Labour Economics 17 (2010) 392–406

Contents lists available at ScienceDirect

Labour Economics

journal homepage: www.elsevier.com/locate/labeco

Life-cycle variations in the association between current and lifetime earnings: Evidence for German natives and guest workers☆

Jan Brenner *

RGS Econ and RWI-Essen, Hohenzollernstr. 1-3, 45128 Essen, Germany

ARTICLE INFO

Article history:
Received 29 October 2007
Received in revised form 18 March 2009
Accepted 25 March 2009
Available online 15 May 2009

JEL classification:
C10
C50
J61

Keywords:
Generalized errors-in-variables model
Life-cycle bias
Lifetime income
Guest workers

ABSTRACT

In many economic models a central variable of interest is lifetime or permanent income which is not observed in survey data sets and typically proxied by annual income information. To assess the quality of such approximations, we use a unique source of lifetime earnings — the German pension system — and focus on two important issues that have been largely ignored in the existing literature. The first is how to deal with zero income observations in the analysis of women. The second is whether these approximations differ between natives and guest workers. For female earners, we find that estimates of the associations between current and lifetime income are highly sensitive to the treatment of zero earnings. The reason turns out to be the highly cyclical nature of the labor supply behavior of mothers. Furthermore, immigrants' income proxies are prone to significantly larger attenuation biases over the entire life-cycle. This result is explained by the larger share of annual income variance attributable to the transitory income component for immigrants. Averaging income over up to 15 years alleviates the attenuation bias as well as the difference in biases between natives and guest workers.

1. Introduction

Permanent or lifetime earnings are an important element in many economic models. Unfortunately, this measure of "economic status" or welfare is typically not observable in (survey) data sets. Most applied researchers hence approximate lifetime income by snapshots of earnings information such as annual income or an average thereof over several years if panel data is available. The arising measurement error is well understood and is typically addressed in the textbook errors-in-variables framework. This model implies an increase in noise if the dependent variable is replaced by a short-term income measure and an attenuation bias of the estimated parameter if the right-hand-side variable is approximated (see e.g. Wooldridge, 2002).

Recently, these simple assumptions are challenged by at least two strands of the economic literature. Among others, Baker and Solon (2003) and Mazumder (2001) show that permanent and transitory components of annual income vary systematically over the life-cycle. Estimating highly structured variance component models, they find that the share of variance due to transitory components is U-shaped along the work career using Canadian and US data. This implies that the textbook attenuation bias should vary along the life-cycle and ought to be minimal when workers are in their prime ages.

The literature on intergenerational income mobility is another field that heavily relies upon the errors-in-variables model. However, in his survey of the respective literature, Solon (1999) observes that estimates of the intergenerational income elasticity increase systematically with the age of sons. Grawe (2006), building on a model suggested by Jenkins (1987), formalizes this relationship and shows in a small meta-analysis that the average age of fathers is also a significant confounding factor of the intergenerational link.

In this study we adopt the generalized errors-in-variables model suggested by Haider and Solon (2006) and discussed in detail in the next section. Haider and Solon (2006) develop a stylized individual income growth model that allows for heterogeneous income growth paths across individuals. The heterogeneity in income profiles implies varying associations of annual and lifetime earnings over the life-cycle. In turn, this suggests that point estimates from studies that proxy lifetime by annual earnings are most likely biased. They suffer from what is called life-cycle bias. Using administrative income data over the entire career of a cohort of US men, Haider and Solon (2006) provide empirical evidence for this prediction. Böhlmark and Lindquist (2006) confirm their findings for a cohort of Swedish men and show that the patterns of associations differ considerably across gender and birth cohorts.

The contributions of this paper are threefold. First, we provide evidence on the association between lifetime and annual earnings for Germany and hence present a test of the external validity of the

☆ The author is grateful to Thomas K. Bauer, David Card, Christoph Hanck, Enrico Moretti, Christoph M. Schmidt and two anonymous referees for their helpful comments and suggestions. Parts of this paper were written while I visited the Center of Labor Economics, UC Berkeley, which I thank for the hospitality. Financial support by the Alfried Krupp von Bohlen und Halbach-Stiftung is gratefully acknowledged.

* Tel.: +49 201 8149285.
E-mail address: brenner@rwi-essen.de.

0927-5371/$ – see front matter
doi:10.1016/j.labeco.2009.03.006

findings of Haider and Solon (2006) and Böhlmark and Lindquist (2006). Second, we examine the impact of including or excluding zero income observations in the analysis of women. Female labor supply varies to a much greater extent than male labor supply over the life cycle. Hence, the estimates of the associations between current and permanent earnings might be fairly sensitive to the treatment of no income in the estimation procedure. Finally, we distinguish between German natives and a group of immigrants, the so-called guest workers. Since the number of immigrants and their descendants are ever rising in the industrialized world the necessity to understand their impact on host labor markets and their societal and economical integration is of utmost importance. Ever more data sources become available that allow researchers to assess these issues and compare natives and immigrants. As we argue in the next section, such comparisons that involve short-term income proxies for lifetime earnings might yield misleading inference.[1]

Our evidence for German men is broadly in line with the evidence for Sweden and the US. Life-cycle biases should be minimized if data in the age range 30 to 40 (35 to 45) is used to proxy permanent income on the left-hand-side (right-hand-side) of the regression. Our bias estimates for German women are less volatile over the life-cycle than the Swedish findings. This should partially be the result of differences in labor supply across countries, in particular of mothers. Further, female estimates are highly sensitive to the treatment of zero income observations. The recommendation for applied work is to drop these observations from the analysis and use current income information around the age of 50 when proxying lifetime earnings. Finally, we only find significant differences between the bias profiles of native and immigrant men and female natives and guest workers, respectively, when permanent income appears on the right-hand-side in an analysis. This is explained by the higher share of variance attributable to the transitory income component for immigrants. It is recommendable to average annual income information if panel data is available since the attenuation bias as well as the discrepancy in biases between immigrants and natives is constantly reduced by using ever longer averages of up to 15 years.

The remainder of this paper is structured as follows. In Section 2 we describe Haider and Solon's (2006) generalized errors-in-variable model and discuss why associations between lifetime and annual income might differ between immigrants and natives. Section 3 describes the estimation procedure and the data analyzed. Our findings are discussed in Section 4, we conclude in Section 5.

2. Associations between annual and lifetime income

2.1. Generalized errors-in-variables model

In what follows we adopt the methodology of Haider and Solon (2006) in modeling the association between annual and lifetime income. Let y_i denote the log of lifetime income and y_{it} the log of annual income in period t. Then the generalized errors-in-variables model is given by

$$y_{it} = \lambda_t y_i + u_{it} \tag{1}$$

where λ_t denotes the time-dependent bias arising when proxying the typically unobservable dependent variable y_i by an annual measure and u_{it} denotes a regression error term.[2] The standard errors-in-variables model is obtained when the λ_t's are restricted to unity.

Fig. 1. Income profiles of male German graduates and skilled workers.

Haider and Solon (2006) offer the following intuition for expression (1). Workers experience individually different labor market careers which are expressed in heterogeneous income profiles over the life-cycle. In particular, the growth rates of annual income vary across (groups of) workers.[3] Therefore, at different stages of the career, the annual income distribution is sometimes a better, sometimes a worse approximation of the distribution of lifetime income.

To illustrate this point, in Fig. 1 we plot the lifetime income profiles of German male university graduates against male workers who completed an apprenticeship along with the annuitized present discounted values of these income profiles.[4] The horizontal lines represent these latter measures of lifetime earnings and show that university graduates attain higher lifetime earnings. At young ages, however, they earn considerably less than workers who completed an apprenticeship. Yet, their income grows faster basically through out their entire career. As a result, the lifetime earnings gap, given by the vertical difference of the horizontal lines, should be underestimated when proxied by current earnings until the age of 35.[5] While the approximation should be fairly precise until the age of 40, the earnings gap should be overstated with income information from older ages.

The striking insight of the model given in Eq. (1) is that a proxy of the dependent variable might create a bias in the estimated coefficients of all covariates in the model. This bias is represented by λ_t. It attenuates the true relationship whenever $\lambda_t < 1$ and amplifies it whenever $\lambda_t > 1$. The standard errors-in-variables assumptions, on the other hand, imply that approximating the left-hand-side variable by current income only increases the noise in the data but yields consistent parameter estimates. According to this textbook model, a real problem only arises if an independent variable is replaced by an imperfect signal which leads to the well known attenuation bias of the estimated coefficient. In the generalized model this attenuation bias

[1] Current research that assesses differences between German natives and immigrants and involves permanent income measures as determinants e.g. analyzes savings behavior (Bauer and Sinning, forthcoming) and home-ownership (Sinning, 2006). Other studies that might be affected by differences in life-cycle bias investigate the heterogeneity in intergenerational income elasticities across different groups of immigrants, among others Borjas (1992, 1994) and more recently Hammarstedt and Palme (2006).

[2] We adopt the convention in Haider and Solon (2006) and omit intercepts from all equations, i.e. all variables should be considered in deviation from their means.

[3] Carroll and Summers (1989) show that different careers and occupations exhibit heterogeneous age-income profiles in the US.

[4] Fig. 1 is based on estimates using a subsample of the data from the main analysis. Annual average income by education group is estimated by tobit models including a constant and a dummy for university graduates. To compute the lifetime income measure, we discount the tobit-estimates by $r = 0.02$. The education information is not used in the main analysis due to too many missing values, in particular in the immigrant samples. For a detailed description of the data and the estimation approach see Section 3 and Appendix A.

[5] It might even be negative at the beginning of the career since college graduates earn massively less than workers in this period.

depends on λ_t and hence is time-varying as well. Formally, Haider and Solon (2006) assume

$$y_i = \theta_t y_{it} + \xi_{it} \tag{2}$$

where the time-varying attenuation bias θ_t is given by

$$\theta_t = \frac{\lambda_t \text{var}(y_i)}{\lambda_t^2 \text{var}(y_i) + \text{var}(\xi_{it})} \tag{3}$$

and ξ_{it} is a regression error term.[6]

To summarize, the generalized errors-in-variables model suggests that all slope coefficients in a regression that proxies the dependent variable lifetime earnings by current income are biased by the factor λ_t which varies over the life-cycle. On the other hand, if lifetime income enters the regression on the right-hand side, then the corresponding coefficient is biased by the factor θ_t. As an example, think of estimating the intergenerational income elasticity (IGE) between a sample of 30-year-old sons (indexed by s) and 55-year-old fathers (f) using one year of annual income information for both generations. Combining Eqs. (1) and (2) yields

$$y_{i30}^{s} = \lambda_{30}^{s}\theta_{55}^{f}\beta y_{i55}^{f} + \text{error}_i,$$

with β denoting the IGE, λ_{30}^{s} the bias arising from approximating sons permanent income by annual information and θ_{55}^{f} the bias induced by the income proxy for fathers.

2.2. Differences between natives and immigrants

One aim of this study is to test whether it is necessary to distinguish subgroups of the population when assessing the quality of annual income measures as proxies for lifetime income. Böhlmark and Lindquist (2006) show that patterns of life-cycle bias differ considerably by gender and birth cohorts using Swedish data. While we distinguish between men and women as well, the additional focus of our analysis is on potential differences between natives and immigrants.

In particular, we analyze the group of guest workers in Germany. The majority of this immigrant group entered Germany since the 1950s until the first oil price shock in 1973.[7] They comprise predominantly blue collar workers from Turkey, former Yugoslavia, Italy, Greece, Spain, and Portugal. After the oil price shock active recruiting from these countries was stopped. Yet, due to family reunification policies, immigration of mainly women and children from these countries continued. While guest workers were encouraged to leave Germany during this period — since the mid-eighties they were offered among other things financial incentives to remigrate —, many of them stayed permanently. As of today, this group of immigrants and their descendants form a visible minority in the German society.

In the context of this study, one needs to ask the question why there might be differences in the association between current and lifetime income of natives and guest workers? One theoretical consideration can be based on the assimilation idea of Chiswick (1978). Due to the limited transferability of human capital between the home and the host countries, immigrants face a competitive disadvantage on the labor market vis-à-vis natives upon arrival. This creates an incentive to invest in additional (country-specific) human capital. As a consequence, the income profiles of immigrants are expected to start on a lower level than those of similarly skilled natives. However, by acquiring the necessary complementary skills to fully exploit their initial human capital, it is assumed that they catch up to native income levels. Hence their trajectories would have to be steeper than those of natives during the assimilation process. The bottom line is that income patterns can be expected to differ systematically between the two groups which in turn might affect the association between earnings along the life-cycle and lifetime earnings.

Another reason why we might expect different patterns in the bias trajectories of German natives and the group of guest workers is the stark difference in their skill distributions. As depicted in Fig. 2, guest workers are highly concentrated in the left tail of the skill distribution while natives on average exhibit higher education levels.[8] This fact might lead to differences in the heterogeneity of income profiles for natives and immigrants if the association between skills and life-cycle income paths is sufficiently strong. Fig. 1 as well as the evidence presented by Carroll and Summers (1989) for the US suggest that such relationships are indeed significant.

Finally, the increasing unemployment in Germany since the 1980s had a much stronger impact on guest workers than on natives and led to significantly higher unemployment rates among this group of immigrants (Bauer et al., 2005b). Since more immigrants experienced sharp breaks in their income profiles due to unemployment than natives, this might have a visible impact on the bias trajectories.

3. Data and estimation

To estimate patterns of life-cycle bias in Germany we use the *Vollendete Versichertenleben* (VVL) 2004 of the Research Data Centre of the German Statutory Pension Insurance (FDZ-RV). For German natives[9] we rely on the Scientific Use File, whereas guest workers are analyzed with data from a larger sample of the VVL.[10] The Scientific Use File of the VVL contains longitudinal information about a random sample of roughly 5% of all individuals born between 1939 and 1974 who received statutory pension payments for the first time in 2004.[11]

The main source of earnings considered in this study is gross annual income subject to social insurance contribution which we deflate to real values in terms of year 2000 Euros using the German Consumer Price Index. Unfortunately, this income information is only reported up to an annually changing contribution ceiling. An additional limitation of this data is its representativeness. Neither civil servants nor most of the self-employed are covered. According to the Federal Statistical Office Germany, however, the VVL should still be representative for at least three quarters of the registered German labor force.[12] The problems of censored income information and incomplete representativeness of the data are shared by the study of

[6] Haider and Solon (2006) argue that it is actually possible that θ_t turns out to be amplifying rather than attenuating.

[7] For an overview of the German migration history since World War II see e.g. Schmidt and Zimmermann (1992) or Bauer et al. (2005a).

[8] The education distributions are calculated for individuals born between 1939 and 1944, the cohort scrutinized in the main analysis of this paper. The figure is based on the German Socio-Economic Panel (GSOEP). Guest workers comprise Turks, former Yugoslavs, Italians, Greeks, and Spaniards. Averages are computed from pooled person-year observations from 1984–2004 to capture potential education dynamics. All statistics are weighted to be representative for the German population. The data used was extracted from the SOEP Database provided by the DIW Berlin (http://diw.de/soep) using the ADD-ON package SOEPMENU v2.0 (Jul 2005) for Stata(R). See Haisken-DeNew (2005) for details.

[9] We only consider natives from the former West Germany since the guest worker program was initiated by the government of the Federal Republic of Germany in the 1950s. Hence, all guest workers came to West Germany which makes the population of this part of Germany the natural comparison group. Throughout the paper we, however, do not emphasize this selection and simply talk about Germans or (German) natives.

[10] All Turks, former Yugoslavs, Italians, Greeks, Spaniards, and Portuguese are defined as guest workers. For a detailed discussion of the VVL as well as the other data sources used to complement it see Appendix A.

[11] The larger data set consists of a 25 % sample. It can be accessed in either one of the two data centers of the FDZ-RV (in Würzburg or Berlin) and can be complemented with more detailed information about the individuals. In this study, we use the nationality of immigrants which is not available in the Scientific Use File.

[12] For more details, see the data description in Appendix A.

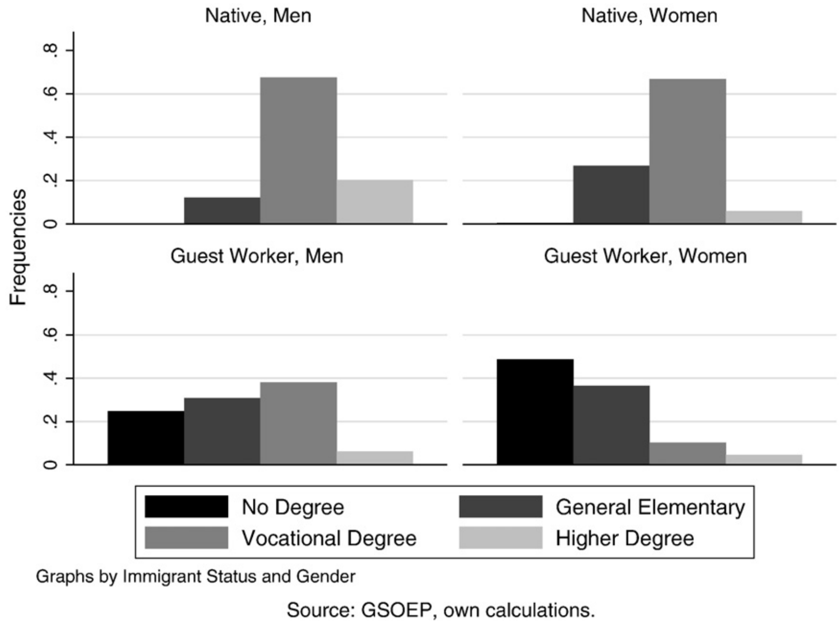

Fig. 2. Skill distributions of natives and guest workers.

Haider and Solon (2006) who also rely on social security earnings data. Böhlmark and Lindquist (2006), on the other hand, use much broader income information from tax registers which yields samples that are representative for the entire Swedish population. Their measure of pretax, total net income contains labor earnings and labor-related transfers, pensions as well as business and capital income.

To maintain comparability with the studies of Haider and Solon (2006) and Böhlmark and Lindquist (2006) 41 years of data are used for German Natives born between 1939 and 1944. These individuals turn 19 years of age in the first year and 59 in the last year of the utilized subsample. Furthermore, we restrict the sample to individuals with at least 10 years of positive income information over their life-cycle. Haider and Solon (2006) report that this criterion reduces their sample size by less than 4% while Böhlmark and Lindquist (2006) state “to lose only a handful of people” (footnote 11, page 885). For the birth cohorts 1940 to 1944 we have to discard 1% (0.002%) of native (immigrant) men and 3.3% (3%) of native (immigrant) women due to this criterion.

Unfortunately, for the birth cohort born in 1939 we lose a substantial amount of observations: 29.2% (8.7%) of German (immigrant) men and 61.7% (51.1%) of German (immigrant) women. These numbers are a result of the data design. Only those individuals born in 1939 who retired at the regular retirement age of 65 are included in the VVL 2004. To be entitled to this regular pension scheme it is sufficient to contribute for five years to the system.[13] This implies that e.g. men who worked for more than five years as an employee in the private sector but then became self-employed or civil servants are included, yet we only see the years of employment relevant to the statutory pension scheme. Similarly, a woman who worked for some years at young age and then became a housewife would be in the data. Furthermore, immigrants who complied with the initial idea of the guest worker contracts and left Germany after five or ten years are represented in this cohort. Since many of these individuals do not work for at least 10 years, they are excluded from our sample which explains the extraordinary high numbers for the 1939 cohort. For all other birth cohorts in the VVL 2004 these types of careers are not observed since (by definition of the data set) these individuals retire at an earlier age, not the regular retirement entry age of 65. For the alternative retirement schemes the minimal contribution period is much longer. In order to assess whether the composition of the 1939 birth cohort has a decisive impact on our results, we redo the analysis for the German samples excluding this birth cohort. We find no significant deviations from our main results presented below.[14]

3.1. Estimation approach

The VVL 2004 consists of individuals who received retirement payments for the first time in 2004. This implies that the entry age into retirement varies systematically with the birth year in our sample. Thus, comparisons of the associations of individuals born in 1939 and 1944, for example, would not only contrast the cohort effect. They would also pick up potential systematic differences in the labor market characteristics of the two groups which are reflected in their retirement age decisions.

To avoid these selection issues, we pool the information for the six birth years from 1939 to 1944 and assume that the life-cycle association between lifetime earnings and annual income is stable over this period.[15] There are two advantages of this approach. First, our samples are considerably larger than most of the samples analyzed in Haider and Solon (2006) and Böhlmark and Lindquist (2006).[16] Second, the pooled samples comprise of individuals who retired at ages 60 to 65 and are therefore more representative than the single birth year samples. As a result, however, the maximum age gap between the youngest and the oldest individual in any calendar year is six years. We therefore prefer to obtain age and not year specific first and second moments. This is the most important deviation from the studies for the US and Sweden.[17] In all other respects, we follow the

[13] There are several ways to contribute to the German pension system, among others employment in the private sector, several types of education, childcare, and military service.

[14] These results are available upon request.

[15] This assumption does not appear to be too restrictive since labor market conditions and consequently individual career prospects should not have changed drastically for individuals born in this period. Haider and Solon (2006) pool information over three, Böhlmark and Lindquist (2006) over five years.

[16] The sample sizes are 8728 and 8470 for native men and women, respectively, and 1308 and 837 for male and female guest workers, respectively.

[17] We also estimate the model obtaining year specific moments as it is done in the other two studies. The results from this specification do not deviate from the presented evidence in any meaningful way and are available upon request.

Table 1
Median incomes and censoring frequencies over the life-cycle.

	Men						Women					
	German natives			Guest worker			German natives			Guest worker		
Age	LC	Income	RC	LC	Income	RC	LC	Income	RC	LC	Income	RC
19	7.66	6474	0.02				13.96	5394	6.22			
20	6.28	7628	0.02				16.82	6348	5.04			
21	6.19	7740	0.11				20.02	7098	3.92			
22	7.28	9625	0.11				24.33	7373	2.54			
23	7.82	11,771	0.28				28.42	7301	1.72			
24	7.81	12,981	0.44				31.16	7105	0.96			
25	6.79	14,505	0.54	3.54	12,798	0	33.15	6813	0.49	23.68	7420	0.29
26	4.10	16,121	1.17	1.94	14,937	0.16	35.54	6560	0.46	10.54	9258	0.26
27	2.72	17,613	2.44	1.36	15,998	0.27	37.18	6382	0.41	13.73	10,128	0
28	1.69	19,261	4.01	0.97	17,493	0.12	37.85	6116	0.36	14.99	10,737	0
29	1.32	21,347	5.50	0.82	18,641	0.31	38.58	6090	0.35	14.84	12,188	0
30	1.30	23,362	9.10	0.46	19,875	0.46	39.00	6114	0.46	15.80	12,666	0
31	1.11	24,818	12.64	0.43	20,671	1.03	39.65	6296	0.46	16.69	13,298	0
32	1.14	26,149	15.80	0.17	21,693	0.84	39.27	6629	0.63	16.23	13,892	0.14
33	0.98	27,156	16.10	0.16	22,369	0.81	38.57	7169	0.59	14.79	14,613	0
34	0.93	27,873	16.49	0.08	23,243	1.10	36.61	8058	0.57	14.02	15,380	0
35	0.83	28,702	16.70	0.08	24,035	1.18	34.59	8559	0.42	12.40	15,993	0
36	0.97	29,702	15.92	0.40	24,745	1.11	32.00	9661	0.44	13.29	16,434	0
37	0.94	30,472	15.17	0.63	25,387	0.79	29.65	10,463	0.55	13.61	16,578	0
38	1.07	31,055	15.01	0.94	25,605	0.79	27.08	10,993	0.51	14.12	16,710	0
39	1.28	31,476	15.03	2.05	25,664	0.95	24.95	11,611	0.38	13.70	16,800	0
40	1.38	31,886	16.65	1.89	25,805	0.95	22.44	12,174	0.58	11.23	16,582	0
41	1.34	32,238	18.11	2.06	26,029	1.11	19.91	12,577	0.66	11.38	16,994	0
42	1.64	32,522	18.71	2.31	26,160	1.51	17.67	13,111	0.83	11.20	16,992	0
43	1.81	33,130	20.19	2.32	26,562	1.28	14.87	13,872	1.09	12.15	17,265	0
44	2.16	33,795	21.09	3.61	26,834	1.12	12.41	14,560	1.07	12.88	17,390	0
45	2.15	34,481	22.18	2.97	27,527	1.20	10.51	15,240	1.38	11.11	18,250	0
46	2.50	35,250	22.81	3.13	28,235	1.36	9.31	15,670	1.43	9.62	18,353	0
47	2.14	36,212	24.25	3.21	28,263	1.44	7.52	16,534	1.54	9.33	19,143	0
48	1.97	36,705	24.61	2.98	28,554	1.45	6.52	17,264	1.77	9.60	18,866	0
49	2.15	36,996	24.80	5.20	28,591	1.76	5.39	17,656	1.63	10.84	18,341	0.14
50	2.22	37,079	24.88	7.77	28,324	1.52	5.29	17,612	1.65	14.89	17,604	0.14
51	2.51	37,605	24.85	10.60	28,495	1.20	6.53	17,667	1.52	18.58	17,438	0
52	3.13	37,439	23.85	14.87	27,895	1.69	7.87	17,484	1.33	22.87	16,896	0
53	4.11	37,033	22.54	19.40	27,291	1.29	9.91	17,250	1.45	25.46	16,057	0
54	5.10	36,578	21.68	21.68	26,831	1.13	10.97	16,969	1.27	30	15,562	0
55	6.45	35,866	13.08	24.59	26,203	0.81	12.49	16,680	0.67	30.94	14,375	0
56	9.05	34,977	18.62	27.93	25,203	1.39	15.86	15,788	1.14	33.96	12,469	0
57	11.18	34,097	17.98	30.79	23,982	0.99	18.10	15,091	0.97	36.79	11,295	0
58	13.95	33,304	16.30	36.60	21,538	0.99	21.22	13,863	0.87	40.77	7,504	0
59	18.45	32,253	16.01	42.67	18,809	0.99	26.58	12,775	0.93	44.79	4,378	0

Notes: RC (LC) refers to the fraction of right-censored (left-censored, i.e. zero) income. Income denotes real median income in year 2000 Euros.

estimation procedure suggested by Haider and Solon (2006). In the first step, we estimate the following tobit models:

$$y_{ia}^{*} = \mu_a + \epsilon_{ia} \qquad (4)$$

with $i=1,\ldots,N$ indexing the individuals at ages $a=19,\ldots,59$ to obtain the means (μ_a) and the variances of the uncensored, partially unobservable annual incomes y_{ia}^{*}. The link between y_{ia}^{*} and the observable, censored outcomes y_{ia} is given by

$$y_{ia} = \begin{cases} U_{it} & \text{if an individual earns income beyond the contribution ceiling} \\ y_{ia}^{*} & \text{if positive income below the contribution ceiling is reported} \\ L_t & \text{if zero income is reported.} \end{cases}$$

It is important to note that the upper thresholds (U_{it}) as well as the lower thresholds (L_t) vary with the year of observation indexed by t, not with the individuals' age. The former value is set every year by the government. Due to the construction of the annual income variable from monthly observations, this censoring point can also vary across individuals within a given year.[18] Whenever an individual reports no labor earnings in a year, income is set to 0.2% of the national average income subject to social insurance contribution of that year and is treated as censored from below.[19] In this manner, we can include observations which would otherwise be lost due to taking logs. Alternatively, we exclude zero income observations and estimate the moments with one-limit tobit models.[20] The treatment of zero income observations might be crucial, in particular for the female samples. They generally supply less labor than men. Furthermore, female labor supply follows a distinct pattern over the life-cycle which is closely related to childcare.[21]

In the second step, the correlations between annual earnings y_{ia}^{*} and y_{is}^{*} for each age combination $a \neq s$ are estimated element by element with bivariate tobit models. Again, we allow for flexible censoring points as described above. The 820 non-redundant off-diagonal elements of the variance–covariance matrix of annual income are then computed by combining these correlations with the variance estimates from the univariate tobit estimations. Unfortunately, for German and immigrant men, we encounter severe

[18] See Appendix A. for details about the construction of the income variable.

[19] Haider and Solon (2006) set income to 50 US $ in such cases.

[20] Böhlmark and Lindquist (2006) exclude zero income observations throughout their analysis. Since their income data is not top coded either, they can compute lifetime earnings directly from the data and estimate Eqs. (1) and (2) by OLS using the annual income information and their measure of permanent income.

[21] A more detailed discussion follows in Section 4.

convergence problems with this approach when we treat zero incomes as left-censored. We therefore cannot report results for these specifications. However, we are confident that the specifications excluding zero income observations should yield very similar results due to the following reasons. Firstly, Haider and Solon (2006) find similar life-cycle associations for both methods. Secondly, the share of zero incomes reported in Table 1 is relatively low for both groups of men, in particular for natives and in comparison with the data used by Haider and Solon (2006).

Finally, we combine the moments from the first two steps and draw 4000 observations from the resulting multivariate normal distribution. Lifetime earnings are computed as the log of the present discounted value of the 41 years of annual income. To maintain comparability to the literature we use $r = 0.02$ as the interest rate for discounting in our main analysis. λ_t and θ_t are then estimated one-by-one by OLS using the 41 years of simulated data and the constructed lifetime income variable. Standard errors of the bias coefficients are obtained by 50 bootstrap repetitions of the entire procedure, once again following the suggestion of Haider and Solon (2006).

To assess whether there are significant differences in the life-cycle bias pattern between Germans and guest workers as well as men and women, we estimate Eqs. (1) and (2) separately by gender for natives and immigrants, respectively.

3.2. Representativeness of the immigrant samples

There are several issues concerning the representativeness of our immigrant samples for the underlying population of guest workers in Germany. A first problem of the VVL 2004 is the fact that the nationality information is collected in 2004. Hence, we cannot identify naturalized immigrants which consequently appear in the native samples. According to Constant et al. (2007) this problem should be moderate. They find that less than a fourth of the eligible Turks and Ex-Yugoslavs and practically none of the guest workers from EU countries take up German citizenship using data from the 2005 wave of the GSOEP.

The rather low share of guest workers naturalizing is reassuring: the results for natives should be unaffected by the small numbers of naturalized immigrants.[22] There might, however, be an effect on the immigrant analysis since there is some evidence that naturalized guest workers are a selected group in terms of their labor market outcomes. Constant et al. (2007) find that education is positively associated with the likelihood to naturalize among former Yugoslavs and Turks. In their analysis of immigrants from Ex-Yugoslavia, Poland, Iran, Lebanon, and Turkey to Germany using data from the Rockwool Foundation Migration Survey administered in 2002, Constant and Zimmermann (2005) find that naturalized immigrants are more likely to work full-time and earn more in full-time employment than immigrants who did not take up German citizenship. These results indicate that our sample could represent the more educated and economically more successful guest workers insufficiently. The possible effect on our results, however, remains unclear. One might speculate that the overall heterogeneity in income profiles might be reduced since the in anyway small group of highly educated guest workers with the potentially steepest and highest profiles is under-represented. As a consequence, the life-cycle bias might be under-estimated since the remaining income profiles are more homogeneous.

A second problem is that most guest workers migrated when they were older than 19 and many, the return migrants, left before they turned 59. Since we only observe immigrants while they are in Germany, this could potentially distort our analysis which relies on

Table 2
Average autocorrelations of different cohorts of men.

Order of autocorrelation	German men	Guest workers	American men	Swedish men
	Aged 43–52		Aged 42–53	Aged 42–55
	Born 1939–44		Born 1931–33	Born 1939–43
1	0.86	0.65	0.89	0.79
2	0.82	0.59	0.82	0.70
3	0.79	0.54	0.78	0.66
4	0.77	0.48	0.75	0.64
5	0.74	0.45	0.72	0.64
6	0.71	0.42	0.69	0.63
Source	Own calculations	H & S (2006)	B & L (2006)	

Notes: Haider and Solon (2006) is abbreviated by H & S (2006), Böhlmark and Lindquist (2006) by B & L (2006).

information over the entire life-cycle. However, as argued in the beginning of this section, only the immigrant cohort born in 1939 should include a sizable number of return migrants due to data design. A glance at the evolution of our sample size for male guest workers over the life-cycle reveals that at age 19 only 101 (out of 1308) individuals are already observable. This number rapidly increases and peaks at age 34 and 1270 observations to remain fairly constant over the rest of the career (1213 observations at age 59). The profile for women is similar, peaking at age 37 and exhibiting a somewhat stronger decline towards the end of the career (from 768 to 672 observations out of 837). These reductions of observations over the life-cycle, probably partially induced by return migration, appear moderate and most likely should not have a sizable effect on our findings. Furthermore, the evidence on the selectivity of return migration from Germany does not suggest a strong distorting impact on our analysis.[23]

All in all, our sample constructed from the VVL 2004 rather seems to be representative of guest workers who permanently stayed in Germany. This interpretation is further backed up by the years of employment of immigrants over the 35 years of data analyzed. On average, male (female) guest workers are employed in 29.3 (24.6) years which is even longer than natives in this time span (28.3 and 21.8 years, respectively). This implies that immigrants complying with the initial idea of the guest worker contracts and returning to their home country after a certain amount of time are only insufficiently represented in the data. However, since the main interest of most immigration studies is on permanent immigrants this selection can actually be considered advantageous.

Due to the relatively low numbers of observations until the age of 24 in both immigration samples, we exclude this period from the analysis. To obtain comparable estimates for natives, we redo these estimations with the shorter time span of 35 years as well. None of the major characteristics of the native bias profiles is affected by the shorter time span. $\lambda_t-(\theta_t-)$profiles are slightly shifted downwards (upwards). Hence, we seem to lose no important information when we compute permanent income discarding income from the very beginning of the work life.

3.3. Sample characteristics

In the left half of Table 1 we depict summary statistics of the samples of native and immigrant men that we analyze. The median incomes and the censoring frequencies from above of natives are

[22] Immigrants of German ethnicity who originate from Eastern Europe, the so-called Spätaussiedler, are identifiable and are excluded from the data.

[23] Constant and Massey (2003), using 14 years of GSOEP data, find that occupational prestige and stable employment are negatively associated with emigration. No significant effect is found with respect to human capital. Furthermore, Constant and Massey (2003) do not detect any significant differences in the labor market income of permanent and return migrants over the life-cycle. Dustmann (2003), using the same data, finds an inversely U-shaped relationship between wages and completed migration durations. Further, he detects a negative association between wage increases and the intended migration duration.

considerably higher at all stages of the career. This is not surprising considering the education distributions depicted in Fig. 2. Differences in censoring from below are relatively small during most of the years, yet considerably higher for guest workers in the end of the careers.[24]

The picture for women is less clear cut. Until the age of 43 the labor force participation of native women in our sample is considerably lower than the participation of female guest workers. The median income of the former group is lower until the age of 49. Towards the end of the career the share of non-participants rises rapidly for immigrant women which is accompanied by a sharp drop in median income. It appears that the women in our sample on average have fairly different labor market participation patterns with respect to their migration status. Finally, censoring from above is low for native women during the entire life-cycle and virtually nonexistent among female guest workers.

4. Empirical evidence

4.1. International comparison

In providing evidence for the association of lifetime income and annual income in a new country, we aim at assessing the external validity of the findings for the USA (Haider and Solon, 2006) and Sweden (Böhlmark and Lindquist, 2006).

4.1.1. Men

We begin the international comparison with findings for men. First, it is important to note the differences in birth cohorts. While we choose the Swedish evidence to match the cohort of our study[25], the American men are born almost a decade earlier. As Böhlmark and Lindquist (2006) show, life-cycle bias profiles vary across generations in Sweden. Hence, differences found between countries might partially be a consequence of the birth cohorts considered. Second, while both the analysis for the US and Germany are based on labor earnings, the Swedish study uses a broader income variable that consists of labor earnings, labor-related transfer payments, as well as pensions, business and capital income. Furthermore, the Swedish data is not censored from above. Hence, Böhlmark and Lindquist (2006) compute permanent income directly from the observed income streams and estimate Eqs. (1) and (2) by OLS, dropping zero annual income observations.

From Table 2 we observe that the average auto-correlation of annual income in the prime ages up to the order of 6 is very similar in the US and Germany. Swedish earnings appear to be the least persistent since auto-correlations of all orders are distinctly lower than in the other two countries.

In the top panel of Fig. 3, $\hat{\lambda}_t$'s for men from the three countries are depicted.[26] The German profile shows exactly the features that could be expected from the life-cycle income profiles of German university graduates and skilled workers depicted in Fig. 1: A massive under-estimation of the lifetime income at the beginning of the career that is rapidly reduced until the early thirties. After the age of 40 the permanent income gap is constantly overestimated.[27] The overall

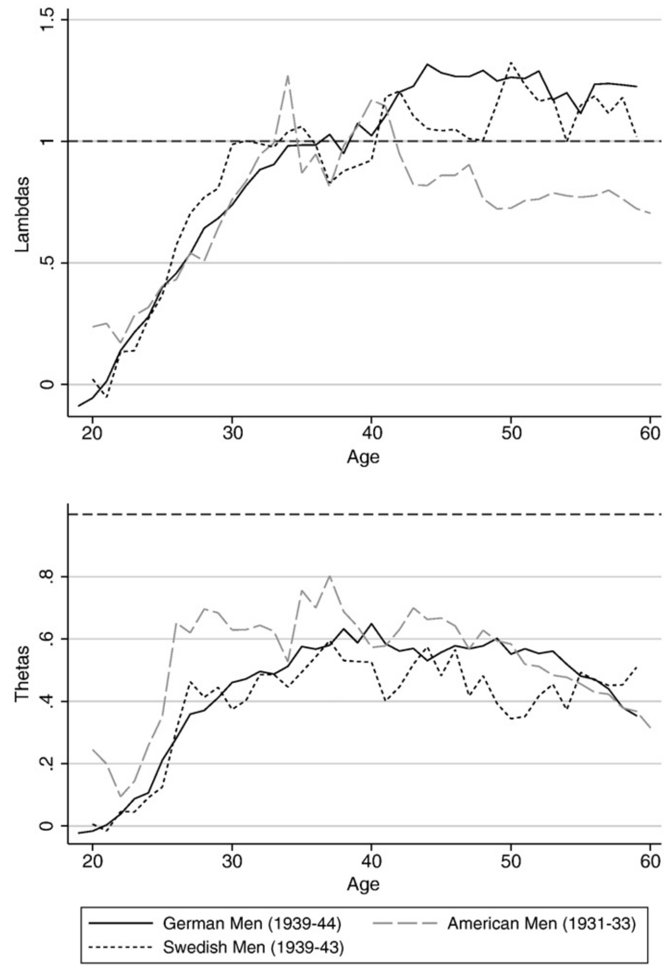

Fig. 3. International comparison of bias profiles for men.

resemblance of the Swedish and the German cohort is striking. For the US data this is only true in the first half of the career. Until the age of 41, 95% confidence bands around the three profiles overlap in almost all years. Thereafter, current income underestimates lifetime earnings in the US contrary to the European countries. This difference is significant in virtually all years.[28]

With respect to the estimated θ_t's, depicted in the bottom panel of Fig. 3, we again find a closer resemblance between Swedes and Germans than between US men and the Europeans, respectively. The shapes of the profiles are similar in all three countries. They start with severe biases and exhibit a more or less flat region over most of the career. At older ages the biases increase again. Overall, the attenuation bias appears to be strongest in Sweden and the least severe in the US.[29] The difference between the US and Swedish estimates are significant in 24 years in the age range 20 to 51, between the US and Germany in 18 years in the age range 19 to 44, and between Germany and Sweden in 10 years in the age range 40 to 54. Yet, for none of the countries, at any stage over the life-cycle, a $\hat{\theta}_t$ close to unity is found.

It is tempting to interpret the similarities of findings for Swedes and Germans to be a result of more similar labor market institutions and educational systems as compared to the US. However, Böhlmark and Lindquist (2006) show that a Swedish cohort of men born in the early 1930s actually resembles the comparable US cohort analyzed by Haider and Solon (2006) at least as closely as it resembles the Swedish cohort discussed in our study. Hence, a significant part of the patterns

[24] To the extent that zero income reflects unemployment this pattern might be explained by the stronger impact of the rising unemployment in Germany in the 1980s on guest workers.

[25] Böhlmark and Lindquist (2006) provide evidence for men and women from three different birth cohorts.

[26] Point estimates of all bias parameters estimated for this study are tabulated in Appendix B.

[27] We perform several robustness checks with respect to the data definition (see Appendix A for details on the data sources used): (i) we increase the interest rate for discounting to 0.04 as suggested by Haider and Solon (2006); (ii) we exclude all of the imputed income information; (iii) we treat annual income as censored from above if at least one month per year is censored, and (iv) if all 12 months are censored. None of these changes has a meaningful impact on our findings. Results are available upon request.

[28] Furthermore, the German parameters are significantly higher than Swedish point estimates at ages 44 to 48 and in the last year.

[29] The magnitude of the bias is defined as the absolute deviation of $\hat{\theta}_t$ from unity.

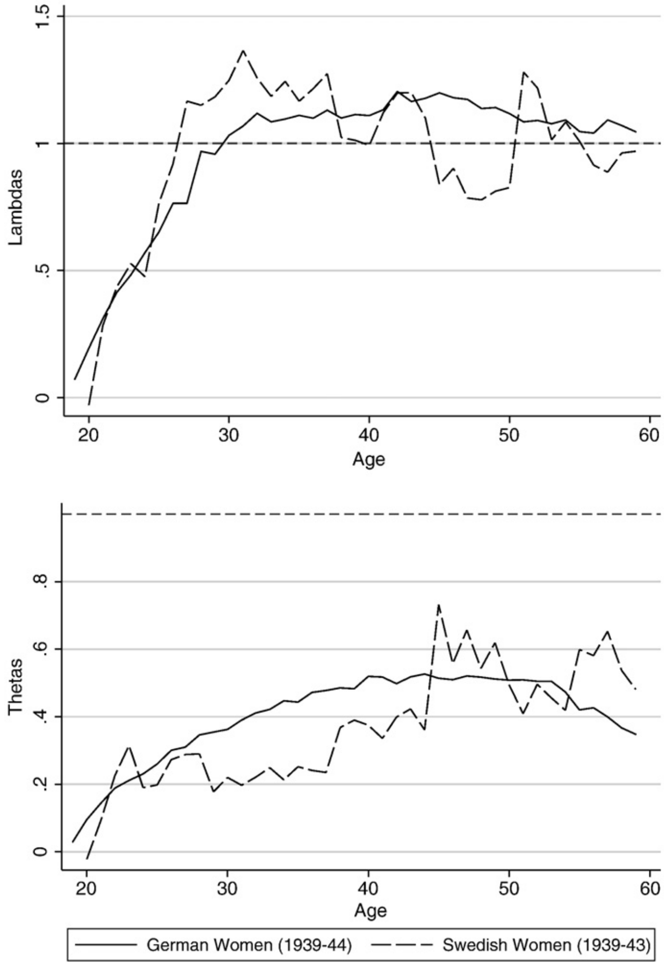

Fig. 4. International comparison of bias profiles for women.

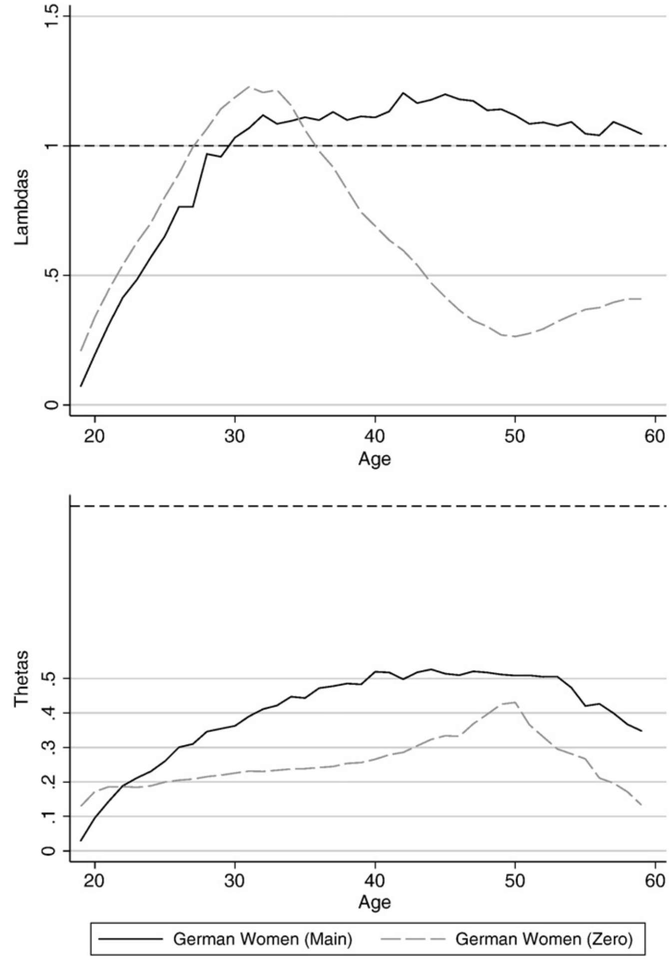

Fig. 5. German women: zero income sensitivity.

found rather seems to be induced by cohort effects. We therefore cautiously conclude that associations between lifetime earning and annual earnings of men appear fairly robust across countries, yet change across generations. There are indications, however, that these intergenerational changes are again similar across countries.

From the perspective of the applied researcher, we can learn that life-cycle bias for men should be minimized between the ages of 30 and 40 when proxying permanent income on the left-hand-side of the equation (e.g. sons' income when estimating intergenerational income mobility). If lifetime earnings is one of the regressors (e.g. fathers' income in intergenerational mobility studies), the age range 35 to 45 appears more adequate. These findings are fairly robust across the three countries that have been analyzed so far.

4.1.2. Women

In the second part of this section we compare our estimates for German women with findings of Böhlmark and Lindquist (2006) for Swedish women.[30] The $\hat{\lambda}_t$-profile of German women, depicted in Fig. 4 strongly resembles our findings for German men. However, the strong attenuation bias is reduced faster and there is no prolonged period of unbiasedness between 30 and 40 as found for men. The amplification bias rather starts in the early thirties and remains fairly constant until the age of 50. In the last years of the career this bias is small, in some years insignificant. The Swedish bias profile is more volatile than the German counterpart. It peaks in the early thirties exhibiting a strong amplification bias, shows a short period of unbiasedness around the age of 40 and then fluctuates around unity until the end of the career.[31] Differences in parameter estimates are significant at the 95% level in eight years. In the age range 45–50 and at age 57 the German parameters are larger, at age 27 it is the other way round.

The $\hat{\theta}_t$-profile of German women appears concave with a flat peak between 40 and the early fifties at around 0.5. The Swedish profile is flat until the mid forties and then jumps to a volatile plateau of up to 0.6 until the end of the career. The German results exhibit significantly lower attenuation biases from age 29 to 44, and higher biases in eight years in the age range 45 to 59. Once again, none of these profiles is close to unbiasedness at any point in the career. Since the female bias profiles are more heterogeneous across countries than the male counterparts, it is hard to give a general recommendation of how to minimize life-cycle biases. Yet, it seems adequate to use data towards the end of female careers, around the age of 50, or, when proxying the dependent variable, even later.

4.2. Zero income sensitivity of the female results

In the analysis presented so far, zero income observations were dropped in the first steps of the estimation procedure. This is necessary since all income information is expressed in logs. However, in particular in the case of women (compare Table 1) labor supply and hence the occurrence of zero income varies substantially over the life-cycle. Ignoring this fact might therefore have a significant impact on

[30] Both, the estimates for Sweden and for Germany exclude zero income observations.

[31] See as well the following subsection for an interpretation of the results in light of the sensitivity of estimates to the treatment of zero income observations.

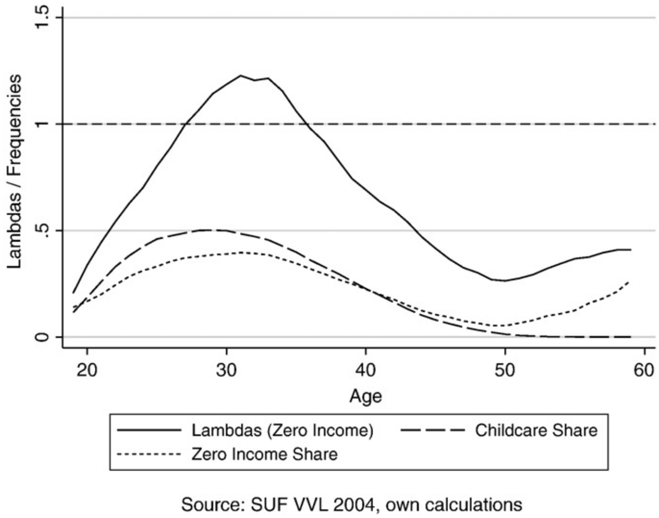

Fig. 6. Sample characteristics and estimates of German women. Notes: lambdas (zero income) refers to estimates including zero income as depicted in Fig. 5 The zero income share at each age is reported in Table 1. Childcare share refers to women staying at home caring for their children at least one month of the calendar year.

the obtained associations of current and permanent income at different stages of female careers.[32]

Böhlmark and Lindquist (2005) investigate this issue by requiring different minimum numbers of non-zero income years for an individual to be included in the analysis. They find stronger life-cycle biases for the more restrictive samples but the differences between the obtained point estimates are rarely significant. While the findings of Böhlmark and Lindquist (2005) indicate that bias patterns are fairly robust to different sample restrictions, they do not answer the question of the effect of including zero income observations in the analysis. Exactly this is what we test in this subsection.

Bias estimates for women including zero income are depicted in Fig. 5. The differences compared to the main results (excluding zero income observations) are dramatic.[33] The trajectories of λ_t are completely different and never close to unity, with two exceptions: the profile crosses the horizontal line at 1 once from below and once from above at ages 28 and 36, respectively. The θ_t-profile starts at a low level, slowly increases, and peaks at age 50, to finally drop sharply until the end of the life-cycle.[34]

In Fig. 6 we try to shed some light on the strange patterns of the bias profiles including zero incomes. To this end, we plot the λ_t-profile for native women along with the share of zero incomes as reported in Table 1, and the share of women that stay at home and care for their children at least one month of the year. Both these variables appear to be positively correlated with the bias profile. Simple regressions of the $\hat{\lambda}_t$-coefficients on either of the two (or both) yield significant positive coefficients and R^2's beyond 0.8. The same regressions using the main parameter estimates as dependent variable exhibit no significant relationships with the covariates and R^2's between 0.02 and 0.11.[35] When excluding zero income years from the analysis one seems to break the typical pattern of labor supply of working mothers over the life-cycle. The information left in the data, childless women and the positive income years of mothers, appears fairly similar to the typically non-interrupted life-cycle information for men.

This might also partly explain the difference in bias profiles between Swedish and German women. Actually, the Swedish trajectories can be viewed as a mixture of the two profiles of German women, the ones excluding and including zero incomes, respectively. Such a finding might be plausible if Swedish women exit the labor market less frequently when having young children at home than German women but rather only reduce hours worked resulting in lower income. The evidence surveyed by Boca et al. (2003) suggests exactly this. While Swedish women in general exhibit higher labor market participation rates than German women, this is particularly true for mothers with small children (aged 0–3) but also with older children.

Böhlmark and Lindquist (2006) further report that the hump of their female λ_t-profile peaks at the same age as the probability of having children aged zero to 16 at home. This finding is robust to excluding women with different numbers of zero income observations. Hence, the binary decision to work or not does not drive this result. We find the identical relationship between the necessity to care for children at home and the hump of the λ_t-trajectory in the German data. However, it is only found if zero income observations, i.e. women who drop out of the labor force to care for their children, are included. This difference in behavior might be a direct consequence of the high, public provision of childcare in Sweden and other aspects of the

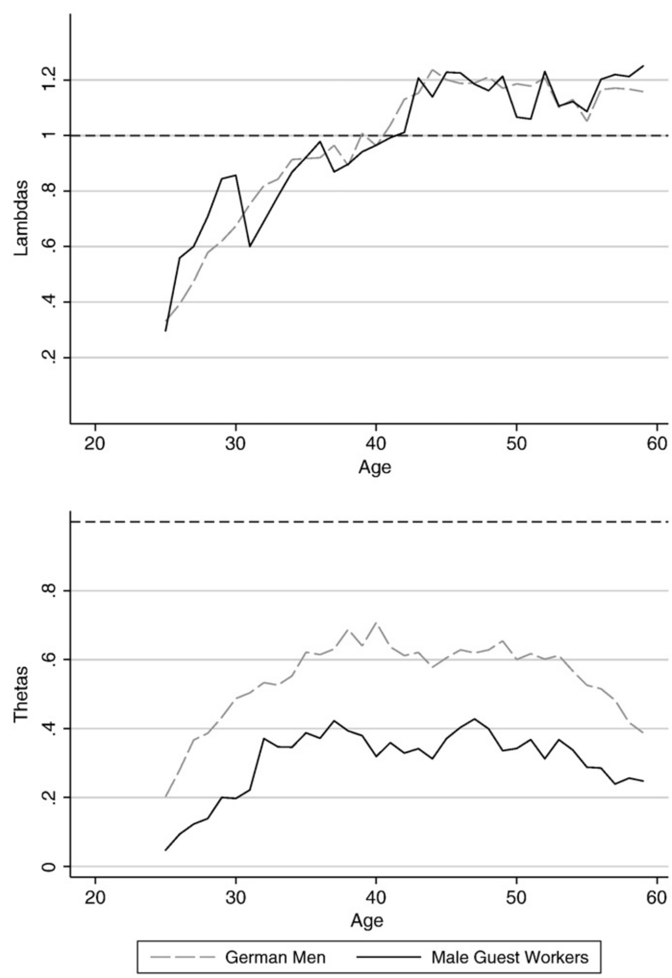

Fig. 7. Bias profiles of male natives and guest workers.

[32] In general, results for men might be affected, as well, although there is no cyclical pattern of labor supply and the share of zero income observations is considerably lower. However, as mentioned in Section 3, we run into convergence problems when estimating the covariance matrices of annual income for native and immigrant men. Hence, we cannot obtain bias estimates including zero income for these groups. Haider and Solon (2006) find only moderate differences for their sample of men, regardless of whether they include zero income or not.

[33] The λ_t estimates for the zero income data are significantly larger in ten out of the first 15 years and lower from age 36 onwards. The θ_t estimates are significantly larger in the first two years and lower in all but five years over the rest of the life-cycle.

[34] We perform some robustness checks considering the imputation value for zero income information. Increasing imputed income to 1% or even 2.5% of average annual income does not change the results qualitatively. However, with every increase the profiles slightly shift upwards.

[35] These results are available upon request.

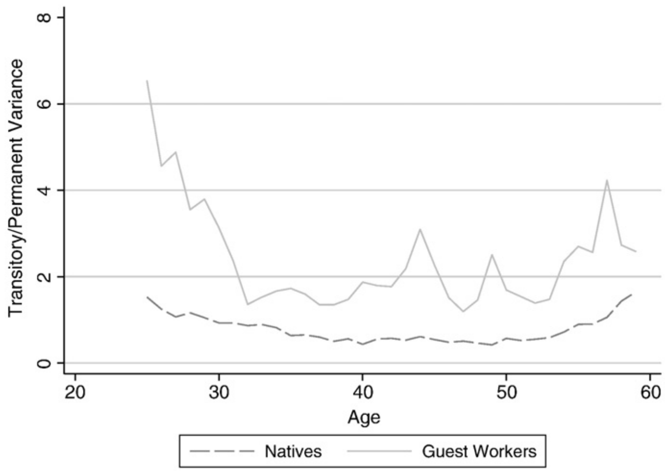

Fig. 8. Ratio of transitory to permanent income variance of men.

Table 3
Average autocorrelations of different cohorts of women.

Order of autocorrelation	German women		Guest workers		Swedish women
	Aged 43–52		Aged 43–52		Aged 42–55
	Born 1939–44		Born 1939–44		Born 1939–43
	Main	Zero	Main	Zero	
1	0.84	0.85	0.70	0.83	0.81
2	0.78	0.72	0.62	0.68	0.69
3	0.74	0.62	0.59	0.57	0.67
4	0.71	0.53	0.55	0.50	0.63
5	0.68	0.45	0.54	0.42	0.55
6	0.66	0.38	0.50	0.36	0.55
Source	Own calculations		Own calculations		B & L (2006)

Notes: Main (zero) excludes (includes) zero-income observations. B & L (2006) refers to and Böhlmark and Lindquist (2006).

extensive public family policy (see e.g. Hoem, 2005). The more volatile life-cycle bias profiles for Swedish women in comparison to German women (excluding zero income observations) might therefore partially be explained by the difference in labor market supply of mothers in these two countries.

The important message for applied researchers from this set of results is to exclude zero income observations in order to minimize life-cycle bias when women are analyzed.

4.3. Native Germans vs. guest workers

4.3.1. Men

We begin the discussion of our findings with the comparison between native men and male guest workers. A first noticeable difference is found with respect to the average income autocorrelations. These are reported in the age range 43 to 52 in Table 2. The immigrants' income is massively less persistent than natives' income during this period. This might indicate the relatively more frequent job terminations among guest workers than among natives since the eighties due to the rising unemployment in Germany.[36]

In Fig. 7 we plot the bias trajectories of native and immigrant men. The λ_t-profiles for both groups are strikingly similar. 95% confidence bands around the native trajectory and the immigrant profile overlap over the entire life-cycle.[37] Hence, neither the unequal education distributions of male natives and guest workers nor the potential immigrant income assimilation over the life-cycle affect income profiles in a way that leads to significant differences in the corresponding bias profiles.

Turning to the θ_t-profiles, again we find similarly shaped patterns for German men and male guest workers. Both trajectories appear roughly concave with a flat peak between the mid-thirties and the mid-fifties. However, the attenuation bias for immigrants is significantly larger over the entire life-cycle. On average, the gap amounts to 0.24 and reaches its maximum of 0.39 at age 40. Hence, point estimates of comparative studies between guest workers and natives that use current income as a proxy of permanent status should yield distorted results since not only are parameters biased in both groups but they are so to a different extent. Consider the following illustrative example: assume that the true IGE of German natives and guest workers is 0.5. Further, assume that we observe sons at an 'unbiased age' and fathers at age 45. We would obtain point estimates of 0.30 for natives and 0.19 for immigrants. Given a sufficient precision of these estimates we would spuriously conclude that mobility among immigrants is higher than among natives.

The question remains what might explain the differences in attenuation between male natives and immigrants. Since the λ_t-trajectories of both groups are very similar, it is plausible that the observed θ_t-gap is a consequence of the 'traditional' attenuation bias. The textbook errors-in-variables model signifies that the attenuation increases with the share of the annual income variance that is attributable to the transitory income component. In the considered case this share would have to be larger for immigrants than for natives to explain the observed bias patterns. In order to assess this conjecture, we use Eq. (3) to estimate the ratio of transitory to permanent income variance,[38]

$$\mathrm{var}(\xi_{it}) \,/\, \mathrm{var}(y_i) = \frac{\lambda_t(1-\lambda_t\theta_t)}{\theta_t}. \tag{5}$$

As can be seen from Fig. 8, the relative transitory variance of immigrants in comparison to native Germans is considerably higher over the entire career which confirms our hypothesis.[39] This is not too surprising given the differences in income correlations (see Table 2) and the evidence on job stability by Uhlendorff and Zimmermann (2006) discussed in footnote 36.

4.3.2. Women

Turning to the findings for women we need to consider two cases: estimates excluding and including zero income. Once again, we begin the comparison with a look at the income autocorrelations depicted in Table 3. In both cases, the income of natives is estimated to be more persistent whereas the difference between the zero results is very small. Moreover, the persistence gaps of the main results are considerably smaller than the corresponding estimates for men.

In Fig. 9 we compare the estimates excluding zero income for German women with those of female guest workers.[40] Although the profile for native women and the immigrants' trajectory exhibit some

[36] Uhlendorff and Zimmermann (2006) compare unemployment dynamics of German men and guest workers using GSOEP data from 1984 to 2004. Accounting for observable and unobservable heterogeneity, they conclude that immigrants (in particular Turks) need more time to find employment than natives. These jobs, however, are as stable as those occupied by comparable natives. Furthermore, high-skilled individuals find more stable jobs and are less frequently unemployed. These findings are in line with the relatively low persistence of immigrants' annual income, specifically if we consider the skill distribution depicted in Fig. 2.

[37] The noticeable differences in the early years of the career are insignificant due to the large standard errors of the immigrant estimates. This is partially a result of the relatively small numbers of observations in this age range. Standard errors of the results presented in this section are tabulated in Appendix B.

[38] We use this relative measure since the lifetime income variance of guest workers is considerably smaller vis-à-vis the variance of natives.

[39] The U-shaped profiles for both groups further confirm findings of Baker and Solon (2003) and Mazumder (2001) reporting similar patterns for US and Canadian men, respectively.

[40] We restrict this discussion to these estimates since the evidence in Section 2 strongly suggests to exclude zero income when analyzing women. However, the bias estimates including zero income for female guest workers are available upon request.

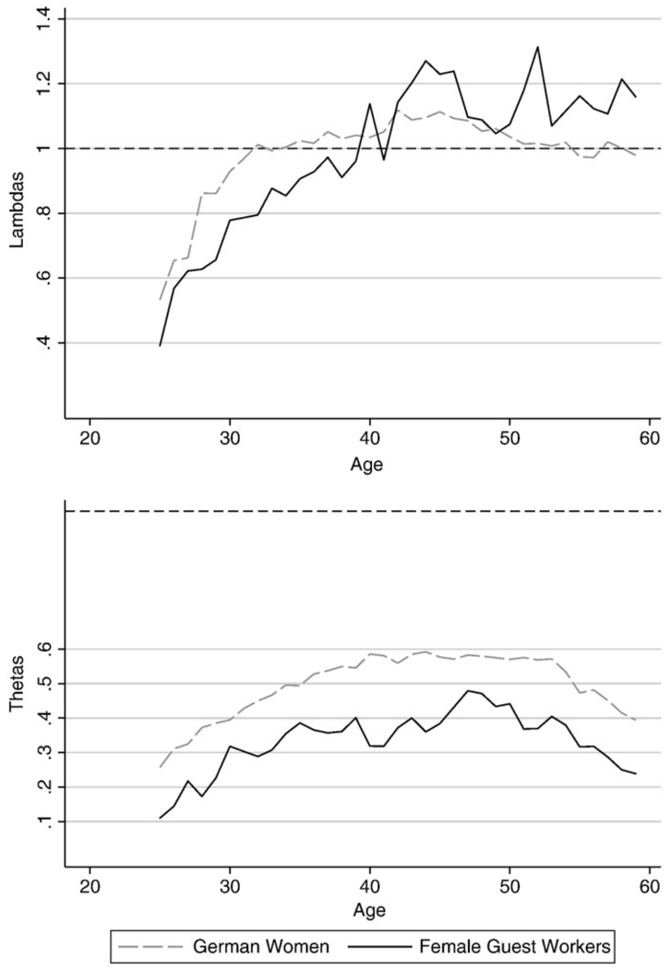

Fig. 9. Bias profiles of female natives and guest workers.

differences confidence bands overlap in all but 4 years. This is partially attributable to the fairly large standard errors of the immigrants' bias parameters resulting from the relatively small sample size. The λ_t-profiles of immigrant men and women are even more similar. 95% confidence overlap at all ages.

The θ_t-profiles of women, depicted in the bottom panel of Fig. 9, roughly appear concave again. Similar to the findings for men, the attenuation bias for guest workers is significantly larger at virtually all ages.[41] The average gap between the two profiles, however, is somewhat smaller at 0.16. Since the bias profiles of immigrant men and women are very similar, the smaller discrepancy between the two groups of women is induced by the higher attenuation bias for native women compared to native men, with θ_t-estimates between 0.5 and 0.6 along the flat peak.[42]

Similar to the case of men, approximating lifetime earnings as a regressor with annual earnings yields significantly different attenuation biases for female guest workers and natives. Conclusions about point estimate heterogeneity based on such approximations should therefore be misleading. As for men above, we further test whether the observed bias gap is partly caused by traditional attenuation bias. Our results (available upon request) again confirm this conjecture.

4.3.3. Impact of averaging annual income

Whenever panel data is available, researchers typically approximate lifetime or permanent income by an average of annual income

Table 4
θ_t estimates based on different income averages.

	1-year results		5-year average		10-year average		15-year average	
	Natives	GW	Natives	GW	Natives	GW	Natives	GW
Men								
$\max_t \hat{\theta}_t$	0.708	0.428	0.828	0.608	0.901	0.742	0.983	0.846
age	40	47	38	40	35	39	33	39
mean $(\hat{\theta}_t^N - \hat{\theta}_t^{GW})$	0.251		0.143		0.066		0.021	
$\max_t (\hat{\theta}_t^N - \hat{\theta}_t^{GW})$	0.389		0.314		0.208		0.234	
Women								
$\max_t \hat{\theta}_t$	0.592	0.479	0.719	0.621	0.782	0.716	0.834	0.812
age	44	47	41	48	39	35	41	32
mean $(\hat{\theta}_t^N - \hat{\theta}_t^{GW})$	0.171		0.113		0.079		0.056	
$\max_t (\hat{\theta}_t^N - \hat{\theta}_t^{GW})$	0.267		0.176		0.115		0.106	

Notes: Guest workers are abbreviated by GW, natives by *N*. 1-year results correspond to the estimates depicted in the bottom panels of Figs. 7 and 9, respectively.

information over several years to reduce the attenuation bias.[43] This consideration is based on the standard errors-in-variables model. In the context of the generalized errors-in-variables model, Haider and Solon (2006) show that averaging of the income variables simply yields an average of the single-year $\hat{\lambda}_t$'s when the dependent variable is approximated. For θ_t, however, no such analytical result exists. In the electronic Appendix of their paper, Haider and Solon (2006) therefore repeat the θ_t-estimations using 5-year averages of the income variable. This reduces the attenuation bias to some extent. Yet, they conclude, in line with Mazumder (2001, 2005), that 5-year averages of annual income are insufficient to completely eliminate this bias.[44]

To investigate the impact averaging has on our results, we reestimate Eq. (2) for each group using five, ten, and 15-year averages. Specifically, we assess whether the differences in attenuation between natives and guest workers can be reduced by using panel information. We summarize our findings in Table 4 and only report some key statistics: the maximal $\hat{\theta}_t$ for each group and at what age it is observed, and the maximal as well as the average gap between natives and immigrants, all by gender.[45]

The attenuation bias is reduced monotonically for all groups the more years are used to compute the average. The bias reductions for immigrants are larger resulting in ever smaller gaps between guest workers and natives. For women virtually no difference is found using a 15-year average and considering the highest θ_t-estimate. However, even though the bias is reduced considerably in all samples, it only vanishes completely for native men when 15 years of data are available.

The θ_t-profiles of both immigrant and native groups steadily shift upwards the more years of data are used to construct the regressor. While the profiles of guest workers and German women are fairly flat, the trajectories of native men exhibit a peak that shifts more and more to the left the more years are used for the averaging.[46] This is reflected in the decline of the age at which the largest $\hat{\theta}_t$'s are observed. To the right of the peaks the profiles decline sharply. This explains the rather small average gaps for men in comparison to the maximal gaps and

[41] The exceptions are the ages 30 and 47.
[42] The attenuation bias of German men is more severe than the bias of native women only in the first six years of the 41-year profiles.

[43] See e.g. Solon (1999) for a discussion concerning the literature on intergenerational income mobility.
[44] Mazumder (2005) shows analytically that averaging of income over multiple years is the less effective in reducing the attenuation bias the stronger the serial correlation of the transitory income component. His empirical analysis uses fathers' income averages of different lengths when estimating intergenerational income elasticities. The evidence suggests that the attenuation bias is steadily reduced when increasing the years of income information used to compute these averages from two to a maximum of 16 years.
[45] We restrict the age range from 32 to 52, the maximum range covered by the 15-year averages with 32 and 52 referring to the midpoints of the intervals, respectively.
[46] The full set of estimates are available upon request.

the still considerable difference between the biggest $\hat{\theta}_t$'s for natives and immigrants using ten or 15 years of data. Finally, with the exception of the mean gap, all statistics suggest that averaging reduces the attenuation bias gap between women by more than the discrepancy between men.

These results confirm the findings by Haider and Solon (2006) that averaging over five years is insufficient to completely eradicate attenuation bias. It rather seems advisable to use as much income information as possible since significant reductions of the bias are achieved by averaging up to 15 years of data. Male natives, however, are an exception by requiring also that the income information is taken from the early stages of the career. A second important effect of long averages is the reduction of differences in the estimated attenuations between immigrants and natives which should help to obtain more comparable estimates.

5. Conclusions

We apply Haider and Solon's (2006) generalized errors-in-variables model to assess the association between annual and lifetime earnings in Germany. In our empirical analysis, using data from the Vollendete Versichertenleben 2004 of the Research Data Centre of the German Statutory Pension Insurance, we distinguish between men and women as well as natives and guest workers.

When comparing our findings for German men to Swedish and American evidence (Böhlmark and Lindquist, 2006 and Haider and Solon, 2006) it appears that the European countries exhibit more similar bias profiles. However, the evidence for the USA is based on data from a different birth cohort. Swedish evidence for the identical birth cohort resembles the US findings more closely (Böhlmark and Lindquist, 2006). We therefore cautiously conclude that the associations between annual and lifetime earnings of men are very similar across these Western industrialized countries, yet change over time. There are indications, however, that these intergenerational changes are again similar across countries.

The evidence for German women, on the other hand, differs considerably from the only available benchmark in the literature, Swedish women (Böhlmark and Lindquist, 2006). The distinct bias profiles might be a reflection of the differences in labor market participation which is considerably higher in Sweden, in particular for mothers. Moreover, it seems highly recommendable to exclude zero income information to minimize the life-cycle bias in female samples.

Finally, we neither find significant differences between the bias profiles of native and immigrant men nor female natives and guest workers when the dependent variable is approximated. However, the attenuation bias for both immigrant samples vis-à-vis the German counterparts is larger when permanent income is a regressor in the analysis. This is explained by the higher share of variance attributable to the transitory income component and most likely a result of the more frequently interrupted careers of guest workers. It is recommendable to average annual income information if panel data is available since the attenuation bias as well as the discrepancy in biases between immigrants and natives is constantly reduced by using ever longer averages of up to 15 years.

Since bias profiles change between generations and population groups, it is not possible to give a universal advice on how to minimize the impact of life-cycle variations in the approximation quality of current earnings for lifetime earnings. The evidence found for men from USA, Sweden, and Germany, however, suggest that income measures in the age range 30 to 40 (35 to 45) should be the least affected by life-cycle bias when dependent (independent) variables are proxied. The results for women are less robust. Yet, it seems adequate to use data towards the end of the career, around the age of 50, or, when proxying the dependent variable, even later. Nonetheless, point estimates of the impact of income proxies across different population groups — men and women, natives and immigrants, young and old — need to be interpreted cautiously.

Appendix A. Data

The main data source of the life-cycle bias analysis is the Scientific Use File (for natives) and an enlarged sample (for immigrants) of the Vollendete Versichertenleben (VVL) 2004 of the Research Data Centre of the German Statutory Pension Insurance (FDZ-RV). These data sets contain longitudinal information about a random sample of roughly 5% (25%) of all individuals born between 1939 and 1974 who received statutory pension payments for the first time in 2004. All episodes that affect the individual pension account starting in the year an individual turns 14 years of age are reported on a monthly basis. For individuals born in 1939 this amounts to a maximum of 624 monthly data points collected in 52 years.

The main determinant of the amount of pensions received is income subject to social insurance contribution.[47] Other documented episodes comprise, among other things, school and professional education, military service, pregnancy and childcare, temporal or permanent disability, as well as spells of unemployment, marginal employment, and self-employment. Our analysis focuses on episodes of any form of employment that generate income. However, the available data limits the representativeness of our analysis to some extent. First of all, civil servants are not included in the data since they are covered by a separate pension scheme. Additionally, most self-employed individuals in Germany can choose whether they want to join the statutory pension system. Furthermore, their earnings are usually not subject to social insurance contributions.[48] In these cases, their optional contributions to the statutory pension insurance do not contain any information about their earnings. As a consequence, we discard all episodes of self-employment in the analyzed samples.

According to the Federal Statistical Office Germany, the share of employment subject to social insurance contribution amounted to 77% in Germany in 1970 and remained fairly constant until the mid 1990s. In the last decade this share decreased to 67% in 2006. Since 1999 also the numbers of marginally employed workers are collected. The combined share of these two groups covered in our data amounts to 81% until 2004, the last year of the VVL.[49] Since no information is provided for the 1950s and 1960s we cannot say with certainty how representative the VVL is for this period. However, since 1970 the data used in our analysis should represent at least three quarters of the registered working population in Germany.

An individual is generally considered marginally employed if her income is below a certain threshold in any given month.[50] The VVL contains information on spells of marginal employment since 1999. The documented contributions to the pension accounts of these episodes unfortunately do not vary with actual income earned. We therefore assign the threshold value to these spells.

Periods of mandatory military service are also included in the analysis. Since the VVL does not provide information about the pay of soldiers, we assign a year-specific average value constructed from information in the soldier pay law.[51]

When enrolled in professional training in the so called Dual Apprenticeship System, again a fixed contribution to the individual pension account is documented, yet no information about the training

[47] Henceforth, labor income and income subject to social insurance contribution are used interchangeably.

[48] There is only a hand full of observations in the VVL that contain information about the income generated in spells of self-employment.

[49] This data can be downloaded from http://www.destatis.de/e_home.htm.

[50] These thresholds are available on the web page of the FDZ-RV.

[51] We collected all pay changes since 1957 published in the Bundesgesetzblatt (see e.g. www.bundesgesetzblatt.de). The monthly average is computed as the basic pay of a Gefreiter (the second lowest rank in the German army hierarchy) as this corresponds to the mean monthly payment during mandatory service as of today (2007).

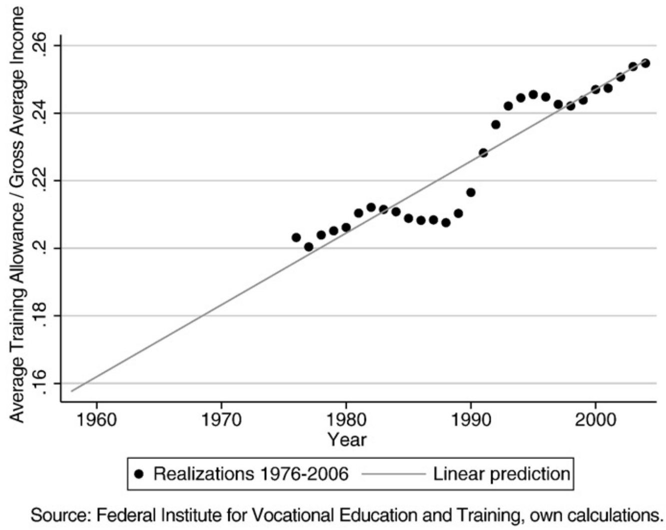

Fig. A.1. Average training allowance over gross average annual income.

allowance. Furthermore, no details about the type of apprenticeship are available.[52] Once again we impute annual averages whenever a training spell is encountered. There is, however, an additional complication to this approach in comparison to the military case. The average training allowances we use are collected by the Federal Institute for Vocational Education and Training.[53] Their time series starts in 1976 and currently covers more than 180 different vocations representative of 88% of German trainees in the West and 80% of trainees in Eastern Germany. Since we focus our analysis on cohorts born between 1939 and 1944, most of the observed training spells in our main data set occur before 1976. For the period from 1958 to 1975 we therefore predict the ratio of average training allowance over gross average annual income (subject to social insurance contribution)[54] by a linear time trend estimated from the observed period 1976 to 2004 (see Fig. A.1).

While the imputed income measures for episodes of professional training, marginal employment and military service are rather crude, the measurement errors induced should be modest for the overall analysis. First, the latter episodes only affect native men and only up to two years of their careers. Furthermore, income variations among individuals who serve at the same time should, for all that we know, be pretty low.

The case of apprenticeships is somewhat more concerning since the variation in training allowances is considerable. Nonetheless, as depicted in Fig. A.1, the average income during these episodes in any case is low, i.e. apprentices are all concentrated in the lower third of the annual income distribution. Furthermore, the years of earnings affected by our approximation should be low since most individuals start an apprenticeship at the age of 15 or 16. Since the analyzed sample uses information from age 19 onwards, most apprenticeship spells are not covered in this data. Additionally, most immigrants probably have received their training, if any, abroad.

Finally, considering marginal employment, measurement errors should be modest as well since this information is only available since 1999 and income variations within this group are per definition limited which reduces the scope of imputation error. Between 1999 and 2004 the income threshold never exceeds 16.5% of the average income subject to social insurance contribution.

[52] There is a variable in the VVL containing the professional degree obtained. However, the information is not specific enough and the share of missing values is very high.

[53] This data can be downloaded at http://www.bibb.de/en/783.htm.

[54] A time series of gross average annual income subject to social insurance contribution can be obtained on the web page of the FDZ-RV.

Table A1
Fractions of non-zero-income-years by employment type.

In percent of total person-year observations			
Group	Marginal employment	Training	Military service
Natives			
Men	0.04	1.61	2.2
Women	0.50	0.84	
Guest worker			
Men	0.03	0.04	
Women	0.49	0.01	

Notes: Source: SUF and 25% Sample of VVL 2004, own calculations.

Table A2
Frequencies of months per year at the contribution ceiling — German men.

Months	Frequency (%)	Cumulated (%)
1	1.15	1.15
2	1.67	2.82
3	2.02	4.84
4	1.38	6.22
5	1.06	7.28
6	2.01	9.30
7	1.10	10.40
8	0.95	11.35
9	1.72	13.07
10	0.71	13.78
11	1.32	15.10
12	84.90	100.00

Notes: Person-year observations of German men born 1939 to 1944 in the age range 19 to 59 who have at least 10 years of positive income. Observations reported if at least one monthly income information is from above. Source: SUF VVL 2004, own calculations. censored from above. Source: SUF VVL 2004, own calculations.

In Table A.1 we depict the fractions of years affected by either military service, professional education, or marginal employment across the four analyzed groups. As argued above, these shares are very small. Hence, we are confident that no systematic distortions of the analyzed life-cycle bias patterns are introduced by our imputations.

The largest share of information utilized to construct life-cycle income profiles finally stems from observed labor income subject to social insurance contribution. It is reported monthly up to an annually changing contribution ceiling,[55] i.e. right censored. In general, censoring of dependent variables can be accounted for by applying for instance tobit models. The problem faced here though is the fact that we have to aggregate our data to annual levels, i.e. we need to sum up 12 observations for each year, respectively. This creates the possibility of encountering individuals whose income information is censored from above in some but not all months of any given year.

This problem, however, turns out to be manageable since individuals in the considered cohorts typically worked for very few employers during their careers. Since the contribution ceiling is constant within any calendar year such cases therefore should only arise in the rare event of starting a new employment or due to promotion within a firm within a calendar year. In fact, in almost 85% of the cases when censoring occurs in a year, all months hit the ceiling for male natives, by far the most affected group (see Table 1), as documented in Table A.2. Nonetheless, to implement the tobit models used in the analysis we have to find a solution for the cases of only some months of censoring. In the main analysis, we therefore treat an annual income observation as censored from above if seven or more months are censored. This leads to individual-specific censoring points since income in the non-censored months is added to the censored data which varies across the affected individuals.

[55] The annual contribution ceilings from 1957 to 2004 are available on the web page of the FDZ-RV.

Appendix B. Point estimates

Table B1
λ_t and θ_t estimates for men corresponding to Figs. 3 & 7.

Age	Natives (main)				Natives (adjusted)				Guest workers			
	$\hat{\lambda}_t$	Std. err.	$\hat{\theta}_t$	Std. err.	$\hat{\lambda}_t$	Std. err.	$\hat{\theta}_t$	Std. err.	$\hat{\lambda}_t$	Std. err.	$\hat{\theta}_t$	Std. err.
19	−0.088	0.038	−0.023	0.010								
20	−0.055	0.038	−0.016	0.011								
21	0.013	0.036	0.003	0.009								
22	0.139	0.037	0.038	0.011								
23	0.215	0.029	0.087	0.012								
24	0.278	0.033	0.106	0.013								
25	0.400	0.022	0.211	0.013	0.331	0.024	0.202	0.015	0.297	0.128	0.047	0.022
26	0.457	0.031	0.282	0.021	0.392	0.026	0.280	0.022	0.559	0.092	0.094	0.017
27	0.537	0.028	0.359	0.018	0.474	0.022	0.367	0.018	0.600	0.078	0.123	0.024
28	0.643	0.027	0.371	0.019	0.578	0.027	0.386	0.021	0.708	0.080	0.139	0.018
29	0.683	0.030	0.412	0.023	0.620	0.023	0.432	0.022	0.844	0.077	0.200	0.023
30	0.738	0.027	0.460	0.023	0.674	0.022	0.487	0.025	0.857	0.101	0.197	0.024
31	0.817	0.024	0.472	0.024	0.753	0.022	0.504	0.026	0.601	0.063	0.222	0.034
32	0.883	0.030	0.496	0.019	0.820	0.033	0.533	0.022	0.690	0.053	0.371	0.025
33	0.905	0.030	0.487	0.025	0.843	0.026	0.526	0.025	0.781	0.066	0.347	0.028
34	0.981	0.031	0.512	0.023	0.914	0.029	0.553	0.025	0.868	0.073	0.346	0.038
35	0.984	0.025	0.576	0.019	0.917	0.024	0.622	0.024	0.922	0.075	0.388	0.032
36	0.985	0.028	0.567	0.031	0.920	0.028	0.614	0.031	0.978	0.060	0.372	0.033
37	1.028	0.030	0.580	0.025	0.965	0.032	0.631	0.025	0.869	0.057	0.423	0.045
38	0.950	0.027	0.632	0.020	0.892	0.024	0.688	0.024	0.897	0.066	0.393	0.064
39	1.073	0.040	0.588	0.020	1.008	0.033	0.640	0.022	0.942	0.075	0.379	0.044
40	1.023	0.026	0.649	0.020	0.963	0.024	0.708	0.022	0.965	0.073	0.319	0.040
41	1.104	0.034	0.585	0.024	1.038	0.034	0.637	0.026	0.992	0.066	0.359	0.037
42	1.202	0.035	0.561	0.019	1.131	0.039	0.612	0.020	1.011	0.073	0.329	0.037
43	1.226	0.043	0.570	0.022	1.153	0.040	0.621	0.025	1.207	0.101	0.342	0.045
44	1.316	0.040	0.531	0.019	1.237	0.038	0.578	0.020	1.139	0.081	0.312	0.048
45	1.282	0.034	0.557	0.024	1.201	0.036	0.605	0.025	1.228	0.105	0.371	0.045
46	1.267	0.031	0.578	0.014	1.188	0.027	0.628	0.016	1.225	0.102	0.404	0.041
47	1.266	0.032	0.569	0.024	1.189	0.034	0.619	0.026	1.185	0.081	0.428	0.033
48	1.291	0.036	0.578	0.020	1.211	0.037	0.628	0.023	1.162	0.102	0.399	0.032
49	1.248	0.034	0.602	0.020	1.170	0.031	0.654	0.020	1.213	0.070	0.336	0.040
50	1.263	0.034	0.552	0.016	1.186	0.032	0.600	0.018	1.066	0.083	0.342	0.028
51	1.258	0.038	0.569	0.019	1.178	0.037	0.617	0.020	1.059	0.067	0.368	0.034
52	1.289	0.037	0.554	0.019	1.208	0.035	0.601	0.021	1.231	0.135	0.312	0.033
53	1.170	0.033	0.561	0.018	1.101	0.027	0.612	0.020	1.106	0.097	0.368	0.033
54	1.199	0.034	0.519	0.021	1.130	0.032	0.567	0.024	1.122	0.085	0.338	0.027
55	1.114	0.033	0.481	0.014	1.051	0.023	0.526	0.013	1.086	0.081	0.288	0.028
56	1.234	0.035	0.471	0.014	1.166	0.029	0.515	0.015	1.203	0.093	0.285	0.030
57	1.237	0.037	0.440	0.012	1.171	0.038	0.482	0.014	1.220	0.116	0.239	0.028
58	1.232	0.038	0.380	0.012	1.167	0.037	0.418	0.014	1.212	0.069	0.256	0.028
59	1.225	0.036	0.354	0.014	1.158	0.041	0.388	0.015	1.250	0.143	0.248	0.041

Notes: Standard errors (Std. err.) obtained by 50 bootstrap repetitions as described in Haider and Solon (2006). Main (adjusted) refers to Native estimates based on 41 (35) years of income data. All estimates exclude zero income observations.

Table B2
λ_t and θ_t estimates for native women corresponding to Fig. 5.

Age	Excluding zero income				Including zero income			
	$\hat{\lambda}_t$	Std. err.	$\hat{\theta}_t$	Std. err.	$\hat{\lambda}_t$	Std. err.	$\hat{\theta}_t$	Std. err.
19	0.072	0.039	0.029	0.015	0.208	0.023	0.129	0.014
20	0.194	0.035	0.095	0.016	0.338	0.027	0.172	0.011
21	0.310	0.035	0.144	0.014	0.445	0.029	0.186	0.012
22	0.414	0.034	0.189	0.014	0.540	0.032	0.186	0.010
23	0.483	0.030	0.211	0.013	0.627	0.035	0.184	0.009
24	0.570	0.030	0.231	0.012	0.701	0.037	0.189	0.009
25	0.651	0.026	0.260	0.012	0.803	0.036	0.200	0.009
26	0.764	0.032	0.301	0.012	0.892	0.039	0.205	0.009
27	0.764	0.030	0.310	0.010	0.995	0.042	0.208	0.008
28	0.968	0.033	0.346	0.012	1.066	0.041	0.215	0.008
29	0.957	0.031	0.354	0.013	1.142	0.041	0.219	0.007
30	1.032	0.033	0.363	0.014	1.187	0.036	0.226	0.007
31	1.068	0.032	0.390	0.016	1.228	0.037	0.231	0.007
32	1.118	0.036	0.411	0.015	1.205	0.040	0.230	0.007
33	1.085	0.024	0.422	0.015	1.215	0.039	0.234	0.006
34	1.096	0.029	0.447	0.015	1.156	0.034	0.237	0.007
35	1.110	0.026	0.443	0.013	1.061	0.031	0.238	0.008
36	1.099	0.023	0.472	0.014	0.980	0.033	0.242	0.009
37	1.130	0.023	0.478	0.010	0.918	0.032	0.245	0.009
38	1.099	0.027	0.485	0.016	0.830	0.031	0.254	0.010
39	1.113	0.026	0.483	0.015	0.744	0.034	0.256	0.011
40	1.109	0.020	0.520	0.013	0.691	0.035	0.266	0.011
41	1.132	0.023	0.518	0.012	0.636	0.032	0.279	0.012
42	1.204	0.021	0.498	0.014	0.596	0.062	0.286	0.035
43	1.165	0.024	0.518	0.014	0.540	0.034	0.305	0.021
44	1.177	0.023	0.526	0.016	0.471	0.030	0.323	0.020
45	1.199	0.023	0.514	0.012	0.416	0.042	0.334	0.035
46	1.179	0.019	0.510	0.014	0.366	0.056	0.332	0.065
47	1.173	0.022	0.521	0.012	0.325	0.041	0.368	0.058
48	1.137	0.022	0.517	0.016	0.302	0.016	0.396	0.026
49	1.141	0.019	0.512	0.012	0.270	0.041	0.425	0.067
50	1.117	0.020	0.508	0.013	0.264	0.031	0.431	0.050
51	1.085	0.024	0.509	0.015	0.276	0.020	0.366	0.028
52	1.090	0.025	0.505	0.013	0.293	0.021	0.330	0.022
53	1.077	0.026	0.505	0.012	0.321	0.021	0.295	0.022
54	1.092	0.026	0.473	0.013	0.345	0.021	0.281	0.021
55	1.046	0.028	0.420	0.014	0.368	0.022	0.267	0.017
56	1.040	0.030	0.426	0.013	0.375	0.027	0.211	0.015
57	1.092	0.030	0.400	0.013	0.396	0.029	0.197	0.016
58	1.070	0.028	0.367	0.012	0.409	0.035	0.171	0.013
59	1.046	0.030	0.348	0.012	0.409	0.038	0.133	0.012

Notes: Standard errors (Std. err.) obtained by 50 bootstrap repetitions as described in Haider and Solon (2006).

Table B3
λ_t and θ_t estimates for women corresponding to Fig. 9.

Age	Natives				Guest workers			
	$\hat{\lambda}_t$	Std. err.	$\hat{\theta}_t$	Std. err.	$\hat{\lambda}_t$	Std. err.	$\hat{\theta}_t$	Std. err.
25	0.533	0.025	0.257	0.013	0.391	0.094	0.110	0.029
26	0.654	0.030	0.311	0.014	0.568	0.103	0.144	0.030
27	0.662	0.026	0.325	0.011	0.622	0.092	0.217	0.032
28	0.862	0.031	0.372	0.013	0.627	0.091	0.173	0.025
29	0.861	0.028	0.385	0.014	0.656	0.068	0.226	0.038
30	0.929	0.030	0.395	0.016	0.778	0.054	0.318	0.029
31	0.969	0.029	0.428	0.017	0.786	0.076	0.303	0.028
32	1.012	0.032	0.450	0.017	0.795	0.060	0.289	0.033
33	0.993	0.022	0.467	0.016	0.877	0.066	0.307	0.032
34	1.004	0.027	0.496	0.016	0.854	0.099	0.355	0.028
35	1.023	0.024	0.494	0.015	0.906	0.050	0.386	0.029
36	1.016	0.021	0.528	0.016	0.928	0.064	0.365	0.028
37	1.052	0.021	0.537	0.012	0.973	0.058	0.357	0.022
38	1.030	0.025	0.550	0.017	0.911	0.069	0.361	0.029
39	1.040	0.023	0.546	0.017	0.961	0.074	0.401	0.027
40	1.034	0.017	0.586	0.015	1.137	0.083	0.319	0.026
41	1.051	0.019	0.581	0.013	0.965	0.067	0.318	0.031
42	1.118	0.018	0.559	0.016	1.143	0.071	0.372	0.024
43	1.088	0.021	0.585	0.015	1.203	0.088	0.400	0.028
44	1.095	0.019	0.592	0.018	1.270	0.068	0.360	0.031
45	1.113	0.020	0.577	0.015	1.229	0.075	0.384	0.032
46	1.093	0.016	0.571	0.016	1.238	0.068	0.431	0.037
47	1.086	0.020	0.583	0.014	1.097	0.060	0.479	0.041
48	1.053	0.019	0.580	0.018	1.088	0.076	0.471	0.036
49	1.061	0.018	0.575	0.014	1.046	0.060	0.434	0.038
50	1.036	0.017	0.570	0.015	1.074	0.058	0.441	0.042
51	1.014	0.021	0.575	0.017	1.179	0.077	0.368	0.030
52	1.016	0.022	0.569	0.015	1.313	0.099	0.370	0.041
53	1.008	0.023	0.572	0.013	1.070	0.077	0.405	0.031
54	1.019	0.023	0.534	0.015	1.116	0.083	0.379	0.042
55	0.974	0.025	0.473	0.016	1.162	0.074	0.317	0.026
56	0.972	0.027	0.482	0.014	1.123	0.081	0.318	0.038
57	1.020	0.027	0.451	0.015	1.107	0.062	0.287	0.026
58	1.001	0.025	0.415	0.013	1.214	0.115	0.250	0.033
59	0.979	0.027	0.394	0.013	1.159	0.108	0.239	0.027

Notes: Standard errors (std. err.) obtained by 50 bootstrap repetitions as described in Haider and Solon (2006). All estimates exclude zero income observations.

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