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Acknowledgments

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1. Introduction

There is a broad interest in the effectiveness of education policy and in the returns to education. However, because of the way that reforms are implemented it is often hard to distinguish their impact from the effect of other confounding factors. Moreover, it is rare that we can measure the long term impact of education policies on earnings.

The impact of compulsory schooling laws, "equal education opportunities" and the potential benefits or costs of non-selective schools are issues that are central to education policy. The issues are important both from a growth and a distributional perspective (see for example Bénabou, 1996). In Western countries there has been a drive to increase compulsory schooling, which is now around 9 years of education (see Card, 1999 for a review). Moreover, in several countries there is an active debate on the merits of selective education, whereby high performing students are placed in special classes or tracks (streaming/tracking).

A Swedish educational reform in the 1950s and 60s offers a nearly ideal opportunity to address some of these issues. In 1950 the Swedish parliament decided to extend compulsory schooling from 7 or 8 years (depending on the municipality) to a 9 year comprehensive school with a centrally set curriculum. What makes this reform of general interest is that it was preceded by a unique nationwide social experiment between 1949 and 1962, when the new school system was finally implemented. In this experiment, the new comprehensive school was implemented gradually, by municipality. The experiment allows us to compare the educational and labor market outcomes of a cohort of individuals going through two different school systems in very similar economic and social environments. The overall effect of the reform on educational outcomes and earnings can, thus, be isolated from the effect of macroeconomic shocks and cohort effects.

In addition to estimating the impact of the reform we assess the mechanisms through which it operated by estimating returns to education. As in earlier studies which have used institutional changes or reforms to identify a return to education[1] we can use the comparisons between the reform and the non-reform municipalities to this e®ect. However, for some groups of individuals we can go further than that: The municipalities belong to 24 di®erent counties each of which can be thought of as constituting a local labor market. By exploiting di®erences in the impact of the reform across counties, probably due to di®erent local labor market conditions, we can estimate the returns to education and, separately, the e®ect of the reform conditional on educational outcomes. The procedure is similar to a di®erence in di®erences estimation method, where the reform assignment acts as a group e®ect and the instruments are interactions between the county of schooling and the reform.[2] Such an approach allows us to go some of the way towards distinguishing the e®ect of the reform through the quantity of education and directly through the other changes that were brought about, the most important of which was the abolition of selection into an academic and vocational track at age 12 or 13. Our ability to do this depends on the reform having had di®erential impacts across counties and this is true primarily for the low ability children of low parental backgrounds. We are unable to o®er clear evidence on this issue for all groups of children.

We interpret our results within a framework where the returns to the reform and to education are heterogeneous. We use propensity score matching to control for any di®erences in the distribution of characteristics between the reform and non-reform municipalities (see Rosenbaum and Rubin, 1983, and Heckman, Ichimura and Todd, 1997). In estimating the returns to education we use Instrumental Variables. The interpretation of Instrumental Variable estimates when the impact of the treatment di®ers across individuals has been discussed extensively in the treatment e®ects liter-

[1] See Angrist and Krueger (1991 and 1992), Butcher and Case (1994), Card (1993), Harmon and Walker (1995), Kane and Rouse (1993).

[2] A similar approach was followed by Du°o (2000) in a recent paper in which she evaluates the e®ects of a major school construction programme in Indonesia on education and wages. She also estimates the returns to schooling by exploiting regional di®erences in the construction programme, which induced di®erences in educational attainment.

ature (Heckman and Robb, 1985, and Imbens and Angrist, 1994). We also exploit the vast array of test scores and parental background variables at our disposal to estimate impacts of the reform and the returns to education for di®erent groups. This allows us to better interpret the impacts we observe and derive conclusions that are of a general interest for economists, beyond the particularities of the Swedish experiments.

The empirical analysis of this social experiment is further enhanced by access to unique data obtained by combining the Individual Statistics (IS) survey[3] with administrative sources. The IS survey is a random sample of about 10 percent of Swedish individuals born in 1948 and was collected in 1961 when the individuals were in sixth grade (aged 12 or 13). The data contains results from a large number of test scores from IQ tests and grades of subjects taught in schools - all test results were obtained before the split into the new and old school systems took e®ect.[4] We obtained earnings data for the 1985-96 period for each individual by matching the sample with tax registers.

The paper is organized as follows. Section 2 gives a brief description of the Swedish education system and the 1950 social experiment and education reform. The details are provided in an appendix. Section 3 presents a simple theoretical framework for interpreting the results. Section 4 discusses estimation and the interpretation of the estimates. Section 5 describes the data-set. Section 6 compares the characteristics of the municipalities assigned to the reform to those not assigned. We then compare the characteristics of the \treated" and \non-treated" individuals and establish that they are very similar in terms of ability. In Section 7 we present results on the impact of the reform on educational attainment, followed by results on the returns to education. Section 8 concludes.

[3] This data-set is provided by the Department of Educational Science at the University of Gothenburg, see e.g. Härnqvist and Svensson (1973).

[4] See Angrist and Krueger (1998) on the importance of using ability measures that are not outcome variables.

2. The 1950 Education Reform

The pre-reform basic education consisted of two main parts: a basic compulsory school (folkskolan) and a junior secondary school (realskolan). The first six years were common for all pupils. After the sixth grade the more able students were selected into the junior secondary school. The selection was in general made on grades. Those who failed to enter junior secondary school continued for one or two years in basic compulsory school. The compulsory schooling was at least seven years and in some municipalities, mainly in city communities, eight years. The basic compulsory schools were administered by the municipalities.

Graduation from the junior secondary school was a requirement for the upper secondary school, which, in turn, was required for one to qualify for higher education. After a reform in 1958, those who graduated from junior secondary schools in general had nine years of schooling, i.e. six years in basic compulsory school and three years in junior secondary school, before they could enter the upper secondary school.

In 1950, the Swedish parliament decided on the principles for a major reform of the school system. There were three stated aims of the reform: (1) Increase the education of the least skilled. The number of compulsory years of education was extended to 9 years for all. In 1949, about 65 per cent of individuals did not complete any education beyond the 7 or 8 year pre reform compulsory school (see Erikson and Jonsson, 1993). (2) To facilitate the transition to higher education levels. Thus all pupils who finished the new compulsory comprehensive school qualified for some secondary education. (3) To promote equality of opportunity. Thus the introduction of a centrally decided curriculum and the abolition of selection at sixth grade were intended to level out differences in educational opportunities between children in different areas or from different socio-economic backgrounds.

The final parliamentary decision on the reform, which set the curriculum of the comprehensive school, was not taken until 1962. In the 13 year period preceding this decision - starting in 1949, the year before the first parliamentary decision - a nationwide experiment with the new school system was carried out. In this experiment the proposed comprehensive school was implemented in entire selected municipalities or parts of city communities. At the time when the cohort we will be looking at (born in 1948) was assigned to the experiment (1960/61) the number of municipalities and city was 1,037. The way that the municipalities were assigned to the reform is described in Appendix A. This assignment was not random but chosen by the experimental committee to form a "representative" sample.

The pupils from low income backgrounds and in the municipalities participating in the reform were paid a (means tested) stipend to help them through the extra time in school. Moreover, an additional child benefit was introduced across all of Sweden at the same time.

Details on how the experiment was conducted and financed are provided in Appendix A, together with a precise description of the institutions and how they changed. We also discuss issues of compliance.

3. A simple Theoretical Framework for Interpreting Results

In this section we summarize some useful results from a simple theoretical model of education choice, that are helpful in interpreting our empirical results. Many of the ideas date from Becker and have been re-examined recently with empirical analysis in mind by Lang (1993), Heckman (1997) and Card (2000).

Consider a simple two period model. In the first period the individual shares his one unit of time between schooling s and work $1 - s$. In the second period the individual works. Denote by $q_i(s)$ the costs of education and by $q_i'(s) > 0$ the marginal cost, including tuition costs for individual i but not including opportunity cost. We assume that the cost function is convex, $q_i''(s) \geq 0$. Wages in the first period are denoted by $w_1 a_i$ while wages in the second period are equal to $w_2 a_i m_i(s)$ where $m_i(s = 0) = 1$ and $m_i'(s) = \frac{\partial m_i(s)}{\partial s} > 0$. a_i is an individual specific endowment of human capital (say ability), while m_i represents the (possibly) individual specific wage returns to education. Utility in both periods is assumed to depend on consumption only and not on education directly.[5] Hence, the choice of education is driven by life-cycle wealth maximization. Education choice is the solution to $\max_s \{(1 - s)w_1 a_i - q_i(s) + \frac{1}{1+r_i} w_2 a_i m_i(s)\}$. The first and second order conditions for the choice of education level s can be written as

$$\begin{aligned} &\text{First order condition:} && \frac{w_2 a_i m_i'(s)}{w_1 a_i + q_i'(s)} = 1 + r_i \\ &\text{Second order condition:} && w_2 a_i m_i''(s) - (1 + r_i) q_i''(s) < 0. \end{aligned} \tag{3.1}$$

where r_i is the borrowing (discount) rate for individual i. In a perfectly competitive market the discount rate is a constant ($r_i \equiv r$). In the presence of liquidity constraints however, certain individuals will face higher discount rates r_i while others lower. It is easy to establish that

$$\begin{aligned} &\text{I} \quad \frac{\partial s}{\partial r_i} = \frac{w_1 a_i + q_i'(s)}{w_2 a_i m_i''(s) - (1+r_i) q_i''(s)} < 0 \qquad \text{II} \quad \frac{\partial s}{\partial a_i} = \frac{(1+r_i) w_1 - w_2 m_i'(s)}{w_2 a_i m_i''(s) - (1+r_i) q_i''(s)} > 0 \\ &\text{III} \quad \frac{\partial s}{\partial w_1} = \frac{(1+r_i) a_i}{w_2 a_i m_i''(s) - (1+r_i) q_i''(s)} < 0 \end{aligned} \tag{3.2}$$

The first expression (I) implies that individuals with a higher discount rate r_i will obtain less education than otherwise. The second expression (II), whose sign follows directly from the first order conditions,[6] implies that individuals with greater ability will obtain more education, despite the increased first period opportunity cost. Finally, the third expression (III) establishes that an improvement in first period labor market opportunities will lead to a decrease in educational attainment. .

We now consider the impact of the education reform on individuals with unskilled parents and those with skilled parents. Assume (for now) that conditional on observed ability, parental education has no influence on either the costs or benefits of education; however suppose that those with unskilled parents are liquidity constrained with borrowing rate $r_i^{unskilled} > r_i^{skilled}$, where $r_i^{skilled}$ is the discount rate for those whose parents

[5] However the costs of education could be thought to include any effort costs.

[6] Note from the first order conditions that $w_2 m_i'(s) > (1 + r_i) w_1$.

are skilled. Given ability, the reform will impact primarily on the education levels of those with unskilled parents, since they will have lower initial levels of education due to discounting. Within that group it should have a greater impact on those with lower ability, since their optimal pre-reform level of education should be lower as implied by the expression for $\frac{\partial s}{\partial a_i}$. Ability may affect $m_i'(s)$ with the same conclusions for this point. For those with no liquidity constraints the change of the compulsory schooling level will still have an impact to the extent that they are low ability and have a very low marginal benefit (relative to cost) of education. Thus, we expect little, or no, impact of the reform among those with skilled parents and high ability.

Finally note that liquidity constraints are just one interpretation as to why children from poorer backgrounds have lower levels of education than equally able children from wealthier parents. Other potential reasons include higher marginal costs (due say to adverse social pressure) or lack of information about the returns.

4. Estimating the Impact of the Reform and the Returns to Education

Our empirical analysis consists of two parts. In the first we evaluate directly the impact of the reform on educational qualifications and on earnings. In the second we use the reform to estimate the returns to education and assess whether the reform had direct impacts on earnings other than those due to the increased levels of education.

4.1. The Impact of the Reform on Educational Qualifications and Earnings

If the reform were truly randomly assigned we could estimate the average impact of the reform simply by comparing the average educational attainment and earnings of those who went through the reform system to the earnings of those who went through the old system. Since random assignment has not taken place we evaluate the impact of the reform on education and earnings using propensity score matching (Rosenbaum and Rubin, 1983 and Heckman, Ichimura and Todd, 1997). The assumption underlying matching is that assignment to reform, conditional on our observables, is random and independent of education and earnings in the non-reform state. This requires that the observable characteristics are sufficient to explain any relationship that the reform assignment may have had to earnings potential in the non-reform state. Matching will make a difference to our estimates of the impact of the reform only to the extent that the distribution of characteristics is different in the control and the treatment (reform) sample.

Formally, denote by $\ln w^1_{it}$ individual i's annual log earnings in period t if the individual has been through the reform system. The same person's log earnings in the non-reform state are $\ln w^0_{it}$. Only one of these quantities is observed for each individual, depending on whether the individual has been assigned to the reform or not. The impact of the reform for individual i then is $\ln w^1_{it} - \ln w^0_{it}$. Define $TT = E[\ln w^1_{it} - \ln w^0_{it} | D_i = 1]$ to be the impact of the reform on those actually assigned to the reform ($D_i = 1$).[7] Under the matching assumption (selection on observables) we can use the earnings of those not assigned to the reform ($D_i = 0$) to estimate the average counterfactual earnings for those who were assigned, i.e. $E[\ln w^0_{it} | X_i, D_i = 1] = E[\ln w^0_{it} | X_i, D_i = 0]$, where X_i represents observed characteristics.

Rosenbaum and Rubin (1983) have shown that it is sufficient to match on the propensity score instead of matching on values of the vector X_i. The propensity score $P(X_i)$ is the probability of assignment to the reform conditional on characteristics X_i. Define the observed log earnings as $\ln w_i = D_i \ln w^1_{it} + (1 - D_i) \ln w^0_{it}$. The treatment on the treated parameter can be written as $TT = E[\ln w_i | D_i = 1] - E_{F^1} \{E[\ln w_i | P(X_i), D_i = 0]\}$ where E_{F^1} denotes that the expectation is taken with respect to the distribution of the propensity score in the treatment sample. The first expression is the unconditional average in the treatment (reform) sample. The expectation in the square bracket of the second part of this expression is the conditional expectation of log wages given the propensity score, in the non-reform sample. This is

[7] Impact of treatment on the treated.

then averaged using as weights the distribution of the propensity score in the sample of the individuals who went through the reform.

To implement this matching estimator we estimate the propensity score using a probit of the reform on our matching variables. We then use cubic splines with 4 knots to estimate $E[\ln w_i | P(X_i); D_i = 0]$ on the sample of individuals not assigned to the reform. We repeat this for the reform sample to estimate $E[\ln w_i | P(X_i); D_i = 1]$. Each individual in the reform sample is matched to his nearest neighbor in the non-reform sample, based on the value of the estimated score. At this point, we also impose a tolerance level; if the absolute difference of the propensity scores between the treated individual and the nearest neighbor in the control sample is not small enough we reject that treated individual and leave him unmatched. We then average the difference between the smoothed earnings of the treated individual (i.e., the estimate of $E[\ln w_i | P(X_i); D_i = 1]$) and $E[\ln w_i | P(X_i); D_i = 0]$ obtained from his nearest non-reform neighbor, over the sample of the individuals assigned to the reform. This method of matching is a modification of a method found by Heckman, Ichimura and Todd (1997) to be particularly efficient in practice.[8] Finally, we use the bootstrap to compute 95% bias corrected confidence intervals for the estimates of TT.[9] The confidence intervals we present allow for the fact that the propensity score is estimated.

We follow the same procedure when evaluating the impact of the reform on educational attainment. We use two measures of educational attainment: years of education and highest qualification.

4.2. Estimating the Returns to Education

We now turn to the estimation of the returns to education, using the reform as an instrument. Within this context we will present a framework that will allow us to test whether the reform operated exclusively through the changes in the quantity of

[8] The modification consists in the fact that we also smooth the earnings of the reform individuals. This tends to improve precision slightly.

[9] See Horowitz (1999).

education it induced or whether it may also have had an additional direct impact on earnings.

Education in Sweden consists of seven levels S_l $(l = 1, \ldots, 7)$, including the pre-reform basic school. Ideally we would estimate returns to each level, allowing for nonlinearity (e.g. sheepskin effects) and for observed and unobserved heterogeneity of the returns at each level.[10] However, given that we have just one discrete instrument identifying the average returns for each level of education separately is not feasible, unless we impose strong functional form assumptions. We thus apply instrumental variables (IV) in the wage equation

$$\ln w_{it} = q_t + \beta' x_{it} + \gamma ed_i + v_i, \tag{4.1}$$

where $ed_i = \sum_{l=1}^{L} \omega_l S_{il}$ and where ω_l are the numbers of years of education required to reach education level l. We estimate this either on the whole population or by group defined by ability and parental education. The question is how does one interpret such an IV estimate when the underlying returns are heterogeneous in the population and there are potential non-linearities in the returns.

First, we take as an instrument the assignment to the reform, which is either zero (not in the reform) or one. To understand what parameter our procedure estimates denote by $S_{il}(z)$ the schooling outcome as a function of the policy assignment. We denote by $w_i(l)$ the wage individual i would have obtain if she had reached education level l. We start by assuming that $\{w_i(l), S_{il}(z = 1), S_{il}(z = 0), l = 1, \ldots, L\}$ are jointly independent of the policy assignment given the observables x.[11]

When the treatment (education here) is binary and the response to the treatment is heterogeneous across individuals, Imbens and Angrist (1994) have shown that IV

[10] See the model of Willis and Rosen (1979) and Björklund and Moffitt (1987) when estimating the returns to training. A more general model would have been that of the Roy model for each education level. See Heckman and Honore (1990) for the identifiability of this model.

[11] These include the county where schooling took place, the county of residence, a large array of test scores, population size in the municipality, aggregate income of the municipality, the local tax rate and whether it is a city or rural community. We also include parental education in some of the regressions.

with a binary instrument such as ours, estimates the average e®ect of the treatment for those who were induced to take up treatment as a result of the policy (LATE[12]). This result relies on the assumption of monotonicity, namely that the policy induces some individuals to take the treatment which they would not otherwise have done and no one to opt out who would have taken the treatment in the absence of the policy.[13] The monotonicity assumption has been shown by Vytlacil (2000) to be equivalent to an assumption that the treatment (education) choice can be modelled using a single index based threshold crossing model.[14] In the case where many intensities are possible (i.e. many education levels) this implies that education choices are ordered and can be explained by a single index model with thresholds that vary with characteristics and are random.[15]

To the extent that the reform only a®ected attendance at the ¯rst two levels of education, IV in 4.1 will estimate the return to education for those who were induced into an extra year of education vis-¶a-vis the earlier statutory minimum. However, if other levels of education were a®ected and under the monotonicity assumption for multiple intensities IV, will identify a weighted average of the returns to education at all levels of intensities a®ected by the reform (see Angrist and Imbens, 1995). The weights are proportional to the impact of the reform on each education level and they sum to one.[16]

In the context of the reform we are considering the monotonicity assumption most

[12] Local Average Treatment E®ect.

[13] Of course, the reverse would also be a suitable monotonicity assumption if the policy tended to reduce treatment. In our context the policy is designed to increase education.

[14] As explained in Vytlacil (2000), this means that we can write D_i = 1(m(x;z) ¸ u); where 1(a) = 1 if a is true and zero otherwise. Hence treatment choice can be expressed as a simple threshold crossing model.

[15] An ordered probit is a special case of this. As shown by Cameron and Heckman (1998) in the ordered probit case this implies that education choices are governed by just one unobservable. However, allowing the thresholds to be stochastic leads to a more general education choice model. We thank Ed Vytlacil who showed us this during a conversation.

[16] Trivially, if the reform just shifted persons from the old statutory level and into the new higher one, the IV estimator will just measure the returns to education at the lower end of the education distribution for the switchers.

probably holds between the old statutory level of education and the new one. The impact of the reform is small further up the education distribution.

The discussion above relies on the assumption that reform assignment was independent of earnings given the observables. However if the reform had some impact on the quality of education it may have a direct effect, as well as through the amount of education obtained. We can address this issue to the extent that the reform had different impacts on education in different counties, possibly due to different education costs or local labor market opportunities at the time when the individuals were making their education choice. In this case we can include the reform assignment indicator among the observables and use the interactions between the county of schooling and the reform indicator as an instrument. Thus we estimate

$$\ln w_{it} = q_t + \delta reform_i + \zeta' x_{it} + \gamma ed_i + v_{i1}; \tag{4.2}$$

where $reform_i$ indicates assignment to the reform (1) or not (0).[17] The instruments are the interactions between the county of schooling and reform assignment.

The LATE interpretation can still be given. However the estimated parameter now will be the weighted average of the impacts across municipalities for those induced to attain higher levels of education by the reform. The weights depend on the number of people induced into higher levels of education by the reform in each municipality. Implementation of this approach requires us to confirm first that the reform did have differential impacts across municipalities. Subject to this we can test whether the reform assignment has an impact on earnings, conditional on educational attainment.

Finally, we consider estimating the returns to education for individuals with different levels of measured ability and different parental education.

[17] Indicators for the county of schooling are also included among the x's: This estimator is basically a differences-in-differences estimator: Difference between reform and non-reform municipalities and then between counties.

5. Data

We use data from the Individual Statistics (IS) project of the Institute for Education at the University of Gothenburg[18] merged with administrative data on education level, reform assignment and pre-tax earnings obtained from tax records for the years 1985 to 1996.

The IS project has produced six separate data-sets corresponding to the birth cohorts 1948, 1953, 1967, 1972, 1977 and 1982. We use the survey for the 1948 cohort as this was the main cohort available that was split between the reform and non-reform (old) system. The 1948 cohort survey was obtained in the spring of 1961 when the respondents were 12 or 13 years of age and most of them in sixth grade in compulsory school by the time of the survey.

All children born the 5th, 15th or 25th in each month in 1948, i.e., about 10 percent of the cohort, were selected to be included in the sample. The potential sample size is 12,166 men and women. With a rate of non-response for the 1948 survey of about 1.8 per cent, the final sample size was 11,950.

In 1961, the experiment with the new comprehensive school was still in progress and, as is evident from Table 12 in Appendix A, 28 percent of the municipalities had implemented the new school. The IS data-set contains a variable for individual assignment to the reform which is obtained from the National school board register and applies to the school year 1960/1961, i.e., the year when interviews and tests were done. About 35 percent of the students in our data-set were assigned to the new school. Assignment to the reform is measured in 6th grade before any switches to a different system could have taken place (see Appendix on compliance).

The data-sets consist of four main parts: (1) Information on the student's social background, socio-economic situation, leisure activities and plans for future studies; (2) Results from IQ and achievement tests; (3) Register information on the students

[18] See Härnqvist and Svensson (1973) for a detailed description of the project and the data.

performance and type of school; (4) Annual earnings obtained from the 1985-1996 Swedish tax registers as well as information obtained from the education registers from 1993.[19]

An important feature of the data, from a reform evaluation perspective, is that all measures of student ability were obtained at an age when all students had the same quantity of education and relate to the year before the children in the non-reform sector were split between the two tracks (vocational and junior secondary). Thus, the test scores are not the outcome of reform assignment or educational choice.

Information on levels of education were obtained from the so called SUN-code from the National Education Register. We use information on seven broad educational levels only, which are comparable before and after the reform. These levels are briefly described in Table 1 together with the corresponding names of equivalent US and UK educational levels. We also report estimated average years of education corresponding to each level.

Given the way the information is collected, and as we use comparatively broad categories for levels of education, there is very little scope for measurement error in education.[20] This is important, particularly given concerns that when we include controls for observed ability we reduce the signal from the education variable relative to the measurement error variance, thus biasing the education effect downwards (see Griliches, 1977). Moreover, as Kane, Rouse and Staiger (1999) in the case of levels of

[19] See Appendix B for descriptions on all these variables.

[20] Statistics Sweden has investigated the quality of the National education register (see Beskrivning av Statistiken Befolkningens Utbildning, 1999) by comparing the data with those obtained in a questionnaire study. It was found out that in 83 % of the cases the results for the level of education from the register coincided with those obtained from the questionnaires. This can be seen as an upper bound of the rate of mis-classification in the National education register. The main source of mis-classification was found to be between level 4 and level 5: Level 4 was over-estimated since several unfinished university education and vocational educations were not reported. Kjellström (1999) has investigated the effect of measurement errors in imputed years of schooling for wage equations including ability measures using the IS survey. His findings show that a reliability ratio of 0.9 has a very small effect on the estimates of returns to schooling. See also a recent paper by Kane, Rouse and Staiger (1999) who look at the impact of measurement error on estimated returns when measurement error is non-classical. They also present evidence that levels of education are much better measured in surveys than years of education.

education measurement error is non-classical and having access to data with as little measurement error as possible is important even when using Instrumental Variables.

We use the all test scores and grades included in the IS data set to measure intellectual ability. In order to obtain a flexible specification we transformed the test scores into decile groups and then generated indicator variables for each decile group and each test score.[21] In a second step, a principal component analysis was carried out on all the indicator variables. Appendix B shows the result from this analysis.

For some of the empirical analysis we will divide the sample into two groups by ability. To do that we use the first principal component (Abil1), i.e., the one accounting for the largest share of the variance. Table 13 in Appendix B shows the factor loadings of the first three principal component. Since the factor loadings of the first principal component are increasing in test scores and grades (positive and increasing for above median test scores; negative decreasing for below median scores) the interpretation of it is unambiguous: It gives high numerical value for high achievers and low for low achievers.

Sweden is divided administratively into 24 counties, each of which contains a number of municipalities within commuting distance of each other. The counties are often used to define local labor markets (see e.g. Westerlund, 1997). Importantly, all counties but one had some reform and some non-reform municipalities.

The final sample size was 5744 men and 5540 women. For each we observe earnings for the entire (or part of) the 1985 to 1996 period.

Level	Description of the Schooling Level	Average Number of Years of Schooling[6]
1	Pre-reform compulsory school	8
2	Post-reform comprehensive (compulsory school) or pre-reform junior secondary school	9
3	Upper secondary school **6** 2 years[1]	11.5
4	Upper secondary school **>** 3 years[2]	13
5	Post upper secondary school **6** 2 years[3]	15
6	Post upper secondary school **>** 3 years (University/College)[4]	17
7	Ph.D. or licentiate[5] degree at a University	21

Notes: [1] This level corresponds mainly to vocational education.

[2]The threee or four year upper secondary schools have a more academic curriculum compared to the those corresponding to level 3 and are required for most studies at the college/university level. Corresponding to sixth form of a comprehensive school (UK) and senior high school (US).

[3]Shorter college or university educations, e.g. educations for nurses and elementary school teachers, as well as un¯nished longer university educations.

[4]Degrees from longer university or college educations, e.g. business administration, law, engineering or medicine.

[5]The licentiate degree is a shorter, compared to the Ph.D., post-graduate university education.

[6]Estimates of the average number of years of schooling for each level of education are obtained from the Swedish Level of Living survey for the cohorts born between 1945 and 1955.

Table 1: Short descriptions of each education level and estimates of average number of years of schooling for each level.

6. Results

6.1. Comparing the Reform and Control Samples

As we described in Section 2 and Appendix A, the municipalities that implemented the reform were not randomly chosen, although an effort was made to create a representative sample. In this Section, we compare the characteristics of the reform and non-reform samples.

The data-set contains information from individual IQ-tests as well as results from several tests on knowledge in different subjects taught at school.[22] The results from this comparison, i.e. the average scores on the different tests for the two sub-groups, are shown in Table 2. As can be seen in Table 2, the average results on these tests are very similar, although one may argue that the municipalities that participated early on had marginally more able students. In most cases the differences are not statistically significant. Moreover, the proportion of skilled parents was higher in the reform municipalities but, again, the difference is insignificant. Our presumption is that these results lend support to the idea that reform assignment is not correlated with unobserved ability.

[21] We did not exclude the 385 individuals who had some missing ability indicators: We combine all ability measures using principal component analysis, to construct ability measures for these individuals.

[22] We know from earlier studies, e.g. Blackburn and Neumark (1995) or Kjellström (1997), that performance on these kind of tests is correlated with "ability" and probably also with individual returns to education.

	Men		Women	
	Non-reform	Reform	Non-reform	Reform
IQ, Opposites	22.1	22.4	22.2	22.8
	(0.14)	(0.20)	(0.15)	(0.20)
IQ, Folding	21.5	22.3	20.0	20.7
	(0.15)	(0.21)	(0.14)	(0.20)
IQ, Mathematics	19.5	19.8	19.0	19.1
	(0.17)	(0.23)	(0.16)	(0.22)
Reading	37.4	38.1	36.7	37.2
	(0.15)	(0.21)	(0.15)	(0.20)
Writing	50.5	51.0	53.3	53.4
	(0.21)	(0.29)	(0.20)	(0.27)
Mathematics	41.7	41.9	40.0	40.1
	(0.19)	(0.28)	(0.19)	(0.26)
English	5.2	5.4	5.7	5.9
	(0.04)	(0.05)	(0.04)	(0.05)
Share with father's education > basic compulsory	0.145 (0.0057)	0.175 (0.0085)	0.136 (0.0056)	0.177 (0.0087)

Source: IS-suvey, 1948 cohort. Standard errors in parentheses

Table 2: Average test scores. Pre- and post-reform school systems. Men and women. Standard errors in parentheses.

Table 3 compares some characteristics of the city communities and municipalities which were included in the experiment to the excluded ones for the 1948 cohort.[23] To measure average income level we use the per capita income tax base in hundreds of SEK in the municipality or city community. The tax level is the percentage level of the proportional municipality/city community income tax. The data applies to 1960, the year before the tests and interviews for the IS survey were carried out. Data on father's education level is obtained from the survey and measures whether or not the individual's father had more than six or seven years of compulsory schooling.

A larger fraction of the pupils in the reform groups live in Stockholm and in Sweden's second and third cities, Gothenburg and Malmo, compared to the control group. However, since the comparison group includes communities in all these three cities the matching method will control for this. The di®erences in the characteristics within the other city communities and municipalities seem quite modest.[24] The municipalities in

[23] The data on characteristics for each of the 914 municipalities and 123 city communities were matched using the municipality code for area of living included in the survey. The number of municipalities and city communities was reduced from about 2500 to 1037 in 1952.

[24] We cannot make income comparisons etc. for the three big cities since these cities all belong to the same municipality and our available data is at the municipality level. However, parts of these cities were allocated to the control group and part to the treatment.

the experiment are on average somewhat larger than in the control group. We include the municipality characteristics in the propensity score that balances the reform and non-reform sample.

A ¯nal issue relates to the pre-reform years of compulsory education. Some municipalities before the reform had 7 years of compulsory schooling while some had 8. At the start of the experiment in 1949 it was required that the participating municipalities had 8 years of compulsory schooling. However this requirement was abandoned after the ¯rst one or two years of the reform and only a®ected somewhere between 14 and 20 municipalities which corresponds to 2.3% of pupils allocated to the reform for our cohort (see Table 12 in Appendix A).[25] Moreover, survey data suggests that this is not a major problem: The average number of years of schooling for those completing level 1 only (the old compulsory school) in the non-reform areas for our cohort, is 7.8 years.[26] Finally, although we cannot control for this potential problem directly,[27] we do know that municipalities with 7 years of schooling tended to be poorer and rural municipalities or municipalities with low population. By matching on these characteristics we should control for any remaining imbalances in pre-reform schooling between the reform and non-reform samples.[28]

When imputing the average years of schooling for each level of education, we have been slightly conservative by setting compulsory schooling to be 8 years for all. Given the proportion of individuals at the old compulsory schooling (about 20%) this potentially reduces slightly the average impact of the reform on schooling by about two weeks at most.

[25] According to an educational scientist, Mac Murray, who has studied the experiment when it was still evolving, the municipalities did not di®er systematically with respect to pre-reform years of compulsory schooling.

[26] Source: Swedish Level of Living Survey, 1948 cohort.

[27] This would involve searching pre-1949 paper records for each of the old 2500 Swedish municipalities. As we understand it, this is an almost impossible task.

[28] That is, we match on whether the municipality was rural or urban, population size, average income and the local tax rate.

	In the experiment (n=4 084; 34.18%)	Control group (n=7 866; 65.82%)
Share living in Stockholm, %	18.56	4.82
Share living in Gothenburg or Malmo, %	11.36	5.44
Share in cities (not Stockholm, Gothemburg or Malmo), %	44.52	37.01
For cities other than Stockholm, Gothenburg and Malmo		
Average population size	28646	33009
Mean income	49.57	47.13
Mean income tax, %	10.63	11.11
Share living in rural municipalities, %	36.92	58.17
For rural municipalities		
Average population size	7751	5750
Mean income	33.04	30.97
Mean income tax, %	10.56	9.48

Note: Characteristics of the city communities and municipalities obtained from o±cial statistics on each area (Source: Årsbok fÅr Sveriges kommuner 1960 and 1961)

Table 3: Comparison between treatment and control municipalities.

6.2. The E®ect of the Reform on Education.

A change in the education system as radical as the 1950 reform could a®ect schooling in a number of ways. There is a direct e®ect due to the increase in the amount of compulsory schooling, potentially a®ecting about 25% of the individuals in the male sub-sample and about 20% among the females. Then there is the set of measures designed to facilitate the transition to higher education, including the abolition of selection at 12 years of age.[29] The curriculum in the level 3 (Upper secondary school **6** 2 years) became more academic, making the transition to the Upper secondary school easier. Finally, a means-tested stipend was introduced to \compensate" disadvantaged families in the reform areas who had to send their children to school longer. The means tested stipend was provided only up to the end of the new compulsory school (see Appendix A for the amounts involved).

The reform may have also a®ected educational attainment through general equilibrium e®ects Lang and Kropp (1986) for example ¯nd evidence that di®erences in compulsory attendance laws in US states a®ect enrollment rates to education levels not directly a®ected by the laws. The mechanism could operate through wages or through a reduction in the signalling value of lower education levels (as the authors argue). In the Swedish case these GE e®ects are likely to a®ect both reform and non-reform areas in a similar way since a) the reform and non-reform municipalities co-exist in the same labor market (i.e. the county) and b) the new system was expected to be implemented nationally. The e®ects we measure should be interpreted as impacts on individuals given the aggregate impact of the experiment and given the expectation that the reform would be implemented nationally, with a resulting increase in educated workers, as we will now show.

Table 4 shows the share of individuals in the di®erent education levels by reform status and gender as well as the implied di®erence between the two. In the

[29] There is a sociological literature on how delayed streaming can a®ect educational choice education, attenuating the e®ects of social background, (see e.g. Erikson and Jonsson, 1993).

fourth column we report the results obtained by propensity score matching for each education level. The propensity score[30] used to balance the reform and non-reform samples includes the county of schooling (which includes both reform and non-reform municipalities),[31] 44 ability indicators, indicators of father's education and characteristics of the municipality, including aggregate income, population size, the local tax rate and whether it is a city, a rural community or other. We report 95% bias corrected bootstrap confidence intervals and the standard deviation of the bootstrap. In computing these we allow for the fact that the propensity score is estimated. As suggested by the bootstrap theory (see Horowitz, 1999) we use the 95% confidence intervals to judge whether an effect is significant. We also report the percentage of matched observations in the reform sample as well as the average number of times that an observation in the control sample was used.[32]

The largest impact of the reform was to shift those who would have stopped at the old compulsory level (basic school) to the new compulsory level (comprehensive school). For the matched sample the proportion stopping at education level 2 (which pre-reform was the Junior secondary school and post reform constituted the new comprehensive school) increased by 10 percentage points for males and by 8 percentage points for females. The importance of controlling for the observable differences in the characteristics of those in the reform and those not is apparent when we compare this result to the unmatched change, which is larger.

As can be seen from the table a small proportion of individuals assigned to the reform completed the pre-reform basic school only, "evading" the increased compulsory schooling level. These persons could have moved out of the municipality included in the

[30] Probability of being assigned to the reform.

[31] A county is an administrative area containing a number of municipalities. There are 24 counties in Sweden.

[32] In actual fact all observations were used in the estimation since we smooth before we match. The total number of men and women in our sample is respectively The way report the numbers in the table shows that there is no serious support problem between the reform and non-reform municipalities. The median number of times that an observation in the control sample was used was always one. However, a few observations were used quite frequently.

experimental group after age 12 when the data on reform assignment was collected, or they could have obtained an exemption from completing the extra compulsory schooling. This highlights the importance of measuring the reform assignment, as we do, before endogenous switching is likely to occur.

At the bottom of the Table we summarize the impact of the reform on post comprehensive education (levels 3 to 7), access to which the reform was intended to improve. There we see that there is no signi¯cant overall increase in post-compulsory education for men - rather there seems to have been some reallocation between types of post compulsory education: For men we observe a marginally signi¯cant 1.7% point decline in level 3 and an increase of 2.4 percentage points in level 5 (post upper secondary · 2 years) which is consistent with the improved access provided by the reform to the more academic types of education.

For females there is a 3.9% points increase in level 3 which is signi¯cant. In contrast to men the reform seems to have caused an overall signi¯cant increase in post compulsory schooling (levels 3-7) of 3% percentage points.

Finally, accounting for all changes implied by the reform male years of education increased by 0.29 of a year and females years increased overall by 0.17 of a year.

As implied by the simple model we presented, the reform is likely to have had di®erent impacts depending on individual ability and family background. In what follows, high ability individuals are de¯ned as those with the ¯rst principal component of our 44 ability indicators above the median (see Appendix B for details). We associate parental background with father's education and we de¯ne \low father's education" as those individuals whose fathers completed only the statutory level of education (also referred to as unskilled). The rest are referred to as \high father's education" or skilled.

The results from the analysis by group is based again on propensity score matching within each group including the same variables as before. These results are shown in Table 5 for males and in Table 6 for females.

Education level	Males				Females			
	Non-Reform	Reform	Change	Change (Matched)	Non-Reform	Reform	Change	Change (Matched)
1. Basic School	0.243	0.046	-0.197	-0.109	0.199	0.034	-0.165	-0.109
	(0.007)	(0.005)	(0.011)	(0.01)	(0.006)	(0.004)	(0.009)	(0.0102)
				[-0.12,-0.096]				[-0.12,-0.084]
2. Comprehensive/	0.093	0.217	0.124	0.101	0.112	0.228	0.116	0.079
Junior Secondary	(0.005)	(0.009)	(0.010)	(0.013)	(0.005)	(0.009)	(0.010)	(0.015)
				[0.07,0.12]				[0.045,0.10]
3.Upper secondary	0.268	0.294	0.025	-0.017	0.349	0.357	0.008	0.039
school 6 2 years	(0.007)	(0.010)	(0.012)	(0.017)	(0.008)	(0.010)	(0.013)	(0.017)
				[-0.047,0.006]				[0.0071,0.070]
4. Upper Secondary	0.169	0.189	0.020	0.001	0.083	0.085	0.002	-0.016
school > 3 years	(0.006)	(0.009)	(0.011)	(0.015)	(0.004)	(0.006)	(0.007)	(0.012)
				[-0.030,0.027]				[-0.045,0.004]
5. Post Upper Secon-	0.070	0.082	0.013	0.024	0.119	0.134	0.015	-0.005
dary 6 2 years	(0.004)	(0.006)	(0.007)	(0.009)	(0.005)	(0.007)	(0.009)	(0.014)
				[0.005,0.042]				[-0.037,0.020]
6. University/College	0.147	0.161	0.014	0.00	0.134	0.159	0.026	0.0204
	(0.006)	(0.008)	(0.010)	(0.013)	(0.005)	(0.008)	(0.009)	(0.013)
				[-0.024,0.023]				[0.004,0.053]
7. Ph.D.	0.010	0.011	0.002	0.001	0.005	0.003	0.002	-0.009
	(0.002)	(0.002)	(0.003)	(0.004)	(0.001)	(0.001)	(0.002)	(0.005)
				[-0.007,0.009]				[-0.028,-0.001]
More than	0.664	0.737	0.073	0.009	0.689	0.738	0.0488	0.030
Comprehensive or	(0.0077)	(0.01)	(0.013)	(0.015)	(0.0077)	0.0100)	(0.0128)	(0.018)
Junior Secondary				[-0.018 0.030]				[-0.001 0.066]
Years of educ.	11.14	11.84	0.70	0.29	11.16	11.77	0.61	0.17
	(0.051)	(0.062	(0.081)	(0.100)	(0.050)	(0.061)	(0.079)	(0.11)
				[0.006, 0.41]				[-0.04, 0.39]
Sample size				5744				5540
Percent in reform				34%				35%
Percent matched				100%				100%
Average number of times controls used				1.9				2

Notes: Standard errors in parentheses. Bias corrected 95% bootstrap con¯dence interval in square brackets.

Source: IS Survey, 1948 cohort.

Table 4: The impact of the reform on educational quali¯cations.

Form men, the largest and most significant impact of the reform on educational attainment is concentrated at the bottom of the education distribution for individuals with unskilled parents (Table 5). Moreover, as expected the impact is largest for the low ability individuals: Their optimal level of education is likely to be lower and hence there is a higher concentration of them at the old compulsory school in the absence of the reform. In terms of years of education, for the children of unskilled parents the implied increase for those with low ability is 0.45 of a year and for those with high ability 0.21. Comparing the high ability individuals in the two parental education groups we see a significant shift from the old to the new compulsory schooling level for the children of the unskilled parents and no such impact for the children of high education parents of similar ability.

When we compare those with low ability in the two parental education groups again the impact is larger for the children of the unskilled parents. In terms of increases in post compulsory schooling, there is an increase of 4.6 percentage points for the low ability children of unskilled parents. The effect points to a relative success of the policy of removing early selection, as far as educational attainment is concerned, but is not very precisely estimated. Finally for the children of skilled parents with high ability there is a decline in level 4 and an increase in University attendance (level 6). This gives rise to a large but very imprecisely (and insignificant) estimated increase in the years of education for this group.

For women the impact of the reform is very high at the bottom of the distribution for those with low ability and unskilled fathers. The impact for high ability women (whether with unskilled fathers or skilled ones) is small and insignificant around the new compulsory schooling level (level 2 - comprehensive). However, in line with the aims of the reform, there is a significant increase in post compulsory schooling for the high ability women with unskilled parents of the order of 4%. Overall it all adds up to the same impact for the years of education for the two groups (about 0.26 and 0.29 of

Father's education	Low	Low	High	High
Ability	High	Low	High	Low
Education level				
1. Basic School	-0.088	-0.199	-0.002	-0.018
	(0.016)	(0.020)	(0.008)	(0.023)
	[-0.12,-0.06]	[-0.23,-0.14]	[-0.057,0.0]	[-0.11,0.015]
2. Comprehensive/	0.062	0.153	0.013	0.098
Junior Secondary	(0.021)	(0.023)	(0.021)	(0.042)
	[0.02,0.11]	[0.10,0.19]	[-0.028,0.050]	[-0.004,0.17]
3. Upper Secondary	-0.007	0.003	-0.002	-0.032
school 6 2 years	(0.027)	(0.028)	(0.033)	(0.073)
	[-0.09,0.03]	[-0.057,0.053]	[-0.062,0.05]	[-0.16,0.10]
4. Upper Secondary	0.030	0.017	-0.17	-0.12
school > 3 years	(0.025)	(0.020)	(0.073)	(0.077)
	[-0.014,0.077]	[-0.021,0.059]	[-0.32,-0.021]	[-0.32,-0.018]
5. Post Upper Secon-	0.017	0.014	-0.004	0.046
dary 6 2 years	(0.018)	(0.021)	(0.053)	(0.053)
	[-0.020, 0.05]	[-0.012, 0.042]	[-0.16, 0.08]	[-0.066, 0.14]
6.University/College	-0.022	0.014	0.12	0.026
	(0.023)	(0.011)	(0.081)	(0.067)
	[-0.062,0.25]	[-0.012, 0.033]	[-0.08, 0.26]	[-0.12, 0.14]
7. Ph.D.	0.008	-0.001	0.029	0
	(0.006)	(0.002)	(0.030)	(0.013)
	[-0.004, 0.021]	[-0.009, 0]	[-0.03, 0.09]	[-0.009, 0.03]
More than Comprehensive/	0.026	0.046	-0.011	-0.080
Junior Secondary	(0.026)	(0.028)	(0.023)	(0.043)
	[-0.036, 0.07]	[-0.012, 0.095]	[-0.048, 0.036]	[-0.16, 0.016]
Years of Education	0.21	0.45	0.69	-0.048
	(0.18)	(0.13)	(0.37)	(0.4)
	[-0.11,0.61]	[0.09,0.64]	[-0.12,1.3]	[-0.76, 0.76]
Sample Size	2421	2479	611	233
Percent in reform	33%	33%	38%	43%
Percent matched	97%	91%	92%	94%
Average times controls used	1.94	1.94	2.1	2.2

Notes: Standard deviation of the bootstrap in round brackets. Bias corrected 95 % bootstrap con¯dence interval in square brackets.

Source: IS Survey, 1948 cohort.

Table 5: The impact of the reform on educational quali¯cations by father's education and ability. Males.

a year respectively). An interesting result is the large increase in level 3 for women of low ability with skilled parents. The bootstrap con¯dence intervals indicates that it is very robust, although the sample size is quite small. This positive e®ect also shows up in the overall sample as seen in Table 4. As we noted above these ripple or \knock on e®ects" probably re°ect the impact of the abolition of early selection in the reform municipalities.

To summarize, the results show a signi¯cant impact of the reform at the bottom end of the education distribution for children from low skilled parents and in particular for the lower ability ones. The reform also a®ected the level of post compulsory schooling and in particular for high ability women with unskilled parents. Although the reform did increase educational attainment for high ability men of unskilled parents the most pronounced e®ects for men are among the low ability ones. Thus the results highlight the importance of both ability and parental background. While the former re°ects the fact that lower ability persons have less to gain (relative to opportunity costs) by more education, the importance of parental background, even for high ability people may be an indication of informational or ¯nancial constraints for this group. We no turn to the impact of the reform on earnings.

6.3. The e®ect of the Reform on Earnings

In Table 7 we present estimates of the impact of the reform on log-earnings for both men and women, based on propensity score matching. We also present the standard deviation of the bootstrap and bias corrected 95% bootstrap con¯dence intervals.

Our ¯rst estimates relate to the entire sample and they represent the average e®ect on log real earnings over the 12 years of earnings data for those who went through the reform system. The individuals were aged between 37 in 1985 and 58 in 1996.[33] These results show positive e®ects for both men (1.1%) and women (3.3%). The e®ect for men is insigni¯cant while for women it is signi¯cant at the 10% signi¯cance level.

[33] We have excluded the individuals who did not have any earnings for the whole year.

Father's education	Low	Low	High	High
Ability	High	Low	High	Low
Education level				
1. Basic School	-0.042	-0.212	-0.001	-0.02
	(0.011)	(0.023)	(0.002)	(0.027)
	[-0.06,-0.018]	[-0.25,-0.17]	[-0.009,0.0]	[-0.088,-0.001]
2. Comprehensive/	0.002	0.187	0.044	-0.01
Junior Secondary	(0.024)	(0.026)	(0.027)	(0.072)
	[-0.08,0.037]	[0.14,0.24]	[-0.013,0.094]	[-0.08,0.08]
3. Upper Secondary	0.030	0.019	-0.084	0.27
school $\leq$ 2 years	(0.025)	(0.030)	(0.048)	(0.077)
	[-0.007,0.081]	[-0.03,0.08]	[-0.21,-0.020]	[0.12,0.43]
4. Upper Secondary	-0.040	0.016	-0.039	-0.095
school $>$ 3 years	(0.020)	(0.012)	(0.061)	(0.06)
	[-0.084,0.007]	[-0.014,0.037]	[-0.14,0.092]	[-0.24,-0.03]
5. Post Upper Secon-	0.0	-0.009	0.022	-0.086
dary $\leq$ 2 years	(0.023)	(0.021)	(0.066)	(0.079)
	[-0.048,0.041]	[-0.056,0.024]	[-0.13,0.13]	[-0.29,0.011]
6.University/College	0.058	0.00	0.069	-0.051
	(0.022)	(0.01)	(0.084)	(0.073)
	[0.0191,0.103]	[-0.022,0.016]	[-0.078,0.24]	[-0.21,0.085]
7. Ph.D.	-0.012	0	-0.012	-
	(0.006)		(0.028)	-
	[-0.034,-0.003]		[-0.07,0.034]	
More than Comprhensive/	0.039	0.025	-0.044	0.033
Junior Secondary	(0.024)	(0.030)	(0.078)	(0.073)
	[0.0004, 0.11]	[-0.027 0.079]	[-0.09 0.013]	[-0.064 0.089]
Years of Education	0.29	0.26	0.17	-0.60
	(0.17)	(0.14)	(0.41)	(0.51)
	[-0.06, 0.64]	[-0.049, 0.51]	[-0.49,1.1]	[-1.1,0.86]
Sample Size	2338	2408	575	219
Reform observations used	33%	35%	38%	44%
Percent matched	100%	94%	98%	93%
Average times controls used	2	1.9	1.7	2.3

Notes: Standard deviation of the bootstrap in round brackets. Bias corrected 95 % bootstrap confidence interval in square brackets.

Source: IS Survey, 1948 cohort.

Table 6: The impact of the reform on educational qualifications by father's education and ability. Females.

The low estimated e®ect overall is not that surprising at this aggregate level since the main impacts of the reform were on the educational achievement of individuals from unskilled parents. Thus we repeat the matching exercise for the di®erent subgroups based on ability and father's education as before.

The results are presented in Table 7. We ¯nd that the highest e®ects of the reform on earnings are among those with unskilled fathers and high ability. Within the low parental education group, for high ability men the estimated impact is nearly four times that of the low ability men. For high ability women the impact of the reform is nearly twice that of the low ability women. This is consistent with the idea that high ability individuals, who were induced to have more education under the reform, had faced barriers in the old system. Under the reform system they obtained more education and bene¯ted from it. Recall that the reform acted through increases in compulsory schooling, through the abolition of selection, and was accompanied by a means tested stipend for those in the reform municipalities. This is important because it reduced the incentive to ¯nd ways of avoiding compulsory schooling by, say, switching municipality and it may have helped ¯nance the post-compulsory increases in schooling that we observed.

It is unclear how the reform could have a®ected those with skilled parents, since there is no ¯rm evidence that it a®ected much their educational outcomes. The other channels would have been the removal of streaming (tracking) or other more subtle changes that may have taken place. The removal of streaming and early selection allowed lower achieving children to mix with potentially higher achieving ones, possibly reducing the performance of the latter. Unfortunately we cannot provide conclusive evidence of such an e®ect. Although the point estimates for individuals with highly educated fathers are negative, the results are far too imprecise, based on the bootstrap con¯dence interval, to make any inference.

In conclusion, there is ¯rm evidence that the reform had a positive impact on the earnings of those from poorer backgrounds with high ability. The results for men and women are similar. However the impact on female earnings seems to have been somewhat higher overall.

	Males	Females
All	0.0113	0.033
	(0.018)	(0.025)
	[-0.025 0.046]	[-0.008 0.092]
Low Fathers Education	0.0164	0.0338
Low Ability	(0.027)	(0.027)
	[-0.038 0.071]	[-0.01 0.0.091]
Low Fathers Education	0.0605	0.0597
High Ability	(0.034)	(0.037)
	[0.013 0.150]	[-0.01 0.140]
High Fathers Education	-0.0640	-0.043
Low Ability	(0.105)	(0.12)
	[-0.29 0.10]	[-0.21 0.11]
High Fathers Education	-0.0880	-0.038
High Ability	(0.105)	(0.085)
	[-0.30 0.17]	[-0.20 0.14]

Note: Results obtained by propensity score matching
Standard deviation of the bootstrap in round brackets. Bias corrected 95% bootstrap con¯dence interval in square brackets.

Table 7: The impact of the reform on Earnings.

6.4. The E®ect of Education on Earnings

We now turn to the estimation of the returns to education. Apart from the direct interest in measuring the returns to education we are also able to investigate whether the reform acted exclusively through the impact on the quantity of education or whether it had a direct impact as well. Finally, we are able to investigate whether the returns to education vary by ability.

We present both OLS and IV results. All regressions use log annual earnings as the dependent variable and include time dummies, the county of schooling and the county of residence. In all cases we include the complete set of ability indicators except for the ¯rst OLS regression for men and women where we exclude them. In all cases we present asymptotic standard errors that are robust for heteroskedasticity and arbitrary serial correlation.

The OLS results are interesting in themselves since it is quite possible that education can be taken as exogenous, given the large set of observable ability indicators and parental background variables. This would require that individuals do not self-select on the basis of their unobserved returns to education when choosing education choice (possibly because they do not know them at that time). It also requires that unobserved earnings ability is mean independent of education choice given observable characteristics.

The OLS results are presented in Table 8 for males and in Table 9 for females. In the upper panels of Tables 8 and 9 we present OLS results pooled over all parental background groups. In the first column, where we exclude the ability variables the returns to years of education are estimated at 6.4% annually for men and 5.2% for women. When we control for ability the male returns drop to 5.4% and female returns drop to 4%. Finally when we allow the returns to vary by ability we find that the returns to the low ability men are 4.3% and 3.5% for low ability women, while for the high ability men these are as high as 6% conditional on observed ability. For high ability women the OLS returns are 4.3%. In the last three columns of Tables 8 and 9 we include the reform indicator which turns out to be insignificant in all cases.

In the lower panels of Tables 8 and 9 we estimate the returns to education, using OLS, separately by father's education and by ability group. The results are very similar to the previous ones with one notable exception: The reform indicator has a positive and marginally significant impact on the earnings of high ability men with unskilled parents. This is an interesting result since this subpopulation would have benefited potentially by the switch to a comprehensive system (which would improve the peer group for them) and by the more academic curriculum. Unfortunately we will not be able to confirm these results using IV, since for this group there are no differential effects of the reform on education across municipalities. Interestingly the point estimates for the reform dummy for the children in the higher parental education group are negative. This could indicate that children from better off backgrounds were

	Men - Ordinary Least Squares						
	All Parental Educations						
	No Ability	All Abilities	Low Ability	High Ability	All Abilities	Low Ability	High Ability
Years of schooling	0.064	0.054	0.043	0.060	0.053	0.043	0.060
	(0.0025)	(0.0032)	(0.005)	(0.004)	(0.003)	(0.005)	(0.004)
Reform					0.021	0.011	0.036
					(0.018)	(0.026)	(0.026)
Obs. (N £ T)	58912	58912	28604	30308	58912	28604	30308

Heteroskedasticity and serial correlation robust asymptotic standard errors in parentheses.

	Men - Ordinary Least Squares							
	Low Father's Education				High Father's Education			
	Low Ability	High Ability	Low Ability	High Ability	Low Ability	High Ability	Low Ability	High Ability
Years of schooling	0.042	0.058	0.041	0.058	0.051	0.051	0.050	0.052
	(0.006)	(0.0046)	(0.006)	(0.005)	(0.015)	(0.007)	(0.015)	(0.007)
Reform			0.019	0.057			-0.052	-0.054
			(0.028)	(0.030)			(0.085)	(0.043)
Obs. (N£T)	26318	23755	26318	23755	2357	6609	2357	6609

Heteroskedasticity and serial correlation asymptotic standard errors in parentheses. All regressions include ability variables.

Table 8: OLS estimates of the returns to years of education. Men.

harmed by the other aspects of the reform, such as the abolition of tracking by ability; nevertheless the e®ect is insigni¯cant.

As is well understood, the OLS results may su®er from selection/endogeneity bias since education choices may be correlated with unobserved ability. We use the education changes induced by the reform to estimate the returns to education which in this case are interpretable as LATE e®ects as discussed earlier in the paper.

In Table 10 we present the estimates for males. The excluded instruments are the interactions of the reform with the county of schooling as well as parental education (low/high). County of schooling dummies are included in the regression. In the ¯rst three columns of the top panel and the ¯rst two of the lower panel we also exclude

Women - Ordinary Least Squares							
	All Parental Educations						
	No Ability	All Abilities	Low Ability	High Ability	All Abilities	Low Ability	High Ability
Years of schooling	0.052	0.040	0.035	0.043	0.040	0.035	0.043
	(0.002)	(0.0024)	(0.0036)	(0.0032)	(0.0024)	(0.0036)	(0.0032)
Reform					0.006	0.0037	0.011
					(0.015)	(0.020)	(0.021)
Obs. (N £ T)	56178	56178	27493	28685	56178	27493	28685

Heteroskedasticity and serial correlation robust asymptotic standard errors in parentheses.

Women - Ordinary Least Squares								
	Low Father's Education				High Father's Education			
	Low Ability	High Ability	Low Ability	High Ability	Low Ability	High Ability	Low Ability	High Ability
Years of schooling	0.037	0.041	0.037	0.041	0.056	0.049	0.056	0.049
	(0.004)	(0.003)	(0.004)	(0.003)	(0.018)	(0.009)	(0.018)	(0.009)
Reform			0.006	0.023			0.01	-0.023
			(0.022)	(0.024)			(0.098)	(0.046)
Obs. (N £ T)	25206	22712	25206	22712	2303	5997	2303	5997

Heteroskedasticity and serial correlation robust asymptotic standard errors in parentheses.

Table 9: OLS estimates of the returns to years of education. Women.

the reform indicator itself. In the lower panel of Table 10 we estimate the model using IV on the sample of people with unskilled parents, thus relaxing the assumption that parental education can be excluded. In the last two columns of that Table we also include the reform indicator as a regressor, providing the least restricted estimates.

The results in Table 10 imply that the IV returns to education on wages are twice as high for the high ability people. This remains true when we control for the reform indicator which is insigni¯cant and very small. When we estimate the model using the children of unskilled parents only (lower panel of Table 10) we obtain a similar picture except that the returns for higher ability men are now lower (5.2% versus 7.3%). Note however, that for the high ability people the instrument is rather weak in the reduced form for education with a p-value of 7.5%. Since the reform had little or no signi¯cant impact on the educational attainment of the individuals with skilled parents we do not present IV results for that group separately.[34]

Recalling the interpretation of IV as a LATE parameter, note that this parameter is not generally invariant to the instrument used, even when the instrument is independent of the unobservables. Thus when we use parental background as an instrument,[35] in conjunction with the reform indicator, the return we estimate is a weighted average of the returns for those individuals who acquired extra schooling because their parents were more skilled (and who would otherwise have obtained less schooling) and of the returns for those induced into higher levels of education due to the reform. These returns may or may not be the same. The point estimates indicate that they may not be, although the di®erences are not signi¯cant.

When we include the reform indicator in the regression and using as instruments the interactions of the reform indicator with the county of schooling the point estimates indicate a positive e®ect of the reform and an even lower return to education. However, the lack of precision prevents us from drawing any ¯rm conclusions. This is despite

[34]See Bound, Jaeger an Baker (1995) and Staiger and Stock (1997) on the importance of using instruments with signi¯cant explanatory power.

[35]Even assuming that it is conditionally independent of the unobservabls in the earnings equation.

Men - Instrumental Variables						
				All Parental Educations		
	All Abilities	Low Ability	High Ability	All Abilities	Low Ability	High Ability
Years of schooling	0.065	0.037	0.073	0.065	0.036	0.072
	(0.009)	(0.015)	(0.011)	(0.009)	(0.017)	(0.011)
Reform				-0.002	0.002	0.009
				(0.014)	(0.022)	(0.020)
P-value - exlusions in the reduced form	0.0	0.0	0.0	0.0	0.0	0.0
Obs. (N£T)	58912	28604	30308	58912	28604	30308

Heteroskedasticity and serial correlation robust asymptotic standard errors in parentheses.
Included regressors: County of residence,county of schooling, year dummies and ability indicators.
Excluded Instruments: Reform (¯rst three columns), parental education, interactions of reform with county of schooling.

Men - Instrumental Variables				
			Low Father's Education	
	Low Ability	High Ability	Low Ability	High Ability
Years of schooling	0.034	0.052	0.018	
	(0.023)	(0.030)	(0.033)	
Reform			0.021	
			(0.030)	
P-value for exluded instruments	0.0	0.075	0.033	0.19
Obs. (N £ T)	26318	23755	26318	23755

Heteroskedasticity and serial correlation robust asymptotic standard errors in parentheses. Included regressors: County of residence, county of schooling, year dummies and ability indicators. Excluded Instruments: Reform (¯rst two columns) and interactions of reform with county of schooling.

Table 10: IV estimates of the returns to years of education. Men.

the fact that the excluded instruments are very significant in the reduced form. We do not report the results on the high ability individuals since the excluded instruments had little explanatory power (p-value 0.19).[36]

The IV results are generally lower than the OLS ones for the lower ability persons and higher for the higher ability ones. Nevertheless, the differences between OLS and IV estimates are not significant.

Turning now to the results for women, which are presented in Table 11, the overall picture is the same, except when we condition on the individual having an unskilled father, in which case the returns become much higher. The results, in the lower panel of Table 11 imply that the returns for high ability women, induced into more schooling because of the reform, are nearly twice as high as the returns for low ability women induced into more schooling. Moreover, the returns are higher than the returns obtained using OLS.

As shown in the lower panel of Table 11, for both ability groups the direct impact of the reform is small and insignificant. Moreover, the estimated returns to education remain unaffected by the inclusion of the reform indicator for the low ability people. Again for the high ability people this is not identified, with the instruments having a 0.57 p-value in the reduced form.

Thus to summarize, when we use parental background as an instrument as well as the reform assignment, we find that the reform had no direct influence on earnings other than the impact it had through the increase in the quantity of education. When we look at the outcomes for children whose parents were unskilled, we reach the same conclusion, although with less precision. However, we have not been able to identify returns to education and reform effects separately for children with high ability and unskilled parents or for children from skilled parents. The switch to a comprehensive system could have had an effect on their performance, but we can offer no evidence

[36] This is because there were no significant differences in the impact of the reform across counties for these individuals.

Women - Instrumental Variables						
	All Parental Educations					
	All Abilities	Low Ability	High Ability	All Abilities	Low Ability	High Ability
Years of schooling	0.043	0.027	0.054	0.042	0.023	0.054
	(0.010)	(0.015)	(0.013)	(0.011)	(0.016)	(0.014)
Reform				0.006	0.024	-0.005
				(0.014)	(0.022)	(0.020)
P-value for exluded instruments	0.0	0.0	0.0	0.0	0.0	0.0
Obs. (N£T)	56178	27493	28685	56178	27493	28685

Heteroskedasticity and serial correlation robust asymptotic standard errors in parentheses in parentheses.
Included regressors: County of residence,county of schooling of schooling, year dummies and ability indicators.
Excluded Instruments: Reform (¯rst three columns), parental education, interactions of reform with county of schooling.

Women - Instrumental Variables				
	Low Father's Education			
	Low Ability	High Ability	Low Ability	High Ability
Years of schooling	0.058	0.091	0.049	
	(0.025)	(0.033)	(0.033)	
Reform			0.013	
			(0.027)	
P-value for exluded instruments	0.0	0.039	0.048	0.57
Obs. (N £ T)	25206	22712	25206	22712

Heteroskedasticity and serial correlation robust asymptotic standard errors in parentheses. Included regressors: County of residence, county of schooling of schooling, year dummies and ability indicators. Excluded Instruments: Reform (¯rst two columns) and interactions of reform with county of schooling.

Table 11: IV estimates of the returns to years of education. Women.

on that other than when we pool the children from different parental backgrounds.

We compare the returns to education for high and low ability individuals. This is possible either when we exclude parental education from wages and use it as an instrument or alternatively when we assume that the effect of the reform only went through the quantity of education. In this case the estimated LATE effect of education on wages is higher for the high ability persons with both the alternative instrument sets.[37] This is true for both men and women and suggests that the high ability children of unskilled parents benefited a great deal from the positive impact of the reform on educational attainment, and probably faced barriers to education before the reform.

Finally, in all IV regressions we include ability variables, county of residence and county of schooling. However, in the results we present we do not include the municipality characteristics (either as regressors or as instruments), as we did in the matching section. When we did this the results in which we use parental education as an instrument remain unaffected. Moreover all the results for women that we report also do not change. However the IV results for men with unskilled parents become very imprecise. In this sense the results on the returns to education for women are more robust than those for men.

7. Conclusions

In this paper we evaluate the impact of a major reform to the Swedish education system. This reform had a number of elements that have either been implemented or are being discussed in many countries. It increased compulsory schooling and introduced a comprehensive school system that was not based on selection by ability into different streams (tracking), as the old system was. Finally, it introduced means tested subsidies for education. The reform was preceded by a unique social experiment where the new comprehensive school was implemented in a number of municipalities at the same time as other municipalities were still operating the old system. Thus we are in the unique position to evaluate a reform of broad interest using exceptional

[37] Including and excluding parental background in wages.

data.

We consider the impact of the reform on educational achievement and on earnings. We ¯nd that the reform raised the overall educational achievement The e®ect was concentrated on those whose father was unskilled and mainly towards the lower part of the education distribution. Nevertheless we do ¯nd evidence that the reform increased educational attainment beyond the new compulsory schooling level, pointing to positive impacts of improving access to higher education levels by abolishing early selection. Within that group most of the impact was on those with lower ability. However when we look at earnings we ¯nd that the largest impact of the reform by far, is on the higher ability men and women, with unskilled fathers. The impact on the lower ability men and women is much smaller.

In the subsequent part of the paper we use the assignment to the reform as an instrument for education in an earnings equation. We argue that reform assignment is a valid instrument, particularly given the large array of characteristics that we observe. We interpret the returns we estimate as the impact of extra education for those induced to it due to the reform (LATE). We show that these returns to education are higher for the high ability individuals, for both men and women. We also present evidence that the reform a®ected earnings only through the increased quantity of education and not directly, at least for some groups as well as on average for the whole a®ected population. However, we have no evidence (other than from OLS) on this, speci¯cally for the children of skilled parents.

Whether the estimated impacts of the reform justify the costs cannot be evaluated from our data. A full cost-bene¯t analysis of the Swedish education would be a worthwhile exercise. However, in this paper we show that the reform entailed substantial gains for children from poorer backgrounds.

Appendix A. The 1950 Education Reform

A1. Background to the Reform

The motivation for the reform was partly to catch up with countries such as the US where high school enrolment was above 80% in the 1940s (see Goldin, 1999) and partly to respond to the increased demand for resources in education. The share of students who joined junior secondary school increased from about 10 percent in 1930 to about 40 percent in 1950 (see Erikson & Jonsson, 1993).

The main controversy in the political debate preceding the reform was between those in favor of a comprehensive school and those who wanted to maintain parallel school systems with a vocational and academic track, based on early selection. A comprehensive school was seen as more \democratic" (see e.g. Paulston, 1968, or Myrdal, 1939) and the expansion of the US high school system was used as a model. The old system, however, was seen by some as providing better preparation for higher education.

The school committee's proposal in 1948 was to replace the old compulsory and junior secondary school with a nine year compulsory comprehensive school. The compromise for those who wanted to maintain the parallel school systems was that the students were able to choose between three di®erent levels after sixth grade: one with a more academic curriculum, one general level and one level which included vocational training. The new comprehensive system was to use teachers who specialized in particular areas.

The reform prolonged compulsory schooling to nine years. There was a centrally decided curriculum for all schools, which di®ered from the pre-reform compulsory schools where the curriculum was decided by the municipalities.

A2. The Evolution of the Experiment

The nation wide experiment with the new comprehensive school started in 1949 and continued until 1962. In the experiment, the proposed comprehensive school was implemented in entire municipalities or parts of city communities, rather than in separate schools or classes. In some cases, the reform was implemented for the incoming 1st grade cohort of students only, while in others it was implemented for students in the fifth grade at the date of assignment. No pupil was switched system after the 5th grade: If they started in the old system they would complete schooling under the rules of the old system.

Municipalities were selected by a committee to implement the reform from a pool of applicants. In the first one or two years of the experiment (i.e. 1949) the 14 selected municipalities fulfilled two main requirements: (1) The length of the compulsory schooling was 8 rather than 7 years in the municipality. (2) The demographic structure of the municipality permitted a continuous flow of pupils into the new school system. These requirements were abandoned after the first one or two years allowing all types of municipalities to be assigned to the reform. We discuss this issue further at the end of section 6.1. Moreover the regrouping of the municipalities in 1952 from about 2500 to 1037 made the second requirement probably irrelevant. Finally it should be noted that many municipalities were being rejected by the committee,[38] which was always attempting to obtain a representative sample. Table 12 shows the development of take-up of the experiment between 1949 and 1962.

A3. Reform Implementation and Financial Incentives for Pupils

Those running the experiment recognized that the increase in compulsory schooling would lead to potentially serious financial burdens on lower income families. Thus a non-taxable universal child allowance of 260 SEK per year and per child (less than 16) was introduced in 1948.[39] This allowance was increased to 400 SEK by the end of the experiment. Second, in 1953, when the first cohort included in the experiment reached 9th grade, means tested stipends were introduced, based on family income. The maximum amount of these stipends were 75 SEK per month in 9th grade and somewhat lower in 8th grade.

A4. Pupil Compliance

[38] For evidence on this up to 1957 See Försöksverksamhet med nioårig skolplikt, 1959.

[39] To get a sense of the importance of these amounts, in 1948 the average annual gross earnings for a male worker amounted to 5,892 SEK and 3,883 for a female worker (Statistisk Årsbok för Sverige, 1950).

Year	Municipalities Cumulative Number	Percentage share	Number of classes	Number of students
1949/50	14	1.3	172	2 483
1950/51	20	1.9	379	7 529
1951/52	25	2.4	682	14 635
1952/53	30	2.9	1 009	22 725
1953/54	37	3.5	1 525	35 784
1954/55	46	4.4	2 516	61 498
1955/56	59	5.6	3 394	84 941
1956/57	71	6.7	4 393	109 694
1957/58	96	9.1	5 702	143 370
1958/59	142	13.5	8 036	196 343
1959/60	217	20.6	11 191	266 042
1960/61	295	28.0	14 283	333 094
1961/62	415	39.4	18 665	436 595

Note: The 1952 division of municipalities (total: 1 052). Source: Marklund (1981).

Table 12: Quantitative development of the comprehensive school experiment 1949 to 1962. The row relevant for the year of the survey (1960/61) for the data used in our main emirical analysis is marked with bold.

We now consider two forms of non-compliance. Due to the nature of the data, neither compromises the evaluation, as we explain below.

It was possible to obtain an exemption from the prolonged compulsory school after 8th grade for special reasons. This will be apparent in our data when even in the reform areas some children left school after eight years. The 1957 evaluation of the experiment showed that about 7.5 percent in 1957 and about 4.5 percent in 1958 used that possibility and quit after 8th grade.[40]

An alternative form of non-compliance was to move municipalities. This was considered to be a problem in the 1957 evaluation at the early phase of the experiment; in particular up to 20% of pupils who wanted to continue with a selective schooling were thought to switch municipalities after 6th grade.[41] However, there is little indication of this based on the number of pupils progressing from sixth grade to the

[40] see page 52 in Försöksverksamhet med nioårig skolplikt, 1959

[41] See page 235 in Försöksverksamhet med nioårig skolplikt, 1959

comprehensive schools in the reform areas for the later part of the experiment, which concerns our cohort, when the new system had become more prevalent (see Pupils in Compulsory Schools in Sweden 1847-1962, 1974).

Neither of these compliance problems seriously a®ects our ability to evaluate the reform: We observe the municipality of schooling at sixth grade, i.e., before the pupils are split up under the old (pre reform) system and while still under the compulsory schooling restrictions of the old system. For switches of municipality to a®ect the exogeneity of reform assignment (conditional on the observables) they would have to take place before the 6th grade. This is much less likely, since the pupils would have little to gain by an early switch. Moreover the o±cial statistics does not show any such evidence of switching.

A5. Incentives for the Municipalities to Implement the Comprehensive School in the Experiment

A potential source of di®erences between areas where the reform was implemented and those where it was not, is likely to be the element of self-selection of municipalities who accepted the implementation of the reform. One issue is whether municipalities viewed it as costly to implement. Implementing the new comprehensive school did involve costs for the municipality. Extending the compulsory schooling required new school buildings as well as hiring new teachers. In addition to that, the more specialized teachers on the last three years of the comprehensive school required a lower teaching load than the teachers in the pre-reform compulsory schools.

The initial invitation to participate was accompanied by a promise that all extra costs would be covered by the central government. However, after a few years, the municipalities who implemented the new school claimed that they actually bore additional costs although they received some support from the central government. The National school board investigated how the municipal income tax rate earmarked for expenditures for basic education changed in all municipalities between 1946 and 1951. It was found out that the tax rate remained constant at 2.56% up until 1949 and then increased to 2.85% in the municipalities which did not implement the comprehensive schools. In the municipalities that implemented the reform, the tax rate increased slightly from on average 2.18% in 1946 to 2.35% in 1949, but markedly to 3.25 in 1951.[42]

In 1953 a new system for ¯nancial support for the municipalities who implemented the reform was introduced. In short, this system implied that 100 percent of the wage costs for specialized teachers were covered by the state. The municipalities also obtained a special allowance to cover housing costs for these teachers. In addition to that, the state subsidized new school buildings by 8 percent and a system to cover additional costs for teaching materials and transports of pupils were implemented. In 1958, and until the end of the experiment in 1962, this system was replaced by a general state subsidy tied to the number of teachers in compulsory schools and a special allowance, paid to the municipality, of 80 SEK for each pupil in the last three years in the comprehensive schools to cover additional costs for teaching materials.

To summarize, there was a net subsidy from the central government to the included municipalities throughout the experiment, although not all additional costs were covered by the government. It can be hypothesized that municipalities who under the pre-reform regime wanted a Junior secondary school but did not have one, or wanted to expand this type of schooling, gained most by joining the experiment.

Appendix B. The IS Data

The IS survey consists of information from four main sources:

(1) Student's social background and socio-economic situation. The information from this block of the data-set is obtained directly from the respondent through the survey questionnaire. The variables measuring mother's and father's education are grouped into four levels: Basic education (\folkskola"), Junior secondary school (\realskola/°ickskola"), Upper secondary school (\gymnasium") and Academic education.

(2) Results from IQ and achievement tests. The IS surveys contains results from

[42] Försöksverksamhet med nioårig skolplikt, 1959, page 40.

Variable/PC	Abil1	Abil2	Abil3	Variable/PC	Abil1	Abil2	Abil3
Indicator Variable for score in Mathematics IQ test				Grade in Mathematics			
MIQ1	-0.150	-0.006	0.085	IMG1	-0.113	0.116	0.193
MIQ2	-0.122	-0.053	0.008	IMG2	-0.276	0.171	0.122
MIQ3	-0.068	-0.060	0.013	IMG3	-0.081	-0.149	-0.326
MIQ4	-0.033	-0.086	-0.023	IMG4	0.240	-0.150	0.041
MIQ5	0.035	-0.124	-0.003	IMG5	0.253	0.138	0.131
MIQ6	0.055	-0.095	0.004	IMG6	0.046	0.062	-0.011
MIQ7	0.109	-0.102	0.042	Grade in English			
MIQ8	0.139	-0.051	0.101	EG1	-0.117	0.132	0.235
MIQ9	0.176	0.411	-0.260	EG2	-0.285	0.184	0.100
Indicator variable for score in verbal IQ test				EG3	-0.054	-0.214	-0.410
VIQ1	-0.168	0.013	0.117	EG4	0.297	-0.113	0.146
VIQ2	-0.145	-0.064	-0.022	EG5	0.213	0.134	0.120
VIQ3	-0.061	-0.089	-0.044	EG6	0.023	0.039	-0.011
VIQ4	-0.021	-0.084	-0.057	Grade in Swedish			
VIQ5	0.027	-0.129	-0.029	SG2	-0.213	0.229	0.341
VIQ6	0.064	-0.119	0.022	SG3	-0.263	-0.041	-0.362
VIQ7	0.108	-0.092	0.054	SG4	0.267	-0.230	0.015
VIQ8	0.157	-0.050	0.138	SG5	0.259	0.142	0.146
VIQ9	0.193	0.411	-0.231	SG6	0.029	0.042	-0.007
Indicator variable for score in spatial IQ test				Variance/total variance in %	8.62	6.19	4.60
SIQ1	-0.129	0.411	0.066				
SIQ2	-0.069	-0.035	0.019				
SIQ3	-0.033	-0.068	0.048				
SIQ4	-0.001	-0.078	0.029				
SIQ5	0.017	-0.069	0.047				
SIQ6	0.040	-0.064	0.030				
SIQ7	0.057	-0.069	0.025				
SIQ8	0.098	-0.039	0.038				
SIQ9	0.131	0.398	-0.298				

Table 13: Loadings for principal components Abil, Abil2 and Abil3.

two types of tests: (a) Results from three di®erent types of IQ tests; (b) Results from test on achievement in di®erent subjects taught in school.

(a) IQ tests. The three di®erent IQ tests measure three di®erent aspects of intellectual ability. First, the verbal ability is measured by the test Opposite (The respondent is asked to choose the opposite of a word from four given choices). Second, the spatial ability is measured with the test Metal folding (The respondent is asked to choose which three dimensional object from four given alternatives that can be obtained from a given °at piece of metal). Third, the mathematical ability is measured through the test Number series (The respondent is asked to complete a given series of numbers).

(b) Achievement tests. The ability in reading, writing, English, and mathematics, all subjects taught in the compulsory school, are measured by standardized tests.

All test scores were collected in 6th grade, i.e. for most children before the impact of the streaming in the pre-reform school system had any e®ect.

(3) Register information on the students performance and type of school. Data on grades were obtained by matching the samples with a national register provided by the National School Board. In the pre-reform grade system the grades were set in seven levels, while the post-reform school applied a ¯ve level scheme. These grading schemes were made comparable by transforming the highest and lowest two levels in the pre-reform scheme to the highest and lowest level respectively in the post-reform scheme. The National School Board register also provided information on the type of school attended, i.e. whether or not the student followed the new, post-reform school system.

(4) Information from the National tax and the National education registers. Data on several variables were obtained when the sample from 1961 were matched with the National tax and National education registers from 1985-1996. Data for earnings are measured as annual pre-tax earning from labor obtained from individual tax returns. The National tax register also contains data for each year 1985-1996 on employment status and whether or not each individual was self-employed.

Table 13 shows the PC loadings for the ¯rst three principal components as well as the percentage share of the total variance that each of these principal components account for. It is evident from the results shown in Table 13 that the ¯rst PC, Abil1, measures high ability, i.e. it gives positive weights to high IQ scores and high grades. This is not true to the same extent for Abil2 and Abil3.

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