5	11.7	14.2	5	17.1	18.0
		(4.9)	(3.3)		(6.2)	(5.4)
		[5-22]	[10-22]		[5-31]	[10-31]
# fathers	259	411	122	290	564	309

Note: Numbers in round parenthesis are standard deviations and those in square brackets denote the range of observed values. Columns labeled (1) refer to the sample used in column (1) of Table 1. Observations of fathers and sons are for 1984 and, respectively, 2003. Columns labeled (4a) refer to the sample used in the estimation of equation (1) that yields estimates shown in column (4a) of table 1. The sample of the latter panel is unbalanced. Therefore, the reported distributions are distributions of averages for each person. See text for a description of wage and earnings data. German data are for the years 1984-2005, U.S. data for the years 1970-2005. For Germany and the US earnings are reported in, respectively, Euros and US dollars of year 2000 (using the consumer price index of the U.S. and, respectively, Germany).

Figure 2: Lifecycle earnings profiles in Germany and the US



Note: Thick lines show predicted earnings when skill groups are allowed to differ with respect to their earnings profils. Thin lines depict predicted earnings when earnings profiles are assumed to be identical. Light thin lines report 95 percent confidence intervals.

Table 3: Robustness checks: attenuation bias vs sample selection. Means and standard deviations of $\hat{\beta}$ in two experiments.

	min. number of obs.: 5		min. number of obs.: 10	
	GSOEP	PSID	GSOEP	PSID
pooled				
Experiment 1	.276	.354	.346	0.369
	(.014)	(.012)	(.029)	(0.019)
Experiment 2	.287	.340	.361	0.346
	(.019)	(.015)	(.039)	(0.025)
unconstrained				
Experiment 1	.330	.359	.367	0.381
	(.015)	(.012)	(.031)	(0.018)
Experiment 2	.337	.345	.370	0.357
	(.020)	(.015)	(.041)	(0.024)

Note: Standard deviations in parenthesis. Experiment 1: Random selection (without replacement) of exactly 5 observations for each person in the sample. Experiment 2: Random selection of 5 consecutive observations for each person in the sample.