Cohort effects and the returns to education in West Germany

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Abstract: Using a Mincer-type wage function, we estimate cohort effects in the returns to education for West German workers born between 1925 and 1974. The main problem to be tackled in the specification is to separately identify cohort, experience, and possibly also age and year effects in the returns. For women, we find a large and robust decline in schooling premia: in the private sector, the returns to a further year of post-compulsory education fell from ten per cent for the 1945-49 cohort to about six per cent for those born in the early 1970s. Cohort effects in men's returns to education are less obvious, but we do find evidence that they, too, have declined. We conclude by identifying possible reasons for the decline.

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I. Introduction

In contrast to the experience in the United States, estimated rates of return for human capital have shown to be relatively constant over time in West Germany (Fitzenberger and Franz 1998; Lauer and Steiner 2000; Steiner and Wagner 1998). The stability of average educational premia over time could, however, conceal much larger changes over cohorts. While the first studies which quantified cohort effects in educational premia in the U.S. appeared some twenty years ago, to date there has been only little research on cohort effects in the returns to education in Germany, [1] although there are strong a priori reasons why they might be present. First, the number of births changed dramatically over the post-war period, with a peak of the baby-boom around 1965 and a steep subsequent decline in fertility. Second, enrolment in higher education rose to levels previously unknown during the decades after 1945. Third, female labour force participation increased widely, almost doubling among women in their thirties over the period from 1970 to 1995. Another factor that could give rise to cohort effects is skill-biased technological change, coupled with the acquisition of specific skills in later age groups. It is, however, possible that all of these factors have no leverage on wages due to the centralised German system of wage determination. If wages are not allowed to vary over age groups, cohort effects should mainly show up in unemployment figures.

In this paper, we concentrate on the question whether any cohort effects in the returns to education can be observed. Our answer is that there are, indeed, significant changes over cohorts, but they are much more marked for women than for men. In the private sector, women born in the 1960s suffer a decline in the returns of three percentage points compared to women born in the 1940s. This development is part of a secular decline affecting all cohorts which we observe, i.e. individuals born between 1925 and 1975. For men, we observe a weak but significant decline between individuals born in the early 1950s and the mid-1960s.

In the following section, we give a brief account of the literature. We distinguish several reasons for cohort effects. We also discuss the empirical evidence available thus far. The third section introduces the dataset and discusses problems of estimation. The results are discussed in section four. In Section five, we present additional results with education levels instead of time spent in education as the independent variable. Section six concludes.

II. Theory and empirical evidence on cohort effects in educational premia

In general, there will be cohort effects if older and younger workers are imperfect substitutes in production. In that case, the relative scarcity of workers across birth cohorts will result in different cohort-specific wage levels. If the relative scarcity varies not only between birth cohorts but also across educational groups within each cohort, there will be differences not only in wage levels but also in the wage premia paid for post-compulsory education. Exogenous shifts in educational attainment between cohorts, such as an increase in the proportion of college- or university-educated workers, will be reflected in the wage premia for education. Katz and Murphy (1992) and Card and Lemieux (2001) attribute much of the observed increase in the college premium in the United States over the 1980s to the slowdown in the growth rate of educational attainment. Similarly, Juhn (1999) finds that, while the demand for high skills grew more ar less steadily over the post-war period, the relative supply of skilled workers increased far less in the 1980s as compared to the 1970s, which led to an increase in wage differentials.

Perhaps less obviously, apart from the proportions of workers choosing certain educational levels, the overall number of workers in a cohort may also matter for educational premia. Thus an exogenous decline in the number of workers – due to changes in fertility, wars or epidemics – may lead to a change in the relative rewards of different skill groups. A first reason for this may be differences across skill groups in the elasticity of substitution between capital and labour. Empirical studies for Germany point to the fact that this elasticity is lower for high-skilled workers (Falk and Koebel, 1998). Under these circumstances, if a smaller cohort enters the labour market, reducing the overall supply of labour, wages will be driven up in the market for qualified workers to a greater extent than in other labour market segments. (The argument rests, of course, on less than perfect substitutability across educational groups.) Second, the elasticity of substitution between younger and older workers may differ across qualification levels. If it is higher at low education levels, the effect of a demographic change will be subdued for these workers and more pronounced among highly skilled workers. According to Stapleton and Young (1988), substitution elasticities do indeed exhibit this pattern, although this finding is contradicted by a more recent paper by Card and Lemieux (2001), who could not detect any significant differences between high-school and college graduates.

How large these effects are depends on the precise form of the production function and the level of technology. Suppose that, following the adoption of new technology, firms depend on the services of young workers who have received their education (e.g., computer literacy skills) relatively recently. Then labour demand for young, well-educated workers becomes very inelastic with respect to their relative wages, while demand elasticities for young workers with little education are unaffected. The faster technological change is, the lower is the substitution elasticity among younger and older skilled workers and the larger are changes in the returns to education over cohorts.

Still another source of cohort effects may be downward rigidity of incumbents‘ wages, caused, for instance, by „social norms“. If young workers with high levels of education enter the labour market in large numbers, one would expect wages of older highly skilled workers to decline. However, older workers will have become accustomed to a certain wage level, and a reduction below the previous level may, by an efficiency wages argument, lead to less effort by these workers. In that case, employers may be unwilling to adjust incumbents‘ wages, thus concentrating the effect of the supply change on new entrants.[2]

There are a number of empirical studies on cohort effects in educational premia, starting with Welch's (1979) investigation into the wage implications of the US baby boom of the 1950s. Welch concentrated on cohort size as a determinant of wages. His estimations of separate wage functions by educational levels imply that wages of college graduates suffered more than others from the expansion of supply caused by the baby boom's entry into the labour market. Freeman (1979) decomposed weekly earnings into age and education components. He found that between 1969 and 1976, the difference between wages of younger and older workers grew significantly (implying a steeper wage-age profile), and that this increase was particularly significant for higher education groups. This development can be attributed to the increase in the number of college graduates following the baby boomers' entry into the labour market.

Recent analyses of education, age and cohort effects on wages have mostly used Mincer's (1974) approach. This procedure yields coefficients for on-the-job experience and the number of years spent in education, with the latter having (under certain assumptions) a structural interpretation as the implied rates of return for an additional year of schooling. Several authors have included cohort variables into the Mincer equation. For instance, Berger (1985) and recently Macunovich (1999) have entered cohort size as an additional explanatory variable. The latter study also tries to separately identify demand and supply effects of demographic change.

In contrast to studies which use cohort size as an independent variable, our focus is not exclusively on demographics because this is certainly not the only cohort-specific impact on wages. Other characteristics pertaining to particular cohorts, such as skills in particular technologies, may also be of importance. Using only size as a cohort-specific explanatory variable may lead to biased estimates if the variables left out from the estimation are correlated with cohort size. Therefore, we estimate Mincer equations with cohorts (consisting of adjacent birth years) entering as dummy variables, both directly on wages as well as in interaction with education.

A similar approach has been taken in a number of other studies. For German data, Fitzenberger et al. (1995) investigate whether the same age-earnings profiles can be observed across cohorts once macroeconomic time effects are accounted for. In one of their estimations (Figures 20 to 24), they look at wages as a function of age, distinguishing between (a) two different birth groups (being five years apart from each other), and (b) four educational categories. Their finding is that there has been, between 1978 and 1983, a reduction in the entry-level wages for workers with completed apprenticeship and/or Abitur (A-levels), while there is no such effect for workers without these qualifications. This points to a decline in educational premia. They do not find a similar effect for university-educated workers, but this may be due to the limitations of their data source: since they use data from social security files, in which income is recorded only up to an upper threshold, their results for highly educated workers suffer from a problem of right-censoring.

For the US, the UK and Canada, Card and Lemieux (2001) decompose age- and year-specific college premia estimated by a simple regression of wages on age and a college dummy into age-, year-, and cohort-specific components. They conclude that, for all three countries, the rise in average schooling premia is due to the entry of cohorts with permanently higher returns to college rather than to a general rise for all age groups.

So far, we have assumed that the number of workers in each age-education category is determined by exogenous factors (such as demography). In reality, individuals make choices on education and labour force participation which depend, inter alia, on wages. A large strand of the literature on the returns to education concentrates on the potential endogeneity of schooling with respect to wages, as well as on measurement error in this variable. In their estimations of cohort effects in the returns to education in Norway, Hægeland et al. (1999) use a two-step procedure to correct for the selectivity into education as well as into labour force participation. The overall differences between OLS and the selectivity estimator appear not to be large, although this may be due to the fact that the two selectivity effects cancel each other out. As a result, Hægeland et al. find that there is no consistent trend in age-specific educational premia between the years 1980 and 1990.

In general, little consensus seems to have emerged regarding the best way to proceed in the face of endogeneity problems. For example, family background variables (in particular, parents‘ education) are often used to instrument schooling.[3] However, this may even further bias upwards the estimated returns (Card, 1999).

From an institutional point of view, the distinction between returns to education in private and in public employment is also an important topic. In many countries, wage structures in the public sector appear to be more compressed, thus diminishing the returns which can be achieved. This may, of course, lead to systematic selection of individuals into the two sectors. Dustmann and van Soest (1998) provide a systematic empirical treatment of this kind of endogeneity, as well as endogeneity in schooling, hours worked and years of work experience. In particular, their results indicate that the positive correlation between education and the choice of the public sector disappears if schooling is treated as endogenous.

In our paper, we do differentiate between sectors but neglect the endogeneity problem. The results should thus be taken as a first empirical assessment of the existence cohort effects in the returns to education in Germany. However, there seems to be no a priori expectation in which direction our estimates of the magnitudes of the cohort effects should be biased in the presence of endogeneity problems.

III. Methodology and Data

Our basic specification is a Mincer equation augmented by cohort dummies and other influences on log earnings

$$\ln w_{it} = \alpha_0 + \alpha_1 X_{it} + \alpha_2 X_{it}^2 + \alpha_3 S_i + \sum_{k=2}^{K} \beta_k D_{ik} + \sum_{k=2}^{K} \gamma_k D_{ik} S_i + Z_{it}\zeta + u_{it}, \qquad (1)$$

$i = 1, \; ..., \; n$

$t = 1984, \; ..., \; 1997$

where w_{it} stands for wages, X_{it} is work experience in years, S_i is years spent in education, Z_{it} is a vector of characteristics, and D_{ik} is a dummy variable which takes the value one if the individual observed is a member of birth cohort k (defined on a number of years) and zero otherwise. As the subscripts indicate, equation (1) is estimated on an (unbalanced) panel of n individuals observed over at most 14 years. The u_{it} are errors which contain individual-specific components, and estimation is performed by random effects-GLS.

Our data base are 14 waves (from 1984 to 1997) of the German Socio-Economic Panel (GSOEP). We confine ourselves to men and women living in West Germany. In order to have cohorts with a sufficient number of observations, our estimations are restricted to individuals born between 1925 and 1974. Several groups of workers are excluded from the dataset: this concerns students, military personnel, pensioners, and civil servants („Beamte“). In some of the estimations, we exclude all public employees from the sample. We estimate separate wage equations for men and women. Only workers with German nationality are represented in the sample used here.

The dependent variable is net earnings per hour worked. The hours measure includes paid overtime. Among the independent variables, we use two different concepts to measure education. The first is the number of years spent in education. The other is the highest degree reached in education. We distinguish seven broad education/qualification categories: (1) secondary schooling without apprenticeship, (2) secondary schooling and completed apprenticeship, (3) master craftsman, (4) Abitur (A-levels), (5) Abitur and completed apprenticeship, (6) polytechnic degree, (7) university degree.

Experience is a crucial variable in our estimations. Rather than using potential experience, which is typically defined as age minus years of schooling minus six years, we construct a variable for actual labour market experience from retrospective data contained in the GSOEP. Individuals are asked about past spells in full-time or part-time employment since the age of 15. The durations of these spells can then be added to obtain measures for total length of experience. The decisive assumption that we make is that spells out of employment (e.g., due to child raising or unemployment) do not contribute towards the accumulation of human capital rewarded by the labour market, and hence to higher wages. In the German case, this assumption is more problematic for the public than for the private sector because public employees‘ salaries rise automatically with age.

We also differentiate between full-time and part-time experience because these may affect productivity and wages differently. An individual is defined to be in part-time employment if he or she works less than 30 hours per week. However, since the number of men in part-time employment is very small, we use variables measuring part-time experience only in the estimations for women.

The experience variable is important because the identification of year, cohort and experience effects hinges on it. Suppose we measured experience as potential experience, e.g. the number of years beyond age 15 minus the years of post-compulsory education. If, in addition, we included dummy variables for each birth year and the current year in our estimations, there would be a linear dependency because birth year plus 15 years plus years of post-compulsory schooling plus years of potential experience always equals the current year.[4] By contrast, our experience coefficients are identified (a) because actual experience differs from potential experience, and (b) because our cohort measures are dummy variables for a number of birth years (five or ten), such that there is variation in the length of experience within each cohort thus defined.[5]

Apart from schooling and experience, there are two other variables that control for human capital accumulation on the job. The first is tenure, the number of years an individual stayed with his or her current employer. The other is the number of months spent in full-time or part-time employment or in unemplyoment during the current year to control for the continuity of employment during the observation period. We also control for region (German Laender, i.e. federal states), industry, and firm size. Year dummies are included to control for the effect of macroeconomic variables on individual wages.

The Mincer equation implicitly assumes that the returns to education are constant throughout the working life. One may object to this assumption because it takes time to realise the full impact of education on productivity (and thus on wages). Moreover, in the presence of seniority wages a young worker is not compensated according to the full productivity effect of his or her education; instead, wages are held back until later ages. Hence, the returns to education may increase with experience. On the other hand, the effect of initial education on productivity will typically decline at long levels of experience because knowledge depreciates. These effects may bias our estimations of cohort effects because the younger cohorts are observed at earlier ages when they do not reap the full benefits from their educational attainment. Similarly, the apparent increase in wages for men over the pre-war cohorts could be due to the greater impact of schooling on productivity for the middle-aged than for older workers.

To correct for this possible distortion, we included an interaction between (full time-) experience and education into our estimations:

$$\ln w_{it} = \alpha_0 + \alpha_1 X_{it} + \alpha_2 X_{it}^2 + \alpha_3 S_i + \alpha_4 X_{it} S_i + \alpha_5 X_{it}^2 S_i + \sum_{k=2}^{K} \beta_k D_{ik} + \sum_{k=2}^{K} \gamma_k D_{ik} S_i + Z_{it}\zeta + u_{it} \quad . \tag{1'}$$

The interaction is allowed to be non-monotonic to take into account both seniority wages and knowledge depreciation. While educational premia are now allowed to vary parametrically both over cohorts and over the life cycle, we are implicitly assuming that the shape of the interaction stays constant over cohorts. A drawback of using interactions is that the schooling coefficient does not any longer have a structural interpretation as the returns to education, as in the original Mincer equation (1).[6]

Another way, apart from using interaction terms between years of schooling and labour market experience, of checking whether the estimated cohort effects in the returns to education are due to misspecification of the experience part of the equation is to estimate the returns to education for different cohorts *observed at the same age*. We thus compare wages of an early cohort observed at an early year to those of a subsequent cohort observed a corresponding number of years later. For instance, we estimate the schooling coefficient for the cohort born between 1962 and 1968, and observed in 1997, and compare it to the coefficient for the cohort born between 1949 and 1955 and observed in 1984.

Again, there does remain an identifying assumption in these estimations. We attribute all of the difference in the returns to education between the two cohort groups to the true cohort effect, while they may also be produced by year effects, since both sub-groups are observed at different calendar years. Judging from our empirical results, however, there is little indication that year effects are present. This is in line with the received wisdom mentioned in the introduction that there have been little changes in returns to education over time in Germany over the last two decades.

IV. Empirical results

GLS estimation results for specification (1) for men and women are displayed in the left columns of tables 1(a) and 1(b). The coefficients for the cohort dummies are given in the upper part of the table, followed by the number of years spent in education and cohort-education interactions (Schooling 30-34, etc.). The birth years 1925 to 1929 form the base category for the cohort variables. Employment status is the number of months spent in full-time or part-time employment or in unemployment during the current year. All other covariates are as defined before.

– table 1 here –

The schooling-cohort multiplicative terms are jointly significant at the five per cent level for both men and women. Since there are interactions, the cohort effects on wages and the returns to education cannot be inferred from a single coefficient alone but must be calculated from the estimated parameters.[7] Figure 1 presents the returns to education by birth cohorts for both men and women. We observe two downward movements in women's returns to education. One affects the cohorts born after 1935, the other cohorts born after 1965. For cohorts born between 1935 and 1965, returns are more or less constant. By contrast, the returns to education for men decrease continuously over cohorts born after 1945. Overall, the decrease in the returns for later cohorts appears to be quite dramatic both for men and for women, falling from about nine per cent for the 1945-49 cohort to about four per cent for the latest cohort.

– figure 1 here –

To a certain extent, however, the impact of age or experience may be wrongly attributed to the cohort effects by specification (1). Results from the estimation of (1‘) are given in the second column of table 1 and in figure 2. In order to obtain a similar cohort average over the returns to education as in figure 1, we fix experience at 15 years of full-time employment. In interpreting the graph, it has to be kept in mind that this inevitably produces out-of sample predictions. For example, since the last year of the observation period is 1997, there are no individuals with 15 years of work experience in the 1970-74 cohort. Hence, the *absolute level* of the returns to education should not be interpreted for cohorts which have, on average, much more or much less labour market experience in the observation period. The purpose of the figures is to show the difference in the returns to education across cohorts, not their level.

We observe that allowing the educational premia to vary over the working life takes out some of the decline found in the results for specification (1) for men, but much less so for women. In particular, the difference in schooling premia between the youngest cohort (1970-74) and the cohort born 20 years earlier now amounts to roughly three per cent for men, as compared to the five per cent found earlier. Indeed, the chi-squared statistic for the joint significance of the schooling-cohort interactions is now below the critical value. By contrast, there is still a clear negative (but non-monotonic) trend over birth years in the returns to education for women. Finally, we find that the interaction between schooling and experience is statistically significant both for men and for women. Therefore, we will continue to work with specification (1‘) instead of (1).

– figure 2 here –

In specification (1‘), we have accounted for experience effects in the returns to education, but we clearly could not account for age effects because of the linear dependency between cohorts, calendar year and age. If, apart from the experience effect, there is also an effect of age on educational premia, this effect is entirely interpreted as a cohort effect in our results. This seems to be a problem mainly in the public sector. In Germany, the salaries of public employees are raised according to age every two years, independently of the work history of the individual. For the public sector, therefore, pure cohort effects are not properly identified. One would also believe that pure cohort effects are more pronounced in the private than in the public sector because wage determination in the public sector is more rigid. Since there are fixed pay scales, the only remaining instrument to adjust wages in the public sector is to grade individuals of different cohorts into different pay categories.

We deal with these problems by estimating specification (1‘) on the basis of a subsample of private employees only. The results (Table 2 and Figure 3) show that there remains a significant decline in educational returns for women. However, the youngest cohort now still earns returns to education of about seven percent, compared to the six percent found in figure 2. The cohort differences in the schooling coefficient are still jointly significant at the five per cent level. For men, however, there is even less indication for a downturn in the returns to education than before, with the youngest cohort actually earning a slightly higher premium than the second youngest cohort.

– table 2 and figure 3 here –

In a further effort to wipe out the experience or age effect in the returns to education, we estimate cohort effects in schooling premia in regressions for specific age groups. We are comparing cohorts which are 13 years apart, because the first and the last waves of the GSOEP in our dataset, the only years we are using, are just 13 years apart. In order to obtain a larger set of observations, we slightly change the definition of birth cohorts to include individuals born within an interval of seven years. In the following, we are looking at the age group of younger workers, i.e. workers aged 24 to 39, because our previous results suggest that cohort effects are particularly pronounced in this group. Since there are, by construction, no age effects in the differences between the two cohorts, we do not have to distinguish between public and private sector wages.

Table 3 presents results from ten separate OLS estimations for both men and women. The estimations differ with respect to the definition of age (or cohort) groups. For instance, the first estimation compares individuals born in the years 1958 to 1964 with the cohort born 13 years earlier, i.e. the 1945-51 cohort. The earlier cohort is observed in 1984, the second cohort in 1997. This means that in both cohorts, the individuals included in the sample are between 33 and 39 years of age. The second estimation concerns individuals in the age group 32 to 38 and so forth. We present a multiplicity of regressions rather than a single comparison of cohorts in order to avoid focusing on a possibly non-representative subset of the data. From the list of regression coefficients, table 3 only contains the schooling coefficient and the schooling-cohort interaction term while all other coefficients are omitted. The list of covariates is the same as in the regressions presented in table 1. Figure 4 displays the schooling-cohort interaction term graphically.

– table 3 and figure 4 here –

Estimation results show that the cohort-schooling interaction is significant at the five percent level in four regressions for men and in five regressions for women. The decline in the returns to education affects earlier cohorts of men than of women. For men, the oldest cohort which suffers a decline compared to individuals born 13 years earlier consists of individuals born 1960 to 1966; and the youngest of individuals born from 1963 to 1969. For female workers, the picture is different in that returns do not recover for cohorts younger than the 1963-69 birth group.

The order of magnitude of the estimated cohort effects is similar to the one estimated on the whole sample in specifications (1) and (1‘). It is one to two per cent for men, and up to four per cent for women. From the figures, we also note that the omission of year effects in the regression is justified. If the interaction term between schooling and cohort picked up a calendar time effect, its coefficient should be constant for all cohorts defined, since all estimations are performed for the years 1984 and 1997. In the empirical results, however, the coefficient exhibits remarkable changes over cohorts.

V. Results for education levels

Finally, we turn to the estimations using educational *levels* rather than durations. It may be argued that in Germany, due to institutional reasons such as entry requirements to particular occupations, degrees reached are more important for individuals' earnings than years of schooling spent to achieve a given degree. In order to retain reasonable sample sizes at each educational level, we define cohorts as ten-year intervals rather than five-year intervals as before. Again, we allow for the interaction between the highest educational degree, on one hand, and labour market experience and its square, on the other. Table 4 gives the estimated coefficients while figure 5 displays the effects of education on wages for the most relevant of the educational categories. As in figure 2, in the figures experience is fixed at 15 years. [8] For the same reason as before, the level of the returns to education should not be interpreted for cohorts observed at ages where 15 years of labour market experience are uncommon.

– table 4 and figure 6 here –

In absolute terms, the steepest decline in educational premia between the 1945-54 and the 1965-74 cohorts is in the group of workers with university education. For men, the premium falls from 128 percent to about 100 percent between these cohorts, while the decline for women is from 148 percent to 83 percent. In relative terms, we also find a marked decline in the premium for apprenticeship. By contrast, there is a positive cohort effect for male graduates from polytechnics. The university premium for the oldest female cohort is extremely high (more than 300 per cent), which is clearly due to the very small number of older university-educated women in the sample. Hence, this cohort-education group is omitted from the graphs. The relative decline in the apprenticeship premium is particularly visible if we calculate the implied rates of return on human capital investment (table 5), taking account of the fact that a higher education level means less time in employment and, therefore, lower lifetime earnings.

– table 5 here –

VI. Conclusions

In contrast to the experience of the United States and other countries, we have found evidence for a decline in the returns to education in West Germany over cohorts born after the Second World War. The decline appears to be much stronger for women than for men, and it affects a larger number of female than of male birth cohorts.[9] It seems to have taken place at high as well as low educational levels. Our findings also appear to be reasonably robust against different specifications of the experience part of the equation. In particular, we have relaxed the assumption that the returns to education are independent of labour market experience, allowing years of schooling and years of labour market experience and its square to interact. We also used sub-samples of workers observed at the same age to get rid of possible age or work experience effects in the cohort-specific returns to education.

A question which we have not tackled in this paper is how the cohort effects in educational premia can be explained. It appears that there are several main candidates for an explanation. First, there was a strong increase in female labour force participation. Second, educational attainment increased over the post-war cohorts, and third, West Germany experienced a baby boom which peaked in the mid-1960s. The expansion of labour supply for jobs primarily taken by women is most probably the prime source of the gender differences found in our estimations. The expansion of educational attainment enhanced the supply of qualified relative to unskilled workers and could have led to a reduction in educational premia. However, this explanation does not sit well with the fact that the decline is not limited to higher education but is also visible in the apprenticeship premia. This suggests that the development of educational premia, insofar as they affect both men and women, may have more to do with demography.

An objection to this explanation could be that, if it were correct, we should observe an increase in educational premia for individuals born after 1968 because cohort size declined hugely after that year. According to our estimations, however, returns to education continued to shrink. We do not find this objection compelling because it is still too early for a precise estimate of the educational premium for these cohorts, many of its members not having entered the labour market by the time they were observed. Future research, however, should investigate whether the elasticities of substitution between age and skill groups do indeed fit the pattern consistent with the demographic explanation.

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Table 1: Returns to years spent in education

a) Men

	(1)		(1')	
Variable	**Coeff.**	**t-stat.**	**Coeff.**	**t-stat.**
Cohort 1930-34	-0.098	-0.62	-0.067	-0.42
Cohort 1935-39	-0.147	-1.12	-0.091	-0.68
Cohort 1940-44	-0.065	-0.49	-0.020	-0.15
Cohort 1945-49	-0.149	-1.11	-0.152	-1.07
Cohort 1950-54	-0.058	-0.44	-0.131	-0.94
Cohort 1955-59	0.019	0.15	-0.108	-0.79
Cohort 1960-64	0.102	0.81	-0.084	-0.61
Cohort 1965-69	0.193	1.5	-0.027	-0.19
Cohort 1970-74	0.197	1.18	-0.068	-0.38
Schooling	0.081	8.14	0.053	4.75
Schooling*Experience			0.003	8.72
Schooling*Experience²			-0.005	-6.07
Schooling 30-34	0.008	0.55	0.005	0.35
Schooling 35-39	0.011	0.96	0.006	0.52
Schooling 40-44	0.005	0.44	0.001	0.10
Schooling 45-49	0.009	0.75	0.009	0.75
Schooling 50-54	-0.001	-0.05	0.006	0.51
Schooling 55-59	-0.010	-0.93	0.001	0.10
Schooling 60-64	-0.018	-1.66	-0.002	-0.14
Schooling 65-69	-0.028	-2.58	-0.009	-0.76
Schooling 70-74	-0.036	-2.49	-0.013	-0.88
Experience	0.026	19.81	-0.006	-1.62
Experience²/100	-0.053	-26.00	0.009	0.87
Tenure	0.001	0.78	0.000	0.48
Tenure²/100	0.005	2.50	0.006	2.79
Employment status	0.014	19.17	0.014	18.57
Unemployment status	0.002	1.49	0.002	1.23
Year Dummies	YES		YES	
Industry Dummies	YES		YES	
Laender Dummies	YES		YES	
Firm size dummies	YES		YES	
Number of observations	19,004		19,004	
Number of individuals	3,124		3,124	
Test of schooling-cohort interaction, $\chi^2(9)$	66.37		10.48	
R^2	0.481		0.484	

All estimations contain random effects.
Test of specification 1' against 1: $\chi^2(2) = 84.93$

b) Women

Variable	(1) Coeff.	(1) t-stat.	(1') Coeff.	(1') t-stat.
Cohort 1930-34	-0.028	-0.12	0.000	0.00
Cohort 1935-39	0.469	2.20	0.506	2.36
Cohort 1940-44	0.366	1.75	0.340	1.58
Cohort 1945-49	0.334	1.68	0.332	1.61
Cohort 1950-54	0.407	2.18	0.376	1.92
Cohort 1955-59	0.631	3.53	0.558	2.94
Cohort 1960-64	0.522	2.93	0.413	2.17
Cohort 1965-69	0.896	5.04	0.758	3.97
Cohort 1970-74	0.975	4.76	0.818	3.78
Schooling	0.116	7.63	0.096	5.71
Schooling*Experience			0.003	4.94
Schooling*Experience²			-0.007	-3.48
Schooling 30-34	0.002	0.11	0.000	-0.01
Schooling 35-39	-0.047	-2.34	-0.050	-2.49
Schooling 40-44	-0.031	-1.60	-0.028	-1.43
Schooling 45-49	-0.025	-1.37	-0.025	-1.30
Schooling 50-54	-0.030	-1.76	-0.027	-1.50
Schooling 55-59	-0.046	-2.87	-0.039	-2.28
Schooling 60-64	-0.037	-2.27	-0.027	-1.53
Schooling 65-69	-0.067	-4.17	-0.055	-3.14
Schooling 70-74	-0.078	-4.22	-0.063	-3.23
Full-time Experience	0.032	19.83	-0.001	-0.12
FT Experience²/100	-0.053	-13.59	0.021	1.00
Part-time Experience	0.006	2.90	0.007	3.10
PT Experience²/100	0.003	0.44	0.002	0.28
Part-time dummy	0.076	10.45	0.075	10.29
Tenure	0.004	4.26	0.004	4.02
Tenure²/100	-0.013	-3.32	-0.012	-3.03
Employment status: FT	0.012	12.94	0.012	12.73
Employment status: PT	0.007	6.65	0.006	6.48
Employment status: UN	-0.002	-0.96	-0.002	-0.95
Married	-0.007	-0.89	-0.009	-1.16
Year dummies	YES		YES	
Industry dummies	YES		YES	
Laender dummies	YES		YES	
Firm size dummies	YES		YES	
Number of observations	13,492		13,492	
Number of individuals	2,604		2,604	
Test of schooling-cohort interaction, $\chi^2(9)$	58.46		33.57	
R^2	0.386		0.389	

All estimations contain random effects.
Test of specification 1' against 1: $\chi^2(2) = 26.03$

Table 2: Estimation results for equation (1‘), private sector employees only

	Men		**Women**	
Variable	**Coeff.**	**t-stat.**	**Coeff.**	**t-stat.**
Cohort 1930-34	-0.057	-0.32	-0.106	-0.28
Cohort 1935-39	0.035	0.22	0.439	1.13
Cohort 1940-44	0.060	0.38	0.348	0.93
Cohort 1945-49	-0.006	-0.04	0.182	0.48
Cohort 1950-54	-0.047	-0.29	0.311	0.86
Cohort 1955-59	-0.024	-0.15	0.545	1.53
Cohort 1960-64	0.093	0.58	0.443	1.24
Cohort 1965-69	0.069	0.42	0.758	2.13
Cohort 1970-74	-0.081	-0.41	0.738	1.97
Schooling	0.062	4.63	0.088	2.56
Schooling*Experience	0.003	8.23	0.003	3.34
Schooling*Experience²	-0.006	-5.71	-0.004	-1.42
Schooling 30-34	0.004	0.24	0.012	0.31
Schooling 35-39	-0.004	-0.28	-0.042	-1.10
Schooling 40-44	-0.005	-0.34	-0.028	-0.75
Schooling 45-49	-0.003	-0.18	-0.007	-0.18
Schooling 50-54	0.000	-0.02	-0.018	-0.50
Schooling 55-59	-0.005	-0.37	-0.034	-0.99
Schooling 60-64	-0.015	-1.13	-0.024	-0.69
Schooling 65-69	-0.015	-1.09	-0.049	-1.39
Schooling 70-74	-0.008	-0.47	-0.050	-1.36
Full-time Experience	-0.008	-1.75	0.004	0.50
FT Experience²/100	0.012	1.09	-0.011	-0.38
Part-time Experience			0.011	3.79
PT Experience²/100			-0.009	-0.91
Part-time dummy			0.061	6.95
Tenure	0.000	-0.29	0.004	2.99
Tenure²/100	0.006	2.58	-0.011	-2.29
Employment status: FT	0.015	17.61	0.012	10.11
Employment status: PT			0.006	4.82
Employment status: UN	0.004	2.02	-0.004	-1.83
Married			-0.011	-1.21
Year dummies	YES		YES	
Industry dummies	YES		YES	
Laender dummies	YES		YES	
Firm size dummies	YES		YES	
Number of observations	15,661		9,344	
Number of individuals	2,969		2,017	
Test of schooling-cohort interaction, $\chi^2(9)$	8.86		19.13	
R^2	0.492		0.378	

All estimations contain random effects.

Table 3: Comparison of different cohorts observed at the same age

a) Men

	1958-64 vs. 1945-51	1959-65 vs. 1946-52	1960-66 vs. 1947-53	1961-67 vs. 1948-54	1962-68 vs. 1949-55
Schooling	0.076	0.078	0.081	0.080	0.083
	(9.00)	(9.80)	(10.39)	(10.15)	(11.54)
Schooling*2nd cohort	-0.008	-0.014	-0.019	-0.022	-0.024
	(-0.91)	(-1.54)	(-2.15)	(-2.63)	(-2.92)
Number of observations	574	592	624	649	668
R^2	0.391	0.407	0.409	0.397	0.394
adjusted R^2	0.353	0.372	0.375	0.364	0.361

	1963-69 vs. 1950-56	1964-70 vs. 1951-57	1965-71 vs. 1952-58	1966-72 vs. 1953-59	1967-73 vs. 1954-60
Schooling	0.076	0.069	0.065	0.067	0.065
	(10.62)	(0.89)	(9.56)	(9.65)	(9.02)
Schooling*2nd cohort	-0.016	-0.009	-0.011	-0.014	-0.020
	(-1.97)	(-1.09)	(-1.36)	(-1.61)	(-2.03)
Number of observations	661	645	627	623	571
R^2	0.396	0.391	0.373	0.347	0.318
adjusted R^2	0.363	0.357	0.337	0.309	0.274

a) Women

Variable	1958-64 vs. 1945-51	1959-65 vs. 1946-52	1960-66 vs. 1947-53	1961-67 vs. 1948-54	1962-68 vs. 1949-55
Schooling	0.056	0.065	0.064	0.075	0.068
	(3.17)	(3.96)	(3.99)	(5.57)	(5.69)
Schooling*2nd cohort	-0.003	-0.007	-0.009	-0.021	-0.027
	(-0.18)	(-0.37)	(-0.52)	(-1.42)	(-1.91)
Number of observations	368	373	401	413	452
R^2	0.500	0.503	0.497	0.519	0.446
adjusted R^2	0.451	0.455	0.452	0.477	0.402

Variable	1963-69 vs. 1950-56	1964-70 vs. 1951-57	1965-71 vs. 1952-58	1966-72 vs. 1953-59	1967-73 vs. 1954-60
Schooling	0.068	0.072	0.073	0.063	0.068
	(6.40)	(7.38)	(8.13)	(7.04)	(7.58)
Schooling*2nd cohort	-0.030	-0.038	-0.037	-0.028	-0.038
	(-2.34)	(-3.23)	(-3.39)	(-2.56)	(-3.24)
Number of observations	451	459	475	485	467
R^2	0.413	0.407	0.398	0.419	0.389
adjusted R^2	0.367	0.361	0.353	0.377	0.339

Table 4: Returns to education levels, men and women

	Men		*Women*	
Variable	**Coeff.**	**t-stat.**	**Coeff.**	**t-stat.**
Cohort 1935-44	-0.019	-0.44	0.056	1.53
Cohort 1945-54	-0.101	-1.85	0.019	0.44
Cohort 1955-65	-0.108	-1.90	0.191	4.37
Cohort 1965-74	-0.144	-2.30	0.217	4.44
Apprenticeship	0.099	1.68	0.209	3.97
Master	0.247	3.12	0.442	4.04
Abitur	-0.069	-0.23	-0.342	-1.47
Abitur+Apprenticeship	0.351	3.28	0.328	2.38
Polytechnic	0.303	3.37	0.347	2.53
University	0.565	5.05	1.274	6.15
Apprenticeship 35-44	0.012	0.26	-0.066	-1.27
Apprenticeship 45-54	0.075	1.31	0.064	1.16
Apprenticeship 55-64	0.032	0.57	-0.076	-1.39
Apprenticeship 65-74	0.006	0.10	-0.077	-1.34
Master 35-44	-0.050	-0.80	-0.130	-1.19
Master 45-54	0.001	0.01	0.025	0.23
Master 55-64	-0.039	-0.54	-0.137	-1.26
Master 65-74	-0.023	-0.29	-0.189	-1.69
Abitur 35-44			-0.357	-1.59
Abitur 45-54	0.044	0.14	0.342	1.37
Abitur 55-64	0.043	0.14	0.285	1.25
Abitur 65-74	0.176	0.58	0.218	0.95
Abitur+Appr. 35-44	0.042	0.38	-0.357	-2.07
Abitur+Appr. 45-54	0.092	0.85	0.128	0.89
Abitur+Appr. 55-64	-0.086	-0.80	-0.084	-0.61
Abitur+Appr. 65-74	-0.162	-1.47	-0.116	-0.81
Polytechnic 35-44	0.077	0.96	-0.352	-2.04
Polytechnic 45-54	0.000	0.00	-0.148	-1.04
Polytechnic 55-64	0.105	1.17	0.176	1.27
Polytechnic 65-74	0.140	1.47	-0.022	-0.14
University 35-44	-0.066	-0.62	-0.684	-2.95
University 45-54	0.016	0.14	-0.556	-2.57
University 55-64	-0.039	-0.35	-0.746	-3.52
University 65-74	-0.116	-1.00	-0.858	-3.98
Apprent.*Experience	0.004	1.26	0.004	1.26
Master*Experience	0.007	1.71	-0.006	-0.96
Abitur*Experience	0.031	3.18	0.066	5.46
Abit.+App.*Experience	0.008	1.82	0.010	1.74
Polytech. *Experience	0.019	4.48	0.022	2.59
Univ. *Experience	0.026	6.67	0.028	3.94
Apprent.*Experience^2	-0.009	-1.63	-0.007	-0.88
Master*Experience^2	-0.015	-1.91	0.003	0.15
Abitur*Experience^2	-0.003	-0.08	-0.123	-3.24
Abit.+App.*Experience^2	-0.018	-1.66	-0.003	-0.14

Table 4 (continued)

	Men		*Women*	
Variable	**Coeff.**	**t-stat.**	**Coeff.**	**t-stat.**
Polytech. *Experience²	-0.036	-3.51	-0.039	-1.81
Univ. *Experience²	-0.063	-6.09	-0.103	-3.48
FT Experience	0.018	6.40	0.026	9.42
FT Experience²/100	-0.037	-6.95	-0.040	-6.01
PT Experience			0.005	2.30
PT Experience²/100			0.005	0.60
Part-time dummy			0.076	10.47
Tenure	0.001	0.75	0.004	4.05
Tenure²/100	0.005	2.62	-0.012	-3.01
Employment status: FT	0.013	17.12	0.011	12.26
Employment status: PT			0.006	6.42
Employment status: UN	0.001	0.52	-0.002	-1.04
Married			-0.015	-1.85
Year dummies	YES		YES	
Industry dummies	YES		YES	
Laender dummies	YES		YES	
Firm size dummies	YES		YES	
Number of observations	19,004		13492	
Number of individuals	3,124		2,604	
Test of schooling-cohort interaction, $\chi^2(24)$	52.12		81.28	
R^2	0.482		0.368	

All estimations contain random effects.

Table 5: Calculated rates of return of different education levels

a) men

Education levels compared		*Rates of return* Cohort 1945-54	*Rates of return* Cohort 1965-74
secondary education only	apprenticeship (+ 3 yrs)	6.31	3.90
secondary education only	university degree (+8 yrs)	9.77	7.98

b) women

Education levels compared		*Rates of return* Cohort 1945-54	*Rates of return* Cohort 1965-74
secondary education only	apprenticeship (+ 3 yrs)	5.62	1.37
secondary education only	university degree (+8 yrs)	7.24	4.21

Note: Results are based on table 4. Implied rates of return are calculated for 15 years of potential working experience:

$$\exp\left\{ \frac{1}{E_j} \Big(\beta_{jk} + \beta_{level*EXP} * 15 + \beta_{level*EXP^2} * 225 + \beta_{EXP} * (15 - E_j) - \beta_{EXP^2} * (15 - E_j)^2 - \beta_{EXP} * 15 + \beta_{EXP^2} * 225 \Big) \right\}$$

where β_{jk} is the coefficient for educational level j estimated for cohort k, $\beta_{level*EXP}$ and $\beta_{level*EXP^2}$ are those for the education-experience interaction terms, β_{EXP} and β_{EXP^2} are the coefficients of experience and its square, and E_j is the number of additional years required for educational level j.

Figure 1(a): Cohort-specific returns to education, men

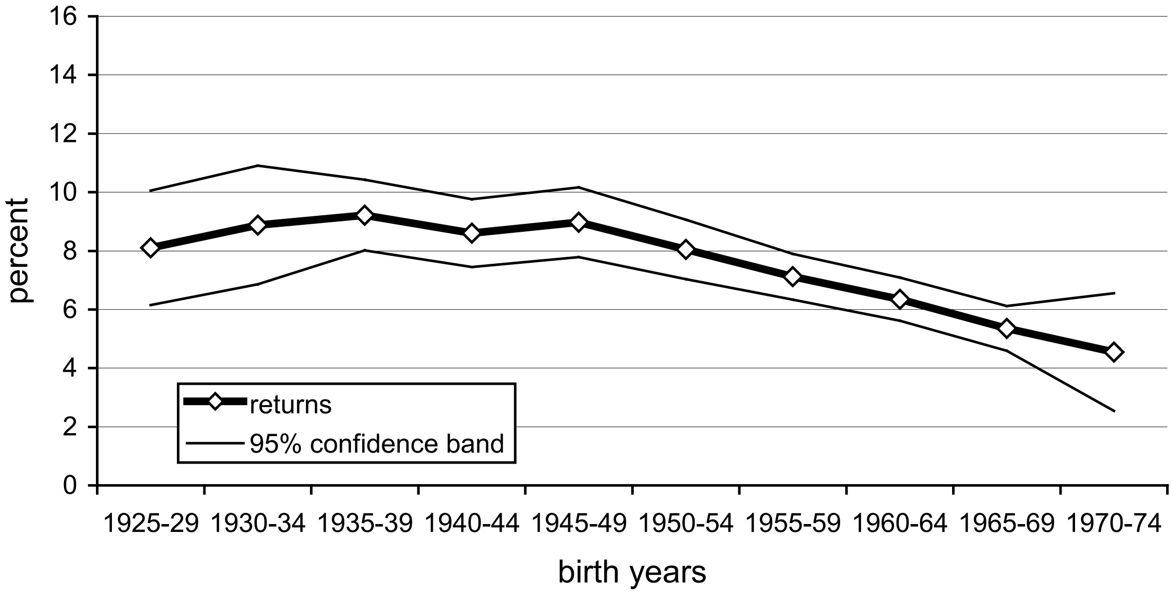


Figure 1(b): Cohort-specific returns to education, women

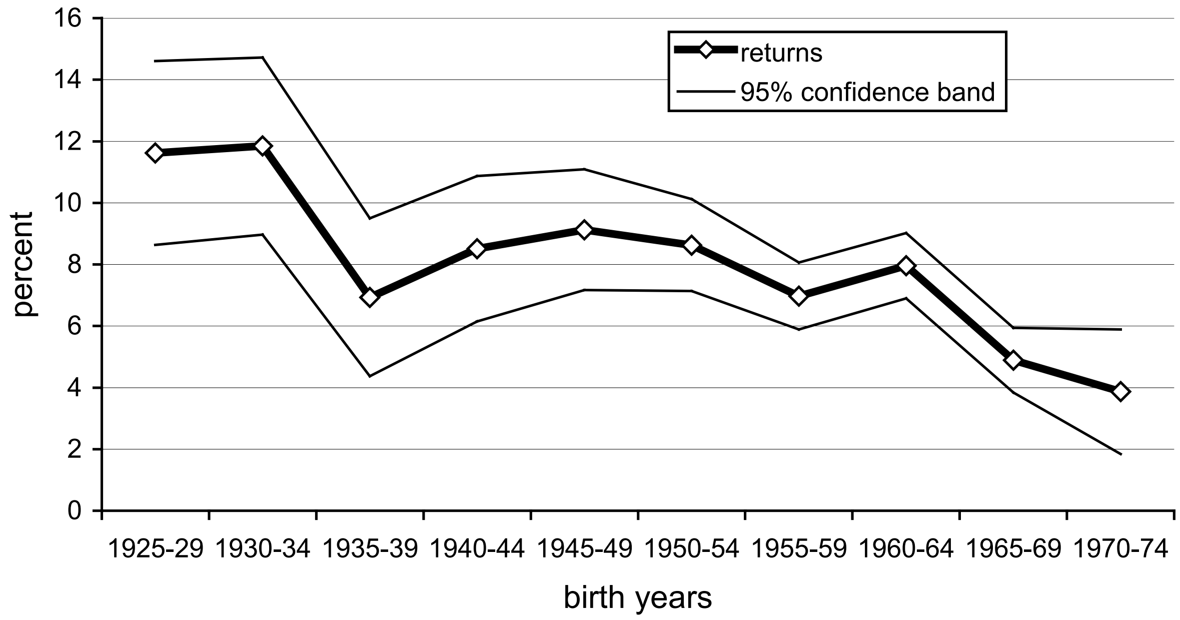


Figure 2(a): Education premium for men, accounting for schooling-experience interaction

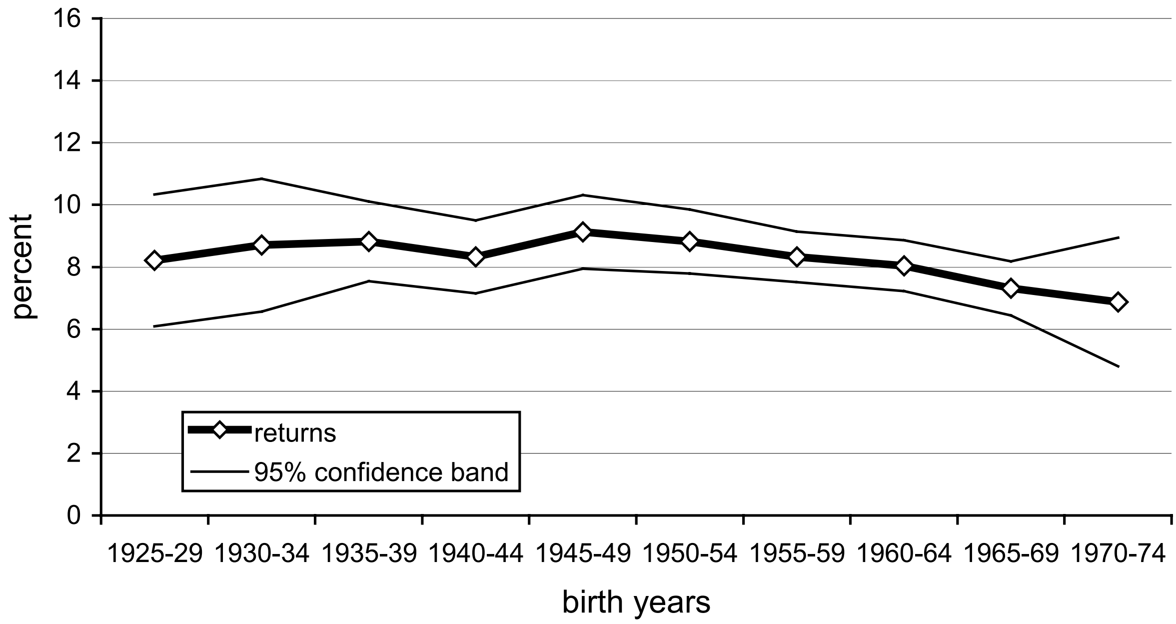


Figure 2(b): Education premium for women, accounting for schooling-experience interaction

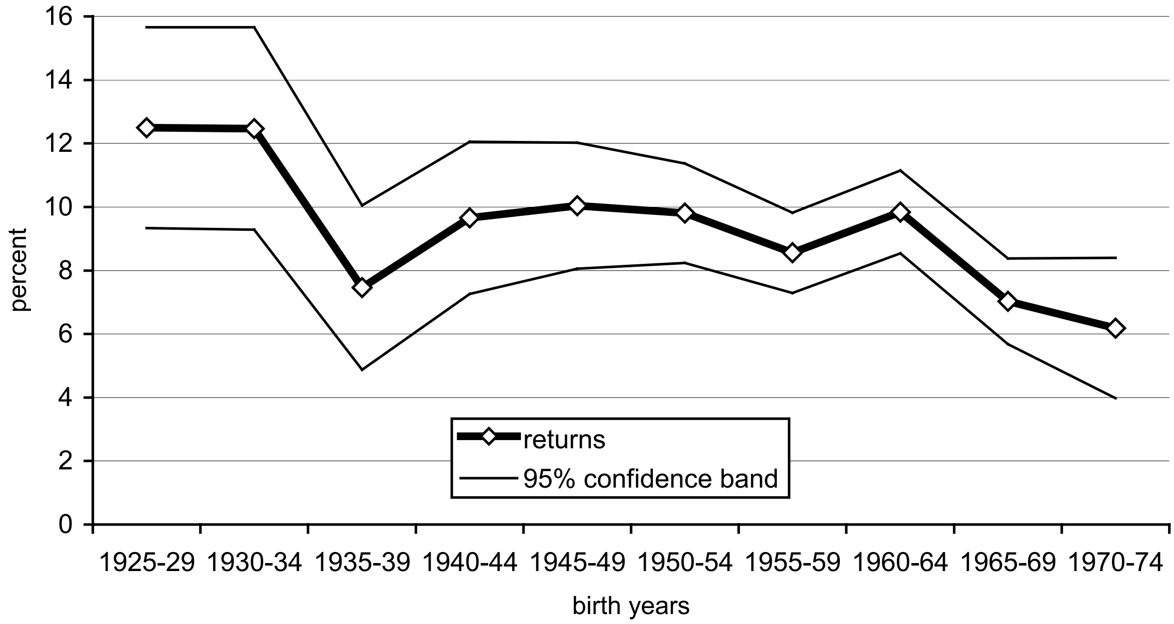


Figure 3(a): Education premium for men, accounting for schooling-experience interaction, private sector employees only

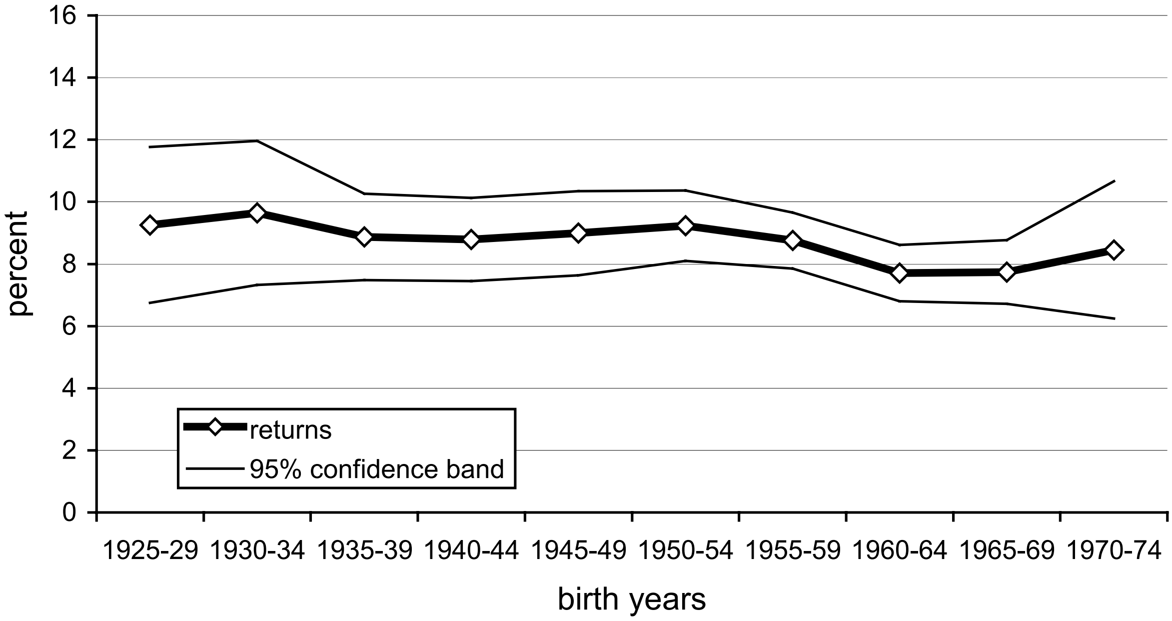


Figure 3(b): Education premium for women, accounting for schooling-experience interaction, private sector employees only

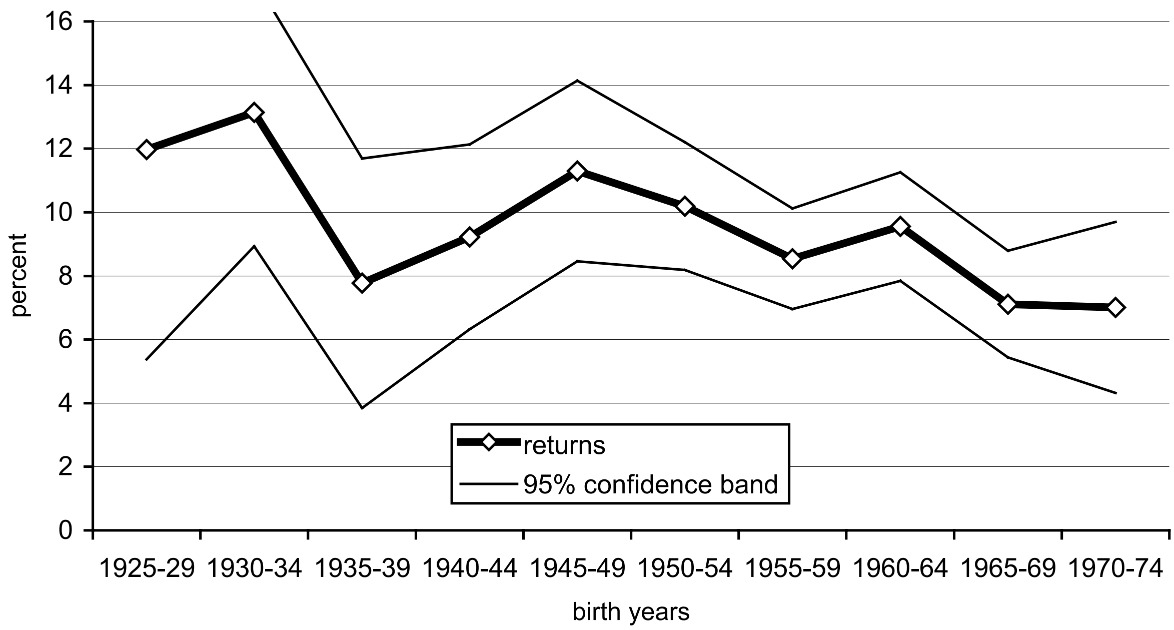


Figure 4 (a): Differences in schooling premia across cohorts, men

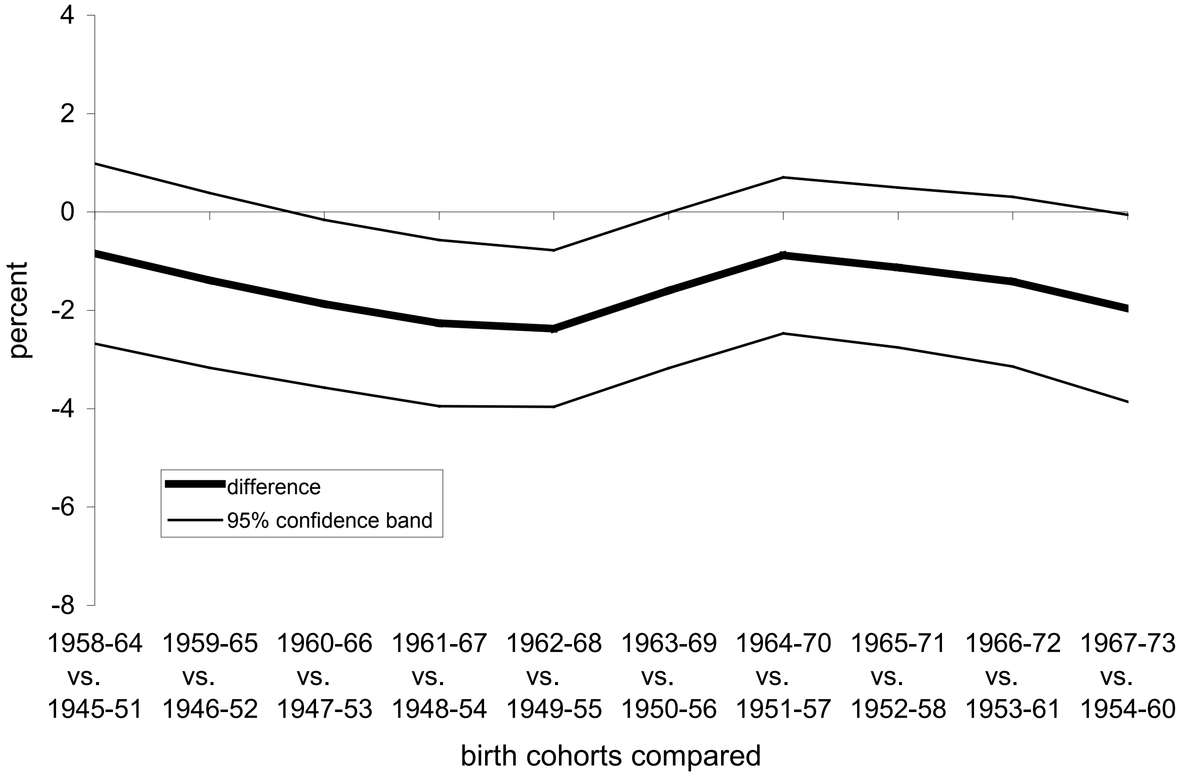


Figure 4 (b): Differences in schooling premia across cohorts, women

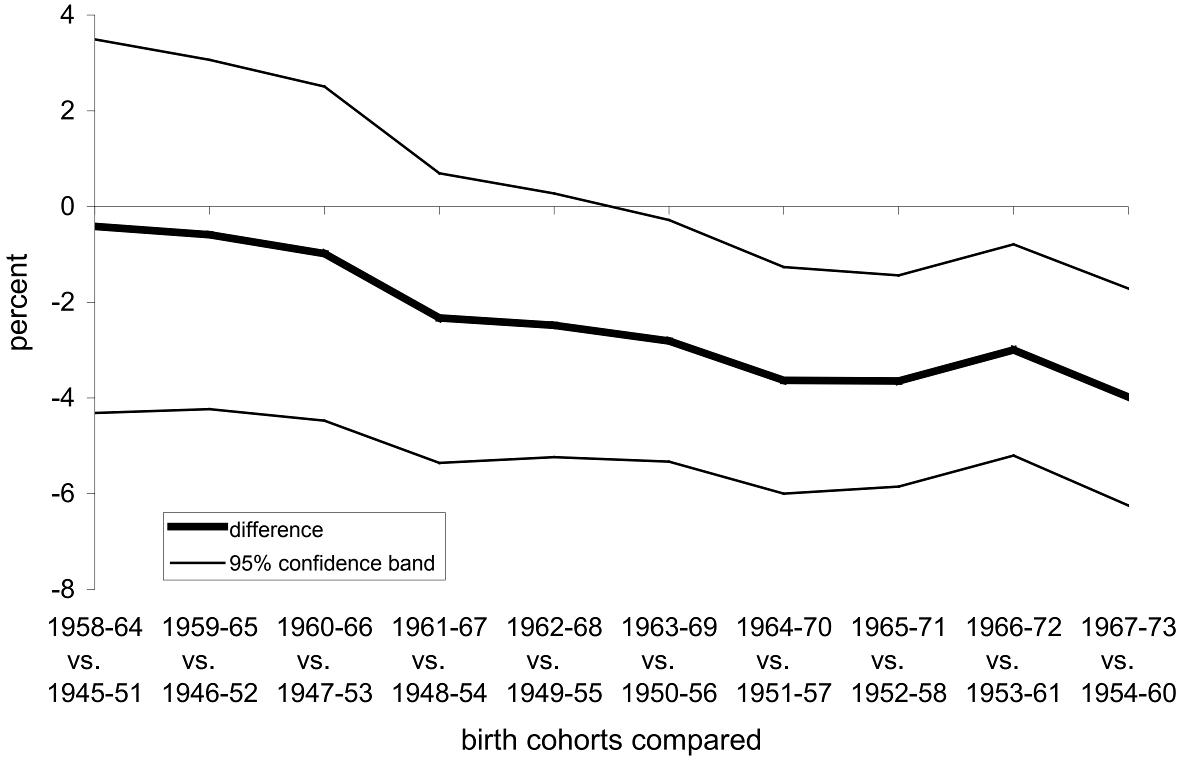


Figure 5(a): Premia for education levels, men

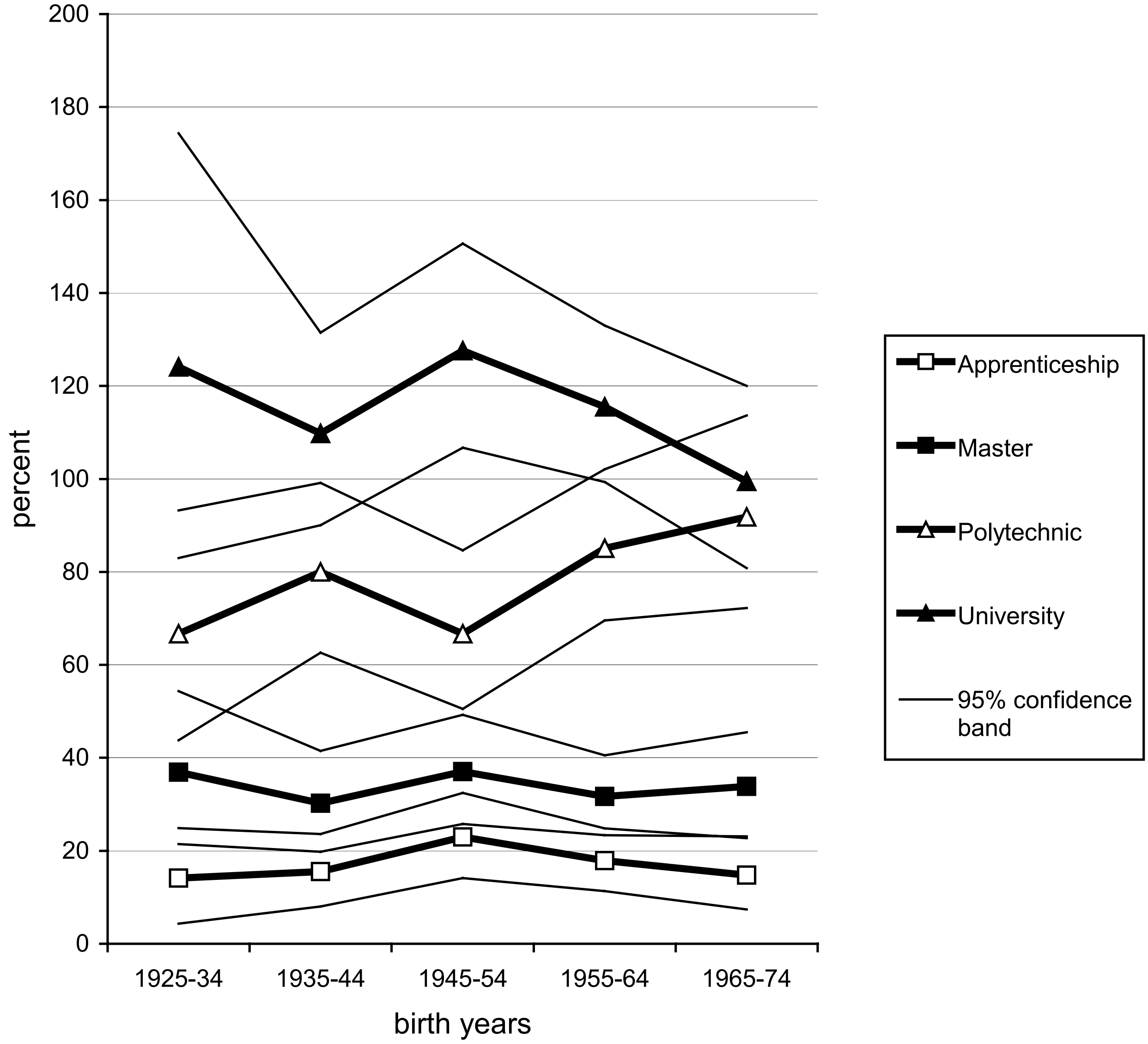
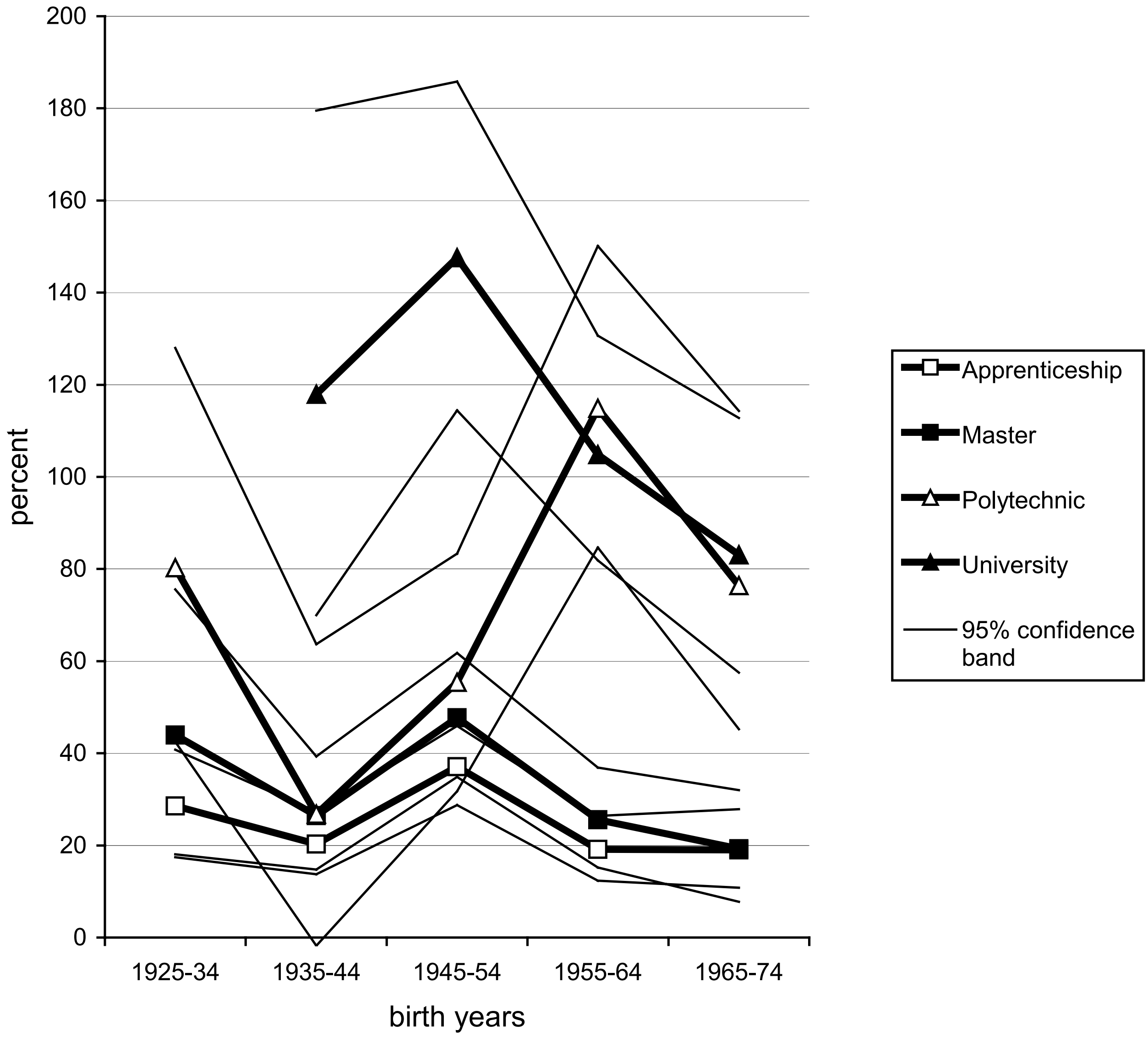


Figure 5(b): Premia for education levels, women



Footnotes

1 Some evidence is contained in Fitzenberger et al. (1995, 2001). Lauer and Steiner (2000) present results similar to those discussed in this paper.

2 Of course, the pay differential between old and young workers would imply that older workers are paid above their productivity, while younger workers are paid below. Employers would then try to dismiss older and hire younger workers, creating unemployment among the first. This may explain why, in the empirical results reported by Zimmermann (1991), an increase in the size of young cohorts has a stronger impact on unemployment among older than among younger workers. At the same time, however, older workers are usually better protected against redundancy than younger workers in Germany due to special employment protection regulations.

3 Using the same datasource as in this paper, Lauer and Steiner (2000) instrument years of schooling by variables such as father's education, parents' employment status, etc. This procedure has only minor effects on the estimated returns to schooling.

4 For a discussion of the identification problem, see Heckman and Robb (1985) who also address identification of interaction and higher order terms.

5 If we include birth year dummies instead of dummies for five-year cohorts, the experience coefficients remain virtually unchanged. Thus identification does not critically depend on the functional form assumed for cohorts. By contrast, since actual experience is not contained in their data, Hægeland et al. (1999) only use the second way of identifying cohort effects. In the following, we use five- (or ten-) year cohort groups in order to keep the number of parameters estimated reasonably low, not as a necessary condition for identification.

6 Another decomposition of the education coefficient in a Mincer equation could be made by using quantile regression techniques as in Hartog et a. (1999).

7 The same is true for the standard errors. In the graphs, standard errors are calculated using the delta method. Suppose the total effect of education on wages is $h(\alpha_1, \alpha_2) = \alpha_1 + \alpha_2 X$, where the α are parameters and X is some independent variable. The variance of the total effect is $Var(\hat{\alpha}) = AV'A$, with A a matrix of partial derivatives $\left[\partial h / \partial \alpha_1 \quad \partial h / \partial \alpha_2\right]$ and V the covariance matrix of the estimated parameters α_1 and α_2 .

8 Figures are percentage differences to the lowest category (no apprenticeship, no higher education). They are calculated as $exp(\beta_{level} + \beta_{level*cohort} + \beta_{level*experience} *15 + \beta_{level*experience^2} *225) - 1$.

9 In their empirical investigation, Fitzenberger and Wunderlich (2000) present evidence which appears to be in conflict with our results. However, since they look at cohort effects in the *levels* of wages at different percentiles of the wage distribution, they do not address education premia directly. Therefore, it is difficult to compare their results with ours.