ABILITY, PARENTAL BACKGROUND AND EDUCATION POLICY: EMPIRICAL EVIDENCE FROM A SOCIAL EXPERIMENT

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Abstract

Following the great expansion of secondary education in the United States between 1910 and 1940, Sweden was one of the first Western European countries to attempt such an expansion by increasing the years of compulsory schooling and and improving access to academic type education by abolishing early selection. The reform was preceded by a large scale area based social experiment where 25% of the contry's municipalities were assigned to the reform. We use this assignment, together with rich individual data to evaluate this major educational interventions. Our key findings are that this reform increased the educational attainment of individuals with unskilled fathers. In addition it caused significant and large increases in the earnings of those with unskilled fathers and above median ability.

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1 Introduction

Most West European countries implemented comprehensive schools between 1950 and the mid 1970s. In most cases, this implied that the number of years of compulsory schooling was extended and the streaming of more able students in separate schools was delayed or abolished (see e.g. Aakvik et al., 2003, or Leschinsky and Mayer, 1990, for an overview). However, the impact of such important educational interventions on outcomes such as earnings is not well established. Sweden was an early mover in this process. Already in 1950 the Swedish parliament decided on a comprehensive schooling reform, which extended compulsory schooling from 7 or 8 years (depending on the municipality) to 9 years. The reform was to a great extent inspired by the great expansion of secondary education in the United States between 1910 and 1940 (see Goldin, 1999). What makes the Swedish reform interesting from an evaluation point of view is that it was not implemented all at once. Because of the controversial nature of the new policy, and to enable evaluations of the reform, it was decided that the reform should be preceded by a "social experiment" where the new system was implemented in a subset of Sweden's more than 1,000 municipalities. The experiment went on until 1962, when the reform was finally implemented.

In this paper, we compare the educational and labor market outcomes of a cohort of individuals going through the two different school systems while living in similar economic and social environments during schooling and working in the same labour market. The school system they faced was determined by whether the municipality they lived in was assigned to the reform or not during the experimental period that preceded the full implementation of the reform.

The unique nature of our data allows us to estimate effects for individuals with different levels of ability and on individuals with lower parental education separately. This allows a greater insight as to how educational interventions operate and which groups are likely to benefit most. Thus our analysis relates directly to key issues of policy as expressed in Heckman (2000), Carneiro and Heckman (2003) and Krueger (2002).

The design of the experiment and the timing of our data is such that a large proportion of the Swedish municipalities were assigned to the reform (about 25%). In addition, those designing the experiment had to ensure that the treatment municipalities were representative of the whole population. This in itself provides an excellent area based treatment/comparison group design. In addition we have at our disposal a particularly rich data set with information at the individual and municipality level, which allows us to correct remaining imbalances between the treatment and the comparison sample, which are inevitable when assignment of the reform (treatment) is not random. We use propensity score matching for this purpose.[1]

Our data was obtained by combining the Individual Statistics (IS) survey[2] with administrative sources on educational attainment and earnings. 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 as well as grades and tests of subjects taught in schools - all test results were obtained before the split into the new and old school systems took effect.[3] We also observe detailed characteristics on the municipality of schooling, such as the number of inhabitants and the average income level at the time as well as indicators for local labor market. When large cities were involved such as Stockholm, which is one municipality, part of the city was allocated to the reform and part not. In addition there are a large number of municipalities in both the reform and the control sample. All this makes it very likely that similar individuals growing up in effectively the same environment and eventually working in the same labour market can be identified

[1]See Rosenbaum and Rubin (1983), and Heckman, Ichimura and Todd (1997)

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

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

in the treatment and control samples. Finally, we observe where individuals were born which allows us to test whether individuals moved in response to the reform and whether this could have biased the results.

The present study is related to two branches of the previous literature on the economics of education. The first one (surveyed in Card, 1999) aims to estimate the causal returns to education by exploring compulsory schooling laws to obtain exogenous variation in the quantity of education. An early example is Angrist and Krueger (1991), who explores the fact that children are affected differently by state compulsory schooling laws in the US, depending on quarter of birth. Harmon and Walker (1995) use an extension of the school-leaving age in the UK,[4] but unlike the reform we study, the two reforms studied by Harmon and Walker were implemented in separate cohorts, making it difficult to distinguish the effects of the reform from cohort effects. Two studies, Margo and Finegan (1996) and Acemoglu and Angrist (2000), use inter state differences in compulsory schooling and child labor laws in the US. However, both these studies have somewhat different focus compared to ours. Margo and Finegan restrict their study only to educational choice, while the primary interest of Acemoglu and Angrist is the "social" returns to education, which in contrast to the "private" returns also reflect externalities of education. To do this it is necessary to compare returns to schooling in different local labor markets. In our study, since each local labor market consists of many municipalities, we are able to study differences in outcomes for two different education systems on the same local labor markets; this however restricts the analysis to measuring the private return to the educational intervention.

The second branch, which contains a substantially larger amount of papers, consists of evaluations of experiments on organizational changes and/or extra resources to education programs (surveyed in e.g. Hanushek, 2002 and Krueger, 2001). Our study differs from most of these papers since it refers to a nation-wide

[4]A recent paper, Oreopoulos (2003), investigates the impact of this reform on a larger set of outcome measures, such as health, leisure and labor activities, and subjective measures of well-being.

experiment and use labor market outcomes, rather than performance measures from schools such as test scores and/or dropout rates, as outcome variable. One exception is, however, Duflo (2002). This study analyses the effects of schooling on earnings by exploiting regional differences in the implementation of a large school construction program in Indonesia in the 1970s. The studies are, however, quite different since in the Swedish case we are evaluating a radical change in the schooling system, typical of many reforms in developed countries, and not so much an improvement in the infrastructure which should lead to an increase in the takeup of schooling.

Several very interesting results are obtained. First, the reform increased overall educational attainment as well as earnings substantially and significantly. But more interestingly it affected different ability groups in different ways. Thus when we focus on the group of individuals whose father was unskilled we find that the largest part of the educational effect is accounted for by those with ability below the median; however, the largest part of the increase in earnings is accounted for by those with ability above the median. There are no significant effects of the reform on the educational attainment of those with skilled fathers. Moreover, we do not identify any significant impact on their earnings either. Thus overall the reform improved the position of those from lower socio-economic groups and in particular those of higher ability.

The paper is organized as follows. Section 2 describes of the Swedish pre- and post-reform education systems as well as the social experiment preceding the education reform. Section 3 discusses estimation and the interpretation of the estimates. Section 4 describes the data set. It also compares characteristics of municipalities assigned to the reform to those not assigned and the characteristics of the "treated" and "non-treated" individuals. Finally, it compares the characteristics of the pupils who changed reform assignment from their municipality of birth. Section 5 presents the results on the impact of the reform on educational attainment and earnings. Section 8 concludes. Appendix A gives additional facts on the data set and, finally, Appendix B presents a simple theoretical framework for educational choice, which is used for interpreting the results.

2 The 1950 Education Reform

2.1 The Pre-Reform School System and Background to the 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 these programs, i.e., all pupils went to the same classes. After sixth grade the more able students were selected for the junior secondary school. The selection was in general made on grades. Those who did not enter junior secondary school continued one or two years in basic compulsory school. The compulsory schooling was, thus, at least seven years and in some municipalities, mainly in city communities, eight years. The basic compulsory schools were administrated by the municipalities.

The junior secondary school was a prerequisite for the upper secondary school, which, in turn, was a prerequisite for higher education. The junior secondary schools were, with some exceptions, administered by the national government. Before 1958, the length of this education varied between three and four years depending on region, but in a reform in 1958 it was unified to three years, which implied that those who graduated from junior secondary schools in general had nine years of schooling before they could enter upper secondary school.

In 1940, a parliamentary committee of experts on education policy was appointed by the government. There was a consensus on some of the problems of the pre-reform school system within the committee. First, by that time, Sweden, compared to other countries, had a relatively short compulsory education: the student finished compulsory school at age 13 or 14. As a comparison, enrollment rates in high-schools were above 80 percent in most parts of the United States (see Goldin, 1999). Second, an increasing proportion of students wanted to continue on to junior secondary school. The share of students who actually continued in the junior secondary school increased from about 10 percent in 1930 to about 40 percent in 1950 (see Erikson & Jonsson, 1993). The resources for that kind of education were, however, not sufficient to meet the demand. Finally, the fact that the curriculum of the schools differed between the municipalities and that there was no unified path to higher education were seen as limitations of the existing educational system.

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 the parallel school systems, split by ability. A similar debate preceded the decision to postpone admission to the selective junior secondary school from the fourth to the sixth grade in compulsory school in 1940. The advocates for a comprehensive school, supported by the social democratic government, argued that such a school system would improve equality of opportunity. The social selection to junior secondary schools was claimed to be a problem. 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.

Those who wanted to maintain the parallel school system, represented in particular by the conservative party in the school committee, were worried that the standard of the students entering upper secondary schools and universities would decrease and that the most able students would be less stimulated if the junior secondary schools were abolished. By that time, most European countries, in particular Germany, had parallel school systems.

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 towards those who wanted to maintain the parallel school systems was that the students were able to choose between three different levels after sixth grade: one with a more academic curriculum, one general level and one level which included vocational training.

In addition to the increase in the number of years of compulsory schooling, the main difference from the pre-reform school system was, that all students went to the same schools rather than separate ones, as before the reform. There was a centrally decided curriculum for all schools, which differed from the pre-reform compulsory schools where the curriculum was decided by the municipalities. English was taught from the fifth grade, which was earlier than most pre-reform compulsory schools. As in the pre-reform junior secondary school, specialized teachers were introduced for different subjects in the last three grades (year 7 to 9) of the proposed comprehensive school.

2.2 The Social Experiment

The nationwide experiment with the new comprehensive school started in 1949, the year before the first parliamentary decision on the comprehensive school reform, and continued until the final curriculum of the post-reform schools was decided in 1962. There was a main evaluation of the experiment starting in 1957 (see *Försöksverksamhet med nioårig skolplikt,* 1959) and the curriculum of the schools in the experiment was changed starting in 1959.

There were at least two reasons as to why a nationwide experiment was set up before the implementation of the new school. First, there was a widespread belief in scientific evaluations among the generation of Swedish politicians who were active at that time, in particular among those involved in education policy.[5] In their view, an experiment was a means for improving different aspects of the proposed new school. Second, and more importantly, it was a way of resolving different views, described above, within the parliamentary school committee. An experiment with a comprehensive school was a first step in a compromise.

In the experiment, the proposed comprehensive school was implemented by areas, entire municipalities or parts of city communities, rather than by separate schools or classes. By the time the experiment started, Sweden was divided into

[5] See Marklund (1981) for several quotes on that.

about 2,500 city communities and rural municipalities. The first two years of the experiment were administered through the parliamentary school committee. The municipalities selected to try out the new system in these first two years (1949 and 1950) had to fulfil two requirements (1) The length of the compulsory schooling should be 8 rather than 7 years - a requirement that was dropped two years later. (2) The demographic structure of the municipality should permit a continuous flow of pupils into the new school system. On this basis 264 municipalities were considered eligible of which 144 declared an interest in implementing the new system. Of these, 11 municipalities and 3 parts of city communities were the first to be selected into the program. The rationale for choosing these particular areas, was that they were considered to be representative, demographically and geographically, for the entire country. After two years, the administration of the experiment was taken over by a special unit of the National school board. As the experiment proceeded, it is less well documented how the municipalities to be included in the experiment were selected. It is, however, evident that the basic structure of the selection was maintained throughout the experiment. First, the municipalities were contacted, or applied directly, to be included in the experiment. From these applications, the administration selected the participants with the aim of obtaining a "representative" sample of municipalities.

Finally, at least up to the evaluation in 1957 (see *Försöksverksamhet med nioårig skolplikt*, 1959), and probably also after that, there were more applications than admissions in the experiment.

When a municipality introduced the new school system it implemented it either for the cohort of pupils who where in fifth grade at the time of the decision or for those who were currently in the first grade, effectively delaying the start of the programme. Table 1 shows the development of take-up of the experiment between 1949 and 1962. These figures show that the number of students in the experiment were quite modest up to the first evaluation in 1957. After the first evaluation, beginning in 1959, the experiment grew rapidly. At the time when the cohort we

Year	Municipalities		Number of	Number of
	Number	Percentage share	classes	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 1: Quantitative development of the comprehensive school experiment 1949 to 1962.

will be looking at (born in 1948) was assigned to the experiment (1960/61) the number of municipalities and cities was 1,037 and Table 1 shows that the reform was implemented in 28 percent of these.

2.2.1 Reform Implementation and Social Policy

Since compulsory schooling was extended in the municipalities where the comprehensive school was implemented, and since some of the students would have started to work immediately after finishing the pre-reform compulsory school, the financial burden for some of the families increased as a result of the experiment. This problem was extensively discussed in the debate proceeding the experiment.

As a result, two different strategies for family support were used. First, general support for families with children. A non-taxable universal allowance for children up to the age of 16 was introduced in 1948. Second, in 1953, when the first cohort included in the experiment reached 9th grade, means tested stipends were introduced in the reform areas.

3 Estimating the Impact of the Reform on Educational Qualifications and Earnings

The design of the Social experiment provides a very powerful starting point for the evaluation, because there is a large number of municipalities assigned to the reform and were chosen so as to be representative of the Swedish population. As a result all types of areas are represented. However, because the reform was not randomly assigned, the treatment and control samples are not necessarily balanced in the sense of having the same distribution of characteristics. We will provide evidence on this issue in a later section. To correct for any imbalances we use propensity score matching when evaluating the impact of the reform on education and on earnings (see, Rosenbaum and Rubin, 1983 and Heckman, Ichimura and Todd, 1997). The assumption underlying matching is that assignment to reform, *conditional on our observables*, is independent of what educational attainment and earnings would have been had the individuals not gone through the reform. Given the way the social experiment was designed and our particularly rich data set this is likely to be the case here, if ever it is.

Formally, denote by $\ln w_{it}^1$ 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 $\ln w_{it}^0$. 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 $\ln w_{it}^1 - \ln w_{it}^0$. Define $TT = E\left[\ln w_{it}^1 - \ln w_{it}^0 | D_i = 1\right]$ to be the impact of the reform on those actually assigned to the reform ($D_i = 1$).[6] 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., $E\left[\ln w_{it}^0 | X_i, D_i = 1\right] = E\left[\ln w_{it}^0 | X_i, D_i = 0\right]$, where X_i represents observed characteristics.

[6] Impact of treatment on the treated.

Rosenbaum and Rubin (1983) shows 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 $\ln w_i = D_i \ln w_{it}^1 + (1 - D_i) \ln w_{it}^0$. 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 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 whether a person went through the reform on our matching variables. We then estimate $E[\ln w_i | P(X_i), D_i = 0]$ on the sample of individuals not assigned to the reform.[7] 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 the 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 the observation 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 the 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]

[7] We use cubic splines with 4 knots placed as the 20th, 40th, 60th and 80th percentile of the propensity score.

[8] The modification consists in the fact that we also smooth the earnings of the reform individ-

There are two main advantages of this method *vis-à-vis* linear regression. First, this approach makes sure that the treated and comparison groups have comparable characteristics and that no comparisons are made by extrapolating into areas where no data actually exists (i.e. we impose common support, although this turns out not to be a major issue in our data). Second, the approach takes full account that the impact of the reform can vary with observed characteristics and estimates the weighted average of the impacts across groups.

In what follows we consider the impact of the reform on earnings and on education attainment, using the method outlined above. Our outcome variable when evaluating the impact of the reform on educational attainment is the level of education achieved (qualification). We also construct a measure of years of education as an outcome, based on the level attained.

Finally, we use the block bootstrap to compute standard errors and bias corrected 95 percent confidence intervals for the estimates of the impact TT (see Horowitz, 1999). The standard errors and the confidence intervals and any test statistics we present allow for cluster effects by municipality which is the unit of treatment (see Moulton, 1986), for dependence over time where we have multiple observations over time (i.e. for the income outcome), as well as for the fact that the propensity score is estimated.

4 Data

4.1 Measurement and Sample Selection

We use data from the Individual Statistics (IS) project of the Institute for Education at the University of Gothenburg[9] 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

uals. This tends to improve precision slightly.

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

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 1 in Section 2, 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.

The data-sets consist of four main parts: (1) Information on the student's social background, socioeconomic situation, leisure activities and plans for future studies; (2) Results from IQ and achievement tests; (3) Register information on the students 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.[10] Appendix A provides the details and descriptive statistics.

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 educational choice.

[10]See Appendix A for descriptions on all these variables.

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 2 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 administrative nature of the data, and the fact that we are measuring levels there is very little scope for measurement error in education.

We use all the 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.[11] In a second step, a principal component analysis was carried out on all the indicator variables. This is useful because for some of the 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 14 in Appendix A 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.

[11] 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.

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 $\leqslant$ 2 years[1]	11.5
4	Upper secondary school $\geqslant$ 3 years[2]	13
5	Post upper secondary school $\leqslant$ 2 years[3]	15
6	Post upper secondary school $\geqslant$ 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 three 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 unfinished 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 2: Short descriptions of each education level and estimates of average number of years of schooling for each level.

4.2 Comparing the Reform and Control 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.[12] Table 3 shows the difference of the average scores in the reform area form the non-reform ones, for the various tests. We also show the differences within the big cities (2nd pair of columns) and the rest (3rd pair of columns). As can be seen, the average results on these tests are very similar. In most cases the differences, which are relative to a scale of 0-100, are not statistically significant and in addition they are very small. The proportion of skilled parents was higher in the reform municipalities but, once we condition on being in a big city or not, the difference becomes insignificant. Our presumption is that these results lend support to the idea that reform assignment is not correlated with unobserved ability.

Test score and parental background difference between reform and non-reform pupils						
	All		Stockholm, Gothenburg and Malmo		Excluding Stockholm , Gothenburg and Malmo	
	Men	Women	Men	Women	Men	Women
IQ, Opposites	0.526	1.409	-1.002	-1.635	-0.052	1.340
	(0.652)	(0.507)	(0.863)	(1.514)	(0.746)	(0.651)
IQ, Folding	2.016	1.657	1.836	1.535	0.826	1.401
	(0.870)	(0.483)	(0.638)	(1.281)	(0.728)	(0.589)
IQ, Mathematics	0.537	0.242	-0.002	-1.634	-0.369	0.440
	(0.794)	(0.652)	(0.435)	(2.110)	(0.859)	(0.758)
IQ, average	1.026	1.103	0.277	-0.578	0.135	1.061
	(0.682)	(0.409)	(0.389)	(1.585)	(0.647)	(0.542)
Reading	0.965	0.943	0.064	-0.760	0.240	0.772
	(0.640)	(0.409)	(0.349)	(0.515)	(0.631)	(0.515)
Writing	0.407	0.305	-0.555	-1.124	-0.062	0.273
	(0.517)	(0.343)	(1.149)	(0.338)	(0.546)	(0.416)
Mathematics	0.124	0.302	-1.381	-1.821	-0.495	0.510
	(0.620)	(0.506)	(1.092)	(0.552)	(0.713)	(0.624)
English	2.786	2.206	-0.580	-2.412	1.937	2.332
	(0.955)	(0.621)	(3.402)	(1.139)	(0.920)	(0.758)
Average test score	0.976	0.827	-0.596	-1.559	0.268	0.870
	(0.673)	(0.413)	(1.771)	(0.465)	(0.647)	(0.531)
Father's education more than basic	0.038	0.026	-0.048	-0.077	0.020	0.023
	(0.018)	(0.013)	(0.054)	(0.057)	(0.016)	(0.015)

Note: All tests cores are normalized to have maximum score at 100. Standard errors adjusted for clustering by municipality in parentheses

Table 3: Differences in average test scores between pupils assigned to pre- and post-reform school systems. Men and women. Standard errors corrected for clustering within municipalities in parentheses.

[12]We know from earlier studies, e.g. Blackburn and Neumark (1995) or Kjellström (1997, 1999), that performance on these kind of tests is correlated with "ability" and probably also with individual returns to education.

Table 4 compares some key characteristics of the city communities and municipalities which were included in the experiment to the excluded ones for the 1948 cohort.[13] 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 differences in the characteristics within the other city communities and municipalities seem quite modest.[14] The municipalities in the experiment are on average somewhat larger than in the control group. We control for this by including the municipality characteristics in the propensity score that balances the reform and non-reform sample. We find that we can always find similar areas to our treatment municipalities among the comparison areas

A final 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. Although at the start of the experiment in 1949 it was required that the participating municipalities had 8 years of compulsory schooling, this requirement was soon abandoned and only affected 2.3 percent of pupils allocated to the re-

[13]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.

[14]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.

	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
Share living in rural municipalities, %	36.92	58.17
Cities other than Stockholm, Gothenburg and Malmo		
Average population size	28 646	33 009
Mean income	49.57	47.13
Mean income tax, %	10.63	11.11
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 official statistics on each area (Source: Årsbok för Sveriges kommuner 1960 and 1961).

Table 4: Comparison between treatment and control municipalities.

form for our cohort (see Table 1 in Section 2)[15] To check whether there was any systematic difference in the years of compulsory education between the municipalities assigned to the reform (for our 1948 cohort) and those not assigned, we used data on the 1930-40 cohort, which are not affected by the reform, drawn from the Swedish Level of Living Survey. The estimated difference in the pre-reform years of compulsory schooling between the treatment and control municipalities is 0.14 of a year and this is not significant (standard error 0.11). This accords with an assessment provided to us by an official of the, Swedish Ministry of Education.[16]

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

[16]Personal conversation with Mac Murray an educational historian, who served as an official at the Ministry of education and National School Board which administered the experiment.

4.3 Mobility between Reform and Non-Reform Municipalities

Using the 1978 census we were able to match into our data the municipality in which our individuals were born. We then found the reform status of the birth municipality as it was in 1960, the date relevant to our evaluation.[17] Using this information we investigate whether there has been any selective movement and we carry out sensitivity analysis for our results.

We found that about 4.3% of the sample was born in a non-reform municipality and changed to a reform one and an equal amount went the opposite way. In itself, these small numbers of individuals who changed reform assignment shows that the scope for bias due to selective mobility between municipalities is very limited. However, we will now look at it in more detail.

In Table 5 we compare characteristic of those who move to those who do not move, conditional on the status of the municipality of birth. There are two main conclusions from this table. First, those who change reform status from the birth municipality (whatever the origin) on average have fathers who are better educated. Second, those who change status to avoid the reform tend to have higher IQ scores (2.2 percentage points). There are no obvious differences between those who move into a reform area and those remaining in the municipality of birth, given the latter was a non-reform one (lower panel of the table).

However, we can condition on these characteristics. The key issue is whether there is selection on unobservables when moving and whether these unobservables are relevant for earnings. To see how much scope there is for selection on unobservables we test whether conditional on these characteristics, being born in a municipality of a certain type causes one to change reform status. Thus in the top panel of Table 6 we show the impact of having been born in a municipality

[17] Of the 11,950 individuals included in the original data-set, we were able to match on information on municipality of birth to 10,949. Of these we were able to classify reform assignment of their municipality of birth for 78.5 percent. Those who we were not able no classify were to a large extent born in large city communities, part of which were assigned to the post-reform school system and part to the old one.

	Male	Female	All
Movers to non-reform municipalities given birth in a reform one			
IQ, average from 3 tests	2.286	2.215	2.242
	(1.155)	(1.271)	(0.883)
Average score in 4 tests on mathematics,	0.678	1.645	1.181
English, reading and writing	(1.135)	(1.077)	(0.821)
Father's education more than basic	0.104	0.101	0.103
	(0.034)	(0.035)	(0.027)
Movers to reform municipalities given birth in a non-reform one			
IQ, average from 3 tests	0.180	0.606	0.346
	(1.155)	(0.918)	(0.786)
Average score in 4 tests on mathematics,	0.716	1.490	1.161
English, reading and writing	(0.995)	(0.810)	(0.656)
Father's education more than basic	0.134	0.123	0.128
	(0.046)	(0.028)	(0.026)

Note: All test scores are normalized to have maximum score at 100. Standard errors in parentheses allow for clustering by municipality.

Table 5: Differences in average test scores and parental background between those who changed and did not change reform assignment condiotional on the reform status of their municipality of birth.

which was subsequently assigned to the reform, on the probability of moving to a non-reform municipality as opposed to staying at the municipality of birth or moving to some other reform municipality. None of these effects are significant at conventional levels.

In the bottom panel of Table 6 we show the impact of being born in a non-reform municipality on the probability of moving to a reform one. We find that there is a marginally significant effect of moving into the reform for low ability individuals with fathers of low education. However, overall the effects are insignificant.

Thus, there does not seem to be any strong evidence of systematic changes of reform status, that cannot be explained away by observable characteristics. Selective mobility is low and can mostly be explained by observed characteristics. Ultimately what matters is whether selective movement (if there is any) is related to unobserved characteristics that are relevant for earnings. Although we cannot provide direct evidence about this, we present sensitivity analysis to eval-

Effect of being born in a reform area on moving to a non-reform one				
	Male		**Female**	
	Father's Education		**Father's Education**	
	Low	**High**	**Low**	**High**
Low Ability	-0.040	-0.016	-0.029	0.030
	(0.030)	(0.050)	(0.026)	(0.051)
High Ability	-0.045	0.061	0.010	0.075
	(0.027)	(0.066)	(0.028)	(0.066)
	All		**All**	
	-0.032		-0.007	
	(0.023)		(0.021)	
Effect of being born in a non-reform area on moving to a reform one				
Low Ability	0.031	0.002	0.048	NE
	(0.015)	(0.010)	(0.023)	-
High Ability	-0.009	-0.047	0.016	-0.023
	(0.017)	(0.052)	(0.017)	(0.048)
	All		**All**	
	0.005		0.026	
	(0.017)		(0.018)	

Note: Standard errors allow for cluster effects by municipality. Controls include ability, county, municipality characteristics and fathers education. NE: perfect fit by characteristics.

Table 6: The effect of being born in a reform municipality on the probability of moving to non-reform areas

uate whether our results are likely to have been affected by this limited selective movement. To preempt, we do not find any evidence of bias.

5 Results

5.1 The Effect of the Reform on Education.

A change in the education system as radical as the 1950 reform could affect schooling in a number of ways. There is a direct effect due to the increase in the amount of compulsory schooling, potentially affecting about 25 percent of the individuals in the male sub-sample and about 20 percent 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.[18] The curriculum in level 3 (Upper secondary school $\leqslant$ 2 years) became more academic, making the transition to the Upper secondary school and beyond 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.

The reform may have also affected educational attainment through general equilibrium effects. Lang and Kropp (1986) for example find evidence that differences in compulsory attendance laws in US states affect enrollment rates to education levels not directly affected 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 effects are likely to affect both reform and non-reform areas in a similar way since a) the reform and non-reform municipalities coexist in the same labor market (i.e. the county or a large city such as Stockholm) and b) the new system was expected to be implemented nationally. The effects we measure should be interpreted as impacts on individuals given the aggregate impact of the experiment and given the expectation that the reform

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

would be implemented nationally, with a resulting increase in educated workers, as we will now show.

Table 7 shows the share of individuals in the different education levels by reform status and gender as well as the implied difference between the two. In the fourth column we report the results obtained by propensity score matching for each education level. The propensity score[19] used to balance the reform and non-reform samples is a logit of the reform indicator on dummies for the county of schooling (which includes both reform and non-reform municipalities),[20] county of residence as an adult in 1990, 44 ability indicators constructed from school grades and special IQ tests, indicators of father's education (5 groups) and characteristics of the municipality, including aggregate income, population size, the local tax rate and whether it is Stockholm, another city or a rural community.[21] Matching is always carried out separately for men and women. Thus the matched estimates are obtained by comparing individuals of the same observed ability, the same gender, living in similar municipalities, with fathers of the same education level, going to school and working in the same county and of the same cohort (all our sample is born in 1948) but who went through different schooling systems. Practically all reform observations were matched and there was no problemof lack of common support between the reform and the non-reform observations.

We report bias corrected bootstrap 95 percent confidence intervals and the standard deviation of the bootstrap. In all cases we allow for clustering at the municipality level which is the treatment unit. Moreover, in computing the standard errors we also allow for the fact that the propensity score is estimated.

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

[19]Probability of being assigned to the reform.
[20]See Section 4.1 for characterization of counties in Sweden.
[21]Table 13 in Appendix A presents descriptive statistics of variables included in the analysis.

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 dropped out after the pre-reform basic school, "evading" the increased compulsory schooling level. These persons could have moved out of the municipality included in the 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.

At the bottom of the Table we summarize the impact of the reform on levels not directly affected by the change in the compulsory schooling laws, i.e. levels 3 to 7, access to which the reform was intended to improve. There we see that there is no significant overall increase for men - rather there seems to have been some reallocation between types of post compulsory education: For men we observe a significant increase of 2.2 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 percentage points increase in level 3 which is significant. In contrast to men the reform seems to have caused an overall significant 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 0.27 of a year and females years increased overall by 0.22 of a year.

The reform is likely to have had different impacts depending on individual ability and family background. Lower achievement/ability may lead pupils to want to drop out earlier and lower parental education (given ability of the children) may restrict educational opportunities, either due to financial factors or because less educated parents provide less encouragement to their children. We illustrate these points in a simple theoretical model in Appendix B.

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	-0.110	0.199	0.034	-0.165	-0.109
	(0.007)	(0.005)	(0.011)	(0.024)	(0.006)	(0.004)	(0.009)	(0.021)
				[-0.161,-0.072]				[-0.150, -0.070]
2. Comprehensive/	0.093	0.217	0.124	0.101	0.112	0.228	0.116	0.082
Junior Secondary	(0.005)	(0.009)	(0.010)	(0.025)	(0.005)	(0.009)	(0.010)	(0.020)
				[0.060, 0.160]				[0.044, 0.122]
3.Upper secondary	0.268	0.294	0.025	-0.013	0.349	0.357	0.008	0.039
school $\leqslant$ 2 yrs	(0.007)	(0.010)	(0.012)	(0.019)	(0.008)	(0.010)	(0.013)	(0.018)
				[-0.056, 0.011]				[0.009, 0.075]
4. Upper Secondary	0.169	0.189	0.020	−0.0003	0.083	0.085	0.002	-0.016
school $\geqslant$ 3 yrs	(0.006)	(0.009)	(0.011)	(0.017)	(0.004)	(0.006)	(0.007)	(0.012)
				[-0.038, 0.030]				[-0.042, 0.007]
5. Post Upper	0.070	0.082	0.013	0.022	0.119	0.134	0.015	-0.006
Secondary $\leqslant$ 2 yrs	(0.004)	(0.006)	(0.007)	(0.010)	(0.005)	(0.007)	(0.009)	(0.015)
				[0.003, 0.043]				[-0.035, 0.024]
6.College/	0.147	0.161	0.014	−0.001	0.134	0.159	0.026	0.020
University	(0.006)	(0.008)	(0.010)	(0.013)	(0.005)	(0.008)	(0.009)	(0.017)
				[-0.024, 0.024]				[-0.010, 0.054]
7. Ph.D.	0.010	0.011	0.002	0.002	0.005	0.003	0.002	-0.008
	(0.002)	(0.002)	(0.003)	(0.005)	(0.001)	(0.001)	(0.002)	(0.005)
				[-0.004, 0.013]				[-0.023, -0.001]
More than	0.664	0.737	0.073	0.009	0.689	0.738	0.0488	0.028
Comprehensive or	(0.0077)	(0.01)	(0.013)	(0.016)	(0.0077)	0.0100)	(0.0128)	(0.017)
Junior Secondary				[-0.031, 0.035]				[0.001, 0.065]
Years of education	11.14	11.84	0.70	0.274	11.16	11.77	0.61	0.215
	(0.051)	(0.062)	(0.081)	(0.115)	(0.050)	(0.061)	(0.079)	(0.130)
				[0.074, 0.540]				[-0.029, 0.508]
Sample size	5,396				5,254			

Notes: Source: IS Survey, 1948 cohort. Standard errors adjusted for clustering by municipality in parentheses. Bias corrected 95% bootstrap confidence interval in square brackets. Controls for matching estimates: 44 ability indicators, county of schooling and residence, father's education, characteristics of the municipality (Stockholm, urban/rural, av income, population size, local tax)

Table 7: The impact of the reform on educational qualifications

In what follows, high ability individuals are defined as those with the first principal component of our 44 ability indicators above the median for the whole population(i.e over all parental backgrounds, see Section 4.1 for the background to this procedure). We associate parental background with father's education and we define "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, shown in Table 8 for males and in Table 9 for females, is based on propensity score matching.[22] For both men and women, 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. Moreover, as expected the impact is largest for the low ability individuals for whom there is a large shift from the lowest level of education to the post-reform compulsory level. There is also a marginally significant increase of 1.7 percentage points in University attendance for men only, which is consistent with the improved access that the reform allowed. No other significant change is observed.

For the high ability individuals with unskilled fathers there is a smaller shift from the old compulsory schooling level to the new one, reflecting the fact that many of this group continued beyond that level anyway; however, this effect is significant both in size and statistically and it demonstrates that this kind of educational intervention is relevant even for high ability individuals (with unskilled fathers). These relatively small changes are not reflected in a significant increase in the average years of education for this group.

For those with parents of a higher education level it was not possible to obtain any precise results when splitting by ability. For the group as a whole it is evident that the increase in compulsory schooling had no significant effect at the bottom of the education distribution.

[22] In this case matching takes place within each group including the same variables as before.

In Table 10 we present results pooled for men and women where precision is improved and the overall picture is confirmed: The reform had a large impact on the educational attainment for low ability individuals with unskilled parents and much less so for the educational outcomes of the high ability ones of the same group. The results for thise with skilled fathers are too imprecise to be informative overall. However, it is confirmed that there was no significant effect of the reform on the educational attainment at the bottom of the education distribution, reflecting the fact that irrespective of ability, most individuals with skilled fathers went beyond the old compulsory school level.

5.2 The Effect of the Reform on Earnings

In Table 11 we present estimates of the impact of the reform on log-earnings for both men and women, based on propensity score matching, exactly as in the previous section. Again, we also present the standard deviation of the bootstrap and bias corrected bootstraped 95 percent confidence intervals.[23] The outcome we consider is average log pre-tax earnings over the period 1985-96 as observed in the tax registers.

Overall the reform increased earnings by a significant 4.4 percent. In terms of point estimates the effect seems to be higher for females than for males. Most of the effect comes from a large and significant increase in the earnings of individuals from low parental background of more than 6 percent. The impact is effectively equal for both men and women and it is highly significant. For individuals with skilled fathers the point estimates are negative, which could point to a detrimental effect of abolition of selection. However, these results are completely insignificant.

We then proceed to estimate the effect of the reform separately for high and low ability individuals of low parental background.[24] Overall, as well as for men

[23] The standard errors and the confidence intervals allow for arbitrary serial correlation in earnings and for clustering by municipality. They also allow for the fact that the propensity score is pre-estimated.

[24] No further insights could be gained by splitting up the High parental education group in

Father's education	Low	Low	Low	High
Ability	All	Low	High	All
Education level				
1. Basic School	-0.136	-0.195	-0.076	-0.009
	(0.026)	(0.032)	(0.022)	(0.014)
	[-0.187, -0.092]	[-0.256, -0.138]	[-0.118, -0.039]	[-0.092, 0.002]
2. Comprehensive/	0.106	0.159	0.052	0.058
Junior Secondary	(0.031)	(0.042)	(0.027)	(0.028)
	[0.049, 0.165]	[0.091, 0.250]	[0.004, 0.104]	[-0.014, 0.103]
3. Upper Secondary	-0.021	-0.009	-0.005	-0.016
school $\leqslant$ 2 years	(0.023)	(0.033)	(0.032)	(0.046)
	[-0.078, 0.014]	[-0.077, 0.061]	[-0.072, 0.043]	[-0.285, 0.035]
4. Upper Secondary	0.034	0.019	0.033	-0.178
school $\geqslant$ 3 years	(0.023)	(0.026)	(0.036)	(0.083)
	[-0.016, 0.073]	[-0.048, 0.059]	[-0.026, 0.113]	[-0.308, -0.010]
5. Post Upper Secon-	0.0178	0.011	0.027	0.045
dary $\leqslant$ 2 years	(0.012)	(0.014)	(0.018)	(0.039)
	[-0.007, 0.040]	[-0.019, 0.032]	[-0.004, 0.069]	[-0.034, 0.103]
6.University/College	-0.004	0.017	-0.040	0.091
	(0.012)	(0.010)	(0.028)	(0.071)
	[-0.024, 0.023]	[-0.004, 0.036]	[-0.088, 0.025]	[-0.001, 0.179]
7. Ph.D.	0.003	-0.002	0.009	0.009
	(0.004)	(0.001)	(0.009)	(0.012)
	[-0.003, 0.013]	[-0.006, 0.000]	[-0.010, 0.024]	[-0.012, 0.033]
More than Comprehensive/	0.030	0.036	0.024	-0.049
Junior Secondary	(0.020)	(0.036)	(0.027)	(0.033)
	[-0.029, 0.061]	[-0.042, 0.096]	[-0.026, 0.085]	[-0.089, 0.058]
Years of Education	0.400	0.527	0.182	0.367
	(0.114)	(0.164)	(0.229)	(0.311)
	[0.202, 0.635]	[0.268, 0.845]	[-0.162, 0.742]	[0.009, 1.331]
Sample Size	4,591	2,453	2,138	805

Notes: Standard deviation of the block bootstrap in round brackets allowing for clustering by municipality. Bias corrected 95 % bootstrap confidence interval in square brackets. Matching controls as in Table 7.

Source: IS Survey, 1948 cohort.

Table 8: The impact of the reform on educational qualifications by father's education and ability. Males.

Father's education	Low	Low	Low	High
Ability	All	Low	High	All
Education level				
1. Basic School	-0.124	-0.208	-0.036	-0.011
	(0.023)	(0.043)	(0.013)	(0.010)
	[-0.167, -0.073]	[-0.296, -0.140]	[-0.061, -0.013]	[-0.063, -0.004]
2. Comprehensive/	0.100	0.191	0.012	-0.013
Junior Secondary	(0.022)	(0.031)	(0.032)	(0.041)
	[0.059, 0.140]	[0.125 ,0.247]	[-0.051, 0.072]	[-0.184, 0.041]
3. Upper Secondary	0.032	0.003	0.039	0.041
school $\leqslant$ 2 years	(0.021)	(0.036)	(0.034)	(0.035)
	[-0.004, 0.087]	[-0.056, 0.092]	[-0.045, 0.095]	[-0.045, 0.099]
4. Upper Secondary	-0.015	0.021	-0.055	-0.110
school $\geqslant$ 3 years	(0.015)	(0.012)	(0.028)	(0.055)
	[-0.051, 0.009]	[-0.004, 0.042]	[-0.105, 0.003]	[-0.230, -0.003]
5. Post Upper Secon-	-0.005	-0.008	-0.028	0.038
dary $\leqslant$ 2 years	(0.020)	(0.021)	(0.034)	(0.042)
	[-0.056, 0.030]	[-0.058, 0.024]	[-0.110, 0.023]	[-0.044, 0.106]
6.University/College	0.022	0.001	0.094	0.053
	(0.014)	(0.011)	(0.030)	(0.047)
	[-0.006, 0.045]	[-0.025, 0.0212]	[0.036, 0.156]	[-0.022, 0.150]
7. Ph.D.	-0.009	-	-0.017	0.002
	(0.006)	-	(0.008)	(0.011)
	[-0.029, -0.001]	-	[-0.033, -0.003]	[-0.048, 0.017]
More than Comprehensive/	0.024	0.017	0.034	0.024
Junior Secondary	(0.020)	(0.032)	(0.035)	(0.042)
	[-0.016, 0.067]	[-0.035, 0.096]	[-0.050, 0.099]	[-0.033, 0.159]
Years of Education	0.238	0.364	0.324	0.358
	(0.139)	(0.181)	(0.212)	(0.258)
	[-0.165, 0.458]	[-0.001,0.726]	[-0.165, 0.703]	[0.002, 1.221]
Sample Size	4,483	2,414	2,069	771

Notes: Standard deviation of the bootstrap in round brackets allowing for clustering by municipality. Bias corrected 95 % bootstrap confidence interval in square brackets. Matching controls as in Table 7. Source: IS Survey, 1948 cohort.

Table 9: The impact of the reform on educational qualifications by father's education and ability. Females.

Father's education	All	Low	Low	Low	High
Ability	All	All	Low	High	All
Education level					
1. Basic School	-0.108	-0.128	-0.195	-0.050	-0.011
	(0.019)	(0.023)	(0.033)	(0.013)	(0.010)
	[-0.143, -0.071]	[-0.169, -0.087]	[-0.266, -0.134]	[-0.071, -0.028]	[-0.063, -0.004]
2. Comprehensive/	0.089	0.101	0.171	0.021	-0.013
Junior Secondary	(0.018)	(0.023)	(0.032)	(0.024)	(0.041)
	[0.051, 0.121]	[0.059, 0.143]	[0.106, 0.234]	[-0.026, 0.067]	[-0.184, 0.041]
3. Upper Secondary	0.013	0.005	-0.005	0.015	0.041
school $\leqslant$ 2 years	(0.013)	(0.018)	(0.029)	(0.021)	(0.035)
	[-0.013, 0.038]	[-0.030, 0.037]	[-0.069, 0.042]	[-0.030, 0.050]	[-0.045, 0.099]
4. Upper Secondary	-0.008	0.012	0.015	-0.005	-0.110
school $\geqslant$ 3 years	(0.011)	(0.014)	(0.014)	(0.021)	(0.055)
	[-0.026, 0.012]	[-0.022, 0.033]	[-0.023, 0.038]	[-0.051, 0.040]	[-0.230, -0.003]
5. Post Upper Secon-	0.007	0.004	0.003	0.010	0.038
dary $\leqslant$ 2 years	(0.009)	(0.010)	(0.014)	(0.016)	(0.042)
	[-0.008, 0.027]	[-0.013, 0.025]	[-0.025, 0.027]	[-0.021, 0.040]	[-0.044, 0.106]
6.University/College	0.009	0.010	0.012	0.013	0.053
	(0.010)	(0.011)	(0.008)	(0.019)	(0.047)
	[-0.008, 0.031]	[-0.013, 0.032]	[-0.002, 0.028]	[-0.021, 0.055]	[-0.022, 0.150]
7. Ph.D.	-0.003	-0.003	-0.001	-0.002	0.002
	(0.003)	(0.003)	(0.001)	(0.005)	(0.011)
	[-0.009, 0.004]	[-0.010, 0.003]	[-0.003, 0.000]	[-0.012, 0.007]	[-0.048, 0.017]
More than Comprehensive/	0.019	0.027	0.025	0.030	0.024
Junior Secondary	(0.012)	(0.014)	(0.024)	(0.020)	(0.042)
	[-0.001, 0.047]	[-0.003, 0.052]	[-0.022, 0.067]	[-0.005, 0.069]	[-0.033, 0.159]
Years of Education	0.247	0.318	0.450	0.223	0.358
	(0.095)	(0.099)	(0.130)	(0.139)	(0.258)
	[0.111, 0.513]	[0.139, 0.530]	[0.232, 0.674]	[-0.206, 0.556]	[0.002, 1.221]
Sample Size	10,650	9,074	4,867	4,207	1,576

Notes: Standard deviation of the block bootstrap in round brackets allowing for clustering by municipality.
Bias corrected 95 % bootstrap confidence interval in square brackets. Matching controls as in Table 7.
Source: IS Survey, 1948 cohort.

Table 10: The impact of the reform on educational qualifications by father's education and ability. Males and females pooled.

	Males and Females Pooled	Males	Females
All	0.044	0.026	0.053
	(0.018)	(0.022)	(0.026)
	[0.019, 0.092]	[-0.009, 0.079]	[0.003, 0.106]
Low Father's Education	0.062	0.060	0.061
All abilities pooled	(0.020)	(0.025)	(0.027)
	[0.032, 0.121]	[0.023, 0.123]	[0.007, 0.117]
Low Father's Education	0.039	0.037	0.028
Low Ability	(0.027)	(0.030)	(0.029)
	[-0.007, 0.084]	[-0.012, 0.106]	[-0.039, 0.071]
Low Father's Education	0.075	0.066	0.074
High Ability	(0.028)	(0.046)	(0.043)
	[0.034, 0.139]	[-0.018, 0.153]	[-0.001, 0.158]
High Father's Education	-0.046	-0.053	-0.006
All abilities pooled	(0.043)	(0.055)	(0.050)
	[-0.103, 0.064]	[-0.164, 0.056]	[-0.096, 0.107]

Note: Results obtained by propensity score matching. Standard errors in round brackets allowing for clustering by municipality. Bias corrected 95% bootstrap confidence interval in square brackets. Matching controls as in Table 7.

Table 11: The impact of the reform on earnings.

and women separately, we find that the point estimates of the impact for high ability individuals is nearly twice as high (more than twice for women).

5.2.1 Mobility and the Impact of the Reform

As we reported in Section 4.3 there was a limited amount of selective mobility in and out of reform municipalities. To assess whether this is likely to bias our results we reestimate the effects of the reform on schooling and earnings using those who remained in the municipality of their birth. Our results are presented in Table 12. The results are almost identical to those obtained from the whole sample and not significantly different for any group analyzed. The p-values reported in the table shows that we cannot reject equal returns to the reform in any of the groups, nor for the entire population. Thus it does not seem to be the case that mobility was driven by unobservable characteristics that are relevant for the determination of earnings.

6 Discussion and 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 of evaluating a reform of broad interest using exceptional data.

We consider the impact of the reform on educational achievement and on earnings. The results show an unambiguous increase in schooling for children of both

high and low ability. The results were too imprecise to draw any useful inferences.

	Males and Females Pooled	Males	Females
All	0.059	0.054	0.060
	(0.019)	(0.026)	(0.030)
	[0.035, 0.117]	[0.004, 0.110]	[0.022, 0.141]
Difference to entire sample, p-value	0.395	0.120	0.643
Low father's education	0.061	0.063	0.065
All abilities pooled	(0.019)	(0.073)	(0.028)
	[0.025, 0.108]	[0.013, 0.118]	[0.026, 0.225]
Difference to entire sample, p-value	0.230	0.860	0.840
Low father's education	0.036	0.037	0.032
Low ability	(0.025)	(0.027)	(0.034)
	[-0.026, 0.081]	[-0.019, 0.085]	[-0.042, 0.080]
Difference to entire sample, p-value	0.583	0.977	0.850
Low father's education	0.081	0.107	0.063
High ability	(0.033)	(0.033)	(0.040)
	[0.031, 0.168]	[-0.004, 0.154]	[-0.008, 0.149]
Difference to entire sample, p-value	1	0.677	0.697
High father's education	-0.007	0.042	-0.060
All abilities pooled	(0.073)	(0.156)	(0.091)
	[-0.117, 0.190]	[-0.052, 0.703]	[-0.238, 0.163]
Difference to entire sample, p-value	0.890	0.187	0.413

Note: Results obtained by propensity score matching. Standard errors in round brackets. Bias corrected 95% bootstrap confidence interval in square brackets. Matching controls as in Table 7.

Table 12: The impact of the reform on earnings. Non-movers

genders originating from poorer backgrounds. The increase in education is particularly large for those below median ability. However, we find that education did increase for the high ability individuals as well, particularly for males. We find practically no effect of the reform on the educational attainment of children from wealthier backgrounds, indicating that very few if any of this group would drop out at the statutory schooling level.

Earnings increased significantly overall as a result of the reform and this is mainly due to a large impact on earnings for the high ability individuals from a lower parental background. While the point estimates are positive for the lower ability individuals of the same parantal background, the effect is not as significant. Set against the impact of the reform on educational attainment, it seems to imply that the extra education obtained by the low ability group did not translate to much higher earnings.

However, the education reform seems to have led to important earnings gains for the high ability group with unskilled fathers, who altogether form about 40% of our sample. These gains have come about despite the relatively low impact of the reform on the educational attainment of this group. This may indicate that a small, but significant number of them faced credit constraint that prevented them from taking extra education, despite its obvious financial advantages for them. They may have also been missinformed about the potential benefits. It should also be pointed out though that the reform did not affect just the quantity of education but also the way this was delivered and its overall quality including curriculum changes at younger ages of high school.

The results obtained in this study have a historical significance and an important message about the relevance of educational interventions. Over and above the direct advantages through schooling, since the effect of the reform was also more apparent among individuals with unskilled fathers, the reform is quite likely to have had an effect on intergenerational income mobility. This result confirms previous findings from the sociological literature (see e.g. Erikson and Jonsson,

1993) on increased social mobility among post-reform cohorts.

More generally the results show that education policy widening access, far from being irrelevant, can have important effects at least for children from poorer socioeconomic backgrounds and with higher cognitive skills. Whether the estimated impacts of the reform justify the costs cannot be evaluated from our data. A full cost-benefit analysis of the 1950 Swedish education reform would be a worthwhile exercise. Finally, if achievement at 6th grade is such an important determinant of future success in schooling, as these results may suggest, understanding how to influence this must be an important issue.

Appendix A. The IS Data

The IS survey consists of information from four main sources:

(1) *Student's social background and socioeconomic 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/flickskola"), Upper secondary school ("gymnasium") and Academic education.

(2) *Results from IQ and achievement tests.* The IS surveys contains results from two types of tests: (a) Results from three different types of IQ tests; (b) Results from test on achievement in different subjects taught in school.

(a) IQ tests. The three different IQ tests measure three different 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 flat 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 effect.

(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 five 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 sample averages of the variables included in the propensity score matching analysis. The standard deviations are reported within parentheses for continuous variable. In addition to the variables shown in the table, the propensity score analysis also included 24 indicator variables for county of schooling and 24 indicators for county of living in 1990. Finally, it included 44 ability indicators derived from the 3 IQ tests (Mathematics, Verbal and folding) and 2 school achivement tests (Swedish and English) The results from the tests were transformed, in a first step, to indicator variables measuring decile group of the test result. We carried out a principal component analysis and we used the first principal component to classify individuals in high (above median) and low ability groups. To control for ability in the propensity score we use all 44 principal components of ability.

Table 14 shows the PC loadings for the first 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 14 that the first 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 $Abil3$.

Appendix B: 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 (2001).

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.[25] Hence, the choice of education is driven by life-cycle wealth maximization. Education choice is the

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

	Males				Females			
Father's education	All	Low	Low	High	All	Low	Low	High
Ability	All	Low	High	All	All	Low	All	All
Log annual earnings	7.106	6.965	7.189	7.312	6.660	6.532	6.743	6.806
	(0.838)	(0.818)	(0.814)	(0.891)	(0.948)	(0.954)	(0.910)	(0.983)
Reform assignment	0.338	0.331	0.329	0.396	0.348	0.345	0.336	0.384
Education level 1	0.164	0.270	0.097	0.017	0.129	0.227	0.057	0.018
Education level 2	0.130	0.156	0.125	0.066	0.151	0.190	0.136	0.071
Education level 3	0.276	0.356	0.233	0.145	0.353	0.444	0.306	0.191
Education level 4	0.182	0.130	0.229	0.217	0.128	0.036	0.126	0.135
Education level 5	0.077	0.055	0.091	0.109	0.148	0.027	0.155	0.218
Education level 6	0.160	0.033	0.211	0.310	0.148	0.027	0.155	0.218
Education level 7	0.011	0.0004	0.014	0.035	0.005	0	0.215	0.349
Father's educ., compulsory	0.814	0.959	0.954	0	0.810	0.953	0.966	0
Father's educ., vocational	0.063	0	0	0.420	0.065	0	0	0.417
Father's educ., secondary	0.051	0	0	0.342	0.055	0	0	0.358
Father's educ., university	0.036	0	0	0.238	0.035	0	0	0.225
Father's educ., unknown	0.036	0.041	0.041	0	0.035	0.047	0.034	0
Schooling in Stockholm	0.101	0.074	0.084	0.225	0.094	0.071	0.081	0.192
Schooling in other major city	0.355	0.326	0.344	0.475	0.339	0.340	0.296	0.444
Schooling in village/rural area	0.544	0.600	0.574	0.300	0.567	0.589	0.623	0.363
Municipality income level	4,189	4,001	4,086	5,037	4,248	4,161	4,046	5,011
SEK in 1960	(1,531)	(1,454)	(1,544)	(1,451)	(1,532)	(1,471)	(1,519)	(1,455)
Municipality population size	111.9	91.0	98.6	21,6	120.7	108.8	102.9	200.3
in 1960 in thousands	(236.9)	(212.7)	(221.7)	(310.6)	(247.6)	(232.6)	(233.2)	(304.0)
Municipality tax level	10.67	10.59	10.61	11.07	10.67	10.69	10.54	10.98
in 1960, %	(1.99)	(1.98)	(1.94)	(2.10)	(2.00)	(1.97)	(2.00)	(2.04)
Number of individuals (N)	5,396	2,453	2,138	805	5,254	2,414	2,069	771
Number of observations on earnings (NxT)	61,973	28,015	24,710	9,248	59,334	25,868	24,612	8,854
Number of non-movers	3574	1695	1460	419	3475	1628	1455	394
Number of observations on earnings, non-mover sample	39059	18087	16344	4628	41658	19525	17428	4705

Note: Non-movers: Individuals who went to school in the municipality of birth. In addition to the variables above we have 24 indicators for county of schooling and 24 indicators for county of living in 1990 3 IQ tests and 2 school achievement tests.

Table 13: Sample averages and number of observation by different sub-samples. Standard deviations within parantheses.

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 14: Loadings for principal components Abil, Abil2 and Abil3.

solution to $\max_s\{(1-s)w_1a_i - q_i(s) + \frac{1}{1+r_i}w_2a_im_i(s)\}$. The first and second order conditions for the choice of education level s can be written as

$$\begin{aligned} &First order condition: && \frac{w_2a_im_i'(s)}{w_1a_i+q_i'(s)} = 1+r_i \\ &Second order condition: && w_2a_im_i''(s)-(1+r_i)q_i''(s) < 0. \end{aligned} \tag{1}$$

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 in this model

$$\begin{aligned} &I \quad \frac{\partial s}{\partial r_i} = \frac{w_1a_i+q_i'(s)}{w_2a_im_i''(s)-(1+r_i)q_i''(s)} < 0 \quad II \quad \frac{\partial s}{\partial a_i} = \frac{(1+r_i)w_1-w_2m_i'(s)}{w_2a_im_i''(s)-(1+r_i)q_i''(s)} > 0 \\ &III \quad \frac{\partial s}{\partial w_1} = \frac{(1+r_i)a_i}{w_2a_im_i''(s)-(1+r_i)q_i''(s)} < 0 \end{aligned} \tag{2}$$

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,[26] implies that in this model 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 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

[26]Note from the first order conditions that $w_2m_i'(s) > (1+r_i)w_1$.

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.

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