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OMITTED ABILITY BIAS AND THE WAGE PREMIUM FOR SCHOOLING: NEW SWEDISH EVIDENCE

by

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Omitted Ability Bias and the Wage Premium for Schooling: New Swedish Evidence

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Abstract—There is extensive debate in the schooling literature about the failure to control for ability. This paper uses two new data sets that include various measures of ability, collected when the respondents were between 12 and 13 years old. The measures include scores from intelligence tests, achievement tests and school marks. In line with general opinion, the estimated wage premiums for education fall considerably when ability is controlled for. The average reduction is around 20 per cent, which is lower than comparable figures obtained in the U.S. I also find that measures associated with mathematics are the most important ones.

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I. Introduction

Economists have long stressed the importance of human capital accumulation for the process of economic growth, and at present human capital plays an important role in models of endogenous economic growth, e g Barro (1991) and Romer (1990). Lately, the discussion in Sweden among politicians and economists has focused on the level of economic return to education and its implications for the incentive to go on to higher education, see SOU (1993:16) and Konjunkturrådets rapport (1997).

Since the 1960s, the returns to schooling, estimated by means of conventional wage regressions, have decreased from approximately eight per cent in 1968 to four per cent in 1991, with the main fall in the early 1970s, see Edin and Holmlund (1995), Björklund and Kjellström (1994). Some argue that, compared to other countries, the level is too low to attract students to further education, see Henrekson (1992), whereas others claim that the return is not extremely low in Sweden, Edin et al. (1993).

Unfortunately, the magnitudes of the estimated returns to schooling referred to in this discussion are potentially biased due to the omission of relevant variables in the earnings function. Since factors such as ability are not captured in conventional wage equations, the general opinion has been that the estimated return to schooling is biased upwards. However, the direction of the bias is impossible to predict from theoretical optimization models (see below).

During the last few decades a number of labour economists have paid much attention to the problem of ability as an omitted variable. A number of U.S. studies have examined whether changes in ability or changes in the return to ability could to some extent explain the change in the return to schooling over time, most notably during the 1980s in the United States. Despite the fact that the growing importance of ability in wage determination is supported by the empirical findings presented in Blackburn and Neumark (1993) and Murnane et al. (1995), the increased role of ability can not entirely explain the sharp increase in the return to schooling for men that occurred in the 1980s.

However, Murnane et al. show that including measures of ability in the wage equation results in a considerable decline (40 to 50 per cent) in the *magnitude* of the coefficient on schooling. Similar results are reported in two other US studies, Griliches (1977), Blackburn and Neumark (1995). They show that including measures of ability in the wage equation reduces the estimated return to education by up to 40 per cent. Furthermore, Blackburn and Neumark (1992) have also examined the relationship between wages and ability in a number of industrial sectors. Apart from measures of ability, they also incorporate the possibility of inter-industry wage differentials in the wage equation. The empirical findings, however, indicate that inter-industry wage differentials are not chiefly attributable to variation in ability.

In these studies, unobserved ability is modelled as a latent variable, with intelligence tests serving as indicators of ability. Some of these tests were done in the last year of high school, whereas some others were done at different ages in connection to the surveys. For instance, Griliches (1977), Blackburn and Neumark (1992) report two sets of estimates, one using the scores from a high school intelligence test and the other using the Knowledge of the World of Work (KWW) test, which examines the respondents' knowledge about the labour market. Murnane et al. (1995) uses the scores from a test of mathematics skill. Blackburn and Neumark (1993, 1995), in turn, use a data set which provides information about the scores from each of the ten components of the Armed Services Vocational Aptitude Battery (ASVAB) tests developed to predict performance in armed forces training programs. The battery of tests consists of achievement tests designed to measure vocabulary, basic science and arithmetic. There are also tests of specific vocational skills and tests of numerical and coding operations.

There are alternative approaches to eliminate ability bias. A common approach is to utilize the relative closeness of such unmeasured factors as ability and childhood conditions by the use of differences between twins or siblings. For instance, Ashenfelter and Krueger (1994) have employed US data for identical twins to study the economic return to schooling. In sharp contrast to the general view, they do not find evidence that unobserved factors cause an upward bias in the ordinary least squares estimates of the return to schooling. They do find that measurement error in education does bias it downwards. This approach, however, can be and indeed has been criticized. Hanushek (1992) shows that parents appear to act in a compensatory manner. Families want to reduce income inequalities between siblings in adult life by sending less promising children for further schooling.

Another problem with twin studies is that the variation in schooling between twins is lower than between randomly selected individuals, which in turn aggravates the problem of measurement errors. Ashenfelter and Zimmerman (1997) use a representative sample of data on fathers, sons and brothers to control for omitted variables. They found that controlling for both omitted variables and measurement errors in reported schooling yields results comparable to conventional ordinary least squares estimates of the return to schooling.

A third alternative is the instrumental variables (IV) approach. Angrist and Krueger (1991), for instance, exploit the impact of compulsory schooling laws on schooling decisions, to create instruments for schooling that are uncorrelated with ability. Using quarter of birth as an instrument for education in the earnings function, Angrist and Krueger find a remarkable similarity between the IV estimates and the OLS estimates of the return to schooling. Thus, the empirical findings differ greatly depending upon choice of method.[1]

Previous cross-sectional studies in Sweden have, to some extent, attempted to control for the effect of unobserved ability on wages. To the best of my knowledge, however, explicit measures of ability have only been used in a minority of studies. Gustafsson (1990) uses test scores from intelligence tests collected when the individuals were between 12 and 13 years old. He finds a relatively small effect of ability on annual income when education is controlled for. However, he does not examine the magnitude of the omitted ability bias in the schooling coefficient. Instead of test scores from intelligence tests, Erikson and Jonsson (1997) use school marks. In conformity with the general opinion, their results suggest that the return to schooling is biased upwards.

Instead of explicit measures of ability, information about various family background variables, like father's and mother's occupation and education, have been more frequently utilized in the estimation. For instance, once age, marital status, place of residence, parental occupation and education dummies are included in the regression, the economic return to education falls, Björklund and Kjellström (1994).

Similar results are presented in Kazamaki Ottersten et al. (1996), who deal with three econometric problems associated with the measurement of the return to education: omitted variable bias, selection bias and measurement error in schooling. Furthermore, instead of family background they use a more general concept: social capital. Their main conclusion is that measurement error in schooling is the important problem. A similar conclusion is drawn by Isacsson (1997). He employs a sample of twins born in Sweden between 1926 and 1958 to correct for omitted ability bias. Correction for ability leads in general to lower estimates of the return to schooling, but in conformity with

1. Card (1995), however, attempts to explain the seemingly contrasting results.

Kazamaki Ottersten et al. he found that the results were sensitive to measurement error in schooling.

The purpose of this study is to shed further light on the magnitude and the direction of the bias in the estimated return to education in Sweden by exploiting two data sets, which previously not have been used to estimate wage equations.[2] The data sets consist of several measures of intelligence and scholastic achievement collected when the respondents were between the ages of 12 and 13. The intelligence tests represent the verbal, spatial and reasoning factors of intelligence. School marks and scores on national standardized school tests are available in *Mathematics*, *English* and *Swedish*.

The advantage of using test scores obtained in the early part of schooling is the similarity in education-related background variables. Thus, in contrast to previous US studies age- and education-related ability differentials are of minor importance in this study. Neither are the intelligence tests truncated, which could be the case with tests of which the main task, after all, are to screen individuals.

Information on earnings and highest educational level is available when the respondents are 40 and 45 years of age, respectively. Thus, another advantage of these two data sets is that people have been in the labour force for a while, and most likely have finished school. Furthermore, the high reliability of highest educational level in the data sets should also be pointed out.

The paper is organized as follows. The nature of the problem is presented in section II. Both theoretical and empirical aspects of the ability–schooling–earnings relationship are discussed. Section III describes the data employed. The empirical findings are presented in section IV. Sensitivity analyses are given in section V. Section VI, finally, summarizes and concludes.

II. The nature of the problem

To illustrate alternative sources of bias in the schooling coefficient of an earnings equation, I introduce a simple model where ability affects both the returns and the costs of education. Similar theoretical frameworks are found in Ashenfelter and Rouse (1996), Blackburn and Neumark (1995) and Card (1995).

2. Gustafsson (1990) uses a data set which basically originates from the same data source, but without the same information on highest educational level and earnings.

A. The model

Assume that the individuals seek to maximize utility which, to keep things as simple as possible, is a function of earnings (y) and schooling (S)

$$U(y(S),S) = \ln y(S) - g(S) . \tag{1}$$

The utility-maximization problem requires that the optimal level of schooling satisfies the first order condition, where marginal benefit $\left(\frac{y'(S)}{y(S)}\right)$ equals marginal cost $(g'(S))$ of education. I assume that the marginal benefit of education (MB) for each individual (i) is related to both ability (A) and years of schooling (S)

$$MB_i = b + kA_i - mS_i. \tag{2}$$

As a consequence of higher productivity, ability and the marginal benefit of education are assumed to be positively related. Furthermore, the marginal benefit is assumed to decrease with years of schooling, because individual marginal productivity decreases with years of schooling.

Further, let the marginal cost of education (MC) be

$$MC_i = d + pA_i + rS_i. \tag{3}$$

Since individuals with higher ability are likely to have higher opportunity costs (foregone earnings) of attending school, but also easier access to scholarships and subsidized loans etc., the effect of ability on the marginal cost of education is ambiguous. The marginal cost is assumed to increase with schooling, i.e. the financing cost rises with years of schooling.

Equating marginal benefit and marginal cost, the optimal individual level of schooling (S^*) becomes

$$S_i^* = \frac{b - d + (k+p)A_i}{m+r}. \tag{4}$$

Thus, the influence of ability on schooling depends on the sign of $(k+p)$.

By integrating the marginal benefit of education (2) over years of schooling, we obtain the log earnings of individual (i)

$$\ln y_i = \mathbf{x}_i'\mathbf{a} + bS_i + kA_iS_i - \frac{m}{2}S_i^2, \tag{5}$$

where ability, family background and other earnings-generating factors constitute the vector $\mathbf{x}_i = (A_i, z_i)'$, and represent the values of the initial stock of human capital and post school investments etc.

According to this model, ability contributes both to a variation in earnings at all levels of schooling and to a variation in the returns to schooling. This contrasts with the majority of previous empirical specifications, which exclude the interaction term between ability and schooling, i.e the marginal benefit of education is independent of the level of ability.[3] However, the specification of the marginal benefit function in Blackburn and Neumark (1995), in which utility is defined over earnings in place of log earnings, leads to a specification for the log earnings without an interaction term between ability and schooling. Instead, ability together with the logarithm of schooling entered linearly in the log earnings equation.

B. Estimation

There are a number of important econometric issues involved in the estimation of the earnings function. These issues are not only restricted to the problem of ability as an omitted variable, but also include errors in variables.

Applying OLS to the earnings function without including ability leads to biased estimates of schooling. When schooling is perfectly measured and ability omitted from the equation, the direction of the bias is determined by the covariance between the two variables. When the optimal level of schooling is determined by equation (4), the direction of the bias is ambiguous. Further, if more regressors are added to the earnings equation it would be insufficient to determine the direction of the covariance between schooling and ability to find out the direction of the bias in the schooling coefficient. Thus, the return to schooling is not necessarily biased upwards.

The situation is even more complicated if schooling is measured with errors. Then the coefficient on schooling would be biased toward zero, and the biases could be rather severe. Moreover, by adding more variables (ability) that affect income mainly via schooling, makes the errors of measurement even worse. As was pointed out by

3. Blackburn and Neumark (1993), Murnane et al. (1995), however, also present estimates for the specification with an interaction between ability and schooling.

Griliches (1977) *"we may kill the patient in our attempts to cure what may have been a rather minor disease originally"*. Thus, the importance of good measures of schooling is crucial in the attempt to control for omitted ability bias.

Despite perfectly measured schooling, correction for ability may still not generate consistent estimates of the return to education. The theoretical specification (5) includes ability, for which the scales of measurement do not exist or at least is hard to measure. Instead, true ability is a latent variable with, for example, intelligence tests and achievement tests serving as indicators of ability. Unfortunately, there may be a discrepancy between the latent variable that is rewarded in the labour market and these indicators. The use of error ridden indicators imply that the OLS estimator is no longer unbiased or consistent.

The instrumental variable (IV) procedure, however, uses the method of moments as an estimation principle to generate a consistent, but not necessarily an asymptotically efficient estimator, in a situation where a regressor is contemporaneously correlated with the error. A legitimate instrument that fulfils the restrictions of being both contemporaneously uncorrelated with the measurement error of ability and correlated with the regressor for which it is to serve as an instrument, would be other measures of ability, see Griliches (1977), Blackburn and Neumark (1995).

In this study some of the available measures of ability are used as error ridden indicators, whereas other measures serve as instruments. In the estimations, I allow included regressors of ablity with accompanying instrumental variables to alternate between the available measures. Furthermore, the existence of measurement errors is tested with the Hausman specification test.

III. Data

This study is based on two nationally representative samples of pupils born in 1948 and 1953. These samples are the first and second age cohorts of the Individual Statistics (IS) project at the Department of Education and Educational Research, University of Gothenburg.[4] The first collection of data started in the spring of 1961 and included all people born in Sweden on the 5th, 15th and 25th of any month in 1948. The second

4. see Svensson (1971) and Härnqvist and Svensson (1973) for a more detailed description of the data sets and the purposes of the IS-project.

collection of data, which was a replication of the procedure described above, started in the spring of 1966 and included pupils born in 1953. This implies that the respondents were between 12 and 13 years of age at the time of the surveys and the majority were in the sixth form of the compulsory school system. The total number of individuals born on these specific days was 12,166 in 1948 and 10,723 in 1953. Due to limited resources, the drop-out rate is higher for the 1953 cohort. The samples cover 11,950 and 9,927 individuals, respectively.

The types of schools attended by the pupils varied at the time of the surveys. The Swedish parliament decided to extend compulsory schooling to nine years in 1950. A three-or-four-year junior secondary school (*realskola*) was established up to the sixth form of the elementary school (*folkskolan*). Furthermore, an experimental comprehensive school (*enhetsskola/försöksskola*) was to replace the old elementary school and the junior secondary school in some selected municipalities. In 1961, 40 per cent of the pupils were enrolled in the experimental comprehensive school.

After a short period of time, the experimental comprehensive school was replaced in 1962 by the present 9-year compulsory comprehensive school (*grundskolan*). This new comprehensive school gradually replaced the old school system in all municipalities and in 1966, more than 80 per cent of the pupils were enrolled in the new comprehensive school. In spite of the fact that there were differences in the type of compulsory school at the time of the surveys, the environment was more or less identical up through the sixth form. Furthermore, school start was at seven years of age in all the systems.

Three main categories of data were collected in the IS-project. The first category comprises information from school records such as type of school, form, school marks and scores on national standardized school tests (*standardprov*) in the sixth form. The school tests (henceforth called achievement tests) covered ***Reading***, ***Writing***, ***English*** and ***Mathematics***. In the new comprehensive school, however, the achievement tests in ***Reading*** and ***Writing*** were merged into one; ***Swedish***. Another difference between the elementary/experimental comprehensive school and the new comprehensive school is the scale of marks. Marks in the elementary/experimental comprehensive school were awarded according to a seven-point letter scale and in the new comprehensive school according to a five-point number scale. Information on marks is available for the following subjects; ***Mathematics***, ***English*** and ***Swedish***.

The second category of data consists of information from replies to questionnaires about the respondents' attitudes to school and spare time interests, and also information on parents' attitude to higher education.

The third category of data consists of the scores from three intelligence tests designed especially for the IS-project. The intelligence tests represent the verbal, spatial and reasoning factors of intelligence. The tests are called *Opposite* (choosing the opposite of a given word from four choices), *Metal folding* (finding the three-dimensional object that can be made from a flat piece of metal from four alternatives) and *Number series* (completing a specific number series). The total test score is determined by the number of correctly answered items. The number of items in each test is 40 and the pupils have 10, 15 and 18 minutes, respectively, to complete the tests.

The advantage of using test scores and school marks collected before the pupils enter the seventh form, is the similarity in education-related background variables. Thus, in contrast to the majority of previous US studies age- and education-related ability differentials are of minor importance in these two data sets.[5] Neither are the intelligence tests truncated which could be the case with tests of which the main task, after all, are to screen individuals. Especially, the test scores on *Number Series* show a well-turned distribution. The other two intelligence tests are somewhat skewed to the left.

Information on respondents' highest level of education was obtained from the educational register (*Utbildningsregistret*). The data sets do not include information on years of schooling. Instead, educational level was classified according to the Standard Classification of Education in Sweden (*SUN*). I distinguish between seven categories, with the highest level of education completed in 1993 as the classifying principle. Consequently, I estimate the wage premiums for education and not the internal rate of return to education.

Category 1 (*ED1*) consists of individuals with less than nine years of education. The majority of these have elementary school (*folkskola*) as their highest educational level, but there are also some with an eight-year (experimental) comprehensive school

5. The influence of sex, education, family size and age on procured ability has been reported in several studies, e g Bränberg et al (1990), Hanushek (1992). Thus, test scores of intelligence tests administered at different ages may lead to different results. Hanushek also presents some evidence that teachers in the US vary dramatically in effectiveness, resulting in differences in performance between students from different classes. A positive relationship between school quality and return to schooling is also reported in Card and Krueger (1992).

(*grundskola*) as their highest educational level.[6] Category 2 (*ED2*) comprises individuals with nine or ten year of education, i. e. individuals with junior secondary school (*realskola*) or (experimental) comprehensive school as their highest educational level. Category 3 (*ED3*) consists of individuals who attended upper secondary school for up to 2 years (*kortare gymnasium*). Individuals with the same type of education as Category 3 but with more than 2 years (*längre gymnasium*) constitute Category 4 (*ED4*). Category 5 (*ED5*) is composed of individuals with post secondary schooling of up to 2 years (*kortare universitetsutbildning*), while individuals with more than 2 years' post secondary schooling (*längre universitetsutbildning*) constitute Category 6 (*ED6*). Finally, Category 7 (*ED7*) comprises individuals with a completed doctoral degree (*forskarexamen*).

In the previous section I have emphasized the importance of adopting high quality measured variables. The quality of the 1991 version of the register that I use, has been evaluated by Statistics Sweden (1997). The actual data set has been confronted with data from a so called "true register". The register referred to is a Swedish Labour Force Survey (AKU) sample created in connection to 1990 Census (FOB-90). This "true register" makes use of information from both sources. Any mis-matches in educational classification in these two sources was further investigated and corrected.

The proportion individuals correctly classified by educational level was 83 per cent. However, one might expect a higher reliability of educational level in the data sets that I use. The reasons are, first that the reported numbers on mis-classification also include missing observations in the educational register. Thus, the proportion correctly classified is higher in samples that only include individuals with available information on educational level. Second, since the respondents were enrolled in the Swedish school system in 1961 and 1966, respectively, the number of foreign educational degrees is small. Thus, both problems associated with classification and missing observations should be of minor importance. The proportion of correctly classified individuals by educational level born in Sweden is 85 per cent, in contrast to 69 per cent for those born outside Sweden.

Third, the figure of 83 per cent presumes that all information on educational levels in the true register really is correct. A more realistic assumption is that both the

6. Having consulted the local education authority, the pupil could leave the comprehensive school after eight years education for alternative education or for appropriate employment.

educational register and the true register suffer from measurement error, but of different magnitude. Therefore, the proportion correctly classified individuals by educational level most likely is higher than 83 per cent. Fourth, the individuals in my samples belong to the same age cohort and face more or less the same educational system. Thus, classification errors in educational levels due to changes in classification of different educations over time would be of minor importance. Fifth and most important, since new and better data have become available after 1991, the quality of the educational register improves continuously. Officers from Statistics Sweden report marked improvement of the whole register. These improvements do not only affect the flow, but also existing information in the educational register. Thus, the proportion correctly classified in my data sets should be far better than 83 per cent.

For information on labour earnings, I rely on data for 1993 (*ÅRSYS 93*) provided by Statistics Sweden. The data set consists of information about annual income from both employment and self-employment. The information originates from companies' returns to the tax authorities (*kontrolluppgifter*). The Department of Education and Educational Research at the University of Gothenburg commissioned Statistics Sweden to link and match the data sets. Since I do not have information on hourly wage, work experience or working-hours (full-time versus part-time), I have restricted my attention to men. I have also restricted the data sets to individuals who report positive earnings from work as employed, with the total reported earnings from work and from self employment amounted to at least SEK 84,000.

Descriptive statistics for the variables are presented in Table 1. Test scores on the intelligence tests are not available for 12.1 per cent of those in the 1948 cohort and for 5.9 per cent of those in the 1953 cohort, mainly a consequence of absence from school on the days of testing. Test scores on the achievement tests are available for 71.7 per cent and 81.8 per cent, respectively. The non-response group includes pupils who were either not in the sixth form or who were absent from school on the day of testing. Finally, school marks in all subjects are available for 94.1 per cent and 96.3 per cent, respectively. The non-response group consists mainly of individuals not in the sixth form. These restrictions and missing observations reduce the size of the samples to 3,364 individuals born in 1948 and 3,018 individuals born in 1953.

Individuals in the (experimental) comprehensive school seem to be more successful in both intelligence tests and achievement tests than those enrolled in the elementary school. Further, the test measuring the ability to complete a specific number

Table 1: Descriptive statistics. Standard deviations in parenthesis and min-max values in brackets.

Variables	1948 cohort		1953 cohort	
	elementary school (1961)	experimental comprehensive school (1961)	elementary school (1966)	comprehensive school (1966)
Intelligence tests				
Opposite Valid values: 0–40	23.19 (6.34) [6–39]	23.60 (6.30) [5–40]	23.50 (6.19) [2–38]	24.73 (6.09) [7–40]
Number Series Valid values: 0–40	20.74 (7.59) [0–39]	21.45 (7.58) [0–40]	20.38 (7.89) [1–39]	21.77 (7.75) [1–40]
Metal Folding Valid values: 0–40	22.20 (7.24) [3–39]	23.31 (7.25) [5–39]	22.22 (7.25) [3–37]	23.38 (7.30) [1–40]
Achievement tests				
Swedish Valid values: 0–103	–	–	–	55.27 (15.47) [9–99]
Reading Valid values: (a): 7–63, (b): 0–95	38.17 (7.24) [14.5–60][a]	38.48 (7.28) [15–59.5][a]	54.75 (15.40) [12–93][b]	–
Writing Valid values: (a): 13–91, (b): 0–109	51.29 (9.93) [21.5–86.5][a]	51.52 (10.03) [16.5–82][a]	61.49 (15.04) [10–106][b]	–
English Valid values: (a): 0–10, (b): 0–98	5.28 (1.75) [1–9.6][a]	5.54 (1.78) [1.2–9.7][a]	52.81 (19.52) [8–98][b]	55.15 (19.89) [7–98][b]
Mathematics Valid values: (a): 10–70, (b): 0–70	42.56 (9.47) [18–69][a]	42.52 (9.70) [15.5–68.5][a]	35.68 (13.16) [7–70][b]	37.95 (13.56) [4–68][b]
Marks				
Swedish Valid values: (a): 0–6, (b): 1–5	3.43 (0.88) [1–6][a]	3.36 (0.89) [1–5][a]	3.37 (0.86) [1–6][a]	3.08 (0.95) [1–5][b]
English Valid values: (a): 0–6, (b): 1–5	2.94 (1.01) [0–6][a]	2.93 (1.03) [0–5][a]	3.04 (0.99) [1–5][a]	3.10 (1.02) [1–5][b]
Mathematics Valid values: (a): 0–6, (b): 1–5	3.29 (1.07) [0–6][a]	3.21 (1.09) [0–6][a]	3.30 (1.09) [1–6][a]	3.29 (1.04) [1–5][b]
Earnings (1993)				
Earnings in thousands' (SEK) Valid values: 84 –	237.29 (111.46) [84.20–1487.80]	249.18 (121.04) [86.30–1838.10]	209.92 (88.97) [84.30–1007.40]	227.68 (97.98) [84.10–1314.60]
Log Earnings Valid values: 4.09 –	5.39 (0.38) [4.43–7.31]	5.44 (0.39) [4.46–7.52]	5.28 (0.34) [4.43–6.92]	5.36 (0.36) [4.43–7.18]
Number of observations	*2194*	*1170*	*547*	*2471*

Table 2: Descriptive statistics of the sum of the three intelligence tests divided according to the respondents' highest educational level in 1993. Standard deviations in parenthesis.

Highest educational level in 1993	1948 cohort: elementary school (1961)	1948 cohort: experimental comprehensive school (1961)	1953 cohort: elementary school (1966)	1953 cohort: comprehensive school (1966)
Less than 9 years (ED1)	55.42 (14.08) N=382	60.16 (15.64) N=19	55.86 (14.53) N=88	51.40 (13.77) N=10
9 years (ED2)	62.22 (15.43) N=167	57.24 (14.59) N=160	65.15 (14.12) N=72	60.26 (15.20) N=475
Upper secondary schooling up to 2 years (ED3)	61.08 (14.70) N=514	62.53 (15.29) N=335	61.27 (15.74) N=164	64.42 (14.75) N=711
Upper secondary schooling more than 2 years (ED4)	67.84 (14.78) N=437	70.61 (14.50) N=240	69.68 (17.70) N=66	72.00 (14.43) N=367
Post secondary education up to 2 years (ED5)	70.32 (15.75) N=240	71.59 (13.58) N=140	73.64 (14.36) N=86	76.04 (14.20) N=423
Post secondary education more than 2 years (ED6)	78.20 (14.03) N=433	78.16 (15.46) N=249	78.59 (15.66) N=70	80.27 (14.57) N=451
Doctoral degree (ED7)	83.24 (11.63) N=21	84.93 (12.45) N=27	73.0 (-) N=1	86.09 (11.61) N=34
Total	66.13 (16.57) N=2194	68.35 (16.60) N=1170	66.11 (16.99) N=547	69.88 (16.48) N=2471

series is characterized by lower means and a higher standard deviation than the other tests. Moreover, even though the test scores are higher for those in the experimental comprehensive school (column one) than for those in the old school system (column two), those in the elementary school had higher marks. Unfortunately, for the 1953 cohort, the two different scales make it impossible to compare the marks of those in the old elementary school and those in the new comprehensive school.

Descriptive statistics of the sum of the three intelligence test scores divided according to the respondent's highest educational level in 1993 are reported in Table 2. The highest educational level completed in 1993 is closely related to the test results, which supports the hypothesis that ability bias might be a serious problem. As reported in Table 1, the test scores are higher for those in the (experimental) comprehensive school, with one exception. The fact that the test score outcomes are in favour for the junior secondary school (row 2) could be due to problems associated with selectivity. The opportunity to go on to further education influences pupils' decisions to attend the junior secondary school, which implies a positive selection. Furthermore, many theoretically oriented pupils dropped-out from the senior level of the comprehensive school and switched to the junior secondary school. However, those who did attend the junior secondary school with the aim of further education, but refrained from further education, may imply a negative selection which will reduce the positive sorting effect.

It is not only the test score outcomes that differ between the different school systems; the proportion of those with a university degree in 1993 differs aswell. A larger proportion of the individuals enrolled in the (experimental) comprehensive school in 1961 and 1966, respectively, are to be found in *ED5* to *ED7* in 1993, which can explain the variation in earnings in Table 1. However, the increase in test results and the higher educational level completed may not necessarily be caused by the reform of the school system. One explanation is that the comprehensive school was the dominant type of school in university districts.

The empirical findings in Table 2 and the conclusions are unchanged when each educational level also is divided into one general and one technical programme. However, whereas the test scores for those with a longer technical post secondary degree (ED6) are substantially higher than for those with a longer general post secondary degree, the opposite is true for those with a longer upper secondary degree, see Table A1 in the appendix.

Tables 3 and 4 display the correlations between the intelligence tests, achievement tests, marks and earnings. As expected, the test that measures the ability to find the opposite of a given word is strongly correlated with achievement tests and marks in *Swedish* (*Reading* and *Writing*) and *English*, respectively. Similarly, the ability to complete a specific number series is strongly correlated with achievement test and mark in *mathematics*. The correlations between the intelligence test representing the spatial factor of intelligence (*Metal Folding*), different achievement tests and marks, however, are rather weak. Furthermore, comparable correlations between the elementary school and the (experimental) comprehensive school are almost the same.

The correlations between the measures of ability and earnings are higher for those in the elementary school than for those in the (experimental) comprehensive school. In addition, the figures are higher for those born in 1948 than for those born in 1953. However, the correlations between the intelligence tests and earnings are influenced by differences in later educational level, which aggravates the interpretation.

Table 3: Correlations between the test scores, marks and earnings divided according to the two compulsory school systems (1961). The correlations for individuals in the elementary school are reported below the main diagonal, whereas the correlations for individuals enrolled in the experimental comprehensive school are reported above the main diagonal.

Elementary school (N=2194) \ Experimental comprehensive school (N=1170)	*Intelligence tests*			*Achievement tests*				*Marks*			*Earnings*
	Opposite	Number Series	Metal Folding	Reading	Writing	English	Maths	Swedish	English	Maths	
Intelligence tests											
Opposite	**1.0**	0.49**	0.38**	0.75**	0.72**	0.64**	0.56**	0.65**	0.57**	0.53**	0.23**
Number Series	0.47**	**1.0**	0.40**	0.54**	0.55**	0.54**	0.69**	0.51**	0.52**	0.62**	0.27**
Metal Folding	0.36**	0.42**	**1.0**	0.38**	0.35**	0.30**	0.44**	0.29**	0.25**	0.40**	0.16**
Achievement tests											
Reading	0.73**	0.51**	0.34**	**1.0**	0.84**	0.73**	0.69**	0.80**	0.68**	0.64**	0.26**
Writing	0.71**	0.52**	0.33**	0.83**	**1.0**	0.78**	0.66**	0.80**	0.73**	0.61**	0.23**
English	0.65**	0.51**	0.32**	0.72**	0.78**	**1.0**	0.67**	0.73**	0.84**	0.63**	0.24**
Maths	0.55**	0.69**	0.43**	0.67**	0.65**	0.66**	**1.0**	0.62**	0.64**	0.86**	0.32**
Marks											
Swedish	0.63**	0.49**	0.28**	0.79**	0.79**	0.71**	0.62**	**1.0**	0.76**	0.64**	0.25**
English	0.58**	0.48**	0.26**	0.65**	0.71**	0.82**	0.61**	0.73**	**1.0**	0.67**	0.26**
Maths	0.50**	0.64**	0.36**	0.60**	0.59**	0.61**	0.84**	0.64**	0.65**	**1.0**	0.32**
Earnings	0.28**	0.25**	0.18**	0.31**	0.27**	0.30**	0.32**	0.25**	0.28**	0.30**	**1.0**

** Statistically significant from zero at 1 per cent level.

Table 4: Correlations between the test scores, marks and earnings divided according to the two compulsory school systems (1966). The correlations for individuals in the elementary school are reported below the main diagonal, whereas the correlations for individuals enrolled in the comprehensive school are reported above the main diagonal.

Comprehensive school (N=2471) / Elementary school (N=547)	*Intelligence tests*			*Achievement tests*					*Marks*			*Earnings*
	Opposite	Number Series	Metal Folding	Swedish	Reading	Writing	English	Maths	Swedish	English	Maths	
Intelligence tests												
Opposite	**1.0**	0.50**	0.37**	0.73**	-	-	0.66**	0.58**	0.60**	0.60**	0.54**	0.23**
Number Series	0.48**	**1.0**	0.37**	0.58**	-	-	0.55**	0.70**	0.53**	0.52**	0.66**	0.21**
Metal Folding	0.38**	0.48**	**1.0**	0.35**	-	-	0.31**	0.41**	0.28**	0.27**	0.38**	0.12**
Achievement tests												
Swedish	-	-	-	**1.0**	-	-	0.79**	0.69**	0.81**	0.73**	0.65**	0.24**
Reading	0.72**	0.50**	0.33**	-	**1.0**	-	-	-	-	-	-	-
Writing	0.68**	0.55**	0.37**	-	0.82**	**1.0**	-	-	-	-	-	-
English	0.63**	0.51**	0.29**	-	0.73**	0.76**	**1.0**	0.68**	0.73**	0.85**	0.63**	0.25**
Maths	0.54**	0.68**	0.45**	-	0.59**	0.65**	0.55**	**1.0**	0.67**	0.64**	0.87**	0.28**
Marks												
Swedish	0.65**	0.50**	0.33**	-	0.78**	0.77**	0.69**	0.56**	**1.0**	0.78**	0.69**	0.23**
English	0.59**	0.50**	0.28**	-	0.71**	0.73**	0.84**	0.56**	0.74**	**1.0**	0.65**	0.24**
Maths	0.53**	0.64**	0.40**	-	0.61**	0.64**	0.55**	0.85**	0.64**	0.62**	**1.0**	0.27**
Earnings	0.21**	0.24**	0.12**	-	0.19**	0.21**	0.20**	0.27**	0.20**	0.20**	0.31**	**1.0**

** Statistically significant from zero at 1 per cent level.

IV. Empirical Findings

Tables 5 and 6 report the estimated effect on earnings of different educational levels for the 1948 cohort and the 1953 cohort, respectively, controlling for alternative sets of variables. The results presented in column one replicate the positive relationship between educational length and earnings found in other Swedish studies.[7] For the two lowest educational levels completed, I distinguish between those enrolled in the old school system (extension old) and those enrolled in the new school system (extension new). The reference group consists of individuals with the comprehensive school as their highest educational level completed (ED2new).

More than two years of upper secondary schooling (ED4) and higher education enter significantly in the equation. Furthermore, the educational wage premiums are substantially lower for the 1953 cohort compared to the 1948 cohort. For instance, the wage premium for more than 2 years of post secondary schooling (ED6) is 41 per cent for those born in 1948 but 36 per cent for the 1953 cohort. This fall in the estimated educational wage premiums is not necessarily an effect of changes in the wage premiums for education alone; it can also arise if an improper grouping scheme is used. One might expect a considerable within group variation in the wage premium for education. For instance, the wage premium for a technical programme may differ from that of a general programme. Thus, changes in the proportion of students in the technical programme can influence the estimated wage premiums for education.[8]

Also, it is notable that the change in the school system seems to have affected earnings negatively. The wage premium for junior secondary school (ED2old) is higher than that of comprehensive school, although it is not statistically significant. It does not emerge from the tables whether the pattern also persists for further higher education.

7. The interpretation of the coefficients on educational level, however, is somewhat different. The parameters on educational level consist of two parts: one direct effect of schooling and one indirect effect due to differences in work experience, i.e.

$$\ln y = \beta_1 + \beta_2 Sch + \beta_3 (Age - Sch - 6) + \varepsilon = \beta_1 + \beta_3 (Age - 6) + (\beta_2 - \beta_3) Sch + \varepsilon$$

8. The estimates in column one of Tables A2 and A3 in the appendix, suggest that the magnitude of the educational wage premium for each educational level differs between the two programmes. Furthermore, the proportion of students of technology in the later age cohort has increased in the higher educational groups, and decreased in the lower educational groups, see Table A1 in the appendix. However, dividing each educational level into one technical and one general programme does not alter previous results. The decline in the wage premium for education between the two age cohorts remains.

Table 5: OLS Log Earnings Equation Estimates (1948 Cohort). Standard errors are reported in parenthesis.

Independent variables	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)
ED1old	-0.031 (0.033)	-0.024 (0.033)	-0.022 (0.032)	-0.019 (0.032)	-0.023 (0.032)	-0.024 (0.032)	-0.020 (0.032)
ED1new	-0.040 (0.082)	-0.037 (0.082)	-0.051 (0.081)	-0.046 (0.081)	-0.044 (0.081)	-0.043 (0.081)	-0.054 (0.081)
ED2old	0.072 (0.038)	0.076* (0.038)	0.062 (0.037)	0.048 (0.037)	0.045 (0.038)	0.044 (0.038)	0.048 (0.038)
ED3	0.050 (0.029)	0.049 (0.029)	0.036 (0.029)	0.031 (0.029)	0.029 (0.029)	0.028 (0.029)	0.028 (0.029)
ED4	0.185** (0.030)	0.177** (0.030)	0.145** (0.030)	0.134** (0.030)	0.137** (0.030)	0.133** (0.030)	0.131** (0.030)
ED5	0.232** (0.032)	0.228** (0.032)	0.192** (0.032)	0.180** (0.032)	0.183** (0.033)	0.181** (0.033)	0.175** (0.033)
ED6	0.412** (0.030)	0.397** (0.030)	0.340** (0.031)	0.318** (0.032)	0.328** (0.032)	0.325** (0.032)	0.313** (0.032)
ED7	0.643** (0.056)	0.624** (0.057)	0.550** (0.058)	0.526** (0.058)	0.541** (0.058)	0.542** (0.058)	0.514** (0.058)
Controls of							
County of residence	yes	yes	yes	yes	yes	yes	yes
Family background	no	yes	yes	yes	yes	yes	yes
Intelligence test scores	no	no	yes	no	no	no	yes
Achievement test scores	no	no	no	yes	no	no	yes
Marks							
dummies	no	no	no	no	yes	no	yes
cont. var.	no	no	no	no	no	yes	no
R^2-adj	*0.215*	*0.222*	*0.234*	*0.241*	*0.238*	*0.237*	*0.244*

Note: The sample size is 3364.
_old: The old school system.
_new: The new school system.
* Statistically significant from zero at 5 per cent level.
** Statistically significant from zero at 1 per cent level.

However, dividing each educational level on the basis of type of school attended by the pupils in 1961, does not give a clear picture (results not reported).

The specification in column two of Tables 5 and 6 attempts to control for the effect of unobserved ability on earnings by the use of family background variables. I have used information on parental education and occupation. However, including these variables does not considerably affect the estimated premiums for education. The magnitude of the wage premiums that are significantly different from zero in the equations are reduced by 2 to 8 per cent. For example, the coefficient on ED4 in Table 6 decreases from 0.121 to 0.111.[9]

9. The coefficients on the family background variables are presented in Table A4 in the appendix.

Table 6: OLS Log Earnings Equation Estimates (1953 Cohort). Standard errors are reported in parenthesis.

Independent variables	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)
ED1old	-0.038 (0.038)	-0.034 (0.038)	-0.024 (0.038)	0.001 (0.041)	-0.016 (0.044)	0.013 (0.041)	0.004 (0.045)
ED1new	0.128 (0.103)	0.128 (0.103)	0.151 (0.103)	0.146 (0.102)	0.148 (0.103)	0.144 (0.102)	0.155 (0.103)
ED2old	0.050 (0.041)	0.054 (0.041)	0.044 (0.041)	0.064 (0.044)	0.062 (0.044)	0.072 (0.044)	0.082 (0.046)
ED3	0.034 (0.019)	0.034 (0.018)	0.028 (0.018)	0.029 (0.019)	0.027 (0.019)	0.031 (0.019)	0.029 (0.019)
ED4	0.121** (0.022)	0.111** (0.022)	0.088** (0.022)	0.084** (0.022)	0.082** (0.023)	0.087** (0.022)	0.082** (0.023)
ED5	0.210** (0.021)	0.203** (0.021)	0.173** (0.022)	0.168** (0.022)	0.167** (0.022)	0.173** (0.022)	0.165** (0.023)
ED6	0.355** (0.021)	0.335** (0.022)	0.297** (0.023)	0.286** (0.023)	0.288** (0.024)	0.290** (0.024)	0.287** (0.024)
ED7	0.485** (0.057)	0.454** (0.058)	0.407** (0.059)	0.391** (0.059)	0.401** (0.059)	0.396** (0.059)	0.397** (0.059)
Controls of							
County of residence	yes	yes	yes	yes	yes	yes	yes
Family back-ground	no	yes	yes	yes	yes	yes	yes
Intelligence test scores	no	no	yes	no	no	no	yes
Achievement test scores	no	no	no	yes	no	no	yes
Marks							
dummies	no	no	no	no	yes	no	yes
cont. var.	no	no	no	no	no	yes	no
R^2-*adj*	*0.177*	*0.187*	*0.194*	*0.199*	*0.196*	*0.198*	*0.198*

Note: The sample size is 3018.
_old: The old school system.
_new: The new school system.
* Statistically significant from zero at 5 per cent level.
** Statistically significant from zero at 1 per cent level.

The addition of explicit ability controls (columns three to seven) does more to reduce the wage premium for education than do the family background variables. The figures indicate an upward bias as large as 25 per cent in some coefficients on education in the specifications that ignore explicit measures of ability. Moreover, the estimates are not very sensitive to alternative definitions of ability.[10] Furthermore, including measures of ability does not eliminate the low effect of family background variables on the wage premium for education. In conformity with previous US studies, the estimates suggest that measures of ability associated with mathematics are important in wage

10.The coefficients on the controls for ability, which correspond to each specification, are reported in Tables A5 and A6 in the appendix. For purposes of comparability, test scores and marks have been standardized.

determination. Furthermore, it does not matter whether marks are represented by dummies or by continuous variables. The estimated wage premiums are similar for the two specifications in columns five and six of Tables 5 and 6. Furthermore, the adjusted R^2s are only slightly higher for the specification in column six.

Thus, not considering omitted ability bias could be of great consequence for inference about the magnitude of the wage premium for education. Furthermore, controlling for ability reduces the differences in the wage premium for the higher educational levels and increases the differences in the premium for the lower levels, when the two age cohorts are compared.[11] These results may be explained by changes in the *selection* into higher education and/or by a reduction in the *return* to ability. However, as indicated in Table 2, there is little evidence that there was an increasing proportion of individuals of high ability in the higher educational groups for the 1953 cohort compared to the 1948 cohort. The coefficients on the ability controls reported in Tables A5 and A6 in the appendix, however, suggest that the wage premium for ability in general is higher for the 1948 cohort compared to the 1953 cohort.

So far, the wage premiums have been held constant. Let us suppose, in line with the theoretical specification (5), that there is an interaction between education and ability in wage equations. The first column of Table 7 and 8, respectively, presents the coefficients on educational levels for the specification without controls of ability. Columns two and five present wage premium estimates of the specifications with test scores on the *Number Series* and the achievement test in *Mathematics* as regressors.

The predicted mean impacts of education on log wages, calculated from coefficients in the specification including an interaction term between test scores on the *Number Series* and the highest completed educational level, are reported in columns three and four. The evaluation point distinguishes the two columns. In columns six and seven, I repeat the estimations of columns threee and four, but using the scores of the achievement test in mathematics in place of the intelligence test scores.

The results reported in columns three and four in Table 7 suggest that the specification with the intelligence test interacted with educational level leads to predicted wage premiums somewhat smaller than comparable premiums presented in column two. Analogous estimates in Table 8, however, do not show the same

11. Similar results are also obtained when each educational level is divided into one general and one technical programme, see Tables A2 and A3 in the appendix.

Table 7: The Predicted Wage Premium of Education (1948 Cohort).

Mean impact	(I)	(II)	(III)[a]	(IV)[b]	(V)	(VI)[a]	(VII)[b]
ED1old	-0.024	-0.024	-0.045	-0.041	-0.022	0.099	0.104
ED1new	-0.037	-0.050	-0.066	-0.064	-0.040	0.057	0.061
ED2old	0.076	0.066	0.045	0.049	0.048	0.339	0.341
ED3	0.049	0.038	0.026	0.027	0.031	0.111	0.112
ED4	0.177	0.150	0.138	0.137	0.134	0.172	0.172
ED5	0.228	0.201	0.195	0.194	0.178	0.375	0.373
ED6	0.397	0.352	0.323	0.323	0.317	0.297	0.296
ED7	0.624	0.566	0.554	0.550	0.526	0.199	0.207
Controls of							
County of residence	yes	yes	yes	yes	yes	yes	yes
Family background	yes	yes	yes	yes	yes	yes	yes
Ability	no	yes[1]	yes[1]	yes[1]	yes[2]	yes[2]	yes[2]
ED_ × Ability	no	no	yes[1]	yes[1]	no	yes[2]	yes[2]
R^2-*adj*	*0.222*	*0.233*	*0.234*		*0.240*	*0.244*	

Note: see Table 5.
(a): Evaluated at mean ability for all.
(b): Evaluated at mean ability for each educational level.
(1): Intelligence test (Number series)
(2): Achievement test (Mathematics)

Table 8: The Predicted Wage Premium of Education (1953 Cohort).

Mean Impact	(I)	(II)	(III)[a]	(IV)[b]	(V)	(VI)[a]	(VII)[b]
ED1old	-0.034	-0.025	-0.051	-0.047	-0.027	0.027	0.033
ED1new	0.128	0.149	0.076	0.087	0.146	0.224	0.241
ED2old	0.054	0.048	0.050	0.050	0.036	0.109	0.110
ED3	0.034	0.030	0.032	0.032	0.024	0.035	0.035
ED4	0.111	0.092	0.095	0.095	0.079	0.093	0.093
ED5	0.203	0.180	0.171	0.172	0.162	0.139	0.139
ED6	0.335	0.306	0.321	0.320	0.282	0.279	0.279
ED7	0.454	0.418	0.312	0.324	0.390	0.211	0.217
Controls of							
County of residence	yes	yes	yes	yes	yes	yes	yes
Family background	yes	yes	yes	yes	yes	yes	yes
Ability	no	yes[1]	yes[1]	yes[1]	yes[2]	yes[2]	yes[2]
ED_ × Ability	no	no	yes[1]	yes[1]	no	yes[2]	yes[2]
R^2-*adj*	*0.187*	*0.194*	*0.194*		*0.199*	*0.200*	

Note: see Table 6.
(a): Evaluated at mean for all.
(b): Evaluated at mean for each educational level.
(1): Intelligence test (Number series)
(2): Achievement test (Mathematics)

consistence. Furthermore, it does not matter whether the predicted impact of educational level on wages is evaluated at the mean ability level for all or at the mean ability level within each completed educational level.

The specification with the achievement test scores interacted with educational level, columns six and seven of Tables 7 and 8, generates estimates that differ considerably from the estimates in column five. Including an interaction term between ability and educational level in the wage equation gives rise to higher wage premiums for lower levels of education, and lower wage premiums for higher educational levels. As above, it does not matter whether the predicted impact of educational levels on wages is evaluated at the mean ability level for all or at the mean ability level within each educational level completed.

Due to higher expected productivity one might expect ability and the marginal benefit of education to be positively correlated, i.e. $k < 0$ in equation (2). However, a positive relationship is not supported by the empirical findings for the 1948 cohort presented in Table A7 in the appendix. The estimates indicate that the return to ability and the marginal benefit of education are negatively related. The coefficients on the interaction terms are generally negative, although not always statistically significant. Similar results are reported in Ashenfelter and Rouse (1996). They show that individuals from families with higher levels of ability receive a lower marginal benefit of education.

V. Sensitivity analyses

In the previous analyses, the implication of omitted ability variables on the estmated wage premium for education has been examined by including measures of ability in the earnings equation.The decision to participate in the labour force, however, may not be independent of ability and schooling. Thus, applying this procedure requires that ability affect the decision to participate in the labour force for different educational groups in a similar way as it affect the wage premiums, or at least that the order of precedence in labour force participation probabilities for different educational levels is unaffected. Otherwise, the magnitudes of the ability bias presented in previous section are disturbed by selectivity bias.

Let us put the individuals into two categories: *workers* and *non-workers* selected according to the income criteria (annual income higher than SEK 84,000). Descriptive statistics are presented in Table 9. Notice that 92 per cent of pupils born in

Table 9: Educational level and test scores for workers and non-workers. Standard deviations in parenthesis.

	1948 cohort		1953 cohort	
Variables	workers	non-workers	workers	non-workers
Educational level completed				
ED1old	0.114	0.107	0.031	0.036
ED1new	0.006	0.007	0.004	0.014
ED2old	0.050	0.055	0.024	0.031
ED2new	0.048	0.072	0.161	0.235
ED3	0.252	0.399	0.292	0.393
ED4	0.201	0.179	0.144	0.123
ED5	0.113	0.100	0.166	0.100
ED6	0.203	0.072	0.168	0.059
ED7	0.014	0.010	0.011	0.009
Intelligence tests				
Number Series	20.99 (7.59)	18.79 (7.55)	21.47 (7.77)	19.46 (7.83)
Metal Folding	22.59 (7.26)	20.97 (7.31)	23.16 (7.30)	22.43 (7.65)
Opposite	23.33 (6.33)	21.72 (6.18)	24.44 (6.12)	23.36 (6.32)
Number of observations	*3364*	*291*	*3111*	*643*

1948 participate in the labour force in 1993, while the proportion for the 1953 cohort is only 83 per cent. In line with previous results in Table 1, test scores are higher for the 1953 cohort. In addition, means of the three intelligence tests are higher for workers compared to *non-workers*. Furthermore, the educational length seems to affect the labour force participation. More than 80 per cent of those in the *non-workers* group have at most upper secondary schooling more than two years (ED4) as highest educational level completed. This in contrast to slightly more than 65 per cent of those in the group of *workers*.

Table 10 reports marginal effects of the probit model. The results suggest that the educational level has a strong positive effect on the labour force participation. In line with previous results, including measures of ability in the probit model reduces the marginal effects of education on the participation probability in a similar way as before. Furthermore, only number series is significantly different from zero in the equations, which parallel the results reported in section IV, see Table A8 in the appendix. Thus, the way data are generated would not affect the implications of omitted ability variables on the estimated wage premiums.

Table 10. Probit analysis. Marginal effects.

	1948 cohort[1]		1953 cohort [2]	
Independent variables	(I)	(II)	(I)	(II)
ED1old	0.039	0.037	0.050	0.049
ED1new	0.024	0.022	-0.259	-0.257
ED2old	0.031	0.0252	0.035	0.032
ED3	0.001	-0.004	0.017	0.016
ED4	0.041	0.032	0.069	0.065
ED5	0.041	0.031	0.090	0.087
ED6	0.047	0.044	0.095	0.093
ED7	0.046	0.031	0.073	0.064
Controls of				
County of residence	yes	yes		
Intelligence tests	no	yes	no	yes

Note:
(1): The sample size is 3655.
(2): The sample size is 3754.
_old: The old school system.
_new: The new school system.

A second source of selectivity bias exists if missing data in the measures of ability are systematically related to ability. Since, the intelligence/achievement tests were taken on a voluntary basis, in the sense of tolerating individuals absent from school on the days of testing, exclusion of non-test takers in the samples would also imply a selection of individuals. It appears that individuals absent from school on the days of testing, for at least one test, had in general lower school marks than the test takers. Moreover, the wage premiums for schooling are higher in samples that also include individuals with missing observations on test scores, see Table 11. Thus, omitting these individuals might affect the magnitude of the omitted ability bias.

However, the estimates reported in Tables 12 and 13 do not suggest that this is a problem. Through information on school marks in mathematics it has been possible to estimate the omitted ability bias for the two groups. The picture that appears is that the reduction in wage premiums for the four highest educational levels is only somewhat higher in samples that also includes individuals with no information on scores from the intelligence tests.

Another potential source of error in the estimated wage premiums for schooling is that erroneous measures of ability would contaminate the ordinary least squares estimates. I follow earlier research and use instrumental variables to generate consistent estimates. In the estimations, I allow included regressors with accompanying

Table 11: OLS Log Earnings Equation Estimates. Standard errors are reported in parenthesis.

Independent variables	1948 cohort		1953 cohort	
	N=3364	N=4239[1]	N=3018	N=3634[1]
ED1old	-0.032 (0.033)	-0.008 (0.027)	-0.038 (0.038)	-0.026 (0.034)
ED1new	-0.040 (0.082)	-0.036 (0.061)	0.128 (0.103)	0.038 (0.062)
ED2old	0.072* (0.038)	0.067* (0.032)	0.050 (0.041)	0.058 (0.038)
ED3	0.050 (0.029)	0.070** (0.024)	0.034 (0.019)	0.046** (0.016)
ED4	0.185** (0.030)	0.190** (0.025)	0.121** (0.022)	0.142** (0.019)
ED5	0.232** (0.032)	0.246** (0.027)	0.210** (0.021)	0.232** (0.019)
ED6	0.412** (0.030)	0.431** (0.025)	0.355** (0.021)	0.383** (0.018)
ED7	0.643** (0.056)	0.661** (0.048)	0.485** (0.057)	0.488** (0.052)
Controls of				
County of residence	yes	yes	yes	yes
R^2-adj	*0.215*	*0.216*	*0.177*	*0.197*

Notes:
(1): includes individuals with missing observations on test scores.
_old: The old school system.
_new: The new school system.
* Statistically significant from zero at 5 per cent level.
** Statistically significant from zero at 1 per cent level.

instrumental variables to alternate between available measures of ability. The instrumental variables estimates are displayed in Table 14. In conformity with previous results by Griliches (1977), Blackburn and Neumark (1995), instrumenting for ability further reduces the wage premium for education. The estimates, however, are in the main only slightly lower than corresponding estimates reported in Tables 5 and 6, with the exception of column 6. Moreover, the performance of Hausman specification tests does not indicate that measurement error in the measures of ability is a problem, i.e. it is not necessary to instrument for ability.

Finally, the earnings function that relates the logarithm of hourly earnings to years of schooling (and years of experience and its square) dominates previous studies. In this perspective, I use two unconventional measures of earnings and schooling: annual earnings and educational level completed, respectively. To examine how sensitive the estimates are for different measures of earnings and schooling, I employ data from the Swedish Level of Living Survey (SLLS) from 1991, see Fritzell and Lundberg (1994) for details. This is the most widely used Swedish data set for wage

Table 12: OLS Log Earnings Equation Estimates (1948 Cohort). Standard errors are reported in parenthesis.

Independent variables	N=3364		N=4239[1]	
ED1old	-0.032	-0.033	-0.008	-0.009
	(0.033)	(0.032)	(0.027)	(0.026)
ED1new	-0.040	-0.045	-0.036	-0.030
	(0.082)	(0.081)	(0.061)	(0.061)
ED2old	0.072*	0.038	0.067*	0.038
	(0.038)	(0.038)	(0.032)	(0.031)
ED3	0.050	0.028	0.070**	0.051*
	(0.029)	(0.029)	(0.024)	(0.024)
ED4	0.185**	0.139**	0.190**	0.146**
	(0.030)	(0.030)	(0.025)	(0.025)
ED5	0.232**	0.182**	0.246**	0.200**
	(0.032)	(0.032)	(0.027)	(0.027)
ED6	0.412**	0.333**	0.431**	0.354**
	(0.030)	(0.031)	(0.025)	(0.026)
ED7	0.643**	0.550**	0.661**	0.567**
	(0.056)	(0.056)	(0.048)	(0.049)
County of residence	yes	yes	yes	yes
Marks (Mathematics)	no	yes	no	yes
R^2-adj	*0.215*	*0.230*	*0.216*	*0.232*

(1): includes individuals with missing observations on test scores.
_old: The old school system.
_new: The new school system.
* Statistically significant from zero at 5 per cent level.
** Statistically significant from zero at 1 per cent level.

Table 13: OLS Log Earnings Equation Estimates (1953 Cohort). Standard errors are reported in parenthesis.

Independent variables	N=3018		N=3634[1]	
ED1old	-0.038	-0.029	-0.026	-0.019
	(0.038)	(0.038)	(0.034)	(0.034)
ED1new	0.128	0.144	0.038	0.055
	(0.103)	(0.103)	(0.063)	(0.062)
ED2old	0.050	0.028	0.058	0.039
	(0.041)	(0.041)	(0.038)	(0.038)
ED3	0.034	0.024	0.046**	0.036*
	(0.019)	(0.018)	(0.016)	(0.016)
ED4	0.121**	0.089**	0.142**	0.113**
	(0.022)	(0.022)	(0.019)	(0.020)
ED5	0.210**	0.170**	0.232**	0.197**
	(0.021)	(0.022)	(0.019)	(0.019)
ED6	0.355**	0.301**	0.383**	0.334**
	(0.021)	(0.022)	(0.0182)	(0.020)
ED7	0.485**	0.417**	0.488**	0.427**
	(0.057)	(0.058)	(0.052)	(0.053)
County of residence	yes	yes	yes	yes
Marks (Mathematics)	no	yes	no	yes
R^2-adj	*0.177*	*0.187*	*0.197*	*0.206*

(1): includes individuals with missing observations on test scores.
_old: The old school system.
_new: The new school system.
* Statistically significant from zero at 5 per cent level.
** Statistically significant from zero at 1 per cent level.

Table 14: Instrumental Variable Log Earnings Equation Estimates. Standard errors are reported in parenthesis.

Independent variables	1948 cohort (N=3364)			1953 cohort (N=3018)		
	(I)	(II)	(III)	(I)	(II)	(III)
ED1old	-0.022 (0.032)	-0.023 (0.032)	-0.022 (0.032)	-0.019 (0.038)	-0.005 (0.041)	-0.005 (0.041)
ED1new	-0.040 (0.081)	-0.048 (0.081)	-0.042 (0.081)	0.146 (0.102)	0.146 (0.102)	0.125 (0.103)
ED2old	0.048 (0.037)	0.044 (0.037)	0.049 (0.037)	0.039 (0.041)	0.055 (0.043)	0.082 (0.044)
ED3	0.030 (0.029)	0.026 (0.029)	0.031 (0.029)	0.024 (0.018)	0.028 (0.019)	0.040* (0.019)
ED4	0.134** (0.030)	0.129** (0.030)	0.134** (0.030)	0.078** (0.022)	0.082** (0.022)	0.114** (0.022)
ED5	0.179** (0.033)	0.175** (0.033)	0.179** (0.032)	0.162** (0.022)	0.165** (0.022)	0.205** (0.021)
ED6	0.319** (0.032)	0.317** (0.032)	0.318** (0.032)	0.282** (0.023)	0.284** (0.024)	0.338** (0.022)
ED7	0.529** (0.058)	0.531** (0.058)	0.528** (0.058)	0.390** (0.059)	0.391** (0.059)	0.453** (0.058)
Controls of						
County of residence	yes	yes	yes	yes	yes	yes
Family back-ground	yes	yes	yes	yes	yes	yes
Intelligence tests	yes	instrument	instrument	yes	instrument	instrument
Achievement tests	instrument	yes	instrument	instrument	yes	instrument
Marks	instrument[1]	instrument[1]	yes[2]	instrument[1]	instrument[1]	yes[2]
R^2-*adj*	*0.239*	*0.239*	*0.240*	*0.199*	*0.198*	*0.191*

_old: The old school system.
_new: The new school system.
(1): dummies
(2): cont. variables
* Statistically significant from zero at 5 per cent level.
** Statistically significant from zero at 1 per cent level.

equations. The data set contains information on hourly earnings, annual earnings, years of schooling and highest educational level completed. To facilitate comparisons, the data set is bounded to include individuals in the age interval 35 to 50 years. Furthermore, only individuals who received income from work as employed and with total annual income higher than SEK 72,000 are included.

The results reported in columns one and two of Table 15, suggest that the magnitude of the educational wage premiums differ between the two measures of earnings. The wage premiums are substantially lower in column one compared to column two. This might be an indication of education-related differences in hours worked. The average number of hours worked, for full-time workers between 35 and 50 years of age, increases gradually from 38.4 hours per week (ED1) to 41.6 hours (ED7).

Table 15: OLS Log Earnings Equation Estimates. Swedish Level of Living Survey 1991. Standard errors are reported in parenthesis.

Independent variables	Dependent variable		Dependent variable	
	Log Hourly Earnings	Log Annual Earnings	Log Hourly Earnings	Log Annual Earnings
SCH			0.047** (0.004)	0.055** (0.004)
ED1	-0.080 (0.047)	-0.044 (0.053)		
ED3	-0.001 (0.042)	0.027 (0.048)		
ED4	0.178** (0.048)	0.200** (0.054)		
ED5	0.245** (0.047)	0.284** (0.053)		
ED6	0.359** (0.049)	0.498** (0.055)		
ED7	0.465** (0.081)	0.587** (0.091)		
Controls of				
Experience	yes	yes	yes	yes
Experience squared	yes	yes	yes	yes
R^2-adj	*0.241*	*0.265*	*0.216*	*0.225*

Note: The sample size is 629.
SCH: years of schooling
* Statistically significant from zero at 5 per cent level.
** Statistically significant from zero at 1 per cent level.

In a similar way, the return to schooling is lower in column three compared to column four.[12] Thus, we can expect that the wage premiums presented in previous section are somewhat too high, due to the incapability of controlling for hours worked. Furthermore, the wage premiums reported in column two are considerably higher than those reported in Tables 5 and 6. The fact that I in Table 15 also control for experience is one possible explanation to the large differences in wage premiums.

In order to understand how omitted ability variables affect the estimated return to schooling, I impute schooling on the basis of mean years of schooling reported for each educational level in the SLLS sample. Table 16 displays the estimates on the imputed schooling coefficient. In conformity with previous results, including family background variables has a small effect on the estimated return to schooling. The coefficients are reduced by roughly 5 per cent. Including different measures of ability, however, reduces the return to schooling by as much as 20 per cent, which is on a level with previous changes in wage premiums. Thus, we can expect that omitted ability

12. Schooling squared is not significantly different from zero.

Table 16: OLS estimates of the imputed schooling coefficient. Standard errors are reported in parenthesis.

	1948 cohort[1]				1953 cohort[2]			
Independent variables	(I)	(II)	(III)	(IV)	(I)	(II)	(III)	(IV)
SCHimp	0.051** (0.002)	0.049** (0.002)	0.042** (0.002)	0.039** (0.002)	0.047** (0.002)	0.044** (0.002)	0.039** (0.003)	0.037** (0.003)
Controls of								
County of residence	yes	yes	yes	yes	yes	yes	yes	yes
Family back-ground	no	yes	yes	yes	no	yes	yes	yes
Intelligence test scores	no	no	yes	yes	no	no	yes	yes
Achievement test scores	no	no	no	yes	no	no	no	yes
Marks	no	no	no	yes	no	no	no	yes
R^2-*adj*	*0.210*	*0.217*	*0.231*	*0.241*	*0.175*	*0.185*	*0.192*	*0.195*

Notes:
(1): The sample size is 3364.
(2): The sample size is 3018.
SCHimp: imputed schooling.
* Statistically significant from zero at 5 per cent level.
** Statistically significant from zero at 1 per cent level.

variables affect the estimated return to schooling and the estimated educational wage premiums in a similar way.

VI. Conclusions

In this study, I have used two Swedish data sets, which could not hitherto have been used for wage equations. The data sets consist of several measures of ability, such as intelligence tests, achievement tests and school marks. The information was collected when the respondents were between 12 and 13 years of age. Furthermore, instead of years of schooling I use information on highest educational level completed.

The empirical findings support the general opinion that the returns to education are biased upwards. Including measures of ability in the earnings equation reduces the wage premiums for education by as much as 25 per cent in some coefficients, with an avarage reduction around 22 per cent for the 1948 cohort and 18 per cent for the 1953 cohort. Further, controlling for ability reduces the observed decline in wage premiums for education between the two age cohorts, but the decline is not eliminated. In conformity with previous US studies, the results suggest that measures of ability associated with mathematics have significant effect on wages.

The magnitude of omitted ability bias presented are lower than comparable figures reported in US studies. However, both differences in the magnitudes of wage preniums and ability bias between countries might originate from errors in variables. For this purpose it would be desirable to have similar measures of ability – perfectly measured and not influenced of differences in age or education.

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Appendix

A1: Descriptive statistics of the sum of the three intelligence tests divided according to the respondents' highest educational level in 1993. Standard deviations in parenthesis.

	1948 cohort		1953 cohort	
Higiest Educational Level in 1993	elementary school (1961)	experimental comprehensive school (1961)	elementary school (1966)	comprehensive school (1966)
ED1	55.43 (14.06) N=383	60.16 (15.64) N=19	55.86 (14.53) N=88	51.40 (13.77) N=10
ED2	62.17 (15.40) N=168	57.24 (14.59) N=160	65.15 (14.12) N=72	60.26 (15.20) N=475
ED3				
General	61.47 (13.74) N=199	63.58 (16.04) N=118	60.38 (15.55) N=56	64.49 (14.64) N=267
Technical	60.82 (15.25) N=317	61.96 (14.87) N=217	61.74 (15.89) N=108	64.39 (14.84) N=444
ED4				
General	67.93 (15.18) N=131	72.23 (13.42) N=93	76.06 (17.32) N=36	75.03 (13.66) N=219
Technical	67.70 (14.71) N=307	69.64 (15.06) N=148	62.03 (15.13) N=30	67.52 (14.42) N=148
ED5				
General	69.88 (15.01) N=168	72.04 (13.77) N=93	72.06 (16.04) N=47	73.96 (14.12) N=245
Technical	71.36 (17.44) N=72	70.68 (13.29) N=47	75.54 (11.96) N=39	78.91 (13.85) N=178
ED6				
General	76.15 (14.14) N=336	76.41 (15.38) N=196	76.78 (15.22) N=50	78.05 (14.75) N=328
Technical	85.30 (11.06) N=97	84.60 (14.12) N=53	82.33 (16.19) N=21	86.05 (12.38) N=124
ED7				
General	82.86 (11.35) N=14	85.56 (12.92) N=18	73.00 (-) N=1	85.39 (12.67) N=18
Technical	84.00 (13.06) N=7	83.67 (12.10) N=9	-	86.88 (10.65) N=16
Numver of observations	*2199*	*1171*	*548*	*2472*

A2: OLS Log Earnings Equation Estimates (1948 Cohort). Standard errors are reported in parenthesis.

Independent variables	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)
ED1old	-0.032 (0.032)	-0.024 (0.032)	-0.022 (0.032)	-0.019 (0.032)	-0.023 (0.032)	-0.024 (0.032)	-0.020 (0.032)
ED1new	-0.040 (0.082)	-0.037 (0.082)	-0.051 (0.081)	-0.046 (0.081)	-0.044 (0.081)	-0.043 (0.081)	-0.054 (0.081)
ED2old	0.073 (0.038)	0.076* (0.038)	0.063 (0.037)	0.049 (0.037)	0.045 (0.038)	0.045 (0.037)	0.048 (0.037)
ED3_							
general	0.024 (0.033)	0.021 (0.033)	0.006 (0.033)	0.000 (0.033)	-0.003 (0.033)	-0.002 (0.033)	-0.004 (0.033)
technical	0.065* (0.031)	0.065* (0.031)	0.053 (0.030)	0.050 (0.030)	0.047 (0.030)	0.046 (0.030)	0.046 (0.030)
ED4_							
general	0.182** (0.035)	0.167** (0.035)	0.132** (0.035)	0.124** (0.036)	0.127** (0.036)	0.121** (0.036)	0.124** (0.036)
technical	0.187** (0.031)	0,183** (0.031)	0.154** (0.031)	0.141** (0.031)	0.142** (0.032)	0.141** (0.031)	0.136** (0.032)
ED5_							
general	0.240** (0.034)	0.236** (0.034)	0.201** (0.034)	0.190** (0.034)	0.192** (0.035)	0.191** (0.035)	0.186** (0.035)
technical	0.217** (0.041)	0.214** (0.041)	0.179** (0.041)	0.161** (0.041)	0.167** (0.041)	0.165** (0.041)	0.157** (0.041)
ED6_							
general	0.386** (0.031)	0.370** (0.031)	0.319** (0.032)	0.299** (0.032)	0.307** (0.032)	0.304** (0.032)	0.295** (0.032)
technical	0.508** (0.039)	0.501** (0.039)	0.427** (0.041)	0.399** (0.041)	0.415** (0.041)	0.411** (0.041)	0.391** (0.041)
ED7_							
general	0.716** (0.066)	0.698** (0.067)	0.625** (0.067)	0.599** (0.067)	0.610** (0.068)	0.612** (0.067)	0.588** (0.068)
technical	0.501** (0.089)	0.489** (0.090)	0.413** (0.090)	0.393** (0.089)	0.417** (0.090)	0.415** (0.089)	0.382** (0.090)
Controls of							
County of residence	yes	yes	yes	yes	yes	yes	yes
Family background	no	yes	yes	yes	yes	yes	yes
Intelligence tests	no	no	yes	no	no	no	yes
Achievement tests	no	no	no	yes	no	no	yes
Marks							
dummies	no	no	no	no	yes	no	yes
cont. var.	no	no	no	no	no	yes	no
R^2-*adj*	*0.219*	*0.226*	*0.238*	*0.244*	*0.242*	*0.241*	*0.247*

Note: see Table 5.

A3:OLS Log Earnings Equation Estimates ((1953 Cohort). Standard errors are reported in parenthesis.

Independent variables	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)
ED1old	-0.039 (0.038)	-0.034 (0.038)	-0.024 (0.038)	0.003 (0.041)	-0.014 (0.044)	0.014 (0.041)	0.008 (0.045)
ED1new	0.129 (0.103)	0.130 (0.103)	0.152 (0.102)	0.149 (0.102)	0.151 (0.102)	0.148 (0.102)	0.159 (0.102)
ED2old	0.049 (0.041)	0.053 (0.041)	0.044 (0.041)	0.065 (0.044)	0.063 (0.044)	0.073 (0.044)	0.084 (0.046)
ED3_							
general	0.028 (0.023)	0.027 (0.023)	0.017 (0.023)	0.020 (0.023)	0.017 (0.024)	0.023 (0.023)	0.018 (0.024)
technical	0.037 (0.020)	0.038 (0.020)	0.033 (0.020)	0.034 (0.020)	0.032 (0.021)	0.036 (0.020)	0.036 (0.021)
ED4_							
general	0.107** (0.025)	0.096** (0.025)	0.061* (0.026)	0.052* (0.027)	0.052* (0.027)	0.056* (0.027)	0.048 (0.027)
technical	0.139** (0.028)	0.132** (0.028)	0.122** (0.028)	0.124** (0.028)	0.122** (0.029)	0.126** (0.028)	0.125** (0.029)
ED5_							
general	0.181** (0.024)	0.174** (0.024)	0.146** (0.025)	0.143** (0.025)	0.141** (0.026)	0.147** (0.025)	0.139** (0.026)
technical	0.250** (0.027)	0.241** (0.027)	0.209** (0.027)	0.199** (0.028)	0.198** (0.028)	0.204** (0.028)	0.197** (0.028)
ED6_							
general	0.330** (0.022)	0.311** (0.023)	0.275** (0.024)	0.264** (0.025)	0.266** (0.025)	0.268** (0.025)	0.264** (0.025)
technical	0.418** (0.031)	0.395** (0.032)	0.347** (0.033)	0.329** (0.034)	0.332** (0.034)	0.334** (0.034)	0.329** (0.034)
ED7_							
general	0.629** (0.076)	0.602** (0.076)	0.552** (0.076)	0.537** (0.076)	0.550** (0.077)	0.543** (0.076)	0.544** (0.077)
technical	0.313** (0.082)	0.273** (0.083)	0.225** (0.084)	0.204** (0.084)	0.211** (0.084)	0.209** (0.084)	0.207** (0.084)
Controls of							
County of residence	yes	yes	yes	yes	yes	yes	yes
Family background	no	yes	yes	yes	yes	yes	yes
Intelligence test	no	no	yes	no	no	no	yes
Achievement tests	no	no	no	yes	no	no	yes
Marks							
dummies	no	no	no	no	yes	no	yes
cont. var.	no	no	no	no	no	yes	no
R^2-adj	*0.182*	*0.192*	*0.199*	*0.204*	*0.201*	*0.203*	*0.203*

Note: see Table 6.

A4: Coefficients on family background variables used in column two of Tables 5 and 6. Standard errors in parenthesis.

Family backgrund variables	1948 cohort	1953 cohort	Family backgrund variables (cont.)	1948 cohort	1953 cohort
Father–educ1	-0.030 (0.026)	0.020 (0.024)	Father–occ5	0.067 (0.097)	-0.192* (0.094)
Father–educ2	-0.005 (0.074)	-0.055 (0.089)	Father–occ6	0.091 (0.066)	0.152** (0.052)
Father–educ3	0.077* (0.037)	0.104** (0.033)	Father–occ7	0.012 (0.021)	0.033 (0.021)
Father–educ4	0.039 (0.076)	0.262** (0.086)	Father–occ8	-0.032 (0.026)	0.017 (0.028)
Mother–educ1	-0.028 (0.021)	0.013 (0.019)	Father–occ9	0.033 (0.020)	0.046* (0.020)
Mother–educ2	0.022 (0.079)	0.017 (0.031)	Mother–occ1	0.041* (0.017)	-0.015 (0.017)
Mother–educ3	-0.065 (0.046)	-0.008 (0.041)	Mother–occ2	-0.068 (0.153)	0.154 (0.322)
Mother–educ4	0.211* (0.102)	0.100 (0.118)	Mother–occ3	-0.187 (0.106)	-0.072 (0.120)
Father–occ1	0.002 (0.064)	0.073 (0.073)	Mother–occ4	-0.021 (0.063)	0.026 (0.060)
Father–occ2	-0.046* (0.019)	-0.043* (0.021)	Mother–occ7	0.067 (0.071)	0.017 (0.057)
Father–occ3	-0.064 (0.076)	-0.127 (0.083)	Mother–occ8	0.053 (0.053)	-0.034 (0.042)
Father–occ4	-0.000 (0.074)	-0.185** (0.067)	Mother–occ9	0.022 (0.032)	-0.003 (0.031)

Note: see Tables 5 and 6.

reference group: juior secondary school.
educ1: elementary school.
educ2: unknown.
educ3: upper secondary school.
educ4: post secondary school.

reference group: workers.
occ1: unknown.
occ2: agriculture and fishermen etc.
occ3: professionals.
occ4: elementary school teachers and others.
occ5: officers.
occ6: managers and wholesale dealers etc.
occ7: sales workers and craftsmen.
occ8: higher grade non manual.
occ9: white collar.

A5: Coefficients on ability variables used in Table 5. Standard errors in parenthesis

Indepeidenl variables	(III)	(IV)	(VI)
Intelligence tests			
Number Series	0.037** (0.007)		
Opposite	0.015* (0.007)		
Metal foldings	0.007 (0.007)		
Achievement tests			
Reading		0.025* (0.011)	
Writing		-0.035** (0.012)	
English		0.008 (0.010)	
Mathematics		0.061** (0.009)	
Marks			
Swedish			-0.011 (0.009)
English			0.006 (0.009)
Mathematics			0.058** (0.008)

Note: see Table 5.
Coefficients on measures of ability used in specifications (V) and (VII)
are available from the author upon request.

A6: Coefficients on ability variables used in Table 6. Standard errors in parenthesis

Independent variables	(III)	(IV)	(VI)
Intelligence tests			
Number Series	0.028** (0.007)		
Opposite	0.010 (0.007)		
Metal foldings	0.004 (0.007)		
Achievement tests			
Swedish		-0.000 (0.010)	
Reading		-0.013 (0.023)	
Writing		0.011 (0.024)	
English		0.003 (0.010)	
Mathematics		0.043** (0.009)	
Marks			
Swedish			
7 point scale			-0.009 (0.022)
5 point scale			-0.004 (0.011)
English			
7 point scale			-0.014 (0.021)
5 point scale			0.009 (0.011)
Mathematics			
7 point scale			0.065** (0.018)
5 point scale			0.038** (0.009)

Note: see Table 5.
Coefficients on measures of ability used in specifications (V) and (VII) are available from the author upon request.

A7: OLS Log Earnings Estimates used in Table 8. Standard errors in parenthesis.

	1948 cohort		1953 cohort	
Independent variables	(III), (IV)	(VI), (VII)	(III), (IV)	(VI), (VII)
ED1old	0.098 (0.086)	0.248 (0.143)	0.072 (0.102)	0.174 (0.109)
ED1new	0.080 (0.287)	0.176 (0.371)	0.296 (0.230)	0.570* (0.293)
ED2old	0.277** (0.101)	0.639** (0.176)	0.029 (0.123)	0.211 (0.132)
ED3	0.093 (0.079)	0.209 (0.131)	-0.001 (0.051)	0.056 (0.052)
ED4	0.227** (0.083)	0.234 (0.137)	0.089 (0.064)	0.115 (0.065)
ED5	0.354** (0.091)	0.569** (0.151)	0.106 (0.064)	0.126 (0.067)
ED6	0.348** (0.086)	0.328* (0.142)	0.353** (0.067)	0.285** (0.070)
ED7	0.640* (0.275)	0.050 (0.417)	0.016 (0.249)	0.108 (0.316)
Intelligence test (Number Series)	0.078** (0.030)		0.027 (0.016)	
Achievement test (Mathematics)		0.107** (0.031)		0.054** (0.017)
ED1_*Ability				
old	-0.007 (0.005)	-0.007* (0.004)	-0.006 (0.006)	-0.007* (0.004)
new	-0.007 (0.015)	-0.006 (0.010)	-0.011 (0.015)	-0.017 (0.011)
ED2old*Ability	-0.012* (0.005)	-0.015** (0.004)	0.001 (0.006)	-0.005 (0.004)
ED3*Ability	-0.003 (0.004)	-0.005 (0.003)	0.002 (0.003)	-0.001 (0.002)
ED4*Ability	-0.004 (0.004)	-0.003 (0.004)	0.000 (0.003)	-0.001 (0.002)
ED5*Ability	-0.008 (0.005)	-0.010** (0.004)	0.003 (0.003)	0.001 (0.002)
ED6*Ability	-0.001 (0.004)	-0.002 (0.004)	-0.002 (0.003)	-0.000 (0.002)
ED7*Ability	-0.004 (0.010)	0.007 (0.008)	0.015 (0.009)	0.005 (0.006)

_old: The old school system.
_new: The new school system.
* Statistically significant from zero at 5 per cent level.
** Statistically significant from zero at 1 per cent level.

A8: Probit analysis. Standard errors are reported in parenthesis.

	1948 cohort[1]		1953 cohort[1]	
Independent variables	(I)	(II)	(I)	(II)
ED1old	0.290 (0.157)	0.305* (0.157)	0.201 (0.147)	0.196 (0.147)
ED1new	0.147 (0.404)	0.144 (0.408)	-0.652* (0.287)	-0.650* (0.286)
ED2old	0.204 (0.182)	0.171 (0.183)	0.131 (0.158)	0.120 (0.158)
ED3	0.007 (0.135)	-0.022 (0.135)	0.060 (0.069)	0.058 (0.069)
ED4	0.311* (0.143)	0.234 (0.145)	0.308** (0.087)	0.289** (0.090)
ED5	0.313* (0.156)	0.225 (0.159)	0.502** (0.089)	0.479** (0.093)
ED6	0.715** (0.156)	0.589** (0.163)	0.773** (0.099)	0.744** (0.105)
ED7	0.400 (0.308)	0.225 (0.316)	0.333 (0.249)	0.284 (0.252)
Number Series		0.010* (0.005)		0.010** (0.004)
Metal Folding		0.006 (0.005)		-0.004 (0.004)
Opposite		0.004 (0.006)		-0.004 (0.005)
Controls of				
County of residence	yes	yes	yes	yes

Notes:
(1): The sample size is 3655.
(2): The sample size is 3754.
_old: The old school system.
_new: The new school system.
* Statistically significant from zero at 5 per cent level.
** Statistically significant from zero at 1 per cent level.

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