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ABSTRACT

Measuring Heterogeneity in the Returns to Education in Norway Using Educational Reforms*

The decision to take more education is complex, and is influenced by individual ability, financial constraints, family background, preferences, etc. Such factors, normally unobserved by the researcher, introduce endogeneity and heterogeneity problems into estimating the returns to education. In this paper, these problems are addressed by estimating a comparative advantage model for schooling, in which the returns to education vary at different levels of education. The model requires that instruments must be specified at each level of education, and we suggest that different school reforms in Norway can serve as suitable instruments. In particular, we exploit the staged implementation of a major reform in the comprehensive school system in the 1960s. We find that the returns to education are strongly nonlinear. In particular, we find that the returns to upper secondary school and shorter programs at regional colleges, together with master's programs at universities, have high returns as measured by wages. Also, we find that the average treatment effect is surprisingly high for medium-length educations (up to two years of college education). This means that increasing the general level of education, which was the intention of the comprehensive school reform of the 1960s and of other school reforms, has the potential to generate a high return in wages, although we do not consider the cost to society. We also find that there is a substantial difference between the average treatment effect and the effect of treatment on the treated for bachelor's and master's degrees at universities.

JEL Classification: C3, I2

Keywords: educational reforms, returns to education

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1 Introduction

There is much controversy regarding the measurement of returns to education, especially because of selection problems and heterogeneity in returns; see, for instance, recent contributions by Card (1999, 2001), Carneiro and Heckman (2002), Carneiro, Heckman and Vytlacil (2001) and Blundell, Dearden and Sianesi (2002). The main problem in measuring returns to education is that the decision to take more education is a complex process. Factors such as individual ability, financial constraints, family background and preferences are usually unobserved by the researcher. This creates an endogeneity problem inherent in most evaluation and labor market studies; see Griliches (1976), Heckman (1974, 1976) and Gronau (1974). An additional problem relates to observed and unobserved heterogeneity in the return parameters of education and the interpretation of different return parameters; see Lang (1991), Willis and Rosen (1979), Card (1995, 1999) and Heckman and Vytlacil (1999). This heterogeneity arises if individuals select their education on the basis of their comparative advantages; see Roy (1951), Garen (1987) and Willis and Rosen (1979).

Estimating a comparative advantage model for schooling with many educational levels (the generalized Roy model)[1], in which returns vary between individuals at each level of education, requires instruments to be specified for each level of education in order to identify the return parameters, unless very restrictive functional form assumptions are imposed. Our main contribution to this literature is to exploit features of different school reforms in Norway to estimate a Roy model for returns to education in which several levels of education are specified.

The main reform we use in our identification strategy is the school reform extending mandatory years of schooling from seven to nine years. This reform took more than ten years to implement. For those years, we observe the same birth cohorts going through both types of compulsory school system. In addition, we use sequential education reforms in Norway at the upper secondary, college

[1] This approach follows Willis and Rosen (1979), Garen (1987), Bjørklund and Moffitt (1987) and Heckman and Vytlacil (1999). Papers estimating the returns to education using this approach usually specify the Roy model for only two levels of education (noncollege and college degrees). For models of this kind using discrete outcome variables, see Aakvik et al. (2000, 2003).

and university education levels. The identification strategy uses a difference in differences approach for identification, as in Duflo (2001), to identify the return to each schooling level. We use as instruments the reform of compulsory schooling and additional information on whether schools were providing the six levels above basic schooling in the municipality, and allow for interactions between the availability of schools at different levels and the compulsory school reform.

As well as using detailed data on educational reforms, we exploit an extremely rich database for Norway consisting of very detailed register information on the human capital characteristics of the individuals for the 1948 to 1957 cohorts that were exposed to the reforms. We also have detailed background information on their parents including their education, income, the number of children in the family, and the municipality in which they grew up. These data are from the 1960 and 1970 Censuses. A feature of the data on employer-employee relationships is exploited when calculating tenure. The net sample comprises 160,000 individuals born from 1948 to 1957.

We assess the effect of the reform on increasing participation rates in higher education in general. Furthermore, we test whether the increased education due to the reforms led to higher returns to education. Family background, especially fathers' and mothers' education, is a very important determinant of participation in higher education. We also find that this effect was weaker after the mandatory school reform was introduced. When estimating returns to education, we find that the returns to education are highly nonlinear and that the Roy model is better than a traditional instrumental variables model in which schooling is a continuous variable. In particular, we find that the returns to upper secondary school and up to two years of education at regional colleges, together with master's programs at universities, have a high return as measured by wages. Also, we find that the average treatment effect is surprisingly high for medium-length education (up to two years of, usually regional, college education). This means that increasing the general level of education, which was the intention of the comprehensive school reform of the 1960s and other school reforms, has the potential to generate a high return in wages, although we do not consider the cost issue for society. We also find that there is a substantial difference between the average treatment effect and the effect of treatment on the treated for bachelor's and master's degrees.

Whereas the difference between the average treatment effect and the effect of treatment on the treated is only a few percentage points for educational levels up to bachelor's degree level, the difference in the average treatment effect and the effect of treatment on the treated is more than 12 percentage points per year for bachelor's and master's degrees.

The rest of the paper is organized as follows. Section 2 discusses the data sets and defines variables used in the analysis. Section 3 provides background information on the school reforms in Norway. Special attention is paid to our identification strategy and the robustness of the instruments. Section 4 presents our econometric framework for estimating the returns to education. Section 5 reports the determinants of school choice from the regression analysis. In addition, we report results from earnings regressions and different policy relevant parameters, such as the average treatment effect and the effect of treatment on the treated, from a model that allows for selection into educational levels on the basis of comparative advantage. The final section presents our conclusions.

2 Data Set and Variables

The main data sources for our study are administrative registers from Statistics Norway. Each individual is characterized by his or her personal identity code and information from different administrative registers is merged for each person in the population. The data set covers persons working in all sectors - private manufacturing, private services, and the public sector - in 1995, for the birth cohorts of 1948 to 1957. For the wage regressions, we use information on experience, seniority, years of education, type of education, annual income, and the employment relationship. We can calculate seniority and quantify the employment relationships because we have a merged employer-employee data set.

In addition to the information on education for each person, we use information on family background for the period in which the person grew up and started compulsory education. This includes parents' or guardian's income, their education and municipality and county. This information is from the National Censuses of Population and Housing in 1960 and 1970; see Vassenden (1987).

We use both the type and number of years of education received in the empirical analysis. *Years of education* is based on the normal duration of the education. It includes only completed (and highest attained) education, and all formal education courses exceeding 300 hours are registered. This variable has 14 values (from seven to 20 years of education). *Type of education* is based on characteristics of the Norwegian education system and Statistics Norway's standard classification of education. We group our sample into the seven levels presented in Table I.

Table I (Definition of education levels applied in the analysis)

This method of defining the education categories in Norway is very common and is used in the Norwegian educational statistics. It reflects the school system and includes vocational schools, upper secondary schools, up to two years of education in regional colleges, university degrees of three to four years, and degrees of five years or more. This classification fits nicely with the Roy model for education, assuming that individuals have comparative advantages at certain educational levels and act on these when choosing education. One would expect that students primarily think in terms of types of education - for instance of becoming a history teacher - and then perhaps consider years of education for instance, of becoming a history teacher in the primary school system requiring an undergraduate university degree or of teaching in high school with a graduate level degree. A model that specifies the type of education is expected to clarify results of student choices based on comparative advantages. Although we have seven different levels of education, we only have five different types of school. The two vocational levels correspond to the same type of school. In addition, the two upper educational levels correspond to university degrees (bachelor's and master's degrees or higher).

Annual *earnings* in 1995 were calculated from annual taxable income as reported in the tax register. *Tenure* is defined as the number of years spent working for the current employer. *Work experience* is based on the number of years in which annual earnings exceed the basic minimum level of the old-age pension. In 1995, this amounted to NOK 40,000 (around USD 5000). This is our best approximation of the number of years spent in the labor market; see Bratberg and Vaage (2000).

To measure labor market outcomes, we use annual earnings. Furthermore, we restrict our sample to full time workers, defined as those working more than 30 hours a week, since annual earnings is used as the outcome variable. Workers holding multiple jobs, self-employed workers and workers participating in labor market programs or receiving unemployment benefits were all excluded from the sample.

For family income, we sum the father's and mother's income in 1970, collected from the Census data. We divide family income into groups based on percentiles. The father's and mother's education is represented by a dummy variable indicating whether or not they have a college education. The data selection process is illustrated in Table II.

Table II (Data selection process)

The ten different male cohorts born between 1948 and 1957 amount to 295,646 individuals. Data on parents is missing for more than 20 percent of the individuals in the sample. Data on parents are important for two reasons. First, they are used to determine where individuals grew up and in which municipality they went to school. This is important information because it is used to construct our instruments (the availability of different types of school). Second, family background variables, such as parental education and income, are important factors in modeling the level of education. We have removed from the sample individuals with missing parental information from the Census data from 1960 or 1970.

When looking at the returns to education, we only use full time working persons. We exclude persons in this age cohort who have no work or are working part-time, or have missing data on tenure. This amounts to about 20 percent of the sample. Descriptive statistics for the net sample are provided in Table III.

Table III (Descriptive statistics for the net sample of 159,452 individuals)

The age in 1995 for persons in our sample varies from 38 to 47 years. Mean work experience is 21 years with a standard deviation of 4, and mean tenure is 7.5 years with a standard deviation of almost 6. Mean years of education is 12.1. Around 10 percent of the sample has a father with a college degree. This is not surprising since the level of education for persons with children born in the 1950s is low. Only 3 percent of the sample has a mother with a college degree. The availability of different types of school is an important determinant of educational choice. Eighty percent of those in our sample lived in a municipality that offered vocational training and education. Sixtyeight percent of the sample lived in a municipality that had upper secondary school. Fortytwo percent had a regional college in the municipality in which they grew up. About 20 percent could go to a university in their home municipality. The universities in Norway are located in Oslo, Bergen, Trondheim, and Tromsø.

3 Education in Norway

From the 1960s to the 1990s, the Norwegian education system went through several major reforms. The starting point was reforming compulsory primary and junior secondary schools, which happened primarily in the 1960s, and extending the minimum mandatory education period from seven to nine years. During a ten year period, two distinct school systems co-existed: there were municipalities with nine years of mandatory schooling and others with only seven years of compulsory schooling. The potential effects are expected to be larger and thus easier to measure in the case of Norway than in most other countries, because the Norwegian reform went further both in unifying the comprehensive school system and in promoting equality of opportunity; see Leschinsky and Mayer (1990).

The reform process then continued to (voluntary) upper secondary schools ("gymnas") in the mid 1970s. At almost the same time, it reached the post upper secondary/college level, where the reform process lasted until the present college structure was launched in the early 1990s. These reforms are used in this paper as part of the identification strategy for estimating the returns to education. The reform of the mandatory schooling period was probably the most influential, and is closest to being a natural experiment. Hence, it constitutes the main element in our identification strategy. We provide information on the aims of the reform, procedures for implementing it and how it was financed, etc.

3.1 The Mandatory School Reform

The mandatory school reform, enacted by the Norwegian Parliament in 1959, was started in 1960, and lasted until 1972.[2] The reform extended the number of compulsory years of schooling and unified the education system at the tertiary level.[3] In the Norwegian pre-reform system, children started school at the age of seven and finished their compulsory education after seven years, i.e., at the age of 14. The old system had a parallel system of voluntary lower secondary education, comprising two years of junior high school ("realskole") in preparation for upper secondary school, followed by university education, and the so-called continuation school ("framhaldsskole") which prepared students for vocational training and general training outside the systems of higher professional training and theoretical education. Selection into junior high school was based on grades, and it was available in some municipalities but not in others. In the new system, the starting age remained at seven years, but the time spent in compulsory education increased to nine years. The nine years were divided into two levels: first, six years of primary school; and second, three years of secondary school, which prepared students for high school.[4]

The aims of the reform were stated explicitly in several governmental background papers. These were: 1) to increase the minimum level of education in society by extending the number of compulsory years of education from seven to nine; 2) to smooth the transition to higher education by unifying the education

[2]See *Lov om folkeskolen av 1959* (Law on primary schooling). The reform had already started on a small and explorative basis in the late 1950s, but applied to a negligible number of students because only three municipalities, each with a small number of schools, were involved. See Lie (1974), Telhaug (1969), and Lindbekk (1992), for descriptions of the reform.

[3]Similar school reforms were undertaken in most other European countries in the same period, notably Sweden, the United Kingdom and, to some extent, France and Germany. Meghir and Palme (2003) present results using Swedish data, and Blundell et al. (1997) do so using U.K. data.

[4]The Norwegian school system has been slightly changed recently by the so-called "Reform97". Children now start at the age of six and the time spent in compulsory education is ten years, of which seven are at primary school and three are at secondary school. In addition, three years are available to all students either preparing for university or for a trade (vocational).

system up to secondary education; and 3) to increase equality of opportunity along socio-economic and, in particular, geographic lines both by providing resources to establish the new comprehensive schools in all municipalities and by securing a common curriculum for all schools.

The reform period lasted from 1960 to 1972, when a new law for mandatory schooling was passed, stating that all municipalities should have introduced the nine-year mandatory school system by 1973. Hence, for more than a decade, the Norwegian compulsory school system was divided into two. The first cohort that was affected by the reform was the one born in 1947. This cohort started school in 1954, and (i) either finished the pre-reform compulsory school period in 1961, or (ii) went to primary school from 1954 to 1960, then entered the post-reform secondary school system from 1960 to 1962. The last cohort to which, in principle, the old system could apply was born in 1959. This cohort started school in 1965 and finished compulsory school in 1972.

3.1.1 Implementation of the reform

The law of 1959 for mandatory schooling established a central administration agency (under the Ministry of Education), The Royal Council for Experimentation in the Schools ("Forsøksrådet for skoleverket"), to direct the implementation of the reform. It was established to "experiment" by using innovations in schools. The reform was officially regarded as an experiment in the 1960s until the new law for primary schools was enacted in 1969; see Lie (1974). Although the agency was in charge of the reform, it was the municipalities (which is the lowest level of of local administration) that decided whether to implement the reform. The municipalities had to apply to The Royal Council for Experimentation in the Schools to implement the new school system for the whole municipality. In 1960, there were 750 municipalities in Norway.[5] In order to apply, the municipalities had to present a plan for the new school in terms of buildings, teachers, and funding. The law of 1959 made it clear that the extra costs of teachers and buildings were funded by the state.

[5] In the mid 1960s, the number of municipalities was reduced to about 450. Because we use the municipalities existing in 1960 to identify the reform municipalites, this change has no effect for the identification strategy used in this paper.

It is difficult to determine the criteria under which applicants were selected by the committee. However, the committee wanted to include different communities to make the sample representative of the whole country and ensure that the plans for buildings and so on were acceptable; Telhaug (1969), Mediås (2000). Another important factor for adopting the reforms has been identified by Lie (1973, 1974). The school director at the county level, who is an appointed representative of the state, played a central role in co-ordinating the adoption process being undertaken by the state and the municipality. So-called "dynamic" school directors played an important role in getting the reforms adopted.[6] This is also supported by the finding of Lie (1974) that all municipalities within a county tended to implement the reform at the same time.

In the public debate during 1950s and 1960s, it was claimed that the old educational system with more streaming was a better preparation for high school and university than the new system, indicating that richer and urban areas may have been slow to implement the reform. It was also claimed in the public debate that nine years of mandatory schooling was of less importance in many rural communities, where, since fishing and farming were the main industries, seven years of compulsory education was regarded as sufficient. In her contemporary study of the reform, Lie (1973, 1974), tested different hypotheses about the diffusion of the reform. For instance, were the richest municipalities the first to implement the reform? Were cities first? Alternatively, did poorer rural areas implement the reform first because there were obvious economic incentives to do so? Lie's (1973, 1974) finding that neither average earnings, taxable income nor educational levels had an impact on reform adoption is interesting. Her explanation for this finding relates to the subsidies provided by the central government. The main finding in her dissertation is that there appear to be neighbour effects in adoption. When one municipality adopts the reform, adoption spreads to neighboring municipalities. One explanation for this pattern might be the co-ordinating role played by school directors at the county level. Other reforms of communication or co-ordination between neighboring municipalities were also

[6] The important role of school directors has been reported in several conversations with people who were school administrators during this period, and with researchers from other fields.

used, including regular meetings between politicians in the same county. Another explanation could be that neighboring municipalities share the same characteristics such as income levels, education levels, and political preferences. This is true to some extent because education levels and incomes are regionally dispersed. However, regions in the far east, north, west and south of the country all have low and high incomes and education levels, so that neighboring municipalities include both rich and poor. In sum, Lie's research supports a complex adoption process without finding support for single important factor to explain the implementation process. In Section 3.2, we undertake a more formal evaluation of the reform as an instrument in the earnings equations that we estimate.

3.1.2 Identification of pupils' school reform status

Information on the type of school attended is not available at the individual level, except for those who left school with the minimum level of educational attainment (seven or nine years). In order to classify individuals and neighborhoods according to a before/after-reform indicator, we identify the implementation year at the municipality level. This indicator is constructed as follows. From the 1960 census population, we restrict our attention to those individuals for whom we can clearly identify the type of compulsory schooling attained. For each cohort and municipality, we count the number of people in each group. If there were no geographical mobility and the reform implemented in a clear-cut way, there would be no overlap of observations. However, because some people move, and hence attend school in other municipalities, and the reform was implemented gradually in some places, we need to examine the relative numbers of graduates from the old and the new systems.

Since the number of people who left school with only the mandatory level of education is likely to be affected by the reform, we cannot compare the number of people with seven years of school with the number of people with nine years of school as their final attainment. Therefore, we define two "intermediate" measures of a switching cohort, from which the final indicator is constructed. First, we calculate the average rate at which students leave with only seven years in the 1946 to 1948 cohort[7] in a given municipality. When the yearly fraction

[7]For the identification of the reform year, we exploit information on all cohorts exposed to

of people who finish with seven years of school drops below this "benchmark" by 50 percent, it is a possible indication that the reform has been implemented. Similarly, we calculate the average rate at which students leave with only nine years of school in the 1957 to 1959 cohort. When the yearly fraction of students who leave with only nine years of school reaches 50 percent of this rate is another indicative measure. The two switching indicators are then used to define the year of implementation as follows. When the two coincide, this is our estimate of reform implementation. If there is a gap between them of one or two years, so that it seems that those with seven years of school finished before those with nine years of school started, we use the indicator of nine years as our estimate. For the case of a one-year overlap, so that it seems that those with nine years of school started before those with seven years of school finished, we manually checked all the larger municipalities (more than 100 students) against Ness (1971) and local informants. For smaller municipalities with a one-year overlap between the possible indicators, we have randomly assigned one of the candidates as our estimate of the reform year. We have dropped from the sample the municipalities for which our two indicators diverge by more than two years and those for which manual assignment of the larger municipalities did not work. This procedure provides a unique estimate of the year of implementation (transformed into a birth cohort by subtracting 13) for 545 of the 728 municipalities.[8] See Raaum et al. (2003) for the relative importance of these rules as applied to the data. Although there will certainly be some measurement error in our reform date taken as a flow indicator of reforms, we expect that any measurement error in the stock of reformed and nonreformed municipalities, for a given year, would be small. The implementation profile is displayed in Figure I and illustrates the gradual change in compulsory schooling in Norway for pupils and municipalities.

Figure I (Implementation of the reform)

the reform, even though we have restricted our samples to the 1948 to 1957 cohort in the rest of the study.

[8]We also tested different ways of defining reform indicators and restricting the sample, including taking out problematic municipalities, taking out problematic cohorts, and using simpler definitions of indicators. The results are robust to these variations.

3.1.3 High school, college and university reforms

The reform of upper secondary schools took place in the mid 1970s. At the same time, the post upper secondary/college level was reorganized, in which the reform process lasted until the present college structure was launched in the early 1990s. All of these reforms will be used as part of the identification strategy for estimating returns to education using the Roy model. The reform of compulsory schooling is also used as a basis for the instrument in this model in which we estimate returns to types of education, by allowing for interactions between the availability of schools at higher education levels and the compulsory school reform. Variation in the availability of different upper secondary schools and colleges/universities is partly explained by educational reforms that led to an expansion in the number of schools. However, most of the variation is along the cross-sectional dimension. Hence, these instruments are to be interpreted primarily as the distance to education; see Card (1995).

We have constructed a database that assigns different types of educational institutions to the different municipalities.[9] Combined with our information on the municipalities in which the individuals grew up, we are able to construct a variable that indicates whether an individual had access to different educational levels in the same municipality. Table IV illustrates availability for the oldest, the median, and the youngest cohort, represented by the fraction of individuals, with access.

Table IV (Availability of different schools. Individual data)

Table V illustrates availability for the oldest, the median, and the youngest cohorts, represented by the fraction of municipalities.

Table V (Availability of different schools in municipalities. N=435)

Both methods of calculating availability show an increase in the density of all schools over time. Obviously, the largest effect is for the compulsory schools,

[9] For the period 1974 to 1992, Statistics Norway provides data in electronic format. For earlier years, we have collected data from issues of Norwegian Educational Statistics (various years). Years after 1992 were not regarded as relevant, since not even the youngest cohort in question (1957) was considered to be in education.

which form the main part of our identification strategy. As for levels higher than compulsory school, important reforms have taken place in the previous decades, all of which (to some extent) were intended to make schools available in more municipalities, or to more individuals. For example, between 1961 and 1970, there was a six percent increase in the number of municipalities providing upper secondary schools, which meant that this level of education was available (within the municipalities) to 71 percent of the individuals in the 1957 cohort, compared with 65 percent in the 1948 cohort. However, our data also allow us to exploit the variation *between* municipalities. The fractions reported in Tables IV and V reveal that this variation is even greater than the variation over time.

3.2 Evaluation of the Instruments

In this section, we discuss the validity of the instruments on the basis of two standard criteria: (i) their impact on the variable(s) for which they serve as instruments; and (ii) their independence of the error term.

3.2.1 Impact of comprehensive reform

Table VI reports the unconditional proportion of persons in different qualification groups by reform status, and the difference in educational attainment between the two subsamples.

Table VI (Observed distribution of qualification levels by reform status)

Comparing pre- and post-reform samples, there is a distinct decrease (6.4 percentage points) in the proportion of individuals with compulsory education only. For the remaining levels, educational attainment is higher for pre-reform than post-reform individuals, particularly for lower educational levels. For example, the proportion of people with upper secondary school is 4.4 percent for the pre-reform sample, and 5.5 percent for the post-reform period, which is an increase of more than 22 percent. The only exception is the highest level of education, where attainment is lower for the post-reform sample; however, the difference is not significant.

Note, however, that the cohorts in question all experienced a major expansion of the general education system. The numbers in Table VI are unconditional of this trend. Because the reform was implemented sequentially from 1959 to 1973, the fraction of individuals with post-reform compulsory education is higher in younger cohorts. Furthermore, the 1960s was a period of major change in the municipality structure, including changes in the localization of schools. Consequently, cohorts and regions should be controlled for. Two other sources frequently debated in the literature, see for instance Card (1999), are differences in ability and differences in liquidity constraints. Unfortunately, direct information on ability (exam scores , IQ-tests, etc.) is very limited in Norway, and is not included in our data set. On the other hand, our data is rich in other types of background information, such as family income and education, and has information on the individuals' parents.

To evaluate the reform's impact on educational attainment conditional on these arguments, we estimate an ordered probit model for levels of education. The explanatory variables included in the schooling equation are cohort dummies (ten cohorts), dummy variables for geographical location (19 counties), family income in 1970 (quartiles) and the level of education for mothers and fathers (dummy variables for college education).

The parameter estimates from the ordered probit model are presented and discussed in Section 5.1. Here, we report *predicted* pre- and post-reform education levels.

Table VII (Predicted distribution of qualification levels by reform status)

It is clear that the reform, taking account of the positive trend in educational attainment, parents' income, etc., had an impact, particularly at the lower levels of higher education. We find counter-intuitive responses at the highest levels of education, i.e., for the second highest and the highest levels of education (bachelor's and master's degrees). This indicates that the reform produced a (small) fraction of "defiers"; i.e., individuals who would have attained the highest level of education under the old system, but fail to do so under the new system. This result should be interpreted with care, however. Reform of comprehensive schools is likely to be a poor predictor of the highest levels of education, and the reported decrease in attainment might be coincidental rather than systematic.

Reform of compulsory schooling is used as the single instrument only in the traditional earning-schooling equation, in which the schooling variable is treated as a continuous variable. This model is discussed in Section 4.1. To take into account possible nonlinearities as well as observed and unobserved heterogeneity, we estimate a Roy model in which we use education as a discrete variable instead of education as a continuous variable. The Roy model is discussed in Section 4.2. To identify the returns to education in this model, we need an instrument for each level of education. We use variation in availability of different post-compulsory schools as part of our identification strategy. We use the availability of these schools *interacted* with the reform of compulsory schooling dummy as our instrument; see for instance Duflo (2001). We illustrate the idea behind the use of this difference-in-differences strategy with a two-by-two table for each type of school. The table shows the average number of years of education[10] for individuals in municipalities that introduced the mandatory school reform and those that did not, and in municipalities in which all other types of schools were available and in those in which they were not.

Table VIII.1-VIII.4 (Means of education by school availability)

The tables show that the average number of years of educational attainment is higher in municipalities that introduced the reform that increased mandatory education to nine years, but it also shows that the average length of education increased more in municipalities with schools providing higher education. The difference in these differences can be interpreted as the causal effect of the educational reforms, under the assumption that the mean years of education would have been the same in areas with and without the higher educational institutions. On average, the effect of the reform in terms of the increase in the duration of education is 0.21 years in municipalities without vocational schools, and 0.20 years for municipalities that did offer this form of education. The difference, 0.01 years,

[10] We use years of education in these tables for convenience only. In our model, we use the level of education, rather than years of education, but to present Table VIII using levels of education would produce 28 different tables, which we do not report here.

is approximately the same for municipalities with and without upper secondary schools, whereas it is distinctly higher, almost 0.17 years, for the highest level of education (University II). The only exception to this pattern is the second highest level (University I, i.e., regional colleges), where the effect of the reform is higher for municipalities not providing this form of education.[11]

3.2.2 Testing the instrument

We now turn to a formal treatment of the question raised in Section 3.1.1, namely, whether it is possible to treat the reform as a natural experiment. If the implementation of the reform was not random across municipalities, a principal methodological problem occurs if we want to use the reform as an instrument. In other words, it rests on the assumption that the comprehensive school reform was introduced randomly and not systematically by, for instance, being introduced first in rich municipalities. If it relied on local financing, more affluent municipalities could have afforded to select themselves into the reform at an early stage. Children in these municipalities would have been more likely to become better educated and earn more as adults. Hence, we need to explore whether the reform implementation is correlated with municipality characteristics that might have (indirect) effects on earnings.

In Table IX we regress the year of implementation against different background variables based on municipality averages, such as parental income and the level of education, age, size of the municipality, etc.

Table IX (Implementation of reform. Regression. Cohort 1948-1957)

It seems reasonable to conclude that there is no systematic pattern in the introduction rate in relation to parental average earnings, education levels and age, or in relation to urban/rural location. When controlling for localization (using dummy variables indicating the respective counties), we find none of the

[11] This finding does not contradict the results in Tables VI and VII, which show that the reform had no effect on the highest level of education, measured using the fraction of individuals that completed degrees. What we report in Table VIII.4, is that the reform had an effect in the average duration of education for all inhabitants in those municipalities that offered the highest forms of education.

other variables are statistically significant. Hence, the reform assignment appears to be determined exogenously, at least with respect to factors that are testable with our data.

Although an even development of the reform by city/rural, rich/poor areas was wanted by the reform committee, and we do not find any pattern, we cannot completely rule out the allocation being systematic in relation to relevant factors. For instance, systematic action on behalf of parents (migration to municipalities with the preferred education system, etc.) cannot be totally ruled out. We have, however, reason to believe that this is a minor problem and, hence, we ignore it in this study; see the discussion in Telhaug (1969).

4 The Model

We estimate two models for the returns to education; one in which schooling is treated as a continuous variable, and one in which we estimate the returns to different levels or types of education. There are several reasons for estimating returns to years of education. First, most studies of the returns to education define education in terms of the number of years spent in school. Thus, to compare our results with previous studies, we define schooling as a continuous variable. Second, the interpretation of the return to schooling is easier if schooling is continuous since the number of estimated parameters is reduced.

However, there are strong arguments for treating schooling as discrete. First, evidence on the returns to education indicates nonlinearities; see Layne-Farrar et al. (1996) and Bound and Jaeger (1996). A model in which schooling is treated as a continuous variable implies that returns to schooling are assumed to be identical for each level of education, which is clearly more restrictive than a specification allowing for nonlinearities. Second, treating schooling as discrete allows us to specify a comparative advantage model. In such a model, the effects of both observed and unobserved factors are different for each educational level, thus allowing for heterogeneity in the returns to education.

In the generalized Roy model (comparative advantage model) we have L potential outcomes associated with each level of education: $y_{1i}, y_{2i}, ..., y_{Li}$, where y_{li} is the outcome for person i if this person takes educational level l. The returns to education in the Roy model are the outcome (log earnings) given the educational level l minus the outcome this person would have had with compulsory education: $(y_{li} - y_{1i})$. This quantity is never observed directly. However, given a flexible model that accounts for both observed (X_i) and unobserved (U_{li}) selection into different educational levels, we can predict the outcome for a person under different educational levels. We then average the individual returns over a given population, such as the total population (for the average treatment effect) or the subpopulation of persons that obtained educational level l (for the treatment effect on the treated).

We estimate two models for returns to education: one linear and continuous in education, and one allowing for nonlinearities and heterogeneity in the returns to education. The two models are analyzed within quite different framework and the second model is specified as an extended Roy model. For both models, we consider a model of log annual earnings (y) in 1995 and analyze only full time employed male workers. Thus, we do not look at females and we do not consider the selection of persons into full time work, part time work or no work.

4.1 Education as a Continuous Variable

4.1.1 Educational attainment

We allow for heterogeneity in the individual returns to schooling and in the individual costs of schooling. The optimal level of schooling in the heterogeneity model is, see for instance Card (1995):

$$S_i^* = \frac{b_i - r_i}{k}, \qquad (1)$$

where b_i is individual ability that generates heterogeneity in the marginal returns to schooling, r_i represents individual differences in opportunities that generate heterogeneity in the marginal cost of schooling, and k is a nonnegative constant. The school reforms may affect both b_i and r_i.

We use an ordered probit model to estimate the effect of control variables and school reforms on completed school outcomes. The ordered probit model is built around a latent regression equation,

$$S_i^* = Q_i\beta_S + \theta_S Z_i + V_i, \qquad (2)$$

where S_i^* represents the optimal level or type of schooling, Q_i is a vector of individual and family background variables, β_S is the effect of observed background variables on educational attainment, Z_i is the instrument used in this specification (school reform), θ_S is the effect of the instrument on educational attainment, and V_i is the error term distributed normally with $E(V) = 0$ and $Var(V) = 1$. We do not observe the latent variable S_i^*. However, the observed optimal choice of education can be modeled in the following way:

$$S_i = l \text{ if } c_{l-1} < S_i^* < c_l,$$

where $l = 1, 2, \ldots, L$ are educational levels and c_l are cut-off levels in the ordered probit model. We have divided qualification levels into seven discrete categories as defined in Table I, i.e. $L = 7$.[12] We define $c_0 = -\infty$ and $c_L = +\infty$; i.e., the two extreme categories 1 and L are open-ended intervals. From the ordered probit model, we can predict the probability of a person attaining the different qualification levels. Thus, we predict $\Pr(S_i = l|Q_i, Z_i)$ for all persons in the sample, and from different versions of this equation. In the ordered probit model, $\Pr(S_i = l|Q_i, Z_i)$ is calculated as

$$\Pr(S_i = l|Q_i, Z_i) = \Phi(c_l - Q_i\beta_S - Z_i\theta_S) - \Phi(c_{l-1} - Q_i\beta_S - Z_i\theta_S), \quad (3)$$

where Φ is the cumulative distribution function of the normal distribution.

From the ordered probit model, we calculate the "generalized residual" for each level of education l. We call the generalized residual ξ and this new variable is used to model unobservables in the earnings equations. The ξs for each educational level are calculated as follows:

$$\xi_{li} = \frac{\phi(c_{l-1} - Q_i\beta_S - Z_i\theta_S) - \phi(c_l - Q_i\beta_S - Z_i\theta_S)}{\Phi(c_l - Q_i\beta_S - Z_i\theta_S) - \Phi(c_{l-1} - Q_i\beta_S - Z_i\theta_S)}, \quad (4)$$

where ϕ is the probability density function of the normal distribution.

[12] We also tested a model with 14 different educational groups based on the number of years of schooling. This variable varies from seven years of schooling to 20 years of schooling. A linear regression model of the number of years of education gives the same results as an ordered probit model if the cut-off levels are the same distance apart. Here, we use an ordered probit model even in the case in which S is treated as continuous in the earnings equations.

As explanatory variables, Q, in the ordered probit model, with which we predict educational outcomes, we use age-cohort dummies, dummy variables for childhood geographical location (19 counties), father's and mother's income in 1970 (quartiles), and father's and mother's education (whether they have college degrees or not). Z is the instrument we use (reform of the compulsory school system). We estimate two versions of the ordered probit model: one in which we use only the reform of compulsory schooling, and the other in which we interact all the background variables with the instrument ($Q \cdot Z$), to allow the instrument to affect the level of schooling differently in terms of family and individual background variables. In Section 4.2, in which we estimate a comparative advantage model, Z is a vector of instruments (different school reforms).

4.1.2 Earnings specification

We consider a model of log earnings of the following form:

$$y_i = \alpha_i + X_i\beta + bS_i \tag{5}$$

where α_i is a person-specific constant, b is the effect of schooling, S is the continuous schooling variable, and X is a vector that includes the following variables in addition to the Q vector in the first step: tenure, tenure squared, actual work experience, actual work experience squared, and location when adult (19 counties).

Equation (5) does not take into account the possibility of heterogeneity in the returns to education.[13] However, the model allows individual heterogeneity to affect the intercept of the earnings equation through α_i. A model that incorporates heterogeneity in the returns to education can be specified by splitting b into two parts, $b = \bar{b} + b_i$, where $\bar{b}$ is the common (average) return to education and b_i is the random return parameter for number of years of schooling, which we allow to vary from person to person. We split α_i into two parts, where $\alpha_i = a_0 + a_i$. We rewrite equation (5) as follows:

$$y_i = a_0 + X_i\beta + (\bar{b} + b_i)S_i + a_i, \tag{6}$$

[13] However, it is possible to estimate heterogeneity in returns to education on observables with this model by allowing for interactions between education and observable characteristics.

where b_i represents heterogeneity in the returns to education, and a_i is heterogeneity that affects the level of earnings. Both b_i and a_i are unobserved variables.

There are two potential sources of bias in equation (6). The first is the standard ability bias problem, which is due to the correlation between a_i and S_i. The second is due to heterogeneity in the returns to education, which is due to the correlation between b_i and S_i. We use a control function approach to model the effect of unobserved factors. We assume

$$E[a_i|S_i, Q_i, Z_i] = \lambda\xi_i$$

and

$$E[b_i|S_i, Q_i, Z_i] = \psi\xi_i,$$

where $\lambda = Cov(a_i, S_i)/Var(S_i)$ and $\psi = Cov(b_i, S_i)/Var(S_i)$. The variable ξ_i is in this case pooled over each educational level since we are analyzing the case in which S is continuous. Hence, we have the following earnings equation :

$$E[y_i|X_i, S_i, \xi_i] = a_0 + X_i\beta + \bar{b}S_i + \psi\hat{\xi}_i S_i + \lambda\hat{\xi}_i; \quad (7)$$

see Card (1999, 2001), and Wooldridge (2003). Applying OLS to equation (7) yields a consistent estimate of the average effect of schooling on earnings. We also estimate an instrumental variables (IV) version of this model, which does not allow for heterogeneity in the returns to education:

$$E[y_i|X_i, S_i, \xi_i] = a_0 + X_i\beta + \bar{b}S_i + \lambda\hat{\xi}_i. \quad (8)$$

The model in equation (8) is used as a benchmark against which other models are compared.

4.2 A Model of Comparative Advantage

We now turn to estimation when education is measured as a discrete variable. In the first stage of the model, that of estimating the school choice model, we use a sequential probit model. From this model, we estimate an endogeneity correction term, which is used in the estimation of the Roy model. This correction term is allowed to work differently for each educational level.

4.2.1 Educational attainment

The sequential probit model can be used when the dependent variable (the level of schooling) can be separated into a sequence of binary choices; see for instance Heckman and Cameron (1998). This is an alternative to the ordered probit model used in Section 4.1, and allows for the flexible estimation of educational attainment. We use the simplest version of the model in which the error term is independent across sequences, mainly because of identification issues; see for instance Taber (2000).

In the model of educational choice, we have L levels of education. School choice is represented by a set of discrete variables: $D_{1i}, D_{2i}, \cdots, D_{Li}$, where D_{li} is the discrete outcome for person i if this person has finished educational level $l-1$. $D_{li} = 1$ if a person completes grade l and $D_{li} = 0$ otherwise. In the first choice (education beyond compulsory education, i.e., educational level 2), we let D_{2i} represent a discrete choice. In the second-choice stage, we let D_{3i} represent a discrete binary choice only when $D_{2i} = 1$, etc.

For the first stage, we write $D^*_{1i} = Q_i\beta_{S_1} + \theta_{S_1}Z_i + V_{1i}$, where we observe $D_{1i} = 1[D^*_{1i} > 0]$; hence, $\Pr(D_{1i} = 1|Q_i, Z_i) = \Phi(Q_i\beta_{S_1} + \theta_{S_1}Z_i)$. Estimation is by standard maximum likelihood on the full sample. For the second stage, we use Bayes' formula to formulate

$$\Pr(D_{1i} = 1, D_{2i} = 1|Q_i, Z_i) = \Pr(D_{1i} = 1|Q_i, Z_i) \cdot \Pr(D_{2i} = 1|D_{1i} = 1, Q_i, Z_i).$$

We write $D^*_{2i} = Q_i\beta_{S_2} + \theta_{S_2}Z_i + V_{2i}$ where we observe $D_{2i} = 1[D^*_{2i} > 0]$; hence, $\Pr(D_{2i} = 1|Q_i, Z_i) = \Phi(Q_i\beta_{S_2} + \theta_{S_2}Z_i)$. Estimation is by standard maximum likelihood on the selected sample. We continue this strategy up to the last educational level (D_{Li}). Since we have seven different educational levels, to estimate the model, we assume independence between the error terms in the different specifications.

The vector of instruments, Z, includes the availability of different types of schools at the municipality level. In particular, we use interaction terms between the reform of compulsory schooling and changes in the availability of other types of school as identifying exclusion restrictions. Our specification requires that we have a valid instrument at each level of education. Variables and instruments used in the regressions are discussed in Sections 2 and 3.

4.2.2 The Roy model

The most flexible method of allowing both for nonlinearities in the returns to education and for heterogeneity in returns, is to specify a Roy model for each level of education using an identifying instrument for each education level. We use a switching regression framework, in which earnings regressions are estimated for each educational level,; see for instance Heckman, Tobias, and Vytlacil (2000). The advantage of using such a framework is that the returns to education are allowed to vary both in terms of observed and unobserved individual factors. This is a demanding framework in the sense that it requires many observations to avoid the problem of missing cells.

The model has the regression specification

$$E[y_{li}|X_i, \xi_{li}, D_{li} = 1] = a_0 + X_i\beta_l + \psi_l\hat{\xi_{li}} \tag{9}$$

for each educational level l. The results from these different regressions can be used to predict the outcome for educational levels other than the one that is observed for individual i. The return to education is then the outcome of schooling level l relative to another schooling level, such as schooling level 1, which is compulsory schooling. Thus, the effect of schooling level l is the difference between the predicted outcome for person i given schooling level l and the predicted outcome for the same person had he or she had only compulsory schooling. The return to schooling is then an average of these differences. For instance, if we average over the whole population, we get the average treatment effect (ATE). If we average over the subsample of persons with the exact schooling level l, we get the effect of treatment on the treated (TT). The TT parameter given specific values of X and S is given by

$$\Delta^{TT(S=l,x)} = x_i(\beta_l - \beta_1) + (\psi_l - \psi_1)\hat{\xi_{li}}. \tag{10}$$

The parameters β and ψ come from the estimation of equation (9) for each educational level, and ξ is calculated from a sequence of probit regression models. Unconditioned estimates of equation (10), i.e. $\Delta^{TT(S=l)}$, can be found by integrating $\Delta^{TT(S=l,x)}$ over the distribution of X.

Both the TT and the ATE are relevant policy parameters in our application in which we are analyzing reforms such as comprehensive school reform. The ATE parameter gives the effect of selecting at random a person from the population into a different educational level than the one in which that person is observed. We can thus predict the effect of school reforms that are intended to increase the level of education in the population as a whole. The effect of the TT parameter is a relevant policy parameter when calculating the returns to schooling for a group of individuals with a particular level of education.

Return parameters in the Roy model, such as the one given in equation (10), have two heterogeneity components. The first is related to observable variables. In equation (10), the effects of observed individual and family background variables (x_i) are allowed to be different for different schooling levels. Thus, the effect of, for instance, family income during childhood is allowed to be different for different educational levels. Second, the effect of unobservables (ξ_i) is also allowed to be different for different schooling levels. Thus, the effect of, for instance, unobserved ability can vary between educational levels. We now have a comparative advantage model in terms of both observed and unobserved factors, in which individuals are allowed to act on their differences. This comparative advantage model (or Roy model) is challenging since we need to estimate earnings equations for each educational level. The estimation of treatment effects can be sensitive to imprecise parameter estimates in the earnings equation.

5 Results

5.1 Estimating Educational Attainment

As noted in Section 3.2.1, we wish to establish the effect of reform of compulsory schooling on educational attainment. Table X reports the ordered probit coefficients for the school choice (level of education), in which, as well as adding the dummy for the old/new regime, we have controlled for a broad range of demographic and socio-economic covariates, the most interesting being parental education and income. Model 1 in Table X is the basic model (which includes cohort and county dummies whose coefficients are not reported to save space). In Model 2 in Table X, the reform dummy is interacted with all the variables of

Model 1.[14]

Table X (Educational choice. Ordered probit model)

In addition to the estimated coefficients, we calculate the effects of changes in the covariates on the probabilities of the respective education levels, reported in Table XI.

Table XI (Effects on cell probability choice of changes in covariates)

First, we note from Table X that pupils from the reformed compulsory schools generally have a significantly higher level of education than those from the nonreformed. The magnitude of the effect is illustrated in Table XI. Post-reform pupils are about 4.5 percent less likely to choose the lowest level of education compared with the pre-pupil group, and about 3 percent more likely to choose the highest level.

Parental education appears to be a strong predictor of the educational choice of the children. If a child's father has a college education, the probability of the child leaving school with only compulsory education (Level 1) is reduced by almost 20 percent, and the probability of attaining a master's degree (Level 7) increases by more than 12 percent. Note, however, that this dependence on the father's education is lower for the post-reform group than the pre-reform group (by 3 percent at the bottom and 2 percent at the top). The effect of the mother's education shows the same pattern, with the magnitude being slightly smaller, as is the change from the old to the new system.

Parental income (the sum of the father's and mother's income) is included in the form of quartiles, the lowest quartile being used as a reference. As expected, higher parental income appears to raise the level of the child's education. The effect increases from being quite modest for the second quartile to being quite large for the fourth. The changes from the old to the new system is less apparent,

[14] We report the results from the ordered probit model using only one instrument (reform of compulsory schooling) since we use this specification when estimating the model when schooling is treated as a continuous variable; see section 4.1. For the model described in Section 4.2, we use several school reforms and interactions between them in our identification strategy; however, the results from these regressions are not reported here.

but seem to suggest a reduced dependence on parental income for the upper quartile; see Table XI. The results of Table XI are depicted in Figures II to VII.

Figures II to VII (Marginal effects in the ordered probit model)

The steepness indicates the respective variable's influence on educational choice, and the difference between the solid and dotted lines indicates the effect of the reform.

5.2 Results from Estimation of the Wage Equation

In this section, we present the results from the estimated wage equations for the different models. We start with the model in which education is treated as a continuous variable and present results from OLS, IV (selection model) and random coefficient models. Then we present the results for the model in which education is treated as a discrete variable representing the type of education. In this model, we allow for differences in returns for different educational levels. The model also allows for heterogeneity in terms of observed and unobserved variables.

5.2.1 Constant returns to years of education

In Table XII, we report the results of the earnings equation in which we assume constant returns to education by specifying the education variable as a continuous variable.

Table XII (Earnings equations, full-time employed men, cohorts 1948-1957)

In column 1, we tabulate the OLS returns to education for male workers for all sectors. Note that we are estimating the wage equation on the birth cohorts 1948-1957, which means that they are 38-47 years of age in 1995. Since prime age males are used, returns to education are higher than if we were to use a wider age range. The returns to education using OLS are 7.5 percent.[15]

[15] In the model in which education is treated as a continuous variable, we have two versions of the model. First, we use the actual number of years of education, which ranges from seven to 20

In column 2, results controlling for the endogeneity of education are presented in which reform of compulsory schooling is used as the identifying instrument. The model used in column 2 is a standard selection model; see equation (8) in Section 4.1.2. The results show that returns to education increase to 10.2 percent. This is a standard result from, for instance the literature using a measure of distance to higher education as an instrument (see Card (1995, 1999)) and for Norway (see Hægeland et al. (1999)). It indicates heterogeneity in returns to education in that the instrument we use picks up the returns to education for the group that complies with the treatment (reform of compulsory schooling). In this case, it is reasonable to suppose that the compliers, who were being pushed to higher educational attainment when the new compulsory school system was introduced, have higher returns to schooling compared with those groups that were unaffected by the reform (for instance, the "always takers" and "never takers"). The reform may thus have affected both the return and cost parameters in equation (1), with some feedback to the level of education. The LATE interpretation of returns to education, which is the interpretation of IV estimates in the case of discrete instrumental variables (see Angrist et al. (1996)) is that our estimated parameter of returns to education is the returns to education for a person acquiring an extra year of education just because of the educational reform and who would have dropped out of education after seven years otherwise. This result contrasts with that from a similar specification in Meghir and Palme (2003), who find no significant effect on returns to education using participation in the reform of compulsory schooling as the instrument.

years. Second, we use the level of education, but treat this variables as a continuous variable. This variable has seven levels (values). We do this to compare our results with the results in which education is a discrete variable in Section 5.2.2. In Table XII, we report the results for the model in which we use seven levels of education, rather than the model with the actual number of years of education. (The results from the regressions that use the actual number of years of education are available upon request.) The results from this model are comparable to the one reported here, although the increase in the IV return parameter compared with the OLS estimates are smaller than the one reported in Table XII. The OLS estimate of the returns to education for the model not reported here is 5 percent, which is in accordance with results from other studies in Norway; see Hægeland et al. (1999), Hægeland (2001), Raaum and Aabø (2000).

The standard interpretation of the IV results indicates that it is pupils with poor family backgrounds or pupils with long travel distances to the nearest schools that are identified when using the school reform as an instrument. This was one of the groups particularly targeted by the government when it introduced the reform. The interpretation that credit constraints matter has been challenged in recent papers; see Carneiro and Heckman (2002) and Carneiro et al. (2001). In these papers, Heckman and his co-authors find support for the notion that, for the United States, income constraints on families cannot explain the results. They emphasize long-run factors such as parental guidance and the genes that form the cognitive and noncognitive abilities of children and which are correlated with long-run parental income.

For a third specification, we estimate the random coefficient model in which the interpretation is an average effect since both endogeneity and heterogeneity are purged from the wage equation. The estimation follows from equation (7) in Section 4.1.2. The return to education is now 6.3 percent on average per unit of education, which is slightly lower than in the OLS specification as well as the selection result reported in column 2 of Table XII. If we interpret this result literally, it means that the returns to education in the population are lower when heterogeneity and endogeneity is controlled for. In our case, this implies that there must have been a slight positive selection into higher education caused by the reform because the average return to education is lower when this is controlled for. This result is in line with the LATE result obtained from the IV estimates presented in column 2, in which the results indicate positive selection into higher education. We also note that the interaction term between the selection parameter, ξ, and years of education is negative in this model, pointing in the same direction.

5.2.2 Returns to education levels

In Table XIII, we first present the regression results from the estimated Roy model for returns to education by defining education in terms of qualification levels from one year of vocational training to master's degree level; see equation (9) in Section 4.2.2. The reference level is compulsory education; see also Table I for definitions of education levels applied in the analysis.

Table XIII (Earnings equations, full time employed men. Roy model)

The table shows that earnings increase with work experience, but at a decreasing rate, at all educational levels. The effect of tenure is the same as that of work experience, but the effect is smaller in magnitude. Note that the effect of one more year of work experience on earnings is much higher for the two highest levels of education (university degrees) than is the effect on lower levels of education. The effect of family background characteristics on earnings is much smaller than the effect of these variables on educational attainment. The effect of having a mother with a college education is about the same as the effect of one more year of work experience. Note also that the effect on earnings of having a mother with a college education is larger than the effect of having a father with a college education. Only 10 percent of the sample had a father with college education, and only 3 percent had a mother with college education; see Table III. The effect of family income on earnings is clearly positive as we go from the first income quartile to the second. This applies for all educational levels. However, the effect of moving to a higher family income quartile is less clear, and is negative for the highest educational levels. The effect of the selection correction term of moving from one educational level to the next is negative for all educational levels. However, the effect is not significantly different from zero for the four highest levels of education in this specification.

We now turn to the estimation of returns to different levels of education; see Table XIV. The estimation of different return parameters is based on equation (10) in Section 4.2.2. The coefficient estimates from Table XIII are used to form the β_l and ψ_l coefficients in equation (10).

Table XIV (Returns to education. Effects are measured in percent)

We report the unconditioned mean log earnings and differences in returns to education in the first two columns. The unconditioned results show that upper secondary school generates a 24 percent return for the three years of education. Two to three years of vocational training gives a much lower return of about 8.6 percent, and one year of vocational training yields a return of about one percent. Note that undergraduate regional college or short university degrees do not give a much higher return compared with that given by upper secondary school on the basis of the unconditional estimates; in fact, it gives a loss in one case. This nonlinearity in returns is a striking result and has been noted before; see Hægeland, Klette and Salvanes (1999).[16] The return to a master's degree is 45 percent relative to that of compulsory school. All these results compare well with the results of others; for instance, for Sweden see Edin and Holmlund (1997) and Meghir and Palme (2003).

The next two columns provide the ATE results and the TT effect allowing for selection on observable and unobservable background characteristics within the comparative advantage model; see Section 4.2. The results are similar to the unconditioned results, but there are slight differences. First, notice that the returns to upper secondary school are lower when measured as an ATE than are the unconditional averages in the population. The Roy model predicts an average effect of a 20 percentage point gain in earnings each year from having an upper secondary school diploma. Second, adjusting for observed and unobserved selection, we find that the average effect of higher education is higher than that implied by the unconditional estimates. Nonlinearities in the average returns to education remain, but are less apparent than in the unconditional case.

Moving to the results for the TT effect, the final column in Table XIV shows that these effects provide higher returns at all levels than the ATE and unconditional effects, except at the vocational level. This means that selection into higher education is important. The TT effect results indicate strongly that returns to education are high for higher education. Considering the ATE results, which we can interpret as the result of an experiment of putting the whole population into a particular level of education, the return to education is lower than for those students at that education level. This result implies that selection on comparative advantage is also important because the returns due to selection into different education levels are important. This finding supports earlier findings in

[16] Note that these categories of 1-2 years and especially 3-4 years of college/university degrees comprise many different types of education, as one year of university without a degree via two years of a college degree to a four-year university degree both in universities (cand.mag. in the Norwegian system), or a degree from a technical university and a degrees from a business school, which are difficult to enter on the basis of high school marks. Hægeland (2000) has analyzed this in detail and the difference in returns is noticeable.

the literature that test for only two levels of education; see Willis and Rosen (1979) and Garen (1984).

From the final column of Table XIV, we find that the effect of a bachelor's degree (level 6) is considerably higher for those actually taking a bachelor's degree than the effect of randomly selecting a person into that particular level of education. In addition, the effect of randomly selecting a person into upper secondary school or a short college programs (one and two year programs), ATE, gives a surprisingly high effect. Thus, increasing the general level of education, the intention of the comprehensive school reform in the 1960s and also of other school reforms, has the potential of giving a high return in wages up to the level of short college programs, although we do not consider the cost issue for society. We also find that there is a substantial difference between the ATE and the TT for bachelor's and master's degrees at universities. Thus, selection effects are important for university degrees.

6 Concluding Remarks

In this paper, we have used a rich data set and a flexible framework for estimating the returns to education. We have studied different return parameters of education, both in a linear and nonlinear framework, in which we allow the effect of education to vary both in terms of observed and unobserved factors. The model allows agents to act on the effect of schooling and, hence, we have assumed forward looking agents who can predict the effects of different schooling levels on wage outcomes. Supply side "shocks" are useful instruments in demand type models and we explored the use of educational reforms as instruments in identifying and interpreting different return parameters of education.

We found that the returns to education are strongly nonlinear and that, because of nonlinearities, the Roy model is better than the traditional IV model in which schooling is a continuous variable. In particular, we found that the returns to upper secondary school and one and two years of education at regional colleges, together with master's programs at universities, have a high return as measured by wages. Also, we find that the average treatment effect, the effect of randomly selecting a person, is surprisingly high for medium-length education (up to two years of college education). This means that increasing the general level of education, the intention of the comprehensive school reform in the 1960s and other school reforms, has the potential of giving a high return in wages, although we do not consider the issue of the cost to society.

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Table I. Definition of education levels applied in the analysis. Birth cohorts 1948-57.

Level	Description
1	Pre/post reform compuls. and jun. sec. school (7-10/9 years)
2	Upper secondary school 1 year; mainly vocational
3	Upper secondary school 2-3 years; mainly vocational
4	Upper secondary school 2-3 years; high school
5	University I, post upper secondary school, 1-2 years
6	University II, post upper secondary school, 3-4 years
7	University III, master level, university degree, 5+ years

Table II. Data selection process.

Male born 1948 to 1957, total		295,646
Missing on parents municipality	65,102	
Missing on family and individual variables	17,565	
Not full time employed or missing tenure	53,527	
Net sample		159,452

Table III. Descriptive statistics for the net sample of 159,452 individuals.

Variables	Mean	Standard deviation
Education in years	12.146	2.7781
Age in 1995	42.729	2.8432
Tenure	7.4685	5.9399
Wage 1995 in NOK	299525	134368
Logwage	12.539	0.3542
Experience in years	21.155	4.0259
Father has college	0.1053	0.3070
Mother has college	0.0326	0.1777
Vocational available in municip.	0.8060	0.3953
Upper secondary available in munic	0.6842	0.4648
University I available in municip.	0.4231	0.4940
University II available in municip.	0.2067	0.4049

Table IV. Availability of different schools. Individual data.

	Cohort=1948	Cohort=1952	Cohort=1957
Number of persons	13541	16101	17474
Compulsory 9 year	0.0598	0.3810	0.9627
Vocational	0.7989	0.7988	0.8153
Upper secondary	0.6538	0.6731	0.7130
University I	0.4027	0.4113	0.4498
University II	0.2141	0.2002	0.2167

Table V. Availability of different schools in municipalities. N=435.

	1961	1965	1970
Compulsory 9 year	0.0210	0.3489	0.9563
Vocational	0.4777	0.4875	0.4889
Upper secondary	0.2472	0.2853	0.3038
University I	0.0833	0.0941	0.1049
University II	0.0111	0.0138	0.0138

Table VI. Observed distribution of qualification levels by reform status. Birth cohorts 1948-57.

Levels		Pre-reform	Post-reform	Change	Change in %
1	Pre/post compulsory	0.2057	0.1418	-0.0638	-0.3105
2	Vocational I	0.1674	0.1831	0.0156	0.0934
3	Vocational II	0.2451	0.2821	0.0370	0.1512
4	Upper secondary	0.0446	0.0548	0.0102	0.2286
5	University I	0.1368	0.1433	0.0064	0.0474
6	University II	0.0972	0.0980	0.0007	0.0080
7	University III	0.1029	0.0966	-0.0063	-0.0613

Table VII. Predicted distribution of qualification levels by reform status. Birth cohorts 1948-57.

Levels		Pre-reform	Post-reform	Change	Change in %
1	Pre/post compulsory	0.1977	0.1434	-0.0542	-0.2744
2	Vocational I	0.1634	0.1839	0.0204	0.1251
3	Vocational II	0.2459	0.2852	0.0393	0.1599
4	Upper secondary	0.0454	0.0554	0.0100	0.2200
5	University I	0.1387	0.1426	0.0038	0.0279
6	University II	0.0991	0.0958	-0.0033	-0.0332
7	University III	0.1095	0.0934	-0.0160	-0.1469

Table VIII.1. Means of education by school availability

		Vocational		
		0	1	
Reform	0	11,608	12,153	0,545
	1	11,821	12,352	0,531
		0,213	0,199	**0,014**

Table VIII.2. Means of education by school availability

		Upper secondary		
		0	1	
Reform	0	11,691	12,212	0,521
	1	11,879	12,419	0,54
		0,188	0,207	**-0,019**

Table VIII.3. Means of education by school availability

		University I		
		0	1	
Reform	0	11,765	12,468	0,703
	1	11,985	12,579	0,594
		0,22	0,111	**0,109**

Table VIII.4. Means of education by school availability

		University II		
		0	1	
Reform	0	11,912	12,61	0,698
	1	12,134	12,664	0,53
		0,222	0,054	**0,168**

Table IX. Implementation of reform. Regression. Cohort 1948-1957.

	Coefficient	Standard error
County2	-2.0084	0.6361
County3	4.6204	5.1743
County4	-0.5769	0.6302
County5	-0.8327	0.6264
County6	-0.7722	0.6161
County7	-1.2283	0.6241
County8	-1.8233	0.6367
County9	-1.2159	0.6284
County10	-2.2932	0.6748
County11	-0.8081	0.5696
County12	-1.6951	0.5417
County13	-0.5687	0.6689
County14	1.0369	0.5459
County15	-1.5363	0.5518
County16	0.0186	0.5608
County17	-1.2770	0.5311
County18	-0.2418	0.5973
County19	-2.7154	0.6836
Father college	2.4052	3.6445
Mother college	10.732	8.2210
Father income	-0.0059	0.0039
Mother income	-0.0080	0.0079
Father age	0.0499	0.1510
Mother age	-0.0936	0.1872
Size municipality/10(	-0.0277	0.0305
Constant term	1970.9	6.6907

Table X. Educational choice. Ordered probit model

	Model 1		Model 2	
	Coefficient	St.error	Coefficient	St.error
Father college	0.7349	0.0098	0.7994	0.0144
Mother college	0.5250	0.0162	0.5627	0.0245
Family income 2	0.0996	0.0074	0.0854	0.0101
Family income 3	0.2604	0.0075	0.2520	0.0104
Family income 4	0.5245	0.0083	0.5439	0.0116
Reformed	0.0401	0.0071	0.1879	0.0447
Ref x Father coll			-0.1171	0.0196
Ref x Mother coll			-0.0594	0.0327
Ref x Fam inc 2			0.0263	0.0149
Ref x Fam inc 3			0.0141	0.0152
Ref x Fam inc 4			-0.0394	0.0167
Cut off level 1	-0.5747	0.0146	-0.5501	0.0176
Cut off level 2	0.0021	0.0145	0.0271	0.0175
Cut off level 3	0.7256	0.0146	0.7508	0.0176
Cut off level 4	0.8705	0.0146	0.8957	0.0176
Cut off level 5	1.3417	0.0148	1.3671	0.0177
Cut off level 6	1.8176	0.0150	1.8433	0.0179
Pseudo R-squared	0.0036		0.0037	
Observations	159,452		159,452	
Cohorts dummies	Yes		Yes	
County dummies	Yes		Yes	

Note: Standard error in parenthesis. Family income is divided into four equally sized groups. All the estimated coefficient are significantly different from zero.

Table XI. Effects on cell probabilities for educational choice of changes in covariates

Variable / Level	Pre-reform	Post-reform
Reformed / 1		-.046
Reformed / 2		-.019
Reformed / 3		-.000
Reformed / 4		.003
Reformed / 5		.016
Reformed / 6		.017
Reformed / 7		.029
Father college / 1	-.196	-.167
Father college / 2	-.083	-.071
Father college / 3	-.004	-.003
Father college / 4	.014	.012
Father college / 5	.068	.058
Father college / 6	.076	.065
Father college / 7	.124	.106
Mother college / 1	-.138	-.123
Mother college / 2	-.058	-.052
Mother college / 3	-.002	-.002
Mother college / 4	.010	.009
Mother college / 5	.048	.043
Mother college / 6	.053	.047
Mother college / 7	.087	.078
Fam. income 2 / 1	-.020	-.027
Fam. income 2 / 2	-.008	-.011
Fam. income 2 / 3	-.000	-.000
Fam. income 2 / 4	.001	.002
Fam. income 2 / 5	.007	.009
Fam. income 2 / 6	.008	.010
Fam. income 2 / 7	.013	.017
Fam. income 3 / 1	-.061	-.065
Fam. income 3 / 2	-.026	-.027
Fam. income 3 / 3	-.001	-.001
Fam. income 3 / 4	.004	.004
Fam. income 3 / 5	.021	.022
Fam. income 3 / 6	.024	.025
Fam. income 3 / 7	.039	.041
Fam. income 4 / 1	-.133	-.123
Fam. income 4 / 2	-.056	-.052
Fam. income 4 / 3	-.002	-.002
Fam. income 4 / 4	.009	.009
Fam. income 4 / 5	.046	.043
Fam. income 4 / 6	.051	.048
Fam. income 4 / 7	.084	.078

Table XII. Earnings equations, full time employed men, cohorts 1948-1957.

	OLS		IV		IV	
	Coefficient	St.error	Coefficient	St.error	Coefficient	St.error
Experience	0.0448	(0.0015)	0.0453	(0.0015)	0.0508	(0.0015)
Experience sq / 100	-0.0700	(0.0040)	-0.0749	(0.0040)	-0.0863	(0.0040)
Tenure	0.0099	(0.0004)	0.0099	(0.0004)	0.0100	(0.0004)
Tenure squared	-0.0051	(0.0002)	0.0050	(0.0002)	-0.0051	(0.0002)
Father college	0.0347	(0.0030)	-0.0008	(0.0042)	0.0548	(0.0046)
Mother college	0.0202	(0.0047)	-0.0033	(0.0051)	0.0375	(0.0053)
Family income 2	0.0162	(0.0022)	0.0119	(0.0022)	0.0178	(0.0022)
Family income 3	0.0430	(0.0023)	0.0307	(0.0025)	0.0466	(0.0025)
Family income 4	0.0954	(0.0025)	0.0703	(0.0032)	0.1049	(0.0035)
Education	0.0749	(0.0005)	0.1026	(0.0024)	0.0634	(0.0027)
Lambda			0.0522	(0.0043)	0.0320	(0.0044)
Education*Xi					-0.0138	(0.0005)
Constant	11.532	(0.0162)	11.443	(0.0177)	11.462	(0.0177)
R-squared	0.2238		0.2245		0.2283	
Observations	159,452		159,452		159,452	
Cohorts dummies	Yes		Yes		Yes	
County dummies	Yes		Yes		Yes	

Notes: Standard error in parenthesis. Family income is divided into four equally sized groups.
All the estimated coefficient are significantly different from zero.

Table XIII. Earnings equations, full time employed men, cohorts 1948-1957. Roy model.

	Level=1	Level=2	Level=3	Levels=4	Level=5	Level=6	Level=7
Experience	.026928	.030970	.055082	.043136	.050999	.080839	.084553
	(0.0040)	(0.0040)	(0.0039)	(0.0096)	(0.0051)	(0.0056)	(0.0057)
Experience sq / 100	-.000440	-.000235	-.001012	-.000877	-.000786	-.001654	-.001561
	(0.0000)	(0.0000)	(0.0000)	(0.0002)	(0.0001)	(0.0001)	(0.0001)
Tenure	.011569	.010768	.012012	.017476	.011923	.006854	.016516
	(0.0008)	(0.0008)	(0.0008)	(0.0025)	(0.0013)	(0.0019)	(0.0018)
Tenure squared	-.004834	-.004446	-.005790	-.009603	-.008124	-.005721	-.011367
	(0.0004)	(0.0004)	(0.0004)	(0.0012)	(0.0007)	(0.0010)	(0.0010)
Father college	.020067	.013230	.013619	.032547	-.005769	.014379	.000442
	(0.0045)	(0.0045)	(0.0042)	(0.0162)	(0.0074)	(0.0095)	(0.0105)
Mother college	.059956	.035636	.040110	.059347	.004724	.015090	.017951
	(0.0049)	(0.0049)	(0.0048)	(0.0220)	(0.0095)	(0.0114)	(0.0122)
Family income 2	.124261	.070130	.081317	.118227	.029707	.082828	.059301
	(0.0063)	(0.0061)	(0.0063)	(0.0330)	(0.0135)	(0.0156)	(0.0168)
Family income 3	.089688	.009227	.020460	.026263	-.015766	-.011849	.025189
	(0.0114)	(0.0100)	(0.0079)	(0.0252)	(0.0122)	(0.0152)	(0.0160)
Family income 4	.110098	.043323	.026597	.026317	-.019253	-.004452	-.001021
	(0.0258)	(0.0205)	(0.0136)	(0.0276)	(0.0132)	(0.0149)	(0.0111)
Xi	.003115	-.146309	-.067551	-.011152	-.079342	-.028732	-.007680
	(0.0287)	(0.0274)	(0.0260)	(0.0485)	(0.0260)	(0.0260)	(0.0237)
Constant term	11.8939	11.7918	11.6999	11.9881	11.9149	11.6058	11.6845
	(0.0460)	(0.0501)	(0.0474)	(0.1255)	(0.0612)	(0.0669)	(0.0621)
R-squared	0.0831	0.0909	0.0863	0.0923	0.0822	0.1360	0.1097
Observations	27810	27925	41981	7914	22336	15565	15921
Cohort dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Predicted log wage	12.343	12.373	12.482	12.681	12.778	12.864	13.191

Notes: Standard error in parenthesis. Family income is divided into four equally sized groups.
All the estimated coefficient are significantly different from zero.

Table XIV. Returns to education, Roy Model. Effects are measured i percent.

Educational level	Descriptive statistics		Roy model, Selection	
	Log earnings	Effect	ATE	TT
Comprehensive	12.397			
Vocational I	12.404	0.7	-2.1	0.5
Vocational II	12.483	8.6	8.6	8.3
Upper secondary	12.638	24.1	20.7	22.5
University I	12.676	27.9	28.4	30.6
University II	12.621	22.4	25.4	37.1
University III	12.853	45.6	52.3	64.4

Note: Effects are calculated based on E(y|x,S=I) - E(y|x,S=1) both for total sample (ATE) and for subgroups (TT). Standard errors are bootstrap using 100 replications. All coefficients are significantly different from zero, and are not reported here.

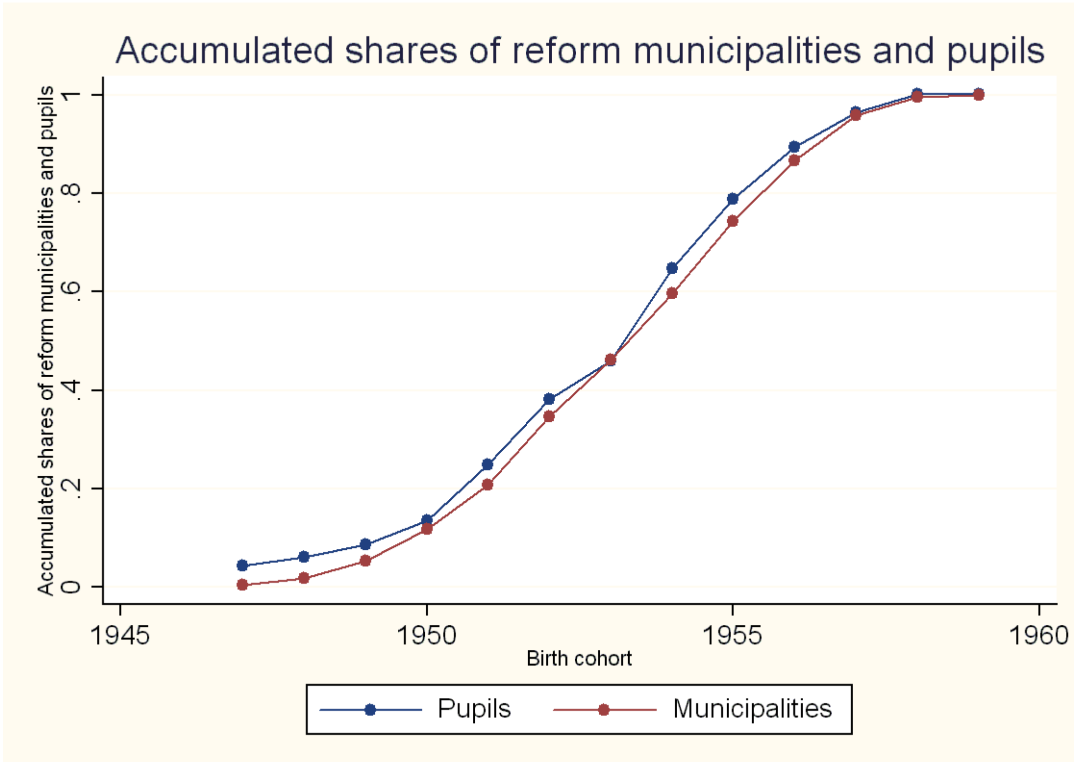

Figure I. Accumulated shares of after-reform municipalities and pupils.

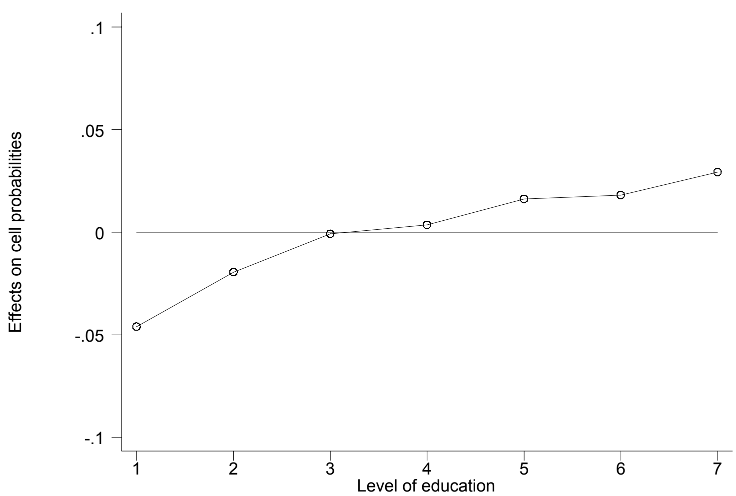

Figure II. The effect of the reform.

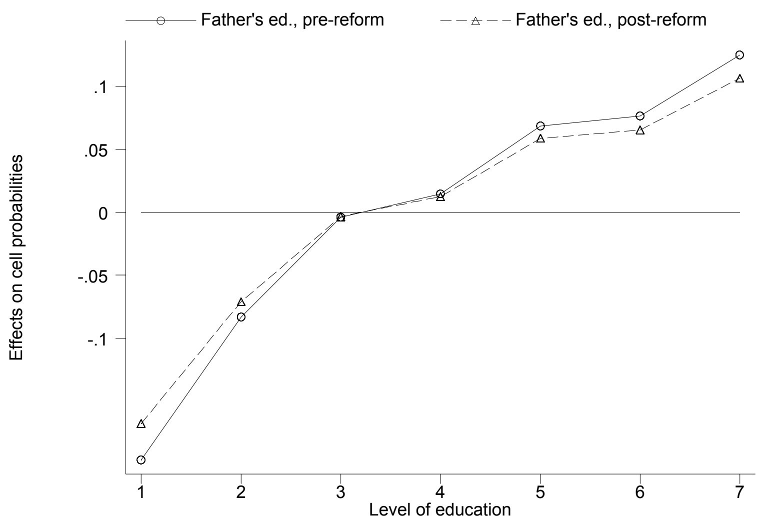

Figure III. The effect of father's education (college or not) before and after the reform.

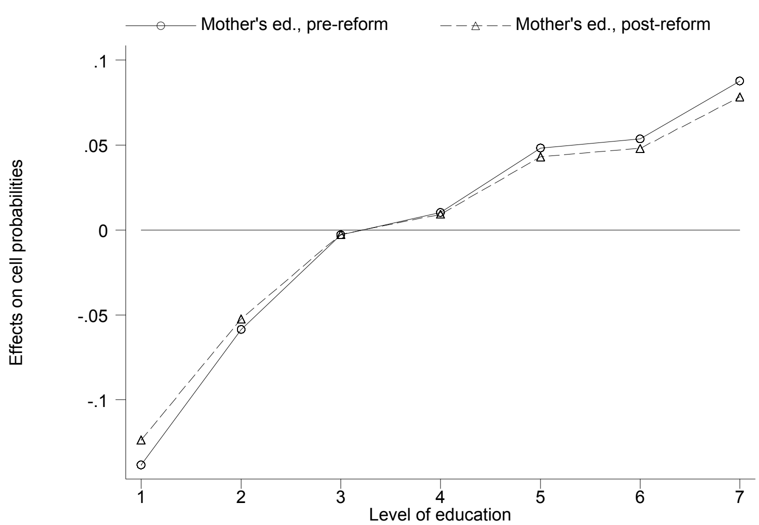

Figure IV. The effect of mother's education (college or not) before and after the reform.

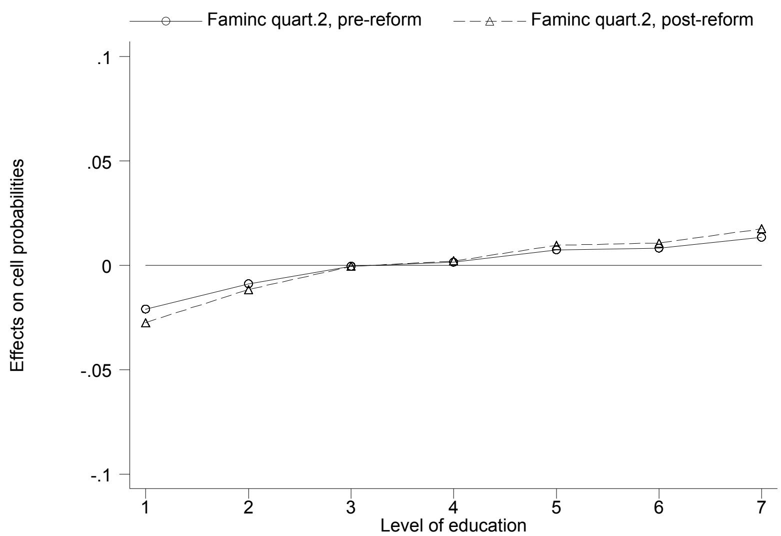

Figure V. The effect of income (second quartile) before and after the reform.

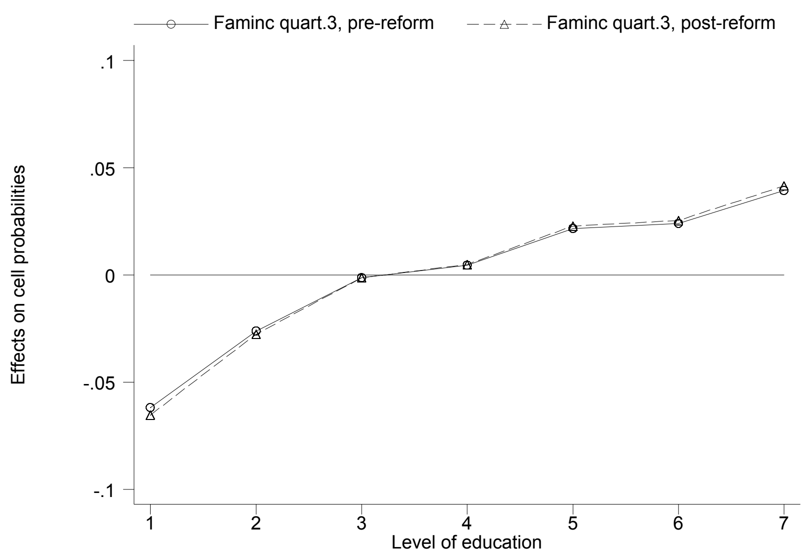

Figure VI. The effect of income (third quartile) before and after the reform.

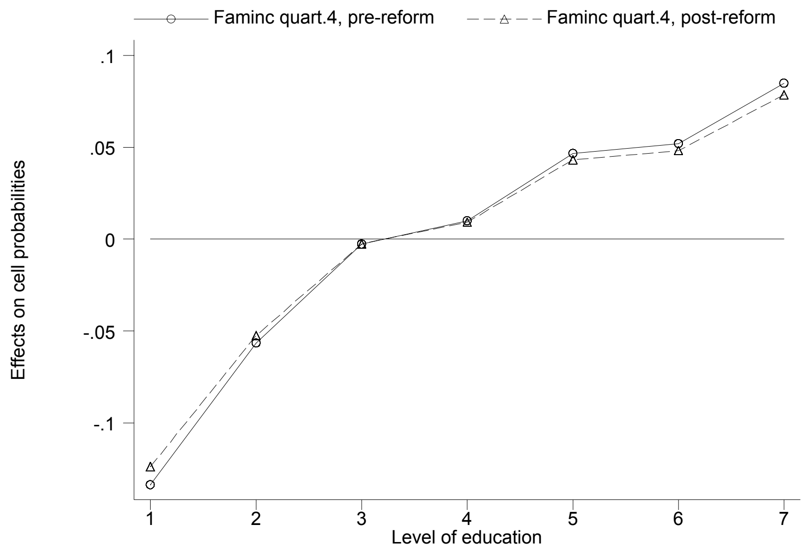

Figure VII. The effect of income (fourth quartile) before and after the reform.

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