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Reassessing Intergenerational Mobility in Germany and the United States: The Impact of Differences in Lifecycle Earnings Patterns

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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 US 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 US. **Keywords:** Intergenerational mobility, lifecycle bias, comparison of Germany and the US.
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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 earnings elasticity, 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.

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.

Although one is interested in estimating the intergenerational correlation of log *lifetime* earnings, the researcher observes earnings only over relatively short time periods. Using "snapshots" as proxies for lifetime earnings is unproblematic if and only if one is willing to assume that lifecycle earnings profiles are reasonably similar for all individuals. Then, the main problem that remains is the prevalent attenuation bias due to the likely mismeasurement of fathers' lifetime earnings (Solon 1989).

A further concern with this procedure using "snapshots" as proxies is that periodic earnings can convey a very misleading picture of the true lifetime earnings if wage growth over the lifecycle is quite different for different groups of people. This point was already raised in Jenkins (1987) and more recently further elaborated by Haider and Solon (forthcoming) and Grawe (forthcoming). The argument is easily understood when considering the most simple case in which there are only two types of workers. Let wage growth be greater for high-skilled than for low-skilled workers *and* at the same time assume that lifetime earnings of the former exceed those of the latter. Then it is easy to show that standard estimators of the intergenerational earnings elasticity are downward inconsistent. Notice that this problem (in contrast to the attenuation bias) would even persist if the process generating periodic earnings was deterministic and not stochastic!

The present paper adds to the literature on intergenerational earnings elasticity in several ways. First, we estimate earnings elasticities between fathers and sons while explicitly allowing different skill groups to have different wage growth over the lifecycle, thus eradicating possible lifecycle biases. Second, making standard assumptions about the income-generating process over the lifecycle we can use a much bigger data set than is commonly used to estimate earnings elasticities, thus limiting the attenuation bias. Third, as does Couch and Dunn (1997), we apply the same estimation strategy on both German and US data to obtain estimates of intergenerational earnings elasticities for both countries which allows us to compare the openness of both societies.

As usual in empirical research, we have to deal with the problem that there is not enough good data available so as to eliminate lifecycle effects in a completely satisfactory manner. Ideally we would want to use earnings data of at least two generations of persons over their full lifecycle. Since this ideal data is not available (neither in the surveys used here, nor in administrative data) we have to make some assumptions about the data generating process that allows us to draw inference about a person's earnings in a given year even when it is not observed. Otherwise we cannot hope to learn about individual lifetime earnings and to estimate intergenerational earnings elasticities that are not biased due to variation in lifecycle earnings profiles. The assumption we make here is that we can learn from the observed wage growth of fathers about the future wage growth of their sons and, vice versa, from the observed wage growth of sons about the unobserved but most likely wage growth of their fathers while they were young.

The data we use in this study comes from the German Socio-Economic Panel (GSOEP), the Panel Study of Income Dynamics (PSID), and the Cross-National Equivalent File (CNEF). Still ignoring lifecycle effects, we estimate the intergenerational earnings elasticity to be 0.24 using German and 0.34 using US wage data. These estimates are considerably higher than the estimates of 0.11 and 0.13 for Germany and the US previously reported in Couch and Dunn (1997). However, the US estimate is still somewhat lower than 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 0.31 in Germany and 0.36 in the US. Thus, at least in the German data we find strong indication of a severe downward lifecycle bias. In the US, by contrast, lifecycle effects do not affect the estimated earnings elasticity by very much. Finally, we also correct lifetime earnings for an effect that has not attracted very much attention in this literature, namely that highly qualified workers enter the labour market a few years later than low qualified workers. So a comparison of wages of both types of workers when both skilled and unskilled workers are economically active, most likely leads to overestimating the actual lifetime earnings of both groups. Taking also account of this effect, we estimate the intergenerational earnings elasticity to be 0.27 in Germany and 0.34 in the US. Based on CNEF data, the respective estimate of the earnings elasticity in Germany is 0.19 and 0.29 in the US. So independent of our estimation method and the data sets used, we conclude that the German society is more "open" than is the US society.

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 therefore be modified. Moreover, this section discusses in more detail the expected direction of the lifecycle bias. Section 3 describes the data used in this study. 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. We check for robustness of these results 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.

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. Important aspects or features of human capital such as, e.g., education or vocational training are however only imperfectly divisible. Taking this indivisibility into account, though, leads to a different interpretation of Y_i^{father} and Y_i^{son} because when the number of, say, professions is finite and, associated with this, there is a finite number of different training costs, then all households would earn identical labour incomes as long as capital markets are perfect.[1] Therefore, in this setting a relation between Y_i^{father} and Y_i^{son}—as suggested by (1)—is plausible (with a non-zero β) only when interpreting incomes very broadly, including asset incomes.

In the literature on intergenerational mobility, however, both Y_i^{father} and Y_i^{son} are usually 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 forthcoming) and estimate the correlation between lifetime *labour earnings* of fathers and sons. Lifetime labour earnings of a member of family i born in period b who enters and leaves the labour market at age T_{ib}^{entry} and respectively T_{ib}^{exit} can be expressed as

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

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 earnings observations. For notational convenience write Y_{ib}^0 for annual earnings of member b of family i when he is 25 years old.

Notice that earnings in period t can always be written as

$$Y_{ibt} = Y_{ib}^0 \times e^{g_{ibt}(t-b-25)}$$

where g_{ibt} denotes the average growth rate of earnings over the interval $(b+25, t)$. In-

[1] Similarly, in Becker and Tomes (1986) 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.

serting this expression into (2) and taking logs yields

$$\log Y_{ib} = \log Y_{ib}^0 + \log \int_{b+T_{ib}^{\text{entry}}}^{b+T_{ib}^{\text{exit}}} e^{(g_{ibt}-r)(t-b-25)}dt = \log Y_{ib}^0 + \phi_{ib} \tag{3}$$

By definition, the variable ϕ_{ib} depends on g_{ibt}, T_{ib}^{entry}, and T_{ib}^{exit}. 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 ($\log Y_{ib}^0$) 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 for all sampled individuals.

The standard practise to estimate the "permanent component" (see for instance Zimmerman 1992) is to first estimate the income-generating function

$$\log Y_{ibt} = \log Y_{ib}^0 + \mathbf{X}_{ibt}\alpha + \nu_{ibt}$$

where the errors ν_{ibt} are mean independent of both "permanent component" and the other covariates $\mathbf{X}$, which are usually a polynomial in age.[2] Taking averages via

$$\widehat{\log Y_{ib}^0} = \overline{\log Y_{ibt}} - \overline{\mathbf{X}_{ibt}}\hat{\alpha}$$

then yields unbiased estimates of individual income at the reference age. With only few observations available per person, estimates of "permanent component" may be quite imprecise leading to the famous attenuation bias (see, e.g., Solon 1989, Solon 1992, Björklund and Jäntti 1997).

If in the above earnings function wage growth and thus the vector α is identical for all individuals and in addition entry and exit age are also the same, then the ϕ's are identical as well. So when making these assumptions, the standard procedure to use the "permanent component" as a proxy for lifetime earnings in (1) *is* justified.[3] However, if different, say, skill groups have different earnings growth rates (different α), the so obtained estimates of β are in general inconsistent.

Adjusting the income-generating function to obtain unbiased estimates of $\log Y_{ib}^0$ is of

[2] 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 income-generating function is in fact a stripped-down version of a Mincer wage equation.

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

course simple; one only has to lift the restriction that the vector of coefficients (α) is identical for all skill groups. But the important point to notice here is that this is not sufficient to eradicate the source of the inconsistency. In fact, improving our estimates of the "permanent component" may actually make things worse, not better. If different skill groups exhibit different earnings growth rates over their lifecycle, to obtain consistent estimates of β in general we need both good estimates of individual earnings at the reference age *and* the correctly estimated ϕ-terms (as shown in (3)). The direction of the induced bias when falsely ignoring the ϕ-terms is discussed next.

Differences in earnings growth Consider two 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.

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. It is identical to the difference in annuitised lifetime earnings and thus crucial for the estimation of intergenerational earnings elasticities.

The figure also 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 in 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.

Differences in training periods Next let growth rates of both workers be identical but assume that low skilled workers enter the labour market at age 20 while high skilled workers spend five more years in education. Then observed earnings when both are 25 years old clearly overstate the actual difference in lifetime earnings. So again we need to adjust annual earnings at the reference age to take account of the late entrance to the labour force of skilled workers. Panel (b) of Figure 2 shows that otherwise the obtained estimate of β would be upward inconsistent: Since lifetime earnings of both skilled fathers and skilled sons are overestimated by the same amount, $\hat{\beta}$ is upward inconsistent if the true slope coefficient is below one.

3 Data

We use two different original data sets, the German Socio-Economic Panel (GSOEP) for Germany and the Panel Study of Income Dynamics (PSID) for the US—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. Since then 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 US, 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.[4] To limit measurement error of reported earnings, which may be severe in the early and late stages of the lifecycle, we only use observations on men who are between 25 and 60 years old.[5] Moreover, we discard observations from men for which earnings are observed in less than 5 years. With respect to fathers this is done to reduce the attenuation bias, with respect to sons to keep the sample homogenous.

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

[5] 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.

The US 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 1970-2003. 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 as from 1988 we also drop observations that are reported to be censored, but at extremely large censoring bounds (1 million or, as from 1994, 10 million dollars). This leaves us with a sample of 525 sons from 421 fathers in the GSOEP and 876 sons from 563 fathers in the PSID.

Another earnings variable we use is on individual labour earnings as provided in the Cross-National Equivalent File (CNEF). Among other data sources for other countries, the CNEF uses data from the PSID and the GSOEP to generate variables that are supposed to be by and large comparable across countries (for a description of the CNEF see Burkhauser, Butrica, Daly and Lillard 2001). Individual labour earnings in the CNEF follow a broader concept of labour earnings. Most importantly, it covers earnings of both employed and self-employed workers. As before, only non-imputed values are used in the analysis. CNEF data is available for all GSOEP waves but, for the PSID, only for the waves 1980-2001. Table 1 presents summary statistics of the key variables used in this study.

Education qualification in both Germany and the US are aggregated into four groups. In the US we group individuals into high school drop-outs, high school graduates, people with some college, and college graduates. Persons of these groups are assumed to enter the labour market at the age of 18, 19, 22, and 24, respectively. In Germany the grouping follows naturally from the German educational system: men without vocational training, with vocational training, with further higher education[6], and with a degree from university or technical college. They are assumed to enter the labour market at age 18, 20, 23, and respectively 25. In both countries all men are assumed to leave the labour market at the age of 60. In both countries skill group two is the largest group and therefore referred to as the reference group.

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 1983-2004 which is 2.1 per cent in the US and

[6]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 3).

2.6 per cent in Germany.

4 Estimation Strategy

Estimation of the intergenerational earnings elasticity β proceeds in two steps. In a first step we estimate lifetime earnings and in the second step we use these results to estimate β.

Step 1 As argued in section 4, to obtain estimates of lifetime earnings we need to estimate both individual earnings at the reference age and the ϕ-terms. Estimating the ϕ's in turn requires estimation of the complete lifecycle earnings profile of both generations, that of the fathers and that of the sons. Sons' (fathers') earnings are mostly observed in the early (late) stages of their careers, so we make the identifying assumption that earnings profiles of both sons and fathers are identical. Moreover, we follow most of the literature in assuming that age effects can be represented by a second-degree polynomial in age. To correct for cohort effects, we further assume that income increases linearly in time.[7] The income-generating function can then be written as

$$\ln Y_{ibt} = \ln Y_{ib}^{0} + \alpha_1 A_{ibt} + \alpha_2 A_{ibt}^{2} + \gamma t + \nu_{ibt} \tag{4}$$

Making this assumption on the functional form of the earnings function, observations from *all* men in the data for which earnings are observed in at least five waves while being in the admissible age range 25-60 can actually be used in the estimation of (4). Importantly, the fact that for most men in the data a father-son link cannot be established does not invalidate the assumption that also for these men the statistical process generating annual earnings is described by the above equation. But using data of all men who appear to be comparable to persons for which such a father-son link *can* be established results in increased precision of the estimates $\hat{\alpha}_1$, $\hat{\alpha}_2$, $\hat{\gamma}$, and hence of the individual effects (earnings at age 25) of fathers and sons and of the ϕ-terms.[8]

[7]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.

[8]There is the chance that sample attrition is not random and that persons for which earnings are observed only very few times are not perfectly comparable to persons whose earnings are observed for at least 5 years. We therefore refrain from using observations on these men also in the first-step estimations.

Step 2: In the second step we use the estimates of Step 1 to compute lifetime earnings of both fathers and sons. These are then inserted into equation (1) to estimate β. Notice that in this second step *estimates* of lifetime earnings are used to obtain the estimate $\hat{\beta}$. Although such simple two-step estimators of the coefficients as used here are consistent, the uncorrected second-stage standard errors are not (see Pagan 1986, Newey and McFadden 1994). We therefore use the bootstrap (with 500 replications) to compute standard errors of all two-step estimates.

5 Empirical Results

5.1 Step 1: estimating lifetime labour earnings

Equation (4) is estimated using OLS. For our purpose a comparison of wage growth over the lifecycle is crucial and therefore in Table 2 we only report the obtained $\hat{\alpha}_1$. In the upper panel we report $\hat{\alpha}_1$ when all three coefficients in (4) are assumed to be identical for all four skill groups. In contrast, the lower panel of the table shows the coefficients when this restriction is lifted and instead α_1, α_2, and γ are allowed to differ across skill groups. There are three things to notice of the results in Table 2. The first is the large number of persons used in the estimations and also the large number of average observations per person. Remember that we use all observations from men aged between 25 and 60 for which we observe earnings in at least five years. Still, the average number of observations per person is more than twice as large.

A second interesting insight to be gained from Table 2 is that earnings growth is very different for different skill groups. The general pattern is the higher educated a person is, the greater his expected earnings growth in the early stages of his career. The results of the pooled estimations reported in the upper panel are actually somewhere in the middle of the respective results of the unrestricted estimations in the lower panel of the table. The third finding to notice is that there is much more variation in earnings growth between skill groups in Germany than in the US. Thus, by the argument developed in section 4, the lifecycle bias is expected to be more severe in Germany than in the US.

The lifecycle bias is the larger, the greater lifetime earnings of high skilled fathers (sons) are exaggerated (understated) when falsely running the pooled estimations instead of allowing the coefficients in (4) to differ across skill groups. Table 3 reports logs of annuitised lifetime earnings of fathers and sons for two skill groups, medium skilled persons (the reference group: High School graduates in the US and men with vocational training in Germany) and high skilled persons (persons with a completed college or university education). Annuities are reported instead of lifetime earnings because the formula

$$\log \text{Annuity}_{ib} = \log Y_{ib} - \log \int_{T^{\text{entry}}}^{60} e^{-r\left(\tau - T^{\text{entry}}\right)} d\tau$$

allows easy conversion of the latter into the former and annuitised incomes are easier to compare with actually observed annual earnings. Notice that in Table 3 it is always assumed that for each skill group the entry age T^{entry} is the same as that of the reference group. So for both fathers and sons the differences in estimated log annuitised earnings of the two skill groups is identical to the respective estimated differences in log lifetime earnings.

The numbers in Table 3 reflect strong differences between the US and the German data. First, due to the *relatively* stronger wage growth of high skilled workers in Germany (as compared to the wage growth of low skilled workers), the estimated average log annuities of high skilled workers dramatically differ whether we use the pooled or the unrestricted version of equation (4). Comparing the estimates in columns 3 and 4 of the table, in the US when pooling skill groups, annuities of high skilled fathers are overstated by 0.07 log points while that of high skilled sons are understated by 0.05 log points. By contrast, in Germany annuities of high skilled fathers are overstated by 0.19 log points and those of high skilled sons understated by 0.11 log points. Using CNEF earnings data, the differences between German and US data is even more striking. In the German section of this data annuities of high skilled fathers (sons) are over(under)stated each with 0.22 log points. Compared with these strong differences the differences of 0.03 and 0.05 log points in the case of high skilled fathers and, respectively, sons in the US is rather modest.

Second, with the noticeable exception of the PSID sample, in all other samples both estimation methods yield roughly identical estimates of annuities of medium skilled men. In the PSID sample, by contrast, annuities of medium skilled fathers seem to be strongly overestimated when pooling skill groups. Taking together, these findings suggest that allowing for lifecycle effects does make a difference for the estimated intergenerational earnings elasticity, particularly so for the earnings elasticity in Germany.

5.2 Step 2: intergenerational earnings elasticities

The main results of this paper are presented in Table 4. In the upper panel of the table we report $\hat{\beta}$ when earnings of both fathers and sons are required to be observed in at least five years. For each sample we report $\hat{\beta}$ for four different specifications of the model. In the first two rows we show the estimates when in Step 1 wage growth is assumed to be identical for all four skill groups while in the following two rows this constraint is removed. In both cases β is estimated holding the entry age to the labour market fixed (at the entry age of the reference group) and allowing the entry age to vary. Estimates in the first row of the table are best comparable to the estimates usually reported in the literature (see, e.g., Solon 1999, 2002) and therefore (and for that reason only) are referred to as our benchmark estimates.[9]

The first thing to observe of the results in Table 4 is the strong difference between estimated intergenerational earnings elasticity in Germany and the US. While in our benchmark estimation we obtain an estimate of 0.235 in the GSOEP sample, in the PSID sample this estimate is 0.343. The latter estimate is only somewhat lower than 0.4, the "reasonable guess of the intergenerational elasticity in long-run earnings for men in the United States" (Solon 1999). It is certainly much lower, though, than the guess of 0.6 noted in Mazumder (2005)—even though in the present study the average number of observations used per father is more than three times as large as in most other studies, thus limiting the unavoidable attenuation bias. The estimates from both CNEF samples support this finding that, when compared to the US, the German society is relatively open. Again the obtained $\hat{\beta}$ is found to be much larger in the US (0.293) than in Germany (0.104).

A second important finding is that taking account of differences in wage growth over the lifecycle can significantly affect the estimated β coefficients and thus can result in arriving at very different conclusions about a society's openness. In fact, in both German samples we find the lifecycle bias to be of significant magnitude. Compared with the benchmark estimates where we lump together all skill groups, in the GSOEP sample the estimated β increases by more than 30 percent to 0.304. In the CNEF the estimate more than doubles! In both US samples for which differences in wage growth between skill groups were found to be much smaller than in Germany, estimates of β also increase, but only modestly. In both the PSID and the CNEF sample $\hat{\beta}$ increases by only 4 percent to 0.357 and, respectively, 0.306.

Thirdly, assuming that men with more years of schooling enter the labour market at a later age is also found to have a strong impact on the obtained estimates. In this specification the estimates of β reduce strongly in both German samples (for instance, in the GSOEP sample $\hat{\beta}$ decreases from 0.235 to 0.200 in the pooled and from 0.308 to 0.266 in the unconstrained estimation) and somewhat less strongly in the US samples.[10]

[9] Remember in the benchmark specification ϕ-terms are identical and hence ignoring them does not affect the estimated intergenerational earnings elasticity.

[10] In both CNEF sample the obtained estimates are about 20 log points lower than in the specification with a fixed entry age. But the relative impact of this change is lower in the US data because of the higher US benchmark estimate.

Summarising, in all three modifications of the benchmark specification we find the estimate to be biased in the direction that was expected from the theoretical discussion in section 4. The magnitude of the lifecycle bias however differs strongly between samples. Allowing for differences in earnings growth over the lifecycle has a much greater impact on the obtained $\hat{\beta}$ in the German than in the US samples. In fact, in the US lifecycle effects on the intergenerational earnings elasticity are modest. So with respect to the large number of studies using US data, the standard procedure of simply ignoring a possible lifecycle bias when estimating β does not seem to lead to very misleading conclusions about the general "openness" of the US society. Finally, the findings of this section suggest that the German society is significantly more open (with respect to earnings potential) than is the US society. Taking account of differences in both wage growth *and* training periods, using wage data we estimate $\hat{\beta}$ to be 0.266 in Germany which compares to an estimate of 0.337 in the US. Using labour earnings of both employed and self-employed workers $\hat{\beta}$ is 0.189 in Germany and 0.285 in the US.

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 by averaging fathers' earnings over up to five years. Ideally, one would like to average over as many years as possible, but this comes at a cost of a severe reduction in degrees of freedom and of an increased likeliness that the sample becomes less representative due to non-random sample attrition. By now many more waves of data have become available and our estimation method actually allows to make use of all available earnings data. So we re-estimate the model using only men for which we have at least *ten* valid earnings observations.[11] The obtained estimates of β of this subsample are presented in the lower panel of Table 4.

Looking at the benchmark estimates when using wage data from the GSOEP and the PSID (columns (2) and (3)), we find that the estimated earnings elasticities increase strongly. In the GSOEP $\hat{\beta}$ increases by 75 percent from 0.235 to 0.413, while in the US data it increases by only 12 percent from 0.343 to 0.398. Still holding entry ages fixed at the entry age of the reference group but allowing for skill specific wage growth over the lifecycle, the increase in $\hat{\beta}$ is not as dramatic, though still sizeable. In the German sample $\hat{\beta}$ increases from 0.308 to 0.453 (47 percent increase), while in the US the estimate changes from 0.357 to 0.406 (10 percent increase). These figures actually suggest that both the German and the US society are comparably open.

[11] Again, to reduce the errors-in-variable bias we are interested in using as many observations of fathers as possible. But to keep the sample in the estimation of Step 1 homogenous, we also require sons' earnings to be observed at least ten times.

Also in the CNEF data $\hat{\beta}$ increases significantly when using the more restrictive and much smaller subsample. In the German CNEF data the benchmark estimate more than doubles (from 0.104 to 0.256) while for the US the estimate only increases from 0.293 to 0.379. In total, taking account of both lifecycle effects, compared with the estimates based on the large father-son sample the difference in openness between the German and the US society diminishes strongly from 0.96 to 0.48 log points—though still not overturning our earlier verdict that the society in Germany is more open than the US society.

5.3 Age-dependence of intergenerational earnings elasticities

The strategy in this paper to correct for lifecycle biases is to add skill-specific components (the ϕ-terms, see equation (3)) to the estimated annual earnings at the reference age (here 25). A different way to eliminate the lifecycle bias would be to take out skill-specific age effects in such a way that the obtained skill-specific differences in average annual labour earnings would reflect the underlying overall wage differences between the skill groups (see also the fine presentation of this argument in Haider and Solon forthcoming). So the idea is to find a specific age such that at this age the difference in observed annual earnings by and large reflects the difference in lifetime earnings.

Suppose such an age could be found, which is always possible with only two skill groups (see Figure 1). Then taking this age as the reference age (instead of choosing ad-hoc 25 as the reference age) eliminates the lifecycle bias the same way as does adding the constant skill-specific components to the estimated individual annual earnings at the age of 25. In other words, the just proposed two-step estimator of β is consistent even without correcting estimates of annual earnings at the reference age *once we correctly specify the reference age*.[12] Remember however that even then we would still have to allow for skill-specific growth rates or use only earnings that are actually observed *at* the reference age.

Figure 3 plots estimated lifetime wage profiles for all four skill groups for both Germany and the US (based on GSOEP and PSID data with a minimum of five wage earnings per person). The thick lines show the estimates of log annual earnings when *not* restricting wage growth to be identical. The thin lines depict estimated earnings profiles with presumed identical wage growth (and so run parallel in both graphs). The level of each thin line is chosen such that for each skill group the implied lifetime earnings are the

[12] Notice however that this approach is not more efficient (in the statistical or the computational sense) than the estimation strategy followed earlier because it still requires estimation of lifetime earnings to determine the correct reference age.

same for both (thick and thin) earnings profiles.[13] Results are only plotted for the specification with identical entry ages.

Apart from showing the much wider wage dispersion in the US, the interesting finding from Figure 3 is that in both countries and in all skill groups the thick and thin lines intersect when individuals are about 35 years old. This finding suggests that the lifecycle bias stressed in this paper can be expected to become extremely small—even without adding the ϕ-terms to the estimated individual fixed effects—when one chooses 35 as the reference age.

For high wage earners, who are predominantly high skilled, above the age of 35 their annual earnings in general exaggerate their lifetime earnings while the opposite is true for low skilled persons who mostly also earn low wages. Therefore, with fathers being almost always above 35 when their wages are observed in the survey, Figure 3 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 (forthcoming) finds. However while 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.

6 Robustness

The present section explores the robustness of our results presented in Table 4. We first check the sensitivity of the estimates with respect to changes in the presumed interest rate. Second, we conduct some experiments that attempt to gauge the magnitude of the error-in-variable bias and, doing so, to disentangle its impact from possible effects that are 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 4 turn out to be robust to 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 while 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.

[13] Denote the estimate of ϕ when pooling observations of all skill groups as $\hat{\phi}^0$ and the respective estimate of skill group $j = 1, 2, \dots$ as $\hat{\phi}^j$. Then lifetime earnings of the two earnings profiles are identical when we add $\hat{\phi}^j - \hat{\phi}^0$ to the average individual fixed effects of each skill group j.

We only discuss the results for GSOEP and PSID samples where the minimal number of valid earnings observations is five. In the US the real interest rate (r) we use for discounting is 2.1 percent, so we re-estimate all models presuming interest rates of 1.5 and 2.5 percent. Since the interest rate only enters the ϕ-terms, the benchmark result (0.343) is unaffected by variation in r. For the other three specifications the range of estimates of β (presuming $\hat{\beta}$ is monotonous in r) is $(0.317, 0.321)$ when wage growth is identical for all skill groups but entry ages vary, $(0.356, 0.359)$ when wage growth is different but entry age is held fixed, and $(0.333, 0.342)$ when both wage growth and entry age are allowed to vary.

In Germany where the wage growth effect is found to be significantly larger, changes in the underlying discount rate have a somewhat larger effect on $\hat{\beta}$. The range of results of the four specifications of the model is (0.235), $(0.197, 0.203)$, $(0.306, 0.312)$, and $(0.260, 0.274)$. The results thus appear robust against misspecification of interest rates of reasonable magnitude.

Errors-in-variables bias vs non-random sample selection The fairly large number of observations per person in the father-son sample (see Table 1) 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 standard errors) of the distribution of the obtained estimates are reported in Table 5.

In the first experiment earnings from five different waves are randomly selected for each father and each son. 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). In a second experiment we therefore draw random samples of five *consecutive* observations per person from the father-son sample. 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.

For brevity Table 5 only records the results of these experiment for the specifications where entry age is held fixed. The first observation to be made is that with one exception all estimates are smaller than their counterparts in Table 4. Using only five instead of all available observations per person increases the noise-to-signal ratio and thereby leads to a reduction in the probability limit of $\hat{\beta}$. The finding that almost all estimates in both experiments are smaller than the actual estimates reported in Table 4 can be interpreted as reflecting this attenuation bias.

Second, we find that in both US samples the estimates in Experiment 2 are always smaller than the estimates of the first experiment. We interpret this as evidence for substantial autocorrelation of the error terms in both US samples. In both German data sets it is not so straightforward how to interpret the results because almost half the reported estimates of the second experiment exceed those of the first.

Finally, notice that the reported estimates in the lower panel of Table 5 are always significantly larger than the respective estimates in the upper panel. This happens to be the case despite the fact that in both samples and in both experiments we always select exactly five observations per person. If the difference in estimates in the upper and lower panel of Table 4 was largely due to the attenuation bias, then in both experiments this difference should vanish. However, it does not. We interpret this finding as evidence that both samples, the father-son sample with a minimal number of five and ten earnings observations per person, are subject to different sample selection procedures. That is, 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 4 and 5 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, 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.

Median regression Second-step estimates are also computed using median regression (MR) because quantile regressions are less sensitive to possible outliers. All four data sets used in this study show huge variation in earnings of both fathers and sons (see Table 1). From the data it is impossible to judge whether this actually reflects the underlying earnings distribution or, at least to some extent, comes from measurement error. Notwithstanding dropping many so-called "censored" earnings in the PSID, particularly in the PSID, but also in the other data sets, though to a minor degree, there are some extremely large labour incomes casting the quality of these particular observations into doubt. When drawing our main conclusions we therefore want to limit the impact of these possible outliers on the obtained estimates of β. MR is one way to do that.

The MR results are in general comparable to those reported in Table 4, both in magnitude and in relation to each other. Here, we only report the MR estimates of the benchmark model. Beginning with the GSOEP for which the OLS estimate of the large father-son sample (where the requirements on the minimal number of observations is five and thus less restrictive than in the small father-son sample) reported in Table 4 is 0.235, the respective MR estimate is 0.216 (standard error 0.040). In the small father-son sample the MR estimate of 0.389 (SE 0.105) is again only somewhat lower than the respective OLS estimate of 0.413. In the PSID data the MR estimate of the big father-son sample is 0.365 (SE 0.038) and thus quite similar to the OLS estimate of 0.343. In the small sample MR yields an estimate of 0.401 (SE 0.047) which is extremely close to the OLS result of 0.398. In the German CNEF data the MR estimates of the large and the small father-son sample are 0.108 (SE 0.053) and respectively 0.243 (0.072). Finally, the two MR estimates of the US CNEF data are 0.294 (SE 0.036) and 0.418 (SE 0.046) which are again very close the OLS estimates reported in Table 4.

Also in the specifications in which wage growth of skill groups is not constrained to be identical, MR and OLS estimates are of similar magnitude. The MR estimates of the big and small father-son sample are 0.287 (SE 0.047) and respectively 0.427 (SE 0.098) in the GSOEP, 0.376 (SE 0.044) and respectively 0.418 (SE 0.046) in the PSID, 0.193 (SE 0.050) and respectively 0.356 (SE 0.091) in the German section of the CNEF, and 0.320 (SE 0.039) and respectively 0.424 (SE 0.049) in the US section of the CNEF. In the light of these in general quite similar MR and OLS estimates, we draw the conclusion that the results reported in Table 4, and thus the main conclusions of this paper, are reasonable robust against possible outliers in the data.

7 Conclusion

This study compares intergenerational mobility in Germany and the US and introduces an estimation strategy that corrects estimates of intergenerational earnings elasticities 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 US society is comparatively rigid.

Our estimates of the intergenerational earnings elasticity in Germany and the US that are best comparable to previous studies are 0.24 and 0.34, respectively. The US estimate seems close to the "reasonable guess" (Solon 1992) of around 0.4, but it should be kept in mind that this "guess" was rather a lower bound of the "true" earnings elasticity. In contrast, in the present study we use a lot more observations per person such that here the attenuation bias can be expected to be much smaller. So our estimate of 0.34 suggests that the US is actually quite a bit *more* mobile than other more recent studies indicated. Still, regression to the mean appears to be much slower than the studies surveyed in Becker and Tomes (1986) suggested.

The lifecycle bias affects the estimates of both countries very differently. We find differences in earnings growth between skill groups in both countries, though the variation in wage growth is estimated to be much stronger in Germany than in the US. This translates into a much more pronounced increase in the earnings elasticity in Germany than in the US once we take account of these differences in growth rates of earnings in the estimation of β. While the German estimate increases by 0.07 log points to 0.31, the US estimate only increases by a modest 0.02 log points to 0.36.

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 35. This fits remarkable well with the results of other studies (Haider and Solon forthcoming, 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 or late 30s.

To gauge the magnitude of the remaining attenuation bias, which has attracted so much attention in this literature, we further conduct a series of experiments. For each father and each son in the sample we randomly select five out of all available observations and then re-estimate β. The difference in the obtained average estimates of the experiments and our previous estimates then provides some insight into the underlying attenuation bias. In the German data we do not find evidence for serially correlated error terms, but in the US data we do. The results from these experiments by and large support the simulation results reported in Mazumder (2005)—though our estimates suggest that Mazumder's attenuation coefficient with $\delta = 0.3$ better describes the data than his "plausible" value of 0.5.

The attenuating effect of right-side measurement error on the estimated slope coefficient becomes the more visible in these experiments, the more observations per person are used in the main regressions. We therefore also estimate the model using a more selective subsample where a father-son pair is used only if earnings of both are observed at least ten times. The striking finding from this exercise is first that the obtained $\hat{\beta}$'s are much larger than in the bigger and less selective sample and second that this increase does not vanish when conducting the experiments. So there seems to exist a trade-off between two evils: The smaller the downward inconsistency due to the errors-in-variables bias, the more selective and thus, the findings suggest, the less representative the sample. If this proves to be true also in future research, correcting the estimates from nationally representative samples in a way as, for instance, proposed by Mazumder (2005) might be the best one can hope for.

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8 Appendix

Figure 1: Different wage growth

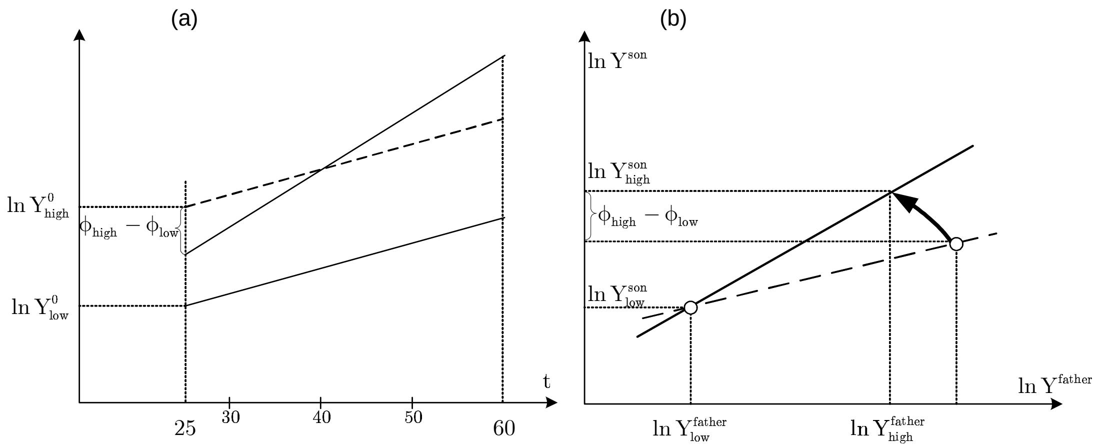


Figure 2: Different entry age

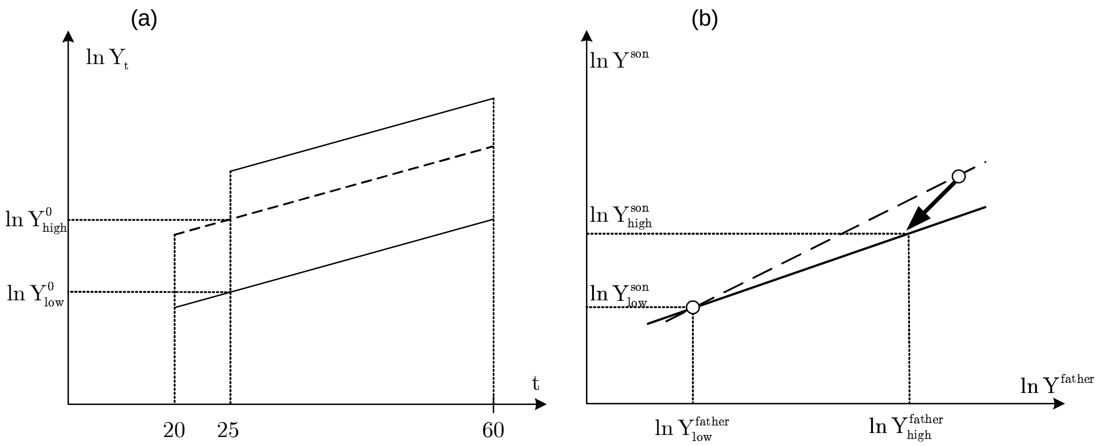


Table 1: Summary statistics

	GSOEP	PSID	CNEF (D)	CNEF (US)
Son's average age	30.4	33.1	30.4	32.0
	(2.45)	(3.63)	(2.45)	(3.29)
	[27-42]	[26-54]	[27-42]	[27-44]
Son's real earnings	31,562	42,689	30,115	43,275
	(12,098)	(31,382)	(13,138)	(28,843)
	[2,597-134,891]	[1,527-465,677]	[2,078-143,447]	[1,617-392,034]
Son's log real earnings	10.26	10.39	10.12	10.43
	(0.36)	(0.59)	(0.52)	(0.56)
	[7.8-11.7]	[7.2-12.2]	[7.2-11.7]	[7.3-12.6]
# obs. per son	10.0	14.2	10.1	12.1
	(4.0)	(6.79)	(3.9)	(4.9)
	[5-22]	[5-31]	[5-22]	[5-20]
# sons	525	876	609	619
Father's average age	50.4	47.9	50.6	51.4
	(4.69)	(6.05)	(4.58)	(4.85)
	[30-58]	[28-59]	[35-58]	[33-58]
Father's real earnings	33,752	48,334	33,525	55,797
	(16,862)	(32,708)	(17,104)	(52,801)
	[7,208-219,753]	[4,512-401,209]	[8,917-202,995]	[4,716-606,513]
Father's log real earnings	10.33	10.55	10.29	10.62
	(0.36)	(0.62)	(0.42)	(0.67)
	[8.7-12.2]	[7.5-12.2]	[8.9-11.9]	[7.6-13.1]
# obs. per father	12.2	17.5	12.1	12.6
	(4.9)	(7.2)	(5.0)	(4.9)
	[5-22]	[5-31]	[5-22]	[5-20]
# fathers	421	563	486	400

Note: Numbers in round parenthesis are standard deviations and those in square brackets denote the range of observed values. The panel is unbalances so the here described distributions are distributions of averages for each person. See text for a description of wage and earnings data. Years for which information on earnings are available are as follows: GSOEP 1983-2004, PSID 1969-2002, CNEF (D) 1983-2004, CNEF (US) 1979-2000. For Germany and the US earnings are reported in, respectively, Euros and US dollars of year 2000 (using the consumer price index of the US and, respectively, Germany).

Table 2: Step 1 estimates of α_1

	GSOEP	PSID	CNEF (D)	CNEF (US)
pooled estimation				
	.033	.046	.053	.055
	(.001)	(.002)	(.002)	(.002)
	53,072	88,225	56,908	65,886
	5,089	6,801	5,462	5,473
unrestricted estimation				
No qualification	.019	.037	.030	.039
	(.002)	(.004)	(.003)	(.006)
	11,389	17,627	11,724	9,965
	1,043	1,567	1,074	945
High School (US) / Vocational Training (D)	.026	.036	.039	.037
	(.002)	(.003)	(.002)	(.004)
	26,434	29,256	27,957	22,530
	2,562	2,313	2,710	1,922
Some College (US) / Higher Vocational Training (D)	.040	.047	.047	.046
	(.004)	(.003)	(.005)	(.005)
	4,939	19,694	5,708	15,905
	476	1,498	546	1,306
College (US) / University (D)	.067	.067	.119	.066
	(.004)	(.003)	(.006)	(.004)
	10,310	21,648	11,519	17,486
	1,008	1,423	1,132	1,300

Note: Cluster robust standard errors in round parenthesis. The following two numbers are the number of observations and the number of persons used in the respective estimation. Sample consists of men aged 25-60 with at least 5 valid observations of individual wage or earnings.

Table 3: Estimated average log annuity of lifetime earnings

		GSOEP	PSID	CNEF (D)	CNEF (US)
Sons:					
medium skilled	pooled	10.28	10.23	10.19	10.34
		(0.016)	(0.029)	(0.025)	(0.035)
	unrestr.	10.26	10.22	10.15	10.32
		(0.016)	(0.032)	(0.024)	(0.033)
	#	287	298	323	213
high skilled	pooled	10.43	10.71	10.19	10.76
		(0.045)	(0.030)	(0.052)	(0.037)
	unrestr.	10.54	10.76	10.41	10.81
		(0.042)	(0.031)	(0.047)	(0.037)
	#	97	301	129	209
Fathers:					
medium skilled	pooled	10.27	10.34	10.18	10.31
		(0.024)	(0.041)	(0.028)	(0.055)
	unrestr.	10.27	10.21	10.20	10.28
		(0.024)	(0.038)	(0.027)	(0.047)
	#	178	186	211	123
high skilled	pooled	10.73	10.86	10.65	10.96
		(0.063)	(0.049)	(0.060)	(0.057)
	unrestr.	10.54	10.79	10.43	10.93
		(0.062)	(0.044)	(0.058)	(0.052)
	#	53	136	64	110

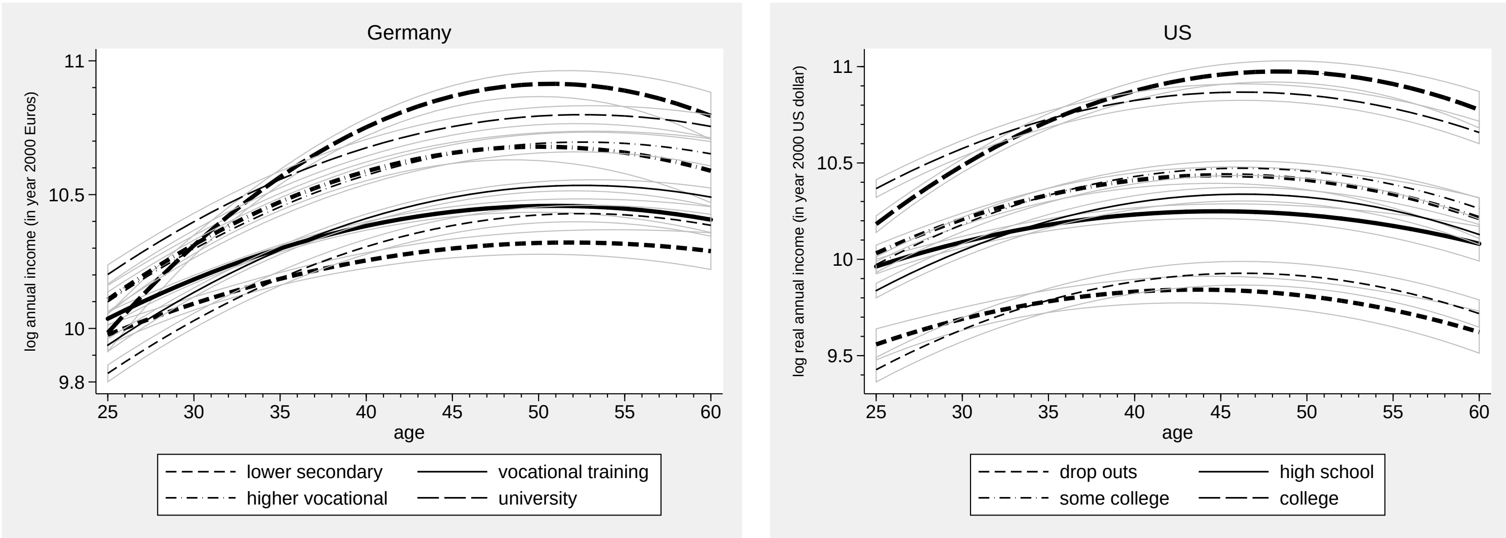
Note: Bootstrapped standard errors in parenthesis. Men aged 25-60 with at least 5 valid observations of individual earnings. Entry age is fixed, that is, persons of both skill groups are assumed to enter the labour market at age 19 in the US and at the age 20 in Germany.

Table 4: OLS Estimates of β from Log Lifetime Earnings Data

	GSOEP (1983-2004)	PSID (1969-2002)	CNEF (D) (1983-2004)	CNEF (US) (1979-2000)
	minimal number of observations: 5			
pooled				
entry age fixed	0.235	0.343	0.104	0.293
	(0.053)	(0.050)	(0.067)	(0.036)
entry age flexible	0.200	0.319	0.081	0.270
	(0.056)	(0.050)	(0.070)	(0.036)
unrestricted				
entry age fixed	0.308	0.357	0.211	0.306
	(0.059)	(0.053)	(0.066)	(0.037)
entry age flexible	0.266	0.337	0.189	0.285
	(0.059)	(0.054)	(0.066)	(0.037)
# fathers/sons	421/525	563/876	486/609	400/619
	minimal number of observations: 10			
pooled				
entry age fixed	0.413	0.398	0.256	0.379
	(0.075)	(0.045)	(0.072)	(0.053)
entry age flexible	0.374	0.372	0.228	0.359
	(0.078)	(0.046)	(0.072)	(0.054)
unrestricted				
entry age fixed	0.453	0.406	0.348	0.383
	(0.077)	(0.043)	(0.083)	(0.053)
entry age flexible	0.399	0.388	0.320	0.368
	(0.079)	(0.044)	(0.084)	(0.054)
# fathers/sons	132/156	328/490	157/187	156/222

Note: Bootstrapped standard errors in parenthesis. In the US High School drop-outs are assumed to enter the labour market at age 18, High School graduates at 19, men with some college at 22, and college graduates at age 24. In Germany entry ages of the four education qualifications less than secondary education, vocational training, higher vocational training, and university are respectively 18, 20, 23, and 25. Entry age is said to be fixed if it is assumed that all men enter the labour market at the entry age of the reference group which are High School graduates in the US and men with vocational training in Germany. Interest rates in the US are set at 0.0208 and in Germany at 0.0259.

Figure 3: Lifecycle earnings profiles in Germany and the US



Note: The light thin lines are 95 percent confidence intervals. See Table 2 for sample size of the plots.

Table 5: Robustness checks: attenuation bias vs sample selection

	GSOEP	PSID	CNEF (D)	CNEF (US)
	minimal number of observations: 5			
pooled				
Experiment 1	0.227	0.322	0.096	0.276
	(0.015)	(0.017)	(0.022)	(0.013)
Experiment 2	0.236	0.298	0.127	0.268
	(0.021)	(0.021)	(0.028)	(0.018)
unrestricted				
Experiment 1	0.283	0.326	0.188	0.277
	(0.017)	(0.016)	(0.023)	(0.013)
Experiment 2	0.288	0.302	0.210	0.269
	(0.022)	(0.021)	(0.029)	(0.018)
	minimal number of observations: 10			
pooled				
Experiment 1	0.388	0.378	0.253	0.340
	(0.027)	(0.023)	(0.044)	(0.030)
Experiment 2	0.384	0.346	0.307	0.312
	(0.041)	(0.032)	(0.055)	(0.036)
unrestricted				
Experiment 1	0.425	0.388	0.341	0.349
	(0.027)	(0.023)	(0.047)	(0.029)
Experiment 2	0.407	0.357	0.287	0.321
	(0.042)	(0.031)	(0.058)	(0.035)

Note: Standard deviations in parenthesis. Entry age fixed (see note of Table 4). 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.

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