

Educational Reform, Ability, and Family Background

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In the period between 1950 and the mid-1970s many European countries carried out major educational reforms that resulted in increases in the number of compulsory years of education, in the introduction of national curricula, and in the abolition, or delay, of the placement of more able students into separate schools at an early age (streaming). Examples of such countries are the United Kingdom, France, and the Scandinavian countries.¹ Generally, it is very difficult to evaluate the impact of such reforms, because they are implemented nationwide simultaneously and thus evaluations have to rely on before-and-after comparisons which may confound the effects of the policy with other macro-aggregate or cohort effects.²

In Sweden, major educational reform was designed in the late 1940s. The main elements of this reform were to (a) increase compulsory

schooling to nine years from seven or eight years; (b) abolish placement based on academic achievement into an academic or nonacademic stream after grade six, i.e., at age 12 or 13; and (c) impose a nationally unified curriculum. An attractive feature of this reform is that it was preceded by a social experiment, albeit not randomized, where school reform was implemented gradually across municipalities. This permits an evaluation approach similar to those that have been used in a number of U.S. studies, which have examined the cross-state and cross-time variation in compulsory schooling laws and child labor laws to estimate their impact on educational attainment or to estimate the returns from education.³

In this study we evaluate the effect of the reform on final educational attainment and earnings. We have survey data on two cohorts of pupils: those born in 1948 and those born in 1953. For a substantial portion of the municipalities, these two cohorts were assigned to different school systems: the 1948 cohort to the old system and the 1953 cohort to the new one. In some municipalities, however, both cohorts were assigned to the old system, while in others both cohorts were assigned to the new, reformed system. This allows us to evaluate the reform using a differences-in-differences methodology, comparing outcomes across cohorts and municipalities. The data for the two cohorts contain information on parental background, IQ test scores, and achievement in school (grades by subject) in grade six. Educational attainment and earnings are merged into the original survey data from the national education register and the 1985–1996 tax records, respectively.

The distinctive feature of this policy experiment, combined with the data, is the ability to compare individuals working in the same labor

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¹ See, e.g., Achim Leschinsky and Karl Ulrich Mayer (1990) for an overview.

² See Colm Harmon and Ian Walker (1995).

³ For the former, see, e.g., Robert Margo and Aldrich Finegan (1996). For the latter, see Joshua Angrist and Alan Krueger (1991); the survey in David Card (1999, 2001); and Daron Acemoglu and Angrist (2001).

market at the same point in time who attended two different school systems. In addition, we are able to look at heterogeneous impacts of parental background and ability. In general, low education of parents tends to be associated with low educational outcomes of their children,⁴ possibly because of liquidity or information constraints or lower investments early in life. Our data allow us to estimate the extent to which the reform benefited those with unskilled parents and to see the extent to which such effects differ by ability. Thus our analysis relates directly to key issues of policy as expressed in James Heckman (2000) and Heckman and Alan Krueger (2004).

I. The 1950 Education Reform and the Social Experiment

In the pre-reform school system, pupils attended a basic compulsory school (*folkskolan*) until the sixth grade. Starting in seventh grade, students with better marks were selected for the junior secondary school (*realskolan*). Those who were not admitted continued one or two years in basic compulsory school and thereafter could continue in full-time vocational education.⁵ Compulsory schooling lasted seven years; in some municipalities, mainly in city communities, it lasted eight years. The junior secondary school was a prerequisite for the upper secondary school, which, in turn, was a prerequisite for higher education.

In 1948, a parliamentary committee proposed to replace the compulsory and selective junior secondary school with a nine-year compulsory comprehensive school. The students would be able to choose between three different routes after sixth grade: one with a more academic curriculum, one general level, and one level that included vocational training. There would be no selection based on grades, however, and all pupils would attend the same schools under the

new system. Finally, all schools would have the same national curriculum.⁶

The proposals led to a nationwide evaluation between 1949 and 1962, when it was decided to implement the new school system nationally.⁷ In the experiment, the proposed comprehensive school was introduced by areas, i.e., entire rural municipalities or parts of city communities, rather than by separate schools or classes. When the experiment started, Sweden was divided into about 2,500 city communities and rural municipalities. The number of municipalities was reduced to 1,037, however, in a reform of the municipality system in 1952, which is the municipal division that we use in our empirical analysis.

The municipalities that would implement the new system were not chosen randomly. The National School Board, which administered the experiment, chose them from a group of applicants to form a representative set based on municipality characteristics (see Sixten Marklund, 1981, Ch. 2). The final decision on assignment was made by the municipality council. A means-tested stipend was also introduced in 1953 in the reform areas to ease the perceived financial burden of extending the years of schooling.

New municipalities were added every year. Students who started in the old system stayed in that system. In 1961, at the time when the data for the first cohort were collected (those born in 1948), about 25 percent of the municipalities were assigned to the new system. The national implementation took place in 1962 but the cohort from which the new school system was implemented varied between municipalities. Some implemented the new system for all cohorts up to those who were in fifth grade at the time. Others, however, implemented it starting only with the cohort of pupils who were in first grade in 1962. This implies that in 1966, when the data for the second cohort (born in 1953)

⁴ See, for example, Lorraine Dearden et al. (2002).

⁵ There was also a second chance of entering junior secondary school after completing basic compulsory education. Pupils admitted to junior secondary at this later point would, however, have to begin at the start of junior secondary, leading to a one- or two-year delay relative to the others of the same cohort who were admitted in the first round.

⁶ The reform is described in Sixten Marklund (1981) and Rolland Paulston (1968). Marklund also offers a description of the social experiment preceding it.

⁷ The official evaluation (*Försöksverksamhet med nioårig skolplikt*, 1959) described in Marklund (1981) was mainly of an administrative nature. No educational or labor market outcomes have been considered before.

were obtained, about 30 percent of the municipalities maintained the pre-reform school system for those born in 1953. Thus, for both the 1948 and the 1953 cohorts there are both treatment (reform) and comparison (old system) municipalities. Importantly, there is a large proportion of municipalities for which the 1948 cohort attended the old system and the 1953 cohort attended the new, reformed one.

II. Data: Measurement and Sample Selection

We use data from the 1948 and 1953 cohorts of the Individual Statistics (IS) project of the Institute for Education at the University of Gothenburg.⁸ The 1948 and 1953 cohort surveys were obtained in the spring of 1961 and 1966, respectively, when the respondents were in the sixth grade of compulsory school. The same sampling strategy was used for both surveys, namely that all children born the fifth, fifteenth, or twenty-fifth day of each month were selected to be included in the sample. With a rate of nonresponse for the 1948 cohort survey of about 1.8 percent and for the 1953 cohort of 7.4 percent, the final sample sizes were 11,950 and 9,927, respectively, or about 10 percent of the entire cohorts.

The original dataset includes information on the educational level of each student's parents; results from three different IQ tests (number series, opposites, and a test of spatial IQ referred to as folding); administrative information on grades (Swedish, English, and mathematics); and type of school. Thus all measures of student ability were obtained in sixth grade. We use the test scores to construct ability indicators included in the empirical analysis. Information on final educational attainment was obtained from the 1990 Swedish education register.⁹ Finally, information on annual earnings and employ-

ment status for each year from 1985 to 1996 was obtained from the Swedish tax registers.¹⁰

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., Olle Westerlund, 1995). Importantly, all counties but one for the 1948 cohort and all counties for the 1953 cohort had some reform and some non-reform municipalities or parts of city communities. The number of observations in the final sample was 10,309 (5,235 men and 5,074 women) for the 1948 cohort survey and 9,007 (4,525 men and 4,482 women) for the 1953 cohort survey. For each we observe earnings for the entire 1985–1996 period, or a part thereof.

III. Estimating the Impact of the Reform on Educational Qualifications and Earnings

To evaluate the impact of the reform, we consider years of education, level of education as measured by two binary outcomes (whether the final completed level of education was the new compulsory level or any other, and whether the completed level of education was more than the new compulsory level or any other), and log annual earnings over the years 1985 to 1996.

The reform was carried out in entire municipalities or communities within large cities. For the 1948 cohort, 35 percent of pupils were assigned to the new system throughout the country, based on where they lived when they were in sixth grade. The remaining pupils were assigned to the old system. In the case of the 1953 cohort, 81 percent were assigned to the reformed system.

The sample can thus be divided into the following groups: (a) municipalities or city communities in which the 1948 cohort was not assigned to the reform but the 1953 cohort was (these are the municipalities that switched); (b) municipalities in which both the 1948 and the 1953 cohorts were assigned to the reform; (c) municipalities in which neither the 1948 nor the 1953 cohort was assigned to the reform.

⁸ See Kjell Härnqvist (2000) for a detailed description of the project and the data.

⁹ We also summarize overall educational attainment using years of education. Since this is not observed directly we impute it by assigning years of education based on the Swedish Level of Living Survey to each of seven educational levels that we observe in the data.

¹⁰ Details of the data can be found in Meghir and Palme (2003).

We make the following assumption: in the absence of reform, the changes in the average outcomes¹¹ between the 1948 and the 1953 birth cohorts living in the municipalities that adopted the reform would have been the same as the changes for those living in the municipalities whose reform status remained the same for these two cohorts—conditional, that is, on observed characteristics. We also assume that municipalities did not change teaching methods or otherwise take preemptive action before they were assigned to the reformed system. Under these assumptions we can evaluate the impact of the reform using differences-in-differences (Orley Ashenfelter, 1978; Heckman and Richard Robb, 1985); that is, we compare the change in average outcomes between the 1948 and 1953 cohorts for individuals living in the municipalities that switched when in sixth grade to the change in average outcomes for the same cohorts of individuals living in the municipalities that did not change status. As a robustness check we also estimate the impacts separately using as comparison groups either individuals living in municipalities that implemented the reform for both cohorts (group [b]) or individuals living in municipalities that kept the old system for both cohorts (group [c]).¹²

The linear regression that implements the differences-in-differences estimator is

$$(3.1) \quad Y_{idm} = b_0 + b_1 d_i + b'_2 \mathbf{m}_i + \alpha r_{idm} + \gamma' \mathbf{x}_{idm} + e_{idm}$$

where Y_{idm} is the relevant outcome observed for individual i belonging to cohort d and municipality m , d_i is a dummy variable indicating the cohort to which the individual belongs, and \mathbf{m}_i is a set of dummy variables indicating the municipality in which individual i went to school;¹³ r_{idm} is 1 for individuals belonging to a

cohort and municipality assigned to the reformed system. The parameter α is the average effect of the reform for those assigned to it among the population from which the sample is drawn.¹⁴ The variables \mathbf{x}_{idm} reflect the observable characteristics of individual students. Finally e_{idm} represents a random error term assumed uncorrelated with r_{idm} conditional on the other regressors, i.e., we assume that $E(e_{idm} | r_{idm}, d_i, \mathbf{m}_i, \mathbf{x}_{idm}) = E(e_{idm} | d_i, \mathbf{m}_i, \mathbf{x}_{idm})$. The additive municipality (m) and cohort effects (d) in (3.1), together with the stated properties of the error term, reflect the assumptions underlying the differences-in-differences approach, as stated above.

For log earnings and years of education, we use ordinary least squares (OLS) on (3.1). For the discrete educational outcomes we use a probit model. This requires the additional assumption that e_{idm} is normal conditional on r_{idm} , d_i , \mathbf{m}_i , and \mathbf{x}_{idm} . However, a linear probability model gives almost identical results. In all cases the covariates x include indicators for test scores and school grades obtained when the pupils were in sixth grade and for the country of work.¹⁵ When we present results pooled across males and females we include a gender dummy on its own and interacted with all \mathbf{x} 's. Since for each individual we have repeated earnings observations over the period 1985–1996, we also include time dummies for these years in the earnings regressions. Thus we effectively compare the earnings of individuals within the same local labor market and year.

In computing the standard errors we allow for arbitrary municipality level spatial correlation and, in the case of earnings where we have multiple observations over time, for arbitrary serial correlation as well.¹⁶

Table A1 in the Appendix presents descriptive statistics and reports the difference in some characteristics between individuals living in

¹¹ Educational attainment and earnings.

¹² In a few municipalities, because of boundary changes, some individuals end up being assigned to the reform and others not. We retain these observations.

¹³ For pupils educated in Stockholm, Gothenburg, and Malmo, we are not able to include fixed effects for the particular community in which they live. However, we include dummies for each of these cities.

¹⁴ So, for example, when we estimate using the pooled sample of males and females with unskilled fathers, the effect is the average effect over that population.

¹⁵ The test scores include three IQ tests as shown in Table A1 in the Appendix, and student grades in Swedish, English, and mathematics.

¹⁶ See Brent Moulton (1986) for the importance of controlling for cluster effects.

switching and nonswitching municipalities. These differences are not significant, implying there would be no bias if we excluded these covariates from the regression. We still include the covariates, however, to improve estimated precision, because they are highly correlated with both educational attainment and earnings.

A reform of this magnitude may have had aggregate effects on the labor market, including on the price of labor. However, since we compare workers within the same labor markets our estimates do not include such effects.¹⁷ They reflect the relative merits of the two school systems given the overall macroeconomic conditions.

IV. Results

A. *The Effect of the Reform on Education*

The estimates of the impact of the reform on education are shown in Table 1. These are presented for the whole sample and by gender. We also break them down by father's education (low and high)¹⁸ and ability (low and high).¹⁹ We refer to fathers with low education as unskilled fathers. For years of education we report the OLS estimate of α from equation (3.1) which represents the effect of the reform measured in years of education. For the educational levels the estimates are the marginal effects of the reform from a probit model, based on (3.1), multiplied by 100 to transform them to percentage point effects on the probability of attaining the level in question.

The average effect of the reform for men and women was to increase the proportion attending through to the new compulsory level by 8.5 percentage points, and beyond that level by 2.6 percentage points, showing that the reform had

an impact beyond the new compulsory level; this is consistent with the aim of the reform to improve access to higher levels of education.

All changes in educational attainment taken together translate into an increase in years of education by 0.298 of a year. These effects are highly significant. The entire effect is due to the increase in the educational attainment of those with unskilled fathers (column 2). Within that group, those with low ability increased their attainment by moving up to the new compulsory level with an almost equal drop in the proportion attending the former compulsory level. For those of higher ability, however, the increase in attainment is reinforced by a large increase beyond the new compulsory level (column 4). Thus it seems that the measures for improving access beyond the new compulsory level had an important impact for the more able students with unskilled fathers.

Table 1 also breaks down the effect by men and women. The main difference revealed here is that the increase beyond the new compulsory level is stronger for women.

B. *The Effect of the Reform on Earnings*

We estimate the impact of the reform on average annual log-pre-tax earnings for both men and women, with positive earnings for all or part of the years in the 1985–1996 period for which earnings are observed.²⁰ We report the OLS estimate of α from equation (3.1) multiplied by 100, so that the reported effects can be interpreted as the percent effects on earnings averaged over the observation period. The results are shown in Table 2.

The overall effect of the reform on earnings at 1.42 percent was small and only significant at the 10.9-percent level. However, this conceals substantial heterogeneity in the effects for different groups of individuals. For those with unskilled fathers the reform increased earnings by 3.4 percent, which is highly significant. In terms of the point estimates, the effect is larger for the high-ability individuals but the difference is not significant. The difference between

¹⁷ See Kevin Lang and David Kropp (1986).

¹⁸ We classify as unskilled or low-education fathers both those who received just the statutory level of education and those whose educational classification is missing from the data.

¹⁹ To define the ability groups we take the average IQ score across three different IQ tests for each person and classify individuals with scores above the sample median as high ability. For the cases where we do not have IQ test scores we base our classification on average school grade at the same age.

²⁰ There was no effect of the reform on the proportion of individuals with positive earnings.

TABLE 1—THE IMPACT OF THE REFORM ON EDUCATIONAL ATTAINMENT

	(1)	(2)	(3)	(4)	(5)
Father's education ^a	All	Low	Low	Low	High
Ability ^b	All	All	Low	High	All
<i>Men and women</i>					
Change in percent attending: ^c					
Comprehensive/junior secondary	8.54 (1.67)	10.31 (2.13)	17.50 (2.60)	1.97 (2.66)	2.15 (1.25)
More than comprehensive/junior secondary	2.61 (1.14)	3.26 (1.42)	1.29 (1.99)	7.35 (2.73)	-1.23 (1.44)
Change in years of education	0.298 (0.075)	0.405 (0.070)	0.467 (0.098)	0.355 (0.095)	-0.130 (0.124)
Years of education in non-reform areas	11.19 (0.032)	10.78 (0.033)	9.89 (0.036)	11.93 (0.055)	13.69 (0.085)
Sample size	19,316	15,989	8,633	7,356	3,327
<i>Men</i>					
Change in percent attending: ^c					
Comprehensive/junior secondary	9.84 (2.35)	12.22 (2.96)	18.57 (3.86)	4.78 (3.92)	2.06 (1.58)
More than comprehensive/junior secondary	0.76 (2.17)	1.15 (2.63)	-0.20 (3.20)	4.04 (3.75)	-1.46 (1.68)
Change in years of education	0.252 (0.081)	0.300 (0.093)	0.450 (0.105)	0.104 (0.155)	0.092 (0.174)
Sample size	9,760	8,084	4,260	3,824	1,676
<i>Women</i>					
Change in percent attending: ^c					
Comprehensive/junior secondary	7.46 (1.85)	8.99 (2.48)	18.18 (2.92)	-1.26 (4.40)	3.20 (2.52)
More than comprehensive/junior secondary	4.65 (1.85)	5.75 (2.15)	2.04 (3.26)	8.59 (4.06)	-2.00 (2.70)
Change in years of education	0.339 (0.105)	0.512 (0.087)	0.479 (0.140)	0.585 (0.127)	-0.415 (0.193)
Sample size	9,556	7,905	4,373	3,532	1,651

Notes: Asymptotic standard errors in parentheses allowing for clustering by municipality.

^a Low father's education: the father had just compulsory schooling of his time or father's education was missing.

^b High ability: individual IQ score above median.

^c This is measured in percentage points.

the two ability groups, however, is high for women and has a p-value of 5 percent; this is consistent with the fact that the reform had a greater effect on educational attainment for higher-ability women than men of the corresponding group, but about the same for low-ability men and women.

One of the striking results (which explains the small overall effect) is the negative impact (-5.6 percent) on the earnings of individuals with skilled fathers, who constitute 17 percent

of the population. This is true for both males and females, despite the fact that there is no significant change in educational attainment for the group. These individuals used to attend primarily the academic junior secondary school, which selected students on the basis of level of attainment in sixth grade. The reform abolished this selection and it seems that this reduced the quality of education and ultimately the earnings of this group.

Finally, since both the amount of education

TABLE 2—THE IMPACT OF THE REFORM ON EARNINGS

	Differences-in-differences		
	Males and females	Males	Females
All	1.42 (0.89)	0.88 (1.37)	2.11 (1.24)
Low father's education All abilities pooled	3.36 (0.91)	3.06 (1.36)	3.79 (1.30)
Low father's education Low ability	2.62 (1.28)	3.23 (2.03)	1.66 (1.59)
Low father's education High ability	4.53 (1.27)	2.97 (1.87)	6.71 (2.02)
High father's education All abilities pooled	−5.59 (1.88)	−7.66 (3.12)	−4.22 (2.72)

Notes: Standard errors in round brackets allowing for clustering by municipality, thus also allowing for arbitrary serial correlation. Coefficients are interpreted as percentage effects on annual earnings.

and the quality/type of education changed for individuals of all abilities among those with unskilled fathers, it is not possible to say which aspect of the reform led to the earnings gains. If all the earnings changes were due to changes in the quantity of education, this would correspond to a return to education of 5.6 percent for the low-ability individuals and 12.8 percent for the high-ability ones (8.4 percent overall); if one accepts that the other changes enhanced earnings for this group by improving the quality of education, these will be upper bounds to the effects of education on earnings. As a benchmark for the magnitude of these effects, note that Anders Björklund (2000) estimated the wage premium per additional year of education to be 4.6 percent for Sweden.

When we break down the effects by gender we note that the effects for individuals with unskilled fathers are higher for women than men, but both are significant. For men we cannot distinguish much of a difference between those of high and low ability; however, for high-ability women the effect is larger than for lower-ability women, and the difference is significant.²¹

²¹ The presence of municipality cluster effects implies that the estimates are correlated across subsamples. We used

Did the reform affect earnings growth or was the effect uniform over the life cycle? Figure 1 shows log real earnings over time for all males and females whose fathers have a low level of education. In the left-hand panel we present the log earnings for those born in 1948 for the years 1985–1996. We have split the sample depending on which type of municipality individuals live in: the line with circles relates to those living in municipalities that had not implemented the reform *and* will still not implement it for the 1953 cohort (*the never treated*). The line with squares relates to those living in municipalities that had already implemented the reform for the 1948 cohort and also for the 1953 cohort (*the always treated*). Finally the line with triangles relates to those living in municipalities that had not implemented the reform for the 1948 cohort but *did* implement it for the 1953 cohort (*the switching municipalities*).

Comparing the *never treated* graph to the one for the *switchers* we can see that there are no significant systematic differences between the two groups for the 1948 cohort. We expect the graph for the *always treated* to be higher since this reflects the effect of the reform. The graph for the *switching* municipalities, relative to the *never treated* once the reform is implemented for the 1953 cohort, reflects the impact of the reform. This is shown in the right-hand panel where the earnings for the 1953 cohort are indicated. The graph for those in the *switching* municipalities has now moved up and overlaps with that for those in the *always treated* group, reflecting the impact of the reform. The graph seems also to suggest that the reform had a positive effect on earnings growth; however, we find this difference to be insignificant once we apply the differences-in-differences estimator for growth of earnings over time.

Sensitivity Analysis.—In Table 3 we present the results of some sensitivity analyses to demonstrate the robustness of the results. For the sake of brevity we present only the average effects across males and females split by father's education,

the block bootstrap and obtained a p-value for the difference of zero.

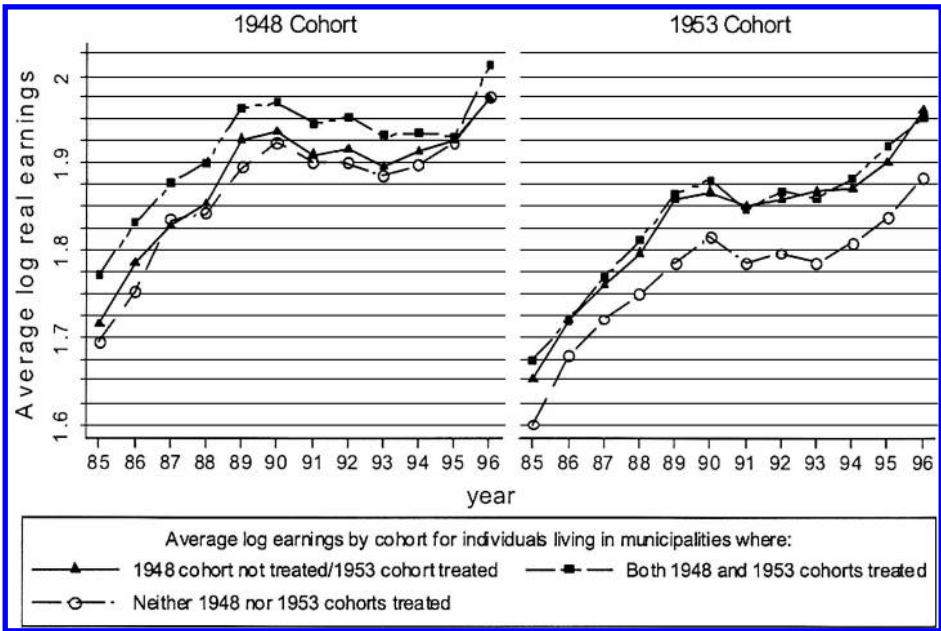


FIGURE 1. AVERAGE LOG EARNINGS FOR THE 1948 AND 1953 COHORTS BY REFORM STATUS AND YEAR

TABLE 3—RESULTS FROM SENSITIVITY ANALYSIS ON THE IMPACT OF THE REFORM ON EARNINGS

	(1)	(2)	(3)	(4)	(5)
	Males and females ^a				
	Alternative comparison groups			Impact of mobility	
	Reform for both cohorts	Non-reform for both cohorts	Excluding Stockholm, Gothenburg, and Malmo	Excluding movers ^b	IV ^c
Low father's education All abilities pooled	3.29 (0.96)	3.14 (0.94)	3.77 (1.18)	3.25 (0.94)	4.20 (2.67)
High father's education All abilities pooled	-6.25 (1.88)	-6.43 (2.05)	-4.11 (3.05)	-4.40 (2.15)	-17.52 (12.79)

^a Asymptotic standard errors allowing for clustering by municipality in parentheses.

^b We exclude those who as a result of a move changed reform status from that implied by the municipality of birth.

^c In IV we instrument actual reform status using the status predicted by the municipality of birth.

which seems to be the most important source of heterogeneity for the impact on earnings.

We reestimated the model using first, as controls, individuals living in municipalities that implemented the reform for both cohorts (column 1, Table 3) and then using as controls individuals living in municipalities that kept the old system for both cohorts. As can be seen in

Table 3, the results are remarkably similar to those presented in Table 2, which uses both comparison groups.

In column 3 we show results when excluding the three big cities of Stockholm, Gothenburg, and Malmo to check whether the effects are different in the rest of the country, which is underrepresented in the reform. Again the

effects are very similar to those reported in Table 2.

Finally, we estimated the effects using propensity score matching based on the 1948 cohort only. These estimates do not control for unobserved municipality characteristics but do not rely on additive (in logs) cohort effects since we compare only within cohort. The results, reported in detail in Meghir and Palme (2003), are less precise but display very similar patterns.

Selective Mobility and the Impact of the Reform.—Families may have moved residences to choose suitable schooling for their children. We use the municipality of birth to assess the extent to which this may have affected the results we report. For some individuals it is not possible to know whether they would have been in the reform or not on the basis of their place of birth, mainly because the municipality borders were adjusted after the birth of our cohorts. Overall we were able to classify 87.3 percent of the sample. Of these, 90.1 percent did not change reform status from that indicated by place of birth. Of the 9.9 percent who we know changed status, 5.3 percent moved from a reform to a nonreform municipality and 4.6 percent moved in the other direction. Further details on this for the 1948 cohort are given in Meghir and Palme (2003).

We can use the birth municipality in two ways. First we reestimate the model only for those who did not change status as a result of moving out of the municipality of their birth. The results are shown in column 4 of Table 3 and they are not significantly different from those in Table 2. Based on a Hausman test,²² for those with unskilled fathers the difference has a t-value of 0.47. For those whose fathers have a higher level of education, we still

get a negative effect but smaller in absolute value. The t-statistic for the difference from the estimate in Table 2 is 1.14, which is not significant at the 5-percent level.

We also reestimated the model using instrumental variables, treating reform allocation as endogenous even conditional on the observables and the municipality fixed effects. The instruments are (a) whether we know the reform status or not based on the birth municipality; and (b) if we know the reform status, whether it is reform or non-reform. The instruments are highly significant with a p-value of zero, both overall and for the two subsamples of higher and lower education of the father. For those with unskilled fathers the point estimate is almost the same (column 5). For those with skilled fathers, however, the IV estimator is too imprecise, although it is still negative as in all other cases.²³ Thus there is no strong evidence that mobility is a source of bias for these results.

V. Conclusion

The reform of the Swedish education system had important effects on the educational attainment and earnings of individuals. We establish a large increase in the share of students reaching the new compulsory level for the group with unskilled fathers and low ability. More interestingly, the reform led to an increase in schooling beyond the new compulsory level for individuals with higher ability and unskilled fathers. This may have helped increase intergenerational mobility by improving access beyond the new compulsory level. We also find that labor market earnings increased significantly for individuals with unskilled fathers; there are also indications, however, that earnings fell for individuals with higher-skilled fathers, indicating that the abolition of selection may have reduced the quality of education obtained by this group. Nevertheless, the reform improved on average both the educational attainment and the earnings of a large part of the population.

²² The rationale for carrying out a Hausman test is as follows: (a) under the null hypothesis that movers are randomly selected, the estimation based on non-movers estimates only the same parameter but is inefficient relative to using the entire sample; (b) under the alternative, the estimates based on each of the samples will converge asymptotically to different points in the parameter space providing power to the test, because the samples are selected endogenously, but each in a different way.

²³ This is because of the combination of the limited mobility and the inability to classify a substantial number of individuals to reform/non-reform status using the municipality of birth.

APPENDIX: DESCRIPTIVE STATISTICS

TABLE A1—DESCRIPTIVE STATISTICS AND DIFFERENCE IN CHARACTERISTICS

Descriptive statistics ^a				
Variable	1948 Cohort		1953 Cohort	
	Reform	Non-reform	Reform	Non-reform
Reform assignment, %		34.8		81.1
Females, %	50.0	48.8	49.8	49.5
Spatial IQ ^b	21.83 (7.16)	21.04 (7.08)	22.16 (7.35)	21.11 (7.29)
Verbal IQ ^b	22.94 (6.64)	22.53 (6.77)	24.00 (6.66)	22.89 (6.72)
Mathematical IQ ^b	19.80 (7.71)	19.70 (7.77)	20.48 (8.01)	19.27 (8.01)
Father's education more than compulsory, %	17.2	13.8	20.9	14.8
Years of schooling	11.93 (2.72)	11.27 (3.09)	11.61 (2.50)	10.84 (2.73)
Less than 9 years of schooling, %	3.0	20.7	1.7	19.0
9 years of schooling, %	21.4	10.2	29.9	20.0
More than 9 years of schooling, %	73.7	67.6	68.4	61.0
Sample size, cross-section	3,583	6,726	7,303	1,704
Log labor earnings	7.29 (0.58)	7.24 (0.60)	7.19 (0.60)	7.11 (0.61)
Sample size, labor earnings	39,239	73,468	78,582	18,394
Differences between municipalities that switched reform status and those that did not				
	Rest of the country ^c		Stockholm, Gothenburg, and Malmo	
	Men	Women	Men	Women
IQ average differences in scores (expressed in percentage of test score)	1.91 (2.46)	0.52 (2.21)	−1.50 (2.10)	3.12 (8.57)
Difference in percentage points of father's education > basic compulsory	2.2 (1.5)	−0.4 (1.4)	4.8 (5.4)	7.7 (5.7)
Area characteristics (rest of the country) ^{c,d}				
	Switchers		Non-switchers	
Average population size	19,100		13,590	
Mean taxable income, SEK in 1960 prices	3,857		3,827	
Mean municipality income tax rate, %	10.04		10.04	

^a Standard deviations in parentheses.
^b Test scores range between 0 and 40.
^c Rest of the country excludes Stockholm, Gothenburg, and Malmo.
^d Area characteristics are not available for reform versus non-reform communities within Stockholm, Gothenburg, and Malmo.

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